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# A Laboratory Investigation of Compliance Behavior under Tradable Emissions Rights: Implications for Targeted Enforcement

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This paper uses laboratory experiments to test the theoretical observations that both the violations of competitive risk-neutral firms and the marginal effectiveness of increased enforcement across firms are independent of differences in their abatement costs and their initial allocations of permits. This conclusion has important implications for enforcing emissions trading programs because it suggests that regulators have no justification for targeting their enforcement effort based on firm-level characteristics. Consistent with the theory, we find that subjects' violations were independent of parametric differences in their abatement costs. However, those subjects that were predicted to buy permits tended to have higher violation levels than those who were predicted to sell permits. Despite this, we find no statistically significant evidence that the marginal effectiveness of enforcement depends on any firm-specific characteristic. We also examine the determinants of compliance behavior under fixed emissions standards. As expected, we find significant differences between compliance behavior under fixed standards and emissions trading programs.

Keywords: enforcement, compliance, emissions trading, permit markets, standards, command-and-control

JEL Classification: C91, L51, Q58

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# A Laboratory Investigation of Compliance Behavior under Tradable Emissions Rights: Implications for Targeted Enforcement

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## 1. Introduction

Although often overlooked, one of the most important design elements of any regulatory policy is how compliance with the policy will be enforced. Within the context of designing emissions trading programs, several authors have provided theoretical analyses of compliance incentives, the consequences of noncompliance, and strategies for enforcing emissions trading programs (e.g., Keeler 1991, Malik 1990, 1992, and 2002, vanEgteren and Weber 1996, Stranlund and Dhanda 1999, Stranlund and Chavez 2000). Taken as a whole, this literature suggests that firms' incentives toward noncompliance in market-based regulation, as well as the design of enforcement strategies to counteract these incentives, are quite different from compliance and enforcement of other policy instruments, particularly command-and-control regulations.

While there is a substantial body of economic theory about compliance and enforcement in emissions trading programs, there is limited empirical evidence regarding the determinants of compliance decisions in these programs. In this paper, we report the results of a series of experiments designed to examine some key factors that may affect compliance decisions. Our primary interest is whether differences in the individual characteristics of firms generate different compliance choices. This is an important issue because, on the face of it, one may suspect that firms with different production processes, abatement technologies, or initial allocations of permits may have different compliance incentives. If this is true, then regulators will be motivated to choose a targeted enforcement strategy, in particular targeted monitoring effort, which is conditioned on firm-level characteristics.

Regulators might use information about firm-level determinants of compliance behavior in different ways depending on what their enforcement objectives are. If some firms tend toward higher violations than others, then a regulator that is primarily motivated to detect those with higher violations will direct a greater share of his enforcement effort toward these firms. On the other hand, suppose that a budget-constrained regulator seeks to distribute his enforcement effort to minimize aggregate violations.[1] In this case, the regulator is not necessarily concerned about whether some firms have higher violations than others. His main concern is to direct his enforcement effort to where it is most productive. That is, he will monitor more closely those firms that are more responsive to increased enforcement than others, regardless of violation level.

Conceptually, Stranlund and Dhanda (1999) show that the individual compliance choices of risk neutral competitive firms in emissions trading programs are independent of differences in any firm-level characteristic. Consequently, regulators have no reason to condition their enforcement effort on firm-level characteristics. Their reasoning is straightforward. Since compliance in emissions trading programs means that a firm holds enough permits to cover its emissions, a risk neutral competitive firm's marginal benefit of noncompliance is what it has to spend for permits to make sure it is compliant; that is, the prevailing permit price. Thus, a firm's compliance decision is made by comparing this permit price with the marginal expected penalty for emissions in excess of permits. Since this marginal benefit-cost comparison does not depend on anything unique to a particular firm, the compliance decision is independent of any firm characteristic.

This result contrasts sharply with the effects of firm-level characteristics on compliance with command-and-control standards. A risk neutral firm's decision about whether to comply with a fixed emissions standard should be determined by the relationship between its marginal

[1] In the case of uniformly mixed pollutants, this is equivalent to seeking to reduce the environmental damage from noncompliance. If pollution is not uniformly mixed, then regulators may wish to target enforcement effort at firms that have greater impacts on environmental quality. We do not address this possibility in this paper.

reduction in abatement costs from exceeding the standard and the marginal expected penalty it faces for the resulting violation. Therefore, firms with higher marginal abatement costs or who face stricter standards will have a greater incentive to be noncompliant. In this way, firm-level characteristics are important determinants of compliance with fixed standards (Garvie and Keeler 1994). A recent paper by Gray and Shadbegian (2004) finds strong support for this conclusion in their analysis of compliance behavior by pulp and paper manufacturers.

Our experimental results are largely consistent with the general hypothesis that individual compliance decisions in emissions trading programs are independent of differences in firms' characteristics, but not completely so. We find that individual violations are independent of parametric differences in their abatement costs. However, individual violations are not independent of the initial allocation of permits. What appears to matter most about the initial allocation of permits is how it determines who will sell permits and who will buy them. We find that those subjects who are predicted to buy permits tend to have higher violation levels than those who are predicted to sell permits. We also test whether the marginal productivity of increased enforcement in reducing individual violations varies according to differences in abatement costs and initial allocations of permits, and find that the marginal productivity of enforcement is statistically independent of firm-specific characteristics.

We also ran a series of experiments in which subjects faced fixed emissions standards to compare compliance behavior under emissions trading and emissions standards. These experiments were identical to our market experiments, except that subjects could not trade their initial allocation of permits. The results of these experiments provide an empirical study of how different compliance behavior is under the two types of policies. Consistent with theoretical expectations (e.g., Garvie and Keeler 1994), subjects with higher abatement costs had significantly higher violations and were much more responsive to increased enforcement.

Clearly our results have important implications for designing enforcement strategies for competitive emissions trading programs. There is little empirical justification for targeting firms in these programs based on differences in their characteristics. While our results suggest that an enforcer whose objective is to detect and penalize firms that tend toward higher violations may wish to monitor permit buyers more closely, irrespective of abatement costs, there appears to be no justification for targeting firms to maximize the productivity of enforcement resources. Under fixed emissions standards, however, there is substantial justification for targeted enforcement strategies that are conditioned on information about firms' abatement costs

Although experimental techniques have been used to evaluate other policy initiatives, including some aspects of emissions trading programs (e.g., Cason 1995, Cason and Plott 1996, Ishikida *et al*. 1998, Isaac and Holt 1999), these techniques have not yet been widely applied to issues of regulatory enforcement, much less to compliance behavior in emissions trading programs.[2] However, laboratory experiments are particularly useful for an inquiry into the determinants of compliance in these programs, because the opportunities for empirical analyses of compliance behavior in existing trading programs appear to be very limited at present. Some programs have achieved such high rates of compliance that there is simply is not enough variation in compliance decisions to conduct meaningful econometric analyses. For example, the enforcement strategies of the Sulfur Dioxide Allowance Trading program and the RECLAIM program of southern California were clearly designed to achieve very high rates of compliance, and have been largely successful in doing so (Stranlund, Chavez, and Field 2002). Other emissions permit markets have not yet fully developed. This is the case for the emissions trading program for total suspended particulates in Santiago, Chile (Montero, Sanchez, and Katz 2002). In fact, because so few trades have taken place in this program, Palacios and Chávez (2004) were

[2] See Alm and McKee (1998) for a review of the experimental literature that focuses on tax compliance.

forced to assume that sources in this program faced fixed standards to conduct an empirical analysis of compliance behavior in this program.

We are aware of only two papers that use laboratory experiments to examine compliance behavior in emission permit markets. Cason and Gangadharan's (2004) experiments involve emissions trading when emissions are stochastic, permits can be banked, and subjects' performance is audited based on their past compliance history. This design allowed them to identify interesting interactions between random emissions shocks, permit banking, and compliance. Our approach is much simpler: emissions are deterministic, banking permits from one period to the next is not allowed, and audits are completely random with a known and constant probability. This approach allows us to generate fundamental results about the potential for targeted enforcement in emissions trading programs that Cason and Gangadharan do not consider.

Murphy and Stranlund (2004) is closely related to our current work in terms of design, but has a different focus. That work considers the effects of changing enforcement strategies on the permit market, and in turn the indirect effects of enforcement on compliance behavior. Although identifying the direct and indirect market effects of enforcement is also important to us in this paper, Murphy and Stranlund (2004) do not examine how the marginal productivity of enforcement may vary across firms, and hence, they do not draw conclusions about whether targeted enforcement is appropriate in emission trading programs. In addition, their work focuses solely on imperfect compliance. We include treatments in which subjects are predicted to be perfectly compliant so that we can examine firm-level determinants of compliance over the full range of enforcement strategies, from weak enforcement with significant noncompliance to strong enforcement that is sufficient to induce perfect compliance. Finally, neither Cason and Gangadharan (2004) nor Murphy and Stranlund (2004) conduct experiments with fixed emissions standards to draw conclusions about how compliance decisions differ under market-based and command-and-control regulation.

The rest of this paper is organized as follows. In the next section we present a positive theory of compliance in a competitive emissions trading program to develop the hypotheses to be tested. In section 3 we provide details of the experiments we designed to conduct these tests. The results of the experiments are presented and discussed in section 4. Section 5 concludes.

## 2. Theory and Hypotheses

Consider a fixed number of risk neutral firms in a perfectly competitive emissions trading program.[3] The abatement costs of firm $i$ are summarized by $c(q^i, \theta^i)$, which is strictly decreasing and convex in the firm's emissions $q^i$. Heterogeneity of the firms is captured by the parameter $\theta^i$, and we assume that total and marginal abatement costs are increasing in this parameter. A total of $Q$ emissions permits are distributed to the firms, free of charge. Firm $i$ receives $l_0^i$ permits initially, and holds $l^i$ permits after trading in a compliance period is complete. Competitive behavior in the permit market establishes a constant price per permit $p$.

If firm $i$ is noncompliant, then its emissions exceed the number of permits it holds and the magnitude of its violation is $v^i = q^i - l^i > 0$. If the firm is compliant, $q^i - l^i \leq 0$ and $v^i = 0$. To check for compliance, each firm is audited with a known probability $\pi$. A firm that is found to be in violation is assessed a penalty, $f(v^i, \phi)$. There is no penalty for a zero violation, but for positive

[3] None of the results that we present in this section are new, so we do not provide rigorous derivations. Interested readers should consult Stranlund and Dhanda (1999) for a complete analysis. It is important to note that the theoretical model we use in this paper can easily be applied to other tradable property rights programs. For example, recent papers by Hatcher (2005) and Chavez and Salgado (2004) are direct applications of the literature on compliance and enforcement of emissions trading to individual transferable fishing quotas. Thus the results of this paper apply more broadly than just emissions trading.

violations, the penalty is positive, strictly increasing and strictly convex. An increase in the parameter $\phi$ increases both total and marginal penalties.

Assuming that each firm chooses positive emissions and permits and never over-complies, then *i*'s problem is to minimize its expected costs, $c(q^i, \theta^i) + p(l^i - l_0^i) + \pi f(q^i - l^i, \phi)$, subject to $q^i - l^i \geq 0$. It is more convenient for our purposes to focus on the choices of violations and emissions rather than on permit demands and emissions. Therefore, we rewrite the problem as $\min_{(v^i, q^i)} c(q^i, \theta^i) + p(q^i - v^i - l_0^i) + \pi f(v^i, \phi)$, subject to $v^i \geq 0$. The following first-order conditions are both necessary and sufficient to determine optimal choices of violation and emissions:

$$\mathcal{L}_v = -p + \pi f_v(v^i, \phi) \geq 0, \ v^i \geq 0, \ \mathcal{L}_v v^i = 0; \qquad [1]$$

$$\mathcal{L}_q = c_q(q^i, \theta^i) + p = 0. \qquad [2]$$

These conditions implicitly define a firm's optimal choices of violation and emissions as $v^i = v^i(\theta^i, \pi, \phi, p)$ and $q^i = q^i(\theta^i, \pi, \phi, p)$. Note that these choices do not depend at all on a firm's initial allocation of permits, simply because $l_0^i$ does not enter either first-order condition. This implies that individual violation levels and the marginal productivity of enforcement across firms do not vary with changes in the initial allocation of permits. [4]

For the purposes of this study, the most important result from this simple model is that the decision to comply and the choice of violation level are independent of any firm-specific characteristics (which are reflected in the parameters $\theta^i$ and $l_0^i$). From [1] it is easy to establish

[4] This is an extension of Montgomery's (1972) result that emissions choices are independent of the initial allocation of permits to environments in which firms can be noncompliant. It is well known that this result does not hold in the presence of market power (Hahn, 1984) or transaction costs (Stavins, 1995). Similarly, compliance choices will not be independent of the initial allocation of permits in the presence of market power (van Egteren and Weber, 1996; Malik, 2002; Chavez and Stranlund, 2003), or transaction costs (Chavez and Stranlund, 2004).

that a firm will be compliant if and only if $p \le \pi f_v(0,\phi)$; that is, a firm is compliant if and only if the permit price is not greater than marginal expected penalty of a slight violation. Note that there is nothing in this decision rule that is unique to an individual firm. Therefore, a firm's decision about whether to comply is independent of any of its characteristics. This independence result extends to the violation level of a noncompliant firm as well. To see this let [1] hold with equality and substitute the firm's choice of violation, $v^i = v^i(\theta^i, \pi, \phi, p)$, to obtain

$$p - \pi f_v(v^i(\theta^i, \pi, \phi, p)) \equiv 0. \qquad [3]$$

Differentiate this identity with respect to $\theta^i$ to obtain $-\pi f_{vv} v^i_\theta = 0,$ which implies $v^i_\theta = 0.$ This result indicates that firms' violations are independent of parametric differences in their abatement costs. Conceptually, therefore, there is no reason for regulators to believe that some firms will be more likely to be noncompliant or tend toward higher violations even though they may have very different abatement cost functions or initial permit allocations. Hence, a regulator that is motivated to target his enforcement resources to detect incidences of noncompliance or higher levels of violation cannot do so productively on the basis of firm-level characteristics.

On the other hand, a regulator that is motivated to target his enforcement effort where it will be most productive in reducing aggregate violations will not necessarily care about individual violation levels, but rather will be focused on the marginal productivity of his enforcement effort across firms. This matter is complicated somewhat by a unique feature of enforcing market-based policies: enforcement will have a direct effect on individual violations, as well as an indirect effect through its impact on the equilibrium of the permit market.

From [3] obtain $v^i_\pi = -f_v / \pi f_{vv} < 0$ and $v^i_\phi = -f_{v\phi} / f_{vv} < 0$, which confirm the unsurprising results that, holding the permit price constant, firms respond to either increased monitoring or increased penalties by decreasing their violations. In principle, firms could reduce their violations by purchasing more permits or reducing their emissions. However, [2] indicates that firms always choose their emissions to equate their marginal abatement costs to the permit price. This rule has nothing to do with the enforcement strategy firms face. Therefore, again holding the permit price constant, firms reduce their violations in response to increased enforcement by purchasing more permits, not by decreasing their emissions.

If every firm increases its demand for permits with increased enforcement, then the equilibrium permit price must increase. To define the equilibrium permit price, first define the vector $\theta = \{\theta^i\}_{i \in N}$, where $N$ is the set of regulated firms. Given that a total of $Q$ permits are issued to the firms, and the enforcement authority has committed itself to monitoring each firm with probability $\pi$ and imposing penalties with parameter $\phi$, the equilibrium permit price is $\overline{p} = \overline{p}(\theta, \pi, \phi, Q)$. This is the implicit solution to the permit market clearing condition, $\sum_N l^i(\theta^i, \pi, \phi, p) = Q,$ where $l^i(\theta^i, \pi, \phi, p)$ is permit demand by firm $i$. It is easy to demonstrate that the equilibrium permit price is indeed increasing in the enforcement parameters; that is, $\overline{p}_\pi > 0$ and $\overline{p}_\phi > 0$. Moreover, an increase in the permit price motivates firms toward higher violations. To demonstrate this, obtain $v_p^i = 1/\pi f_{vv} > 0$ from [3]. The intuition here is that the permit price represents the marginal cost of compliance; thus, a higher permit price increases the incentive toward noncompliance.

The equilibrium violation of a firm is defined as $\overline{v}^i(\theta^i, \pi, \phi, Q) = v^i\left(\theta^i, \pi, \phi, \overline{p}(\theta, \pi, \phi, Q)\right)$. The marginal productivity of increased enforcement on the violations of firm $i$ is therefore $\overline{v}_\pi^i = v_\pi^i + v_p^i \overline{p}_\pi$ and $\overline{v}_\phi^i = v_\phi^i + v_p^i \overline{p}_\phi$. The direct effect of increased enforcement is to reduce the violation of an individual firm ($v_\pi^i < 0$ and $v_\phi^i < 0$), but the indirect effect motivates higher violations ($v_p^i \overline{p}_\pi > 0$ and $v_p^i \overline{p}_\phi > 0$). It is easy to show that the direct effect dominates the indirect effect so that individual violations fall with increased enforcement. Murphy and Stranlund (2004) confirm this hypothesis in the laboratory.

Now let us turn to possible differences in the marginal productivity of enforcement across firms. Suppose that $\overline{v}_{\pi}^{i} > \overline{v}_{\pi}^{j}$ for two firms, *i* and *j*. That is, at the margin, the regulator's monitoring effort is more productive in reducing the violation of firm *i* than firm *j*. If the regulator's objective is to reduce aggregate violations and the marginal cost of monitoring the two firms is the same, the regulator would be motivated to monitor firm *i* somewhat more closely than firm *j*. Conceptually, however, since a firm's choice of violation is independent of its type and its initial allocation or permits, the marginal productivity of the regulator's enforcement is independent of these parameters as well.[5] This implies that differences in $\overline{v}_{\pi}^{i}$ and $\overline{v}_{\pi}^{j}$ cannot be attributed to differences in *i*'s and *j*'s abatement costs or their initial allocations of permits.

Matters are very different for firms that face fixed emissions standards. If firm *i* faces an emissions standard $s^i$, its compliance problem is to choose emissions to minimize $c(q^i, \theta^i) + \pi f(q^i - s^i, \phi)$, subject to $q^i \geq s^i$. Equivalently, it chooses its violations to minimize $c(v^i + s^i, \theta^i) + \pi f(v^i, \phi)$. The first-order condition is $c_q(v^i + s^i, \theta^i) + \pi f_v(v^i, \phi) \geq 0$; if this is strictly positive, then $v^i = 0$. Clearly, the firm is noncompliant if and only if $-c_q(s^i, \theta^i) > \pi f_v(0, \phi)$; that is, if and only if the firm's marginal abatement costs evaluated at the standard is greater than the marginal expected penalty of a slight violation. It is straightforward to show that a noncompliant firm's violation is increasing in its marginal abatement costs [ $v_{\theta}^{i} < 0$ ] and decreasing in the emissions standard [ $v_{s}^{i} < 0$ ]. Thus, a regulator who is motivated to detect and penalize noncompliant firms and those that tend toward higher violations should target firms with higher marginal abatement

[5] That is, $v_{\theta}^{i} = 0$ implies $v_{\pi\theta}^{i} = v_{\phi\theta}^{i} = v_{p\theta}^{i} = 0$ and consequently, $\overline{v}_{\pi\theta}^{i} = 0$

costs and/or lower standards. Contrast this to competitive emissions trading for which regulators cannot use differences in marginal abatement costs or the initial allocation of permits to target noncompliant firms or those with higher violations.

In further contrast to permit markets, the marginal productivity of increased enforcement with standards is likely to depend on firms' abatement costs and emissions standards. Although, in general, it is not possible to identify the firms for which a regulator's enforcement effort is more effective, if firms' abatement costs and the penalty function are quadratic, as we assume in our experimental design, then it is straightforward to show that $v^i_{\pi\theta} < 0$ and $v^i_{\pi s} < 0$. These indicate that the marginal productivity of increased monitoring in reducing violations is greater for firms with higher marginal abatement costs or that face lower standards. The same holds for the marginal productivity of increased penalties. Thus, to use his enforcement resources most effectively, a regulator is justified in targeting firms with higher marginal abatement costs or lower standards.

Our results about violation choices, the marginal productivity of enforcement, and the potential for targeted enforcement are summarized in the following hypotheses:

**Hypothesis 1:** Under a competitive emissions trading program, the violation levels of risk neutral firms are independent of differences in their abatement costs and their initial allocations of permits.

**Hypothesis 2:**Under a competitive emissions trading program, the marginal productivity of increased enforcement (either increased monitoring or increased penalties) on the violation levels of risk neutral firms is independent of differences in the firms' abatement costs and their initial allocations of permits.

**Hypothesis 3:** Under fixed emissions standards, the violation levels of risk neutral firms are higher for firms with higher marginal abatement costs and/or lower emissions standards.

**Hypothesis 4:** Under fixed emissions standards, the marginal productivity of increased enforcement (either increased monitoring or increased penalties) on the violation levels of risk neutral firms is greater for firms with higher marginal abatement costs and/or lower emissions standards.

## 3. Experimental Design and Procedures

### *3.1 Experiment design*

The experiments were designed to test our hypotheses about the relationships between individual firm characteristics and violation choices, but the subjects were placed in a more neutral environment. We framed the experiments as a production decision in which permits conveyed a license to produce, rather than an emissions decision, to avoid introducing potential biases due to individual attitudes about the environment or emissions trading. During each period of the experiment, subjects simultaneously chose to produce units of a fictitious good and traded in a market for permits to produce the good. Participants could produce as many units of the good as they wished (up to a capacity constraint) regardless of the number of permits that they owned. However, at the end of the period, each individual was audited with a known, exogenous probability. If an individual was audited and found to be non-compliant (*i.e.*, total production exceeded permit holdings), then a penalty was applied.

Subjects received a benefit from their choice of production, $q$, which was generated from a linear marginal benefit function $b_i'(q) = 18 - \beta_i q$. Each experiment had eight subjects divided evenly into two types; subjects were randomly assigned a type. Subjects with a high marginal benefit function ($\beta_H = 1$) also had a greater production capacity (17 units). Subjects with a low marginal benefit function ($\beta_L = 2$) could produce up to 8 units. Production was constrained to be a whole number. High marginal benefits from production in our experiments are equivalent to high marginal abatement costs in emissions trading. Therefore, throughout the remainder of the paper we denote subjects with high marginal production benefits as *HighMAC* subjects, and subjects with low marginal production benefits as *LowMAC* subjects.

The treatment variables in this paper are the probability of an audit, the marginal penalty function, the initial permit allocation, and the total supply of permits. Table 1 summarizes the experimental design. Each of the twelve cells was repeated three times. Although our main focus is on compliance behavior in permit markets, we also ran several experiments in which subjects faced fixed standards to compare compliance behavior under emissions trading and emissions standards.

To be compliant in the market experiments, subjects were required to possess sufficient permits, $l$, to cover their production choices. Limiting the total number of permits imposed an aggregate production standard. We consider two aggregate standards. In the low aggregate standard experiments ($Q_L = 28$), denoted *LowStandard*, each of the four *HighMAC* subjects was allocated three permits, and the four *LowMAC* subjects were each given four permits. We call this the (nearly) uniform initial allocation. In the high aggregate standard experiments ($Q_H = 56$), denoted *HighStandard*, there were two different initial allocations of permits. With a uniform initial allocation, each of the eight subjects in an experiment started with seven permits. With a nonuniform initial allocation, the *HighMAC* subjects began with 13 permits, and the *LowMAC* subjects had a single permit. Reallocating the initial allocation of permits in this way changes the prediction about which subjects would sell permits and which would buy permits. In the competitive equilibrium, the *LowMAC* subjects would be the net sellers of permits with the uniform initial allocation, and net buyers of permits when the initial allocation is nonuniform.

To check for compliance, each subject's record was examined with a known probability $\pi$. If a subject was audited and found to be non-compliant, that is $q > l$, then she was assessed a penalty that was generated from a linearly increasing marginal penalty function, $f'(q-l) = F + \phi(q-l)$. By changing the parameters of the marginal expected penalty function, $\pi f'(q-l) = \pi[F + \phi(q-l)]$, we developed four enforcement strategies, which we label *High*, *Medium*($\pi_H$), *Medium*($\pi_L$), and *Low*.[6] The *High* enforcement strategy involved a high audit probability and a high marginal penalty function ($\pi_H = 0.70$, $F = 17.5$, $\phi = 1.43$). All *High* marginal expected penalty treatments were parameterized to induce perfect compliance by risk neutral agents. With the high aggregate standard, however, the incentives for perfect compliance were much stronger. For the other three levels of enforcement, subjects were expected to choose to be noncompliant. The treatments *Medium*($\pi_H$) and *Medium*($\pi_L$) involved the same marginal expected penalties, but *Medium*($\pi_H$) had a higher monitoring probability and a relatively low marginal penalty function ($\pi_H = 0.70$, $F = 6$, $\phi = 1.43$), whereas *Medium*($\pi_L$) had a lower monitoring probability and a higher marginal penalty function ($\pi_L = 0.35$, $F = 12$, $\phi = 2.90$). Our intention here was to examine whether the subjects reacted differently to monitoring and penalties. The *Low* marginal expected penalty treatments had the weakest enforcement strategy with the low monitoring probability and a low marginal penalty function ($\pi_L = 0.35$, $F = 2$, $\phi = 2.90$). Enforcement parameter values were chosen, in part, so that the expected marginal penalty functions are parallel to each other—each has a slope of about one.

[6] The enforcement parameters were the same for each subject type with the exception that, since *LowMAC* subjects could only produce up to eight units, only the first eight steps of the penalty schedule was displayed for them.

*3.2 Experiment procedures*

Participants were recruited from the student population at the University of Massachusetts, Amherst. Subjects were paid $7 for agreeing to participate and showing up on time, and were then given an opportunity to earn additional money in the experiment. These additional earnings ranged between $5.68 and $17.49, with a mean of $13.67 ($\sigma$ = 1.46). Earnings were paid in cash at the end of each experiment. Each experiment lasted about 2 hours.

The experiments were run in a computer lab using software designed specifically for this research. To familiarize subjects with the experiments, we ran a series of training experiments. In the first stage of the trainers, students read online instructions that included interactive questions to ensure that they understood the instructions before proceeding. After everyone had completed the instructions and all questions were answered, the training experiment began. These practice rounds contained all the same features as the "real data" experiments with the exception that we used a different set of parameters. The data from the trainers were discarded.

For the real data sessions, we recruited participants from the pool of trained subjects. Subjects were allowed to participate in multiple sessions. A total of 176 subjects participated in 36 eight-person experiments. Prior to the start of the real data experiments, subjects were given a summary of the experiment instructions.[7] The experimenter read these instructions aloud and answered any questions. Each subject was given a calculator, a pencil and paper. Each experiment consisted of 12 identical rounds. At the start of each period, the eight subjects were each given an initial allocation of permits and $10 in experimental cash.

A unique feature of our experiments is that the production decisions and permit market trading were unbundled into two separate, but simultaneous, activities. We did this to allow for

[7] Available upon request.

the possibility that the production levels and permit holdings could differ, thereby introducing a compliance decision. During the period and concurrent with the production decision, subjects had the ability to alter their permit holdings by trading in a continuous double auction. In the auction, individuals could submit bids to buy or asks to sell a single permit. The highest bid and lowest ask price were displayed on the screen. A trade occurred whenever a buyer accepted the current ask or a seller accepted the current bid. After each trade, the current bid and ask were cleared and the market opened for a new set of bids and asks. The trading price history was displayed on the screen.

Each period lasted a total of five minutes. The permit market was open for the entire period, but production had to be completed in the first four minutes. The one-minute reconciliation period gave subjects a final opportunity to adjust their permit holdings. After each period ended, random audits were conducted and penalties were assessed. All information relating to audit outcomes was private.

As noted earlier, we also conducted a series of experiments with a fixed standard for the subset of treatments in Table 1 that have a uniform initial permit allocation. These standards experiments were the same as their market counterparts, except that the permits were not tradable. Therefore, the initial permit allocation became the fixed standard. We use these treatments to test Hypotheses 3 and 4.

This design yields observations on individual decisions and market outcomes; of particular interest in this paper are violations and permit prices. Giving subjects different production benefit schedules and initial allocations of permits allow us to test for the impacts of these differences on individual violations under both emissions trading and fixed emissions standards (Hypotheses 1 and 3). Moreover, varying the enforcement parameters allows us to test whether the marginal productivity of enforcement is sensitive to differences in production benefits and initial permit allocations or fixed standards (Hypotheses 2 and 4).

## 4. Results

We begin our analysis of the results with a discussion of general patterns in the permit price and individual violations. We use some of these patterns to motivate our econometric model specifications when estimating these variables. The econometric analysis is used to test our specific hypotheses. Because our theoretical development suggests that an individual's violation decision is conditioned on the permit price, we first estimate this price and then use the estimated price as an instrumental variable when modeling violation choices. Because individuals make decisions in multi-round experiments, we control for repeated measures using linear random effects models. We omit the data from the first period to minimize the effects of learning and price discovery. This omission does not have a qualitative effect on any of our conclusions.

In addition to the data reported in this paper, we also ran a separate series of "perfect compliance" experiments for the low and high standard with uniform permit allocations. These experiments were identical to those described in this paper, except that emissions were assumed to exactly equal the final permit balance; that is, noncompliance was not allowed and audits were unnecessary. As expected, observed prices and quantities in these experiment quickly converged to the competitive equilibrium. Therefore, we are confident that any deviations from the competitive prediction in the discussion below reflect treatment effects related to the compliance decision and are not artifacts of the subject pool or experimental design.[8]

[8] The results from our "perfect compliance" experiments are available upon request.

*4.1 General patterns in market experiments*

Table 2 presents some summary statistics of permit prices. Note that mean and median prices tend to be higher than the competitive equilibrium prediction, but they move as expected: prices are higher when the aggregate standard is reduced and when the marginal expected penalty is increased. Note, however, that with the *HighStandard* treatments, for a given marginal expected penalty, mean and median prices are higher for the nonuniform initial allocation of permits. This, of course, is not consistent with the conventional hypothesis that behavior and market outcomes under emissions trading programs are independent of the initial allocation of permits.

Table 3 presents some summary statistics for individual violations. Mean and median violations also move as expected. Given the aggregate standard, they tend to be lower with higher marginal expected penalties, and given the enforcement strategy, they tend to be lower with the *HighStandard*. Conceptually, this latter result is due to the lower permit prices resulting from a higher aggregate standard.

We note an interesting pattern in mean and median violations that plays an important role in how we analyze and interpret violation choices. The competitive equilibrium prediction is that individual violations should be uniform across subjects in each treatment. However, mean and median violation levels clearly differ by subject type, but whether *HighMAC* subjects tend toward higher or lower violations than *LowMAC* subjects appears to depend on the initial allocation of permits. In particular, consider the eight uniform allocation treatments. The *HighMAC* subjects are predicted to be the net buyers of permits in these treatments, and they clearly tend toward higher violations than the *LowMAC* subjects. On the other hand, for the four nonuniform allocation treatments, the *LowMAC* subjects are predicted to be the permit buyers, and they are the ones that tend toward higher violations. It appears that the differences in violations by subject type may be determined in part by how the initial allocation of permits determines whether subjects will be net buyers or sellers of permits. This speculation is easily tested and we will do so shortly.

*4.2 Market regression results and tests of hypotheses*

Table 4 presents the results of a linear random effects model of the permit price. The dependent variable is the price of each trade in period $t = 2,\ldots,12$ of group $j = 1,\ldots,27$. The marginal expected penalty, the aggregate standard, and the initial allocation are modeled as fixed effects. Assuming risk neutral subjects, since the *Medium*($\pi_H$) and *Medium*($\pi_L$) enforcement strategies have the same marginal expected penalties, in theory both should lead to identical market outcomes. A test of the null hypothesis that the coefficients on the dummy variables for the *Medium*($\pi_H$) and *Medium*($\pi_L$) treatments are equal cannot be rejected at any conventional level of significance (Wald $\chi^2(1) = 0.92$, $p = 0.34$). Thus, decreasing monitoring and increasing penalties, but leaving the marginal expected penalty function unchanged had no affect on market prices.

The price estimation results in Table 4 confirm the impressions we reached by comparing average prices across treatments. The coefficients for the three marginal expected penalties are increasing, indicating that increased enforcement puts upward pressure on the equilibrium price. The *NonUniform* coefficient is positive and significant, as we expected from our perusal of the average price results, even though theory predicts otherwise. Lastly, the coefficient on *HighStandard* is negative and significant, indicating the unsurprising result that permit prices fall with a greater supply of permits.

Table 5 presents the results of a two linear random effects models for individual violations. Model 1 estimates the effects of the treatment variables on violations. Model 2 adds the interaction effects needed to test Hypothesis 2, which requires that we capture all possible combinations of subject type and treatment cells in Table 2. Using an instrumental variable approach, *PriceHat* is the estimated price from the model in Table 4. *NetSeller* is a fixed effect that equals one if the subject is predicted to be a net seller (*HighMAC* subjects for the nonuniform allocations and *LowMAC* subjects for the uniform allocations). *HighMAC* is a fixed effect that equals one for *HighMAC* subjects. Note the negative and significant impact of enforcement, and the positive and significant impact of price. These are consistent with the theoretical model we presented in section 2.

While comparing mean and median violations across treatments, we suggested that differences in violations by subject type may be determined in part by how the initial allocation of permits determines whether subjects will be net buyers or sellers of permits. This is confirmed by the estimation results. In Model 1, the *NetSeller* coefficient is negative and significant. Although *NetSeller* is not significant in Model 2, a test of the joint hypothesis that *NetSeller* and the four terms interacted with *NetSeller* are all equal to zero is rejected (Wald $\chi^2(5) = 27.6$, $p = 0.00$). On the other hand, the coefficient on *HighMAC* is small and not statistically significant in both Models 1 and 2.[9]

Hypothesis 1 asserts that individual violations are independent of any subject-level characteristic. Our tests of this hypothesis are mixed. We have strong support for the hypothesis that violations are independent of differences in subjects' marginal abatement costs. However, individual violations are not independent of how the initial allocation of permits determines which subjects will sell permits and which will buy them. The implications of these results for targeted enforcement are clear. A regulator that is motivated to find and penalize those firms with higher violations has some justification for targeting firms that buy permits, but has no reason to target firms on the basis of information regarding individual abatement costs.

[9] With Model 2, each of the four interaction terms containing *HighMAC* is not statistically significant. We also fail to reject the joint hypothesis that *HighMAC* and these four interaction terms are all equal to zero (Wald $\chi^2(5) = 0.37$, $p = 0.996$).

Although we find that permit buyers tend to have greater violation levels, it does not necessarily follow that a regulator that wishes to direct his enforcement effort to where it is most productive should target these firms. The estimates of the marginal productivity of enforcement in Table 6 reflect the sensitivity of violation choices by subject type to changes in enforcement pressure for a given aggregate standard and initial allocation. The values in the *HighMAC* and *LowMAC* columns are the estimated marginal changes in individual violations as enforcement is increased. Of course, these marginal effects have to include both the direct effects of enforcement and the indirect effects through the change in the permit price. Letting $\Delta E$ denote a change in enforcement, given an aggregate standard and initial allocation of permits, we calculated $\Delta v^i / \Delta E + (\Delta v^i / \Delta p)(\Delta p / \Delta E),\ i = \{HighMAC,\ LowMAC\}$, for each increase in enforcement, aggregate standard, and initial allocation of permits. $\Delta v^i / \Delta E$ is the direct effect of the change in enforcement, while $(\Delta v^i / \Delta p) \times (\Delta p / \Delta E)$ is the indirect price effect. Values for $\Delta v^i / \Delta E$ and $\Delta v^i / \Delta p$ were calculated using Model 2 in Table 5, and $\Delta p / \Delta E$ was calculated using the price equation in Table 4.

The last column of Table 6 contains the *p*-values for a test of the hypothesis that the marginal productivity of enforcement is equal across subject types.[10] Note that the estimates of these marginal effects are consistently higher for the *HighMAC* subjects when the initial allocation of permit is uniform, but these effects are higher for the *LowMAC* subjects when the initial allocation is not uniform. However, none of the differences in marginal productivities between subject types are statistically significant. Thus, we have strong support for Hypothesis 2—the marginal productivity of enforcement is independent of differences in subjects' abatement costs

[10] Although Table 6 compares subjects based on marginal abatement costs, the results would be identical if we instead compared them based on whether they were predicted to be net sellers or buyers. This is because all *HighMAC* subjects are net buyers with a uniform allocation and net sellers with a nonuniform allocation (the reverse is true for *LowMAC* subjects).

and initial allocation of permits. This suggest that a regulator that seeks to direct his enforcement effort to where it is most productive does not have a statistically significant justification for doing so on the basis of firm-level characteristics.

*4.3 Standards*

We do not expect this "no targeting" policy prescription to hold when permits are not tradable. Table 7 presents summary statistics for our fixed standards experiments with a uniform allocation of both low and high aggregate standards; we did not conduct any nonuniform allocation treatments.[11] Recall that these standards experiments were identical to the permit market experiments, except that subjects were not able to trade their initial permit allocation. As predicted, *HighMAC* subjects tended to have greater violation levels. The estimation results in Table 8 confirm this: the coefficient for *HighMAC* subjects is positive and highly significant. Moreover, the negative and significant coefficient for *HighStandard* indicates that individual violation levels tend to decrease as the standard is relaxed. Therefore, we have strong support for Hypothesis 3 and the enforcement implication that a regulator who is motivated to find and penalize firms with higher violations has justification for targeting firms with higher marginal abatement costs and lower emissions standards.

We use the interaction effects of Model 2 in Table 8 to test for differences in the marginal productivity of enforcement. Similar to Table 6 for the case of emissions trading, Table 9 includes the marginal productivity of enforcement under fixed standards and presents the results of tests for the equality of these values across subject types. Hypothesis 4 posits that violations of subjects with higher marginal abatement costs will be more responsive to changes in enforcement. This is true with our experimental design except in two cases. With a low standard,

[11] This implies that we have one low and one high individual standard for both *HighMAC* and *LowMAC* subjects.

our prediction is that the change from the low marginal expected penalty to either of the medium marginal expected penalties has the same effect on violations for both subject types (see Table 7); this is simply an artifact of the experimental design, in particular that emissions choices are discrete. As expected, when enforcement changes from *Low* to *Medium*($\pi_H$) there is not a statistically significant difference in the marginal productivity of this change across subject types ($p$ = 0.78). However, there is the unanticipated significant difference in marginal productivities when enforcement changes from *Low* to *Medium*($\pi_L$) ($p$ = 0.02). More to the main point of Hypothesis 4, however, we find that marginal productivity of enforcement is significantly higher for the *HighMAC* subjects for six of the eight cases in which we expect to observe this difference.

Hypothesis 4 also posits that the marginal productivity of increased enforcement is higher when individual standards (defined by the initial permit allocation) are lower. Observe in Table 9 that for each subject type the effect of each change in enforcement is greater when they face the low standard. However, these differences are statistically significant in only two of the five comparisons for the *HighMAC* subjects, and in only one of the five comparisons for the *Low MAC* subjects.

Although are results concerning Hypothesis 4 are mixed, we do have strong support for the policy recommendation that, under fixed standards, regulators who are motivated to direct their enforcement resources to where they will be most effective are justified in targeting firms with higher marginal abatement costs. For the purposes of this paper, however, the most important message is that while firm-level characteristics are important determinants of the effectiveness of enforcement under command-and-control standards, the marginal productivity of enforcement does not depend on these characteristics under a competitive emissions trading program.

**5. Conclusions**

Compliance behavior in emissions trading programs is fundamentally different from compliance behavior under emissions standards. Conceptually, the compliance choices of risk neutral firms that operate in a competitive emissions trading program are independent of parametric differences in their abatement costs and their initial allocations of permits. Therefore, regulators have no justification for targeting their enforcement efforts either to find and punish those firms that tend to higher violations, or to maximize the productivity of their enforcement efforts in reducing aggregate violations. On the other hand, since the compliance choices of firms that face fixed emissions standards depend on their abatement costs and the standards they face, regulators will be motivated to target firms with higher marginal abatement costs and those that face lower standards.

The results of our laboratory experiments generally support the conclusions of theoretical models of risk neutral compliance behavior in emissions trading programs, but not completely so. We found that the subjects' violations are independent of differences in their abatement costs, but their violations are not independent of how the initial allocation of permits determines which of them will sell permits and which will buy permits. Those subjects that were predicted to buy permits tended to have higher violation levels than those who were predicted to sell permits. Despite this, we found that individual differences in abatement costs or initial allocations of permits do not have a statistically significant impact on the marginal productivity of increased enforcement in reducing individual violations. Concerning targeted enforcement, then, regulators may be motivated to direct their enforcement efforts at firms that are net buyers of permits it they wish to find and penalize the most significant violators, but there is no theoretical reason or statistical evidence that targeting firms based on firm-specific characteristics to reduce aggregate violations is a productive strategy.

We also ran a series of experiments in which subjects faces fixed standards. Our results from these experiments are mostly consistent with the conclusions of a model of risk neutral compliance behavior. More importantly, however, the results serve to highlight how compliance behavior and, by implication, the design of enforcement strategies are fundamentally different under emissions trading and fixed standards.

Our finding that net buyers of permits tended toward higher violation in our emissions trading experiments could be consistent with an endowment effect that has been documented in a number of experimental settings (for a review see Kahneman, Knetsch and Thaler 1990). That net sellers of permits tended to have lower violations than buyers suggests that they tended to hold on to more permits than one would expect in a competitive equilibrium. While we find the possibility of an endowment effect intriguing, we hesitate to attribute our result to this phenomenon, because our experiments were not specifically designed to test for this effect; therefore we are unable to determine whether this effect is the source of the difference in the violation levels of permit buyers and seller. Moreover, market experiments like ours that use induced values in a double auction typically report highly efficient outcomes (Smith 1982). Indeed, as already mentioned, this is precisely what we observe in our "perfect compliance" experiments. Therefore, the initial allocation effect that we observe must be related in some way to the introduction of the compliance decision. While it is possible that the compliance decision induces an endowment effect, further research is needed to determine whether this is the case.

Our hypotheses concerning compliance behavior were formed from models of risk neutral firms; however, individual risk preferences are a relevant consideration in any model of compliance. In the theoretical literature on compliance and enforcement of emissions trading only Malik (1990) allows for non-neutral risk preferences, but he does not provide clear predictions of the qualitative impacts of enforcement on violation levels. Our experience suggests that specific predictions about these effects require severely limiting assumptions about agents' utility functions. Interestingly, there may be a conceptual basis for focusing on risk neutrality. Rabin (2000) has demonstrated that expected utility theory implies that people are approximately risk neutral when stakes are small, such as in our laboratory setting.

In our case, the model with risk-neutral firms performs quite well. It provides the comparative static results that are important for examining the potential for targeted enforcement, and the results are largely supported by the experimental data. In general we do believe, however, that experimental studies that examine compliance behavior in various settings could benefit from information about subjects' risk preferences. Unfortunately, there is no consensus about how to elicit these preferences. We believe that this is another important area for future research.

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**Table 1. Experimental design**

| | | *Uniform Allocation* | | *Nonuniform Allocation* |
|---|---|---|---|---|
| | | *Low Standard* | *High Standard* | *High Standard* |
| *Marginal expected penalty* | *High* | Both market & standards | Both market & standards | Market only |
| | *Medium*($\pi_H$) | Both market & standards | Both market & standards | Market only |
| | *Medium*($\pi_L$) | Both market & standards | Both market & standards | Market only |
| | Low | Both market & standards | Both market & standards | Market only |

Each cell repeated 3 times with 8 participants per group. Data from one subject dropped from the treatment with the *Low* marginal expected penalty and *Low Standard* due to bankruptcy after 3 periods.

**Table 2. Permit Price Summary Statistics**

| | Marginal Expected Penalty | Competitive Equilibrium | Mean | Median | Standard Deviation |
|---|---|---|---|---|---|
| Low Standard | *High* | 12 – 13 | 12.57 | 12.68 | 0.95 |
| | *Medium*($\pi_H$) | 8 - 8.20 | 9.61 | 9.30 | 1.18 |
| | *Medium*($\pi_L$) | | 13.26 | 13.50 | 1.84 |
| | *Low* | 6 | 8.11 | 7.90 | 1.55 |
| High Standard | *High* | 8 | 7.94 | 8.00 | 0.76 |
| | *High Nonuniform* | | 9.74 | 9.00 | 2.40 |
| | *Medium*($\pi_H$) | 6 - 6.20 | 7.09 | 7.25 | 1.02 |
| | *Medium*($\pi_H$) *Nonuniform* | | 7.79 | 7.60 | 1.10 |
| | *Medium*($\pi_L$) | | 6.74 | 6.85 | 0.58 |
| | *Medium*($\pi_L$) *Nonuniform* | | 7.24 | 7.20 | 1.39 |
| | *Low* | 4 | 3.97 | 4.00 | 0.74 |
| | *Low Nonuniform* | | 6.50 | 7.00 | 1.36 |

**Table 3. Summary Statistics for Individual Violations (Emissions Trading)**

| | Marginal Expected Penalty | Competitive Equilibrium | Firm MAC | Mean | Median | Standard Deviation |
|---|---|---|---|---|---|---|
| Low Standard | *High* | 0 | High | 0.4 | 0 | 0.8 |
| | | | Low | 0.2 | 0 | 0.5 |
| | *Medium*($\pi_H$) | 3 | High | 3.1 | 3 | 1.0 |
| | | | Low | 2.0 | 2 | 1.5 |
| | *Medium*($\pi_L$) | | High | 1.9 | 1 | 2.0 |
| | | | Low | 1.2 | 1 | 1.4 |
| | *Low* | (4 or 5) | High | 3.7 | 3 | 2.0 |
| | | | Low | 3.3 | 3 | 1.5 |
| High Standard | *High* | 0 | High | 0.1 | 0 | 0.5 |
| | | | Low | 0.1 | 0 | 0.4 |
| | *High Nonuniform* | | High | 0.1 | 0 | 0.4 |
| | | | Low | 0.2 | 0 | 0.5 |
| | *Medium*($\pi_H$) | 1 | High | 1.2 | 1 | 1.1 |
| | | | Low | 0.7 | 0 | 1.3 |
| | *Medium*($\pi_H$) *Nonuniform* | | High | 0.6 | 0 | 1.2 |
| | | | Low | 1.3 | 1 | 1.2 |
| | *Medium*($\pi_L$) | | High | 1.4 | 1 | 1.3 |
| | | | Low | 0.8 | 0 | 1.5 |
| | *Medium*($\pi_L$) *Nonuniform* | | High | 0.8 | 0 | 2.3 |
| | | | Low | 1.5 | 1 | 1.7 |
| | *Low* | (2 or 3) | High | 3.6 | 3 | 2.4 |
| | | | Low | 1.4 | 1 | 1.3 |
| | *Low Nonuniform* | | High | 1.8 | 1 | 2.0 |
| | | | Low | 2.8 | 2 | 2.0 |

**Table 4. Random Effects Estimation of Permit Price**

| Variable | Permit Price |
|---|---|
| *Intercept* | 8.32 (0.50)*** |
| *HighMEP* | 4.08 (0.58)*** |
| *Medium*($\pi_H$)*MEP* | 2.28 (0.58)*** |
| *Medium*($\pi_L$)*MEP* | 2.84 (0.58)*** |
| *HighStandard* | -4.20 (0.50)*** |
| *NonUniform Allocation* | 1.25 (0.50)*** |
| Wald $\chi^2(5)$ | 126.96*** |

4060 observations. Standard error in parenthesis. ** $p < 0.05$; *** $p < 0.01$

**Table 5. Random Effects Estimation of Individual Violations (Emissions Trading)**

| Variable | Model 1 | Model 2 |
|---|---|---|
| *Intercept* | 2.44 (0.27)*** | 2.60 (0.76) *** |
| *NetSeller* | -0.65 (0.15) *** | -1.26 (-0.77) |
| *HighMAC* | 0.05 (0.15) *** | 0.34 (0.77) |
| *HighMEP* | -2.98 (0.24) *** | -3.51 (0.56) *** |
| *Medium*($\pi_H$)*MEP* | -1.49 (0.21) *** | -1.71 (0.43) *** |
| *Medium*($\pi_L$)*MEP* | -1.77 (0.21) *** | -2.00 (0.42) *** |
| *PriceHat* | 0.10 (0.03) *** | 0.11 (0.11) |
| *PriceHat*×*NetSeller* | | 0.01 (0.11) |
| *PriceHat*×*HighMAC* | | -0.04 (0.11) |
| *HighMAC*×*HighMEP* | | 0.05 (0.59) |
| *HighMAC*×*Medium*($\pi_H$)*MEP* | | 0.01 (0.46) |
| *HighMAC*×*Medium*($\pi_L$)*MEP* | | -0.01 (0.45) |
| *NetSeller*×*HighMEP* | | 1.00 (0.59) * |
| *NetSeller*×*Medium*($\pi_H$)*MEP* | | 0.43 (0.46) |
| *NetSeller*×*Medium*($\pi_L$)*MEP* | | 0.48 (0.45) |
| Wald $\chi^2$ | 211.9*** | 221.5*** |

3168 observations. Standard error in parenthesis. * $p < 0.10$; *** $p < 0.01$

**Table 6 – Marginal Productivity of Enforcement by Firm Type (Emissions Trading)**

| | **Enforcement Change** | ***HighMAC Firms*** | ***LowMAC Firms*** | **Test of Equality ($p$-value)** |
|---|---|---|---|---|
| ***Low Standard Uniform*** | *Low* to *Medium*($\pi_L$) | -1.66 | -0.93 | 0.73 |
| | *Low* to *Medium*($\pi_H$) | -1.54 | -1.02 | 0.58 |
| | *Low* to *High* | -3.08 | -1.89 | 0.38 |
| | *Medium*($\pi_L$)to *High* | -1.43 | -0.95 | 0.32 |
| | *Medium*($\pi_H$) to *High* | -1.54 | -0.87 | 0.16 |
| ***High Standard Uniform*** | *Low* to *Medium*($\pi_L$) | -1.79 | -1.16 | 0.59 |
| | *Low* to *Medium*($\pi_H$) | -1.45 | -0.87 | 0.68 |
| | *Low* to *High* | -3.15 | -1.99 | 0.32 |
| | *Medium*($\pi_L$)to *High* | -1.35 | -0.83 | 0.27 |
| | *Medium*($\pi_H$) to *High* | -1.70 | -1.12 | 0.23 |
| ***High Standard Nonuniform*** | *Low* to *Medium*($\pi_L$) | -1.42 | -1.87 | 0.56 |
| | *Low* to *Medium*($\pi_H$) | -1.11 | -1.52 | 0.63 |
| | *Low* to *High* | -2.13 | -3.10 | 0.63 |
| | *Medium*($\pi_L$)to *High* | -0.71 | -1.24 | 0.43 |
| | *Medium*($\pi_H$) to *High* | -1.03 | -1.59 | 0.40 |

**Table 7. Summary Statistics for Individual Violations (Fixed Standards)**

| | Marginal Expected Penalty | Competitive Equilibrium | Firm MAC | Mean | Median | Standard Deviation |
|---|---|---|---|---|---|---|
| Low Standard | *High* | 1 | High | 0.6 | 1 | 1.0 |
| | | 0 | Low | 0.1 | 0 | 0.3 |
| | *Medium*($\pi_H$) | 5 | High | 4.1 | 4 | 2.1 |
| | | 1 | Low | 1.2 | 1 | 0.6 |
| | *Medium*($\pi_L$) | 5 | High | 3.0 | 2 | 3.6 |
| | | 1 | Low | 1.4 | 1 | 1.5 |
| | *Low* | 7 | High | 5.0 | 4 | 2.2 |
| | | 3 | Low | 2.1 | 2 | 0.8 |
| High Standard | *High* | 0 | High | 0.6 | 0 | 1.2 |
| | | 0 | Low | 0.0 | 0 | 0.2 |
| | *Medium*($\pi_H$) | 3 | High | 2.7 | 3 | 0.8 |
| | | 0 | Low | 0.1 | 0 | 0.3 |
| | *Medium*($\pi_L$) | 3 | High | 2.2 | 1.0 | 2.5 |
| | | 0 | Low | 0.3 | 0 | 0.5 |
| | *Low* | 5 | High | 3.6 | 3 | 2.1 |
| | | 1 | Low | 0.5 | 0 | 0.5 |

**Table 8. Random Effects Estimation of Individual Violations (Fixed Standards)**

| Variable | Model 1 | Model 2 |
|---|---|---|
| *Intercept* | 2.27 (0.24)*** | 2.02 (0.31) *** |
| *HighMAC* | 1.99 (0.20) *** | 3.04 (0.36) *** |
| *HighMEP* | -2.47 (0.28) *** | -1.93 (0.48) *** |
| *Medium*($\pi_H$)*MEP* | -0.79 (0.28) *** | -0.84 (0.48) * |
| *Medium*($\pi_L$)*MEP* | -1.07 (0.28) *** | -0.59 (0.48) |
| *HighStandard* | -0.94 (0.20) *** | -1.51 (0.36) *** |
| | | |
| *HighMAC*× *HighMEP* | | -2.57 (0.62) *** |
| *HighMAC*× *Medium*($\pi_H$)*MEP* | | -0.17 (0.62) |
| *HighMAC*× *Medium*($\pi_L$)*MEP* | | -1.47 (0.62) ** |
| *HighStandard*× *HighMEP* | | 1.44 (0.62) ** |
| *HighStandard* × *Medium*($\pi_H$)*MEP* | | 0.45 (0.62) |
| *HighStandard*× *Medium*($\pi_L$)*MEP* | | 0.38 (0.62) |
| *HighStandard*× *HighMEP* × *HighMAC* | | 0.07 (0.72) |
| *HighStandard*× *Medium*($\pi_H$)*MEP* × *HighMAC* | | -0.35 (0.72) |
| *HighStandard*× *Medium*($\pi_L$)*MEP*× *HighMAC* | | 0.28 (0.73) |
| Wald $\chi^2$ | 211.9 *** | 287.1 *** |

1337 observations. Standard error in parenthesis. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

**Table 9 – Marginal Productivity of Enforcement by Firm Type (Fixed Standards)**

| | Enforcement Change | ***HighMAC Firms*** | ***LowMAC Firms*** | **Test of equality (*p*-value)** |
|---|---|---|---|---|
| ***LowStandard*** | *Low* to *Medium*($\pi_L$) | -2.07 | -0.60 | 0.02 |
| | *Low* to *Medium*($\pi_H$) | -1.01 | -0.84 | 0.78 |
| | *Low* to *High* | -4.50 | -1.93 | 0.00 |
| | *Medium*($\pi_L$)to *High* | -2.43 | -1.33 | 0.13 |
| | *Medium*($\pi_H$) to *High* | -3.49 | -1.10 | 0.00 |
| ***HighStandard*** | *Low* to *Medium*($\pi_L$) | -1.41 | -0.22 | 0.06 |
| | *Low* to *Medium*($\pi_H$) | -0.91 | -0.39 | 0.41 |
| | *Low* to *High* | -2.98 | -0.49 | 0.00 |
| | *Medium*($\pi_L$) to *High* | -1.57 | -0.26 | 0.07 |
| | *Medium*($\pi_H$) to *High* | -2.07 | -0.10 | 0.01 |

**Appendix: Instructions Summary**[12]

Thank you for agreeing to participate in today's experiment. You have all seen a version of this experiment before. Before we begin, I would like to review the instructions for today's experiment.

It is very important to remember that although the experiment may be similar, some or all of the numbers may have changed. Do NOT assume that any of the information or results from a previous experiment will be useful in helping you to make your decisions today.

The purpose of the experiment is to give you an opportunity to earn as much money as possible. What you earn will depend on your decisions, as well as the decisions of others. As before you can produce as many units as you want regardless of the number of permits you own, but you could face a financial penalty if you do not own a permit for each unit you produce.

- During the period, you can earn money in two ways:
  1. Produce units of the fictitious good. For each unit you produce, you will earn a specified amount of money that will be added to your cash balance.
  2. Sell permits in the permit market. The selling price you receive for a permit will be added to your cash balance.
- Money will be subtracted from your cash balance if:
  1. You choose to buy additional permits. The purchase price you pay will be deducted from your cash balance.
  2. You are audited and if the total number of units you produce exceeds the number of permits you own.

*Production Highlights*

- Your Earnings from Production table tells you how many units you can produce and how much you will earn from each unit you produce. You might earn a different amount of money for each unit produced.
- Production of each unit takes a specified amount of time
- You can only produce one unit at a time.

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[12] This instructions summary was given to students and read aloud by the experimenter before each session. During the trainers, subjects read a more detailed set of online instructions. The text of the detailed instructions is available from the authors upon request.

- The Production Timer tells you how much time is left for you to produce more units.
- In order to start production of a unit, there must be sufficient time on the Production Timer to complete production of the unit.
- To start production or to place an order for additional units, click the plus (+) button. If production is idle, then production will begin immediately.
- You can cancel units that have been ordered if production has not yet begun. To do so, click the minus (–) button.
- Earnings from the units produced are automatically added to your cash balance when production is completed.
- The last row of the "Earnings from Production" table tells you the maximum number of units you are able to produce.
- Under the "Earnings from Production" table, you can see the production status of each unit (produced, in production, or planned).

*Permit Market Highlights*

- You will be given an opportunity to buy and/or sell permits in the Permit Market.
- There are 4 ways in which you can participate in the market:

1. Make an offer to buy a permit.
    a. To do so, enter your price next to the My Buying Price and click Buy.
    b. All buying prices must be GREATER than the Current Buying Price.
2. Make an offer to sell a permit.
    a. To do so, enter your price next to the My Selling Price and click Sell.
    b. All selling prices must be LOWER than the Current Selling Price.
3. Purchase a permit at the Current Selling Price.
    a. To do so, enter the Current Selling Price next to My Buying Price
    b. or click the Buy button next to the Current Selling Price.
4. Sell a permit at the Current Buying Price.
    a. To do so, enter the Current Buying Price next to My Selling Price
    b. or click the Sell button next to the Current Buying Price.

- After each trade is completed, your permit balance will be automatically updated. Your cash balance will automatically be updated to reflect price you paid to buy the permit, or the price you received for selling the permit. This is shown in the My Balances section of your screen.

*Auditing Highlights*

- The computer monitor always knows how many permits you own and your cash balance. The computer does not know how many units you actually produced unless you are audited.
- There is an XX% chance that you will be audited, and (1-XX)% chance you will not be audited.
- If you are audited, the computer monitor will check to see how many units you actually produced. If the number of units you produced exceeds the number of permits you own, you will receive a financial penalty. The Permit Shortfall Table lists the penalties you will face.

To summarize, your total earnings for the period will be calculated as follows:

| | |
|---|---|
| | Your initial cash balance |
| + | Earnings from production of the good |
| + | Selling price for permits you sell in the permit market |
| – | Purchase price for permits you buy in the permit market |
| – | Penalties for a permit shortfall (only if you are audited and if you over produced) |
| = | Total earnings for the period |

At the end of the experiment, we will add up your total earnings for each period and you will be paid in cash for these earnings. Please raise your hand if you have any questions.