Journal of Economic Behavior & Organization
Vol. 61 (2006) 217–233

JOURNAL OF
Economic Behavior
& Organization

www.elsevier.com/locate/econbase

Direct and market effects of enforcing emissions trading programs: An experimental analysis

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Received 3 June 2004; received in revised form 12 January 2005; accepted 1 March 2005
Available online 15 May 2006

Abstract

Since firms in an emissions trading program are linked through the permit market, so too are their compliance choices. Thus, enforcement strategies for trading programs must account for the direct effects of enforcement on compliance and emissions decisions as well as the indirect effects that occur due to changes in permit prices. Our experimental results are consistent with theoretical predictions about both a negative direct effect of enforcement on individual violations and a countervailing market effect through the permit price. Furthermore, there is no direct effect of enforcement on the emissions choices of firms, only a negative price effect.

JEL classification: C91; L51; Q58

Keywords: Compliance; Enforcement; Experiments; Permit markets; Emissions trading

1. Introduction

By exploiting the power of a market to allocate pollution control responsibilities, well-designed emissions trading programs promise to achieve environmental quality goals more cheaply than traditional command-and-control regulations. It is clear, however, that the potential of emissions trading is jeopardized if these programs are not enforced well. In recognition of this fact, there is

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0167-2681/$ – see front matter
doi:10.1016/j.jebo.2005.03.013

now a significant literature on compliance and enforcement of emissions trading programs (e.g., Keeler, 1991; Malik, 1990, 1992, 2002; van Egteren and Weber, 1996; Stranlund and Dhanda, 1999; Stranlund and Chavez, 2000). In general, this literature suggests that compliance behavior in emissions trading programs is likely to be very different from behavior under command-and-control standards or fixed emissions taxes. One of the more important differences is that firms in an emissions trading program are linked together through the permit market while they operate largely independently under both command-and-control policies and emissions taxes. Thus, compliance and enforcement of emissions trading programs are inextricably linked to permit markets. Indeed, any factor that affects compliance decisions will in turn impact the permit market, which has its own indirect effect on compliance via the permit price.

For this study we have designed and conducted laboratory experiments to examine the direct and indirect market effects of enforcement on pollution and compliance decisions. Our hypotheses about these effects are derived from the simplest possible model of imperfect compliance in an emissions trading program. Our goal is to provide empirical tests of several fundamental results from the existing theory. A theoretically sound and empirically validated understanding of such fundamentals is critical for the appropriate design and implementation of enforcement strategies for market-based policies, and provides a baseline for theoretical and empirical extensions into more complicated environments.

Most of our hypotheses are supported by the experimental data. One of the most important of these is that there is a direct effect of enforcement on individual violations as well as a countervailing market effect through the permit price. Increased enforcement through increased monitoring or higher penalties motivates firms to reduce their violations by purchasing more permits. This puts upward pressure on the equilibrium permit price, but higher permit prices motivate firms toward greater violations. Our experimental data are consistent with the theoretical prediction that the direct effect is always larger so that increased enforcement results in lower violations. However, the basic conclusion in this regard should be clear: the productivity of enforcement pressure in reducing noncompliance in emissions trading programs is partially offset by a countervailing price effect. Regulators who ignore this price effect would over-estimate the effectiveness of any attempt to reduce violations.[1]

The experimental results also provide strong support for a somewhat surprising result about enforcement and emissions choices: there is no direct effect of enforcement on the emissions choices of firms; there is only a negative price effect.[2] That is, a firm's choice of emissions is independent of the enforcement strategy it faces, but this choice is not independent of the price of permits. An important implication of this conclusion is that the only way that increased enforcement can have an impact on environmental quality is if it is large enough and applied widely enough to lead to an increase in the equilibrium permit price. Increased enforcement pressure applied to a single firm or a small subset of firms will have *no* environmental impact.

[1] Another policy implication of this indirect price effect is that directing more enforcement pressure at a subset of firms, presumably to motivate them toward greater compliance, may involve a cost that regulators may not have recognized. The firms that are targeted with more enforcement pressure will purchase more permits to reduce their violations, thereby putting upward pressure on the equilibrium permit price. A higher permit price, however, motivates all the other firms in the program toward larger violations. Therefore, targeting groups of firms to increase their compliance may be accompanied by reduced compliance by other firms. This cannot happen under command-and-control regulation or a fixed emissions tax because firms under these regulations are not linked together through a permit market.

[2] Malik (1990) shows that this result obtains when audits of firms' emissions are random. Harford (1978) noted a similar result in the case of an emissions tax.

Matters are quite different for emissions standards and taxes. Under fixed emissions standards, adjusting emissions levels is the only way a firm can change its level of noncompliance. Thus, increased enforcement of emissions standards will reduce emissions and improve environmental quality. In the case of a fixed emissions tax, however, increased enforcement will have absolutely no effect on emissions. In this case, as in the case of competitive emissions trading, firms' emissions choices are independent of changes in enforcement strategies. In contrast to emissions trading, however, the "price" of emissions is fixed, so the indirect effect on emissions from enforcement cannot occur.

Although this work was motivated primarily by our desire to trace out the direct and market effects of enforcement, we did discover another effect that contradicts a standard theory of compliance behavior. Compliance choices by risk-neutral competitive firms in emissions trading programs should be independent of the initial allocation of permits. This is consistent with the well-known result that the emissions choices of perfectly competitive firms in emissions trading program are independent of initial allocations (Montgomery, 1972).[3] Our results contradict both of these conclusions. What appears to matter most here is how the initial allocation of permits determines who will be net sellers of permits and who will be net buyers. Our analysis suggests that net sellers tend to retain more permits and have lower violations and higher emissions than the competitive equilibrium prediction, while net buyers hold fewer permits and tend toward higher violations and lower emissions. Since fewer permits change hands, permit prices tend to be higher than competitive equilibrium predictions.

Although experimental techniques have been used to evaluate many other policy initiatives, including some aspects of emissions trading programs (e.g., Cason, 1995; Cason and Plott, 1996; 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.[4] We know of only one other paper that examines emission permit markets when compliance may be imperfect. Cason and Gangadharan's (this issue) experiments involve permit trading when emissions are stochastic, permits can be banked, enforcement is incomplete, and subjects' performance is audited based on their past compliance history. With this complicated design, they are able to identify interesting interactions between random emissions shocks, permit banking and compliance. Our approach is much simpler: emissions are deterministic, banking is not permitted, and audits are random with a known and constant probability. This approach allows us to generate fundamental results about the direct and indirect effects of different enforcement strategies on compliance and emissions that Cason and Gangadharan do not address.

The results of this paper make it clear that the compliance behavior of firms is linked together in emissions trading programs through the normal workings of permit markets. We provide a model of these linkages in the next section. In Section 3 we provide details of the experiments we designed to test for these linkages. The results of the experiments are presented and discussed in Section 4. We conclude in Section 5.

[3] It is well known that Montgomery's independence 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).

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

2. The direct and indirect effects of enforcement: theory and hypotheses

In this section we quickly derive our hypotheses about the direct and indirect effects of enforcement on emissions and violations in an emissions trading program. None of the hypotheses are new: all have either been presented elsewhere in the literature or are easily deduced from existing models (e.g., Malik, 1990; Stranlund and Dhanda, 1999).

Consider a fixed number of risk neutral firms in a perfectly competitive emissions trading program. The gross profit of firm i is $b(q^i, \theta^i)$, which is strictly concave in the firm's emissions q^i.[5] Firm heterogeneity is captured by the parameter θ^i and we assume that total and marginal profits 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. Net expenditure or revenue from trading in the permit market is $p(l^i - l_0^i)$.

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 probability π. 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 violations, the penalty is positive, strictly increasing and strictly convex. An increase in the parameter ϕ increases both total and marginal penalties.

In this study we focus on imperfect compliance in emissions trading programs.[6] Therefore, a noncompliant firm's choice of emissions and permits maximizes $b(q^i, \theta^i) - p(l^i - l_0^i) - \pi f(q^i - l^i, \phi)$, yielding the following first-order conditions:

$$b_q(q^i, \theta^i) - \pi f_v(q^i - l^i, \phi) = 0, \tag{1}$$

$$-p + \pi f_v(q^i - l^i, \phi) = 0. \tag{2}$$

Eqs. (1) and (2) are necessary and sufficient to determine a noncompliant firm's optimal choices of emissions, permit demand, and the resulting violation level. Note that these choices do not depend on a firm's initial allocation of permits, l_0^i.[7]

The signs of the comparative statics of the firm's choices are contained in Table 1.[8] These results have rather intuitive explanations. From (1), note that a noncompliant firm chooses emissions so that its marginal profit from increased emissions is equal to the marginal expected penalty. From (2), the firm chooses its permit demand so that the permit price is equal to the marginal expected

[5] See Montgomery for a demonstration of the concavity of profit in emissions for firms that are price-takers in input and output markets. Since the formulation of $b(q^i, \theta^i)$ is quite general, strict concavity can be guaranteed in many non-competitive settings as well.

[6] Some well-known programs have been very successful in maintaining nearly perfect compliance, for example, the SO_2 Allowance Trading program. It is widely understood that the major contributors to the compliance success of this and other similar programs are the continuous emissions monitoring systems and sophisticated data transmission technologies that are required of all sources. Implementing emissions trading policies beyond their current applications implies moving them into contexts in which inducing and maintaining perfect compliance will be more difficult. These more difficult environments motivate our focus on imperfect compliance behavior.

[7] This model can easily be applied to other tradable property rights programs. For example, recent theoretical papers by Hatcher (2005) and Chavez and Salgado (2005) are direct applications of the literature on compliance and enforcement of emissions trading to individual transferable fishing quotas.

[8] Complete derivations of these results are available from the authors. Alternatively, see Stranlund and Dhanda for essentially the same table.

Table 1
Comparative statics for direct effects on an individual firm's choices

	Choice		
	Emissions	Permits	Violations
Parameter			
π	$q^i_\pi = 0$	$l^i_\pi > 0$	$v^i_\pi < 0$
ϕ	$q^i_\phi = 0$	$l^i_\phi > 0$	$v^i_\phi < 0$
p	$q^i_p < 0$	$l^i_p < 0$	$v^i_p > 0$
θ^i	$q^i_\theta > 0$	$l^i_\theta > 0$	$v^i_\theta = 0$

penalty. Therefore, we have $b_q(q^i, \theta^i) = \pi f_v(q^i - l^i, \phi) = p$. Holding the permit price constant, a firm responds to increased enforcement (either increased monitoring or increased penalties) by reducing the size of its violation $[v^i_\pi < 0, v^i_\phi < 0]$ to keep the marginal expected penalty equal to the permit price. This is the *direct* effect of increased enforcement on violations. Since the marginal expected penalty remains equal to the permit price, the firm does not change its emissions $[q^i_\pi = q^i_\phi = 0]$ as a *direct* response to a change in enforcement pressure. This implies that a firm reduces its violations solely by purchasing more permits $[l^i_\pi > 0, l^i_\phi > 0]$ and not by changing its emissions.

Since increased enforcement increases the demand for permits, the price of permits will increase. To demonstrate this, 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 that the enforcement authority has committed itself to monitoring each firm with probability π and imposing penalties with parameter ϕ, the equilibrium permit price is $\bar{p} = \bar{p}(\pi, \phi, Q, \theta)$, which is the implicit solution to the permit market clearing condition, $\sum l^i(\pi, \phi, p, \theta^i) = Q$. (Summations are taken over the entire set of regulated firms.) In the usual fashion obtain[9]:

$$\bar{p}_\pi = -\frac{\sum l^i_\pi}{\sum l^i_p} > 0 \quad \text{and} \quad \bar{p}_\phi = -\frac{\sum l^i_\phi}{\sum l^i_p} > 0. \tag{3}$$

The signs of $\bar{p}_\pi$ and $\bar{p}_\phi$ follow from $l^i_\pi > 0$ and $l^i_p < 0$ (see Table 1). Because increased monitoring or penalties motivate noncompliant firms to purchase more permits, the equilibrium permit price increases.

The direct and indirect market effects of enforcement on a firm's emissions and violations can be determined from the equilibrium values of these choices:

$$\bar{q}^i = \bar{q}^i(\pi, \phi, \bar{p}(\pi, \phi, Q, \theta), \theta^i), \qquad \bar{v}^i = \bar{v}^i(\pi, \phi, \bar{p}(\pi, \phi, Q, \theta), \theta^i). \tag{4}$$

From (4) obtain $\bar{q}^i_\pi = q^i_\pi + q^i_p \bar{p}_\pi$. Notice the potential for a direct effect of monitoring on emissions and an indirect effect through the permit price. However, recall that the direct effect is zero $[q^i_\pi = 0$ from Table 1]. Therefore, $\bar{q}^i_\pi = q^i_p \bar{p}_\pi < 0$, indicating that firms are motivated to reduce their emissions because increased monitoring puts upward pressure on the equilibrium permit price and they respond to this by decreasing their emissions $[q^i_p < 0]$. The same conclusion follows if there is a change in the penalty for noncompliance through a change in the parameter θ.

[9] Substitute the equilibrium permit price into the market clearing condition to obtain an identity. Differentiate this identity with respect to the enforcement parameters and rearrange the results to obtain (3).

Since aggregate emissions must fall with increased monitoring or penalties, given a fixed permit supply, aggregate violations must also fall. This would not be completely obvious by considering how individual equilibrium violations change with monitoring or penalties. From (4) obtain $\bar{v}^i_\pi = v^i_\pi + v^i_p \bar{p}_\pi < 0$. Again, the direct effect of monitoring is to reduce the violation of an individual firm [$v^i_\pi < 0$]. However, increased monitoring increases the equilibrium permit price, and firms respond to this by increasing their violations [$v^i_p > 0$] because the price of compliance is higher. Since aggregate violations must fall with increased monitoring, each individual's violation must also fall. Thus, the direct effect of monitoring outweighs the indirect price effect. Increasing the penalty has the same qualitative effects as an increase in monitoring.

Our results about the equilibrium effects of enforcement on emissions and violation choices are summarized in the following hypotheses.

Hypothesis 1. The equilibrium effect of monitoring and penalties on individual violations is composed of a negative direct effect and a positive indirect market effect via the permit price. The direct effect is stronger than the price effect, so the total effect of increased monitoring or penalties is to reduce equilibrium violations.

Hypothesis 2. The direct effect of monitoring and penalties on individual emissions is zero, but the indirect market effect is negative.

We should also formalize the hypothesis that the initial distribution of permits should have no impact on equilibrium outcomes.

Hypothesis 3. A redistribution of the initial allocation of permits has no effect on firms' choices of emissions and violations. Consequently, the equilibrium permit price is also independent of a redistribution of the initial allocation of permits.

Finally we would like to point out a result that is not central to the main focus of our work but that nevertheless plays an important role in the analysis of compliance behavior in emissions trading. Stranlund and Dhanda show that differences in firms' violations in a competitive emissions trading program are independent of the differences in their marginal benefits from increased emissions [$v^i_\theta = 0$ in Table 1]. This counterintuitive result follows from the conclusion that firms choose their violations to equate the marginal expected penalty to the going permit price and that nothing unique to a firm is part of this decision rule.[10] Therefore, we have

Hypothesis 4. Individual violations are independent of differences in firms' marginal benefits of increased emissions.

3. Experimental design and procedures

3.1. Experiment design

The experiments were designed to test our hypotheses about direct and indirect market effects on individual emissions and violations, 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

[10] In contrast, a firm that faces a fixed emissions standards will tend toward higher violations if its marginal benefits from emissions are higher because higher marginal benefits from emissions imply higher marginal benefits of noncompliance (Garvie and Keeler, 1994).

Table 2
Experimental design

Marginal expected penalty	Allocation
Medium(π_H)	Low standard, uniform
Medium(π_L)	High standard, uniform
Low	High standard, non-uniform

The nine treatments were constructed by interacting the three marginal expected penalties with the three allocations. Each treatment was repeated three times with eight participants per group.

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.[11]

Subjects received a benefit from their choice of production, q, according to an "Earnings from Production" schedule, 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 firm types. Subjects were randomly assigned a firm type. High marginal benefit (High MB) firms had higher production benefits ($\beta_H = 1$), and could produce up to 17 units. Low MB firms had lower production benefits ($\beta_L = 2$), and could produce up to eight units. Production, q, was constrained to be a whole number.

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 2 summarizes the experimental design. Each of the nine treatments was repeated three times.

To be compliant, 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 ($Q_L = 28$), each of the four High MB firms was allocated three permits, and the four Low MB firms were each given four permits. We call this the (nearly) uniform initial allocation. In the high aggregate standard experiments ($Q_H = 56$), 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 non-uniform initial allocation, the High MB firms began with 13 permits, and the Low MB firms had a single permit. Reallocating the initial allocation of permits in this way changes the prediction of which firms would sell permits and which would buy permits. In the competitive equilibrium, the Low MB firms would be the net sellers of permits with the uniform initial allocation and net buyers of permits when the initial allocation is non-uniform.

To check for compliance, each subject's record was examined with probability π. If a subject was audited and found to be non-compliant, that is $q > l$, then she was penalized according to a penalty schedule generated from the quadratic function, $f = F(q - l) + (\phi/2)(q - l)^2$, where F and ϕ are positive constants. Note that the penalty function is strictly convex, so that each additional unit of violation brings a higher marginal penalty. By changing the parameters of the marginal expected

[11] Consistent with most market experiments, subjects knew their own circumstances (e.g., parameters, decisions, outcomes) but knew nothing about those of the other participants.

penalty function, $\pi f' = \pi[F + \phi(q - l)]$, we developed three enforcement strategies, which we label Medium(π_H), Medium(π_L), and Low in Table 2.

The treatments Medium(π_H) and Medium(π_L) involve the same marginal expected penalties, but Medium(π_H) has a higher monitoring probability ($\pi_H = 0.70$) and a relatively low marginal penalty function, whereas Medium(π_L) has a lower monitoring probability ($\pi_L = 0.35$) and a higher marginal penalty function.[12] Our intention here was to examine whether the subjects reacted differently to monitoring and penalties. The Low treatment was constructed to be a weaker enforcement strategy with the low monitoring probability, $\pi_L = 0.35$, and a low marginal penalty function. 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). Subjects were expected to choose to be noncompliant in all treatments.

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 \$6.83 and \$17.50, with a mean of \$13.57 ($\sigma = 1.49$). Earnings were paid in cash at the end of each experiment. Each experiment lasted about two 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 138 subjects participated in 27 eight-person experiments. Prior to the start of the real data experiments, subjects were given a summary of the experiment instructions.[13] 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 the possibility that the production level 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

[12] The subjects were given penalty schedules that were generated from the marginal penalty function $f' = F + \phi(q - l)$. The parameters of the penalty schedule (F, ϕ) for each marginal expected penalty treatment are ($F = 6$, $\phi = 1.43$) for Medium(π_H), (12, 2.90) for Medium(π_L), and (2, 2.90) for Low. The schedule was the same for each firm type with the exception that, since Low MB firms could produce a maximum of only eight units, only the first eight steps were displayed for these firms.

[13] The instructions summary is available at: http://www.umass.edu/resec/faculty/murphy/instructions/jebo_instructions.pdf.

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.

This design yields observations on individual decisions about production, violations and permit holdings, as well as market outcomes, particularly with respect to permit prices. Varying the enforcement parameters allows us to test our hypotheses about the direct and indirect effects of changes in enforcement (Hypotheses 1 and 2). The change in initial allocation from uniform to non-uniform provides the basis for testing whether individual choices and market outcomes are independent of the initial allocation of permits (Hypothesis 3). Finally, giving subjects different production benefit schedules allows us to test whether violations are independent of this difference (Hypothesis 4).

4. Results

We begin our analysis of the results of our experiments with a discussion of general patterns in the permit price and of individual violations and emissions decisions.[14] 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 and hypotheses suggest that an individual firm's emission and violation decisions are conditioned on the permit price, we first estimate this price and then use the estimated price as an instrumental variable when estimating emissions and violation choices. Because these are 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 a uniform allocation. These experiments were identical to those described in this paper except that emissions were assumed to equal the final permit balance exactly; that is, noncompliance was not allowed. 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.[15]

4.1. General patterns

Table 3 presents some summary statistics of permit prices. Note that the average price in each treatment tends to be higher than the competitive equilibrium prediction but moves as expected:

[14] We remind the reader that the experiments were framed as a production decision, rather than emissions, to avoid introducing any biases. In this section, we will refer to an emissions decision since that was the initial motivation for the research.

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

Table 3
Permit price summary statistics

Marginal expected penalty	Competitive equilibrium	Mean	Median	Standard deviation
Low standard				
Medium(π_H)	8–8.20	9.61	9.30	1.18
Medium(π_L)		13.26	13.50	1.84
Low	6	8.11	7.90	1.55
High standard				
Medium(π_H)	6–6.20	7.09	7.25	1.02
Medium(π_H) non-uniform		7.79	7.60	1.10
Medium(π_L)		6.74	6.85	0.58
Medium(π_L) non-uniform		7.24	7.20	1.39
Low	4	3.97	4.00	0.74
Low non-uniform		6.50	7.00	1.36

prices are higher when the supply of permits is reduced and when the marginal expected penalty is increased from low to medium. This latter result suggests that enforcement could have an indirect price effect on individual behavior. Note however that with the high standard, for a given marginal expected penalty, average prices are higher when the initial allocation of permits is not uniform. Thus, our theoretical expectation that the initial allocation of permits should have no impact on individual choices and market outcomes (Hypothesis 3) appears to be in doubt.

Tables 4 and 5 present some summary statistics for individual violations and emissions. Rather than discuss all the relationships that are apparent in these data, we leave that for the econometric analysis. However, an interesting pattern emerges from these summary statistics that plays an important role in how we analyze and interpret individual choices of emissions and compliance. In Table 4 note that mean and median violation levels clearly differ by firm type. This appears to refute our hypothesis that individual violations should be independent of differences in firms' marginal benefits from emissions (Hypothesis 4). However, note that whether High MB firms tend toward higher or lower violations than Low MB firms depends on the initial allocation of permits. In particular, consider the six uniform allocation treatments. The High MB firms are predicted to be the net buyers of permits in these treatments, and they clearly tend toward higher violations than the Low MB firms. On the other hand, for the three non-uniform allocation treatments, the Low MB firms are predicted to be the permit buyers, and they are the ones that tend toward higher violations. It appears, therefore, that the differences in violations by firm type may not have as much to do with the difference in their benefits from emissions, but rather whether the initial allocation of permits makes them net buyers or sellers of permits. Of course, this speculation is easily tested and we will do so shortly.

As with violations, mean emissions also show a consistent pattern. In Table 5, for the six treatments with a uniform allocation, the High MB firms are predicted to be net buyers of permits, and their emissions are significantly lower than the competitive equilibrium prediction. On the other hand, for the three non-uniform allocation treatments in which High MB firms are predicted to be net sellers of permits, their emissions are slightly higher than predicted, although in all three cases this difference is not statistically significant. The opposite is true for Low MB firms. In the treatments involving a uniform allocation of permits, the Low MB firms are predicted to be net sellers of permits, and their average emissions are consistently greater than the competitive

Table 4
Summary statistics for individual violations

Marginal expected penalty	Competitive equilibrium	Firm MB	Mean	Median	Standard deviation
Low standard					
Medium(π_H)	3	High	3.05	3	0.98
		Low	1.98	2	1.45
Medium(π_L)		High	1.86	1	1.99
		Low	1.20	1	1.40
Low	4 or 5	High	3.66	3	1.95
		Low	3.26	3	1.54
High standard					
Medium(π_H)	1	High	1.17	1	1.10
		Low	0.73	0	1.30
Medium(π_H) non-uniform		High	0.63	0	1.24
		Low	1.35	1	1.20
Medium(π_L)		High	1.42	1	1.34
		Low	0.80	0	1.46
Medium(π_L) non-uniform		High	0.77	0	2.26
		Low	1.48	1	1.71
Low	2 or 3	High	3.61	3	2.38
		Low	1.41	1	1.31
Low non-uniform		High	1.76	1	2.00
		Low	2.81	2	2.04

equilibrium. The opposite tends to be true when they are predicted to be net buyers of permits in the three non-uniform allocation treatments.

Thus, it appears that net sellers of permits tend toward higher emissions and lower violations. This implies that net sellers of permits are also inclined to retain more permits than predicted. Consequently, fewer permits change hands than predicted, which is consistent with our observation that average permit prices are higher than the competitive equilibrium predictions.

4.2. Regression results and tests of hypotheses

Table 6 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 (Low versus Medium), the aggregate standard (Low versus High), and the initial allocation (Uniform versus NonUniform) are modeled as fixed effects. Note that we have combined the two medium marginal expected penalties. Assuming risk-neutral subjects, since the Medium(π_H) and Medium(π_L) enforcement strategies have the same marginal expected penalties, in theory both should lead to identical market outcomes. We tested a model of price with separate dummy variables for these two treatments. An F-test of the null hypothesis that the coefficients on the dummy variables for the Medium(π_H) and Medium(π_L) treatments are equal cannot be rejected at any conventional level of significance ($F = 1.55$, $p = 0.21$). We conducted the same exercise for individual emissions and violation decisions and found the same result. Thus, decreasing

Table 5
Summary statistics for individual emissions

Marginal expected penalty	Competitive equilibrium	Firm MB	Mean	Median	Standard deviation
Low standard					
Medium(π_H)	9	High	7.34	7	1.19
	4	Low	4.69	5	0.96
Medium(π_L)	9	High	5.77	5	2.00
	4	Low	4.23	4	1.29
Low	11	High	8.33	8	1.69
	5	Low	5.58	6	0.72
High standard					
Medium(π_H)	11	High	10.37	10	1.09
	5	Low	5.53	5	0.96
Medium(π_H) non-uniform	11	High	11.35	10	2.54
	5	Low	4.63	5	0.74
Medium(π_L)	11	High	10.59	11	1.28
	5	Low	5.63	5	0.88
Medium(π_L) non-uniform	11	High	11.15	11	1.42
	5	Low	5.09	5	1.18
Low	13	High	11.84	12	1.71
	6	Low	7.18	7	0.71
Low non-uniform	13	High	13.12	13	1.53
	6	Low	5.45	5	1.19

monitoring and increasing penalties but leaving the marginal expected penalty function unchanged had no effect on individual decisions and market outcomes.

The price estimation results in Table 6 confirm the impressions we reached by comparing average prices across treatments. The coefficient on *MediumMEP* indicates that increased enforcement due to either a higher penalty or a higher monitoring probability puts upward pressure on the equilibrium price. In a moment we will show that this shift in price will have an indirect effect on individual emissions and violations. The *NonUniform* coefficient is positive and weakly significant, which contradicts Hypothesis 3, as we expected from our perusal of the average price

Table 6
Random effects estimation of permit price

Variable	Permit price		
Intercept	8.26	(0.56)	**
MediumMEP	2.56	(0.53)	**
Non-uniform	1.11	(0.61)	+
High standard	−4.04	(0.61)	**
Wald χ^2 (3)		69.52	**

Three thousand and forty-four observations. Standard error in parenthesis.
** $p < 0.01$.
+ $p < 0.10$.

Table 7
Random effects estimation of individual violations and emissions

Variable	Model 1: Violations			Model 2: Emissions		
Intercept	2.44	(0.37)	**	8.18	(0.44)	**
PriceHat	0.12	(0.05)	*	−0.51	(0.06)	**
MediumMEP	−1.66	(0.27)	**	−0.09	(0.32)	
NetSeller	−0.83	(0.07)	**	1.63	(0.06)	**
HighMB	0.06	(0.07)		5.19	(0.06)	**
Wald χ^2 (4)		202.2	**		6784.5	**

Two thousand three hundred and seventy-six observations. Standard error in parenthesis.
* $p<0.05$.
** $p<0.01$.

results. Lastly, the coefficient on *HighStandard* is strongly negative and significant, indicating the unsurprising result that permit prices fall with a greater supply of permits.

Model 1 in Table 7 presents the results of a linear random effects model for individual violations. Using an instrumental variable approach, *PriceHat* is the estimated price from the model in Table 6. Since the impact of the non-uniform allocation clearly differs by firm type, depending upon whether the firm is predicted to be a net seller or buyer of permits, we capture this effect with the variable *NetSeller*, which is a fixed effect that equals one if the firm is predicted to be a net seller (High MB firms for the non-uniform allocations and Low MB firms for the uniform allocations). *HighMB* is a fixed effect that equals one for High MB firms.

From Model 1 in Table 7, note the positive and significant impact of price and the negative and significant impact of enforcement. Consistent with Hypothesis 1, increased enforcement has a negative effect on individual violations as well as a countervailing positive impact through the permit price. From the price equation in Table 6, an increase in enforcement from Low to Medium induces a \$2.56 increase in price. Combining this with the *PriceHat* coefficient of 0.12 in Table 7 yields a positive price effect of enforcement on individual violations of +0.31. The coefficient on *MediumMEP* in Table 7 indicates a direct effect on violations of −1.66. The total effect of increasing enforcement is, therefore, −1.35. As predicted, the productivity of enforcement in reducing violations is partially offset by the resulting increase in permit prices. In this case, the price effect reduces the direct effect of enforcement by 18%.

Our estimate of the equilibrium effect of increased enforcement on individual violations is only a bit smaller than the competitive equilibrium effect, but the component effects are not very close. In the competitive equilibrium, the direct effect of enforcement on violations is −3.60 and the price effect is +2.10. Compare these to our estimated effects of −1.70 and +0.31, respectively. The total effect is −1.50, which is only slightly higher than the −1.39 estimated effect. More interesting is the difference in the strength of the indirect price effect. The competitive equilibrium prediction is that the indirect price effect of enforcement reduces the direct effect by about 58%, whereas our estimate of this value is 18%.

While comparing mean and median violations across treatments, we suggested that differences in violations by firm type may have more to do with whether firms are predicted to be net buyers or sellers of permits rather than with differences in their benefits from emissions. This is confirmed by the estimation results. Note that the coefficient on *HighMB* is small and not significant, while the *NetSeller* coefficient is negative and significant. Thus, we have strong support for our hypothesis that violations are independent of differences in firms' marginal benefits of emissions (Hypothesis

4), but they are not independent of how the initial allocation of permits determines which firms will be net sellers or buyers of permits (contrary to Hypothesis 3). Indeed, those that are predicted to be net sellers have significantly lower violation levels than those that are predicted to be net buyers.

Let us now turn to the analysis of individual emissions decisions. Recall that our second hypothesis about the direct and indirect effects of enforcement is that a change in enforcement has no direct effect on emissions, only a negative price effect. Model 2 in Table 7 presents the results of linear random effects models for individual emissions. Consistent with Hypothesis 2, the effect of the marginal expected penalty is small and insignificant, while the permit price has a negative and significant effect on emissions. Thus, as predicted, the only impact of increased enforcement on emissions is through its effect on permit prices. Recall from the price results in Table 6 that increasing the marginal expected penalty leads to an increase in the permit price of \$2.56. Multiplying this by the coefficient on *PriceHat* in the emissions equation yields −1.29 as the total effect of increasing enforcement on individual emissions. This is only a bit smaller than the predicted effect of −1.58.

As with individual violations, the significant positive coefficient on the *NetSeller* variable is consistent with our suspicion that those who are predicted to be net sellers of permits tend to emit more than those who are predicted to be net buyers of permits. Lastly, note that the strongly positive coefficient on *HighMB* is consistent with the prediction that those with higher abatement costs will tend to emit more (recall $q^i_\theta > 0$ from Table 1).

5. Concluding remarks

Compliance behavior in emissions trading programs is fundamentally different from compliance behavior under other environmental policies such as emissions standards and emissions taxes. Since firms in an emissions trading program are linked together through the permit market, so too are their compliance choices. This implies that enforcement strategies for trading programs must account for the direct effects of enforcement on compliance and emissions decisions, as well as indirect effects that occur because changes in enforcement can induce changes in permit prices. These indirect market effects are not present when firms face fixed emissions standards or taxes.

The results of our laboratory experiments generally support the conclusions of a theoretical model of risk neutral behavior in emissions trading programs. The productivity of increased enforcement pressure to reduce noncompliance is partially offset by a countervailing price effect. Our estimate of the size of this offset is smaller than predicted, but nevertheless its magnitude is such that it cannot be ignored. Regulators who ignore this indirect price effect could significantly over-estimate the effectiveness of any attempt to reduce violations in an emissions trading program.

Furthermore, there is no direct effect of enforcement on individual emissions choices, only a price effect. One might reasonably expect that increased enforcement would lead to lower emissions, which we find to be true, but this occurs only if increased enforcement induces a higher permit price. Unless an increase in enforcement pressure is sufficient to affect the market price, it will have no impact on individual emission choices. Regulators should be aware that modest increases in enforcement pressure might have little or no impact on emissions levels and environmental quality.

All of our hypotheses have been confirmed by our experimental results, except one. Contrary to theoretical predictions, the initial allocation of permits has a significant impact on individual choices of violations and emissions as well as on permit prices. Those who were predicted to be net sellers of permits tend to have lower violations than those who were predicted to be net buyers of permits. However, consistent with theoretical predictions, individual violations are independent of differences in their emissions benefit functions. Thus, the only distinction across firms that drives differences in their individual violations appears to be whether they are net sellers of permits or net buyers, not differences that determine how emissions affect their profits.

The initial allocation of permits has a significant impact on individual emissions as well: net sellers of permits tend toward higher emissions while net buyers tend toward lower emissions. That net sellers have higher emissions and lower violations than in the competitive equilibrium implies that they also hold more permits. Consequently, fewer permits change hands than predicted, which is consistent with our observation that average permit prices are higher than the competitive equilibrium predictions.

This is consistent with an endowment effect that has been documented in a number of experimental settings (for a review see Kahneman et al., 1991). However, we hesitate to attribute our result to this phenomenon, because our experiments were not specifically designed to test for an endowment effect; therefore we are unable to determine whether this effect is the source of our results. 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. Interestingly, Cason and Gangadharan do not observe a similar net seller effect in their permit market compliance experiments.

Individual risk preferences are a relevant consideration in any model of compliance. While it is possible that the high permit prices (relative to predictions assuming risk-neutrality) and the mean and median tendency to over-comply suggest that risk aversion might be an important factor, we are also reluctant to attribute our results to risk aversion. The general pattern of average prices and average violation choices can be explained by other phenomenon, including some kind of "endowment effect" as discussed above. This problem and the fact that there is no consensus about how to control for individual risk preferences in experiments such as ours suggests that we cannot, with any confidence, attribute our results to a particular pattern of individual risk preferences.

In the theoretical literature on compliance and enforcement, only Malik (1990) allows for non-neutral risk preferences. He shows that emissions choices are invariant to risk attitudes. Therefore, our hypotheses concerning emissions choices (independence of enforcement and initial allocation of permits) do not depend on the assumption of risk neutrality. Unfortunately, no one, including Malik, has provided clear predictions of the qualitative impacts of enforcement on violation levels with non-neutral risk preferences. Our experience, however, 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, as in our laboratory setting.

In our case, the model with risk-neutral firms performs quite well. It provides the clear predictions about the direct and indirect market effects of enforcement that are the focus of this paper, and the results are largely (but not entirely) 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 an important area for future research.

It is clear that if emissions trading programs are to fulfill their theoretical promise of cost-effective pollution control, they must be enforced well. Designing appropriate enforcement strategies requires a comprehensive understanding of compliance behavior in these programs. The theory of compliance and enforcement of emissions trading programs is well advanced, but there are virtually no empirical analyses of the fundamental results of this literature. Further experimental analyses, similar to that contained in this paper, would help to develop a more theoretically and empirically balanced understanding of compliance behavior in emissions trading programs.

Acknowledgements

The research is funded by U.S. EPA—Science to Achieve Results (STAR) Program grant #R829608 and by the Center for Public Policy and Administration, University of Massachusetts-Amherst. This material is based upon work supported by the Cooperative State Research Extension, Education Service, U.S. Department of Agriculture, Massachusetts Agricultural Experiment Station, and the Department of Resource Economics under Project No. MAS00871. Maria Alejandra Velez, Carrie Puglisi and Maria Claudia Lopez provided outstanding research assistance. Glenn Caffery programmed the software for this project. Wendy Varner provided valuable administrative support. We are also grateful to Julie Hewitt for useful comments on an earlier draft of this work. The authors take full responsibility for any errors or omissions.

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