Journal of Economic Behavior & Organization
Vol. 61 (2006) 199–216

JOURNAL OF
Economic Behavior
& Organization

www.elsevier.com/locate/econbase

Emissions variability in tradable permit markets with imperfect enforcement and banking

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Received 18 June 2004; received in revised form 12 January 2005; accepted 23 February 2005
Available online 15 May 2006

Abstract

In this laboratory experiment on emissions trading, subjects face exogenous, random emissions shocks after making production and emission control plans. In some sessions subjects can bank their unused permits for future use. After a reconciliation-trading period following the shock realization, subjects report their emissions to the regulatory authority and are placed in different inspection groups depending on their compliance history. We identify important interactions between banking, compliance and enforcement. Banking smoothes out the price variability arising from imperfect emissions control. Price stability comes at a cost, however, since noncompliance and emissions are significantly greater when banking is allowed.

JEL classification: C91; Q20; D80

Keywords: Emissions trading; Correlated shocks; Banking; Laboratory experiments

1. Introduction

Tradable emissions permit programs are being adopted by environmental regulators in a wide variety of applications ranging from local (e.g., ground-level ozone) and regional (e.g., acid precipitation precursors) to a global scale (e.g., international trading of greenhouse gas emissions permits). These innovative programs create incentives for firms to reveal their private marginal valuation for an additional permit, which in turn reveals the firm's private marginal abatement

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0167-2681/$ – see front matter
doi:10.1016/j.jebo.2005.02.007

costs. Some of the firms may be able to switch to a pollution-decreasing technology faster or relatively cheaply compared to other firms, or they may switch to a different kind of input that generates less pollution. Through emissions trading, firms that can control pollution at a lower cost make the emissions reductions, and trading will continue until marginal abatement costs are equalized across firms. When marginal costs are equalized, in theory a given environmental standard is reached at lowest cost.[1]

Many researchers, however, have shown that to realize the maximum benefits from a tradable permit program critical attention must be given to many practical implementation details. Theoretical research has shown that important issues such as the properties and property rights of the permits being traded, the possibility of banking of permits, the transactions costs involved in trading, and the market power of some of the participants must be considered in designing an emissions trading program. Moreover, the cost savings achieved by permit trading are not likely to be realized if compliance with the program is not well-enforced.

Most emissions trading programs are either in their infancy or are still in the planning or early implementation stage. Consequently, field data to help address these issues remains scarce. To help to fill this empirical gap, in this paper we use laboratory methods to examine the interaction of three important issues facing regulators: the impact of banking, uncertainty regarding emissions, and the enforcement and compliance with regulations. While each of these design features are important in their own right, interacting them and examining the functioning of the permit market when these complications exist simultaneously creates a challenging environment that is difficult to understand using existing theoretical models.[2] We find that the relationship between emission shocks and price changes is significantly stronger without banking, so banking helps to smooth out the price variability arising from the imperfect control of emissions. This greater price stability comes at a cost, however, since noncompliance and emissions are significantly greater with banking allowed.

In some permit programs participants can bank their unused permits for future use. Banking allows efficiency gains to be achieved by equalizing (discounted) marginal abatement costs across time. The increased flexibility of banking can also potentially provide an additional source of abatement cost reduction (e.g., see Kling and Rubin, 1997). Cronshaw and Brown Kruse (1999) designed experiments to study the features of the permit market initiated under the 1990 US Clean Air Act and found that subjects were able to achieve about two-thirds of the gains theoretically available from banking alone and an additional 39–78 percent of the potential gains when trading was allowed. Godby et al.'s (1997) and Mestelman et al.'s (1999) experiments also indicate that banking helps to smooth out the prices of permits. While some emissions trading programs allow unlimited banking (e.g., the federal sulfur dioxide trading program in the United States), some do not allow any banking (e.g., the RECLAIM program in Southern California) while others

[1] One of the first and most ambitious tradable emissions schemes was the United States Federal SO_2 Trading Program (Title IV 1990 Clean Air Act Amendments), which regulates sulfur dioxide emissions that cause acid rain from electricity generation and other industrial sources. Another example is the Regional Clean Air Incentives Market (RECLAIM), established by the South Coast Air Quality Management District in Southern California, which regulates nitrogen and sulfur oxide emissions from industrial sources in the South Coast air basin. Examples of point source water quality trading schemes include the Cherry Creek Basin Trading Program for phosphorous in Colorado (US), and the Hunter River Salinity Trading Scheme, which regulates saline water exports from point sources in New South Wales, Australia. Most of these programs have reported considerable cost savings.

[2] Most models focus on only a subset of these issues; for example, Mrozek and Keeler (2004) presents a model that highlights how emissions trading improves compliance when emissions are uncertain, but in an environment with perfect enforcement and without banking.

impose restrictions on the aggregate total of permits that can be banked (e.g., the Ozone Transport Commission in the Eastern United States).

Introduction of uncertainty in demand or in abatement cost technology is another issue that is of interest in tradable permit markets. Ben-David et al. (2000) explore in the laboratory the specific effects of uncertainty in tradable permit markets on prices, trading volume and the firms' ability to realize cost savings. They focus on two types of uncertainty: uncertainty regarding the time at which the permit allocation will be reduced, and uncertainty regarding the magnitude of the reduction. They find that neither kind of uncertainty has an impact on trade volume and prices.

In this paper we introduce uncertainty in the form of random correlated and uncorrelated shocks to the abatement and emissions target specified by the firms. Such variation could occur, for example, due to unexpectedly low production or due to superior abatement equipment performance. Unexpected variation in emissions can also be correlated across firms in many applications. For example, all firms in any particular region would experience similar weather patterns, which would correlate their emissions that are related to energy production. Emissions could also be correlated across firms due to economic growth cycles. During high growth periods, for example, most firms would need more permits. Emissions uncertainty and correlation could thus affect permit prices. Emissions uncertainty also interacts with the choice of banking rules chosen by the regulator. For example, in a model with costly enforcement, Innes (2003) shows that efficient (first-best) regulation of uncertain emissions cannot by achieved without permit banking.

The third feature we examine in this paper is the regulatory compliance strategy followed by firms in an emissions trading market when they are subject to imperfect monitoring by a regulatory agency. Emissions trading with imperfect enforcement has been considered by, among others, Malik (1990) and Stranlund and Dhanda (1999). Continuous emissions monitoring is practical for some applications, such as SO_2 emissions from large, stationary sources, but in many other applications compliance relies on self-reporting and must be enforced. The only other experimental emissions trading research we are aware of with imperfect enforcement is the recent work by Murphy and Stranlund (in press). They focus on varying the penalty schedules and environmental standard in a setting in which audit probabilities are unaffected by the individuals' decisions.

Our experiment, by contrast, features a dynamic enforcement strategy in which audit probabilities depend on past compliance and inspections. Economists and policy makers have been puzzled by evidence that most firms comply with environmental regulations, in spite of the fact that the frequency of inspections is not very high in most programs, and even when violations are discovered, fines or penalties are not very severe (e.g., see Eckert, 2004). Static models of regulatory compliance cannot resolve this puzzle, but dynamic models suggested by Harrington (1988), Greenberg (1984) and Landsberger and Meilijson (1982) can. In these dynamic models, the firms that are found to be in violation in one period are moved to a separate group in the next period. In this "high enforcement" group they are subject to more frequent inspection and higher fines. Hence they have an incentive to comply with regulations in order to avoid being moved into this second inspection group.[3] In our experiment, the firms respond to the auditing probabilities they face. The regulator's enforcement policy is an exogenous environmental parameter, not a choice variable for any subject.

[3] A weakness of this type of conditional audit rule pointed out by Raymond (1999) is that firms' compliance costs must be identical and known to the regulator. Interestingly, these otherwise strong conditions are actually satisfied in competitive permit markets since marginal compliance costs for all firms equal the (observable) permit price.

It is important to examine the interaction among these three features of tradable permit markets. Banking is very useful to minimize unexpected short or long positions at the end of a trading period and therefore reduce uncertainty and risk in the emission market. Allowing for the possibility of correlated random shocks to emissions increases the parallelism between the experiment and the field, and it also allows us to study the impact of these shocks on permit prices and on compliance strategies. Earlier research has focused on these issues separately. The impact of emission shocks on prices is significantly stronger when subjects are not allowed to bank permits, so banking clearly helps in stabilizing prices in the permit market. Stability in prices in emissions markets is important for a variety of reasons, including for encouraging the appropriate level of expenditure and R&D investment on abatement technology. We find that banking, however, leads to lower compliance with regulations. The benefits to underreporting emissions are greater when unused permits can be banked for future use or sale. Regulators therefore would need to consider this trade-off between the price stability that banking provides and the increase in emissions due to non-compliance.

The rest of the paper is organized as follows. Section 2 describes the experimental design and the procedures followed by the subjects. Section 3 identifies the hypotheses that we test using laboratory data. Section 4 presents the results on compliance behavior of subjects, the emissions generated, and the permit prices in the market, and Section 5 concludes.

2. Experimental design

We conduct 16 sessions, in each of which eight subjects trade emissions permits in a computerized double auction, plan targeted emissions and abatement, and make regulatory reporting and compliance decisions. We use this environmental terminology in describing the experimental design and results, but subjects saw more neutral terminology. For example, we called emissions permits simply "coupons", and we referred to marginal abatement costs as marginal "production costs".[4] Table 1 summarizes the essential components of the experimental design. We conducted the experiment using the University of Zurich's Z-tree program (Fischbacher, 1999). All subjects were undergraduate students at Purdue University.

Subjects participate in a randomly determined number of periods that is unknown to them. Each period is divided into different stages. In some stages subjects trade emission permits and in other stages they are required to plan abatement and report emissions. Subjects can "legally" avoid incurring abatement costs by holding and redeeming emissions permits. All subjects start stage 1 of each period with a number of permits determined by an initial allocation, adjusted upward by previous period banking or downward by enforcement penalties. They can then adjust their permit holdings by buying and selling in the double auction permit market.[5] Each subject has to set an abatement target and can adjust permit holdings, knowing that to be in compliance the sum

[4] We chose to present neutral terminology to subjects because including environmental terms might influence subjects differently. These differences between subjects would lead to reduced experimental control over their preferences. Employing neutral terminology is very common in economics experiments.

[5] The continuous double auction market is commonly used in economics experiments to generate a competitive trading environment with relatively accurate and rapid price discovery. Both sellers and buyers are free to submit public offers to sell or buy at any time, or to accept an offer and immediately execute a transaction. We employ an order improvement rule, requiring new buy offers to exceed the current best buy offer and requiring new sell offers to be less than the current best sell offer. The zTree program we use immediately updates traders' portfolios following a transaction, and it maintains a public order book of buy and sell offers. We restrict all trades and offers to one permit at a time.

Table 1
Summary of the experimental environment

Subjects
Undergraduate students all of whom participated in a training session before participating in the data sessions
Eight subjects in each session. There are four types of subjects in each session, with two in each type. The types differ by the marginal abatement costs (see Table 2)
Sessions
Sixteen sessions, with 11–17 periods in each session. The end period is determined randomly
Each period is divided into four stages
Treatments
Banking of permits: banking allowed in half of the sessions, and not allowed in the other half
Shocks to emissions: correlated shocks in half of the sessions and uncorrelated shocks in the other half
A balanced 2 × 2 design, with four sessions in each of the four treatment conditions
Stage 1: permit trading
Subjects start this stage with an initial allocation of permits that is adjusted upward if banking is allowed in that session or downward due to enforcement penalties
Subjects can buy or sell permits in a double auction permit market
Three-minute trading period
Stage 2: abatement target
Subjects enter an abatement target
Their actual abatement however could be lower or higher by a random amount that is revealed to them at the start of stage 3
Stage 3: actual abatement and reconciliation market
Subjects learn about the random shock that they face
This random shock can be −2, −1, 0, 1, 2
In eight of the sessions subjects face correlated random shocks: all shocks are either iid drawn from (−2, −1, 0) with probabilities (40 percent, 40 percent, 20 percent) or are iid drawn from (0, 1 2) with probabilities (20 percent, 40 percent, 40 percent)
In eight sessions subjects face uncorrelated shocks: the five possibilities (−2, −1, 0, 1, 2) are equally likely, drawn iid
After they learn about their actual abatement, the market is open for trading for 2 min
Stage 4: reporting emissions
Subjects report emissions
To be in compliance subjects must have the sum of abatement plus permits ≥ 10
If not compliant they pay a per unit fine and their permits for next period are reduced by the amount the sum falls below 10
Also if the actual and reported abatement does not match, subjects pay a fine
Enforcement
Dynamic framework: audit probability depends on past compliance and inspections
Two inspection groups. Subjects start the experiment in group 1
If subjects pay a fine for any violation, they are moved to group 2 where they are inspected more frequently and have to pay a higher fine for violations
If subjects are inspected in group 2 and found compliant, they are moved back to group 1

of the abatement and permits must be greater than or equal to 10 units. The first stage market is open for trading for 3 minutes. Marginal abatement costs and the initial permit endowment varied across subjects as shown in Table 2, providing gains from trade. Fig. 1 displays the aggregate demand (derived from the avoided abatement costs) and aggregate supply of permits in period 1. For the aggregate endowment of 32 permits, which is also the target level of emissions, prices in the interval [88,91] clear the market. Type 1 and Type 2 firms buy 4 and 2 permits in equilibrium, respectively, and Type 3 and Type 4 firms sell 2 and 4 permits, respectively, so a total of 12 permits are exchanged in equilibrium.

Table 2
Assigned marginal abatement costs

Units of abatement	Type 1 (firms 1–2)	Type 2 (firms 3–4)	Type 3 (firms 5–6)	Type 4 (firms 7–8)
1	53	67	27	35
2	61	70	35	38
3	70	74	44	42
4	80	79	53	47
5	91	86	63	**54**
6	103	95	**73**	**63**
7	116	106	**84**	**74**
8	130	**119**	**98**	**88**
9	**145**	**134**	**113**	**105**
10	**161**	**151**	**129**	**125**
Permit endowment	2 per firm	3 per firm	5 per firm	6 per firm

Note: Permit endowment, pre-trading, allows firms to avoid the abatement costs shown in bold.

In stage 2 subjects enter an abatement target. Their actual abatement, however, and therefore realized emissions, could differ from the target specified by a random shock. Actual abatement is equal to the target abatement plus the random shock, which can be −2, −1, 0, 1, or 2. This simple distribution is easy for subjects to understand, and it is similar to the shocks used by Godby et al., except that they employed a smaller range of −1, 0 or 1. In one of the treatments examined in this paper, we focus on the correlation of the shocks experienced by subjects. In eight of the sessions subjects face uncorrelated random shocks, and in the other eight sessions subjects face correlated random shocks. Uncorrelated shocks imply that the five possibilities (−2, −1, 0, 1, 2) are equally likely and independently drawn across subjects. In the sessions with correlated shocks, there is an equal chance each period that everyone has nonpositive or nonnegative shocks; that is, all shocks are either iid of (−2, −1, 0) with probabilities (40 percent, 40 percent, 20 percent) or are iid over (0, 1, 2) with probabilities (20 percent, 40 percent, 40 percent). Positive abatement shocks lead to lower levels of realized emissions and can thus be interpreted as negative

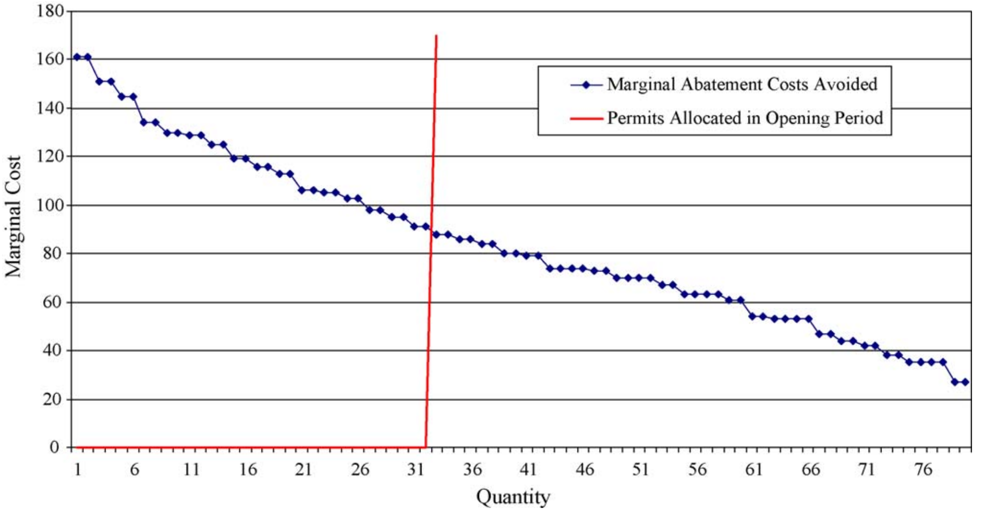

Fig. 1. Avoided abatement costs and total permits available.

emission shocks. Similarly negative abatement shocks are similar to positive shocks to emission levels.

In stage 3 subjects learn about the amount and direction of their random shock and hence their actual abatement and emissions level. Subjects pay the abatement costs corresponding to their actual, realized abatement. Then, the permit market is opened for trade for another 2 minutes. Emissions trading programs in the field frequently feature this type of reconciliation period for trading following the conclusion of the control period and the realization of actual emissions. The price impact of the emissions shocks is likely to be strongest during this reconciliation period. Another treatment in this paper focuses on banking. In eight sessions subjects are allowed to carry forward their unused permits to the next period, and in the remaining eight sessions any unused permits expire and cannot be banked.

The regulator always perfectly observes the number of permits held by each subject, as would be the case, for example, with a well-designed permit registry. Emissions are self-reported, however, to an "inspector" in the last stage of each period, stage 4. Following this report, the inspector randomly determines whether to inspect the firm to determine actual emissions. If the subject is inspected, the inspector observes the actual emissions and also whether the abatement and emissions report submitted by the subject is truthful. If the actual abatement and permits sum is less than 10, then subjects pay a fine for each unit below 10, and the permits they start the next period with are reduced by the amount that this sum falls below 10.[6] If this sum is greater than 10, then they are not fined, and in sessions where banking is allowed they can carry over these "unused" permits to the next period. Subjects are also fined if their abatement report is greater than actual abatement, and they pay a fine for each unit that these amounts differ. They pay the maximum fine calculated by these two methods. If the subject is not inspected, the inspector just checks whether the sum of the abatement report and permits is greater than or equal to 10. If this sum is less than 10, then this subject would pay a fine for each unit below 10, and the permits she starts the next period with are reduced by the amount the sum falls below 10. If this sum is greater than 10, then she is not fined and can carry over these "unused" permits to the next period.

Based on the dynamic enforcement, conditional audit model of Harrington, our enforcement regime employs two inspection groups. If the subject pays a fine for any type of violation while in group 1, she is automatically moved to group 2. If she is inspected while in group 2 and is found to be in full compliance, then she is automatically moved back into group 1. Everyone starts the experiment in group 1. The chances of being inspected and the per-unit fines for violations differ in the two groups. In group 1, subjects are inspected with a probability of 0.3, and if caught violating they are fined 60 for each unit of violation and they are moved to group 2, which has a higher inspection probability (0.5) and a higher per-unit fine for violating (150). These inspection rates and fines do not change during the experiment. Although the expected per-unit penalty in group 2 is only 75 (150 × 0.5), the enforcement leverage provided by the two inspection groups makes full compliance optimal for subjects in this group even when permit prices (i.e., the marginal compliance cost) rise as high as 132.[7]

The end period of the experiment is determined randomly. Subjects know that eight periods are conducted with certainty in each session, and then after the eighth period and after every subsequent period the experimenter rolls a six-sided die. If this die roll comes up "6" then the

[6] Reducing the following period's permit allocation is a common penalty in emissions trading markets.

[7] The formula determining this threshold permit price is $p_2F_2 + [p_2\beta(p_2F_2 - p_1F_1)]/[1 - \beta(1 - p_1)]$, where p_i is the group i inspection probability, F_i the fine per unit of violation in group i, and β is the discount factor or continuation probability (Harrington).

experiment ends immediately; otherwise, the session continues for another period. The experimenter reveals the outcome of each single roll at the end of each period beginning after the eighth period. A total of 11–17 periods were conducted in each session.

In all 16 sessions reported here subjects were experienced as they had participated in a training session where they made decisions relating to compliance and also participated in up to five periods of the four-stage permit-trading environment just described, but with different parameters. These training periods are not reported here, but Cason and Gangadharan (2006) report an analysis of the compliance training data in which subjects participated in seven separate treatments with different compliance costs and enforcement parameters. Prior to the sessions reported here, the experimenter reviewed the instructions and conducted a quiz that the subjects needed to answer before participating in the experiment. We implemented a balanced 2 × 2 design, with four sessions in each of the four treatment conditions (banking versus no banking, and correlated versus uncorrelated emission shocks). One session in each treatment employed double-experienced subjects who had already participated in one full session. At the end of each session, Experimental dollars were converted to U.S.$, at a rate of 300 Experimental Dollars = 1 U.S.$. Including instruction time, sessions lasted about 2 hours, and the subjects earned an average of $26.42 per person.

3. Hypotheses

This experimental design allows us to address several principle hypotheses.[8] The first four concern compliance behavior of agents when reporting emissions.

Hypothesis 1. Noncompliance is greater for agents in inspection group 1.

Hypothesis 2. Noncompliance is greater when permit prices are higher.

Hypothesis 3. Noncompliance is not different for agents who face greater marginal abatement costs.

Hypothesis 4. Noncompliance is greater following positive aggregate and individual shocks to emissions.

The first two hypotheses follow directly from an application of Harrington's (1988) model of enforcement leverage. Permit prices determine the marginal compliance cost for all firms, because any firm could achieve compliance by acquiring permits. Of course, in equilibrium the permit price equals the marginal abatement cost that is equalized across firms. For the enforcement parameters employed in the experiment, for all marginal compliance costs (permit prices) in the interval (18,132), the optimal compliance policy is to violate in inspection group 1 and comply in inspection group 2. Subjects should comply in all inspection groups for prices less than 18, and they should never comply in any inspection group for prices greater than 132.[9] Most prices were

[8] As discussed below, some of these hypotheses follow directly from theoretical models specified elsewhere, and others are simply informally consistent with the spirit of certain theoretical models. While our experimental design incorporates some of the features of the models in these papers, it is not possible to represent fully the existing separate theoretical models on banking, uncertainty and enforcement in one experimental design.

[9] As already mentioned, the end period of the experiment is randomly determined. However, in periods 1–7 before the die rolls begin, subjects know that a minimum number of additional periods will be conducted with certainty. This raises the maximum permit price for which the "violate in group 1 and comply in group 2" strategy is optimal, effectively increasing the range of prices for which this compliance policy should be adopted.

in the (18,132) interval, although a few were greater than 132. Therefore, we expect significantly more noncompliance for subjects when they are in group 1.

In theory the threshold permit price of 132 is a critical point where noncompliance should increase significantly. In previous work that manipulated compliance costs and enforcement parameters across a variety of treatments, Cason and Gangadharan found that noncompliance rates increased gradually as the relative payoff to noncompliance increased, rather than a sharp switch from one corner solution to the other when compliance costs passed through the critical threshold. This earlier study employed a quantal choice model of bounded rationality that can fit this compliance pattern accurately. Correspondingly, we expect a more smooth adjustment of compliance rates to permit prices, indicated by Hypothesis 2.

Hypothesis 3 follows from Stranlund and Dhanda, who point out that unlike traditional models of regulation in which individual compliance costs determine incentives for compliance, when emissions permits are tradable the permit price represents the marginal compliance cost that is relevant for all agents' compliance decisions. Therefore, we expect that even though our subjects faced significantly different (pre-trade) marginal abatement costs, they should not comply at significantly different rates since they could all obtain permits at the same price on the market. Note that this research hypothesis, since it concerns the *absence* of a difference in behavior across agent types, corresponds to the null hypothesis used in the statistical tests that are discussed in the next section.

Hypothesis 4 postulates a relationship between emissions shocks and compliance, operating indirectly through permit prices, and it is related to Hypothesis 2. Positive emissions shocks are likely to increase permit prices, increasing compliance costs and decreasing compliance, but if the shocks are not fully reflected in permit prices, some subjects facing positive, idiosyncratic shocks may not be able to acquire sufficient permits in the reconciliation period and may be noncompliant.

The next hypothesis concerns the environmental performance of the system, and is the emissions counterpart to Hypothesis 2:

Hypothesis 5. Emissions are greater when permit prices are higher.

We expect that emissions will be significantly greater when permit prices are higher, if (according to Hypothesis 2) the higher prices cause more agents to be noncompliant.

The final hypotheses concern permit prices:

Hypothesis 6. Permit prices are less volatile when permits can be banked for future use.

Hypothesis 7. Permit prices are higher on average, particularly late in the period, following positive (aggregate) shocks to emissions. This relationship between emissions shocks and permit prices is stronger without permit banking.

Hypothesis 8. Permit prices are lower on average when more permits were banked in the previous period.

Hypotheses 6 and 7 are closely related. When positive (negative) aggregate emissions shocks occur, agents require more (fewer) permits than they had planned. This puts upward (downward) pressure on permit prices in the reconciliation trading period. If agents cannot bank permits, unused permits expire and become worthless when the period ends. Therefore, prices could plummet following negative emissions shocks and sellers dump unneeded permits on the market. The inability to bank permits can also result in price spikes following positive emissions shocks. Because of the permit expiration, agents do not wish to hold a large "buffer" of permits to insure them against positive emission shocks. Consequently, when those shocks do occur, an overall shortage of permits exists, driving up their price.

Banking allows agents to hold a buffer of permits to insure against price spikes, and it also eliminates their incentive to dump unused permits on the market. As more permits are banked, however, this can affect price *levels*. In particular, Hypothesis 8 indicates that greater banking is expected to result in lower permit prices since banking effectively increases the supply of available permits. This is analogous to shifting out the vertical supply of permits on Fig. 1 above.

4. Results

In this environment with emissions shocks and imperfect enforcement, allowing banking of permits leads to a reduction in permit price volatility, but banking also reduces compliance and increases emissions. Figs. 2 and 3 illustrate the price volatility effects, which are formally documented later in Section 4.2. These figures also help the reader to understand the price data generated by the experiment. The vertical lines represent breaks between periods. Fig. 2 displays transaction prices in session BUN3, which features permit banking and uncorrelated emissions shocks. Prices are relatively stable at approximately 80 experimental dollars. This is a bit lower than the initial market-clearing price interval of 88–91, but prices should decline when the stock of banked permits increases above zero (as occurs in this and most other sessions with banking). Fig. 3 displays transaction prices in session NBCO3, which features no permit banking and correlated emissions shocks. The large price volatility in this session is striking, particularly the late period trades after the emissions shocks are realized. Low prices late in the period, such as indicated in periods 3, 4 and 13, follow negative emissions shocks that cause the unbankable permits' value to plummet. High prices late in the period, such as in periods 8 and 11, follow positive emissions shocks that lead to a shortage due to the limited available permits.

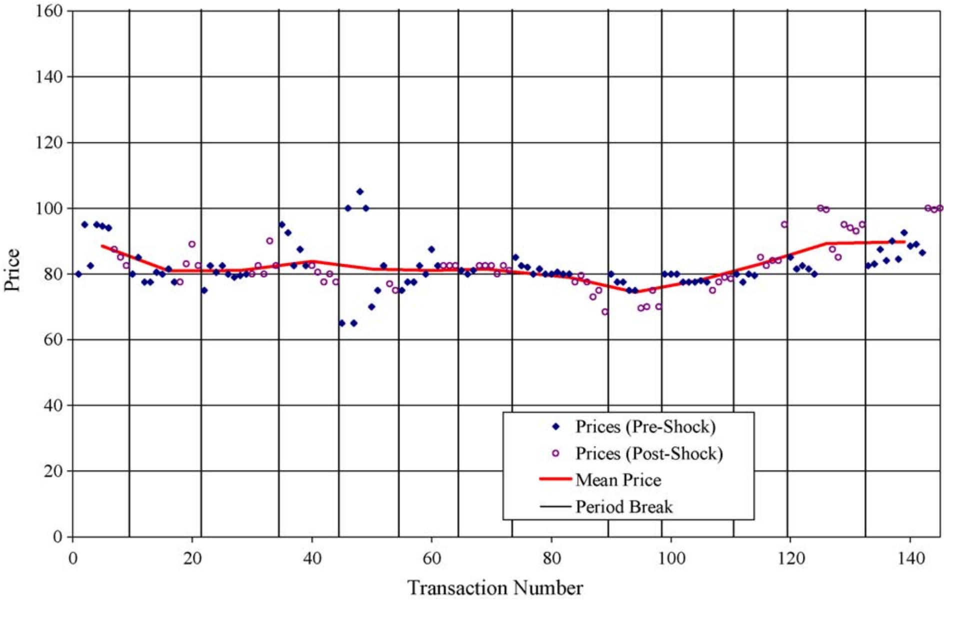

Fig. 2. Transaction prices for session BUN3, with banking and uncorrelated emissions shocks.

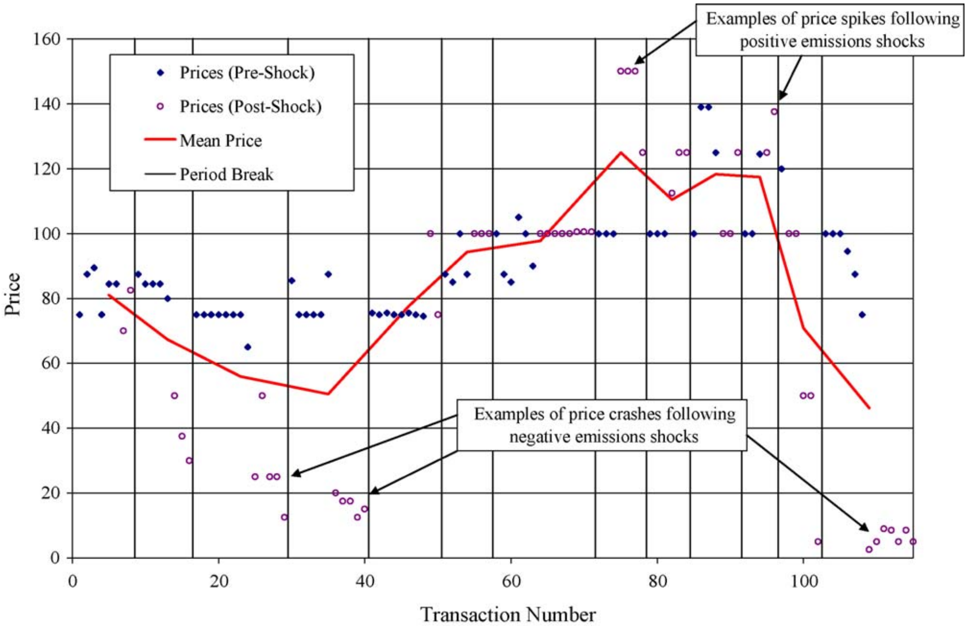

Fig. 3. Transaction prices for session NBCO3, with no banking and correlated emissions shocks.

4.1. Compliance and emissions

Recall that emissions permit trading equalizes the cost of compliance across firms since the market price of permits determines marginal compliance costs. Recall also that for the two-state enforcement program and the fines, inspection rates and group transition probabilities chosen for our experiment, traders should violate when in inspection group 1 and comply when in inspection group 2 for prices in the interval (18,132). The vast majority of prices are in this interval, and for some sessions this interval contains all transaction prices. Compliance is optimal in both inspection groups for lower prices, and violation is optimal in both inspection groups for higher prices.

Table 3 presents results using multivariate regression models.[10] These models evaluate the contribution of the (potential) impact of multiple influences on the compliance choices of subjects. These panel regressions employ a random effects error structure, with the subject representing the random effect. The empirical models allow compliance to depend on the amount the individual banks in the previous period, which inspection group the individual is in when she enters the new period, the banking dummy (=1 in sessions where banking is allowed), the correlated shocks dummy (=1 in sessions where the shocks are correlated across all subjects), the magnitude of the emissions shocks experienced by the individual (defined as the difference between the target emissions and the actual emissions realized), the magnitude of the total shocks faced

[10] We also conducted conservative, nonparametric Wilcoxon's signed-rank tests with exactly one summary statistic value per session (in order to satisfy the statistical independence required for this kind of a test). These tests are valuable as they require a minimum of statistical assumptions. They support the results obtained from the multivariate regressions; hence to conserve space they are not reported.

Table 3
Random effects models on compliance decisions

Explanatory variables	Probit (=1 if comply)		Amount of violation (Tobit)	
	(1) Model 1	(2) Model 2	(3) Model 1	(4) Model 2
Amount individually banked	0.0100 (0.0196)	0.0093 (0.0198)	−0.0034 (0.0477)	−0.0009 (0.0467)
Inspection group 2 dummy	1.4438[†] (0.1402)	1.4530[†] (0.1408)	−4.1411[†] (0.3207)	−4.1746[†] (0.3321)
Dummy for banking	−0.5520[*] (0.2435)	−0.4984[*] (0.2294)	1.5902[**] (0.5465)	1.8024[**] (0.5624)
Dummy for correlated shocks	−0.0680 (0.2362)	−0.0148 (0.2315)	−0.3016 (0.5049)	−0.1384 (0.5456)
Amount of individual shocks	−0.3392[†] (0.0506)	−0.3426[†] (0.0503)	0.9012[†] (0.1214)	0.9060[†] (0.1220)
Amount of aggregate shocks	−0.0395[†] (0.0143)	−0.0389[†] (0.0141)	0.0972[†] (0.0342)	0.0977[†] (0.0338)
Post-shock transaction price[a]	−0.0199[†] (0.0055)	−0.0201[†] (0.0054)	0.0456[†] (0.0127)	0.0458[†] (0.0127)
Marginal abatement cost		−0.0052[*] (0.0030)		0.0065 (0.0064)
Constant	2.3901[**] (0.5567)	2.8546[**] (0.6110)	−5.4188[**] (1.3586)	−6.3299[**] (1.6543)
Observations	1274	1274	1274	1274
Number of subjects	128	128	128	128
Wald Chi-squared (7)	232.44		349.49	
Prob > Chi-squared	0.000		0.000	
Wald Chi-squared (8)		231.95		338.17
Prob > Chi-squared		0.000		0.000

Standard errors in parentheses.

[a] Instrumental variable (predicted value from the price regression used): First stage price regression: observations = 1384, R-squared = 0.33).

[*] Denotes significant at 5 percent (assessed using two-tailed tests to correspond to the research hypotheses being tested).

[**] Denotes significant at 1 percent (assessed using two-tailed tests to correspond to the research hypotheses being tested).

[†] Denotes significant at 1 percent (assessed using one-tailed tests to correspond to the research hypotheses).

by all the subjects in that period, and the price of permits. The price of permits is endogenous, as higher compliance rates could increase the average permit prices in the post shock trading stage. We therefore estimate a price equation based on only exogenous variables and use the predicted price from this estimation in the compliance equation, the standard instrumental variables technique.[11]

Columns 1 and 2 of Table 3 present results from a random effects probit model for the compliance choices. The probit model examines the factors that determine the subjects' binary decision to comply with or violate regulations. The model specification in column 2 includes the marginal abatement costs faced by subjects to examine if higher marginal abatement costs lead to lower compliance performance.[12] The probability of compliance is significantly higher when subjects are in inspection group 2, providing further support for Hypothesis 1. The marginal effects on the probit regression (not shown in the table) indicate that being in inspection group 2 increases the probability of compliance by 28 percent. The probability of compliance decreases when subjects experience higher individual and total shocks to emissions, supporting Hypothesis 4. Notably, the individual shock is an order of magnitude more important than the aggregate shock experienced by the market. A one-unit increase in the individual shock decreases the probability of compliance by 7 percent whereas the same change in the aggregate shock decreases compliance by only 0.8 percent. These shocks decrease the supply of permits in the market, thus increasing permit prices and decreasing the level of compliance. In sessions with banking, the probability of compliance is lower by 11 percent.

The likelihood of compliance is also significantly lower when permit prices are higher, supporting Hypothesis 2. The marginal abatement cost of the individual is also significant, however, with higher costs reducing the probability of compliance. The impact of this variable is not very strong, however, which provides evidence for Hypothesis 3.[13] The impact of prices on compliance is approximately four times stronger than the marginal cost effect. The marginal abatement cost of the individuals provides little extra information, beyond permit prices, to better predict compliance behavior.[14]

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[11] The exogenous variables used to estimate the price equation are the following: inspection group that the individual is in when she enters the new period, dummy for banking, dummy for correlated shocks, magnitude of individual shocks, magnitude of aggregate shocks, the fines paid by the individual in the previous period and the amount banked in this period. The dependent variable employed is the post shock transaction price. (Model quality: $F(7, 1376) = 106.87$ and Prob > $F = 0.000$, R-squared = 0.33, observations: 1384).

[12] There are four types of subjects in the experiment, with two subjects in each type. The marginal abatement cost variable takes a value of 145 for type 1 subjects, 119 for type 2 subjects, 73 for type 3 subjects and 54 for type 4 subjects. We also conducted robustness tests by including a dummy for high marginal abatement cost, and this dummy is not significant in explaining compliance.

[13] The marginal abatement cost loses its significance in explaining compliance when we include an additional explanatory variable (net seller) to capture the initial endowment of permits of the subjects. Four of the subjects are net sellers of permits as their initial endowment is larger than what they should retain given the market price of permits. The other four subjects are net buyers. Murphy and Stranlund report that their net sellers have significantly higher compliance levels than their net buyers. In our data, we find that being a net seller does not lead to greater compliance. Hence neither the initial endowment nor marginal abatement costs of subjects can predict compliance as accurately as can the price of permits.

[14] Murphy and Stranlund vary the enforcement level and separate out the direct and indirect price impact of increased enforcement on compliance behavior. They show that increased enforcement encourages firms to increase their compliance levels by purchasing more permits (direct effect). This however puts upward pressure on prices in the permit market and eventually leads to less compliance by firms (indirect effect). The direct effect is always larger so that increased enforcement leads to higher compliance levels; however the indirect price effect is often significant, and ignoring it could over-estimate the effectiveness of enforcement programs. Our experiment does not vary the enforcement parameters, so it does not trace out these direct and indirect effects.

Table 4
Random effects regression on aggregate market level emissions

Explanatory variables	Estimate (standard error)
Post-shock transaction prices[a]	0.0277 (0.0573)
Dummy for banking	1.9949 (2.6312)
Amount banked and brought forward	0.4964[**] (0.0783)
Magnitude of aggregate shocks	0.9612[**] (0.1273)
Constant	32.1056[**] (5.6705)
Observations	188
Number of sessions	16
Wald Chi-squared (4)	252.57
Prob > Chi-squared	0.000

Standard errors in parentheses.
[a] Price instrumented for, to correct for endogeniety (First stage Price regression: Observations = 188, Wald Chi-squared (5) = 164, Prob > Chi-squared = 0.0000). The independent variables used in the price equation are: dummy for banking, dummy for correlated shocks, magnitude of total shocks, amount banked and brought forward and an interaction dummy between the magnitude of total shocks and no banking.
[**] Denotes significant at 1 percent (assessed using two-tailed tests). Prices are not statistically significant in explaining emission patterns using a one-tailed test.

Columns 3 and 4 of Table 3 present estimates from a random effects tobit model on the magnitude of violation. The tobit model accounts for the lower threshold of zero for the amount violated. The results mirror those from the probit model and show that the amount of noncompliance is significantly higher when banking is allowed. Higher permit prices later in the period also increase the amount violated. Note that marginal abatement costs do not have a significant impact on compliance in the tobit model.

Compliance rates alone may not be the most important measure of system performance because a regulator might not be concerned about lower compliance unless it led to higher emissions. Our data strongly establish this inverse relationship between compliance and emissions. Recall that the target emissions (the number of issued permits) is 32 per period. Actual emissions exceed this target on average in all eight sessions with banking allowed, but in only one-half of the sessions without banking. Using Wilcoxon's test based on one observation per session, the data reject the hypothesis that average emissions rates are the same with and without banking (two-tailed p-value = 0.048). Emissions are not significantly different for the two types of shock correlation.

Table 4 presents a random effects regression model of emissions to determine the factors that affect the total amount of emissions generated by the subjects in each period. The 16 sessions represent the random effect in this panel. The estimates show that an additional permit banked and carried forward to the current period leads to a 0.5 increase in total emissions. The positive and highly significant coefficient for the aggregate shocks indicates that an increase in the magnitude of aggregate shocks to the market increases emissions by 0.96. Transaction prices do not significantly increase market-level emissions in this model, providing no support for Hypothesis 5.[15]

[15] The perceptive reader may have noticed that we do not report a standard efficiency measure of overall system performance. This is because overall efficiency depends on the social cost of emissions. Since emissions frequently exceed the per-period target of 32, our realized efficiency depends on the environmental damage function, which is not specified in the experiment.

Table 5
Average absolute price difference, before and after emissions shock, for 16 individual sessions

Sessions	Banking allowed	No banking
Uncorrelated emission shocks		
BUN1 and NBUN1	8.71	25.24
BUN2 and NBUN2	4.08	15.35
BUN3 and NBUN3	5.70	22.04
BUN4 and NBUN4	6.23	7.63
Treatment mean	6.18	17.56
Correlated emission shocks		
BCO1 and NBCO1	5.85	31.90
BCO2 and NBCO2	11.31	20.15
BCO3 and NBCO3	12.74	33.75
BCO4 and NBCO4	12.09	24.72
Treatment mean	10.50	27.63

4.2. Permit prices

As is expected (Hypothesis 7) a positive relationship exists between the emissions shock and the change in average transaction prices after the emission shock is realized, with a greater emissions shock usually associated with a larger price increase. There is a much stronger relationship between shocks and price changes for the sessions without banking than for sessions with banking allowed. When banking is allowed average prices usually change following the shock by less than 20 experimental dollars. Without banking, average prices frequently change by more than 20 experimental dollars, and sometimes by as much as 60 experimental dollars. Table 5 presents the average absolute difference in average prices before and after the emissions shock is realized, separately for all 16 sessions. A Wilcoxon's test based on these 16 independent observations strongly rejects the null hypothesis that these differences are equal in the banking and in the no banking treatments (one-tailed p-value = 0.006). In fact, only the lowest no banking average difference (7.63) is lower than any of the eight differences with banking allowed. This provides strong support for Hypothesis 6. The differences also appear greater on average when emission shocks are correlated than when they are uncorrelated, but these differences are not quite significant (two-tailed p-value = 0.177).[16]

Average transaction prices vary across sessions due to levels of banking and the presence of emissions shocks. Table 6 documents some parametric evidence on these price movements using random effects panel regressions, with the session representing the random effect. Column 1 presents results when the dependent variable is the average price for the trades that occur before the emission shock, column 2 when the dependent variable is the average of all transaction prices, and column 3 when the dependent variable is the average of the post shock (reconciliation period) transaction prices. The impact of the aggregate shocks is clear from this table and provides support for Hypothesis 7. Positive emissions shocks lead to a significant increase in post shock prices

[16] Results are similar when using an alternative measure of price volatility that does not distinguish the pre- and post-shock trading periods. The average coefficient of variation in transaction prices, calculated based on all transaction prices within a period regardless of when they take place, also strongly rejects the null hypothesis that volatility is equal in the banking and in the no banking treatments (Wilcoxon's one-tailed p-value = 0.004). This provides further evidence to support Hypothesis 6.

Table 6
Random effects regressions on average transaction prices

Explanatory variables	(1) Pre-shock prices	(2) Overall prices	(3) Post-shock prices
Dummy for banking	−1.0509 (9.2052)	6.8513 (9.4307)	17.1023 (10.0266)
Dummy for correlated shocks	3.6337 (9.0766)	1.7805 (9.2994)	0.4523 (9.8534)
Magnitude of aggregate shocks	−0.1331 (0.2407)	0.1560 (0.2461)	0.5854‡ (0.2892)
Magnitude of aggregate shocks × banking not allowed dummy	−0.0994 (0.3333)	1.0928† (0.3409)	2.4281† (0.3991)
Amount banked in previous period	−0.5206† (0.1368)	−0.6475† (0.1399)	−0.9447† (0.1635)
Constant	106.3346** (7.8673)	100.5616** (8.0603)	93.2672** (8.5408)
Observations	190	190	188
Number of sessions	16	16	16
Wald Chi-squared (5)	15.74	55.65	172.12
Prob > Chi-squared	0.007	0.000	0.000

Standard errors in parentheses.
** Denotes significant at 1 percent (assessed using two-tailed tests to correspond to the research hypotheses).
† Denotes significant at 1 percent (assessed using one-tailed tests to correspond to the research hypotheses).
‡ Denotes significant at 5 percent (assessed using one-tailed tests to correspond to the research hypotheses).

(column 3) and have no impact on the early period prices and the overall prices. The interaction between the shocks and banking is also evident. In sessions where banking is not allowed, the post shock transaction prices increase by 2.4 experimental dollars on average for each unit of the shock, compared to the case when banking is allowed. Banking permits in the previous period reduces the transaction prices significantly in the current period, and this is irrespective of whether the early, the late or the overall prices are employed as the dependent variable. This provides evidence supporting Hypothesis 8. The magnitude of the impact of banking is, however, highest for the late period prices, with an additional banked permit reducing post shock prices by 0.94 experimental dollars. This is significantly different from the average change in the marginal abatement cost function per unit (i.e., the average slope in Fig. 1 aggregate marginal abatement cost curve) of 1.70.

5. Conclusion

The economic and environmental gains expected from emissions trading programs depend on how these programs are implemented in the field. In this paper we focus on three design details relevant for permit market performance: the effect of allowing permit banking, the impact of uncertainty regarding emissions, and the incentives to comply with regulations. Although there is some empirical work on the relevance of banking in permits markets and the impact of imperfect enforcement, to our knowledge this paper is the first to examine the interaction between banking, emissions shocks and compliance. This can provide the first steps towards designing more optimal permit market regulations to achieve emissions targets with limited enforcement budgets and imperfect compliance.

Using laboratory methods we find that banking helps to smooth out the price volatility arising from the uncertainty over emissions. Price stability is important to equalize marginal abatement costs across firms and to provide the appropriate incentives for R&D into new emission control technologies. Allowing subjects to bank permits, however, leads to less compliance, and emissions are significantly higher with banking. The role of banking in promoting noncompliance is somewhat puzzling since subjects could oversell permits within the current period and not comply.

It is not necessary to bank permits to not comply, but our subjects more frequently chose to underreport emissions at the end of the period when they could bank permits. Our laboratory subjects apparently perceived that the benefits of underreporting emissions are greater when banking is allowed since unused permits can be carried over and used or sold in subsequent periods. Such incentives seem under-appreciated in the literature; for example, in Innes' model banking reduces noncompliance and therefore economizes on enforcement costs. Our results therefore highlight an important trade-off between price volatility and noncompliance that regulators need to consider when designing an emission permit program with imperfect enforcement. More research is needed, however, to understand better the incentives for noncompliance in the presence of banking.

Other banking rules may provide different compliance incentives, and they could be investigated in future theoretical and empirical work. For example, as noted in the introduction the Ozone Transport Commission in the Eastern U.S. restricts the aggregate level of banking to limit the year-to-year variability in emissions. In particular, if the total amount of banked emissions (by all sources) exceeds 10 percent of the total allowable emissions for the year, a 2-for-1 discount is applied to the use of the extra banked allowances. This could reduce the benefits of banking and increase incentives for compliance, relative to the unlimited banking studied in the present paper. We believe this is an interesting direction for future work.

Acknowledgements

We thank Ashraf Al Zaman for valuable research assistance, and for helpful comments we thank two anonymous referees and conference and seminar participants at the Iceland Workshop on Experiments in Natural Resource Economics, the Economic Science Association meetings, Econometric Society Australasian Meetings, the University of Maryland, Tilburg University and Georgia State University. This research has been supported by a grant from the U.S. Environmental Protection Agency's National Center for Environmental Research (NCER) Science to Achieve Results (STAR) program. Although the research described in the article has been funded in part through EPA grant number R829609, it has not been subjected to any EPA review and therefore does not necessarily reflect the views of the Agency, and no official endorsement should be inferred.

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