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Session V Proceedings

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A Laboratory Comparison of Uniform and Discriminative Price Auctions for Reducing Non-point Source Pollution*

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Abstract

Land use changes to reduce non-point source pollution, such as nutrient runoff to waterways from agricultural production, incur opportunity costs that are privately known to landholders. Auctions may permit the regulator to identify those management changes that have greater environmental benefit and lower opportunity cost. This paper reports a testbed laboratory experiment in which landowner/sellers compete in sealed-offer auctions to obtain part of a fixed budget allocated by the regulator to subsidize pollution abatement. One treatment employs uniform price auction rules in which the price is set at the lowest price per unit of environmental benefits submitted by a seller who had all of her offers rejected, so sellers have an incentive to offer their projects at cost. Another treatment employs discriminative price rules that are not incentive compatible, because successful sellers receive their offer price. Our results indicate that subjects recognize the cost-revelation incentives of the uniform price auction as a majority of offers are within 3 percent of cost. By contrast, a majority of offers in the discriminative price auction are at least 10 percent greater than cost. But the regulator spends more per unit of environmental benefit in the uniform price auction, and the discriminative price auction has superior overall market performance.

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1. Introduction

Auctions have become common to allocate scarce resources. Recent applications of economic theory and experimental economics to auction design have substantially improved the performance of auctions and have also helped to expand their applications to a broad range of problems. One area where auctions have attracted attention is in allocating resources to protect the environment. Many environmental problems stem from agricultural land management practices. These include rising salt and nutrient levels in rivers and bays, wetlands degradation, destruction of remnant vegetation and dryland salinity. Non-point sources such as farms generate a substantial fraction of certain types of pollution, and it is difficult or prohibitively expensive to identify the amount and the source of many non-point emissions. Landowners have more information than the regulators about their production plans and their costs of reducing pollution. An incentive mechanism like an auction is well suited to address this information asymmetry and encourage different landowners to reveal their private opportunity cost of land management changes. This would help the regulator to identify the land use options with greater environmental benefit but lower opportunity cost.

The theoretical advantages of auctions to mitigate environmental problems are well understood (e.g., Latacz-Lohmann and Hamsvoort, 1997). However, using auctions to solve environmental problems in practice requires more empirical research. In this paper we use experimental methods to examine two kinds of auction designs for "environmental procurement:" uniform price auctions and discriminative price auctions. Landowners offer projects that generate environmental improvement in these auctions. More specifically, sellers offer projects with different costs and different levels of environmental benefits to the regulator, who ranks the offers on the basis of their offer price and the potential environmental improvement. The regulator has a fixed monetary budget and uses it to buy a maximum of one project from each seller, which corresponds to a specific land use change. In the uniform price auction, all participants submit sealed offers and successful sellers receive a uniform price (per unit of environmental benefit) equal to the lowest rejected offer. In the discriminative price auction, each successful seller receives the actual price offered, rather than a single price common to all sellers. In the discriminative price design the sellers face uncertainty about acceptance, but not about price, since the price obtained from the regulator equals the offer if the offer is accepted. When contemplating raising her offer, a seller trades off the decreased probability of acceptance against a higher trading surplus conditional on acceptance. She has an incentive to misrepresent her costs and submit offers higher than her true reservation values, because otherwise she would earn no trading surplus.

By contrast, in the uniform price auction all the successful sellers receive a market-clearing price that exceeds their offer and is set by a seller who does not trade. In these auctions each seller has the incentive to reveal her true costs, since submitting an offer greater than the cost of a unit lowers the probability of selling that unit but does not raise the price at which the item might be sold. We find that offers are substantially closer to costs in the uniform price auction compared to the discriminative price auction. Nevertheless, for the experimental parameters we employ, the overall performance of the discriminative price auction is superior.

Formal analysis of these types of sealed bid auctions dates back to Vickrey (1961), who compared the incentives resulting from different auction procedures. He obtained an important revenue equivalence theorem, which states that under the assumptions of bidder risk neutrality, independent private valuations, symmetry among buyers, single unit demand, payments a function of bids only and zero transaction costs incurred in bid creation and implementation, different auction formats yield the same expected revenue to the auctioneer. Much of the theoretical literature following Vickrey examines the robustness of this result to the introduction of alternative assumptions about buyers and sellers.[1] Empirical research comparing uniform and discriminative price auctions has used both field data and data from laboratory experiments. Kagel (1995) provides a survey of the early auction research. Smith (1982) reports the results of a number of experiments for multi-unit auctions in which the bidders submit single unit bids. The results neither support nor refute the revenue equivalence theorem. Cox et al. (1985) find that subjects failed to follow their dominant strategy of bidding equal to values in multiple unit, uniform price, sealed bid auctions. Cox et al. (1982) and Kagel et al. (1987) provide laboratory evidence that subjects respond strategically to the different incentives that alternative auction formats generate. Tenorio (1993) uses data from the Zambian foreign exchange auction to analyze the effects of a change in auction format from uniform price to discriminative price and finds that after controlling for other factors, the uniform price format yields higher average revenue than the discriminative price format. Umlauf (1993) reports similar results for auctions undertaken by the Mexican treasury.

Theoretical research on auctions cannot be directly applied to the auctions examined in this paper, however, because environmental goods and services violate many of the assumptions for the revenue equivalence theorem. For example, the auctions discussed in this paper assume that sellers offer multiple projects for sale, but because of the interaction of the environmental

[1] Holt (1980) shows that for risk averse buyers, the discriminative auction results in higher expected revenue. Maskin and Riley (2000) relax the assumption of symmetry and assume that the buyers' reservation values are not identically distributed. In this case the revenue equivalence theorem does not hold and the ranking of different auctions would depend on how the distributions vary across buyers. Some researchers have argued that the uniform price auction has a lower winner's curse in common value environments and results in greater revenue to the seller than would a discriminative auction (see Milgrom, 1989; Bikhchandani and Huang, 1993). However this work was based on a single-unit auction theory and Back and Zender (1993) show that this result is critically dependent on the assumption that the good was indivisible.

benefits across projects the regulator would choose at most one project from each seller.[2] In this setting, sellers may not make optimal offers independently on each project. Instead they could infer that certain projects have a higher potential probability of winning and therefore they might focus their efforts on obtaining profits on these projects. Since they know that the regulator will purchase at most one project from each seller, they could make less aggressive offers on their other projects so as to avoid competing with themselves across projects. Moreover, the fixed budget constraint for the regulator implies that the number of projects accepted is endogenous. Hence our environment is not consistent with any particular existing theoretical model, and it is unlikely that any new tractable theory could capture these complications that are present in most relevant field applications.

Nevertheless, we can implement these realistic complications in our laboratory testbed and compare the behavior and performance of these two auction institutions. Our results show that laboratory subjects understand the cost revelation incentives of the uniform price auctions, with most submitted offers near the actual costs. By contrast, in the discriminative price auction almost all offers are greater than cost. For the parameters we employ, however, the discriminative price auctions result in more efficient environmental protection than the uniform price auctions. All three performance indicators we employ show that the discriminative price design leads to significantly greater overall performance, even though the discriminative auction rules are not incentive compatible.

The rest of the paper is organized as follows. Section 2 describes the experimental design and Section 3 presents the results. Section 4 provides a brief discussion of the findings.

[2] For example, the installation of grassed swale drains with sediment traps to reduce nutrient loads would reduce the environmental benefit of decreased fertilizer applications. The benefits of these two alternative mitigation strategies are therefore interrelated, but the benefits would be evaluated separately in the auction for simplicity. To avoid the complication of project interactions we limit each seller to supply at most one project.

2. Experimental Design

2.1 Environment and Procedures

Experimental subjects are undergraduate students from Purdue University and the University of Melbourne. All participated in only one session reported here and had no previous experience in sealed offer auctions. We report 20 sessions, 10 conducted at Melbourne (5 in each auction format) and 10 conducted at Purdue (5 in each auction format). All sessions have 36 trading periods. In each session eight seller subjects offer items in a computerized sealed offer auction. Each auction period sellers can offer to sell three items that correspond to different land use changes and have different environmental benefits. Sellers submit offers using an electronic form on a web browser. After all offers are submitted, the server sorts the offers and ranks them on the basis of the offer price and the quality of the items (quality is the environmental benefit) and calculates the allocation for the period. The auctioneer buys the lowest-price projects per unit of quality, subject to the constraint that at most one item is bought from each seller and total auction expenditures are no greater than the auction budget (25,000 experimental dollars per period). The two auction institutions differ only in how they determine trading prices; see Table 2 below for a specific example. Once the allocation is made the results are conveyed to the subjects electronically.

As is usual in experimental economics, we use neutral terminology in the instructions to refer to the different items that sellers could offer. The appendix contains the experiment instructions. Subjects are asked to record the profits made in each of the 36 periods in their record sheets and they are paid privately in cash after the experiment. The conversion rate used for the Purdue sessions was 1000 experimental dollars = 1 U.S. dollar and the conversion rate used in Melbourne was 600 experimental dollars = 1 Australian dollar. Sessions typically lasted 60 to 90 minutes, including the instruction time. Average subject earnings were about US$24 each in the Purdue sessions and were about A$34 each in the Melbourne sessions.

As already noted, the revenue equivalence theorem does not apply in this environment, so it is possible that the relative efficiency of the two auction institutions under study might be sensitive to the specific parameters chosen for the laboratory testbed. This potential parameter sensitivity is not uncommon in laboratory research, but it is more relevant here because of the lack of clear theoretical predictions for the discriminative price auction and because we wish to strengthen the external validity of our results for potential field applications. We therefore chose parameters that correspond to costs and environmental benefits estimated specifically for nutrient runoff. In particular, we employ cost and quality parameters representing the estimated opportunities for environmental improvement through land use change in the Port Phillip watershed, in southern Victoria, Australia (also see Cason, Gangadharan and Duke, 2003). All subjects have their costs and quality drawn from broadacre (field cropping) and grazing land uses, which are the activities that represent the largest land use in the watershed (57 percent of the land) and contributes to 53 percent of annual nitrogen pollution.

Subjects make offers based on different costs and qualities to represent the heterogeneity across different activities on the same land and between the same activities on different plots of land. We introduce heterogeneity by drawing costs and environmental quality for each land use change independently for each seller, each period, from the uniform distributions based on the ranges shown in Table 1.[3] We use the same sequence of drawn values in all 20 sessions to minimize across session variation and to improve the power of our comparison across auction

[3] The benefit ranges shown in Table 1 represent the best available estimates given the soil type and topography of the Port Phillip watershed. The cost ranges were developed through consultation with private landholders. For additional details, see Cason, Gangadharan and Duke (2003).

institution treatments. Sellers know the costs of their land use change projects, but they do not know the associated quality (environmental benefit). We do not reveal the environmental benefits to sellers because a primary conclusion of Cason, Gangadharan and Duke (2003) was that this information led sellers to misrepresent their costs more for high-benefit projects, and this reduced total abatement and other performance characteristics of the auction. In order to enhance the external validity of the experiment, we also did not provide sellers with any information about other sellers' costs and quality or the distributions that are used to generate the costs and qualities. They are told simply that the costs and quality levels would be different across sellers and could change from period to period. They also do not know the regulator's budget, which is fixed at $25,000 experimental dollars in all periods, but they are informed that the experimenter purchases the lowest priced items per unit of quality, spending the fixed budget in each period. At the end of each auction period sellers only learned which item (if any) they sold and the price they received.

2.2 Treatments and Predictions

Our goal in this experiment is to compare the performance characteristics of uniform price and discriminative price auctions. In the uniform price treatment if sellers sell an item they receive a price that is greater than or equal to the price that they offer to sell at. The uniform price in the market is determined by the lowest price per unit of quality submitted by a seller who had all of his or her offers rejected. In the discriminative price treatment sellers receive their exact offer price when they sell an item. Both auctions employ the *greedy algorithm* that finds the best local solution by accepting the items that have the lowest price per unit of quality, subject to the other constraints that (1) no more than one item is purchased from each seller and that (2) total expenditures do not exceed the overall auction budget.[4]

Table 2 presents an example from period 31 in two sessions to illustrate the rules. In both auction formats the algorithm first calculates ratio of the offer price to the environmental benefit for each project, and then prioritizes projects according to this ratio from lowest to highest. The top panel of Table 2 shows this ranking and allocation for a discriminative price session. The first and second projects in this ranking are sold, but the third is not because the algorithm already bought a project from seller 5. The auction only purchases five projects because the cumulative cost is $24,505 and no additional projects can be purchased with the $495 remaining in the auction budget. The bottom panel of Table 2 shows results in a uniform price session. Again, only five projects are sold. All are sold at the offer/benefit ratio of a seller (7) who submitted the lowest ratio (49.33) but had all of her offers rejected. For example, instead of his red-unit offer of 2999, seller 1 received 49.33 times his environmental benefit (124.46) = $6,140 for this project. Total auction expenditures are $23,073 this period.

The standard revenue equivalence results do not apply in these auctions since sellers have multiple items to offer, they do not observe the quantity of environmental benefits for their items, the number of items purchased is endogenous since it is based on an overall auction budget, not to mention other practical reasons equivalence results often do not apply such as risk aversion and bounded rationality. Our focus is therefore *not* on comparing the outcomes of these auctions to theoretical predictions, but rather it is on comparing the relative empirical performance of the two auction institutions for this environmental management application. Because of the multiple items per seller and the differing, unknown environmental benefits for each project, this environment is too complex to provide clear theoretical predictions.

[4] We could have implemented a more complex algorithm that is more likely to find the globally optimal solution, but at the cost of not being able to explain the auction purchase rule to sellers. We chose this simple algorithm since our goal is to study auction rules that could be implemented in the field with a reasonable level of transparency.

Nevertheless, it is useful to have some theoretical benchmarks based on simplifying assumptions to motivate the institutional comparison.

The most reasonable benchmark for the uniform price auction is full revelation: offer=cost. In this type of "first-rejected-offer" uniform price auction sellers usually have a dominant strategy to offer their projects at cost. This is because submitting an offer below cost would only increase the probability of acceptance if the price received falls below cost, and submitting an offer above cost is very unlikely to raise the price.[5] For the actual realized costs and environmental benefits draws employed in the experiment, these uniform price auction rules extract 72.4 percent of the maximum possible abatement under full cost revelation.

Sellers costs are distributed independently in this laboratory environment, so independent private value auction theory for multiple-unit discriminative price auctions provides a benchmark approximation in the discriminative price auction treatment. Since sellers receive the price they offer, they clearly have an incentive to offer prices above costs. How much above costs they should offer depends on the number of sellers in the auction and the number of units accepted by the auctioneer. Our experiments employed N=8 sellers, and the sellers could infer over time from the rate that they successfully sold that typically the auctioneer purchased Q=5 units each period.[6] If, as a first approximation, sellers behave *as if* they know Q and that it is stable, and they prepare offers on each of their three units independently, we can estimate how much they will offer above cost based on standard results from Vickrey auctions (see, e.g., Cox, Smith and

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[5] An offer above cost could occasionally raise price in this environment because sellers' different projects have different environmental benefits and the auction has a monetary budget constraint. It is therefore possible to construct examples in which a seller could raise the offer price on one of her items above cost and have a different (higher environmental benefit) item accepted, which would in turn exclude different rivals' items and raise the uniform cutoff price. Sellers do not observe their projects' environmental benefits, nor do they observe the offers or costs of their rivals; therefore, the incomplete information setting of our experiment—chosen to reflect reasonable incomplete information in any field implementation—makes the identification of this misrepresentation incentive rather implausible.

[6] Exactly Q=5 units were sold in 64 percent of the periods, and the Q sold was 4, 5 or 6 in 99 percent of the periods.

Walker, (1984), for the relevant formula). As shown below in Figure 1, the equilibrium offer function under these simplifying assumptions is nonlinear and substantially exceeds cost for low cost draws. For our parameters the low-range cost draws have equilibrium offers that are two or three times higher than cost based on this approximation. For the actual realized cost and environmental benefits draws employed in the experiment, these discriminative price auction rules extract only 54.8 percent of the maximum possible abatement if this offer function approximation is accurate. This is substantially below the benchmark prediction for the uniform price auction (72.4 percent), so we expect that uniform price auction rules will result in more efficient pollution abatement than discriminative price rules.

3. Results

Figures 1 and 2 present an overview of the offer data.[7] Figure 1 indicates that nearly all offers (99%) exceed cost as expected in the discriminative price auction. Most offers (73%) lie in a band between cost and cost+$1000, and 45% are within $500 of cost. Figure 2 shows that offers are dramatically different in the uniform price auction. The scatterplot of offers is more centered on the offer=cost reference line (indeed, the offer dots practically obscure this line). While there is some variation in offers relative to costs and nearly two-thirds (64%) of the offers are above cost, 80% of the offers are within $500 of cost. In the first subsection we summarize the impact of the auction rules and these offers on overall market performance, before we return to analyze the offer behavior in more detail in Subsection 3.2.

[7] This figure, and all the analysis that follows in this section, excludes a small number of offers that were obvious typographical errors. These occurred when sellers accidentally left a digit off of their offer, such as making an offer of 1,030 with a cost of 9,250 in the discriminative price treatment. This seller clearly intended a different offer (such as 10,300) since the offer of 1,030 virtually guarantees her a loss of 8,220, and this occurred in period 35 when this seller had plenty of experience. We excluded a total of 25 such typographical errors, out of 17,256 offers submitted (0.14%). We also lost all 24 offers from one period in one uniform price session due to a data recording error.

3.1 Overall Market Performance

Following Cason et al. (2003), we compare the auction formats using three market performance measures. These measures differ from the standard allocative efficiency measures typically applied in laboratory auction research. For the auction to be allocatively efficient, it must select the least costly projects. But in this policy application, to improve efficiency the auction also needs to select projects with high environmental benefits (quality). The first market performance measure, called P-MAR (for the *Percentage of Maximum Abatement Realized*), is the amount of pollution abatement realized by the auction mechanism, as a percentage of the highest amount of abatement that could be achieved with the government's auction budget. This maximum is based on the realized cost and benefit draws each period. This maximum abatement target could be achieved, for example, if the government knew both the cost and quality of each project and could implement its selected projects at their cost.[8]

Figure 3 shows that average P-MAR is greater in the discriminative price auction than in the uniform price auction in all 36 periods. The left side of Table 3 presents P-MAR averaged across periods, separately for each session. The lowest efficiency across the 10 discriminative price sessions (80.8%) is greater than the highest efficiency across the 10 uniform price sessions (74.2%), so a nonparametric Wilcoxon test based on one (statistically independent) observation per session strongly rejects the hypothesis of equal efficiencies (p-value=0.0014).

[8] Sometimes this maximum abatement would occur in the discriminative price auction if all sellers offer their projects in the auction at cost. Cost-revealing seller behavior does not always result in maximum abatement, however. The auction ranks the offers on the basis of their offer/quality ratio, and selects those with the lowest ratios. This greedy algorithm does not always result in the maximum abatement achievable for a fixed budget, due to the discrete set of projects acceptable in any auction period. Some higher abatement projects could be excluded from the auction allocation due to a cost that exceeds the fixed budget, while higher offer/quality ranking projects are accepted because of their lower overall cost. Consequently, some rearrangement of the selected projects can sometimes modestly increase the total abatement realized. To determine the selected projects that maximize pollution abatement, we calculated the total abatement for the 4^8=65,536 possible project combinations each period, and determined the greatest abatement among all the affordable project combinations. If all sellers offered their projects at cost, then the discriminative price auction selects the combination of projects that maximize abatement in 12 of the 36 periods. In 28 of the 36 periods, full cost revelation achieves at least 95 percent of the maximum possible abatement.

The regression shown in the first column of Table 4 presents additional parametric evidence that controls for other factors such as experience (time period) and subject pool. These panel regressions are based on a random effects error structure, with the session representing the random effect, in order to account for the correlation of market outcomes within a session. We include a dummy variable for the experiment site to account for any cultural or demographic differences across subjects. We also include ln(period) to allow the model to capture differences in performance across periods. The negative and highly significant estimate on the uniform price treatment dummy variable indicates that P-MAR efficiency is about 15 percentage points lower in the uniform price auction than in the discriminative price auction. Although Figure 3 does not indicate any pronounced trend over time, the positive and significant ln(period) term indicates that performance improves modestly across periods.

The second market performance measure provides an alternative summary of the auctions' ability to obtain the most abatement for the auction budget. We use P-OCER (for the *Percentage of Optimal Cost-Effectiveness Realized*) to refer to the actual quantity of abatement per dollar spent in the auction, as a percentage of the quantity of abatement per dollar spent in the "maximal abatement" solution to this problem described above. It differs from P-MAR because different amounts are spent in this auction when it selects a discrete set of projects. Presumably the unspent resources have some alternative value, so a reasonable objective is to maximize the abatement per dollar.

Figure 4 and the middle of Table 3 show that P-OCER, like P-MAR, is uniformly higher in the discriminative price auction than in the uniform price auction (Wilcoxon p-value=0.0014). The regression in the second column of Table 4 indicates that P-OCER efficiency is on average about 11 percentage points higher in the discriminative price auction. The positive and significant ln(period) term indicates that like P-MAR, P-OCER increases across time.

The third performance measure is seller profits. Seller profits represent money "left on the table" that the government "overspends," relative to the actual cost of implementing the land use changes. Therefore, lower seller profits are better from the government's perspective.

Figure 5 shows that sellers always earn higher profits on average in the uniform price auction, and in some periods their earnings are dramatically higher—even double the profits of the discriminative price auction. The right side of Table 3 shows that similar to the efficiency calculations, the highest average seller profits in the discriminative price auction (4840) is less than the lowest seller profits in the uniform price auction (5467), so the Wilcoxon test also strongly rejects the hypothesis of equal seller profits across treatments (p-value=0.0014). The seller profits regression model in the third column of Table 4 also mirrors those of the abatement efficiency models. Seller profits are significantly higher in the uniform price auction, by over 3,000 experimental dollars per period on average. The negative ln(period) variable indicates that these profits fall over time, however. Overall, the results in Figures 3 through 5 and Tables 3 and 4 indicate that market performance is lower in the uniform price auction.

3.2 Offer Behavior

In this section we examine the individual offers made by sellers by estimating empirical offer functions that relate offers to cost draws. First, however, recall that our design employed the same set of cost draws across all 20 sessions; i.e., we use the same set of 8 sellers × 3 items × 36 periods = 864 cost draws in each session. Thus, we can pair the same cost draws for each of the 10 pairs of sessions and compare the corresponding offers across treatments. This simple and direct comparison between the offers indicates that offers are on average 572 experimental dollars higher in the discriminative price session (standard error of the mean = 43).

Table 5 presents random effects regressions of seller offer functions separately for the two treatments. Columns 1 and 2 report the results for the discriminative price treatment and column 3 presents the uniform price treatment. The dependent variable is the seller's offer price, and the explanatory variables include costs faced by sellers for the different projects, a dummy variable for the site of the experiment, and time (the natural logarithm of the period number). We report both linear and nonlinear specifications for the discriminative price treatment, since the theoretical approximation in Figure 1 suggests a nonlinear specification for this institution.[9] Note, however, that the nonlinear term ($cost^2$) is not significantly different from zero.

The results show that there is a strong positive relationship between the project cost and the offers in both the uniform and discriminative price treatments. In fact, the coefficient on the cost variable is not significantly different from one for either auction format, indicating a similar one-to-one relationship between costs and offers in both treatments. These estimated offer functions instead differ in their intercepts. The intercept in the uniform price auction is not significantly different from zero, so combined with the cost coefficient not different from one these estimates support the conclusion that sellers on average made offers equal to cost. That is, sellers' behavior on average is consistent with the revelation incentives for this auction institution discussed at the end of Section 2.

By contrast, the intercept in the discriminative price auction is significantly greater than zero. The estimate indicates that offers were on average at least 1,000 experimental dollars above cost. Figure 1 displays a quadratic offer function fit through all the offers in this treatment, and it

[9] In particular, the theoretical approximation shown in Figure 1 is fit accurately with the quadratic specification $Offer = 7573 - 0.429Cost + 0.000067Cost^2$.

shows that on average the relationship between offers and costs is approximately linear. More importantly, this figure illustrates that sellers of low-cost projects in this incomplete information environment did not overstate their costs when submitting offers nearly as much as predicted by our benchmark approximation. These low-cost projects are particularly important for the overall efficiency and abatement realized in the auction, since they are most likely to be accepted by the auctioneer. Sellers offered these projects at prices closer to costs than we predicted, which is why the discriminative price auction performed better than the uniform price auction.

4. Discussion

Auctions allow an environmental regulator and landholders to use information about environmental benefits and land use management costs to achieve improvements in the environment. In the auctions testbedded here the agency uses public resources to subsidize land use changes that aim to reduce pollution. It is important therefore to ensure that the agency's environmental budget is well spent and this is where the details for the actual design of the auction become critical.

The laboratory auctions reported in this paper compare uniform price allocation rules with discriminative price rules. The offer function estimates indicate that offers were not significantly different from costs in the uniform price treatment, so sellers on average made offers in this auction format that were consistent with the cost-revelation incentives of this institution. Nevertheless, this auction format does not achieve full efficiency, since the uniform price was set by the first rejected seller's offer, and all successful sellers received this price per unit of quality. Since successful sellers receive prices that exceed their offers and offers were approximately equal to costs, prices exceed costs and some inefficiency occurred.

The offer function estimates indicate that offers exceed costs by at least 1,000 experimental dollars on average in the discriminative price treatment, and that each increase in costs by one dollar is matched with an increase in the offer by one dollar. Prices are set equal to offers, so submitting offers above costs is the only way that sellers can earn positive profits in this auction institution. This auction is also not fully efficient, but the results indicate that the inefficiency and the amount sellers are "overpaid" relative to their project costs is lower in the discriminative price auction than the uniform price auction. This occurred because sellers did not "mark up" offers above cost as much as suggested by an approximation based on multi-unit discriminative auction theory.

It is important to emphasize that these conclusions are based on a particular parameterization of project costs, land uses and potential environmental benefits. We chose these parameters carefully to approximate the conditions for a specific environmental problem being considered for land use change auctions, but these conclusions may not hold in other situations. For example, intuition from auction theory suggests that the degree to which sellers submit offers above cost in the discriminative price auction should depend on the number of sellers (N) relative to the number of items purchased (Q). We do not believe that it is worthwhile to search for conditions that would reverse the efficiency ordering of uniform and discriminative price auctions. We believe such conditions exist, such as less competitive situations with higher Q relative to N. Instead, it is more useful to determine whether the ordering clearly established in this initial experiment continues to hold in other settings that approximate non-point source pollution in other regions and land uses. We plan to conduct such experiments in the near future to evaluate the behavioral robustness of these findings.

We should also emphasize that these laboratory testbed experiments represent only the first preliminary step in the long process from auction design to field implementation. Following the robustness checks with other parameters, it will be useful to conduct experiments with actual landholders, using the environmental terminology—and the relevant value judgments that environmental protection and property rights evoke in this population. The preferred auction design that emerges from these experiments can then be evaluated in small-scale field experiments with landholders, implementing actual land use changes. The preliminary results reported here suggest that uniform price auction rules may not perform better than discriminative price rules, even though they have better cost-revelation incentives.

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Appendix: Instructions for Uniform Price Auction Treatment (Discriminative Price Auction instructions are similar)

General

This is an experiment in the economics of decision making. The instructions are simple and if you follow them carefully and make good decisions you will earn money that will be paid to you privately in cash. All earnings on your computer screens are in Experimental Dollars. These Experimental Dollars will be converted to real Dollars at the end of the experiment, at a rate of ______ Experimental Dollars = 1 real Dollar. The important thing to remember is that the more experimental dollars you earn, the more real dollars that you take home at the end of the experiment.

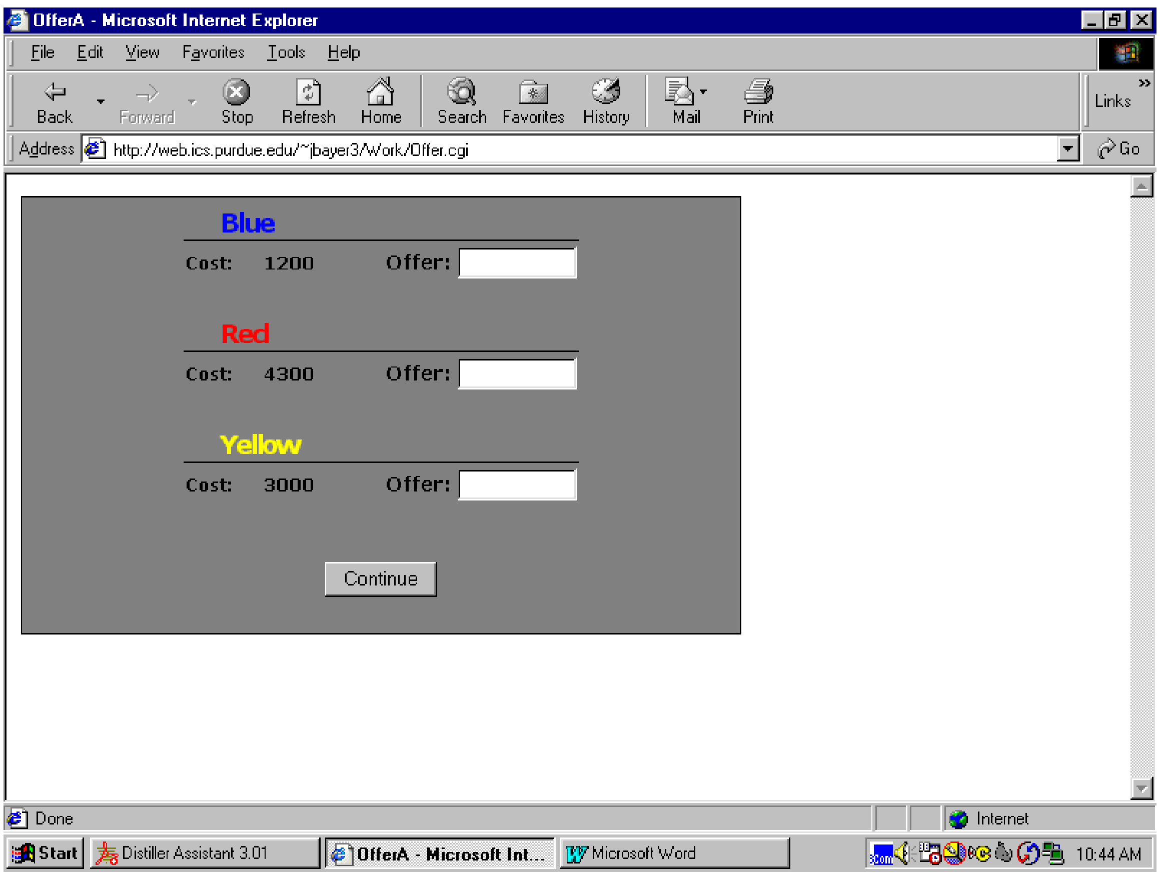
We are going to conduct a set of auctions in which you will be a seller in a sequence of periods. During each auction period you will sell up to one item. You have up to three types of items to sell, called Blue, Red and Yellow items. These items have different levels of "quality" that are valued differently by the experimenter, who is the buyer. Your quality levels may change from period to period, and they may be different from the quality levels of other participants. You can sell only one item per period, and if you sell that item then you must pay that item's cost. If you do not sell any item in a period then your earnings are zero for that period. Notice that you do not pay an item's cost unless you sell that item. Your costs may also change from period to period, and they may be different from the costs of other participants.

Your costs for each of the three types of items are displayed on your computer screen each period, as shown in the example figure on the next page. The profits from sales (which are yours to keep) are computed by taking the difference between the sale price of an item and the cost of that item. (How price is determined will be explained shortly.) That is, [your earnings = (sale price of item) – (cost of item)].

Suppose, for example, that the cost for your Blue item is 110. If you sell your Blue item at a price of 160, your earnings are:

Earnings = 160 – 110 = 50

Notice that if you sell an item for a price that is less than its cost, then you lose money on that sale.



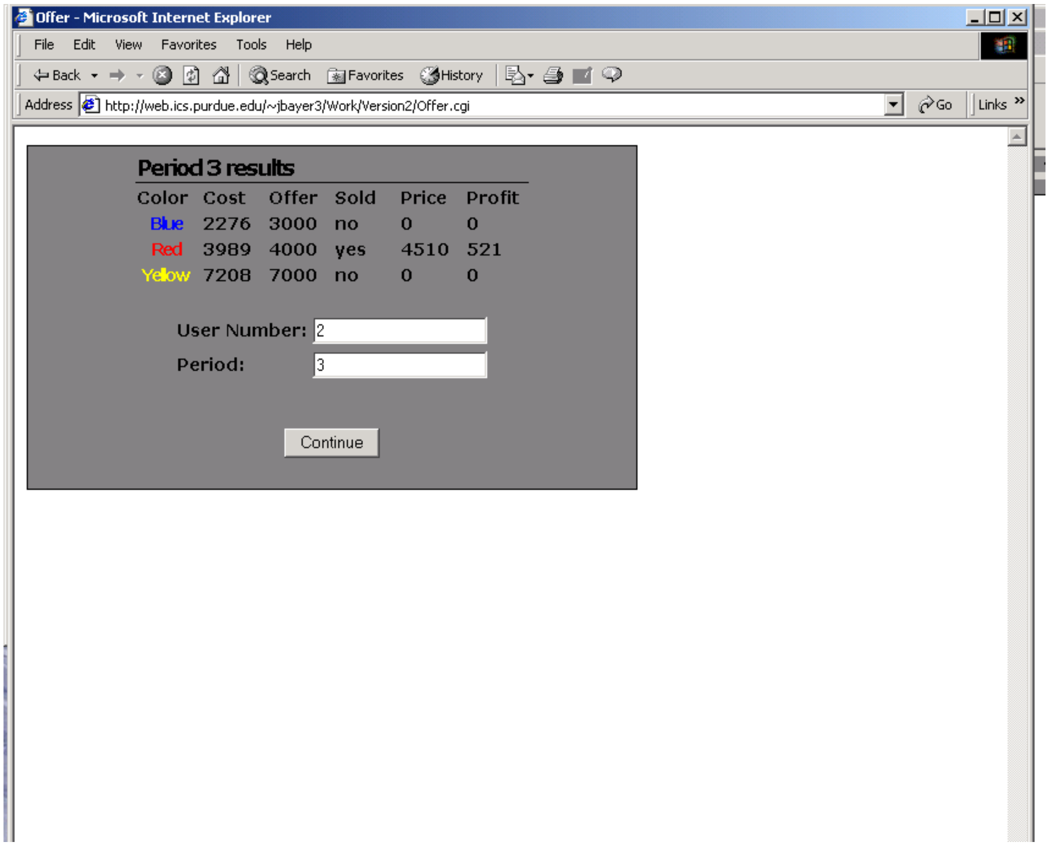
How Your Price is Determined

The price you receive if you sell an item and which (if any) item you sell is determined using a "sealed offer" auction. In each period you submit an "offer sheet" through your web browser, which lists the minimum amount that you wish to receive for each item. [Do not use a dollar sign when entering your offers on your web browser.] If you sell an item, you will receive a price that is greater than or equal to the price you indicated on your offer sheet for that item.

After everyone submits their offer sheets, the experimenter's computer then ranks the offers on the basis of the offer price and the quality of the items. The experimenter purchases the lowest priced items per unit of quality, spending all of the fixed and constant (and unknown to you) "budget" that is available in the auction. (In the case of a "tie," where two or more items are offered at the same per-quality-unit price but the experimenter cannot purchase them all, the computer randomly determines which item or items are purchased.) Sometimes you may sell an item that you offer at a higher price than some other item when that item has a higher quality. Sometimes you may not sell any item. Remember, the experimenter will buy no more than one item from each seller.

The price you receive if you sell an item is NOT determined by any of the offers you submit. Instead, everyone who sells an item in a period receives the same price per unit of quality, and this price is set by the lowest price per unit of quality submitted by a seller who had all of his or her offers REJECTED. Thus, your profit is not decreased by submitting offers lower than the lowest rejected offer that determines the price. The lower your offers the more likely you will have an offer below the lowest rejected offer and therefore make a successful sale.

In other words, by submitting lower offers you increase the likelihood that you make a sale, but lower offers do not directly reduce the price you receive since the price you receive is determined by a different (rejected) seller's offer. As long as you make offers that are no lower than your items' costs, you have no chance of losing money because if you sell an item you receive a price that is at least as high as your offer price. But if you make offers that are lower than your costs you run the risk of selling at a price less than your cost. This is because the lowest rejected offer could then also be less than your cost and result in a price for you that is less than your cost.



After each auction period, the experimenter will tell you when to click the "Continue" button to display the auction results. An example results screen is shown above. It indicates which (if any) item you sell by a "yes" in the Sold column. The results screen also displays the price you receive and the profit on the sale. Circle the color of the one item (if any) that is accepted in the column (1) of your Personal Record Sheet. Then enter the cost of this item, your offer price, the price you receive for the item, and your profit in the other columns of the record sheet. Use a calculator to keep track of your total (cumulative) Experimental Dollar earnings in the rightmost column (6) of your Record Sheet. The results page will automatically increment the period number by 1 for the next period, so after you write down your results on your Record Sheet you should simply press Continue to move to the next period.

Summary

- Seller earnings on a sold item = sale price of item – cost of item
- Sellers have three types of items, which can have different costs and quality levels valued differently by the experimenter (who is the buyer). Your costs are shown on your computer screen each period.
- Costs and quality levels may change from period to period and vary across sellers.
- Sellers submit offer prices for three types of items, but the experimenter will buy no more than one item from each seller.
- The experimenter purchases the lowest price items per unit of quality, and spends a constant budget in every auction.
- If you sell an item the price you receive is determined by the lowest price per unit of quality offered by a seller who has all of his or her offers rejected in the auction.

Are there any questions now before we begin the experiment?

Personal Record Sheet for User Number ____________

Period Number	Circle Color Sold (if any) (column 1)	Cost of Sold Item (column 2)	Offer Price for Sold Item (column 3)	Price Received for Sold Item (column 4)	Profit this Period (col. 4 – col. 2) (column 5)	Cumulative Profit (all Periods) (column 6)
1	Blue Red Yellow None					
2	Blue Red Yellow None					
3	Blue Red Yellow None					
4	Blue Red Yellow None					
5	Blue Red Yellow None					
6	Blue Red Yellow None					
7	Blue Red Yellow None					
8	Blue Red Yellow None					
9	Blue Red Yellow None					
10	Blue Red Yellow None					
11	Blue Red Yellow None					
12	Blue Red Yellow None					

Table 1: Cost and Environmental Benefit (Quality) Parameters

Note: Each of the eight sellers drew costs and benefits for three land use or management changes, one from each of the three categories indicated below, corresponding to 150 ha in land area.

Land Use or Management Change	Cost Range	Nitrogen Reduction Range
Filter/Buffer Strips	$15-65 per ha/year	0.35-0.875 kg/ha/year
Stabilize Soil Erosion	$15-65 per ha/year	0.28-1.05 kg/ha/year
Best Management Practices	$17.5-65 per ha/year	0.35-0.70 kg/ha/year

Sources: Argent, R.M. and Mitchell, V.G. (1998) FILTER: *A Nutrient Management Program for the Port Phillip Catchment*. Centre for Environmental Applied Hydrology, The University of Melbourne.
Documentation of "Best Management Practices" for Nutrient Reduction and Management in Dryland and Irrigated Agriculture, a report by Rendal McGuckian Consultants for Agriculture Victoria, Department of Natural Resources and Environment (1996).

Table 2: Example Costs, Environmental Benefits and Offers for Two Sessions (period 31)

Period	Seller ID	Project "Color"	Environmental Benefit	Project Cost	Offer	Offer/Benefit Ratio	Ratio Ranking	Price-setting Ratio	Project Sold?
Discriminative Price Auction									
31	1	blue	73.19	6120	7219	98.63	19		
31	1	red	124.46	2889	3988	32.04	1		Yes
31	1	yellow	79.85	5377	6476	81.10	17		
31	2	blue	55.99	4818	4988	89.09	18		
31	2	red	153.41	9047	9247	60.28	9		
31	2	yellow	95.24	7265	7410	77.80	16		
31	3	blue	80.64	8698	9200	114.09	20		
31	3	red	68.07	3089	3900	57.29	8		
31	3	yellow	97.7	5960	6600	67.55	12		
31	4	blue	91.66	4901	6901	75.29	15		
31	4	red	111.26	5688	7600	68.31	13		
31	4	yellow	79.51	8772	11777	148.12	24		
31	5	blue	98.3	2848	3600	36.62	2		Yes
31	5	red	85.86	2969	3500	40.76	3		
31	5	yellow	84.45	4687	5200	61.57	10		
31	6	blue	74.3	3287	4200	56.53	7		Yes
31	6	red	153.19	9037	10000	65.28	11		
31	6	yellow	86.11	9380	10200	118.45	22		
31	7	blue	91.9	6117	6617	72.00	14		
31	7	red	126.03	6217	6717	53.30	6		Yes
31	7	yellow	77.04	9689	10000	129.80	23		
31	8	blue	124.42	4859	6000	48.22	4		Yes
31	8	red	53.34	4899	6200	116.24	21		
31	8	yellow	102.6	3691	5000	48.73	5		
Uniform Price Auction									
31	1	blue	73.19	6120	6255	85.46	19	49.33	
31	1	red	124.46	2889	2999	24.10	4	49.33	Yes
31	1	yellow	79.85	5377	5888	73.74	18	49.33	
31	2	blue	55.99	4818	4100	73.23	17	49.33	
31	2	red	153.41	9047	8500	55.41	12	49.33	
31	2	yellow	95.24	7265	6500	68.25	16	49.33	
31	3	blue	80.64	8698	8698	107.86	21	49.33	
31	3	red	68.07	3089	3090	45.39	8	49.33	Yes
31	3	yellow	97.7	5960	6500	66.53	14	49.33	
31	4	blue	91.66	4901	4901	53.47	11	49.33	
31	4	red	111.26	5688	5688	51.12	10	49.33	
31	4	yellow	79.51	8772	8772	110.33	23	49.33	
31	5	blue	98.3	2848	1500	15.26	2	49.33	Yes
31	5	red	85.86	2969	1600	18.63	3	49.33	
31	5	yellow	84.45	4687	2500	29.60	5	49.33	
31	6	blue	74.3	3287	3300	44.41	7	49.33	Yes
31	6	red	153.19	9037	9050	59.08	13	49.33	
31	6	yellow	86.11	9380	9400	109.16	22	49.33	
31	7	blue	91.9	6117	6117	66.56	15	49.33	
31	7	red	126.03	6217	6217	49.33	9	49.33	
31	7	yellow	77.04	9689	9689	125.77	24	49.33	
31	8	blue	124.42	4859	4200	33.76	6	49.33	
31	8	red	53.34	4899	5000	93.74	20	49.33	
31	8	yellow	102.6	3691	1	0.01	1	49.33	Yes

Table 3: Overall Performance by Session

	Average P-MAR		Average P-OCER		Average Seller Profits	
	Discriminative Price	Uniform Price	Discriminative Price	Uniform Price	Discriminative Price	Uniform Price
	82.8%	69.4%	86.5%	81.6%	4722	6723
Ten	85.2%	72.6%	90.3%	82.1%	3923	6682
Individual	84.3%	72.4%	88.6%	83.1%	4383	6528
Sessions	80.8%	74.2%	86.9%	84.9%	4840	5467
in Each	88.6%	69.6%	94.6%	77.4%	2501	7828
Treatment	90.7%	70.4%	97.0%	79.9%	2108	7242
	88.8%	71.1%	95.5%	80.8%	2387	6593
	88.8%	71.7%	94.6%	80.5%	2555	6962
	88.4%	73.4%	94.4%	82.8%	2527	6098
	88.4%	67.2%	94.4%	80.2%	2932	5952
Treatment Mean	86.7%	71.2%	92.3%	81.3%	3288	6608

Table 4: Regression Models for Market Performance Measures

Variable	Percentage of Maximum Abatement Realized (P-MAR)	Percentage of Optimal Cost Effectiveness Realized (P-OCER)	Seller profits
Intercept	0.83*** (0.01)	0.89*** (0.01)	4785.56*** (407.9)
Dummy =1 if Uniform price treatment	-0.15 *** (0.01)	-0.11*** (0.01)	3307.06 *** (386.91)
Dummy = 1 if site = Melbourne	0.01 (0.01)	0.02*** (0.01)	-645.16 (386.92)
Ln(period)	0.01*** (0.002)	0.01*** (0.003)	-436.96*** (86.95)
Observations	694	694	694
R-squared	0.57	0.38	0.41

Notes: Standard errors are in parentheses.
***: denotes a coefficient that is significantly different from zero at 1-percent.
All models are estimated with a random effects error structure, with the session as the random effect.

Table 5: Seller Offer Function Estimates

Variable	Discriminative (1)	Price Treatment (2)	Uniform Price Treatment (3)
Intercept	1415.94*** (113.92)	1631.19*** (189.84)	307.02 (235.14)
Costs	0.98*** (0.009)	0.90*** (0.058)	1.03*** (0.02)
$Costs^2$		0.0000067 (0.0000047)	
Dummy = 1 if site = Melbourne	-311.99*** (111.9)	-311.94*** (114.4)	329.97 (248.34)
Ln(period)	-141.76*** (22.91)	-141.79*** (22.90)	-157.12*** (43.68)
R-squared	0.58	0.58	0.28
Number of Observations	8621	8621	8610

Notes: Standard errors are in parentheses.
***: denotes a coefficient that is significantly different from zero at 1-percent.
All models are estimated with a random effects error structure, with the subject as the random effect.

Figure 1:

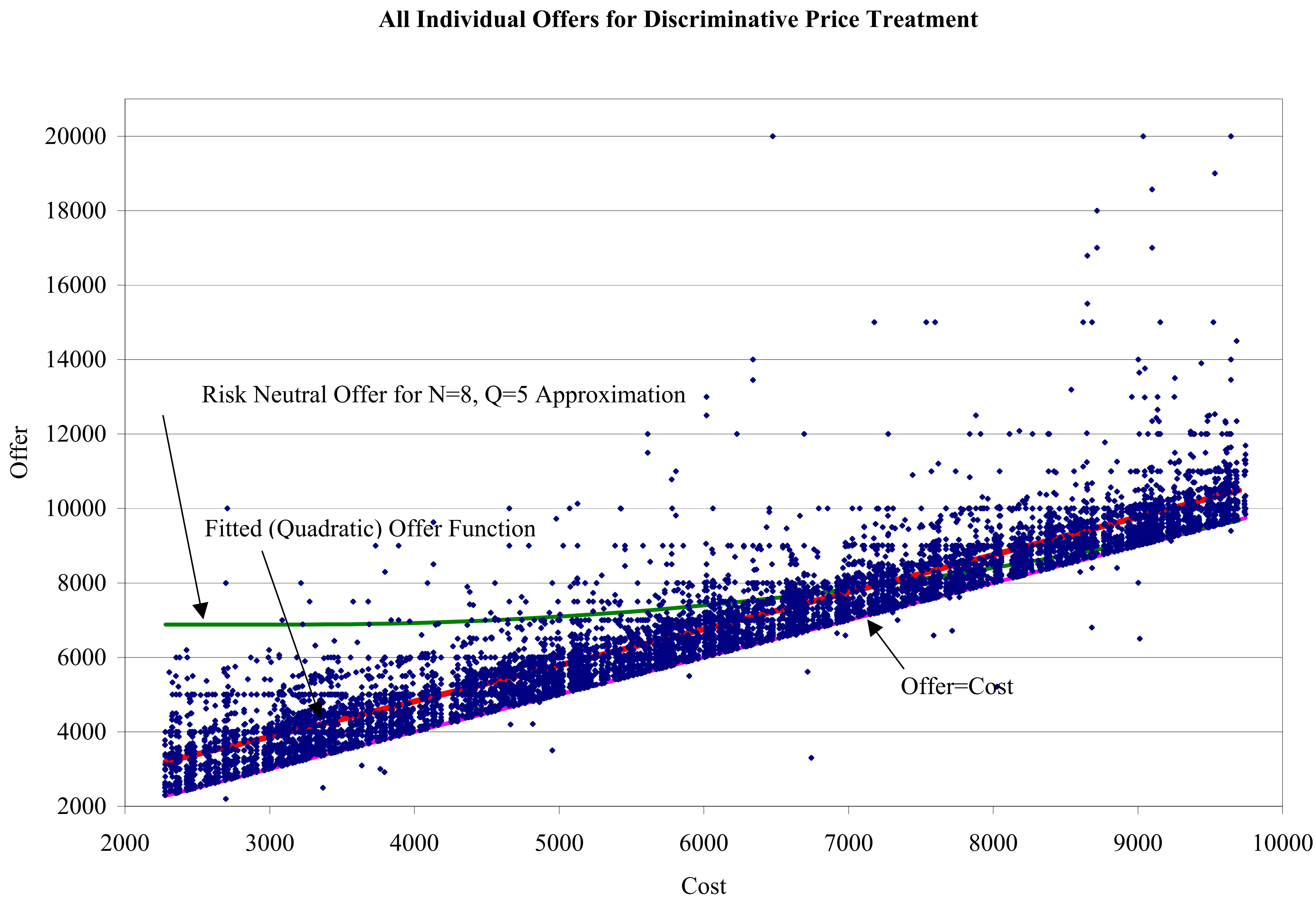

All Individual Offers for Discriminative Price Treatment
Offer
Cost
20000
18000
16000
14000
12000
10000
8000
6000
4000
2000
2000
3000
4000
5000
6000
7000
8000
9000
10000
Risk Neutral Offer for N=8, Q=5 Approximation
Fitted (Quadratic) Offer Function
Offer=Cost

Figure 2:

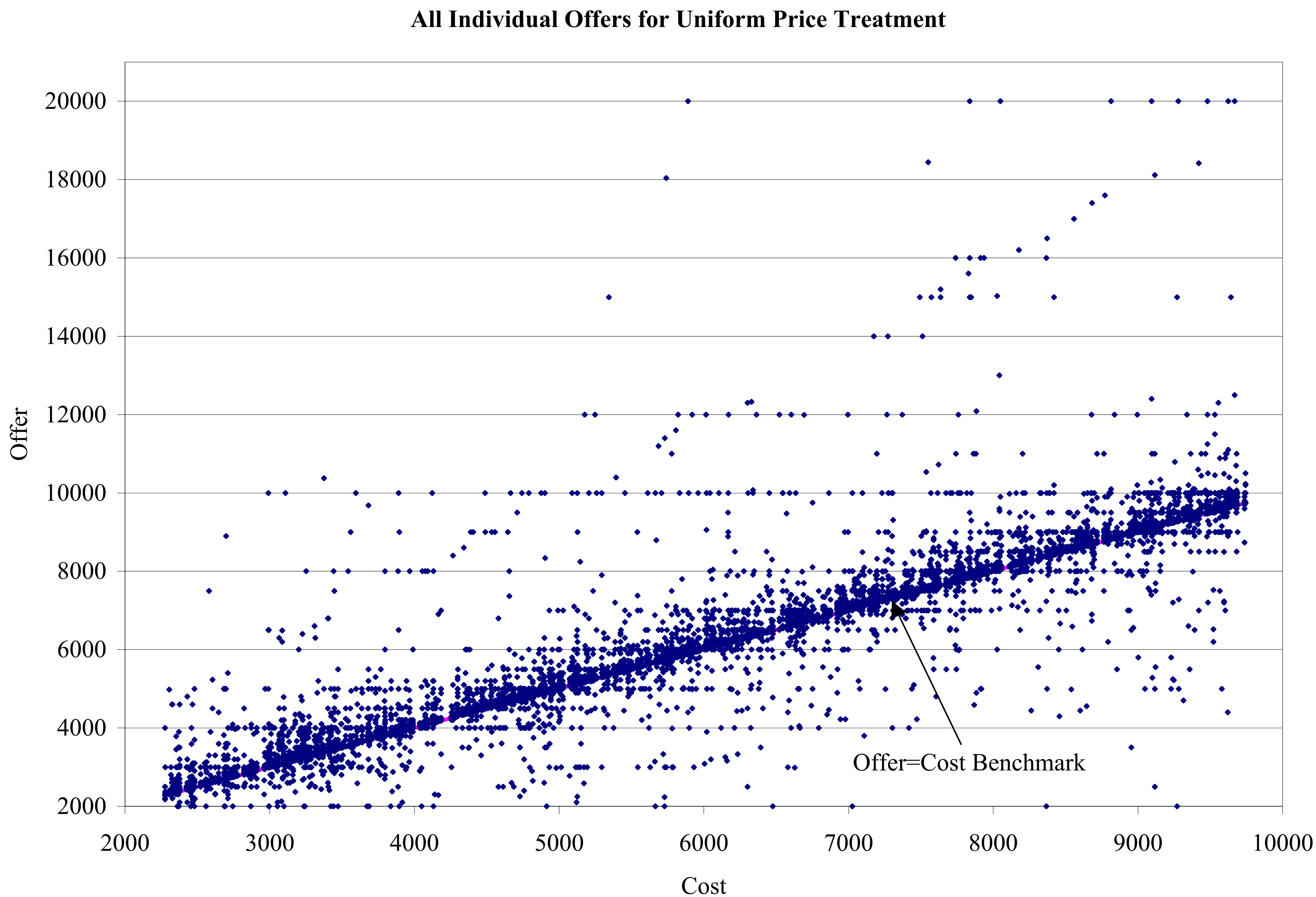

All Individual Offers for Uniform Price Treatment
Offer
20000
18000
16000
14000
12000
10000
8000
6000
4000
2000
2000
3000
4000
5000
6000
7000
8000
9000
10000
Cost
Offer=Cost Benchmark

Figure 3:

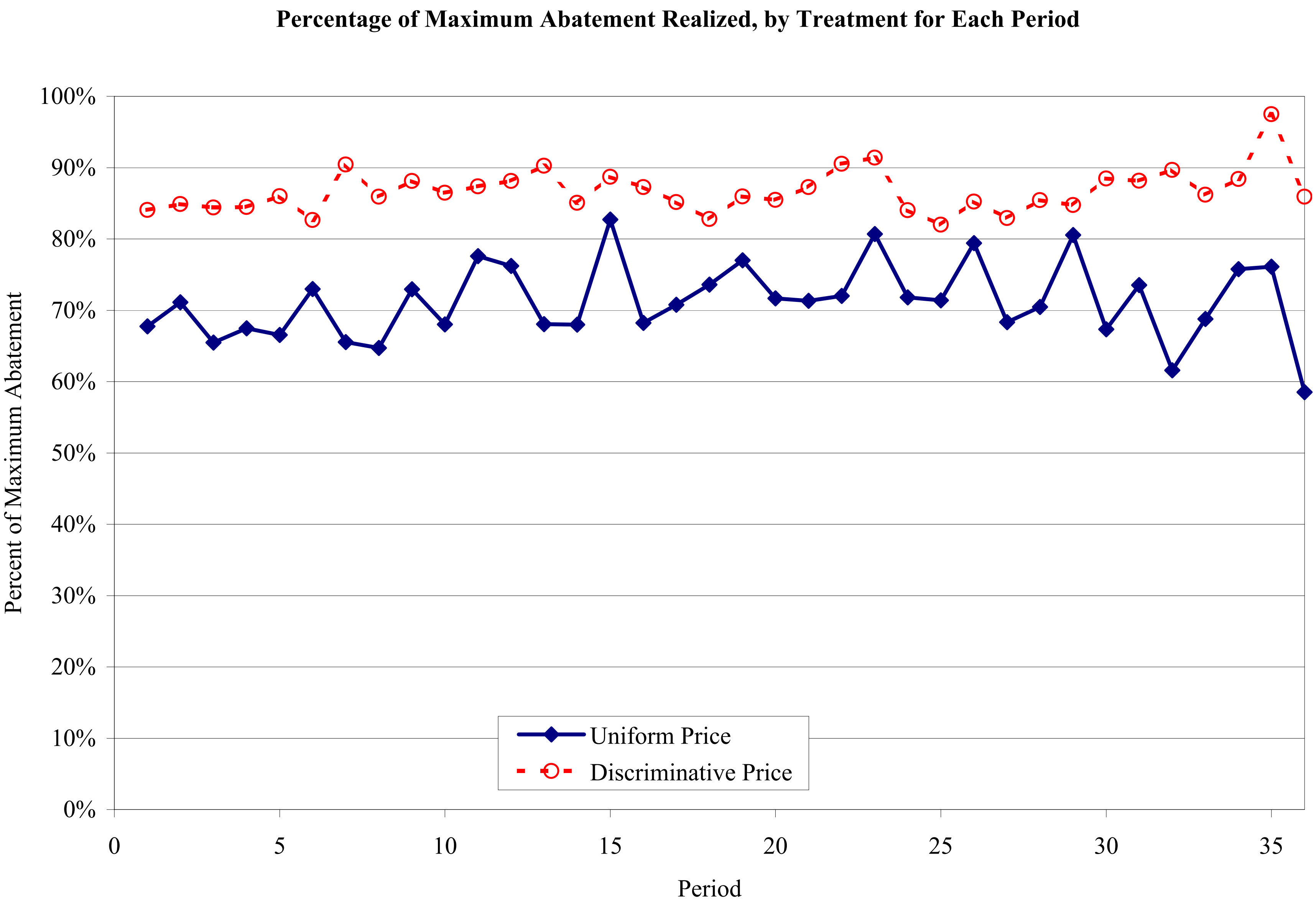
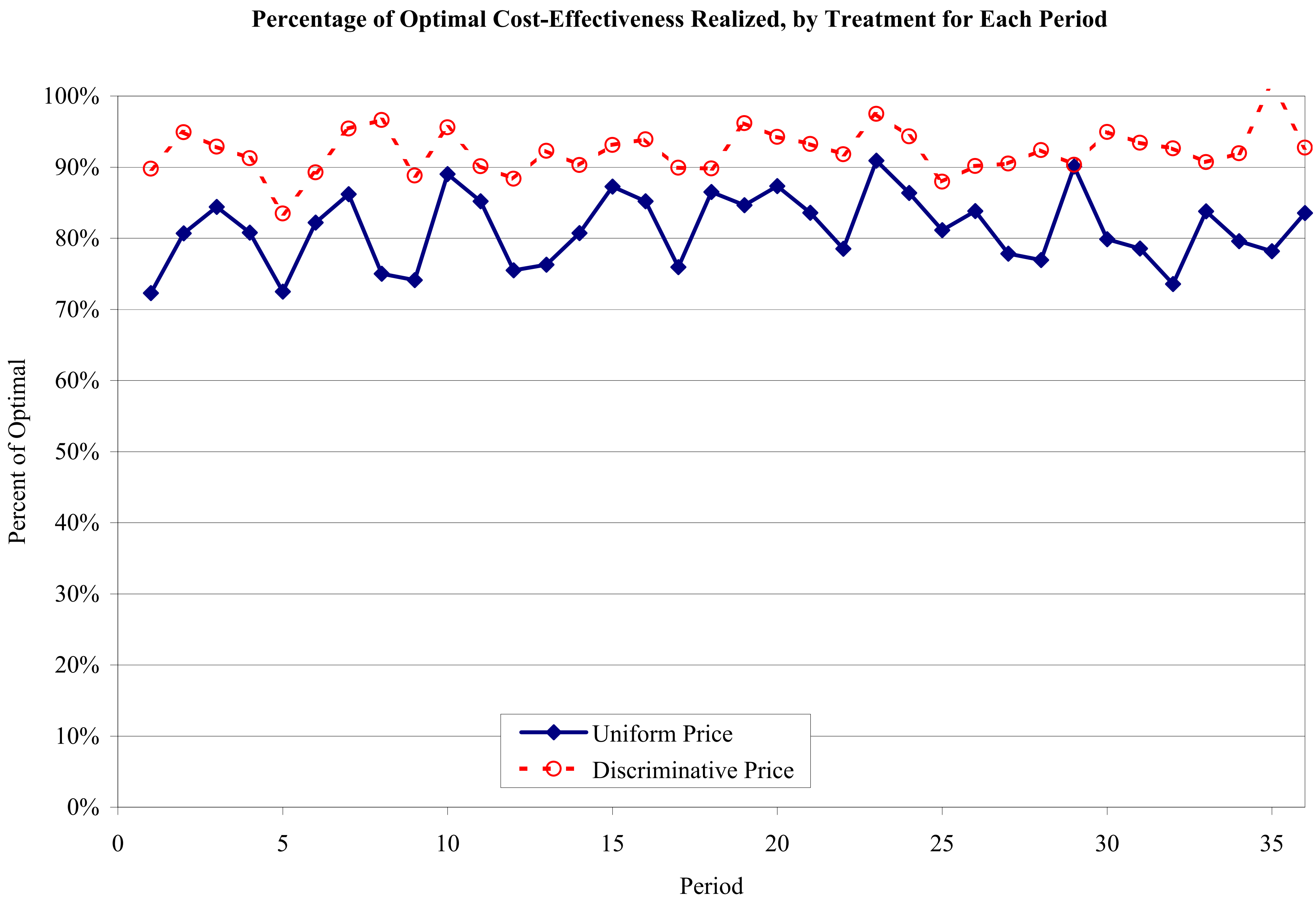
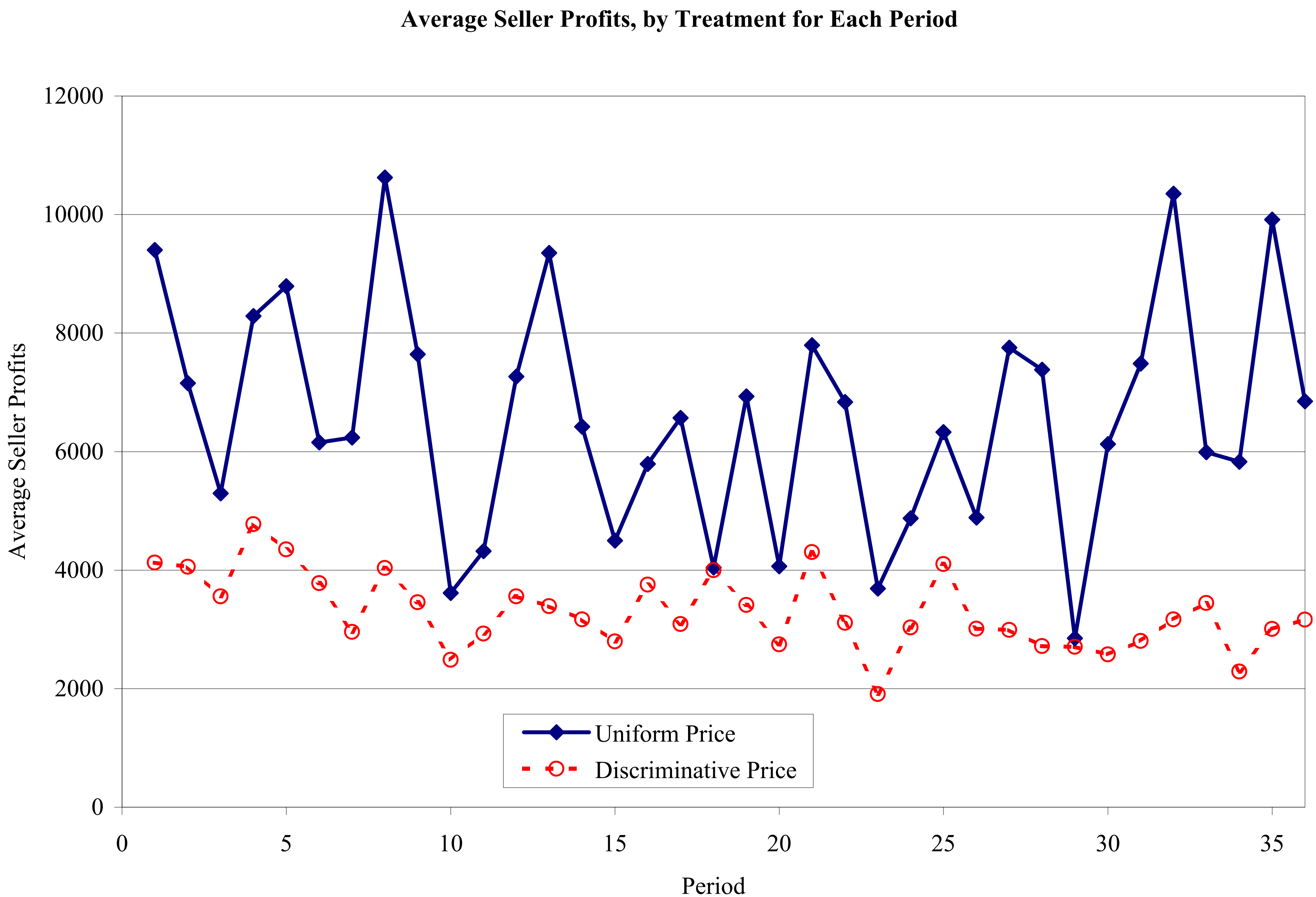

Percentage of Maximum Abatement Realized, by Treatment for Each Period
Percent of Maximum Abatement
100%
90%
80%
70%
60%
50%
40%
30%
20%
10%
0%
0
5
10
15
20
25
30
35
Period
Uniform Price
Discriminative Price

Figure 4:



Percentage of Optimal Cost-Effectiveness Realized, by Treatment for Each Period
100%
90%
80%
70%
60%
50%
40%
30%
20%
10%
0%
Percent of Optimal
0
5
10
15
20
25
30
35
Period
Uniform Price
Discriminative Price

Figure 5:


Average Seller Profits, by Treatment for Each Period
Average Seller Profits
Period
0
2000
4000
6000
8000
10000
12000
0
5
10
15
20
25
30
35
Uniform Price
Discriminative Price

May, 2003

An Experimental Analysis of Compliance Behavior in Emissions Trading Programs: Some Preliminary Results

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Abstract

While there is a substantial body of economic theory about compliance and enforcement in emissions trading programs, and readily available information about how existing emissions trading programs are enforced, there are no empirical analyses of the determinants of compliance decisions in emissions trading programs. This paper contains preliminary results from laboratory experiments designed to examine compliance behavior in emissions trading programs.

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May, 2003

An Experimental Analysis of Compliance Behavior in Emissions Trading Programs: Some Preliminary Results

1. Introduction

Emissions trading programs are an innovative approach to controlling pollution that continues to gather support from policy makers and members of the regulated community. Conceptually, emissions trading programs are quite simple, yet have very powerful implications. By exploiting the power of a market to allocate pollution control responsibilities, well-designed trading programs promise to achieve environmental quality goals more cheaply than traditional command-and-control regulations.

Despite the perceived advantages of market-based environmental policies over traditional command-and control approaches, it is clear that the efficiency gains expected of emissions trading programs will not materialize if these programs are not enforced well. In fact, the problem of enforcing market-based pollution control programs is seen by some as one of the most important barriers to the widespread implementation of emissions trading programs [Russell and Powell (1996)]. There is now a fair-sized literature that addresses certain aspects of the problem of noncompliance in emissions trading systems, most of which is theoretical in nature. Some of this literature focuses on the consequences of noncompliance [Keeler (1991), Malik (1990, 1992, 2002), and vanEgteren and Weber (1996)], while some recent work in this area is devoted to the question about how to design enforcement strategies for emissions trading programs [Stranlund and Dhanda (1999), and Stranlund and Chavez (2000)].

While the theoretical work on compliance and enforcement in emissions trading programs progressed through the 1990's, full-scale emissions trading programs were implemented; most notably the Sulfur Dioxide Allowance Trading Program—the centerpiece of the EPA's Acid Rain Program—and the Regional Clean Air Incentives Market (RECLAIM) Program of the South Coast Air Basin of California. Thus, it is now possible to compare the theory of enforcing emissions trading programs to the actual practice of doing so. [See Stranlund, Chavez, and Field (2002) for such a comparison].

While there is a substantial body of economic theory about compliance and enforcement in emissions trading programs, and readily available information about how existing emissions trading programs are enforced, there are no empirical analyses of the determinants of compliance decisions in emissions trading programs. To begin to fill this empirical gap, we have embarked on an effort to design and conduct laboratory experiments to test existing theories about compliance behavior in emissions trading programs. In this paper we report on our experimental designs and provide preliminary results from these experiments.

Over the last 40 years, laboratory experiments have provided researchers with a better understanding of markets and human behavior. Experimental research has a well-established framework that is widely accepted for testing existing theories and for the design and analysis of public policies [Smith (1982), Bjornstad *et al*. (1999)]. Although, experimental techniques have been used to evaluate many other policy initiatives, including some aspects of emissions trading programs [Cason (1995), Cason and Plott (1996), Ishikida *et al*. (1998), Isaac and Holt(1999)], and individual compliance decisions for income taxation [Beck *et al*. (1991), Alm *et al*. (1992a), Alm (1998)], to our knowledge these techniques have not yet been used to analyze compliance and enforcement of environmental policies, including emissions trading programs.

At the simplest level, enforcement of any regulation is characterized by two components: monitoring to detect violations and the assessment of sanctions if a violation is found. Conceptually, a risk-neutral firm's decisions about whether to comply with a fixed emissions quota should be determined by the relationship between its marginal costs of reducing emissions and the marginal expected penalty it faces for a violation. (The marginal expected penalty is the probability of being found in violation times the marginal sanction for the violation). The reason is simple: when facing an emissions standard, the benefit to a firm of emitting more than the standard allows is the cost it would have to incur to reduce its emissions to satisfy the standard. Therefore, a firm's marginal control costs exactly indicate its marginal benefit of non-compliance to the standard.

Firms' compliance incentives when emissions quotas are tradable are quite different. Since compliance in this setting means holding enough permits to cover emissions, a competitive firm's marginal benefit of non-compliance is what it would have to spend for permits to make sure it is compliant; that is, the prevailing permit price. Furthermore, firms in an emissions trading program are linked together through the functioning of the permit market, whereas they are largely independent under command-and-control policies. Thus, because an individual firm's compliance decision is made by comparing the prevailing permit price to the marginal expected penalty, which summarizes the regulatory enforcement strategy the firm faces, enforcement decisions and how they impact compliance behavior are important determinants of prevailing permit prices. This suggests that any analysis of compliance behavior in emissions trading programs must examine both the direct effects on individual decisions from changes in say enforcement strategies, aggregate standards, and other exogenous factors, as well as the indirect effects that work through changes in prevailing permit prices.

The rest of this paper proceeds as follows: In the next section we specify the hypotheses about market prices and compliance decisions that will be tested with the experimental data. In the third section, we lay out our experimental design. We include ten different experimental treatments that vary according enforcement strategies and aggregate emissions standards. In the fourth section, we present some preliminary results from these experiments. At this date, these experiments are incomplete and the results have not been subjected to rigorous statistical tests. Therefore, the results we present should not be taken as conclusive. However, they do suggest that a fair number of theoretical hypotheses about compliance behavior are likely to be supported by the experimental data.

2. Hypotheses

The results we present address several hypotheses about how prevailing permit prices and compliance choices vary with changes of an aggregate emissions standard and changes in marginal expected penalties. The comparative static analysis that generated these hypotheses are in Appendix A.

Hypothesis 1: *The market price for permits should be decreasing as the aggregate standard increases.*

The first hypothesis is a simple test of a basic economic prediction: all else equal, as the supply of permits increases, the price of a permit should decrease.

Hypothesis 2: *Aggregate violations should be decreasing as the aggregate standard increases.*

Because price is a key determinant of compliance decisions in emissions trading programs, changes in the aggregate standard will also change aggregate levels of noncompliance. A higher aggregate standard will reduce aggregate violations, because the lower permit price will reduce firms' incentives toward noncompliance.

Hypothesis 3: *Aggregate violations should be decreasing as the marginal expected penalty is increased.*

Of course, for a fixed aggregate standard, aggregate violations will respond to changes in the enforcement strategy as well. A higher marginal expected penalty (either a higher probability that a violation will be discovered, or increased marginal penalties for violations) implies a reduced incentive toward noncompliance.

Hypothesis 4: *The market price for permits should be increasing as the marginal expected penalty increases.*

Enforcement strategies will also affect firms' demands for emissions permits. Imagine an emissions trading program in which there is a fair amount of noncompliance. To reduce the amount of noncompliance, suppose that the marginal expected penalty is increased. This will motivate the noncompliant firms to demand more permits to reduce the magnitude of their violations, which will put upward pressure on the equilibrium permit price.

Hypothesis 5: *For two enforcement strategies that generate the same marginal expected penalty schedules, but with different monitoring probabilities and marginal penalty schedules, permit prices and rates of non-compliance should be identical.*

A basic result from the economic theory of law enforcement is that the probability of punishment and the severity of punishment are perfect substitutes for deterring noncompliance by risk-neutral agents [Becker (1968)]. However, risk-averse agents will be deterred more effectively by the severity of punishment, while the reverse is true for those who are risk seekers. Experimental results reported in Block and Gerety (1995) and Anderson and Stafford (2003) suggest that, at least in experimental settings, sanctions have a qualitatively larger effect on compliance behavior than auditing probability. Nearly all of the theory of compliance in emissions trading programs assumes risk-neutral firms.[1] We will address the issue of the relative impacts of monitoring and punishment with this hypothesis.

[1] Malik (1990) appears to be the sole exception.

Hypothesis 6: *Individual violations in a permit market should be identical if firms are monitored with the same probability and they face the same penalties, even though they have different marginal abatement costs.*[2]

Stranlund and Dhanda (1999) argue that the differences in the size of individual violations of risk-neutral firms that trade emissions permit competitively should be independent of differences in their abatement costs. Thus, if two firms are audited with the same probability and the same enforcement effort is applied to each, they both should have the same level of violation even though one may employ a less-advanced emissions-control technology or use a dirtier production process. This suggests that, since nothing distinguishes the compliance incentives of competitive firms in emissions trading programs, there is no reason for an enforcer to contemplate a targeted enforcement strategy. That is, provided that penalties are applied uniformly, the firms should be monitored with the same probability.[3] We will address the empirical validity of this conclusion with this hypothesis.

3. Experimental Design and Procedures

3.1 Experiment design

The experiments are designed to test hypotheses about the different factors that might influence a firm's compliance decisions when emissions permits are tradable. During each period of the experiment, subjects simultaneously choose to produce units of a fictitious good and trade in a market for permits to produce the good. Participants can produce as many units of the good as they wish (subject to production capacity constraints) regardless of the number of permits that they own. However, at the end of the period, each individual is audited with a known probability. If an individual is audited and found to be non-compliant (i.e., total production exceeds permit holdings), then a penalty is applied. The treatment variables in this paper are the aggregate standard (or supply of permits), the probability of audit, and the marginal penalty function.

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[2] This result should extend to differences in initial allocations of permits as well. We plan to test this hypothesis, but have not yet run the experiments we have designed to do so.

[3] This result does not hold with emission standards. Garvie and Keeler (1994) show that firms with higher marginal abatement costs should be monitored more closely, because their incentives for noncompliance are greater than firms with lower marginal abatement costs.

Subjects received a benefit from their choice of production, q, according to the "Earnings from Production" schedules shown below in Table 1. If q is thought of as emissions, these marginal benefit functions are marginal abatement costs functions. Note that Type-A subjects have higher marginal benefit functions than Type-B subjects. Each experiment consisted of four Type-A subjects and four Type-B subjects. Subjects could choose any level of production up to a capacity constraint, which was eight units for Type-A subjects and 17 units for Type-B subjects.

To be compliant, subjects were required to possess permits, l, to cover their production choices. Limiting the number of permits put into circulation imposed a cap on aggregate production. We chose two aggregate standards: one high ($Q_H = 56$) and the other low ($Q_L = 28$). In the high aggregate standard experiments, each of the eight subjects in an experiment received an initial allocation of seven permits. In the low aggregate standard experiments, each of the four Type-A firms was allocated three permits, and the four Type-B firms were each given four permits.

To check for compliance, subjects' records were examined with a known probability π. If a subject was examined and was found to be non-compliant; that is, $q > l$, they were penalized according to a penalty schedule generated from a quadratic penalty 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 penalty.

Table 1. Earnings from production for each subject type
(E$ for each unit permit shortfall)

Earnings from Production (E$ for each unit produced)		
Unit Produced	**Type-A**	**Type-B**
1st	17	16
2nd	16	14
3rd	15	12
4th	14	10
5th	13	8
6th	12	6
7th	11	4
8th	10	2
9th	9	
10th	8	
11th	7	
12th	6	
13th	5	
14th	4	
15th	3	
16th	2	
17th	1	

Notes

- Earnings from Production are expressed as marginal, not total, dollars.
- The Earnings from Production schedule is a discrete approximation to the quadratic benefit function $b(q) = \alpha q - (\beta/2)q^2$, where α and β are positive constants, chosen in part to guarantee that $b(q) > 0$ for all feasible levels of production, q. The benefit function parameters are: $\left[(\alpha_A = 17, \beta_A = 1), (\alpha_B = 16, \beta_B = 2)\right]$. The subscripts A and B denote subject type.

By changing the parameters of the marginal expected penalty function, $\pi f' = \pi[F + \phi(q - l)]$, we developed four enforcement strategies which we labeled *High*, *Med*(π_H), *Med*(π_L), and *Low*. (The tag *Med* should be read "medium"). The *High* marginal expected penalty (*MEP*) function was designed to induce perfect compliance to the aggregate standards, Q_L and Q_H, using a high monitoring probability ($\pi_H = 0.70$) and a relatively high marginal Permit Shortfall Penalty function. The treatments *Med*(π_H) and *Med*(π_L) generate the same marginal expected penalties, but *Med*(π_H) uses the high monitoring probability and a relatively low marginal penalty function, whereas *Med*(π_L) uses a low monitoring probability ($\pi_L = 0.35$) and a higher marginal penalty function. Subjects were expected to choose to be noncompliant when facing both of these medium marginal expected penalty functions. The marginal expected penalty function *Low* was constructed to be the weakest enforcement strategy, with the low monitoring probability, π_L, and a low marginal penalty function. Enforcement parameter values were chosen, in part, so that the marginal expected penalty functions are parallel to each other—each has a slope of approximately one. The parameters for each experiment are shown in Table 2.

Table 2. Enforcement strategy parameters

	Aggregate Standard	
Enforcement Strategy	$Q_L = 28$	$Q_H = 56$
High MEP	(π_H, ϕ_1, F_1)	(π_H, ϕ_1, F_1)
Med(π_H) *MEP*	(π_H, ϕ_1, F_2)	(π_H, ϕ_1, F_2)
Med(π_L) *MEP*	(π_L, ϕ_2, F_3)	(π_L, ϕ_2, F_3)
Low MEP	(π_L, ϕ_2, F_4)	(π_L, ϕ_2, F_4)

The enforcement parameter values are $(\pi_L,\ \pi_H) = (0.35,\ 0.70)$, $(\phi_1,\ \phi_2) = (1.43,\ 2.90)$ and $(F_1,\ F_2,\ F_3,\ F_4) = (17.5,\ 6,\ 12,\ 2)$. The values for ϕ and F generate the Permit Shortfall Penalty schedules shown in Table 3.

Table 3. Permit Shortfall Penalties
(E$ for each unit permit shortfall)

Permit Shortfall	High	*Med(π_H)*	*Med(π_H)*	*Low*
1st unit	18.9	7.4	14.9	4.9
2nd	20.4	8.9	17.8	7.8
3rd	21.8	10.3	20.7	10.7
4th	23.2	11.7	23.6	13.6
5th	24.7	13.2	26.5	16.5
6th	26.1	14.6	29.4	19.4
7th	27.5	16.0	32.3	22.3
8th	28.9	17.4	35.2	25.2
9th	30.4	18.9	38.1	28.1
10th	31.8	20.3	41.0	31.0
11th	33.2	21.7	43.9	33.9
12th	34.7	23.2	46.8	36.8
13th	36.1	24.6	49.7	39.7
14th	37.5	26.0	52.6	42.6
15th	39.0	27.5	55.5	45.5
16th	40.4	28.9	58.4	48.4
17th	41.8	30.3	61.3	51.3

Notes
- Permit Shortfall Penalties are expressed as marginal, not total, dollars.
- The Permit Shortfall Penalty schedule was the same for each subject type with the exception that since Type-B firms could only produce a maximum of eight units, only the first eight steps in the penalty function were displayed.
- The Permit Shortfall Penalty schedule is a discrete approximation to the marginal penalty function $f' = F + \phi(q - l)$.

Table 4 summarizes the experimental design in a 5×2 matrix, where *MEP* denotes Marginal Expected Penalty. Each cell in the table was repeated twice.[4] The two columns represent the different aggregate standards (or total number of permits available), while the five rows reflect the different enforcement strategies.

[4] We will be running a third repetition of each cell in the Fall. The data reported in this paper include two experiments per cell.

Table 4. Experimental design

Enforcement Strategy	Aggregate Standard	
	$Q_L = 28$	$Q_H = 56$
Forced Compliance	*A*	*B*
High MEP	*C*	*D*
Med(π_H) *MEP*	*E*	*F*
Med(π_L) *MEP*	*H*	*I*
Low MEP	*K*	*L*

In addition to the four *MEP* experiments that allowed non-compliance, we also ran a set of *Forced Compliance* experiments. By removing the ability to be non-compliant, and therefore any risks associated with a possible audit, these experiments provide a baseline against which the market outcomes of the other experiments can be compared. The *Forced Compliance* experiments are procedurally similar to other permit market experiments such as Cason *et al*. (1999) and Franciosi *et al*. (1999). During the period, subjects could only trade permits and did not make concurrent production decisions. Instead, production automatically occurred after the trading period ended, and production exactly equaled the minimum of the total number of permits owned or the maximum number of units that could be produced. (We permitted individuals to hold more permits than their maximum production capacity to allow for possible speculative trading). In both the *Forced Compliance* and *High MEP* treatments, firms are expected to be compliant, that is $q = l$. In the former treatment, this result is trivial since noncompliance is not possible. In the latter treatment, although the parameters are set such that a risk-neutral individual would choose to be perfectly compliant, noncompliant choices are possible. Because the competitive equilibrium outcomes in these two treatments are identical, this will allow us to draw some inferences about how permitting non-compliance affects individual decisions and market prices.

3.2 Experiment procedures

Table 5 summarizes the key aspects of the experiments.

Table 5. Experiment Summary

- Subjects
  - All subjects participated in a 2-hour training session prior to participating in real data sessions.
  - 54 University of Massachusetts students recruited from a pool of 116 trained subjects.
  - Paid $7 for participating, plus experiment earnings (mean $14, range $10-$17).
- Number and Type of Subjects
  - 8 subjects, 4 of each type
    - Type-A: High marginal abatement cost
    - Type-B: Low marginal abatement cost
- Periods and Length
  - 12 five-minute periods during which subjects could produce units and trade permits.
  - Data from first two periods were discarded.
- Production
  - Producing units generates "Earnings from Production" (i.e., redemption values).
  - Production allowed only during first four minutes of period.
  - Each unit produced sequentially; production takes 10 seconds/unit.
  - Maximum number of units a subject can produce: Type-A=17, Type-B=8.
- Permit Market
  - Permit market open for entire five-minute period.
  - Continuous double auction.
  - Permits cannot be banked for future use.
- Auditing
  - Each individual faced same probability π of being audited.
  - Random audits occur after production and market trading period is over.
  - Permit Shortfall Penalty function applied if audited and production exceeds permit holdings. The marginal penalties are increasing in the size of the shortfall.

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 $10 and $17, with a mean of $14. Earnings were paid in cash at the end of each experiment. Each experiment lasted about 2 hours.

The experiments were run in a computer lab using software designed in Visual Basic specifically for this research. To familiarize subjects with the experiments, we initially ran a series of training experiments. In the first stage of the trainers, students read online experiment instructions, which included interactive questions to ensure that students 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 training parameters. The data from the trainers were discarded.

For the real data sessions, we recruited participants from the pool of 116 trained subjects. Subjects were allowed to participate in multiple sessions. A total of 74 subjects participated in 20 eight-person market compliance experiments. Table 6 shows the distribution of the number of experiments in which an individual participated. The median and mode were two and one experiments, respectively. The maximum number of experiments was six, and the mean was 2.2.

Table 6. Distribution of the number of experiments in which an individual participated

Number of Experiments Participated In	Number of Subjects	Percent
1	29	39%
2	20	27%
3	15	20%
4	5	7%
5	4	5%
6	1	1%
Total number of subjects	74	100%

Prior to the start of the real data experiments, subjects were given a summary of the experiment instructions (see Appendix B). The experimenter read these instructions aloud and answered any questions. The review of the instructions took about 10 minutes.

Each experiment consisted of 12 identical five-minute rounds. At the start of each period, the eight subjects were each given an initial allocation of permits and E$10 in cash.[5] Data from the first two rounds of each experiment were discarded.

5 During the experiment, subjects earned experimental dollars (E$) that were converted to US dollars at a pre-announced exchange rate.

Each unit of the good was produced sequentially by clicking on a button that initiated the production process. Production of a single unit took 10 seconds. After production of the unit was completed the "Earnings from Production" were immediately added to the individual's cash balance. Subjects were able to "plan" future production by indicating the total number of units to produce. Once production of a unit was completed, if there were any "planned" units, the 10-second production process for the next would automatically begin. Subjects could increase or decrease their "planned" production, but units that were "in progress" or "completed" were committed and could not be changed. That is, subjects could alter planning decisions about units not yet produced, but they could not undo production of a good after the 10-second production process had begun.

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. Often in permit market experiments, perfect compliance is assumed (i.e., production exactly equals the number of permits owned at the end of the trading period) and subjects earn income based on their final permit holdings plus any net income from permit market trading [e.g., Cason *et al*. (1999), Franciosi *et al*. (1999)]. In our experiments, permits are useful because they are an instrument for choosing compliance rates, and they could also generate capital gains from speculative trading. Therefore, during the period and concurrent with the production decision, subjects also had the ability to alter their permit holdings by trading in a continuous double auction (CDA). In the CDA, individuals could submit bids to buy or asks to sell a single permit (provided that they had a permit available to sell). The highest bid and lowest ask price were displayed on the screen. A trade occurred whenever a buyer accepted the current ask or a seller accepted the current bid. After each trade, the current bid and ask were cleared and the market opened for a new set of bids and asks. The trading price history was displayed on the screen.

Each period lasted a total of five minutes. The permit market was open for the entire period, but production had to be completed in four minutes. The four-minute production time was more than sufficient for a subject to produce up to his or her capacity constraint. We provided the additional minute of permit trading after production was completed to give subjects a final opportunity to adjust their permit holdings. The computer screen displayed the time remaining for both production and the permit market.

As soon as a period ended, random audits were conducted and penalties were assessed. All information relating to audits penalties were private and not shared with the others in the experiment.

Since it was possible for individuals to lose money either through permit trading or permit shortfall penalties, we implemented a bankruptcy rule. If an individual's cash balance ever fell below negative E$800, he or she was declared bankrupt and was no longer allowed to participate. No subjects ever sustained a significant negative cash balance, let alone approached the bankruptcy threshold. We also instituted a price ceiling of E$20 above which offers to trade permits were not allowed. This ceiling was set above the highest possible "Earnings from Production" so had anyone paid this price for a permit she or he would have lost money. This constraint was non-binding as the maximum permit price in any experiment was E$14.

4. Results

In this section, we present some preliminary results from the experiments. At this date, the experiments are incomplete and the results have not been subjected to rigorous statistical tests. Therefore, the results we present should not be taken as conclusive. However, they do suggest that a fair number of theoretical hypotheses about compliance behavior are likely to be supported by the experimental data. Since we are primarily interested in equilibrium behavior, data from the first two periods were discarded to minimize the effects of learning, leaving us with 10 periods per experiment. We begin by presenting some simple descriptive statistics comparing competitive equilibrium and observed outcomes. We then make some observations about the hypotheses discussed above.

The competitive equilibrium outcomes presented in Table 7 assume that subjects are risk-neutral and trade in a perfectly competitive market. Note that the competitive equilibrium outcomes in the *Forced Compliance* and *High MEP* treatments are identical, likewise the *Med*(π_L) *MEP* and *Med*(π_H) *MEP* have the same competitive equilibrium. Table 8 contains the average observed outcomes for each subject type and treatment. The final permit balance, production quantity, and level of violations for each subject type are the mean of 80 observations per cell (two experiments, four subjects per type, 10 periods). In Table 8, the first line of each cell is the mean outcome from the two experiments. The second line is the percent difference between the mean outcome and the corresponding competitive equilibrium outcome in Table 7; a positive value indicates that the mean observed value exceeds the competitive equilibrium value.[6]

Table 7. Competitive Equilibrium Outcomes

Low Aggregate Standard (Q_L = 28)							
		Type-A			Type-B		
Enforcement Strategy	**Permit Price**	**Permits** l_A	**Production** q_A	**Violations** $v_A=q_A-l_A$	**Permits** l_B	**Production** q_B	**Violations** $v_B=q_B-l_B$
A. *Forced Compliance*	(12, 13)	5	5	not applicable	2	2	not applicable
C. *High MEP*	(12, 13)	5	5	0	2	2	0
E. *Med*(π_H) *MEP*	(8, 9)	6	9	3	1	4	3
H. *Med*(π_L) *MEP*	(8, 9)	6	9	3	1	4	3
K. *Low MEP*	6	(6, 7)	11	(5, 4)	(1, 0)	5	(4, 5)

High Aggregate Standard (Q_H = 56)							
		Type-A			Type-B		
Enforcement Strategy	**Permit Price**	**Permits** l_A	**Production** q_A	**Violations** $v_A=q_A-l_A$	**Permits** l_B	**Production** q_B	**Violations** $v_B=q_B-l_B$
B. *Forced Compliance*	8	(9, 10)	(9, 10)	not applicable	(5, 4)	(4, 5)	not applicable
D. *High MEP*	8	(9, 10)	(9, 10)	0	(5, 4)	(4, 5)	0
F. *Med*(π_H) *MEP*	(6, 7)	10	11	1	4	5	1
I. *Med*(π_L) *MEP*	(6, 7)	10	11	1	4	5	1
L. *Low MEP*	4	(10, 11)	13	(3, 2)	(4, 3)	6	(2, 3)

[6] For those cases in which the competitive equilibrium is a range, we used the value in the range that was closest to the mean value, i.e., how far is the mean value from just falling into the range.

Table 8. Average Observed Outcomes[a]

Low Aggregate Standard (Q_L = 28)							
		Type-A			Type-B		
Enforcement Strategy	**Permit Price**	**Permits** l_A	**Production** q_A	**Violations** $v_A=q_A-l_A$	**Permits** l_B	**Production** q_B	**Violations** $v_B=q_B-l_B$
A. *Forced Compliance*	12.64	4.81	4.81	not applicable	2.19	2.19	not applicable
C. *High MEP*	12.41	4.31	4.78	0.46	2.69	2.89	0.20
E. *Med*(π_H) *MEP*	9.54	4.54	7.56	3.03	2.46	4.81	2.35
H. *Med*(π_L) *MEP*	12.07	3.95	5.35	1.40	3.05	4.20	1.15
K. *Low MEP*	8.19	4.75	8.08	3.33	2.25	5.44	3.19
High Aggregate Standard (Q_H = 56)							
		Type-A			Type-B		
Enforcement Strategy	**Permit Price**	**Permits** l_A	**Production** q_A	**Violations** $v_A=q_A-l_A$	**Permits** l_B	**Production** q_B	**Violations** $v_B=q_B-l_B$
B. *Forced Compliance*	7.78	9.11	9.11	not applicable	4.89	4.89	not applicable
D. *High MEP*	7.67	9.00	9.16	0.16	5.00	5.09	0.09
F. *Med*(π_H) *MEP*	6.63	9.29	10.48	1.19	4.71	5.64	0.93
I. *Med*(π_L) *MEP*	6.79	9.49	10.69	1.20	4.51	5.20	0.69
L. *Low MEP*	3.59	8.36	11.83	3.46	5.64	7.44	1.80

a These averages are from two experiments for each treatment. Each experiment consists of 10 periods (we ran 12 periods and dropped the first two).

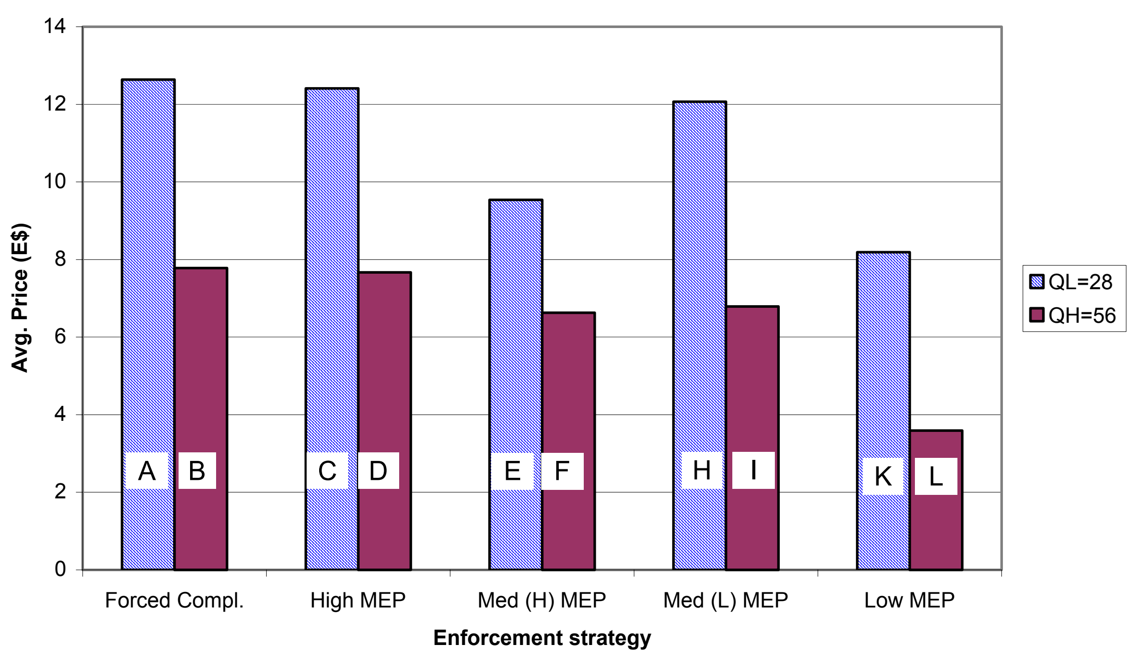
In six of the twelve experiments we ran (two experiments per treatment), the average permit price approximately equaled the competitive equilibrium price, and in five experiments the average price exceeded the equilibrium price. In only one of the 12 experiments was the average permit price below the equilibrium price.

Within each experiment the observed prices were relatively stable. As a measure of price dispersion, we calculated the mean absolute percentage price difference between the individual trade prices and the mean price for the experiment. This measure of dispersion ranged between 2% and 7%. With the same measure, Newell, Sanchirico, and Kerr (2002), found average price dispersion of 2 % for the SO_2 market over 2001-2002; 5% for nitrogen oxide trading in the northeastern U.S. over the same period, and 28% for the RECLAIM markets over 1995-2002. They also found that for the markets that make up New Zealand's individual transferable fishing quotas (ITQs), dispersion of ITQ lease prices averaged 35% in 1987 and 25% in 2000, while the average dispersion of ITQ sales prices was about 25% in 1987, falling to 5% in 2000. It is reassuring that that average price dispersion in our experiments is in line with, and sometimes much lower than, existing markets for tradable property rights, suggesting that our experimental markets are functioning reasonably well.

Hypothesis 1: *The market price for permits should be decreasing as the aggregate standard increases.*

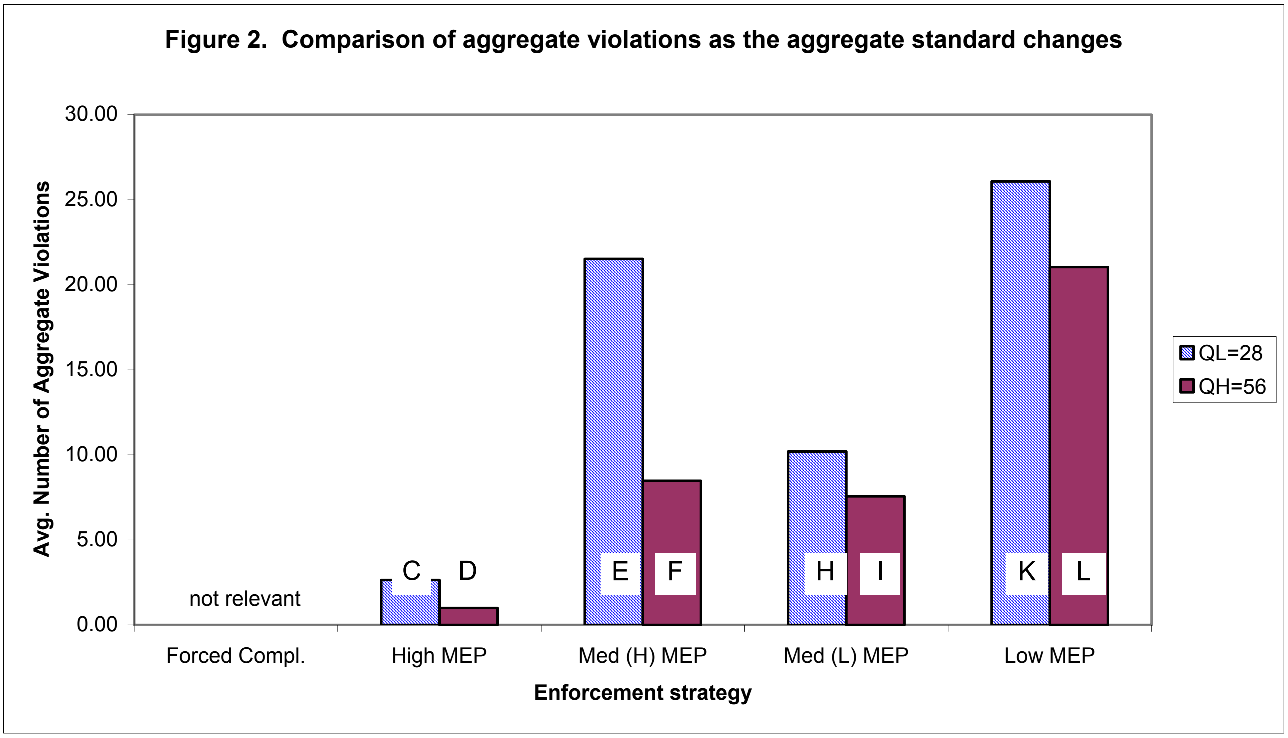
For each enforcement strategy, Figure 1 contains a simple pairwise comparison of the mean price for low and high aggregate standard (Q_L and Q_H) treatments. The letters on the bars (E, F, etc.) refer to the treatment cell from Table 4. Although the data in Table 8 suggest that observed prices may sometimes differ from the competitive equilibrium prices, the average price is moving in the hypothesized direction with respect to changes in the aggregate standard for all enforcement strategies—average prices are clearly lower when the aggregate standard is high (Q_H).

Figure 1. Comparison of average prices as the aggregate standard changes



Hypothesis 2: *Aggregate violations should be decreasing as the aggregate standard increases.*

Table 8 indicates that in the low aggregate standard experiments (Q_L), average rates of non-compliance are generally lower than the competitive equilibrium. In the high aggregate standard experiments (Q_H), however, the results are mixed. Figure 2 compares aggregate violations as the aggregate standard changes. As hypothesized, aggregate violations are lower with the higher aggregate standard when the subjects faced the *Med*(π_H) and *Low* marginal expected penalties. However, the reverse is true with the *Med*(π_L) marginal expected penalty. Recall from Tables 7 and 8 that, relative to the prediction for the H treatment ($8-9), the actual average price is quite a bit higher ($12.07). Furthermore, the predicted aggregate violation (24) for the H treatment is much higher than average aggregate violations (about 10). The high average price for this treatment is consistent with the low aggregate violation—low noncompliance implies stronger demand for permits and consequently higher permit prices.



Hypothesis 3: *Aggregate violations should be decreasing as the marginal expected penalty is increased.*

Figure 3 is a re-formatted version of Figure 2 to highlight how average aggregate violations vary with the marginal expected penalties. As hypothesized, aggregate violations are higher with the *Low* marginal expected penalty as compared to the *Med*(π_H) and *Med*(π_L) marginal expected penalties. Note again the low average aggregate violation for the H treatment.

Hypothesis 4: *The market price for permits should be increasing as the marginal expected penalty increases.*

Given an aggregate standard, weaker enforcement and higher noncompliance implies lower permit prices. Figure 4 highlights the how average permit prices change as the marginal expected penalty decreases. Consistent with Hypothesis 4, note that average permit prices for both the low and high aggregate standards (Q_L and Q_H) are lowest with the *Low* marginal expected penalty.

Figure 3. Comparison of aggregate violations as the MEP changes

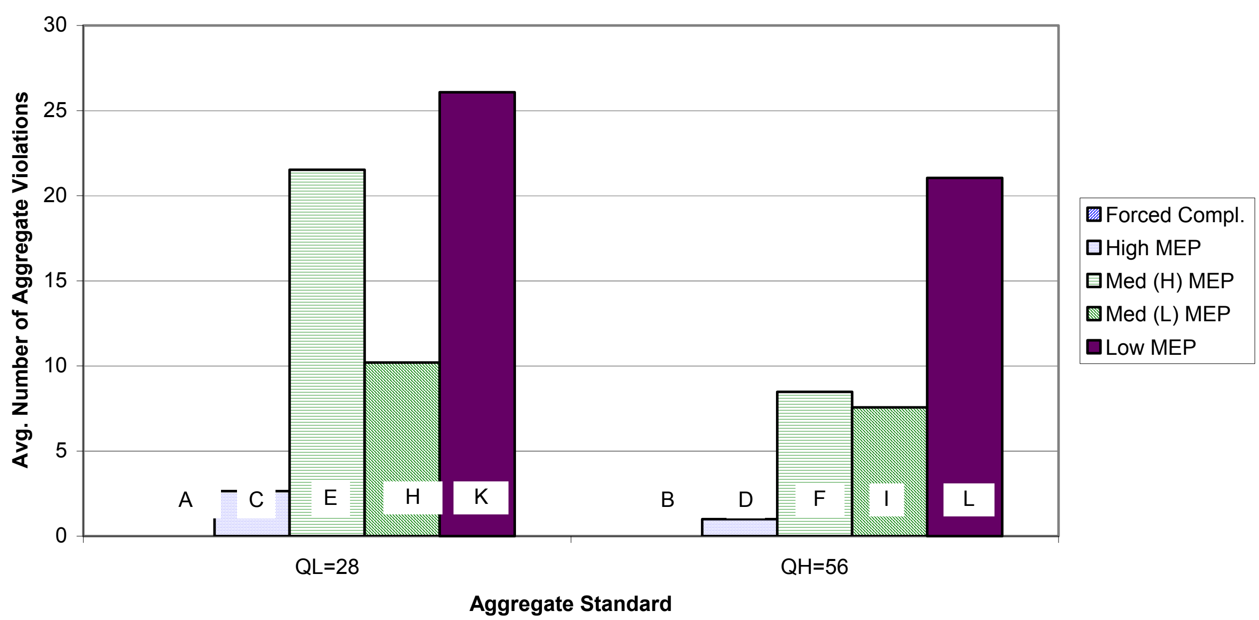
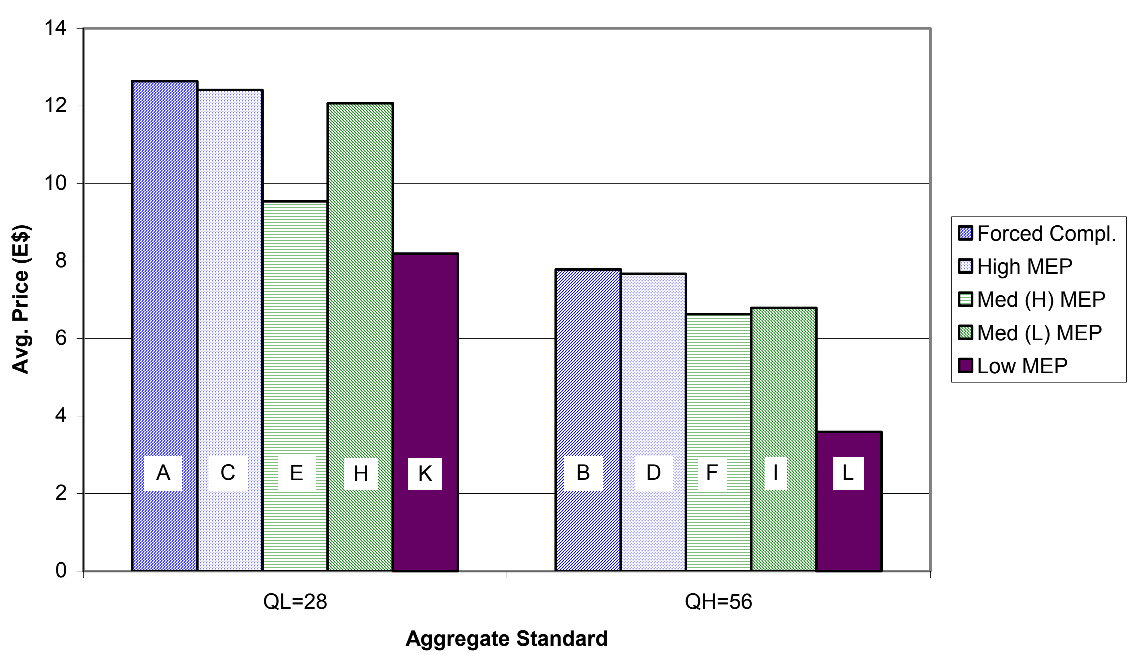


Figure 4. Comparison of average prices as the MEP changes



Hypothesis 5: *For two enforcement strategies that generate the same expected marginal penalty schedules, but with different monitoring probabilities and marginal penalty schedules, rates of non-compliance and permit prices should be identical.*

Refer to treatments E, H, F and I in Figures 3 and 4 to compare average aggregate violations and permit prices for the *Med*(π_H) and *Med*(π_L) marginal expected penalties. It appears that the data are not likely to support Hypothesis 5. Average aggregate violations are clearly not the same. Furthermore, violations are higher for the *Med*(π_H) marginal expected penalty than the *Med*(π_L) marginal expected penalty when the aggregate standard is low (Q_L), while they are lower when the aggregate standard is high (Q_H). At this stage of the research, we are unable to say anything about whether subjects' violation choices are more or less responsive to the probability of apprehension or the severity of punishment.

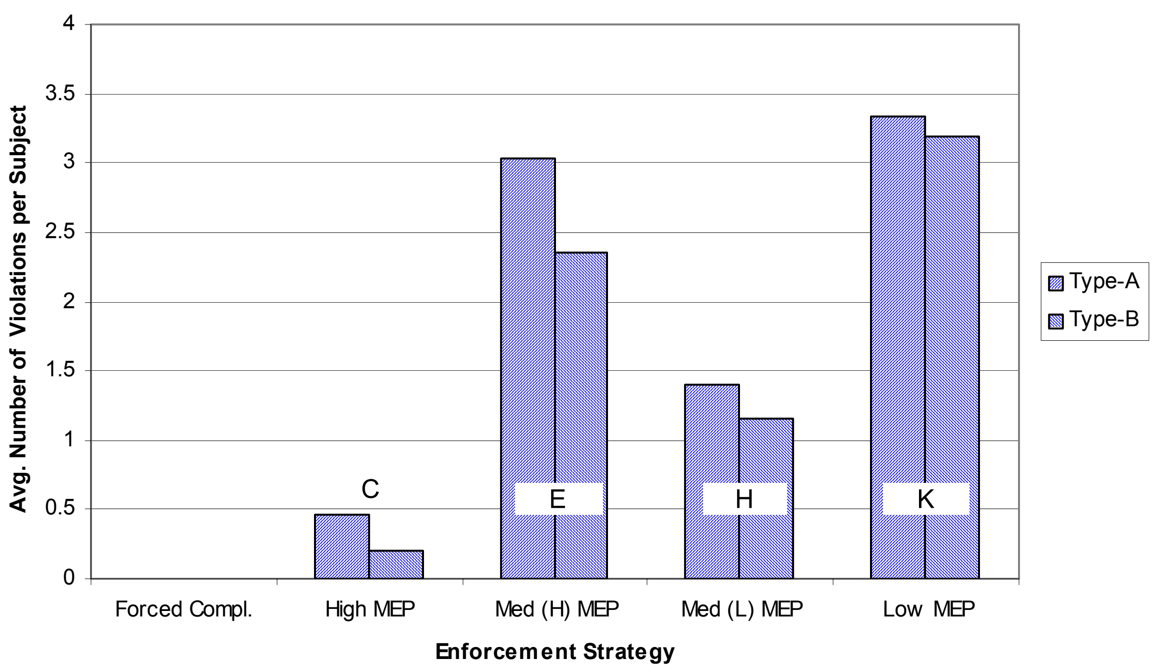
Looking now at average prices in Figure 4, we can see that there may be some support for the hypothesis in the high aggregate standard experiments (Q_H)—the average price in both the *Med*(π_H) and *Med*(π_L) marginal expected penalty treatments are about the same ($6.63 and $6.79, respectively). (These prices are within the competitive equilibrium price range of $6-7). In the low aggregate standard experiments (Q_L), however, the average price with the *Med*(π_L) marginal expected penalty is $12.07 which is significantly higher than the average price of $9.54 with the *Med*($\pi_H$) marginal expected penalty. The competitive equilibrium price for both treatments is between $8 and $9. It is premature to speculate as to why the average permit price was so high in the two experiments with the low standard and *Med*(π_L) marginal expected penalty. It is seems likely that average values mask a group-specific effect. For example, the mean prices in the two experiments with the low standard (Q_L) and the *Med*(π_L) marginal expected penalty (cell H) were quite different: $12.97 and $9.72: with the low standard and the *Med*(π_H) marginal expected penalty (cell E), the mean prices were $8.93 and $10.49. Not surprisingly, a simple t-test of the null hypothesis that the mean price of the two groups within the treatment is equal is strongly rejected at the 1% level of significance. As we subject the data to a thorough statistical analysis, we will probably need to control for group effects.

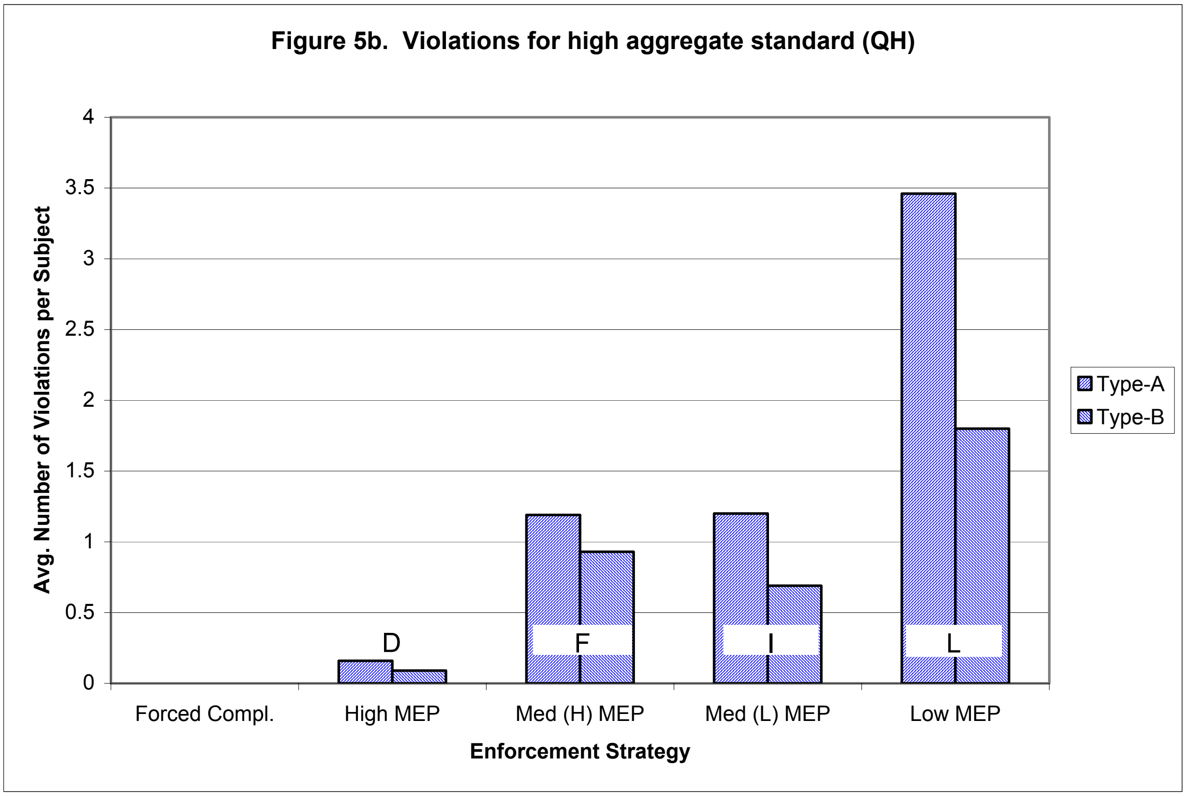
Hypothesis 6: *Individual violations in a permit market should be identical if firms are monitored with the same probability and they face the same penalties, even though they have different marginal abatement costs.*

Figures 5a and 5b compare average individual violations by subject type for the low (Q_L) and high (Q_H) aggregate standard, respectively. Recall from Table 6 that Type-A subjects have the higher marginal benefit functions (marginal benefit is synonymous with marginal abatement cost). These figures suggest that for all enforcement strategies and aggregate standards, the average level of violation is higher for Type-A subjects than for Type-B subjects, which is inconsistent with Hypothesis 6. However, note that for four of the six treatments reported (H, K, F, and L), these values are quite close to each other. Therefore, even if these differences turn out to be statistically significant, it is possible that they may not be "economically significant."

Concurrent with running the market experiments reported in this paper, we have also been running experiments that are identical except that permits are not tradable. We are doing so in order to compare compliance behavior with transferable permits to compliance with fixed emissions standards. Our preliminary results from these fixed standards experiments suggest that the differences in violation levels between Type-A and Type-B subjects are significantly larger than the differences in the market experiments.

Figure 5a. Violations for low aggregate standard (QL)





5. Concluding remarks

While there is a substantial body of economic theory about compliance and enforcement in emissions trading programs, and readily available information about how existing emissions trading programs are enforced, there are no empirical analyses of the determinants of compliance decisions in emission trading programs. Furthermore, there are no empirical analyses of various elements of actual or proposed enforcement designs. Toward filling these gaps, the overall objective of this research is to use laboratory experiments to test a number of hypotheses about compliance behavior and enforcement strategies for emissions trading programs.

Clearly, it is premature to draw any significant conclusions from this preliminary presentation of the results. Although the observed average prices and violations may differ from the competitive equilibrium predictions, there does appear to be general support for most of the comparative static predictions. Permit prices and aggregate violations are generally responding to changes in aggregate standard and enforcement strategies in a manner that is consistent with our hypotheses. However, the simple averages we present mask variations across periods and groups and it is also possible that risk attitudes could play an important role in individual decisions.

We expect that further analysis of this data will provide policy-makers, regulators and researchers with a more comprehensive understanding of compliance behavior and the effectiveness of various enforcement tactics in emissions trading programs than is currently available. This will lead to a better understanding of how market mechanisms and incentives in managing environmental problems should be designed, implemented, and managed to meet environmental quality goals cost-effectively.

Appendix A: Comparative statics of compliance under competitive emissions trading

Basic assumptions

Throughout we consider a fixed set of heterogeneous, risk-neutral firms that are grouped by type into a set K. There are n^k identical firms of type k. We assume competitive behavior so that a single firm's choices have no affect on the equilibrium of the emissions permit market; however, we assume that there are enough firms of each type so that the aggregate choices of firms of a particular type will impact the market. At the time the firms make their choices, a fixed number of emissions permits have been allocated to the firms free-of-charge, and the enforcement authority has committed itself to a type-specific monitoring and enforcement program.

The emissions-control (abatement) costs of a k-type firm are summarized by $c(q^k, \alpha^k)$, which is strictly decreasing and convex in the firm's emissions q^k [$c_q(q^k, \alpha^k) < 0$ and $c_{qq}(q^k, \alpha^k) > 0$; throughout subscripts denote partial derivatives in the usual manner]. Firm heterogeneity is captured by the shift parameter α^k. We assume that total and marginal abatement costs are increasing in α^k; that is, $c_\alpha(q^k, \alpha^k) > 0$, and $-c_{q\alpha}(q^k, \alpha^k) > 0$.

Suppose that a total of Q emissions permits have been issued and that possession of a permit confers the legal right to release one unit of emissions. Let l_0^k be the number of emissions permits that are initially allocated to each k-type firm, and let l^k be the number of permits each of these firms holds after trade. Assume competitive behavior in the permit market so that trade establishes a constant price per permit p. If a k-type firm is noncompliant, its emissions exceed the number of permits it holds and the magnitude of its violation is $v^k = q^k - l^k > 0$. If a firm is compliant, $q^k - l^k \leq 0$ and $v^k = 0$.

We allow the probabilities with which firms are audited (monitoring) and penalties to vary among firm-types, but not among firms of the same type. Suppose that each k-type firm is audited with constant probability π^k. We have in mind here that the enforcement authority commits to auditing $\overline{n}^k < n^k$ firms of type k at random so that $\pi^k = \overline{n}^k / n^k$. If a firm is found to be in violation, a penalty $f(v^k, \phi^k)$ is imposed. Assume that the penalty for a zero violation is zero but the marginal penalty for a zero violation is greater than zero [$f(0, \phi^k) = 0$ and $f_v(0, \phi^k) > 0$]. Furthermore, for a positive violation the penalty is increasing at an increasing rate in the level of the violation [$f_v(v^k, \phi^k) > 0$ and $f_{vv}(v^k, \phi^k) > 0$]. The parameter ϕ^k is a shift parameter with $f_\phi(v^k, \phi^k) > 0$ and $f_{v\phi}(v^k, \phi^k) > 0$.

Assume that each firm chooses positive emissions and permits, and never over-complies. Then, a k-type firm's problem is to choose emissions and permits to

$$\begin{aligned} &\min \quad c(q^k, \alpha^k) + p(l^k - l_0^k) + \pi^k f(q^k - l^k, \phi^k). && \textbf{[1]} \\ &s.t \quad q^k - l^k \geq 0. \end{aligned}$$

The Lagrange equation for this problem is $\theta^k = c(q^k, \alpha^k) + p(l^k - l_0^k) + \pi^k f(q^k - l^k, \phi^k) - \eta^k(q^k - l^k)$ and the Kuhn-Tucker conditions are:

$$\theta_q^k = c_q(q^k, \alpha^k) + \pi^k f_v(q^k - l^k, \phi^k) - \eta^k = 0; \qquad \textbf{[2a]}$$

$$\theta_l^k = p - \pi^k f_v(q^k - l^k, \phi^k) + \eta^k \leq 0,\ \theta_l^k \times (q^k - l^k) = 0; \qquad \textbf{[2b]}$$

$$\theta_\eta^k = q^k - l^k \geq 0,\ \eta^k \geq 0,\ \eta^k \times (q^k - l^k) = 0. \qquad \textbf{[2c]}$$

Given our assumptions about abatement costs and the penalty schedule, [2a-c] are necessary and sufficient to determine the firm's optimal choices of emissions and permits uniquely.

Individual Choices

Whether a k-type firm is compliant or noncompliant, it chooses its emissions so that the price of a permit is equal to its marginal abatement cost; that is,

$$q^k(\alpha^k, p) = \{\, q^k \mid c_q(q^k, \alpha^k) + p = 0\}. \qquad \textbf{[3]}$$

To see this, suppose at first that the firm is noncompliant so that $q^k - l^k > 0$. Then, [2b] and [2c] require $\theta_l^k = \eta^k = 0$. In turn, [2a] becomes $c_q(q^k, \alpha^k) + \pi^k f_v(q^k - l^k, \phi^k) = 0$, and [2b] becomes $p - \pi^k f_v(q^k - l^k, \phi^k) = 0$. Taken together, [2a] and [2b] then imply $c_q(q^k, \alpha^k) + p = 0$. Now suppose that the firm is compliant. In this case its objective function reduces to $c(q^k, \alpha^k) + p(q^k - l_0^k)$, the minimization of which requires $c_q(q^k, \alpha^k) + p = 0$.

Consistent with an observation by Malik (1990, pg. 101), when the probability with which a firm is audited is constant as in the case of random audits, a firm's choice of emissions is independent of the intensity with which it is monitored and the enforcement pressure applied to it. As Malik notes, and we repeat here, this does not imply that the equilibrium distribution of emissions among the firms is independent of the particular monitoring and enforcement policy -- the policy will affect a firm's equilibrium choice of emissions, but only through its impact on the equilibrium permit price.

Turn now to the firm's demand for emissions permits. When it is compliant the number of permits it demands is simply equal to its choice of emissions; that is, $l^k(\alpha^k, p) = q^k(\alpha^k, p)$. When the firm is noncompliant its demand for emissions permits is

$$l^k(\alpha^k, \pi^k, \phi^k, p) = \{\, l^k \mid p - \pi^k f_v(q^k(\alpha^k, p) - l^k, \phi^k) = 0\}. \qquad \textbf{[4]}$$

To obtain [4] note from [2b] and [2c] that $q^k - l^k > 0$ implies $\eta^k = 0$ and $\theta_l^k = p - \pi^k f_v(q^k - l^k, \phi^k) = 0$. Substitution of the firm's choice of emissions $q^k(\alpha^k, p)$ into $p - \pi^k f_v(q^k - l^k, \phi^k) = 0$ yields [4]. Note that although a noncompliant firm's choice of emissions is not affected by the monitoring and enforcement effort applied to it, its demand for emissions permits is.

Having specified a firm's choice of emissions and its demand for permits, we can now turn to its choice of violation. We start with its choice of whether to be compliant or not: A k-type firm is compliant if and only if

$$p - \pi^k f_v(0, \phi^k) \leq 0. \qquad \textbf{[5]}$$

Although this result is not new, one aspect of it has been overlooked; namely, [5] does not depend on α^k. A firm's decision to be compliant or not depends only on the relationship between the permit price and the marginal expected penalty of a vanishingly slight violation, not on parametric characteristics of its emissions-control costs. In fact, Stranlund and Dhanda (1999) show that this independence extends to the choice of violation by a noncompliant firm. That is, a k-type firm's choice of violation, including whether it is compliant or not, is independent of the abatement cost shift parameter α^k. (We will verify this result in a moment).

Since a noncompliant firm's choice of violation does not depend on its marginal abatement costs, we can use this fact and [3] and [4], we write the firm's choice of violation

$$v^k(\pi^k, \phi^k, p) = q^k(\alpha^k, p) - l^k(\alpha^k, \pi^k, \phi^k, p). \qquad \textbf{[6]}$$

[3], [4], and [6] describe the choices of emissions, permits, and violation of a noncompliant *k*-type firm. The marginal impacts of α^k, π^k, ϕ^k, and p on theses choices are presented in Table 1 and derived below.

Let $\beta = (\alpha^k, \pi^k, \phi^k, p)$. Then, assuming a non-compliant firm, [2a] and [2b] can be written as the following identities:

$$c_q(q^k(\beta), \alpha^k) + \pi^k f_v(q^k(\beta) - l^k(\beta), \phi^k) \equiv 0; \qquad \textbf{[7]}$$

$$p - \pi^k f_v(q^k(\beta) - l^k(\beta), \phi^k) \equiv 0. \qquad \textbf{[8]}$$

Differentiate [7] and [8] with respect to α^k and place in matrix form:

$$\begin{bmatrix} c_{ee} + \pi^k f_{vv} & -\pi^k f_{vv} \\ -\pi^k f_{vv} & \pi^k f_{vv} \end{bmatrix} \begin{bmatrix} q_\alpha^k \\ l_\alpha^k \end{bmatrix} = \begin{bmatrix} -c_{e\alpha} \\ 0 \end{bmatrix}, \qquad \textbf{[9]}$$

where q_α^k and l_α^k denote derivatives of q^k and l^k with respect to α^k. Let H denote the Hessian matrix in [9]. Its determinant is

$$|H| = (c_{qq} + \pi^k f_{vv})\pi^k f_{vv} - (\pi^k f_{vv})^2 = c_{qq}\pi^k f_{vv} > 0. \qquad \textbf{[10]}$$

The solutions to [9] are

$$q_\alpha^k = \frac{1}{|H|}\begin{vmatrix} -c_{q\alpha} & -\pi^k f_{vv} \\ 0 & \pi^k f_{vv} \end{vmatrix} = \frac{-c_{q\alpha}\pi^k f_{vv}}{c_{qq}\pi^k f_{vv}} = \frac{-c_{q\alpha}}{c_{qq}} > 0,$$

and

$$l_\alpha^k = \frac{1}{|H|}\begin{vmatrix} c_{qq} + \pi^k f_{vv} & -c_{q\alpha} \\ -\pi^k f_{vv} & 0 \end{vmatrix} = \frac{-c_{q\alpha}\pi^k f_{vv}}{c_{qq}\pi^k f_{vv}} = \frac{-c_{q\alpha}}{c_{qq}} > 0.$$

As asserted earlier, $v_\alpha^k = q_\alpha^k - l_\alpha^k = 0$. This reveals that, holding monitoring, enforcement and the permit price constant, a change in some parameter that affects the abatement costs of a firm has no effect on its choice of violation. The intuition behind this result is as follows: The marginal expected benefit to a firm of a marginal reduction in its violation is the marginal expected penalty it avoids, which clearly does not depend on the firm's characteristics. To reduce its violation it may purchase the legal right to emit, the marginal cost of which is the equilibrium permit price, or it may reduce its emissions, the marginal cost of which is $-c_q(q^k, \alpha^k)$. But, the firm always chooses its emissions to equate its marginal abatement costs to the price of an emissions permit (see [3]). Hence, the marginal cost of reducing its violation is simply equal to the permit price, and therefore, independent of the firm's marginal abatement costs.

The result that $v_{\alpha}^{k} = 0$ also suggests that a difference in the violations of any two types of firms is independent of differences in their abatement costs. Thus, if two firms are audited with the same probability and they face the same penalties, they should have the same level of violation even though they may have very different marginal abatement costs.

Now differentiate [7] and [8] with respect to π^k to obtain:

$$H\begin{bmatrix} q_{\pi}^{k} \\ l_{\pi}^{k} \end{bmatrix} = \begin{bmatrix} -f_v \\ f_v \end{bmatrix}. \qquad \textbf{[11]}$$

The solutions to [11] are

$$q_{\pi}^{k} = \frac{1}{|H|}\begin{vmatrix} -f_v & -\pi^k f_{vv} \\ f_v & \pi^k f_{vv} \end{vmatrix} = \frac{-f_v\pi^k f_{vv} + f_v\pi^k f_{vv}}{c_{qq}\pi^k f_{vv}} = 0,$$

and

$$l_{\pi}^{k} = \frac{1}{|H|}\begin{vmatrix} c_{qq} + \pi^k f_{vv} & -f_v \\ -\pi^k f_{vv} & f_v \end{vmatrix} = \frac{f_v(c_{qq} + \pi^k f_{vv} - \pi^k f_{vv})}{c_{qq}\pi^k f_{vv}} = \frac{f_v}{\pi^k f_{vv}} > 0,$$

Furthermore, $v_{\pi}^{k} = q_{\pi}^{k} - l_{\pi}^{k} = -f_v / \pi^k f_{vv} < 0$. We have already noted that a firm's choice of emissions is independent of the monitoring and enforcement effort applied to it (see [3]); therefore, its choice of violation is affected by monitoring and enforcement only through induced changes in the number of permits it chooses to hold. For example, if a *k*-type firm is monitored more intensely, then noncompliance is a relatively less attractive strategy. Hence, it is motivated to reduce its violation ($v_{\pi}^{k} < 0$) by purchasing more permits ($l_{\pi}^{k} > 0$), not by reducing its emissions ($q_{\pi}^{k} = 0$).

Now differentiate [7] and [8] with respect to ϕ^k to obtain:

$$H\begin{bmatrix} q_{\phi}^{k} \\ l_{\phi}^{k} \end{bmatrix} = \begin{bmatrix} -\pi^k f_{v\phi} \\ \pi^k f_{v\phi} \end{bmatrix}. \qquad \textbf{[12]}$$

The solutions to [12] are

$$q_{\pi}^{k} = \frac{1}{|H|}\begin{vmatrix} -\pi^k f_{v\phi} & -\pi^k f_{vv} \\ \pi^k f_{v\phi} & \pi^k f_{vv} \end{vmatrix} = \frac{(\pi^k)^2[-f_{v\phi}f_{vv} + f_{v\phi}f_{vv}]}{c_{qq}\pi^k f_{vv}} = 0,$$

and

$$l_{\phi}^{k} = \frac{1}{|H|}\begin{vmatrix} c_{qq} + \pi^k f_{vv} & -\pi^k f_{v\phi} \\ -\pi^k f_{vv} & \pi^k f_{v\phi} \end{vmatrix} = \frac{\pi^k f_{v\phi}(c_{qq} + \pi^k f_{vv} - \pi^k f_{vv})}{c_{qq}\pi^k f_{vv}} = \frac{f_{v\phi}}{f_{vv}} > 0.$$

Furthermore, $v_\phi^k = e_\phi^k - l_\phi^k = -f_{v\phi}/f_{vv} < 0$. The effects of increasing marginal penalties are qualitatively the same as increased monitoring.

Lastly, differentiate [7] and [8] with respect to *p* to obtain

$$H\begin{bmatrix} q_p^k \\ l_p^k \end{bmatrix} = \begin{bmatrix} 0 \\ -1 \end{bmatrix}. \qquad \textbf{[13]}$$

The solutions to [13] are

$$q_p^k = \frac{1}{|H|}\begin{vmatrix} 0 & -\pi^k f_{vv} \\ -1 & \pi^k f_{vv} \end{vmatrix} = \frac{-\pi^k f_{vv}}{c_{ee}\pi^k f_{vv}} = -\frac{1}{c_{ee}} < 0,$$

and

$$l_p^k = \frac{1}{|H|}\begin{vmatrix} c_{qq} + \pi^k f_{vv} & 0 \\ -\pi^k f_{vv} & -1 \end{vmatrix} = \frac{-(c_{qq} + \pi^k f_{vv})}{c_{qq}\pi^k f_{vv}} < 0.$$

From these obtain

$$v_p^k = q_p^k \text{ - } l_p^k == \frac{-\pi^k f_{vv} + c_{qq} + \pi^k f_{vv}}{c_{qq}\pi^k f_{vv}} = \frac{1}{\pi^k f_{vv}} > 0.$$

A higher permit price implies that purchasing the legal right to emit is a relatively less attractive option than reducing emissions, so a firm is motivated to hold fewer permits and reduce its emissions ($q_p^k < 0$ and $l_p^k < 0$). In addition, a higher permit price makes noncompliance a relatively more attractive option so that a firm is motivated to increase its violation ($v_p^k > 0$).

	Emissions (q^k)	***Permits*** (l^k)	***Violation*** (v^k)
α^k	$q_\alpha^k = \frac{-c_{e\alpha}}{c_{ee}} > 0$	$q_\alpha^k = l_\alpha^k > 0$	$v_\alpha^k = 0$

π^k	$q^k_\pi = 0$	$l^k_\pi = \dfrac{f_v}{\pi^k f_{vv}} > 0$	$v^k_\pi = -l^k_\pi < 0$
ϕ^k	$q^k_\phi = 0$	$l^k_\phi = \dfrac{f_{v\phi}}{f_{vv}} > 0$	$v^k_\phi = -l^k_\phi < 0$
p	$q^k_p = -\dfrac{1}{c_{ee}} < 0$	$l^k_p = \dfrac{-(c_{qq} + \pi^k f_{vv})}{c_{qq}\pi^k f_{vv}} < 0$	$v^k_p = \dfrac{1}{\pi^k f_{vv}} > 0$

Table 1: Comparative statics of a firm's choices of emissions, permits and level of violation.

Equilibrium comparative statics

We turn now to characterizing the equilibrium of an emissions permit market with noncompliant firms. Define the vectors $\boldsymbol{\alpha} = (\alpha^k)_{k \in K}$, $\boldsymbol{\pi} = (\pi^k)_{k \in K}$, and $\boldsymbol{\phi} = (\phi^k)_{k \in K}$. Given that a total of Q permits are issued to the firms, and the enforcement authority has committed itself to a type-specific monitoring and enforcement program $[\boldsymbol{\pi}, \boldsymbol{\phi}]$, the equilibrium permit price is $\overline{p} = \overline{p}(\boldsymbol{\alpha}, \boldsymbol{\pi}, \boldsymbol{\phi}, Q)$. Using [4], the equilibrium permit price must equate aggregate demand for permits to aggregate supply; that is, $\overline{p}$ must satisfy

$$\sum n^k l^k(\alpha^k, \pi^k, \phi^k, \overline{p}) \equiv Q. \qquad \textbf{[14]}$$

(Summations throughout are taken over the entire set K). Combining [14] with [3], [4], and [6] gives us equilibrium emissions, permits, and violations:

$$\overline{q}^k(\boldsymbol{\alpha}, \boldsymbol{\pi}, \boldsymbol{\phi}, Q) = q^k(\alpha^k, \overline{p}(\boldsymbol{\alpha}, \boldsymbol{\pi}, \boldsymbol{\phi}, Q));$$
$$\overline{l}^k(\boldsymbol{\alpha}, \boldsymbol{\pi}, \boldsymbol{\phi}, Q) = l^k(\alpha^k, \pi^k, \phi^k, \overline{p}(\boldsymbol{\alpha}, \boldsymbol{\pi}, \boldsymbol{\phi}, Q));$$
$$\overline{v}^k(\boldsymbol{\alpha}, \boldsymbol{\pi}, \boldsymbol{\phi}, Q) = v^k(\pi^k, \phi^k, \overline{p}(\boldsymbol{\alpha}, \boldsymbol{\pi}, \boldsymbol{\phi}, Q)). \qquad \textbf{[15]}$$

Let us first examine the equilibrium effects of a change in the aggregate supply of permits. (The qualitative directions of the equilibrium comparative statics are summarized in Table 2). From [14] obtain

$$\partial\overline{p}/\partial Q = 1\Big/\sum n^k l^k_p < 0. \qquad \textbf{[16]}$$

The sign follows from $l^k_p < 0$ (refer to Table 1), and indicates that the equilibrium price is decreasing in the supply of permits. Using [15] and [16] obtain:

$$\partial\overline{q}^k/\partial Q = q^k_p \overline{p}_Q > 0;$$
$$\partial\overline{l}^k/\partial Q = l^k_p \overline{p}_Q > 0;$$
$$\partial\overline{v}^k/\partial Q = v^k_p \overline{p}_Q < 0.$$

More permits induce a lower permit price. Firms respond to the lower price by increasing their emissions and permit holdings, while decreasing their violations. Note that aggregate emissions are increasing in the supply of permits, while aggregate violations are decreasing the supply of permits.

Now turn to the equilibrium effects of a change in the monitoring of *h*-type firms, holding the monitoring of the other types constant. From [14] obtain

$$\partial \overline{p} / \partial \pi^h = -n^h l_\pi^h / \sum n^k l_p^k > 0 . \qquad \textbf{[17]}$$

The sign of [17] follow from $l_\pi^h > 0$ and $l_p^k < 0$ (Table 1). Intuitively, increased monitoring of noncompliant firms of a particular type motivates them to purchase more permits ($l_\pi^h > 0$) to reduce the magnitude of their violations ($v_\pi^h < 0$). This increased demand for permits then puts upward pressure on the equilibrium permit price. From [15] obtain:

$$\partial q^k / \partial \pi^h = \begin{cases} q_\pi^h + q_p^h \partial \overline{p} / \partial \pi^h = q_p^h \partial \overline{p} / \partial \pi^h < 0, \text{ for } k = h; \\ q_p^k \partial \overline{p} / \partial \pi^h < 0, \text{ for } k \neq h. \end{cases} \qquad \textbf{[18]}$$

[18] indicates that emissions of all firms fall as one type is monitored more closely, because this increased monitoring puts upward pressure on the equilibrium permit price.

To examine the effect of increased monitoring on equilibrium violations, it is convenient to begin with aggregate violations,

$$\sum n^k \overline{v}^k(\alpha,\pi,\phi,Q) = \sum n^k v^k(\pi^k,\phi^k,\overline{p}(\alpha,\pi,\phi,Q))..$$

Differentiate this with respect to π^h to obtain

$$\frac{\partial}{\partial \pi^h}\left[\sum \overline{v}^k\right] = n^h v_\pi^h + \partial \overline{p} / \partial \pi^h \sum v_p^k.$$

Substitute for $\partial \overline{p} / \partial \pi^h$ from [17] and use $v_\pi^h = -l_\pi^h$ from Table 1 to write this last as

$$\frac{\partial}{\partial \pi^h}\left[\sum \overline{v}^k\right] = n^h v_\pi^h \left[1 + \frac{\sum n^k v_p^k}{\sum n^k l_p^k}\right] \qquad \textbf{[19]}$$

To sign [19], first recall from Table 1 that $v_p^k = e_p^k - l_p^k > 0$. This along with $e_p^k < 0$ and $l_p^k < 0$ implies $|l_p^k| > v_p^k$. Consequently, the bracketed term of [19] is positive. Since $v_\pi^h < 0$, [19] is negative, indicating that aggregate equilibrium violations fall with more intense monitoring of *h*-type firms.

Now turn to individual violations. From [15]:

$$\partial\overline{v}^k/\partial\pi^h = \begin{cases} v_\pi^h + v_p^h\,\partial\overline{p}/\partial\pi^h < 0 \text{ for } k = h; \\ v_p^k\,\partial\overline{p}/\partial\pi^h > 0 \text{ for } k \neq h. \end{cases} \quad \textbf{[20]}$$

The sign of $\partial\overline{v}^k/\partial\pi^h$ for types $k \neq h$ follows from $v_p^k > 0$ and $\partial\overline{p}/\partial\pi^h > 0$. Finally, since aggregate violations are decreasing in π^h and the equilibrium violations of all $k \neq h$-type firms are increasing in π^h, the equilibrium violations of h-type firms must be decreasing in π^h.

Although more intense monitoring of h-type firms leads them to reduce their equilibrium violations, this is not immediately obvious from [20]. A firm's equilibrium violation-response to more intense monitoring is made up of a two countervailing effects. Holding the permit price constant, more intense monitoring of h-type firms motivates each of them to reduce their violation [$v_\pi^h < 0$] by purchasing more permits [$l_\pi^h < 0$]. But as a result of their increased demand for emissions permits, the equilibrium permit price increases [$\partial\overline{p}/\partial\pi^h > 0$], which motivates each of them to increase their violation [$v_p^h < 0$]. However, the direct effect of more intense monitoring outweighs the indirect price effect so that more intense monitoring of one group of firms leads each of them to decrease their equilibrium violation. In contrast, all other firms only experience the price effect, so more intense monitoring of one group leads all of them to increase their violations. This finding should serve as a cautionary note for enforcement of emissions trading programs—efforts to induce greater compliance by one group of firms will be partially thwarted because these efforts lead all other firms to become less compliant.

Increasing the penalty for one type of firm has the same qualitative effects as increasing the monitoring of one type. Clearly, increasing the monitoring or penalties of all firms at once will lead all of them to reduce their violations.

Let us now consider the equilibrium impacts of a parametric change in the marginal abatement costs of one type of firm. Recall that a firm's choice of violation is independent of its marginal abatement costs. However, as the following proposition indicates, a change in the marginal abatement costs of one type of firm will affect the equilibrium violations of all firms through the permit price.

From [14] obtain

$$\partial\overline{p}/\partial\alpha^h = -n^h l_\alpha^h \Big/ \sum n^k l_p^k > 0,$$

the sign of which follows from $l_\alpha^h > 0$ and $l_p^h < 0$. Thus, an increase in the marginal abatement costs of one type of firm leads to an increase in the equilibrium permit price. From [15] obtain

$$\partial\overline{v}^k/\partial\alpha^h = v_p^k\,\partial\overline{p}/\partial\alpha^h > 0 \;\forall\, k \in K,$$

indicating that an increase in the marginal abatement costs of one type of firm will lead to higher violations by all firms, because of upward pressure on the equilibrium permit price. Since aggregate violations are increasing in the marginal abatement costs of a firm type, so too are aggregate emissions. That is,

$$\frac{\partial}{\partial \alpha^h}\left[\sum \overline{v}^k\right] > 0 \text{ and } \frac{\partial}{\partial \alpha^h}\left[\sum \overline{q}^k\right] > 0.$$

As for individual emissions, from [15] obtain

$$\partial \overline{q}^k / \partial \pi^h = \begin{cases} q_\alpha^k + q_p^k \partial \overline{p} / \partial \alpha^h > 0 \text{ for } k = h; \\ q_p^k \partial \overline{p} / \partial \alpha^h < 0 \text{ for } k \neq h. \end{cases}$$

The sign of $\partial \overline{q}^k / \partial \pi^h$ for types $k \neq h$ follows from $q_p^k < 0$ and $\partial \overline{p} / \partial \alpha^h > 0$. Finally, since aggregate emissions are increasing in α^h and the emissions of all $k \neq h$-type firms are decreasing in α^h, the equilibrium emissions of h-type firms must be increasing in their marginal abatement costs.

	Q	π^h	ϕ^h	α^h
Price $\overline{p}$	(−)	(+)	(+)	(+)
Violations				
Violations, type h	(−)	(−)	(−)	(+)
Violations, types $k \neq h$	(−)	(+)	(+)	(+)
Aggregate violations	(−)	(−)	(−)	(+)
Emissions				
Emissions, type h	(+)	(−)	(−)	(+)
Emissions, types $k \neq h$	(+)	(−)	(−)	(−)
Aggregate emissions	(+)	(−)	(−)	(+)

Table 2: Equilibrium comparative statics

Appendix B: Instructions Summary

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

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

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

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

Production Highlights

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

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

Permit Market Highlights

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

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

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

Auditing Highlights

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

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

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

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

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May 2, 2003 2:30PM
Question and Answer Session

Q: Alex Farrell, University of California at Berkeley:
This question is actually an observation for Murphy and Stranlund. In the air pollution markets that we have seen so far, people have mentioned there are really good monitoring capabilities and enforcement hasn't really been much of an issue. But, for climate, for the variety of sources of greenhouse gases, there's a wide array of potential for monitoring and difficulties in monitoring. So one of the things that might be interesting in that context, I don't know if you've been thinking about it or if you can, is to have participants who vary dramatically on the MEP and probably not through the penalty so much as the likelihood of the successful audit. I think that could vary a lot and that may be true in some other instances as well.

Jim Murphy and John Stranlund
A. The probability that you are going to get caught is likely to be fairly important. There is not an obvious way to model that. We are cognizant of that problem, although the law and economics literature is almost silent about it. It's certainly something that we ought to consider in the future.

A. I would also like to add that this first round of experiments that we designed had a very neutral frame to it. It was cast, and conceptually we motivated it in our heads, with respect to air pollution, but when we presented it to students we didn't tell them it was air pollution permit markets—there's no environmental context per se. In fact, all they're trading is permits giving them the right to produce. The rationale behind that was hopefully to come up with some broader results that aren't peculiar to just air pollution permit markets but possibly to water, and maybe down the road to something like climate, although I'm not sure I'm ready to make that leap with what we have, but at least, hopefully, we'll have some water implications with what we're doing.

Q: Dallas Burtraw, Resources for the Future:
Question for Tim. I'll start with the premise of what I thought you said and then ask my question—if my premise is wrong, please correct me. The green line in the discriminating price auction I interpreted as being sort of what theory suggested would be optimal individual behavior—is that what you're saying? And, if that's the case, would you expect (say if there was an agent who you could hire to give you guidance on how to behave in the market or if there was enough repeated experimentation in the market) to see conversions to some other behavior other than what you observed? And, finally, how then are the measures of market performance under some kind of theoretical construct?

A. Tim Cason:
That green line, that nonlinear alpha function, was actually an approximation based on simplifying assumptions to see if people behaved as if they didn't have multiple projects they could sell—to see if people behaved as if they knew the range of costs that others were drawing from. It was an attempt to see whether that approximation was correct or not. Since the offers were not in line with that at all—if they were on that green line,

then the discriminate price auctions performance would have been considerably worse, because the offer is much higher than the cost. That approximation doesn't seem to hold—the functional form doesn't even seem right.