Working Paper Series in

Environmental & Resource Economics

An Experimental Test of Ambient-Based Mechanisms for Nonpoint Source Pollution Control

by

Christian A. Vossler, Gregory L. Poe,
William D. Schulze and Kathleen Segerson

CORNELL UNIVERSITY

ERE 2002-01
WP 2002-35

It is the Policy of Cornell University actively to support equality of educational and employment opportunity. No person shall be denied admission to any educational program or activity or be denied employment on the basis of any legally prohibited discrimination involving, but not limited to, such factors as race, color, creed, religion, national or ethnic origin, sex, age or handicap. The University is committed to the maintenance of affirmative action programs which will assure the continuation of such equality of opportunity.

An Experimental Test of Ambient-Based Mechanisms for Nonpoint Source Pollution Control

Christian A. Vossler*
Department of Applied Economics and Management, Cornell University, Ithaca, NY 14853-7801

Gregory L. Poe
Department of Applied Economics and Management, Cornell University, Ithaca, NY 14853-7801

William D. Schulze
Department of Applied Economics and Management, Cornell University, Ithaca, NY 14853-7801

Kathleen Segerson
Department of Economics, University of Connecticut, Storrs, CT 06269-1063

*Please direct correspondence to Christian A. Vossler, Department of Applied Economics and Management, 157 Warren Hall, Cornell University, Ithaca, New York 14853-7801. E-mail: cav22@cornell.edu. Telephone: 607-255-2085. Fax: 607-255-9984. We thank Jeremy Clark, Kent Messer, Kerry Smith, and participants at the Second World Congress and Camp Resources meetings in 2002. Research partially supported by USDA Regional Project W-133 and the Kenneth L. Robinson Endowment.

Abstract:

This paper presents an experimental investigation of the three ambient-based mechanisms proposed by Segerson [*J. Environ. Econom. Management* **15**, 87-98 (1988)] for controlling emissions from a group of nonpoint source polluters: a marginal tax/subsidy, a fixed penalty, and a mechanism that combines the two. To parallel likely conditions in a small watershed, in half of the experiment sessions we allow the group of polluters to engage in costless, non-binding discussion (cheap talk). In sessions without discussion, we find that the tax/subsidy instrument achieves the pollution target, while the fixed penalty and combined mechanisms do not. However, the tax/subsidy does not induce compliance at the individual level. Allowing discussion renders the tax/subsidy ineffective by encouraging gross overcompliance, but notably increases the usefulness of the fixed penalty and combined mechanisms. Differences in outcomes are likely attributable to the dissimilar marginal incentives under the three mechanisms.

Key Words: nonpoint pollution, mechanism design, experimental economics, cheap talk

JEL Codes: C92, D7, H23, Q18, Q25

I. Introduction

There is a growing economics literature on incentive-based approaches for controlling agricultural and other nonpoint source pollution. Since individual firm emissions are prohibitively expensive to observe, proposed economic solutions base regulation on input use, abatement practices, emissions proxies, or ambient pollution levels (for a recent review, see Shortle and Horan 2001). Much of the focus in recent years has been on ambient-based instruments, which were first proposed by Segerson (1988) and draw upon the principal-agent literature (e.g., Holmström 1982). Under Segerson's general incentive scheme, a group of polluters pays penalties if ambient pollution at a common receptor point is above a pollution target or receives subsidies if pollution is below the target. The liability of each polluter depends on the abatement efforts of all polluters, not just her own, as well as stochastic environmental interactions. Segerson develops three policy instruments: (1) a tax/subsidy where all firms pay (receive) a tax (subsidy) for each unit of pollution above (below) the pollution target; (2) a fixed penalty imposed on all firms whenever the ambient concentration exceeds the target; and (3) a tax/subsidy combined with a fixed penalty.

In a recent experimental study, Spraggon (2002) finds that the tax/subsidy instrument is effective in achieving the pollution target but the fixed penalty is not. However, the tax/subsidy approach does not induce compliance at the individual firm level, and can hence be viewed as inequitable. Cochard, Willinger, and Xepapadeas (2002) experimentally test these two instruments as well, but find that the fixed penalty induces compliance while the tax/subsidy results in gross overcompliance. Given their design parameters, however, these results are not surprising. The fixed penalty they employ is eight times greater than necessary to induce compliance theoretically, and firms have an overwhelming incentive to meet the pollution target, as earnings are negative otherwise. Pollution is consistently below the target in their tax/subsidy experiment, and is indicative of tacit collusion among firms, since group profit increases from subsidy payments outweigh losses in net revenue from decreased sales in the experiment. Although Spraggon identifies the theoretical possibility of collusion, he does not observe such an outcome in his experiments. The difference in tax/subsidy experiment outcomes may be attributable to the smaller group sizes (four versus six) used by Cochard, Willinger, and Xepapadeas.

This paper presents an experimental investigation of all three ambient-based mechanisms proposed by Segerson. The other studies do not test the instrument that combines the tax/subsidy and fixed penalty, but it is important to consider. Except in the case of a linear damage function, the regulator needs the combined approach to achieve long run efficiency and to ensure the optimal entry and exit of firms in a watershed. Further, we test the three policy instruments with and without allowing experiment participants to engage in "cheap talk" discussion. Originating in the game theory literature (see Farrell 1987), cheap talk refers to costless, nonbinding communication between players made prior to actual commitments. In the cheap talk experiments, the group of polluters can discuss abatement strategies in response to the ambient mechanism imposed on them but cannot make binding agreements. The cheap talk experiments are an important contribution to the literature as they mimic a real-world situation where the regulator imposes a policy instrument on a small number of firms in a watershed. Weersink *et al*. (1998) suggest that ambient taxes may be best suited for managing pollution problems in small watersheds. In this situation, firms facing potentially large tax payments have the incentive to discuss compliance strategies, and presumably incur negligible transaction costs in doing so. The regulator, and associated extension or outreach agencies, may also wish to bring firms together to discuss the implications and details of a new, unfamiliar policy action. Although Segerson does not consider the possibility of discussion among firms, some researchers imply that such discussion may render the tax/subsidy approach ineffective (Hansen 1998; Spraggon). In particular, overabatement can result in subsidies that are much higher than the increase in abatement costs. The economics laboratory provides a venue from which to explore behavioral interactions between competitors that are not considered in theoretical models.

Our experimental design differs noticeably from other experimental studies in this area. First, similar to Plott's (1983) seminal experimental study on the efficiency of tax and trade externality mechanisms, participants in our study compete to sell their product in a two-sided auction market. Therefore, our design introduces uncertainty in the production outcome and revenue from sales. Chambers and Quiggin (1996) stress that production uncertainty can significantly affect nonpoint pollution levels and should not be ignored. In the other studies, participants choose a decision number analogous to a production decision and calculate revenue through a deterministic quadratic payoff function. Second, we introduce context in the sense that we tell participants that they are (hypothetically) operating a firm, are located along a water resource near other firms, and jointly cause pollution through their production, which in turn causes societal damages. While the introduction of context is not usual protocol, these design elements are desirable for establishing a baseline for a future effort of testing policy mechanisms with real farmers and for using the experiment as a teaching tool. Third, participants in each experimental session go through trading periods without any regulatory intervention before facing a pollution control mechanism. This sequence of events parallels a real-world policy situation, establishes that pollution is excessive without policy intervention, and serves as a baseline for assessing the effectiveness of the incentive mechanisms. In other studies, participants either face a policy mechanism or do not.

Our motivation for using laboratory experiments to test nonpoint pollution mechanisms is simple: there are no real world data on how firms respond to the incentives underlying these mechanisms (Shortle and Horan), especially in areas that would involve non-compliance (Alm, McClelland, and Schulze 1992, 1999). At a minimum, appropriately designed experiments reveal whether people identify and respond to incentives in a way consistent with economic theory. If laboratory subjects fail to respond in a theoretically predicted manner, then it is widely accepted that there is little hope of success for the theory in a real world setting. Experiments may also provide insight on the appropriateness of the assumptions underlying theoretical models and, given observed behavior in the laboratory, offer suggestions on modifying policies to avoid potential problems in the field.

The organization of the paper is as follows. The next section briefly outlines the Segerson ambient-based mechanisms. Section III describes our experimental design and data. Section IV presents experiment results and hypothesis tests. In the final section, we summarize our findings and discuss their implications for nonpoint pollution policy and research.

II. Overview of Ambient-based Mechanisms

Segerson's work shifted the focus from pollution control mechanisms that require monitoring the decisions of individual firms to mechanisms that require monitoring ambient pollutant concentrations. As a result, the regulator's large informational burden is reduced. The regulator needs to obtain some knowledge concerning the fate and transport of pollutants (as would be the case for any efficiency regulation), the value of marginal damages at the desired pollution target, and individual emission and abatement cost functions. In the special case of a linear damages function, knowledge of each firm's emission and abatement cost function is unnecessary.

Using Segerson's original notation, let $x(a,e)$ be the ambient concentration of a single pollutant at a common receptor point, where a is a vector containing each firm's abatement level and e is a random variable. The random variable takes into account uncertain and stochastic environmental factors (e.g., rainfall and water currents) that affect the relationship between firm emissions and ambient levels. Let $D(a,e)$ be the damages of pollution function and $F(\bar{x}, a)$ be the probability that the ambient pollution level is less than a pollution target, denoted as $\bar{x}$.[1] Firms, indexed by i, are risk-neutral and have (possibly) different emission and abatement cost functions. Tax liabilities are determined as follows:

If $x > \bar{x}$, then *each* firm makes a payment equal to $t_i(x - \bar{x}) + k_i$

If $\bar{x} \geq x$, then *each* firm receives a payment equal to $t_i(\bar{x} - x)$

Each polluter chooses the socially optimal level of abatement in response to t_i, a marginal tax/subsidy, and k_i, a fixed penalty, chosen by the regulator in any of the following ways:

(a) $k_i = 0$ and $t_i = E[D' \bullet \partial x/\partial a_i]/E[\partial x/\partial a_i]$,

(b) $t_i = 0$ and $k_i = -E[D' \bullet \partial x/\partial a_i]/E[\partial F/\partial a_i]$,

or

(c) t_i is arbitrary and $k_i = (-E[D' \bullet \partial x/\partial a_i] + t_i E[\partial x/\partial a_i])/(\partial F/\partial a_i)$,

where E is the expectations operator and derivatives are evaluated at the optimum. Hereafter in the paper, we refer to (a) as the tax/subsidy, (b) as the fixed penalty, and (c) as the combined approach. Regardless of instrument choice, pollution is a public bad and all polluters benefit

[1] Segerson defines a benefit function instead of a damage function, but we use the notion of pollution damages in the experiments.

from an individual firm's abatement efforts. If the ambient level is greater than the pollution target, each firm pays a tax and/or a penalty regardless of whether its individual abatement efforts are optimal.

In the case of a linear damage function (or a function that is approximately linear in the vicinity of the optimum) and a pure tax/subsidy scheme, the tax/subsidy rate is equal to marginal damages and the regulator needs to know what marginal damages are at the pollution target only.[2] In this case, when ambient levels are one unit higher than the target, *each* firm pays a tax equal to marginal damages and the total tax revenue is the number of firms multiplied by the tax rate. That is, the mechanism requires "*each* polluter to incur the marginal damage associated with the targeted level of pollution, with the end result being a *multiple* of damage costs collected (distributed) when taxes (subsidies) are employed" (Herriges, Govindasamy, and Shogren 1994, p. 266).[3]

With the fixed penalty mechanism, liabilities are discontinuous at a threshold level akin to provision points in public goods funding mechanisms (Rondeau, Schulze, and Poe 1999). As long as ambient levels remain above $\bar{x}$ a fixed penalty is imposed on all firms. If ambient levels are at or below $\bar{x}$, no penalty is assessed.

The combined approach provides a multitude of possible t and k combinations. While the magnitude of opportunities may appear daunting, this feature is perceived to be an advantage of the mechanism. It is well known that the effect of tax and subsidy schemes on profitability can influence entry/exit decisions of firms from an industry. Variable k values allow the regulator

[2] In the more general case of a nonlinear damage function, Hansen (1998) and Horan, Shortle, and Abler (1998) show that a tax scheme based on observed total damages rather than the ambient concentration allows the regulator to implement a uniform policy with only knowledge of the damage function in hand.
[3] Several authors have raised concerns that the "resulting monetary transactions and associated costs would be substantial" (Horan, Shortle, and Abler 1998, p. 193). Recognizing this, attention has turned to the budget-balancing implications of the tax/subsidy mechanism (e.g., Xepapadeas 1991; Herriges, Govindasamy, and Shogren; Horan, Shortle, and Abler).

some flexibility in using lump sum transfers for controlling incentives for firm entry and exit, thus engendering the possibility of long-run as well as short-run efficiency.

III. Experimental Procedures and Design

In the spring and summer of 2002, we conducted experiments with 144 Cornell University undergraduate student participants recruited from classes in the Department of Applied Economics and Management and the Department of Economics. All participants had at least an introductory understanding of microeconomic theory and approximately 80% had prior experience in economics experiments. Experiments took place in the Laboratory for Experimental Economics and Decision Research (LEEDR) using GreenWeb, an Internet-based computer platform developed at Cornell.[4]

Experiment sessions lasted approximately an hour and one half and subjects earned between $20 and $60, paid in cash immediately after the experiment. After subjects read instructions, the lead author gave a PowerPoint presentation outlining the experiment and answered any questions.[5] After one trading period was completed, subjects had a final opportunity to ask questions. This procedure ensured that subjects understood how the market worked and how to interpret the market results displayed by the computer. The computer made all relevant calculations. There were no practice periods and the experiment consisted of thirty independent trading periods.

In each experimental session, six participants (indexed by $i = 1,\ldots,6$) play the role of identical, independent firms that can each produce up to five units of a good during each of the thirty trading periods. Total production costs for the firm, $c_i(q_i)$, are increasing in the quantity

[4] The current web address is www.pserc.cornell.edu/greenweb/1.2/greenweb.html.
[5] PowerPoint files are available upon request. We used this presentation format to help facilitate close replication of experiments.

sold, q_i, and at an increasing rate. Specifically, total costs are \$50, \$200, \$500, \$1000, and \$1800 for $q_i = 1,\ldots,5$. The number of units sold (produced) in a given period and the price paid for units are determined through a uniform price sealed-bid/offer auction.[6] Each participant submits five price offers, one corresponding to *each* unit they can produce. The participant's offer prices can be the same for each unit or differ. The auction ranks all (thirty) offers from lowest to highest. On the demand side, computer agents represent consumers that submit bids equal to their marginal benefits.[7] Specifically, the highest bid is equal to \$1,700 and each subsequent bid is \$50 less than the previous one. The auction ranks bids from highest to lowest. The average price of the last accepted bid and offer (where the bid and offer arrays intersect) determines the market-clearing price, p. This clearing price is paid to all accepted offers. After each trading period, the computer displays all profit calculations relevant for the individual firm, total group output, and ambient pollution. The production costs and demand-side bids do not change during the course of the experiment. Participants know nothing about buyers except that they are experienced in this type of auction and can potentially buy all units offered.

Each unit sold results in one unit of ambient pollution on average. Specifically, the pollution level is equal to the total output from the group, Q, plus a random term, e, distributed uniformly on the interval [-1, 1].[8] A firm reduces its emissions (abates) by producing less.[9] Each unit of output results in expected pollution damages to society of \$500, such that

[6] This is also known as a clearinghouse auction or single price auction. The New York Stock Exchange begins each trading day with this auction. In general, this auction captures approximately 90% of available economic surplus in the absence of an externality (Kagel and Roth 1995). The auction fares better than this in our application since the only buyers in the market are computer agents programmed to submit bids equal to their maximum willingness to pay.

[7] Although the seminal experimental paper on pollution control mechanisms (Plott 1983) used a double auction, we opted to have human participants on supply side of the market only, as we are specifically interested in firm behavior. In addition, this experiment is somewhat complex and having demand-side subjects adds unnecessary complication.

[8] When output is zero, e is constrained to be zero.

[9] Output reduction is just one way to reduce pollution. Alternatively, we could have firms choose from a set of abatement technologies, as in the marketable permit experiments of Ben-David et al. (1999). However, we abstract in order to simplify the setting. Ben-David et al. simplified their experiments by fixing the output price.

E[$D(Q,e)$]=\$500Q. Figure 1 presents the marginal benefit, private marginal cost, and social marginal cost arrays for this experiment. The competitive solution (marginal benefit = private marginal cost) is Q = 24 and the socially optimal solution (marginal benefit = social marginal cost) is Q = 18, a reduction in pollution of 25%.[10] Since all firms are identical, an equitable distribution of profits has each firm selling four units in the competitive outcome and three units in the optimal outcome.

There are two parts to the experiment. The first part consists of ten trading periods with no pollution control mechanism and the second consists of twenty periods with the mechanism. The two parts of the experiment reflect a real-world sequence of events. After each non-mechanism period, participants observe the pollution level and the social damages from pollution but these measures do not directly affect profits in any way. The second experiment part is identical to the first, except for the addition of a policy mechanism. Participants receive information detailing the treatment-specific mechanism only after completing the first ten trading periods. We impose one of three possible mechanisms on each group:

(a2) tax/subsidy: $k = 0$ and t = \$500,

(b2) fixed penalty: $t = 0$ and k = \$1000,

or

(c2) combined approach: t = \$250 and k = \$500,

where the parameters for the policies are calculated using equations (a), (b), and (c) in Section II, and the pollution target, $\bar{x}$, is set to the socially optimal level of 18 units.[11] Since marginal damages are equal to \$500/unit (at the pollution target and otherwise), the optimal tax/subsidy rate is \$500/unit. That is, whenever pollution is above (below) 18 units each firm pays a tax

[10] Spraggon's and Cochard, Willinger, and Xepapadeas' experiments involve a 75 % reduction and a 28% reduction, respectively.

[11] We omit subscripts since the tax/subsidy and fixed penalty amounts are the same for homogenous firms.

(receives a subsidy) of \$500 for each unit of pollution above (below) 18. Since there is, on average, a one-to-one relationship between pollution and abatement, $E[\partial x/\partial a] = -1$. Given the error distribution, deviating from the optimum by one unit changes the probability of paying the penalty from 0.5 to 1.0; hence, $E[\partial F/\partial a] = 0.5$.[12] It follows that the fixed penalty is \$1,000 and imposed whenever pollution exceeds 18 units. For the combined approach, the marginal tax/subsidy rate is arbitrarily set to \$250, resulting in an optimal fixed penalty of \$500. Given our design parameters, the only Nash equilibrium in each mechanism treatment occurs when each firm chooses the socially optimal level of abatement (production). However, for the tax/subsidy and combined mechanism treatments, the symmetric group outcome that maximizes profits is when output is zero. Even though firms forego all profits from sales, the magnitude of subsidy payments offsets this. The incentive to collude and overabate is due to the non-budget balancing characteristic of the mechanism (Hansen 1998).

For each no-mechanism period, profits for the firm are:

$$\pi_i(q_i) = pq_i - c_i(q_i) + 500 \tag{1}$$

where p and q_i are determined in the market. The extra \$500 in per period profits reflects government subsidies or off-farm income. This is included to decrease the variance in subject earnings and to decrease the likelihood of negative earnings. Profits under the three mechanisms are:

$$\pi_i(q_i) = [pq_i - c_i(q_i) + 500] - 500*(x - \bar{x}) \tag{2a}$$

$$\pi_i(q_i) = [pq_i - c_i(q_i) + 500] - 1000J \tag{2b}$$

$$\pi_i(q_i) = [pq_i - c_i(q_i) + 500] - 250*(x - \bar{x}) - 500J \tag{2c}$$

[12] We randomly drew a set of thirty error terms from the specified distribution before the experiments and used the same set of errors in each session. The probability of a positive error term for any mechanism-imposed period is indeed 0.5.

where $J = 1$ if $x > \bar{x}$ and equals 0 otherwise. Directly following the experiment, participants receive cash payments equal to the profits across all periods multiplied by the factor $1 US / $2,000 EXP.[13]

We conducted eight sessions each of the tax/subsidy, fixed penalty, and combined approach. In four of the eight sessions for each mechanism, we allowed participants to engage in cheap talk. In the relevant sessions, cheap talk took place before the first mechanism round (period 11) and before the eleventh mechanism round (period 21) only. The only discussion guidelines were that participants could not threaten each other or arrange for any side payments. There was no communication of any kind between participants in sessions without cheap talk. We include the general set of instructions for all treatments as Appendix A and include the supplemental instructions for the tax/subsidy treatment with cheap talk as Appendix B. Instructions include (color) screen shots that subjects encounter during the experiments. Supplemental instructions are similar for other treatments and available upon request.

IV. Results

Table 1 presents the average group output for non-mechanism and mechanism periods, as well as efficiency calculations for all experiment sessions. Figure 2 presents a time series of the average group output by treatment and Figure 3 presents the group output for all sessions. Consistent with Spraggon, we calculate an efficiency measure defined as the percentage of available economic surplus (ES) captured over and above the surplus in the (theoretical) perfectly competitive outcome:

[13] In lieu of extremely high payouts observed for the first session of the tax/subsidy treatment with cheap talk, the exchange rate is $1 US / $3,500 EXP for the remaining sessions of this treatment.

$$\text{Efficiency} = \frac{ES_{observed} - ES_{competitive}}{ES_{optimal} - ES_{competitive}} \times 100\% \qquad (3)$$

Efficiency equals 100% when the group output is 18 units and each firm produces three units. Efficiency equals 0% when the group output is 24 units and each firm produces four units. Deviations from these two efficiency levels occur when output is reduced or increased and/or higher cost units are sold instead of lower cost units. Efficiency can be negative if the surplus captured is less than that under perfectly competitive conditions. Tax or subsidy payments are not included when calculating efficiencies.

In this section, we first analyze behavior in the absence of regulatory intervention. Second, we test whether the observed outcome under the various mechanisms is equal to the theoretical prediction. Third, we test whether group output is different across mechanisms. To test our hypotheses, we employ one and two-sample t-tests, where the average outcome across trading periods in a session is treated as one independent observation (i.e., n=4 for each mechanism treatment). To remove the trends in the data due to learning effects, the first five non-mechanism and the first ten mechanism periods are not included in hypothesis tests. Results are robust to other, arbitrary groupings of outcomes. The results of our tests coincide with the basic intuition gleaned from reviewing the data. We employ five-percent significance levels for all tests.

A. Non-Mechanism Outcomes

Before imposing the mechanism, all subject groups consistently fail to reach the theoretically competitive solution of 24 units. The slight inefficiencies observed are not unexpected, as the incentive exists for sellers in this multi-unit auction to try to manipulate price with their higher cost units. However, the uniform price auction mechanism is a commonly used alternative to the highly efficient double-auction, which performs slightly better on efficiency grounds. More importantly, the use of the uniform price auction allows us to concentrate solely on supply-side behavior.

Across all sessions, the average group output for all non-mechanism periods is 22.2 units and the average for Period 10 is 22.8 units. Figure 2 reveals a slight upward time trend for all treatments, suggesting some sort of "invisible-hand" type market learning over time. One-sample t-tests reject the hypothesis of equality between the observed outcome and the perfectly competitive outcome for each treatment. We do not reject these same hypotheses using outcomes from Period 10 only, except for the combined approach with cheap talk treatment. As such, output is roughly at the perfectly competitive level when we impose the mechanism and certainly above socially optimal levels.

B. Mechanism Outcomes without Cheap Talk

In the treatments where no discussion takes place among subjects, the results of the three mechanisms are mixed. Output in all treatments declines during the initial mechanism periods, with only the output in the fixed penalty sessions returning to near-competitive levels. The latter result is consistent with behavior in voluntary contributions public good experiments where, in the absence of discussion, some players initially contribute to the public good but gradually reduce their contributions towards zero upon observing that overall contributions are suboptimal (Isaac and Walker 1988). The tax/subsidy fares well in achieving the optimal output level and averages 18.35 units over the last 10 trading periods. This output is not statistically different from the theoretical optimum ($p = 0.280$). Over the same set of periods, the fixed penalty treatment averages 21.55 units and the combined approach averages 19.35 units. The fixed penalty outcome is statistically different from the optimum (p = 0.000) and different from the other two mechanisms. The combined approach output is not statistically different from the theoretically predicted outcome, although only marginally so (p=0.070). The overall results make sense, as the tax/subsidy provides the greatest incentives at the margin over all output levels. Given the distribution of the error term, the fixed penalty only provides incentives at the margin when output is close to the optimum. From the perspective of a firm, there is no guarantee that reducing output leads to higher earnings. Our results for the tax/subsidy and fixed penalty treatments are consistent with the findings of Spraggon.

Similar to the findings of Spraggon, we do not observe compliance at the individual level for the tax/subsidy treatment. That is, although average output is near the optimum, each firm is not consistently selling three units. Higher cost units are sold, thus reducing the efficiency measure from 100%. Average efficiency is only 56.2% and very similar to the efficiency of the combined approach (52.0%). It became evident through debriefing sessions that one or two subjects in each session intentionally withdrew too many units from the auction – by submitting extremely high offer prices – in order to safeguard against paying relatively high taxes. This contrasts Segerson's theoretical assumption that firms are risk-neutral. Those subjects that sold too many units knew that they would earn more money by selling less, but cited a desire to earn more money than their competitors (and hence did not play their dominant strategy). Subjects who sold too many units commonly said they would have sold less if output exceeded the target level, as their profits would have sufficiently decreased.

C. Mechanism Outcomes with Cheap Talk

Communication among firms in a market has a pronounced impact on the effectiveness of the mechanisms. For each mechanism, output in cheap talk sessions is significantly lower than in sessions without cheap talk (tax/subsidy: p=0.030; fixed penalty: p=0.000; combined: p=0.018). In all sessions of the treatment with the tax/subsidy and cheap talk, participants realized that the group maximizes profits when output is zero. This collusive outcome is also the most profitable in the Spraggon and Cochard, Willinger, and Xepapadeas experimental studies of this type. A firm has the incentive to deviate from the zero output strategy if it assumes that no one else will deviate. However, once someone deviated in a session, an unraveling process apparently took place where more units were sold in each subsequent period. This "environmental shirking" is similar to the free-riding behavior observed in voluntary contributions experiments for public goods (e.g., Isaac and Walker). Only one of four groups was successful in implementing and sustaining the profit maximizing group strategy for all periods. The other three groups did not successfully implement this strategy. At the same time, group output never exceeded 17 units, indicating that subjects were substantially risk averse relative to the discontinuous fixed penalty. Overall, group output is marginally statistically different from the optimum (p=0.064). The average output over the last 10 periods is only 6.88, but there is a considerable variance in this measure. We note that participants in each tax/subsidy session were aware of the zero output collusive strategy.

The results for the fixed penalty are remarkably consistent across sessions. During the first discussion in all four sessions, subjects agreed on a group output of 17 units. During second-round discussions, subjects reiterated this strategy. To help ensure its success, subjects in all sessions devised a plan where, in each period, five people sold three units and one person sold two. Participants took turns selling only two based on an assigned rotation. Given the error distribution of pollution, a group outcome of 17 just ensures no penalty will occur and is the strategy that maximizes group profits. Over the last 10 periods, output in all sessions is statistically different from the optimum (p = 0.004) and averages 17.05.

The combined approach is the most effective in terms of achieving proximity to the pollution target. The average output over the last ten periods, 17.25 units, is not statistically different from the target (p = 0.299). As in the tax/subsidy treatment, maximum group earnings occur when group output is zero. Two groups discussed this strategy, but subjects were not convinced that the strategy maximized profits, and noted that market price increased as output decreased. This suggests that the mixed approach made the most profitable solution less transparent. Instead, participants in these two groups attempted to sell two units each. In the remaining two sessions, subjects agreed to sell three units each so that expected tax payments were zero. It is somewhat surprising that no group implemented the strategy used in the fixed penalty sessions to avoid paying taxes (and associated penalty) altogether. Although group earnings are higher with this strategy than when everyone sells three units, at the firm level the difference between earnings from selling the third unit (given everyone else sells three units) and the change in tax payments is subtle (around $25 in experimental dollars). Efficiency in this treatment is the highest at 75.2%, followed closely by the fixed penalty mechanism (72.1%).

V. Concluding Remarks and Directions for Future Research

This study presents experimental tests of the three ambient-based mechanisms for controlling nonpoint pollution proposed by Segerson: the marginal tax/subsidy, a fixed penalty imposed whenever the ambient concentration exceeds the target, and an approach that combines a marginal tax/subsidy with a fixed penalty ("combined approach"). The taxes firms pay or subsidies firms receive coincide with deviations between the observed ambient pollution level and a pollution target, as well as the stochastic nature of biological resources. As such, the financial outcome of the policy depends on the combined behavior of all firms. Our study extends the existing experimental literature by testing the combined approach, needed to achieve long-run efficiency in the presence of a nonlinear damage function, and by allowing respondents to engage in non-binding discussion. Allowing group discussion reflects a real-world situation where a small number of firms in a watershed have incentives to discuss strategies in order to avoid (potentially) large financial losses.

When firms cannot discuss abatement strategies, the tax/subsidy performs the best and ambient pollution levels are not statistically different from the specified pollution target. The fixed penalty mechanism performs poorly, probably because individual firms do not have incentives at the margin to change their decision when ambient pollution is far from optimal. The performance of the combined approach lies between the other two.

When discussion occurs between subjects, which we argue is the most likely case if such policies are implemented on a watershed-by watershed basis, average output is statistically below the pollution target in the tax/subsidy and fixed penalty treatments. In the tax/subsidy treatment, subjects easily identified that the group would maximize earnings by limiting production to zero: indeed, on average output was some 50% below optimal. In the fixed penalty treatment, subjects are successful in implementing a strategy where output is exactly one unit below the target. Given the random variation in ambient pollution, the group thus manages to avoid the penalty in all periods. The combined approach yields output that is not statistically different from the target pollution level. The fixed penalty and combined approaches work well in terms of economic efficiency and equity of earnings among firms.

Two important and related issues arise from this research. The first issue is how large the tax/subsidy rate and/or the fixed penalty are likely to be in relation to earnings from output sales. A second issue is how to modify the mechanisms in order to avoid large subsidy payments, and similarly to prevent firms from overcompliance in the presence of potentially large fines for noncompliance and random environmental factors. The tax/subsidy rate and/or fixed penalty depend largely upon societal damages, which are determined exogenously to polluters. As such, the tax/subsidy rate and/or fixed penalty are likely to be very watershed specific. Unfortunately, there is little information available on the damage functions of polluters in small watersheds. It is plausible that damage-income disparities may be large or small and induce different types of behavior in either circumstance.

When polluters are likely to discuss strategies with one another, it is especially important to investigate modifications of the tax/subsidy and combined mechanisms under conditions where a group of regulated firms has the incentive to collude and underabate to collect large subsidy payments. This incentive is a result of the non-budget balancing property of the mechanisms. In particular, firms collectively have the incentive to overabate when the marginal revenue from group output sales is less than the tax/subsidy rate multiplied by the number of firms. As evident from our experimental results, one way to reduce this incentive is by varying the tax/subsidy and fixed penalty rate within a combined approach. For both the tax/subsidy and combined approaches, one can set a pollution cut-off level below which the tax/subsidy rate is zero. Hansen (1998) shows theoretically, in the case where the regulator imposes a nonlinear tax equal to total damages, that such a scheme can be developed that prevents polluters from forming a coalition and reducing pollution to suboptimal levels. With a fixed penalty mechanism, the regulator may want to account for some of the random environmental factors that serve to increase ambient pollution (e.g., weather) and announce that she will not impose a penalty under the more extreme sets of circumstances. Such an approach does not induce optimality, but may prevent firms from overly excessive abatement in the presence of potentially large government fines. In a similar vein, Horan, Shortle, Abler design a state-dependent ambient mechanism with a tax rate that is determined after the realization of the values of all random variables.

The current experimental research on ambient-based pollution control instruments is an important and informative step in the research on nonpoint source pollution mechanisms. We maintain that our research provides a baseline for extending emerging theoretical analysis on this subject (e.g., Xepapadeas; Chambers and Quiggin; Horan, Shortle, and Abler; Hansen 2002) on many levels. First, it may be desirable to impose design elements that more accurately reflect a firm's decision-making environment. For instance, we can implement emissions functions that are heterogeneous across firms and have firms jointly choose their output level and abatement effort or we can alter firm abatement-ambient pollution relationships. Second, our experimental results may be sensitive to the chosen design parameters and we can modify the damage function and the random variability of ambient pollution as a stress test. Third, the behavior of undergraduate economics students may differ from decision makers in agriculture. Once a mechanism passes muster in the academic laboratory, it remains likely that we can learn much by having relevant decision makers as experiment participants and discussing potential policy mechanisms with them.

References

Alm, James, Gary H. McClelland, and William D. Schulze. 1992. Why Do People Pay Taxes? *Journal of Public Economics* **48**: 21-38.

Alm, James, Gary H. McClelland, and William D. Schulze. 1999. Changing the Social Norms of Tax Compliance by Voting. *Kyklos* **52**(2): 141-171.

Ben-David, Shaul, David S. Brookshire, Stuart Burness, Michael McKee, and Christian Schmidt. 1999. Heterogeneity, Irreversible Production Choices, and Efficiency in Emission Permit Markets. *Journal of Environmental Economics and Management* **38**(2): 176-194.

Chambers, Robert G., and John Quiggin. 1996. Non-Point-Source Pollution Regulation as a Multi-Task Principal-Agent Problem. *Journal of Public Economics* **59**: 95-116.

Cochard, François, Marc Willinger, and Anastasios Xepapadeas. 2002. Efficiency of Nonpoint Source Pollution Instruments in the Presence of Endogenous Externality among Polluters: An Experimental Approach. Paper presented at the Second World Congress of Environmental and Resource Economists, Monterey, CA, June 24-27, 2002.

Farrell, Joseph. 1987. Cheap Talk, Coordination, and Entry. Rand Journal of Economics **18**(1): 34-39.

Hansen, Lars G. 1998. A Damage Based Tax Mechanism for Regulation of Non-Point Emissions. *Environmental and Resource Economics* **12**: 99–112.

Hansen, Lars G. 2002. Regulation of Non-Point Emissions. *Environmental and Resource Economics* **21**: 303-316.

Herriges, Joseph, Ramu Govindasamy, and Jason Shogren. 1994. Budget-Balancing Incentive Mechanisms. *Journal of Environmental Economics and Management* **27**: 275-285.

Holmström, Bengt. 1982. Moral Hazard in Teams. *Bell Journal of Economics* **13**: 324-340.

Horan, Richard D., James S. Shortle, and David G. Abler. 1998. Ambient Taxes when Polluters have Multiple Choices. *Journal of Environmental Economics and Management* **36**: 186-199.

Isaac, R. Mark, and James M. Walker. 1988. Communication and Free-Riding Behavior: The Voluntary Contributions Mechanism. *Economic Inquiry* **26**: 585-608.

Kagel, John H., and Alvin E. Roth (Eds.). 1995. *The Handbook of Experimental Economics*. Princeton, New Jersey: Princeton University Press.

Plott, Charles R. 1983. Externalities and Corrective Policies in Experimental Markets. *Economic Journal* **93**: 106-127.

Rondeau, Daniel, William D. Schulze, and Gregory L. Poe. 1999. Voluntary Revelation of the Demand for Public Goods Using a Provision Point Mechanism. *Journal of Public Economics* **72**(3): 455-470.

Segerson, Kathleen. 1988. Uncertainty and Incentives for Non-Point Source Pollution. *Journal of Environmental Economics and Management* **15**: 87-98.

Shortle, James S., and Richard D. Horan. 2001. The Economics of Nonpoint Pollution Control. *Journal of Economic Surveys* **15**(3): 255-289.

Spraggon, John. 2002. Exogenous Targeting Instruments as a Solution to Group Moral Hazards. *Journal of Public Economics* **84**: 427-456.

Weersink, Alfons, John Livernois, Jason F. Shogren, and James S. Shortle. 1998. Economic Instruments and Environmental Policy in Agriculture. *Canadian Public Policy* **24**: 309-327.

Xepapadeas, Anastasios. 1991. Environmental Policy under Imperfect Information: Incentives and Moral Hazard. *Journal of Environmental Economics and Management* **20**: 113-126.

Table 1. Summary of Experimental Results: Group Output and Efficiency

	No Mechanism (Periods 1-10)[a]		Mechanism (Periods 11-30)[b]	
	Output	Efficiency (%)	Output	Efficiency (%)
Tax/Subsidy				
Session 1	21.0	39.7	18.7	47.2
Session 2	22.9	17.4	19.2	53.2
Session 3	21.4	41.5	18.7	48.6
Session 4	22.9	22.3	18.2	75.6
Average	22.1	30.2	18.7	56.2
Tax/Subsidy with Cheap Talk[c]				
Session 1	22.0	33.3	13.1	-42.6
Session 2	21.1	36.4	14.7	29.5
Session 3	23.2	13.8	8.7	-221.2
Session 4	21.1	45.6	0.0	-461.5
Average	21.9	32.3	9.1	-174.0
Fixed Penalty				
Session 1	20.8	35.6	21.6	46.8
Session 2	22.8	20.3	21.2	44.6
Session 3	22.5	29.5	21.3	34.2
Session 4	22.6	11.8	21.0	40.4
Average	22.2	24.3	21.3	41.5
Fixed Penalty with Cheap Talk[c]				
Session 1	21.9	41.8	18.6	53.7
Session 2	22.1	7.4	17.3	84.2
Session 3	23.0	15.6	17.1	65.3
Session 4	22.9	18.5	17.3	85.1
Average	22.5	20.8	17.6	72.1
Combined Approach				
Session 1	22.9	22.6	20.1	65.9
Session 2	22.1	33.3	19.7	61.9
Session 3	21.9	36.4	18.1	13.8
Session 4	22.2	17.4	20.4	66.4
Average	22.3	27.4	19.5	52.0
Combined Approach with Cheap Talk[c]				
Session 1	23.0	14.9	18.4	80.1
Session 2	22.7	23.3	18.5	78.2
Session 3	21.8	24.1	17.0	63.5
Session 4	22.0	39.0	18.0	80.9
Average	22.4	25.3	18.0	75.7

[a] Theoretical prediction is an output of 24 units and efficiency of 0%.
[b] Theoretical prediction is an output of 18 units and efficiency of 100%.
[c] Informal group discussion allowed before periods 11 and 21.

Table 2. Hypothesis Test Results

Hypothesis	**Test Type**	***t*-test statistic (p-value)**
Sessions without Cheap Talk		
$Output_{tax/subsidy} = 18$	One sample	1.32 (0.280)
$Output_{fixed\ penalty} = 18$	One sample	16.01 (0.000)
$Output_{combined\ approach} = 18$	One sample	2.77 (0.070)
$Output_{tax/subsidy} = Output_{fixed\ penalty}$	two sample, unequal variances	-10.97 (0.000)
$Output_{tax/subsidy} = Output_{combined\ approach}$	two sample, unequal variances	-1.86 (0.136)
$Output_{fixed\ penalty} = Output_{combined\ approach}$	two sample, unequal variances	4.23 (0.013)
Sessions with Cheap Talk		
$Output_{tax/subsidy} = 18$	One sample	-2.86 (0.064)
$Output_{fixed\ penalty} = 18$	One sample	-7.98 (0.004)
$Output_{combined\ approach} = 18$	One sample	-1.25 (0.299)
$Output_{tax/subsidy} = Output_{fixed\ penalty}$	two sample, unequal variances	-2.62 (0.072)
$Output_{tax/subsidy} = Output_{combined\ approach}$	two sample, unequal variances	-2.70 (0.074)
$Output_{fixed\ penalty} = Output_{combined\ approach}$	two sample, unequal variances	-0.33 (0.763)

Note: mean output for the last 10 periods in a session are the units of observation.

Figure 1. Experiment Supply and Demand Schedules

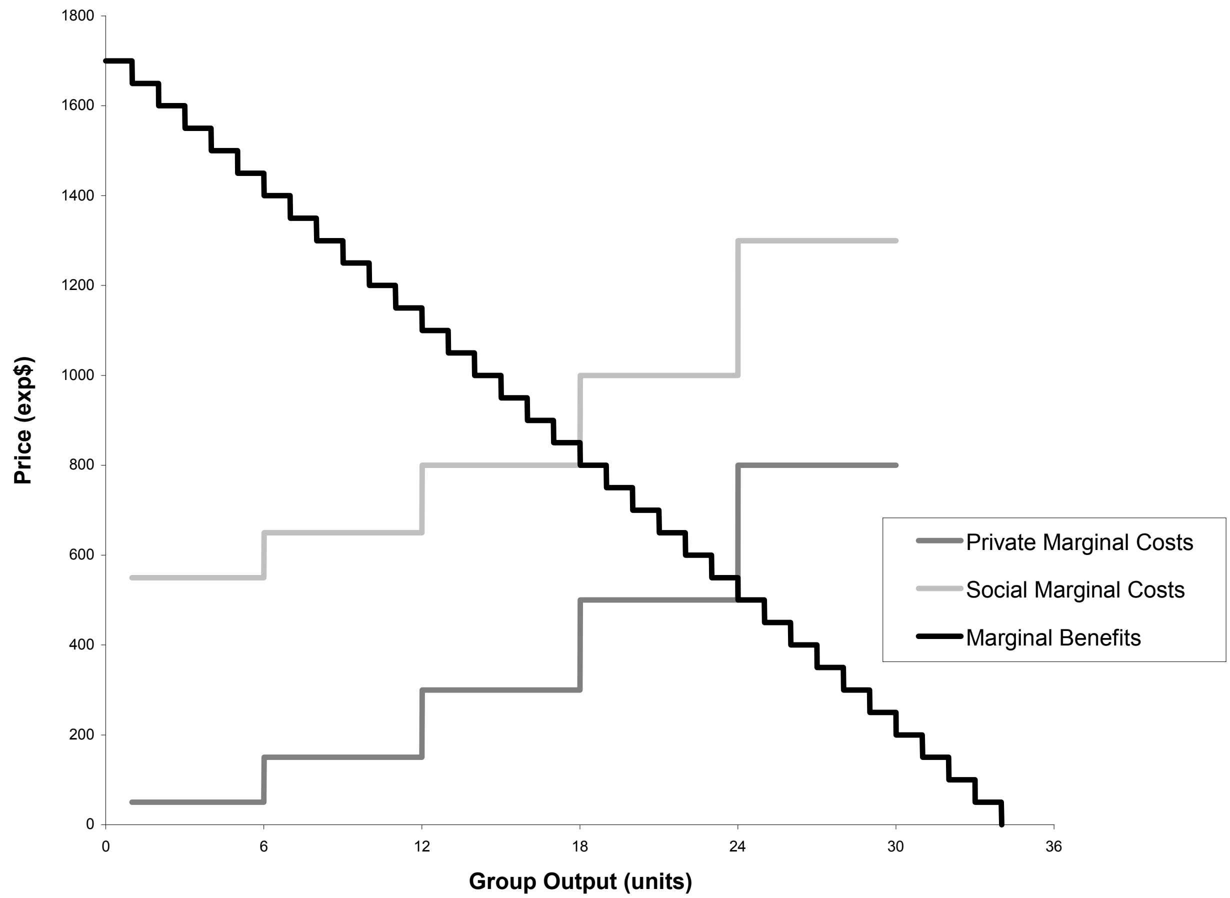
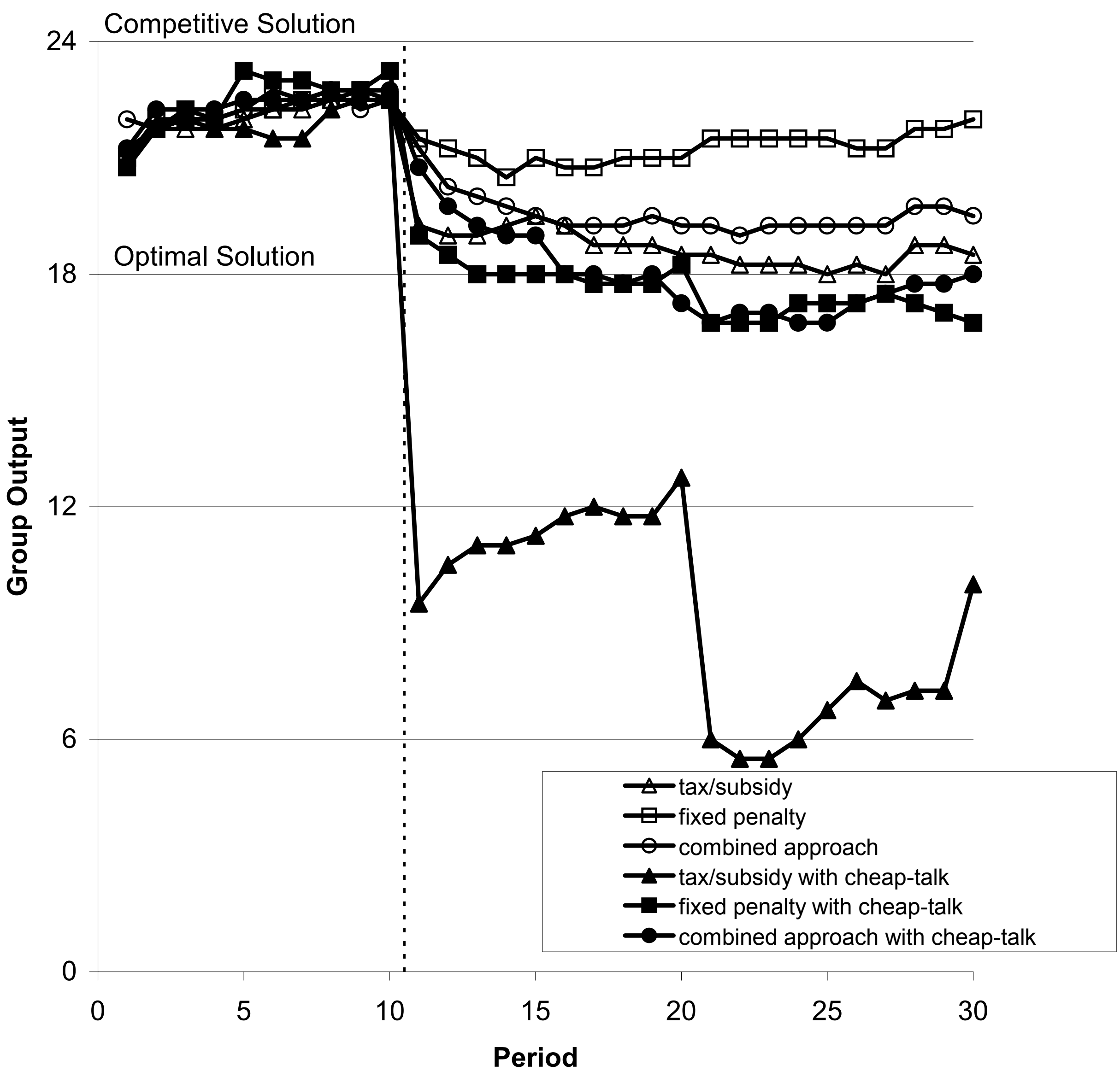
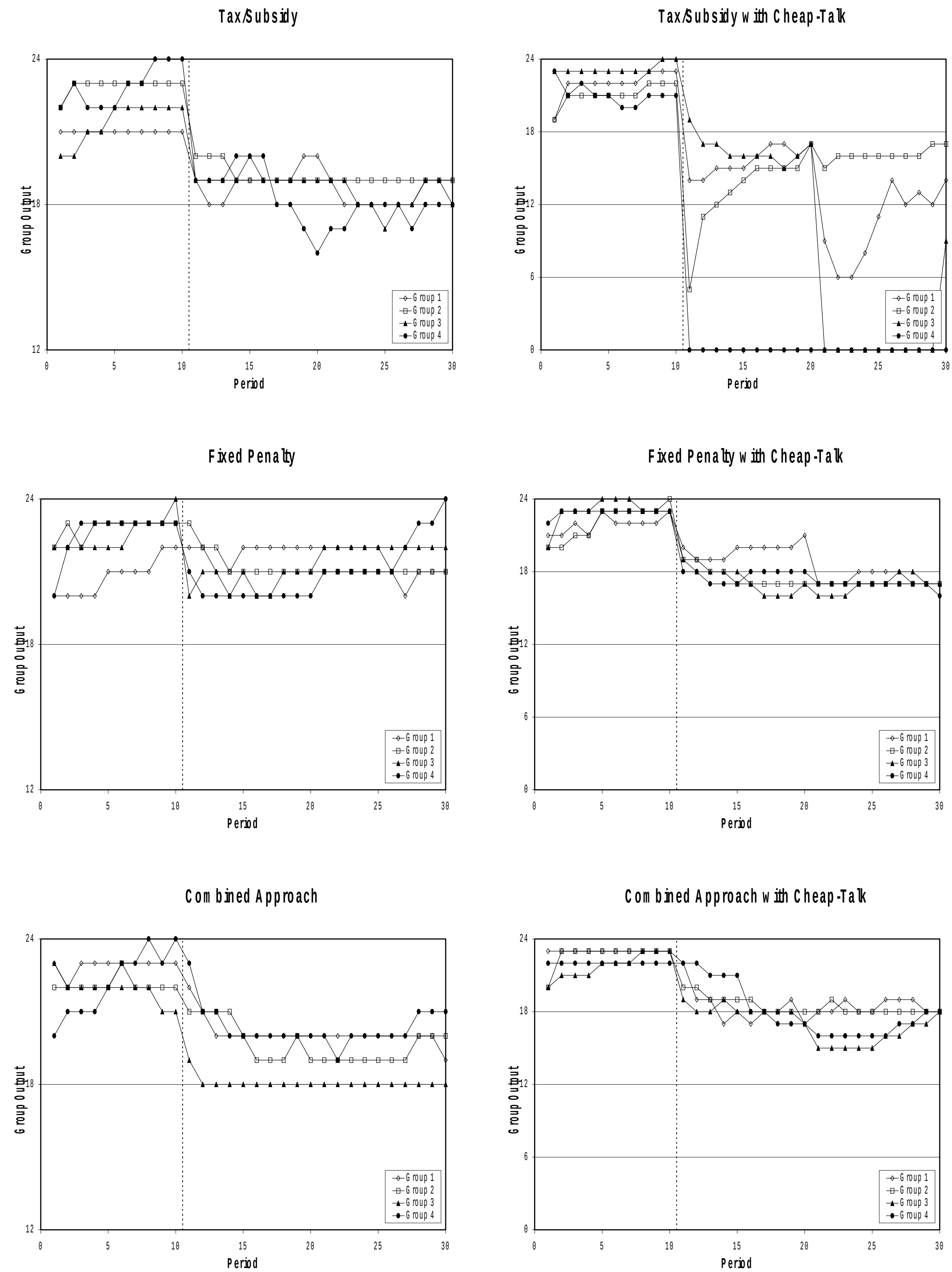


Figure 2. Average Time Series of Experiment Treatments



Note: Mechanism imposed in periods 11-30 only.

Figure 3. Time Series of All Sessions



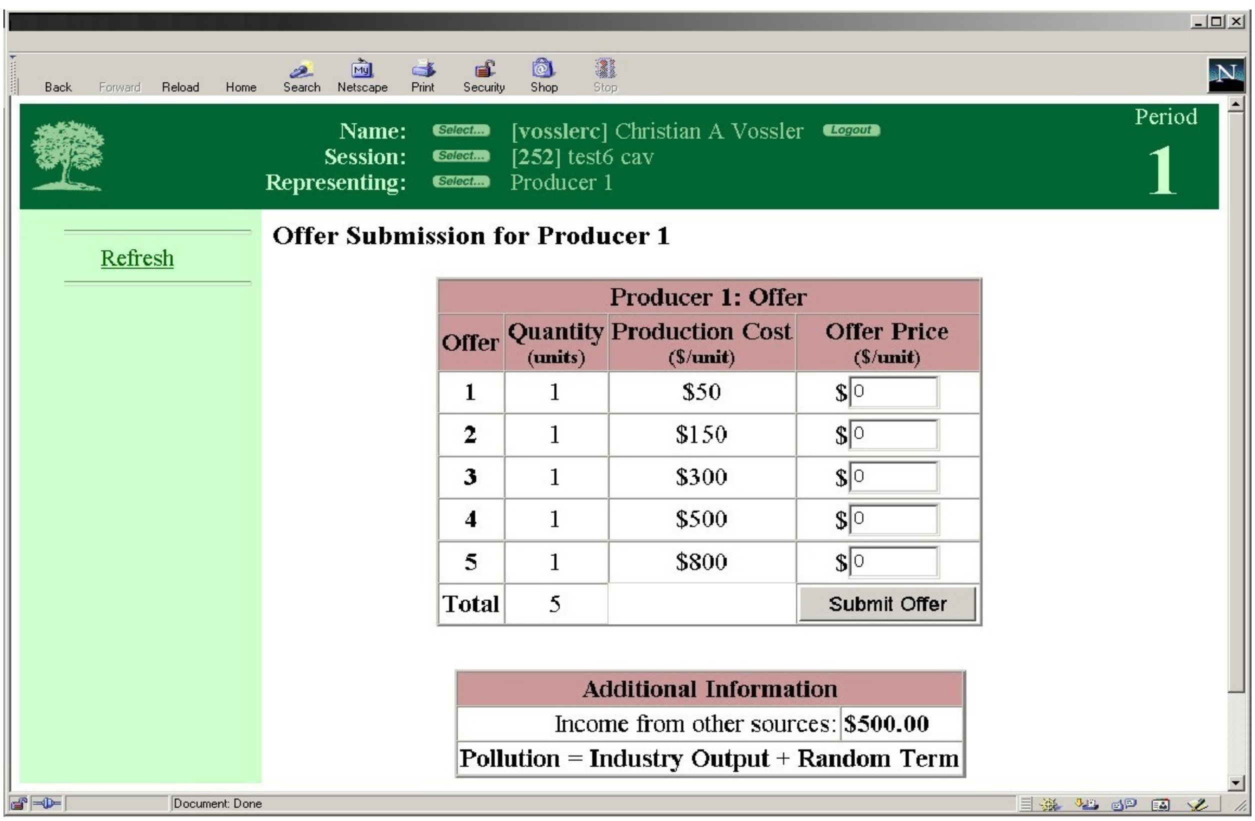
Appendix A. General Experiment Instructions.

Notes: **Bold** *typeface is used to highlight terms you will encounter during the experiment. There are concepts that are likely to be unfamiliar to you. Everything will be explained after you have read the instructions and you will be able to ask questions.*

Introduction

This is an experiment in the economics of decision making. The experiment has two parts, consisting of 10 and 20 trading periods, respectively. In this experiment, you operate one of six firms in an industry. Human participants control the five other firms. Your firm and the other five firms interact each trading period by competing to sell a product in a market. Because of this market interaction, your earnings depend on the behavior of others as well as your own behavior. The consumers in this market behave like experienced buyers and are represented by the computer. In each period, you have the opportunity to sell up to 5 units of your product. You only produce what actually gets sold. The cost of producing each of your 5 units is $50, $150, $300, $500, and $800, respectively. In other words, your **production costs** rise as your output increases. You determine the **offer price** - the price at which you are willing to sell your product - for each of these units and type these offers in the offer submission web page (shown below). This is the only decision you have to make and your goal is to make as much money as possible. In addition to product sales, in each period you earn $500 from your other business activities.

For every $2,000 earned in the experiment you will receive $1 in cash.



The six firms are located near a common water resource. Some by-products of your firm enter the water, affecting water quality. The more you and your competitors produce, the higher the level of water **pollution**. Under average conditions each unit produced by you or your competitors results in one unit of **pollution**.

A variety of factors such as stream flow and the rate of surface runoff affect **pollution** levels. For example, heavy rainfall increases surface runoff, increasing **pollution** levels. High stream flow results in relatively less **pollution**, as the ability of the waterway to assimilate your by-products increases. Unfortunately, factors such as these are unpredictable due to complex physical, chemical and biological relationships. Taking into account the uncertainty surrounding realized **pollution** levels, the relationship **Pollution** = **Industry Output** + **random term** holds. **Industry Output** is the sum of all units sold by you and the other firms in your market. The **random term** is equal to zero on average and takes on values between -1.0 and +1.0. Each number in this range has an equal chance of being selected.

Pollution directly affects the well-being of water resource users. For example, high **pollution** levels affect the health of fish, causing losses to fisherman. Also, poor water quality affects swimming and other recreation activities. In general, each unit of **pollution** results in a $500 loss in well-being to others. *The **pollution** level does not affect your profits in any way during the first part of the experiment but might in subsequent parts.*

Determining Market Price

The buying and selling of your industry's product is determined in a single price auction. In this auction, the simulated consumers submit bids to buy your product. These bids are ranked from highest to lowest in price. You and your competitors submit offers to sell your product. The offers are ranked from lowest to highest in price. In the auction, the cheapest offers are accepted in rank-order as long as the bid price is greater than the **offer price**. The **market price** is the average of the bid price and **offer price** of the last unit sold.

For example, suppose the **offer prices** are $4, $6, and $8, and the bid prices are $10, $7, and $5. Using the auction, 2 units are sold at a **market price** of $6.50. The consumer is willing to pay $7 for the 2nd unit while the producer is willing to sell this unit at a price of $6.00. A 3rd unit is not traded since the consumer is willing to pay $5, but the producer is only willing to sell at $8. So, this auction provides a way of making sure that only mutually beneficial trades take place. Note that you sell low cost units first. To make this happen in the market, your **offer price** for high cost units must not be less than your **offer price** for low cost units.

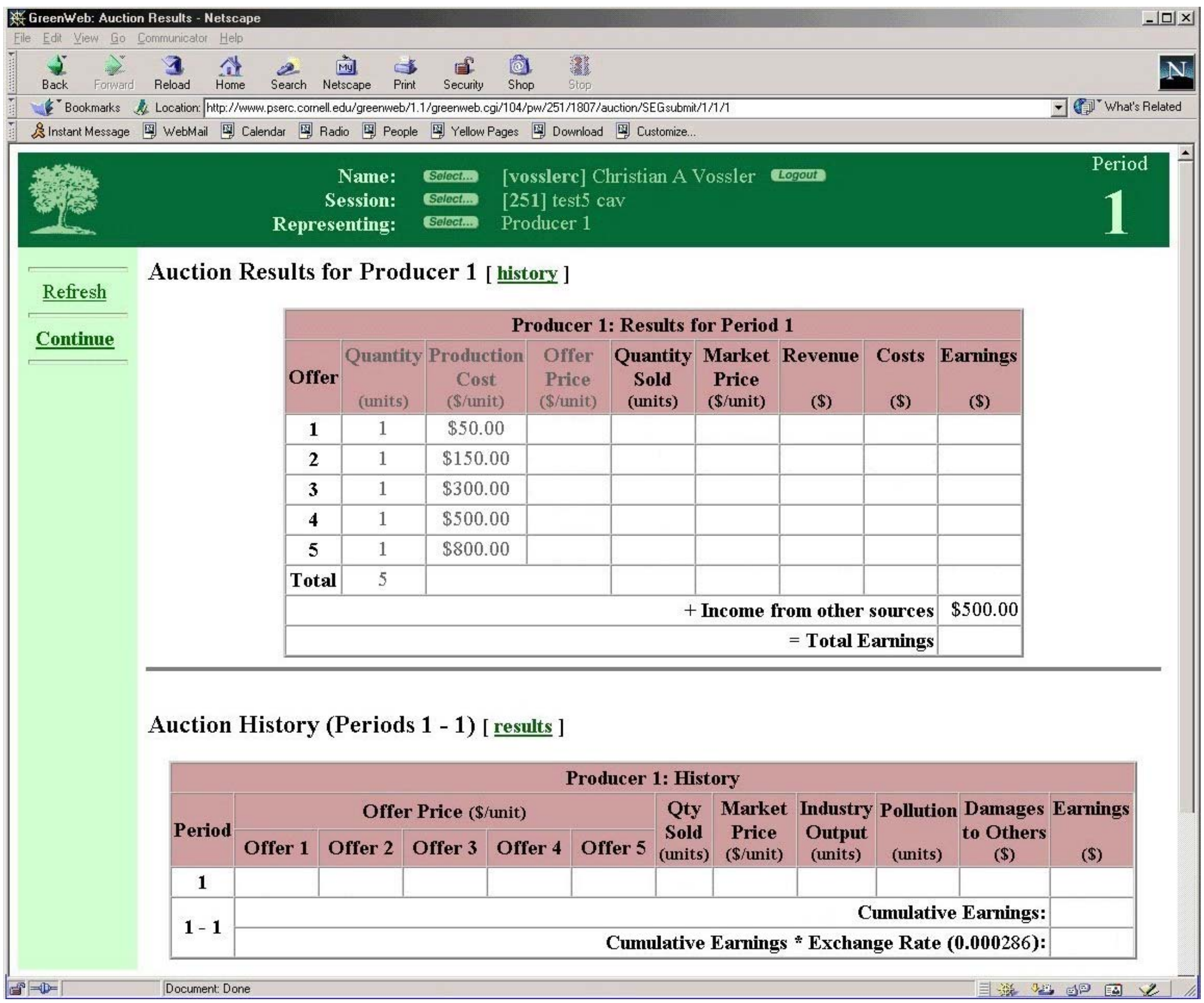
*Note: The **market price** may be higher than an unaccepted **offer price**, as a result of how the auction works. In the example above, suppose you submitted an **offer price** of $6.25. This price would be lower than the market price of $6.50, but you would not sell anything.*

Determining Your Earnings

The goal of this experiment is to choose your **offer prices** in order to make as much money as possible. After you submit offers, the computer determines the **market price** based on all offers submitted by consumers and the six firms in your market. You will then see a results page, similar to the one shown below.

The top table presents your **revenue**, **costs**, and **earnings** for each unit sold and displays your **total earnings** for the current period. Your **total earnings** are calculated as the difference between **revenue** and **costs**, plus the $500 in **income from other sources. Revenue** is the number of units sold multiplied by the **market price**. **Costs** are the total production costs for all units sold.

The bottom table presents the results from each trading period you have been through. It also displays information on the **industry output** and **pollution** for each period. The last two rows present your overall experimental earnings and actual earnings (your take home pay).


GreenWeb: Auction Results - Netscape

File Edit View Go Communicator Help

Back Forward Reload Home Search Netscape Print Security Shop Stop

Bookmarks Location: http://www.pserc.cornell.edu/greenweb/1.1/greenweb.cgi/104/pw/251/1807/auction/SEGsubmit/1/1/1 What's Related

Instant Message WebMail Calendar Radio People Yellow Pages Download Customize...

Name: Select... [vosslerc] Christian A Vossler Logout
Session: Select... [251] test5 cav
Representing: Select... Producer 1

Period 1

Refresh

Continue

Auction Results for Producer 1 [history]

Producer 1: Results for Period 1								
Offer	Quantity (units)	Production Cost ($/unit)	Offer Price ($/unit)	Quantity Sold (units)	Market Price ($/unit)	Revenue ($)	Costs ($)	Earnings ($)
1	1	$50.00						
2	1	$150.00						
3	1	$300.00						
4	1	$500.00						
5	1	$800.00						
Total	5							
							+ Income from other sources	$500.00
							= Total Earnings	

Auction History (Periods 1 - 1) [results]

Producer 1: History												
Period	Offer Price ($/unit)					Qty Sold (units)	Market Price ($/unit)	Industry Output (units)	Pollution (units)	Damages to Others ($)	Earnings ($)
	Offer 1	Offer 2	Offer 3	Offer 4	Offer 5						
1											
1 - 1										Cumulative Earnings:	
										Cumulative Earnings * Exchange Rate (0.000286):	

Document: Done

Appendix B. Supplemental Instructions for Tax/Subsidy Treatment with Cheap Talk.

Part 2 of this experiment is identical to Part 1, with one important exception. In order to protect the water resource, the regulatory authority believes that the **pollution** level should be at or below 18 units for every trading period. In order to achieve this, the authority implements the following policy: If **pollution** is less than 18 units, you receive a **subsidy** of $500 for *each* unit of **pollution** less than 18 units. If **pollution** is greater than 18 units you pay a **tax** of $500 for *each* unit of **pollution** above 18 units.

Pollution is determined by the production from ALL SIX FIRMS in your market. Recall that pollution is equal to the total **industry output** plus a **random term** that takes on values between -1.0 and +1.0. On average, **pollution** is equal to **industry output**. NOTE THAT WHETHER YOU RECEIVE A **SUBSIDY** OR PAY A **TAX** WILL DEPEND NOT ONLY ON YOUR OWN OUTPUT (AND HENCE **POLLUTION**) BUT ON THE OUTPUT (**POLLUTION**) OF ALL THE OTHER FIRMS AS WELL.

In the results table you will see a new row corresponding to the **tax** or **subsidy** amount – whichever is applicable. As before, information on the **pollution** and **industry output** is found in the history table.

Examples

Suppose that **industry output** is 20 units and the **random term** is 0.0. Therefore, **pollution** is 20 units. Since **pollution** is two units greater than 18, everyone pays a **tax** of 2*$500=$1,000.

Suppose that **industry output** is 17 units and the **random term** is -1.0. Therefore, **pollution** is 16 units. Since **pollution** is two units less than 18, everyone receives a **subsidy** of 2*$500=$1,000.

Group Discussion

Before period 11 begins, the firms in your market will have 5 minutes to informally discuss the new policy. The only guidelines for the discussion are that you cannot make any threats to the other participants and there are to be no side payments of any kind.

There will be another group discussion later on in the experiment.

OTHER A.E.M. WORKING PAPERS

WP No	Title	Fee (if applicable)	Author(s)
2002-34	Making the Decision to Merge: A Case Study of U.S. Dairy Cooperatives		Henehan, Brian
2002-33	An Exchange on the Compensation Principle in Resettlement		Cernea, M. and R. Kanbur
2002-32	Stable Partnerships, Matching, and Local Public Goods		Kanbur, R. and S. Clark
2002-31	Samuelson Machines and the Optimal Public-Private Mix		Kanbur, R. and S. Clark
2002-30	An Incentive Compatible Self-Compliant Pollution Policy under Asymmetric Information on Both Risk Attitudes and Technology		Peterson, J. M. and R. N. Boisvert
2002-29	Market Efficiency, Competition, and Communication in Electric Power Markets: Experimental Results		Chapman, D., C.A. Vossler, T.D. Mount, V. Barboni, R.J. Thomas and R.D. Zimmerman
2002-28	The Shadow Economy in Post-Soviet Tajikistan		S. Kyle
2002-27	Are Joint Ventures with Local Firms an Efficient Way to Enter a Culturally Distant Market? The Case of Japanese Entry into the United States		J. F. Francois, T. Roehl, and J. M. Hagen
2002-26	Causes and Consequences of Food Retailing Innovation in Developing Countries: Supermarkets in Vietnam		Hagen, J. M.
2002-25	Management of National Parks in Developing Countries: A Proposal for an International Park Service		Chapman, D.
2002-24	Subsidized Vehicle Acquisition and Earned Income in the Transition from Welfare to Work		M. T. Lucas and C. F. Nicholson
2002-23	Effects of Food and Health Spending Patterns on the Health of the Elderly		Gómez, M. and C. K. Ranney
2002-22	Do Healthier Diets Cost More?		Ranney, C. K. and P. E. McNamara
2002-21	Population Growth and Poverty Measurement		Chakravarty, S. R., R. Kanbur and D. Mukherjee
2002-20	Achieving Efficiency and Equity in Irrigation Management: An Optimization Model of the El Angel Watershed, Carchi, Ecuador		Evans, E. M., D. R. Lee, R. N. Boisvert, B. Arce, and T. S. Steenhuis

Paper copies are being replaced by electronic Portable Document Files (PDFs). To request PDFs of AEM publications, write to (be sure to include your e-mail address): Publications, Department of Applied Economics and Management, Warren Hall, Cornell University, Ithaca, NY 14853-7801. If a fee is indicated, please include a check or money order made payable to Cornell University for the amount of your purchase. Visit our Web site (*http://aem.cornell.edu/research/wp.htm*) for a more complete list of recent bulletins.