DETERRING COLLUSION: SOME EXPERIMENTAL EVIDENCE ON THE RELATIVE EFFECTIVENESS OF CHANGES IN DETECTION AND SANCTION LEVELS

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INTRODUCTION

Recent theoretical and empirical analysis suggests that collusive price fixing is most efficiently controlled by the infrequent imposition of relatively large fines or penalties. Applying the economic analysis of crime to the antitrust area, Elzinga and Breit (1973) demonstrate in a rather straightforward manner that a low probability of discovery coupled with very large penalties for those convicted of collusion is the optimal method of deterring price fixing as long as firms are not risk takers.[1] The implication of their finding is that one ought to expend less effort finding antitrust violations and concentrate more on punishing those we do apprehend. This, in part, follows from Elzinga and Breit's assertion that antitrust violators are likely to me more responsive to the size of the penalty than to the chance of detection.

In an empirical study of antitrust enforcement in the highway construction industry, Block et al. (1985) presented evidence that is consistent with Elzinga and Breit's hypothesis. Essentially, these authors found that markups in the paving industry appear to have been more responsive to the increases in the severity of antitrust sanctions that occurred in the late 1970s and early 1980s than they were to the increases in the chances of being indicted for collusion which occurred during the

[1]This result abstracts from the problem of mistakes in enforcement. If such mistakes are considered then it is no longer obvious that a large penalty with low detection probability is an optimum strategy for controlling collusion (see, for example, Block and Sidak (1981a)). The Breit and Elzinga results also assume away the possibility of imposing an expected sanction that is in effect large enough to cause collusion to have a negative expected value for all colluders. For as we know, if the expected value is negative only risk takers will engage in the activity. If risk takers provide the only pool of potential colluders, then it may be optimal to have high detection rates and moderate penalties.

same period. The data, at least in the highway construction industry, does seem to suggest that increases in the severity of the sanctions for collusion is a more powerful technique for inducing compliance with the antitrust laws than increasing detection levels.[2]

Interesting as these empirical findings are, they are only suggestive. Estimating the relative effectiveness of increases in enforcement and punishment with any degree of precision is virtually impossible with the type of data available in the antitrust area. There is simply not enough independent variation in enforcement and punishment levels to accurately parcel out the effect of each on the level of collusion.[3] Compounding this problem is the more basic question of how to measure both the level of collusion and the level of enforcement. In Block et al. (1985) the argument is made that the level of the markup itself is the appropriate measure of collusion. While this is undoubtedly true in theory, the practical problems involved in actually measuring the markup should not be underestimated. In practice devising a robust measure of collusive

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[2]Of course even if collusion were more responsive to changes in penalties than detection rates it still might not be optimal to punish only a few violations quite severely. If increases in sanction level were only attainable at a high relative cost, it might pay to use high detection levels and low penalties. This, however, does not appear to be the case. While punishing an offender is now an expensive business, it is still relatively cheaper than detecting and prosecuting violators.

[3]Perhaps the best illustration of this problem is in highway construction. During the period when antitrust enforcement in that industry (1975-82) was most active, both detection levels and punishment levels moved together. This was an industry that was hit with the explosion in the enforcement after 1975. Unfortunately for statistical purposes, sanction levels also 'exploded' during that same period. For more details on this problem see Feinstein et al. (1985) and Block et al. (1985).

activity is likely to prove elusive in all but a handful of markets.[4] Moreover, even in those markets where such a measure exists it will not always be possible to construct a reliable measure of enforcement. For example, in Block et al. (1985) while the authors were able to devise a robust measure of collusion they were not able to extend this to the enforcement variable.[5]

Because developing the empirical measures necessary to assess the absolute and relative impacts of increases in enforcement and punishment on the degree of collusion present so many difficulties this would seem to be an area in which an experimental test of theory might be especially appropriate. While "laboratory" tests of economic theory have their own shortcomings, especially in terms of external validity, they can be a useful alternative when the use of field data and/or field experiemnts does not provide an adequate method of confronting the predictions of theory.

In this paper we use experimental methods to test the predictions of theory concerning the deterrence of collusive price setting. Specifically we test the prediction that collusive markups decline in both the

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[4]Although the estimate of the markup in Block et al. (1985) did quite well at predicting collusion, it is not clear that equivalently reliable estimates of markups are available for other markets. In fact, in most cases it would not seem possible to even carry out a test of the reliability of this proxy for collusion. It was only because in one state (North Carolina) the Attorney General had obtained the cooperation of indicted contractors in identifying past collusion contracts that the authors were actually able to test the predictive power of their measure of collusion.

[5]In this case, because these authors were unable to observe (or at least directly estimate) the number of collusive bids, they were unable to come up with an unambiguous empirical proxy for the probability that a collusive bid would be detected.

probability of detection and the severity of the penalty. In addition, we test for the implications of risk aversion both in terms of the odds required by colluders to engage in this risky activity and in terms of the relative responsiveness of colluders to changes in detection and sanctions levels.[6] In the later case we test whether collusion is, in fact, more sensitive to changes in the severity of penalties than to the certainty of detection.

We would argue that finding consistency with the predictions of risk aversion in a laboratory setting has powerful implications for behavior in the "real world." After all, if colluders act in a risk averse manner in a laboratory, where gains as well as losses are modest and the environment is well controlled, can we really expect any less from agents in actual market situations where the gains and losses are likely to be extremely large and where there is uncertainty attached to virtually every aspect of their behavior and the environment.[7] Finding that subjects in a controlled laboratory environment react in a manner that is consistent with risk aversion, i.e., their behavior is more strongly influenced by changes in penalty levels than detection levels, would create a strong presumption that managers in a real world setting would behave in a similar manner.

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[6]Because antitrust violations are committed by managers of legitimate business firms, the predominance of risk aversion seems to be a plausible assumption (see Breit and Elzinga (1985) p. 429).

[7]Moreover, since the subjects used in this experiment (young undergraduate college students) may tend to be slightly less risk averse than a typical group of business managers, finding consistency with risk aversion here would tend to bolster our confidence in the results.

A SIMPLE MODEL OF DETERRENCE

We now formalize our discussion of deterrence by specifying the choice problem facing a cartel in an economy where collusive price fixing is illegal. Assuming that the cartel is unconcerned with the distribution of profits among its members, it will set prices at the monopoly level whenever the following expression is positive:[8]

$$L \equiv (1-\delta)\, U(e+\pi^*) + \delta U(e+\pi^*-F) - U(e) \qquad (1)$$

where U(.) is the cartel's objective function which has the property that $U'(.)>0$, δ is the probability that the cartel (having set the price at a monopoly level) will be discovered, π^* is the profit from setting the price at the monopoly level, F is the penalty if the cartel is discovered, and e is the total earnings of the cartel members under a competitive regime.

It is apparent from Eq. 1 that the likelihood that a cartel will find collusion an optimal strategy decreases in both the probability of detection, δ, and the severity of the sanction, F, and increases in the potential gain from collusion, π^*. Note that if the cartel's objective function displays risk aversion ($U''<0$), L will only be positive when there are expected gains from price fixing; i.e., only when $\pi^*-\delta F>0$, and it will not always be positive even in these cases. In fact, if the cartel's objective function is characterized by decreasing absolute risk aversion, the likelihood that L will be positive, and hence the cartel will choose

[8]We simplify the problem below by considering that the only alternatives facing the cartel are the competitive price (no collusion) or the monopoly price (full markup). As long as the probability of detection is independent of the markup, these will in fact be the only two choices considered by the cartel (see Block et al., 1981b).

collusion, will, all things being equal, increase earnings (e) of the firms (or their agents) in the cartel increase.[9]

If the antitrust authorities set δ and F such that $\pi^*-\delta F<0$ only risk takers will even be interested in collusion. By making collusion an unfair bet the authorities can in effect choose to make price fixing attractive only to 'gamblers.' On the other hand if, as is likely to be the case in practice, the detection and penalty levels cannot be set so as to make collusion an unfair bet, individuals with quite different attitudes toward risk will all be potential price fixers.[10]

Now it is straightforward to establish that the deterrent effect of increasing the magnitude of the penalty for collusion is more, less or just as powerful as an equivalent increase in the chances of detection, as the cartel's objective function evidences risk aversion, risk preference or risk neutrality. In other words, the elasticity with respect to F is greater than, equal to, or less than the elasticity with respect to δ as U" is less than, equal to, or greater than zero. Consequently it is, of course, also true that an increase in the enforcement level that is <u>compensated</u> for by a decrease in the penalty, i.e., an increase in δ such that

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[9]Proof of this follows by differentiating L with respect to e which gives

$$\partial L/\partial e = U'(e + \pi^*) - U'(e) - \delta[U'(e + \pi^*) - U'(e + \pi^* - F)]$$

the mean value theorem implies there exists a $\overline{w}$ and $\hat{w}$ such that $\partial L/\partial e = \pi^* U''(\overline{w}) - \delta F U''(\hat{w})$ for $e \leq \overline{w} \leq e + \pi^*$ and $e + \pi^* - F \leq \hat{w} \leq e + \pi^*$, now $\partial L/\partial e$ written this way is positive if expected profit from collusion is positive, i.e., $\pi^* > \delta F$ and $U''(\overline{w}) > U''(\hat{w})$. However, decreasing absolute risk aversion implies $U'''(w) > 0$ and $\overline{w} > \hat{w}$ which further implies $\partial L/\partial e$ is positive.

[10]Policy makers may object to high fines on grounds of 'equity' or they may argue that high fines may actually deter legitimate business practices (see Breit and Elzinga, 1973).

$$dE(\pi) = -d\delta F - \delta dF = 0 \tag{2}$$

or

$$dF/d\delta = -F/\delta, \tag{3}$$

will lead to an increase, no change or a decrease in the extent of collusion as the cartel evidences risk aversion, risk neutrality or risk preference.[11]

Hence, whether the degree of collusion in any market is more or less responsive to changes in the penalty level than changes in the detection level will depend both on the ability of the antitrust authorities to influence the expected payoff from collusion and on the inherent distribution of attitudes toward risk in the population of managers. If the antitrust authorities can set detection levels and sanction levels such that collusion is an unfair bet, whatever collusion remains will be more responsive to change in detection levels than sanction levels.[12] This would be the result of having chased all of the risk avoiders out of the

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[11]In this case, $\frac{dL}{d\delta} = \frac{\partial L}{\partial \delta} + \frac{\partial L}{\partial F}\frac{dF}{d\delta}$, now partial differentiating (1) with respect to both δ and F and substituting these results along with (3) into this above expression for $\frac{dL}{d\delta}$ implies

$$\frac{dL}{D\delta} = U(e + \pi^* - F) - U(e + \pi^*) + U'(e + \pi^* - F)$$

the mean value theorem gives

$$\frac{dL}{d\delta} = F[U'(e + \pi^* - F) - U'(w^*)]$$

for $e + \pi_n - F \leq w^* \leq e + \pi^*$. Therefore $dL/d\delta$ is negative, zero or positive whenever $U''(.)$ is positive, zero or negative. A similar result was proven by Christiansen (1980) for the crime of tax evasion. In his model, a risk averse individual will lower the level of income tax evaded if the penalty level is increased and the detection level is lower, holding the expected value from tax evasion constant.

[12]This is strictly true only for increases in δ and F. Decreases in δ and/or F could cause the expected profit to become positive ($\pi^*-\delta F \geq 0$) in which case risk neutral or risk averse managers might predominate.

pool of potential price fixers by setting δ and F such that $\pi^*-\delta F<0$.

However, if, as is likely to be the case, the expected gains from collusion cannot be eliminated and, $\pi^*\delta F>0$, then the relative responsiveness of collusion to change in detection and sanction levels will depend on the distribution of attitudes toward risk in the population of managers. Under these circumstances, to the extent that risk averse managers predominate, increases in the level of penalties will be a more powerful mechanism for assuring compliance with the antitrust law than equivalent increases in detection levels.[13]

[13]This or course holds only for 'small' increases in δ and F, since increases in δ and F sufficient to cause $\pi^*-\delta F<0$ would reverse this result.

EXPERIMENTAL DESIGN

In order to actually test the absolute and relative impact of changes in detection rates and sanction levels on collusive behavior we used an economic laboratory experiment.[14] One of the most important characteristics of this experiment, and of most economic experiments, is that the decisions made by the subjects have direct and nontrivial monetary consequences.[15] This characteristic gives subjects an incentive to use their participation in the experiment in the most effective and maximizing manner. Lackadaisical behavior on the part of a subject will usually be costly for the subject. The strong link between performance

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[14]True experimental tests of the deterrency hypothesis are quite rare. A search of the recent literature on deterrence revealed only two laboratory tests or 'simulations' of deterrence in areas related to collusion. In Friedland et al. (1978) and Friedland (1982) the authors use experimental methods on human subjects (law students in one case and undergraduate psychology majors in the other) in a laboratory setting to test the deterrent effect of stochastic audits and penalties on tax evasion. While the results of these experiments are not inconsistent with the deterrence hypothesis (i.e., increases in expected sanctions reduce tax evasion), neither one was designed to actually test this hypothesis.

The major concern in these papers was of the relative impact of changes in enforcement and sanction levels on behavior. In Friedland et al. (1978) the researchers found that severity is a more powerful deterrent than certainty (. . . 'large fines tend to be a more effective deterrent than frequent audits' . . .) however in Friedland (1982), the author claims that 'the results of this study support the proposition that variation in the probability of threat enactment has a stronger effect on the threatened person's behavior than the variation in the severity of these threats.' The results in Friedland et al. (1978) and Friedland (1982) on the relative effects of certainty and severity appear to be contradictory. Unfortunately at this point there are no unambiguous experimetnal findings on the relative effectiveness of changes in detection and sanction levels.

[15]A bothersome aspect of both tax evasion simulations referred to in Fn.14 is that the incentives for consistent behavior do not appear to have been extremely strong. For example, in Friedland et al. (1978) the instructions to the subjects state: 'A small money prize will be divided up among you, at the end of the game, in proportion to each person's total net income.' A similar instruction was read to the students in Friedland (1982).

and monetary reward in these experiments increases the likelihood that subjects will act so as to maximize their earning or other benefits (utility) from the experiment.

In the specific experiments reported on below, subjects were given the opportunity to earn money (or 'profits') by participating in an auction.[16] During some experiments there were substantial monetary gains to be had for colluding with other auction participants, i.e., for forming and operating a sellers' cartel. Antitrust enforcement was simulated by the random imposition of penalties on colluding sellers. If a cartel was 'detected', a penalty was assessed against the subjects' profit. Since penalties for collusion had a direct and significant impact on subjects' total earnings, there was a strong financial incentive to avoid them.[17]

All experiments were run as sealed offer auctions. Subjects acted as sellers and attempted to win a production contract which granted them the exclusive right to sell a 'ficticious' commodity. Each experiment consisted of a series of trading weeks each composed of five periods or 'trading days'. Every trading day or period sellers would write their offers to sell on a slip of paper. After collecting the offers from all

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[16]The average payment to subjects in these experiments was about $65 which represented 25% of the subjects' average monthly income in the month before the experiment.

[17]Empirical results from the psychology literature provide some indication of how firms may respond to the possibility of paying large fines for price fixing. In one example reported by Kahneman and Tversky (1979), 98 individuals were asked to choose between an eight out of ten chance of losing $4,000 versus a certain loss of $3,000. In this hypothetical situation, 92 percent of the individuals indicated they preferred the risk of losing $4,000 to the certain loss of $3,000. This result indicates that individuals are risk-taking in losses, therefore a group of individuals acting as a cartel may choose to risk a large penalty rather than give up the monopoly profits from collusion, even when the expected profit from collusion is slightly negative.

sellers, the offers would be ranked from lowest to highest. The individual that submitted the lowest offer was declared the winning seller and all purchases were made from that seller. Only the amount of the winning offer and quantity sold would be announced and written on a chalkboard.

No subjects were used as buyers. The quantity purchased was determined by a demand rule generated prior to the experiment. The only information subjects received about this demand rule was the maximum quantity that could be purchased during one trading day. In terms of cost information subjects were given their per unit production cost for an entire trading week. They were given only their production costs and were not told the production costs of the other sellers or even the distribution from which these production costs were drawn. In all experiments sellers were given the same constant per unit costs.[18] The subjects were students at the University of Arizona and were recruited through an advertisement at the student employment office and by announcements in undergraduate economic classes.

Twenty-four subjects participated in five separate sealed-offer experiments over a six week period.[19] The five experiments were of three types: 1) One was a competitive trainer; 2) one was a cartel trainer; and

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[18]It would have greatly complicated the cartel's decision process and the experimental design to have actually drawn sellers' costs from a nontrivial distribution. Moreover, pilot studies suggested that this complication would not have affected our results. See Block and Gerety (1985) for a report on these pilot studies.

[19]Five groups completed all of the experiments in the four weeks prior to 'spring break' at the University. However because one group did not complete its final experiment until the week after spring break, the entire set of experiments took six weeks to complete.

3) three were what we refer to as cartel-deterrence experiments.[20] Subjects were paid a $3.00 participation fee for each experiment and this fee was paid on the day of the experiment. The subjects also earned profits in the experiment as sellers and these profits were carried over from one experiment to the next. After the final experiment subjects were given a check for the profits they had accumulated over all five experiments.

Competitive and Cartel Trainers

The experiments began with the competitive trainer in which twenty-eight subjects participated in groups of three, six, seven and twelve subjects each.[21] Subjects were not permitted to talk during this experiment. The competitive trainer served two purposes: 1) it provided training for subjects in the mechanics of the experimental auction process and 2) it provided a baseline or reference point for the 'competitiveness' of the sealed-offer auction with identical constant cost firms when communication between sellers is prohibited.[22] The competitive trainer was followed by the second type of experiment, the cartel trainer.

The twenty-four subjects who participated in the cartel trainer were divided into six groups. The characteristic that differentiated the cartel

[20]A complete copy of the instructions for the three types of experiments can be found in Appendix A.
[21]Four subjects discountinued their participation in the experiments after the competitive trainer because of scheduling conflicts.
[22]The sealed offer auction with price setting firms has many characteristics in common with the Bertrand oligopoly model. Therefore, the theoretical prediction of the competitive equilibrium is price equal to the winning firms constant per unit production cost.

trainer from the competitive trainer was the possibility of communication between sellers. A brief 'recess period' was provided for the sellers prior to each trading day and the subjects were told that during the recess period they were free to talk to one another about any topic. Obviously the cartel trainer gave sellers the opportunity to collude. However since demand was unknown, sellers had to search out the optimal price, i.e., the price that maximized cartel profits.

The cartel trainer acted both as a test of simple cartel theory and as a mechanism for sellers to accumulate profits. The profits from these experiments were carried over to the deterrence experiment where sellers faced the possibility of penalties. This was an important element of the experimental design since for penalties to have a deterrent effect the sellers must have sufficient actual profits at risk. They must, in fact, have enough profits so that each seller is always solvent enough to meet his/her penalty obligation and the use of a profit buildup during the cartel training period helped assure that this condition held.[23] Also, allowing subjects to accumulate earnings in this fashion may increase the saliency of the experiments, and hence its external validity.

Two demand specifications were used in the cartel trainer. Demand I had a maximum quantity of 20 units, and given the cost specification, a monopoly markup of $0.25, a monopoly quantity of 10 units and a monopoly profit of $2.50. Demand II had a maximum quantity of 30 units, a monopoly markup of $0.25, a monopoly quantity of 15 units, and a monopoly profit of $3.75. Demand I was used for the first six to fifteen periods. Since

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[23]There was no bankruptcy in these experiments. If a seller's total profits were negative at the end of all experiments then he/she was paid nothing.

subjects did not know the exact demand rule, they were allowed to search for the monopoly price. If the subjects found the monopoly price for Demand I and the winning offer remained unchanged for three periods, the demand rule was changed to Demand II; otherwise they were given up to fifteen periods to search for the monopoly price. Demand II was used for the next five to fourteen periods. The exact number of periods depended on the group's ability to find the monopoly price for Demand I. Again the subjects were allowed to search for the monopoly price.

The cartel trainer also functioned to introduce subjects to the random penalty process that was used to simulate antitrust enforcement. After the twentieth period of the cartel trainer, new instructions were given to the subjects. These instructions explained the penalty process. Specifically subjects were told that whenever the winning offer was greater than the minimum cost of all sellers there was a chance that a penalty would be assessed against all sellers. The 'contribution rule' was simple. If a penalty is assesed that penalty is split equally among all sellers in the experiment.[24] At the beginning of each period, the size of the penalty and probability that it would be imposed was announced. After announcing the penalty and probability levels but prior to the making of offers, the sellers were given a recess period. After the offers were submitted, if the winning offer was greater than the minimum cost of all sellers, a bingo cage was used to determine whether a penalty would actually be imposed. The bingo cage contained 20 balls and

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[24]Block and Gerety (1985) reported on experiments where the winning seller was required to pay the entire penalty. This 'contribution rule' in some cases forced subject's profits negative and was therfore of limited utility in testing simple deterrence theory.

depending on the probability level, 2, 4 or 10 of these bingo balls were designated as penalty balls.[25] If on a roll of the bingo cage one of the penalty balls was chosen, then the subjects had to pay the penalty; otherwise no penalty was charged.[26] The instructions used to explain the penalty mechanism also informed the subjects the experiment was guaranteed to run ten additional periods or 'trading days' and that after the tenth period there was a one-tenth chance that the experiment would end after that period (subjects were not told the experiment would be automatically terminated if this random process did not end the experiment within five periods).

Table I presents the penalty specifications used for the periods twenty-one through thirty-five of the cartel trainer. It also gives the demand rule that was used for each period as well as the expected profit from collusive price setting in each of these periods. A contract bonus was used to compensate the winning seller when the winning offer equaled that seller's cost. It equaled $0.50 for the first twenty periods and increased to $1.00 for the remainder of the cartel trainer and for the

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[25]The bingo cage and bingo balls were in full view of the subjects. Subjects were allowed to inspect the bingo balls to verify the actual numbers of penalty and nonpenalty balls being used in any trading period. Moreover when the bingo cage was used the bingo ball chosen was given to one of the subjects and the subject read the number on the ball aloud.

[26]The penalty in these experiments became a possibility only if price was set above minimum cost. Subjects could talk about prices during the recess period but if they decided not to set price above the minimum cost they would not be subject to the penalty mechanism. In other words, as long as collusion resulted in a 'competitive' outcome the colluders were not subject to 'antitrust enforcement' in these experiments. An alternative rule, and perhaps one somewhat closer to actual practice might make exposure to the penalty mechanism a function of whether the subjects agreed to meet, i.e., collude, rather than a function of what collusive decision they reached.

TABLE I

CARTEL TRAINER

DEMAND AND PENALTY SPECIFICATIONS

PERIOD	DEMAND*	PROBABILITY	PENALTY	EXPECTED PROFIT	PERIOD TYPE**
21	II	.25	$ 5.00	+2.50	L
22	II	.25	14.00	+ .25	L
23	II	.25	10.00	+1.25	L
24	II	.25	20.00	-1.25	L
25	II	.25	30.00	-3.75	L
26	I	.25	14.00	-1.00	L
27	I	.25	5.00	+1.25	L
28	I	.25	10.00	+0.00	L
29	I	.25	30.00	-5.00	L
30	I	.25	20.00	-2.50	L
31	I	.1	25.00	+0.00	RE
32	I	.25	10.00	+0.00	RE
33	II	.5	10.00	-1.25	RE
34	II	.25	10.00	+1.25	RE
35	II	.5	10.00	-1.25	RE

*Demand I: Monopoly Profit = 3.75 and Demand II: Monopoly Profit = 2.50

**L: Learning Period and RE: Random Ending Period (1/10 probability of termination) three deterrence experiments when penalties were possible.

Deterrence Experiments

The final three experiments were pure cartel deterrence experiments in which subjects faced the possibility of penalties in all periods. Subjects' profit from the competitive and cartel trainers were carried forward and served as their assets or wealth going into these cartel deterrence experiments.[27] All the profits earned and penalties paid in the final three experiments were added to or subtracted from these profits.[28] As is clear from Table II, the initial "assets" of the bidding groups as they began the deterrance experiments were not trivial. It was important for this to be the case if we were to be able to face the cartel members with meaningful consequences when the penalty specifications involved low detection probabilities and high sanction levels.

Thirteen different penalty specifications, i.e., distinct combinations of detection and penalty levels, were used in the deterrence experiments. Table III shows these penalty specifications as well as the corresponding expected profit levels. These penalty specifications were chosen so as to provide tests of the effect on collusive markups of: 1) a change in only the expected value of collusion, 2) a change in only the dispersion of returns to collusion, and 3) a change in both the expected value and dispersion of returns to collusion. In terms of changes in detection and

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[27]Subjects' record sheets for the experiment informed them of their previous profits from earlier experiments.

[28]All of the profits from a winning bid were credited to the low bidder. A prorated share of any penalty was subtracted from the accumulated earnings of all bidders in the cartel.

TABLE II

SUMMARY STATISTICS ON EARNINGS

Bidding Group	No. of Sellers	Initial "Asset" Position*	Week 1 Profit	Week 1 Accum. Earnings	Week 2 Profit	Week 2 Accum. Earnings	Week 3 Profit	Week 3 Accum. Earnings	Week 4 Profit	Week 4 Total Earnings
1	5	68.03	18.75	86.78	42.50	129.28	33.60	162.88	32.50	195.38
2	4	62.41	11.24	73.65	46.00	119.65	29.00	148.65	17.50	166.15
3	3	75.86	28.76	104.62	47.63	152.25	53.03	205.28	45.00	250.28
4	3	76.30	24.86	101.16	3.82	104.98	49.63	154.61	52.20	206.81
5	4	71.94	9.50	81.44	64.60	146.04	15.00	161.04	49.50	210.54
6	5	70.90	5.00	75.90	22.00	97.90	59.84	157.74	37.50	195.24

ORDER OF EXPERIMENTAL TREATMENTS FOR GROUPS

Bidding Group	Week 1	Week 2	Week 3	Week 4
1	Trainer	B	A	C
2	Trainer	A	B	C
3	Trainer	A	B	C
4	Trainer	A'	B'	C'
5	Trainer	B'	A'	C'
6	Trainer	A'	B'	C'

*Accumulated earnings from competitive and cartel trainer.

TABLE III

PENALTY SPECIFICATIONS

EXPECTED PROFIT*	PROBABILITY = .1		PROBABILITY = .25		PROBABILITY = .5	
	Penalty	Type**	Penalty	Type**	Penalty	Type**
+2.00	5.00	B	--		--	
+1.25	12.50	B	5.00	A,C	--	
0	25.00	B,C	10.00	A,B,C	5.00	A,B,C
-1.25	--		15.00	B,C	--	
-2.50	50.00	A,C	20.00	A,C	10.00	A,C
-5.00	--		30.00	A,B,C	15.00	B,C
-7.50	--		40.00	A,C	--	

*Winning offer = monopoly price

**Experimental treatment where penalty specification appeared.

sanction levels these three effects correspond to: 1) a change in the detection level holding constant the sanction level, 2) a change in both the detection and sanction levels that exactly offset each other so as to leave expected profit constant, and 3) a change in the sanction level holding constant the detection level.

Penalty Specifications

Several aspects of the penalty specifications in Table III require elaboration. Given that one of the major objectives of these experiments is to test for risk aversion in penalties, it might strike the reader as odd that the specification contains so many negative expected values. There are two reasons for this. First, the earlier experiments by Friedland (1982) as well as our own pilot studies indicated that we might find a willingness to take negative expected values in these experiments. Second, the experiments in this series were designed both for testing the implictions of rational choice theory and for testing for differences in risk attitudes across subject pools. In terms of this second objective, we wanted to have enough range in the experiment to exhaust even the most risk-loving subjects.[29] Nevertheless, for the present purpose this design turns out to give us quite substantial redundancy in negative expected values.[30]

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[29]A report in differences in risk attitudes across subject pools appears in Block and Gerety (1987).
[30]However, when these experiments were run on more risk prone populations, such as prisoners, having this large number of negative expected values turns out to be absolutely necessary.

The reader might also be struck by the concentration of specifications around the fair bet ($E(\pi)=0$), especially since we are interested in risk-averse behavior. Again the rationale for this is previous practice, and here we rely on Friedland et al. (1978), where all of the analysis was performed with fair bets.[31]

Experimental Treatments

The penalty specifications in Table III were used to create three specific types of experimental treatments: A, B, and C. (The details of these specifications are shown in Table IV.) Subjects saw treatments A and B during the first two deterrence experiments with treatment C being used in the final experiment. The three cartel deterrence experiments were designed to run a maximum of forty periods. Treatments A and B had three distinct types of trading periods. The first eight trading periods were designed as a learning segment. In these periods the eight penalty amounts and probability levels were presented in random order. This exposed the subjects to all eight penalty specifications that they would see in the remainder of the experiment. The next twenty-four periods were what we term systematic periods.[32] In these trading periods the eight different penalty specifications were presented three times each. The order of these periods is such as to generate either a dispersion effect, a probability effect or a penalty effect. For example, in treatment B

[31]Why it is that we should expect to find apparently risk-averse individuals engaging in essentially fair bets is an anomaly that we discuss in more detail later in the text.
[32]The purpose of using a learning segment followed by systematic periods was to control for obvious learning behavior and hence give the theory its best chance to predict individual behavior.

TABLE IV

EXPERIMENTAL TREATMENTS

Period	Type A Probability	Type A Penalty	Type B Probability	Type B Penalty	Type C Probability	Type C Penalty
1	.25	20.00	.10	5.00	.10	25.00
2	.50	5.00	.25	15.00	.10	25.00
3	.25	10.00	.10	25.00	.25	10.00
4	.25	40.00	.25	30.00	.25	10.00
5	.10	50.00	.50	5.00	.50	5.00
6	.25	30.00	.10	12.50	.50	5.00
7	.50	10.00	.25	10.00	.25	5.00
8	.25	5.00	.50	15.00	.25	5.00
9	.10	50.00	.10	5.00	.25	15.00
10	.25	20.00	.10	5.00	.25	15.00
11	.50	10.00	.10	5.00	.25	20.00
12	.10	50.00	.10	12.50	.25	20.00
13	.10	50.00	.10	12.50	.25	20.00
14	.25	20.00	.10	12.50	.25	40.00
15	.25	20.00	.10	25.00	.25	40.00
16	.50	10.00	.10	25.00	.25	30.00
17	.50	10.00	.10	25.00	.25	30.00
18	.25	10.00	.25	10.00	.50	15.00
19	.25	10.00	.25	10.00	.50	15.00
20	.50	5.00	.25	10.00	.50	15.00
21	.50	5.00	.50	5.00	.25	15.00
22	.50	5.00	.50	5.00	.25	40.00
23	.25	5.00	.50	5.00	.25	30.00
24	.25	5.00	.50	15.00	.25	10.00
25	.25	5.00	.50	15.00	.50	10.00
26	.25	10.00	.50	15.00	.50	10.00
27	.25	30.00	.25	15.00	.50	10.00
28	.25	40.00	.25	15.00	.50	5.00
29	.25	30.00	.25	15.00	.10	25.00
30	.25	30.00	.25	30.00	.10	25.00
31	.25	40.00	.25	30.00	.10	25.00
32	.25	40.00	.25	30.00	.10	50.00
33	.25	5.00	.50	15.00	.10	50.00
34	.50	5.00	.25	10.00	.10	50.00
35	.25	10.00	.10	12.50	.10	25.00
36	.10	50.00	.50	5.00	.25	20.00
37	.25	40.00	.25	30.00	.50	10.00
38	.50	10.00	.10	25.00	.50	15.00
39	.25	20.00	.25	15.00	.25	40.00
40	.25	30.00	.10	5.00	.50	10.00

Notes: For Types A and B, periods 1 through 8 are learning periods, periods 9 through 32 are systematic, and 33 through 40 are the random ending. Type C has no learning periods with systematic running from periods 1 through 34 and the random ending begins with period 35.

Treatments A', B' and C' had the same penalty specifications as A, B and C except the order they were presented differed. The main difference was the systematic periods were presented in reverse order.

(Table IV), in periods 9-17 we hold constant the detection probability at .1 and vary the penalty from $5.00 to $12.50 to $25.00 in order to test for a penalty effect when the expected payoff is nonnegative. In periods 15-23 we hold the expected value constant at zero and test for the effect of dispersion changes by varying the penalty between $25.00 and $5.00 and the detection probability between .1 and .5. In periods 21-26 we test for a penalty effect at nonpositive expected values by holding the probability at .5 and varying the penalty between $5.00 and $15.00 while in periods 24-29 we hold the penalty constant at $15.00 and vary the detection between .25 and .5 to assess the impact of a pure probability effect when the expected payoff is negative. Finally, if we consider periods 30-32 in conjunction with periods 27-29 we have a test of a pure penalty effect when expected payoffs are negative.

In the systematic segment of these experiments at least one of the following is held constant from one period to the next; probability level, penalty amount or the expected profit from collusion. Any change in the penalty specifications from one period to the next is such that it allows the cartels a point of reference in making its decision in the next period. In the analysis of the data below we always begin with the results of the systematic periods.

The final eight periods in treatments A and B were random-ending periods. Here the eight penalty specifications were again presented in random order. However, before each period the bingo cage was used to determine if the experiment would continue. The experiment had a one-tenth probability of ending prior to each period from the thirty-third through the fortieth periods. Random ending periods were used to control for end of experiment effects where subjects might engage in strategic behavior as they anticipate the end of the experiment.

In experimental treatment C there were only two period types. The first thirty-four periods were systematic periods and the last six were random-ending periods. The subjects were not made aware of any of the design characteristics of any of these experiments. Subjects were told only how many periods were guaranteed before the beginning of the random-ending process.[33]

Learning Behavior

A question that comes up in economic experiments that is not adequately addressed by theory is the issue of how a subject's experience effects behavior. In other words, how do a subject's choices change as he/she becomes more experienced? Running the experiments over five weeks allows subjects to gain a substantial amount of experience with choices that have varying expected values. By comparing the final week with the previous two weeks, we can observe whether or not behavior changes after subjects gain experience with these types of uncertain choices.[34]

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[33]In addition to the specifications shown in Table III we also used variants A', B', and C' that had the same penalty and probability combinations as A, B, and C but the order of the systematic periods was reversed. (See Note in Table IV for a discussion of specifications A', B', and C'.) The rationale for using both A and A' is to test for order effects within an experiment. A second test for order effects involved presenting some groups with specification A the first week, then B the second week while other groups faced specification B in its first week and A in the second week.

[34]Since subjects' profits are not paid until the end, changes in the degree of risk aversion or preference could be a result of wealth effects and not learning. However any reversal in risk attitudes (i.e., risk preferring to risk aversion) could not be traced back to a wealth effect.

EXPERIMENTAL RESULTS

In testing the theory discussed above, two basic questions were considered. First, and most fundamental, was the question: Does the level of collusion decrease as the expected profit from collusion falls? Second, and central to this paper, was the question: Is the response to a decrease in expected profit larger if the decrease was generated by an increase in the size of the penalty than if it was generated by an increase in the detection rate? In other words, is collusion more responsive to changes in penalty levels than detection levels? Since an affirmative answer to this question is implied by risk aversion, we also inquire in this paper as to the other direct implication of risk aversion in this environment.[35] Specifically, we investigate whether collusive activity occurs basically only when the expected profit from collusion is positive.

Competition vs. Collusion

Although the main focus of this paper is to test the hypotheses related to deterrence, the comparison of the results from the four competitive trainers and the penalty-free periods of the cartel trainer is quite enlightening. The actual auction process was identical in the competitive and cartel trainers. However as we discussed above, sellers in the cartel trainer were allowed to communicate with each other prior to making their offers to sell.

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[35]While we did not try to actually measure the degree of risk aversion exhibited by our bidders, we can compare the behavior of bidding groups to determine if the revealed level of risk aversion is the same across the groups.

In Figure 1 we display the winning offers in each period for the four bidding groups in the competitive trainer. As is immediately apparent from the data in Figure 1, the sealed offer auction where sellers' costs are identical and communication is prohibited provides very strong incentives for the winning offer to rapidly converge to the minimum seller's cost. In three of the four experiments, the winning offer was only one cent above sellers' cost by the fourth period and remained there for the remainder of the experiment.[36] In Group 3 the winning offer never fell as low as in the other groups; the lowest winning offer did not get below $3.05 or five cents above minimum cost, even by the tenth period.[37] While the size of this group alone may have led to this result, the effect of the small number of bidders was likely aggravated by a remark made by one of the bidders during the experiment. Although seller communication was not permitted in this experiment, one seller after the third period commented that if bidding continued in this manner the winning offer would soon equal

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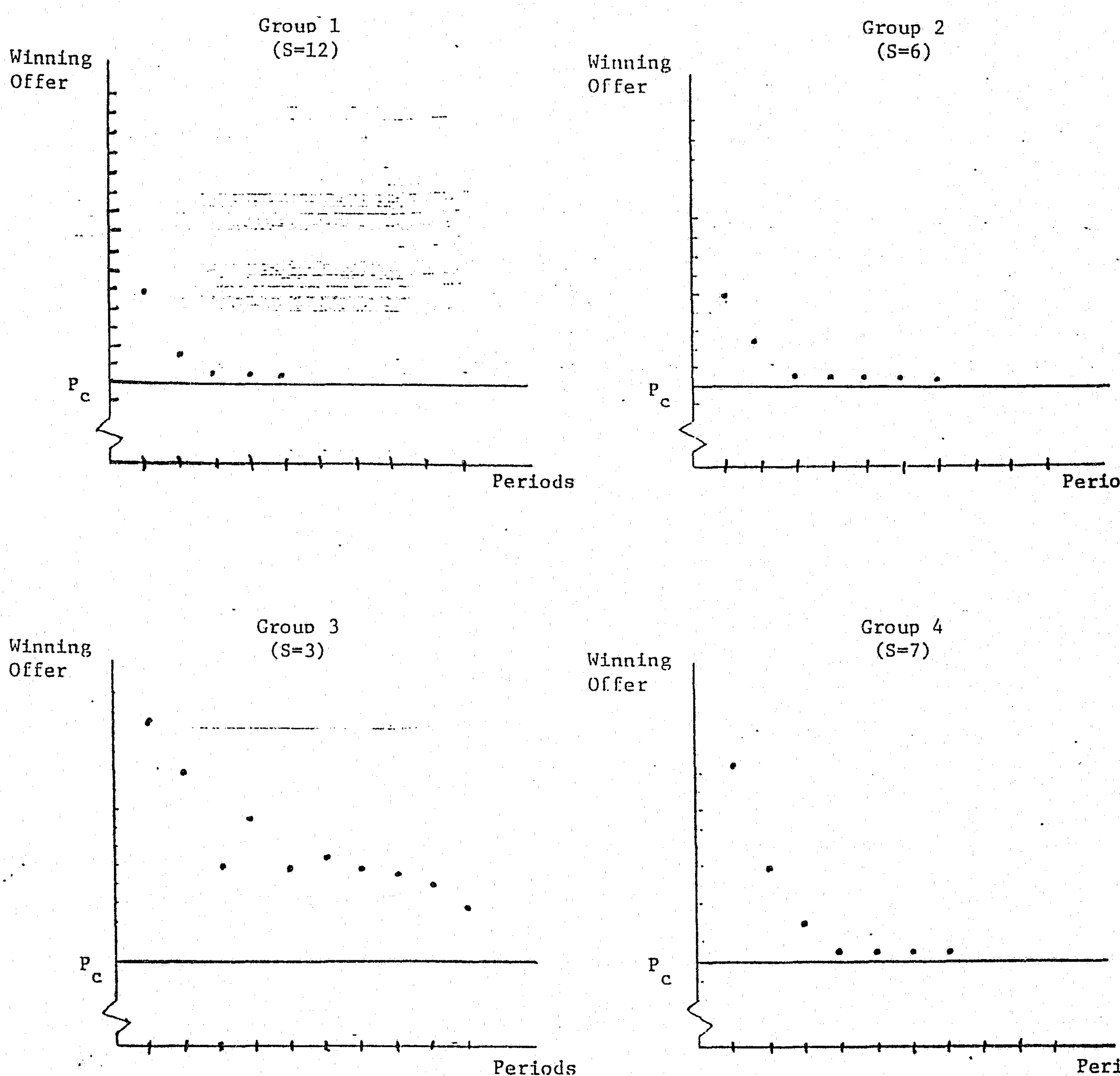
[36]The experiment was not allowed to run for an extended period once the winning seller's profit margin equaled one cent. It was clear from earlier competitive sealed offer experiments that this particular market mechanism led to highly 'competitive' outcomes and continuing the experiments once minimum costs have been achieved for several periods simply increases the subjects' frustration over low profits and reduces their willingness to remain in the experiments (see Block and Gerety, 1985).

[37]It is interesting to note that there appears to be a rough correlation between the number of bidders in the group(s) and the speed of convergence of the winning offer to the competitive price.

[38]A similar comment was made by one of the bidders in Group 1. However, in Group 11 there were 7 bidders and the remark seems not to have made any difference.

FIGURE 1

WINNING OFFERS FROM COMPETITIVE TRAINTERS



P_c = Production Cost

$3.01.[38] The winning offer immediately jumped five cents. However, the winning offer monotonically decreased from the sixth period on, reaching its lowest level of $3.05 by the final period.

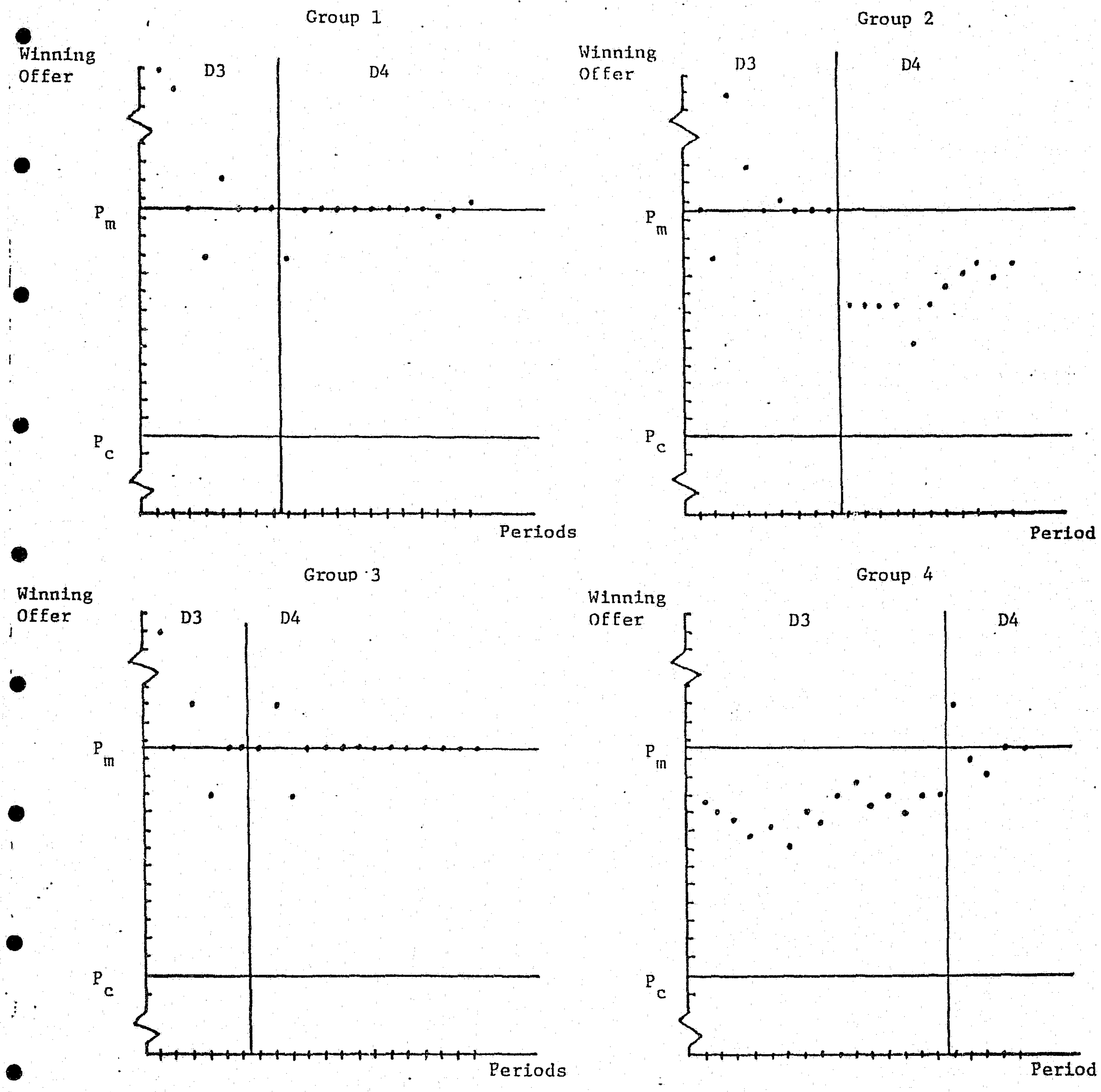
In Figures 2 and 3 we display the winning offers of the experiments where these same sellers were allowed to communicate prior to submitting their offers and there was no 'antitrust enforcement.' These were the so-called cartel trainers. Also, indicated in Figure 2 by a vertical line, is the point at which we changed from Demand I to Demand II. For all groups and for both demands the winning offers in these experiments were significantly above cost and in ten out of twelve bidding environments (six groups and two demand levels), the winning offer equaled the monopoly price within five or six periods.[39] The striking difference between the results of the competitive and cartel experiments provide dramatic evidence on the strength of the incentive to collude in sealed-offer markets.

Antitrust enforcement aside, the most serious threat to cartel stability is the inability in many circumstances of cartel members to detect secret price cutting. The problem is, of course, mitigated in a sealed-offer auction since the lowest offer is the only relevant price in the market. If the winning seller and lowest offer is truthfully reported, then the sealed-offer auction is likely, as Stigler (1964) noted,

[39]For the two cases where the winning offer was not equal to the monopoly price, it is clear from Figure 2 that the winning offer was significantly above minimum production cost. Moreover the demand functions in this experiment involve discrete steps which cause them to be local maxima and in one case the winning offer converged to a local maximum. In the other case the final winning offer was within one cent of a local maximum.

FIGURE 2

WINNING OFFERS FROM CARTEL TRAINERS



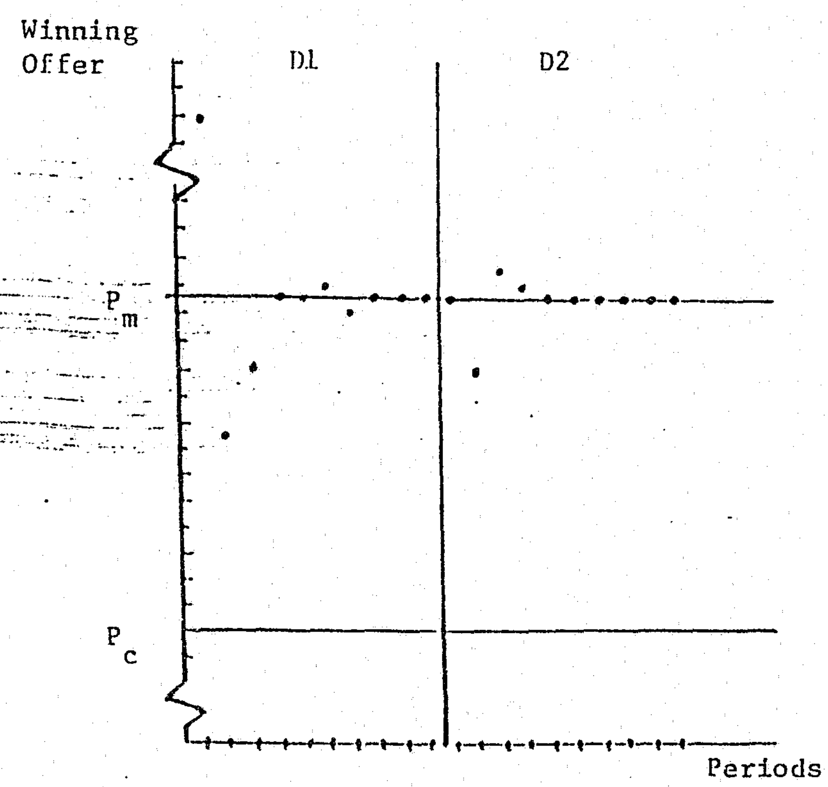
P_c = Production Cost

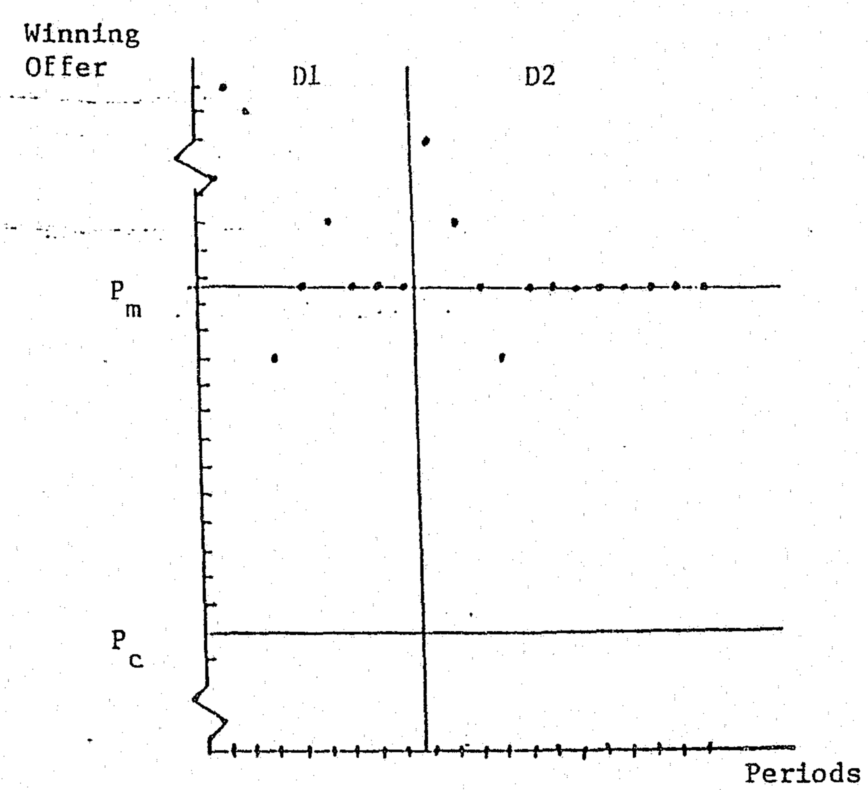
P_m = Monopoly Price

FIGURE 3

WINNING OFFERS FROM CARTEL TRAINERS

Group 5





Group 6

P_c = Production Cost

P_m = Monopoly Price

to be extremely conducive to stable collusive agreements.[40] We chose this institutional form precisely for that reason. We wanted an auction setting that would, in the absence of antitrust enforcement, yield a stable cartel. After all, our purpose in this research was not to study cartel stability but rather primarily to test the efficacy of various penalty structures in controlling cartel behavior.

Deterrence Results

The results from the three cartel deterrence experiments are summarized in Table V and Table VI. Column 1 in both tables gives the expected profit corresponding to each penalty specification in that row. In columns 2, 3 and 4 we report as the uppermost entry the percentage of times the winning offer was greater than the minimum production cost of all sellers for each penalty specification and in column 5 we report the overall proportion of low bids greater than cost for that level of expected profit. Also reported for each penalty specification is the total number of times that sellers saw that specific penalty specification during the systematic part of the experiment. For example, according to the data in column 5 of Table V, during the 156 systematic periods where the expected profit to bid rigging was zero, sellers risked the penalty and

[40]In the absence of side payments, it appears the most difficult problem faced by a bidding cartel in sealed-bid auctions is simply the frequency of auctions. Since the sealed-offer auction is a winner-takes-all environment, effective collusion requires either a large number of auctions or explicit side payments. Since side payments probably dramatically increase both the probability that a cartel is detected and the penalty imposed when they are detected, cartels are likely to pose the most problems for antitrust authorities in markets where there are frequent auctions, especially if the auctions are of approximately the same value. Obviously, our experimental design captures the elements of the type of auctions that is most problematic to enforcement authorities.

TABLE V

CARTEL DETERRENCE EXPERIMENTS

All Groups, Systematic Periods

Percentage of winning offers exceeding winning seller's cost.

(Number of Total Observations)

Expected** Profit	Probability of Detection* 0.1	0.25	0.5	Total
+2.00	100% (18) Week 2 3 4 6/6 12/12 -			100% (18)
+1.25	100% (18) Week 2 3 4 6/6 12/12 -	97% (30) Week 2 3 4 12/12 6/6 11/12		98% (48)
0	10% (48) Week 2 3 4 0/6 0/12 5/30	54% (56) Week 2 3 4 14/18 5/18 10/18	70% (54) Week 2 3 4 13/18 14/18 11/18	46% (156)
-1.25		0% (36) Week 2 3 4 0/6 0/12 0/18		0% (36)
-2.50	11% (36) Week 2 3 4 4/12 0/6 0/8	0% (36) Week 2 3 4 0/12 0/6 0/18	3% (36) Week 2 3 4 0/12 0/6 2/18	5% (108)
-5.00		0% (52) Week 2 3 4 0/16 0/18 0/18	0% (36) Week 2 3 4 0/6 0/12 0/18	0% (88)
-7.50		0% (36) Week 2 3 4 0/12 0/6 0/18		0% (36)

set prices above costs in 72 cases or equivalently in 46% of the relevant periods. Of course, as the data in Table V makes clear, this willingness to risk the penalty at this expected value varied substantially by penalty specification. When the chance of detection was 1 in 10 and the penalty was $25.00, sellers risked the penalty only 10% of the time. When the chance of detection was 1 in 2 and the penalty $5.00, sellers risked the penalty 70% of the time. Finally in the last row of the entry for each penalty specification is a breakdown of sellers' behavior on a week by week basis. Table VI is identical to Table V except all period types (systematic, learning and random-ending) are considered.[41]

One characteristic of the results reported in Table V and VI is obvious. Sellers were extremely responsive to expected profits. Almost every time the expected profit was greater than zero, sellers raised the price above cost risking the penalty and conversely almost never when the expected profit was negative did the cartel risk a penalty by raising the price above cost. Of the 95 periods in our experiments when the expected profit from collusion was positive, in only 3% of these cases were our sellers deterred from raising price above costs and risking a penalty. However when the expected profit from collusion was negative, as it was in 364 periods, our sellers failed to be deterred from setting prices above costs in only 2% of these cases. Given this overwhelming responsiveness of sellers in these experiments to expected returns, it is hardly surprising that their behavior conformed to the more specific implications

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[41]Logit analysis was used to test the restriction that no statistical difference existed between the systematic period versus the learning and random-ending periods. The results from this statistical test indicated there is no significant difference between the systematic periods versus the learning and random-ending periods.

TABLE VI

CARTEL DETERRENCE EXPERIMENTS

All Groups, All Periods

Percentage of winning offers exceeding winning seller's cost.

(Number of Total Observations)

Probability of Detection

Expected* Profit	0.1	0.25	0.5	Total
+2.00	100% (26) Week 2: 9/9, 3: 17/17, 4: –			100% (26)
+1.25	93% (27) Week 2: 9/9, 3: 16/18, 4: –	98% (41) Week 2: 19/19, 3: 10/10, 4: 11/12		96% (68)
0	12% (60) Week 2: 0/9, 3: 2/17, 4: 5/34	50% (72) Week 2: 18/26, 3: 8/28, 4: 10/18	72% (71) Week 2: 20/26, 3: 20/27, 4: 11/18	46% (203)
-1.25		0% (44) Week 2: 0/9, 3: 0/17, 4: 0/18		0% (44)
-2.50	14% (44) Week 2: 6/17, 3: 0/9, 4: 0/18	0% (49) Week 2: 0/16, 3: 0/9, 4: 0/24	4% (56) Week 2: 0/17, 3: 0/10, 4: 2/29	5% (149)
-5.00		0% (67) Week 2: 0/23, 3: 0/26, 4: 0/18	0% (53) Week 2: 0/9, 3: 0/20, 4: 0/24	0% (120)
-7.50		0% (50) Week 2: 0/17, 3: 0/9, 4: 0/24		0% (50)

of the deterrence hypothesis discussed below.

The experiment included three sets of penalty specifications where the penalty or sanction level was the same ($5.00, $10,00 or $15.00) and the only variation in the specification was the variation in the detection level. These sets were designed to test the proposition that collusion would unambiguously decline in response to an increase in the detection rate. While the results can be inferred from the data in Table VI, a more formal analysis is presented in Table VII where in columns 1 and 2 we report the results of logit regressions for penalty levels of $5.00 and $10.00.[42] The dependent variable in these regressions is DCOL which is equal to one if the sellers set their price above their minimum cost and zero otherwise. The independent variable, Probability is the detection rate. In column 3 we report similar logit regressions except in this case we formally recognize the limited number of detection rates. In these regressions the variable PROB was replaced with a dummy variable DH where

DH = 1 if PROB = .5 and DH = 0 otherwise

Obviously, the deterrence hypothesis implies that this dummy variable is negative. As we would expect, the results in columns 1-4 indicate that our sellers' behavior with respect to changes in detection rates conforms perfectly to the predictions of theory.

In addition to the three sets of penalty specifications where only the detection probability varied the experiment also included three sets of specifications where only the sanction levels varied. These three sets are represented by the columns 2, 3, and 4 in Tables V and VI. Moving down

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[42]Penalty level $15.00 is not included because in neither of the specifications were there any nonzero values for DCOL. Of course, this is not unexpected because both cases involve negative expected values.

TABLE VII

LOGIT ESTIMATES OF DETECTION EFFECTS

	(1)	(2)	(3)	(4)
Dependent Variables	DCOL	DCOL	DCOL	DCOL
Penalty Level	\$5	\$10	\$5	\$10
Independent Variables:				
Constant	6.44 (2.042)	3.41 (0.86)	3.69 (0.012)	0.056 (0.24)
Probability	-11.01 (4.18)	-13.41 (3.03)		
DH			-2.75 (1.046)	-3.35 (0.76)
Number of Observations	112	128	112	128
Log Likelihood	-46.91	-58.51	-46.91	-58.51

each of these columns involves a change in only the sanction level. Again as is apparent from the raw data itself (Tables V and VI), the results of the experiment conform almost perfectly to the predictions from theory. Only in one isolated case do sellers appear not to decrease their willingness to set prices above cost in response to an increase in a sanction level. These results are formalized in Table VIII where estimated coefficients from logit regressions appear for detection levels .1, .25, and .5 in columns 1-5.[43] The dependent variable in these regressions is once again DCOL and the independent variable is Penalty which represents the penalty level. In columns 4 and 5 of this same table we present the results of logit regressions for the pooled sample in which both penalties and detection levels were allowed to vary.[44] DQ in these regressions is a dummy variable that equals 1 if probability was greater than or equal to one-quarter and equaled zero if the probability equaled one-tenth. The variable D10 equals 1 if the penalty amount was $10.00 and

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[43]The data used was limited for the regression where the probability of detection equaled 0.25. As can be seen in Table VI the groups were completely deterred when the probability of detection equaled 0.25 and expected profit from collusion was negative. Therefore, the last three penalty specifications, with a 0.25 detection level and sanction levels of $20.00, $30.00 and $40.00 were eliminated from the data sample. Although the construction of the data sample was rather arbitrary, the purpose of the logit analysis is to test the relative responsiveness of collusion to changes in the penalty specification. There did not exist any variablity in the groups' behavior when the probability level was 0.25 and the expected profit was negative. However, in order to capture the relative responsiveness of collusion when the penalty increased from $10.00 to $15.00 penalty specification with a 0.25 probability and a $15.00 fine was included in the data sample.

[44]The data sample for the regressions in columns 4 and 5 included all the observations used for the logit regressions reported in columns 1-5 in Table VIII.

TABLE VIII

LOGIT ESTIMATES OF PENALTY AND DETECTION EFFECTS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Variable	DCOL	DCOL	DCOL	DCOL	DCOL	DCOL	DCOL
Detection Level	.1	.25	.25	.5	.5	All	All
Independent Variables:							
Constant	1.68 (0.56)	7.32 (2.039)	3.69 (1.012)	5.17 (0.89)	0.94 (0.26)	3.79 (0.55)	3.84 (0.78)
Penalty	-0.096 (0.022)	-0.73 (0.21)		-0.85 (0.15)		-0.17 (0.024)	-0.19 (0.034)
Probability						-5.91 (1.017)	
DQ							-1.45 (0.55)
DH							-1.35 (0.28)
D10			-3.63 (1.04)		-4.23 (0.77)		
Number of Observations	131	113	113	127	127	371	371
Log Likelihood	-63.42	-54.58	-54.58	-50.84	-50.84	-200.93	-200.93

zero otherwise. Once again the conformity of these results with theory is immediately apparent. Increases in sanction levels unambiguously tended to suppress collusive price setting in these experiments.

The fact that sellers in these experiments set price above costs in virtually all of the cases when doing so has a positive expected profit and in virtually none of these cases where the expected payoff is negative strongly suggests risk neutrality on the part of our sellers. This inference is reinforced by the nearly 50/50 overall split of sellers in setting price above costs when the expected value of such activity is zero. Judging from the last column of Table V and VI it would appear that sellers are indifferent between collusion and competitive pricing when the expected profit from collusion was zero. However a closer examination of the results in Tables V and VI reveal some interesting contradictions. As we noted in a previous section of this paper, risk neutral sellers should not, holding the level of expected profit constant, be more deterred by one penalty specification than by another. Nevertheless at expected profit equal to zero it is clear from the results in Tables V and VI that a penalty specification with a low level of detection and a high penalty was a stronger deterrent than high detection and low penalty. In terms of these reactions to penalty specifications our sellers appear to be, in fact, risk averse. A similar penalty effect appears to be also present at expected profit $1.25 when you consider all periods (Table VI).

In Tables IX and X we report on a more formal consideration of these penalty specification effects. Table IX displays the results of logit estimates of penalty specification effects at zero expected profits. The regressions were conducted on systematic periods only as well as all periods types. The dependent variable is DCOL. The independent variables

TABLE IX

LOGIT ESTIMATES OF DISPERSION, WEALTH AND EXPERIENCE

(Zero Expected Profit Level)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dependent Variable	DCOL	DCOL	DCOL	DCOL	DCOL	DCOL	DCOL	DCOL	DCOL	DCOL	DCOL	DCOL
Sample	SYS	ALL	SYS	ALL	SYS	ALL	SYS	ALL	SYS	ALL	SYS	ALL
Independent Variables:												
Constant	1.64	1.60	1.58	1.69	1.15	1.13	0.86	0.94	0.80	1.04	0.37	0.47
	(0.34)	(0.30)	(0.77)	(0.65)	(0.95)	(0.82)	(0.30)	(0.26)	(0.78)	(0.66)	(0.95)	(0.82)
Penalty	-0.15	-0.15	-0.15	-0.15	-0.15	-0.15						
	(0.027)	(0.024)	(0.028)	(0.025)	(0.029)	(0.025)						
Profit			0.00040	-0.00063	0.0093	0.0092			0.00043	-0.00077	0.0093	0.0092
			(0.0050)	(0.0044)	(0.0080)	(0.0071)			(0.0050)	(0.0044)	(0.0080)	(0.0071)
Week 3					-1.32	-1.23					-1.32	-1.24
					(0.57)	(0.48)					(0.57)	(0.48)
Week 4					0.22	0.052					0.22	0.039
					(0.53)	(0.49)					(0.53)	(0.49)
DG10							-0.72	-0.88	-0.72	-0.88	-0.75	-0.92
							(0.40)	(0.35)	(0.40)	(0.35)	(0.41)	(0.36)
D25							-2.30	-2.08	-2.31	-2.06	-2.32	-2.08
							(0.55)	(0.47)	(0.55)	(0.47)	(0.57)	(0.49)
Number of Observations	156	203	156	203	156	203	156	203	156	203	156	203
Log Likelihood	-86.14	-113.80	-86.14	-113.79	-83.38	-110.40	-86.14	-113.71	-86.13	-113.69	-83.37	-110.28

TABLE X

TEST FOR DISPERSION EFFECTS

(Expected Profit = $1.25)

	(1)	(2)	(3)	(4)
Dependent Variable	DCOL	DCOL	DCOL	DCOL
Sample	All	All	All	All
Independent Variables:				
Constant	4.64 (1.76)	3.69 (1.012)	4.18 (3.41)	3.41 (3.00)
Penalty	-0.16 (0.17)		-0.15 (0.17)	
Profit			0.0020 (0.21)	0.002 (0.021)
D12.50		-1.16 (1.25)		-1.15 (1.26)
Number of Observations	68	68	68	68
Log Likelihood	-11.83	-11.83	-11.83	-11.83

TABLE XI

PROPORTION OF LOW BIDS ABOVE MINIMUM COST

By Bidding Group, All Period Types

(Expected Profit = Zero)

Fine \ Group	1	2	3	4	5	6
$25	0	.20	.10	.11	.30	0
$10	.08	.31	.55	.58	.83	.67
$5	.75	.08	1.00	.58	1.00	.92
Total	.29	.20	.56	.45	.74	.54

are: 1) PENALTY and D25 (defined above); 2) PROFIT, which is total profits earned by the bidding group up to the current market period; 3) WEEK 3 and WEEK 4 which are dummy variables for the week of the experiment (WEEK 3 is equal to 1 if the bid was made during the third or fourth week; and WEEK 4 is equal to 1 only if the bid was made during the fourth week); and 4) DG10 which is a dummy variable that is equal to 1 if the penalty is greater than or equal to $10.00. Because of the way DG10 is defined, D25 measures the marginal penalty effect between $10.00 and $25.00. Hence risk neutrality requires both DG10 and D25 to be zero while risk aversion implies both are negative.[45] Likewise, because of the way the variables WEEK 3 and WEEK 4 are defined, WEEK 4 measures the marginal effect of the bid being submitted in the last week of the experiment. Hence if there is learning going on in these experiments then both WEEK 3 and WEEK 4 should have the same sign.

In Table X we display the results of a similar set of logit regressions for the cases where expected profit is +$1.25. Here D12.50 is a dummy variable that is equal to 1 when the penalty is $12.50.

As we would expect, the results of these logit regressions confirm our casual observations from Table V and VI. All things being equal, for nonnegative expected values, the larger the penalty the larger the deterrent effect. While this independent penalty effect is statistically significant only when the expected value is zero, it was, as indicated by the results in Table X, also present when the expected value from

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[45]A chi-square statistic was calculated for the regression reported in column 8 which tested the restriction that the coefficients for DG10 and D25 are zero. The value of the chi-statistic was equal to 37.89 which is clearly significant at the 99 percent level of confidence.

collusion was positive. When the expected value of collusive price fixing was nonnegative, the sellers appeared to be more responsive to changes in penalties than detection rates.

It is clear that the evidence from our experiments on the relative responsiveness of sellers to changes in penalties and detection rates is inconsistent with risk neutrality.[46] Our sellers were not equally responsive to changes in penalty levels and detection levels as they should have been had they been acting in a risk neutral manner. Moreover, in the case of most practical concern, i.e., where expected profits are nonnegative, sellers are more responsive to changes in the penalty level than the detection level. Considering the behavior of individual bidding groups tends to reinforce this conclusion. As the data in Table XI and the results of the logit regressions on individual group data in Table XII reveal, in all but one case (Group 2), sellers appear to be more responsive

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[46]Even in the rare cases when sellers take the risk of a penalty by setting price above cost when the expected value is negative, it appears as if they are not indifferent with respect to penalty specification. The data in Tables V and VI indicates that the sellers are less responsive to changes in penalties than detection rates when expected values are negative. This, of course, is what one would expect. Since taking negative expected values is consistent only with risk preference and since risk takers would be more responsive to changes in detection rates than sanction level, our findings in the case when expected profit equals -$2.50 are consistent with theory. Unfortunately the individual group level data does not support this interpretation. While 4 out of 6 groups took some negative expected values, no group set the winning offer above cost for more than one penalty specification involving a negative expectation. Of the 4 groups that took options involving negative expected values, 2 groups took specifications involving large penalties and low detection rates (1/10, $50.00) and 2 took specifications involving high detection rates and low penalties (1/2, $10.00).

[47]Appendix B reports the results from the six groups. Tables C1 through C6 break down the results in Table VI into the individual groups.

TABLE XII

LOGIT ESTIMATES OF THE DISPERSION EFFECT FOR INDIVIDUAL BIDDING GROUPS

(Zero Expected Profit Level)

	Group 1		Group 2		Group 3		Group 4		Group 5		Group 6	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dependent Variable	DCOL	DCOL	DCOL	DCOL	DCOL	DCOL	DCOL	DCOL	DCOL	DCOL	DCOL	DCOL
Independent Variables:												
Constant	4.60 (1.69)	1.099 (0.67)	-1.62 (0.79)	-1.39 (0.79)	1.77 (1.23)	0.18 (0.61)	1.21 (0.70)	0.34 (0.59)	4.56 (1.80)	2.40 (1.044)	4.10 (2.18)	2.40 (1.044)
Penalty	-0.70 (0.25)		0.018 (0.051)		-0.16 (0.081)		-0.12 (0.057)		-0.22 (0.83)		-0.34 (0.24)	
D10		-3.50 (1.24)		0.57 (0.99)				0.000 (0.83)				-1.70 (1.21)
D25				-1.59 (1.20)		-2.38 (1.22)		-2.42 (1.21)		-3.25 (1.25)		
Number of Observations	24	24	35	35	21	21	33	33	22	22	24	24
Log Likelihood	-10.19	-10.19	-17.45	-16.47	-10.83	-10.83	-19.74	-19.44	-9.55	-9.55	-11.08	-11.08

to increases in penalties than increases in detection rates.47

In considering both the individual bidding group data and the aggreagate data, it is our results for penalty specifications involving zero expected profits that are most interesting. After all, it is only here that most of the solutions are internal; i.e., don't involve either complete deterrence or complete lack of deterrence. It is also true that the quantitative implications of our results are quite dramatic. For example, they imply that increases in penalties are about six times as powerful in reducing collusive behavior as are increases in detection rates. However, these magnitudes, as well as the predominance of corner solutions for penalty specifications other than those where the expected value is zero, may be a result of the laboratory environment and may not be directly applicable to choices involving actual market behavior.

Risk Aversion or Risk Neutrality

Our results on the relative responsiveness of sellers to changes in penalty levels and detection levels are basically consistent with risk aversion. That is, our results suggest that increase in penalty levels are more powerful in deterring collusion than are increases in detection levels. However these same experimental results evidence a major inconsistency with risk aversion. Specifically, sellers in these experiments took on average almost 50% of the risky options presented when the expected payoff from these options was zero. They took almost all (98% to be precise) risky options when the expected profit was positive. While the fact that almost all of the risky options involving positive expected profit were chosen might be due to the experimental design, ($1.25 might have been too far from 0 to induce partial deterrence) it is not so easy to dispose of the prevalence of risky choices at zero expected profit levels.

If we look at the data on individual bidding groups we find that the proportion of risky choices at a fair bet (E(!)=0) ranges from 20% to 75%. Interestingly enough the bidding group (Group 2) whose relative responsiveness to penalties and detection rates is least consistent with risk aversion has the lowest proportion of risky choices at E(!)=0. Overall the group data does not reveal any systematic relationship between the proportion of risky actions chosen at E(!)=0 by a specific group and the strength of its aversion to risk as measured by its relative responsiveness to changes in penalties and detection rates.

There really is no immediate theoretical reconciliation, at least in terms of the expected utility hypothesis, of the simultaneous implications of risk neutrality and risk aversion that we find in our results. It might be that subjects have difficulty calculating around the zero level and hence don't actually realize its a fair bet.[48] The uniformity of sellers' responses around the zero level in both a positive and negative direction must give us some pause in accepting any explanation that relies on error in calculations. Sellers seem to do just fine in figuring out when expected payoffs are positive and negative and it does seem strange that

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[48]There exist numerous examples from the economic and psychological literature which indicate individual's behavior is quite often inconsistent with expected utility thoery (see Kahneman and Tversky, 1979 and Marchina, 1983, as well as references therein). Several alternative theories have been proposed to replace the independence axiom of expected utility theory. In general, these theories imply an individual's expected utility function is no longer linear in probability. As was noted in footnote 17 above, it is possible that individuals may act risk-taking in losses if pricing at cost is perceived by subjects as having to be $2.50 in 'insurance' to avoid a large but uncertain loss.

their powers of calculation suddenly leave them when the expectation of the risky venture is zero.

Another and perhaps equally troublesome explanation of our results might lie in the nature of this experiment and to some extent the experimental process itself. If subject boredom is a problem then we might find subjects setting price above costs more often than they would if they were only concerned with the monetary consequences of the experiment. Just sitting there and setting the price equal to costs seems to be quite boring for subjects. By setting the price above costs the subjects at least get to observe a roll of the bingo cage. If setting the price above costs makes participation in the experiment more interesting, we may be observing some consumption of recreation by subjects in this experiment. Such consumption would tend to increase sellers' participation at all expected values including zero. Nevertheless, for risk averse sellers, as long as the recreation of setting prices above cost involved only the process and not the outcome, the frequency of collusive price setting would at any expected profit level decline with the dispersion of the collusion option. Hence at any expected profit level including zero, we would expect the consumption of such recreation to decline with the size of the penalty. If recreational price fixing is a problem in these experiments its effect would be to shift the observed penalty effect toward the zero expected profit level. While in the absence of a boredom effect we might expect to see a strong penalty effect at a positive expected value, if boredom is a problem this penalty effect is likely to be attenuated. Instead of a strong penalty effect at positive expected value we are likely to observe as we do above, a strong penalty effect at expected value zero; where but for boredom risk averse subjects would fear to tread. If our subjects are using the collusive price setting options to consume recreation then our results are consistent with risk aversion. The subjects do appear to reduce their consumption of this "commodity" as its costs in terms of risk and/or expected value increases. One problem with this 'consumption' of recreation explanation of our results is that while it is not inconsistent with the 'knife edge' cessation of collusive price setting at negative expected values we observe in this experiment, it does not imply this behavior.49

Wealth Effect

Since setting prices above costs is risky activity, at least when sellers are subject to antitrust penalties, we would expect the willingness of sellers to engage in the activity to be sensitive to their wealth or asset position. As we noted above, if managers are risk averse and if their preferences are characterized by declining absolute risk aversion, then we would expect collusive price fixing to become more attractive as the manager's wealth position increases.

In this experiment we investigate wealth effects by using the natural variation in profit levels over the course of the experiment. Since we did not actually pay out profits until the experiment was over, the accumulated profits at any point in time is a reasonable measure of the

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49There does exist some impirical evidence that individuals tend to take more risks if they participate in a group discussion prior to making a decision (see Matital (1982)). This so-called 'risky-shift' phenomenon appears to be robust across groups of different age, occupation and nationality. Why 'risky-shift' in our experiment should affect only the groups' willingness to take fair gambles and not apparently its willingness to absorb risk in terms of penalty structures seems puzzling at best. We do present evidence below that indicates that group size influences the level of risk a group of individuals is willing to take.

individual seller's, and hence, the cartel's, 'experimental wealth.' Moreover, because the earnings from the experiment represent a significant proportion of the subject's total earnings over the duration of the experiment, variations in their 'experimental wealth' are likely to represent nontrivial variation in their overall wealth position.

The PROFIT variable in Table IX is the accumulated profits of the bidding group (cartel) at the beginning of each trading period. As the results in these tables indicate, this is not a particularly well-behaved variable. In Table IX the PROFIT variable alternates in sign depending on exactly what other controls are in the regression. Even in what might be considered the most complete specifications (columns 6 and 12) while the coefficient is positive, it is insignificant.

There is, of course, an argument that the relevant income variable in this situation is the individual level income variable and not groups' earnings. That is, when it comes to income on wealth levels, it's the individual cartel member's income or wealth level that's important and not the aggregate income level. Whatever the theoretical merits of including per capita earnings (PC PROFIT) instead of total earnings in the regression, the empirical evidence is clear. As the results in columns 1-4 of Table XIII indicate, both per capita income and total income are negatively related to the willingness of a cartel to risk penalties.[50] Moreover neither measure of income is statistically signficant at conventional levels of significance.

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[50]The sample for this regression included only the data were the groups were partially deterred, i.e. the data when the proportion of colluision for a given penalty specification did not equal zero percent of one hundred percent. A more complete data sample had no significant influence on the regressions results.

TABLE XIII

LOGIT ESTIMATES OF PER CAPITA WEALTH, LUCK AND LEARNING EFFECTS

EXPECTED PROFIT	(1) All (Restricted)*	(2) Zero	(3) All (Restricted)	(4) Zero	(5) All (Restricted)	(6) Zero
COEFFICIENTS						
Constant	5.00	3.03	4.62	2.75	4.98	3.13
	(1.17)	(1.43)	(0.80)	(0.82)	(0.83)	(0.86)
Penalty	-0.16	-0.15	-0.16	-0.15	-0.16	-0.15
	(0.023)	(0.026)	(0.024)	(0.026)	(0.024)	(0.026)
Probability	-5.84		-5.81		-5.88	
	(1.05)		(1.06)		(1.06)	
Profit	-0.0059	-0.0074				
	(0.0092)	(0.012)				
PC Profit			-0.0090	-0.018	-0.0096	-0.017
			(.017)	(0.023)	(0.017)	(0.023)
OUTERN					-0.0010	-0.0012
					(0.0005)	(0.0007)
Week 3	-0.43	-0.46	-0.57	-0.54	-0.62	-0.62
	(0.51)	(0.67)	(0.38)	(0.51)	(0.39)	(0.52)
Week 4	-0.22	0.79	-0.16	0.70	-0.16	0.70
	(0.50)	(0.66)	(0.37)	(0.51)	(0.37)	(0.51)
Luck	0.020	0.026	0.019	0.028	0.017	0.025
	(0.011)	(0.016)	(0.011)	(0.015)	(0.012)	(0.016)
Log Likelihood	-191.59	-109.02	-191.65	-108.91	-189.94	-107.42
Number of Observations	371	203	371	203	371	203

*Includes only the penalty specifications where the subjects were partially deterred (i.e., all penalty specifications where $1.25 _ Expected Profit _ -$2.50, except for penalty specifications (.25, 15.00, -1.25) and (.25, 20.00, -2.50)). It excludes all data from penalty specifications where the subjects were completely deterred (proportion of collusion was zero percent) and penalty specifications where the subjects were completely undeterred (proportion of collusion equaled one hundred percent).

Realizations vs. Anticipations: Influence of Luck

There is some evidence from previous experimental studies that realizations have an independent influence on decisions that could not be predicted by theory. For example, in an experimental study of risk attitudes among rural farmers in India, Binswanger (1980) found that how 'lucky' an individual was in the experiment appeared to be an important factor in determining subsequent choices. Individuals receiving the best payoff in the previous period were most likely to take more risks in the current period.51 While the wealth variable in our experiment captures some aspects of what might be called luck, it reflects both outcomes and decisions. In order to measure luck in a more direct fashion we constructed a variable that simply represented the relationship between realized income and anticipated income. The latter is measured by the expected income corresponding to the individual's past decisions. Specifically we constructed the luck variable as follows:

$$Luck_t = \sum_{i=1}^{T-1} (!_i - (EP_i \quad DCOL_i))$$

As is apparent from Table XIII, the effect of luck is not very sensitive to the precise specification and it is nearly always statistically significant. All things being equal, the willingness to collude in any period is positively related to how lucky the bidding group had been in the past; i.e., the higher actual profits were relative to anticipated profits,

51Binswanger (1980) regressed various personal characteristics against an experimental measure of risk aversion. His measuring of luck was calculated by summing the past values of a variable X_i, where X_i equaled 1 if in period i the indivdual received the best payoff , equaled -1 if the indivdual received the worst payoff and equaled zero if the individual chose the certain payoff.

the more likely the cartel was to choose the collusive or risky options. Luck appears to be a consistent predictor of risk-taking behavior. Moreover, when luck is included in the equation, to the extent that profit has an effect on the willingness to take risk, it appears to dampen the cartel's willingness to engage in a risk-taking venture such as collusion.

Bidding Group Characteristics

Up to this point our analysis has been concerned exclusively with the effects of experimental structure and earnings on the decision to collude, or more specifically on the decision to risk prices above minimum costs. Of course it is basically for these structureal factors and experimental earnings that economic theory has clear implications. It is, however, of some interest to inquire as to the effects of the bidding group characteristics themselves on the decision to risk penalties by setting prices above minimum costs. In Table XIV, and to some extent in Table XIII, we present a series of logit regressions that include controls for various bidding group characteristics. These controls include: number of bidding group members, outside earnings of members, sex compositon of group and risk attitudes of members.[52] The controls for outside earnings Outside Earnings is the average outside monthly earnings that the subjects

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52This data was obtained from an organizational meeting held prior to the experiments. In this meeting individuals were given a 'screener' which was used to obtain information on individuals' age, sex, years in school and other outside sources of income. The measure of risk aversion was based on eight hypothetical questions that appeared in the screener which were taken from Schoemaker (1980). These questions asked subjects to make choices over prospects which involved uncertain outcomes. Their answers were classified as either risk taking, risk neutral, or risk averse. The risk averse measure for the group was based on the percentage of risk averse responses by the group to these eight hypothetical questions.

TABLE XIV

LOGIT ESTIMATE OF GROUP COMPOSITION EFFECTS

	(1) All EP	(2) EP=0	(3) All EP	(4) EP=0
Constant	10.12 (2.63)	9.79 (3.77)	10.40 (1.64)	11.28 (2.24)
Penalty	-0.15 (0.23)	-0.19 (0.032)	-0.15 (0.023)	-0.18 (0.031)
Probability	-5.67 (1.07)		-5.67 (1.07)	
Profit	-0.019 (0.0061)	-0.024 (0.0092)	-0.019 (0.0060)	-0.023 (0.0088)
Outside Earnings	-0.0021 (0.00085)	-0.0029 (0.0013)	-0.0021 (0.00063)	-0.0034 (0.00092)
Percent Female	0.0020 (0.012)	0.013 (0.018)		
Risk Aversion	-0.14 (0.031)	-0.27 (0.052)	-0.14 (0.031)	-0.25 (0.049)
Size	1.21 (0.47)	2.81 (0.73)	1.17 (0.35)	2.28 (0.54)
WEEK 3	--	--	--	--
WEEK 4	0.49 (0.46)	1.63 (0.70)	0.50 (0.46)	1.56 (0.68)
Luck	0.025 (0.009)	0.039 (0.014)	0.025 (0.0094)	0.035 (0.014)
Log Likelihood	-176.91	-88.44	-176.94	-88.85
Number of Observations	371	203	371	203

indicated on their screener. Sex composition is controlled for by using the variable Percen Female which is the percentage of the bidding group that is female. Size is simply the number of bidders in the cartel. Risk attitudes, or more specifically the degree of risk aversion reported by the bidders, is measured by the variable Risk Aversion. This variable was constructed by using the replies to a series of hypothetical questions on risk-taking situations included in the screener. Risk Aversion is specific to each bidding group and is the average proportion or risk averse replies for that particular bidding group.

As the results in Table XIV indicate the outside income of the subjects does appear to be a consistent determinant of the decision to risk prices above minimum costs. The sign on the coefficient (negative) while consistent with the sign of the coefficient for experimental income, is puzzling. Once again subjects appear to display increasing relative risk aversion. Increases in the level of income whether its from the experiment or from outside sources, appear to cause subjects to be <u>less</u> willing to undertake risky activities. The consistency if not the direction of this result is comforting.

In terms of group compositon it appears from the results reported in Table XIV that, at least in our sample, the higher proportion of females in the group and/or the larger the bidding group the more willing the group is to bear risk by setting the low bid above the minimum cost. Why female bidders should be more willing to bear risks than their male counterpart is not obvious. The results on the size of the bidding group on the other hand do have some basis in theory. Since the only meaningful effect of group size in this experiment is on the degree of risk spreading it should come as no surprise that large groups are in fact more willing to take risks.[53]

Finally, the results in Table XIV concerning Risk Aversion are comforting. It is reassuring to see that those groups with members whose answers to hypothetical questions about risky situation showed the most amount of risk aversion took the fewest gambles in actual practice. As the results in Table XIV indicate, the average percentage of risk averse answers appears to be negatively correlated with a bidding group's willingness to risk penalties by setting their low bid above the minimum cost. Moreover, the correlation appears to be quite stable and highly significant. In this particular case, hypothetical and actual behavior appear to be quite strongly correlated.

<u>Experience</u>

The dummy variables WEEK 3 and WEEK 4 in the logit regressions reported in Tables IX and XIII were used to test for experience effects. If learning by subjects over the entire experimental period is an important factor then, as we indicated above, the signs in both variables should be of the same sign. However, whenever these experience variables are included the sign of WEEK 3 is negative while WEEK 4 alternates in sign. Also, the standard error on these coefficients indicates these effects are never significant. Therefore, the a priori belief that experience might influence subjects behavior appears to be incorrect.

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[53]Since coordination was not a problem and cheating was virtually absent in this experiment, group size was only potentially important in terms of the pooling effect.

CONCLUDING COMMENTS

In this paper we subjected the economic theory of choice under uncertainty as applied to collusive price stting to a laboratory test. What we found is that at least under experimental conditions the willingness of sellers to risk the imposition of penalties for collusion is extremely sensitive to the expected returns from that activity. In the 95 experimental periods when the expected profit from collusive pricing was positive, sellers in these experiments set the price above minimum cost in 97% of these periods. On the other hand, of the 364 periods in which expected returns to collusive pricing were negative, sellers set price above minimum costs in only 2% of these cases. When collusive pricing was a fair bet, i.e., when the expected returns to setting prices above minimum costs was zero, sellers did so nearly 50% of the time.

Perhaps the most significant finding in these experiments, however, is the strong indication that large penalties are more effective in deterring collusive price setting than are high detection levels. For example, while it is true that sellers set prices above costs in 46% of the periods when the expected returns were zero they did so in only 10% of the cases when the fine level was $25.00 but in 70% of the cases when the fine level was $5.00. Moreover, we found the phenomenon that the severity of the sanction is more powerful than the certainty of detection in controlling collusion to be quite

general in our experimental populations. Of the six bidding groups used in this experiment, only one group's behavior was not consistent with this pattern. As we note above, what is somewhat perplexing is that while bidders appear to be risk averse in their responses to penalty specifications, i.e., large fines are more powerful deterrents than high detection levels, these same bidders react to expected values in a more risk neutral fashion. After all, among risk averse bidders we should not be finding a significant number of collusive bids when the expected profit from doing so is zero. We suggest some possible reconciliations in the text, but this is clearly a phenomenon that requires additional analysis.

Another somewhat anomalous result in these experiments is the effect of income on the decision to collude. Basically we found in these experiments that as a cartel's accumulated profits increased, whether measured in total or per capita terms, the willingness of these bidders to risk a penalty by setting their bid above minimum costs decreased. This was also true for outside earnings. Cartels with members who had the highest average outside earnings appeared least willing to bear risk. Our results clearly contradict the implications of decreasing absolute risk aversion and appear to be consistent with the findings that unsuccessful firms are more likely to be involved in collusion than more successful firms.[54]

The bidding group characteristics that we found to be related to the collusive decisions were the size of the group and individual's stated attitutde toward risk. Size, as we would have expected, was positively related to the decision to collude. Since the only effect of size in these experiments appears to be on risk taking the result is comforting but hardly surprising. Risk attitudes were measured by subjects' responses to hypothetical questions on risky choices and it is interesting and relevant

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[54]Asch and Seneca, 1976 in an empirical study of collusion report evidence which indicates that 'unsatisfactory profit performance may motivate firms to collude' (p.7).

to observe that such hypothetical responses are correlated with actual choices. The relationship between the composition of the bidding group by gender and risk taking behavior was investigated and no stable relationship was found.

As this brief summary indicates, the results of our experiments are both interesting and relevant, but perhaps most important of all it demonstrates the potential of laboratory experiments for testing issues of penalty specification and enforcement strategy. Although the experiments reported in this paper were used to test issues of interest to antitrust authorities the method has quite general applications. Our results indicate that experimental methods are a useful alternative to field testing in an area where observing activity levels is problematic. The same approach that is used in this paper might be employed to test sanction policies in areas as diverse as pollution control and security violations.

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APPENDIX A

INSTRUCTIONS TO COMPETITIVE MARKET AND CARTEL

COMPETITIVE TRAINER

ECONOMIC EXPERIMENT

This is an economics experiment to test how individuals make decisions. It is possible to earn a lot of money. How much money you earn depends on your decisions and the decisions of others in the experiment. The amount you earn will be recorded by us. You will participate in a series of experiments. All the money you earn in these experiments will be paid to you after your participation is over.

How you earn this money will now be explained. Read this section carefully. The better you understand the experiment, the more money you will make.

INSTRUCTIONS

In this experiment you will act as a seller in a market. You will be selling a make-believe commodity. The market where you sell this commodity is designed as a "sealed-offer" auction. You are probably familiar with "oral" auctions, where an auctioneer shouts out a price and individuals signal him to bid on the good. A sealed offer auction is different in two ways:

1. You make "offers" to SELL a commodity rather than "bids" to purchase the commodity.

2. All offers to sell are secret. You write down your offers on a piece of paper. No other sellers will know your offer unless you win the auction.

The experiment is run over several periods. You can think of each period as a trading day. In each period, or trading day, you will be competing against other sellers attempting to win a production contract. The winner of the production contract will be the seller whose offer to sell is the lowest among all the sellers.

How is the auction run? You will be given your Record Sheet before the experiment begins. It will allow you to keep track of one "week" of trading days. Below is an example of how one trading day will look on your Record Sheet:

Trading Day (Example)			
A Offer	$ ______		
B Production Cost	$ 6.00	Winning Offer	$ ______
C Actual Quantity Sold	______	Quantity Sold	______
Period Profit (A − B) x C	$ ______		
Total Profit	$ ______		

Note the production cost is already filled in. This tells you how much it cost to produce one unit of the commodity. You will then decide on your offer to sell one unit of the commodity. You will be given slips of paper to write your offer on. This slip of paper will be collected and all sellers' offers will be used to determine which seller made the lowest offer to sell.

As an example, let's say there are three sellers in the experiment. Each seller has the following Production Cost and makes the following offers to sell:

	Production Cost	Offer
Seller 1	$ 6.05	$ 6.60
Seller 2	6.10	6.50
Seller 3	6.00	6.22

The winner of the production contract is Seller 3 since his offer of $6.22 is the lowest offer. How much will be bought from the sellers in the example above? Seller 1 and Seller 2 will selling nothing since their offers to sell were too high. The amount Seller 3 sells is determined by a demand rule. This demand rule is constructed by us prior to the experiment. It will be the same demand rule for the entire experiment. The demand rule gives the amount of the commodity that is purchased for different winning offers. The only thing you will be told about the demand rule is the maximum quantity that might be purchased. Whether the maximum quantity is actually bought depends on the winning offer. The maximum quantity that could be purchased will be announced at the beginning of the experiment. For the example above, let's say the maximum quantity was announced at 20 units. This means the most that could be purchased in any trading period is 20 units. How much was purchased from Seller 3? We would use the demand rule to find this out. Given that seller 3's offer was $6.22, let's say the quantity actually purchased from Seller 3 is 10 units. We can now calculate the profits for Seller 3 in the example period:

Seller 3's Profits = (Winning Offer - Production Cost) X Quantity Sold

= ($6.22 - $6.00) X 10

= $0.22 x 10

= $2.20.

Seller 3's Record Sheet would then look like:

A Offer	$ 6.22		
B Production Cost	$ 6.00	Winning Offer	$ 6.22
C Actual Quantity Sold	10	Quantity Sold	10
Period Profit	$ 2.20		
Total Profit	$ 2.20		

Remember, Seller 1's and Seller 2's offers were not accepted. They would sell nothing and their profits would be zero in that period. Seller 2's Record Sheet would look like this:

A Offer	$ 6.50		
B Production Cost	$ 6.10	Winning Offer	$ 6.22
C Actual Quantity Sold	0	Quantity Sold	10
Period Profit	$ 0		
Total Profit	$ 0		

Seller 1's Record Sheet would be similar but his Offer and Production Cost figures would be different.

The last page of the instructions is your Record Sheet for the one week of trading (5 trading days). If the experiment runs longer than one week you will be given a new Record Sheet. There is a place to write down the maximum quantity. This will be announced before the experiment begins. You will be given slips of paper to write down your offers to sell. A trial period will be run to help you better understand the experiment.

It is important to remember the following:

1. After being given your production cost you must decide on your offer to sell.

2. To win the production contract, your offer must be lower than all the other sellers' offers.

3. Your profit equals:
   (Offer - Production Cost) X Quantity Sold.
   Your profits depend on the difference between your offer and production cost times the quantity sold. If you sell nothing then your profits are zero.

4. Keep a running total of your previous period profits using the total profit row on your Record Sheet.

RECORD SHEET

NAME ______________________ TRADING WEEK ______

SELLER # _____ MAXIMUM QUANTITY ______

PROFITS PREVIOUS WEEK $ __________

Trading Day 1

A Offer $ ______

B Production Cost $ ______ Winning Offer $ ______

C Actual Quantity Sold ______ Quantity Sold ______

Period Profit (A - B) x C $ ______

Total Profit $ ______

Trading Day 2

A Offer $ ______

B Production Cost $ ______ Winning Offer $ ______

C Actual Quantity Sold ______ Quantity Sold ______

Period Profit $ ______

Total Profit $ ______

Trading Day 3

A Offer $ ______

B Production Cost $ ______ Winning Offer $ ______

C Actual Quantity Sold ______ Quantity Sold ______

Period Profit $ ______

Total Profit $ ______

Trading Day 4

A Offer $ ______

B Production Cost $ ______ Winning Offer $ ______

C Actual Quantity Sold ______ Quantity Sold ______

Period Profit $ ______

Total Profits $ ______

Trading Day 5

A Offer $ ______

B Production Cost $ ______ Winning Offer $ ______

C Actual Quantity Sold ______ Quantity Sold ______

Period Profit $ ______

Total Profit $ ______

INSTRUCTIONS

In today's experiment the procedure followed will be changed slightly. The auction will be run exactly as yesterday, except for inclusion of a recess period. This recess period will be held prior to the submitting of offers. This brief recess period will allow you to talk to other sellers. The recess period will be held in a separate room from where the auction takes place. ANY TOPIC MAY BE DISCUSSED. There are two distinct differences between the recess room and the auction room. First, you will not be allowed to bring any papers from the auction room to the recess room or vice-versa. Secondly, talking amongst sellers can only take place in the recess room. DO NOT talk about anything while you are in the auction room. Address any questions you have to the experimenter.

Also in today's round we will pay the winning seller a contract bonus. This contract bonus is a fixed amount of money that is added to the winning seller's profit. The amount of the contract bonus will be announced before the experiment begins.

You will be told your profits from yesterdays experiment. It will be written on your Record Sheet for the first trading week. Remember, we will record the profits you earn for the entire week. You will be paid the sum of your profits on Thursday.

RECORD SHEET

NAME ______________________ TRADING WEEK ______

SELLER # _____ MAXIMUM QUANTITY ______

PROFITS PREVIOUS WEEK $ ________

Trading Day 1

A Offer $ ______

B Production Cost $ ______ Winning Offer $ ______

C Actual Quantity Sold ______ Quantity Sold ______

Period Profit (A - B) x C $ ______

Total Profit $ ______

Trading Day 2

A Offer $ ______

B Production Cost $ ______ Winning Offer $ ______

C Actual Quantity Sold ______ Quantity Sold ______

Period Profit $ ______

Total Profit $ ______

Trading Day 3

A Offer $ ______

B Production Cost $ ______ Winning Offer $ ______

C Actual Quantity Sold ______ Quantity Sold ______

Period Profit $ ______

Total Profit $ ______

Trading Day 4

A Offer $ ______

B Production Cost $ ______ Winning Offer $ ______

C Actual Quantity Sold ______ Quantity Sold ______

Period Profit $ ______

Total Profits $ ______

Trading Day 5

A Offer $ ______

B Production Cost $ ______ Winning Offer $ ______

C Actual Quantity Sold ______ Quantity Sold ______

Period Profit $ ______

Total Profit $ ______

UNIVERSITY OF ARIZONA

Auction Project

(Screener)

October 21, 1985

This screener is to familiarize you with the type of decisions you will be making in the auction project. However, in the actual experiments that we will have next week you will do less reading and you will be actively involved as a seller. In all the experiments including this screener you will make money. How much money you make depends on your decisions. There is NO RIGHT decision but the better the decision the more money you make.

The experiments you will be in are designed to be like real life business situations. You make money in these experiments buying and selling a make-believe product. The only thing we are concerned with is the decisions you make.

Every time you are in an experiment you will make money. You will earn money in two different ways.

1) You will make money just for being in the experiment. We will call this your experiment "fee". At the end of the experiment you will be paid a fixed fee or amount of money. This will be paid to you after the experiment is over.
2) In addition to the experiment "fee" you will also earn money by participating in the experiment. This is your "profit". We will keep track of the "profits" you make in the experiments. The profits you earn from participating in the experiments will be paid every Friday. All money will be paid in cash.

SECTION 1

This first section is to obtain some general information. You need to include your name so that we can pay you the money you win in the final section. You do NOT have to answer any of the other questions.

NAME ____________________

1. What is your age? ____________
2. What year of school are you in? ____________
3. What is your major? ____
4. At the present time, are you:

   Married ____
   Widowed ____
   Divorced ____
   Separated ____
   Never Married ____

5. What is your race?

   Black ____
   Chicano/Latino ____
   White ____
   Asian ____
   Indian Native ____
   Other ____

6. a) Do you have a job at present? Yes____ No____

   b) If yes, how much money do you gross per month? $________

7. What other income do you receive per month? (i.e., from parents, inheritance, scholarships, etc.)

   $________

8. a) Do you own a car? Yes____ No____

   If yes,

   b) Year ______ Make __________________ Model __________________

   c) Do you have theft insurance on the car? Yes____ No____

9. Do you ever gamble? (i.e., make bets with your friends, go to Las Vegas, etc.) Yes____ No____

   If yes, how much of your total monthly income do you use to gamble?

   0 - 9% ____
   10% - 19% ____
   20% - 29% ____
   30% - 39% ____
   40% - 49% ____
   Over 50% ____

10. a) In the last year, have you had any moving violations? Yes____ No____

    b) If yes, how many and for what? ________________________________

11. In order to loan $10 to a stranger who was certain to repay you, how much would you want back a month from now? $____________

    How much would you want if he paid you a year from now? $__________.

SECTION 2

This situtation will be explained to you by the person who is conducting the test. Please pay close attention, and ask any questions about the example you may have.

STOP - And wait for Experimenter to explain.

After the problem is presented to you by the experimenter, answer the following question.

1) There are 100 balls in each of 2 urns. Some are blue and some are red. You must choose the urn from which you want a ball to be drawn. But before you choose consider the following:

a) If a red ball is drawn from the urn you will win $10.00.

b) If a blue ball is drawn you will win between $10.00 and $20.00.

c) The number of blue balls in each urn is between 0 and 100.

d) The number of blue balls is probably not the same in both urns.

e) The payoff, if a blue ball is chosen, is probably not the same for both urns.

Finally, before you are asked to choose from which urn you want to pick a ball, you can learn one of two things about the urns.

a) The number of blue balls in each urn

or

b) The payoff of each urn if a blue ball is chosen.

1) Circle the one you would want to know

a) Number of blue balls in each urn.

b) The payoff if a blue ball is drawn from the urn..

c) Either the number of blue balls or the payoff, it doesn't really matter.

In this section we list several situations and ask you to choose between two alternatives in each situation. Circle the alternative you would prefer if you had to make a choice. If you think both are about the same, circle both a and b.

Situation 1

a. An equal chance of getting either \$6.00 or \$10.00.

b. \$8.00 for sure.

Situation 2

a. An equal chance of getting either \$1,000.00 or \$2,000.00.

b. \$1,500.00 for sure.

Situation 3

a. 1 out of 4 chance of getting \$10.00 or 3 out of 4 of getting \$2.00

b. \$4.00 for sure.

Situation 4

a. 1 out of 4 chance of getting \$4,000.00 or 3 out of 4 chance of getting \$800.00.

b. \$1,600.00 for sure.

Now imagine that you are faced with two unfavorable situations, neither of which you want to be in. However, suppose you have to make a choice. Which situation would you rather be in?

Situation A: you stand a 1 out of 1000 chance of losing $10,000.

Situation B: you can buy insurance for $10 to protect you from this loss.

ANSWER: I prefer A I prefer B Indifferent

strong preference								weak preference

Consider the following situation. Someone offers to flip a coin with you. If the coin comes up heads he will pay you $10.00, if the coin comes up tails you pay him $10.00. You are sure that the coin is "fair" meaning it is equally likely that either a head or tail will occur. Also, you are certain this individual will pay you the $10.00 if heads comes up. Would you take his offer?

1) Definitely Yes

2) Probably Yes

3) Maybe Yes, Maybe; No I'm indifferent

4) Probably Not

5) Definitely Not

Put down either 1, 2, 3, 4, 5,

1) Answer ______________

What if the same offer was made for $2.00

2) Answer ______________

SECTION 3

In this final section you will play some bets for actual money ($$$). All the money you make will be deposited in your bank account later this week.

In these bets you must decide the minimum amount you are willing to sell each bet for. At the end of this section all bets will be played.

The bets will look like this:

	BAD LUCK PAYOFF	CHANCE POINT	GOOD LUCK PAYOFF
(Example Bet)	0	50	100

What is the minimum amount you would sell this bet for?

(Example) SELLING PRICE ____________________.

You will be asked to name selling prices for 20 bets. After you have stated your selling price for all 20 bets then we will either:

a) buy the bets from YOU at a price equal to or greater than your selling price, or

b) play the bet and you win either 0 or 100 cents.

Whether we buy the bet from you or you play the bet depends on chance. First, we draw a number between 0 and 100 at random. This represents the amount we are willing to pay for that bet. If it is at least as much as your selling price then we buy the bet from you and you receive the amount (in cents) that was drawn. If the number drawn was less than your selling price you will NOT sell the bet, and instead you play the bet.

Now lets see how this would work out on one sample bet. Let's say your selling price was 40 cents for the above bet and the number drawn was 60. This means that we will buy the bet from you for $0.60. If the number drawn equaled 30 then you would not sell the bet but would play it instead. If you play, then we will do the following:

We will randomly draw another number between 0 and 100. This number is compared to the "Chance Point" (equal to 50 in the example above). If the number drawn is equal to or less than 50 then you receive the "Bad Luck" payoff of 0 cents ($0.00). If the number is greater than 50 then you will receive the "Good Luck" payoff of 100 cents ($1.00).

Remember, your SELLING PRICE should be the amount of money that makes you not care whether you receive your selling price or actually play the bet.

It is in your best interest to give your true selling price. Let's say on the bet given above the TRUE value of that bet to you is $0.50. If you report a SELLING PRICE of $0.70 this would not be in your best interest. If the first number drawn is equal to 60, we would not buy your bet and you would be forced to play the bet even though you would rather have sold it for $0.60. Suppose you understated the SELLING PRICE at $0.30 and the number drawn is 40. Then you would be forced to sell the bet even though you would rather have played it.

NAME ______________________________ TEST ____
Please Print

RECORDS SHEET

BET	BAD LUCK PAYOFF	CHANCE POINT	GOOD LUCK PAYOFF	
1	0	50	100	SELLING PRICE ____________
2	0	80	100	SELLING PRICE ____________
3	0	15	100	SELLING PRICE ____________
4	0	90	100	SELLING PRICE ____________
5	0	60	100	SELLING PRICE ____________
6	0	30	100	SELLING PRICE ____________
7	0	25	100	SELLING PRICE ____________
8	0	45	100	SELLING PRICE ____________
9	0	70	100	SELLING PRICE ____________
10	0	55	100	SELLING PRICE ____________
11	0	10	100	SELLING PRICE ____________
12	0	35	100	SELLING PRICE ____________

Signature ______________________________

NAME ______________________ TEST __________

Please print

BET	BAD LUCK PAYOFF	CHANCE POINT	GOOD LUCK PAYOFF	
13	0	85	100	SELLING PRICE __________
14	0	40	100	SELLING PRICE __________
15	0	5	100	SELLING PRICE __________
16	0	65	100	SELLING PRICE __________
17	0	75	100	SELLING PRICE __________
18	0	20	100	SELLING PRICE __________
19	0	95	100	SELLING PRICE __________
20	0	38	100	SELLING PRICE __________

Signature ______________________

APPENDIX B

Group Tables:

Proportion of time each group

set winning offer above cost for

the three cartel deterrence

experiments.

CARTEL DETERRENCE EXPERIMENTS

Group 1, All Period Types

Percentage of Winning Offers Exceeding Winning Seller's Cost

Probability of Detection

Expected* Profit	0.1	0.25	0.5	Total
+2.00	100% Wk. 2 3 4 Total 4/4 – – 4/4			100% 4/4
+1.25	100% Wk. 2 3 4 Total 4/4 – – 4/4	100% Wk. 2 3 4 Total – 5/5 2/2 7/7		100% 11/11
0	0% Wk. 2 3 4 Total 0/4 – 0/6 0/10	8% Wk. 2 3 4 Total 0/4 1/5 0/3 1/12	75% Wk. 2 3 4 Total 4/4 4/5 1/3 9/12	29% 10/34
-1.25		0% Wk. 2 3 4 Total 0/4 – 0/3 0/7		0% 0/7
-2.50	0% Wk. 2 3 4 Total – 0/5 0/3 0/8	0% Wk. 2 3 4 Total – 0/5 0/4 0/9	0% Wk. 2 3 4 Total – 0/5 0/5 0/10	0% 0/27
-5.00		0% Wk. 2 3 4 Total 0/2 0/4 0/3 0/9	0% Wk. 2 3 4 Total 0/4 – 0/4 0/8	0% 0/17
-7.50		0% Wk. 2 3 4 Total – 0/5 0/4 0/9		0% 0/9

*When winning offer equaled Monopoly Price.

CARTEL DETERRENCE EXPERIMENTS

Group 2, All Period Types

Percentage of Winning Offers Exceeding Winning Seller's Cost

Probability of Detection

Expected* Profit	0.1					0.25					0.5					Total	
	%	Wk. 2	3	4	Total	%	Wk. 2	3	4	Total	%	Wk. 2	3	4	Total	%	
+2.00	100%	–	4/4	–	4/4											100%	4/4
+1.25	100%	–	4/4	–	4/4	86%	5/5	–	1/2	6/7						91%	10/11
0	20%	–	0/4	2/6	2/10	31%	3/5	1/5	0/3	4/13	8%	1/5	0/4	0/3	1/12	20%	7/35
-1.25						0%	–	0/4	0/3	0/7						0%	0/7
-2.50	0%	0/5	–	0/3	0/8	0%	0/4	–	0/4	0/8	0%	0/5	–	0/5	0/10	0%	0/26
-5.00						0%	0/4	0/4	0/3	0/11	0%	–	0/5	0/4	0/9	0%	0/20
-7.50						0%	0/5	–	0/4	0/9						0%	0/9

*When winning offer equaled Monopoly Price.

CARTEL DETERRENCE EXPERIMENTS

Group 3, All Period Types

Percentage of Winning Offers Exceeding Winning Seller's Cost

Expected* Profit	Probability of Detection: 0.1	0.25	0.5	Total
+2.00	100% Wk. 2 3 4 Total – 4/4 – 4/4			100% 4/4
+1.25	100% Wk. 2 3 4 Total – 4/4 – 4/4	100% Wk. 2 3 4 Total 5/5 – 2/2 7/7		100% 11/11
0	10% Wk. 2 3 4 Total – 1/4 0/6 1/10	55% Wk. 2 3 4 Total 4/4 1/4 1/3 6/11	100% Wk. 2 3 4 Total 4/4 4/4 3/3 11/11	56% 18/32
-1.25		0% Wk. 2 3 4 Total – 0/4 0/3 0/7		0% 0/7
-2.50	43% Wk. 2 3 4 Total 3/4 – 0/3 3/7	0% Wk. 2 3 4 Total 0/4 – 0/4 0/8	0% Wk. 2 3 4 Total 0/4 – 0/5 0/9	13% 3/24
-5.00		0% Wk. 2 3 4 Total 0/4 0/4 0/3 0/11	0% Wk. 2 3 4 Total – 0/5 0/4 0/9	0% 0/20
-7.50		0% Wk. 2 3 4 Total 0/4 – 0/4 0/8		0% 0/8

*When winning offer equaled Monopoly Price.

CARTEL DETERRENCE EXPERIMENTS

Group 4, All Period Types

Percentage of Winning Offers Exceeding Winning Seller's Cost

Probability of Detection

Expected* Profit	0.1	0.25	0.5	Total
+2.00	100% Wk. 2 3 4 Total – 4/4 – 4/4			100% 4/4
+1.25	80% Wk. 2 3 4 Total – 4/5 – 4/5	100% Wk. 2 3 4 Total 5/5 – 2/2 7/7		92% 11/12
0	11% Wk. 2 3 4 Total – 1/4 0/5 1/9	58% Wk. 2 3 4 Total 4/4 0/5 3/3 7/12	58% Wk. 2 3 4 Total 2/4 4/5 1/3 7/12	45% 15/33
-1.25		0% Wk. 2 3 4 Total – 0/4 0/3 0/7		0% 0/7
-2.50	43% Wk. 2 3 4 Total 3/4 – 0/3 3/7	0% Wk. 2 3 4 Total 0/4 – 0/4 0/8	0% Wk. 2 3 4 Total 0/4 – 0/5 0/9	13% 3/24
-5.00		0% Wk. 2 3 4 Total 0/4 0/4 0/3 0/11	0% Wk. 2 3 4 Total – 0/5 0/4 0/9	0% 0/20
-7.50		0% Wk. 2 3 4 Total 0/4 – 0/4 0/8		0% 0/8

*When winning offer equaled Monopoly Price, Group 4 consistently set its winning offer equal to 20 cents above cost rather than the monopoly markup of 25 cents above cost. This causes expected profit to be 20 cents lower than actually stated above.

CARTEL DETERRENCE EXPERIMENTS

Group 5, All Period Types

Percentage of Winning Offers Exceeding Winning Seller's Cost

Expected* Profit	Probability of Detection 0.1	0.25	0.5	Total
+2.00	100% Wk. 2 3 4 Total 5/5 – – 5/5			100% 5/5
+1.25	100% Wk. 2 3 4 Total 5/5 – – 5/5	100% Wk. 2 3 4 Total – 5/5 2/2 7/7		100% 12/12
0	30% Wk. 2 3 4 Total 0/5 – 3/5 3/10	83% Wk. 2 3 4 Total 3/5 4/4 3/3 10/12	100% Wk. 2 3 4 Total 5/5 4/4 3/3 12/12	74% 25/34
-1.25		0% Wk. 2 3 4 Total 0/5 – 0/3 0/8		0% 0/8
-2.50	0% Wk. 2 3 4 Total – 0/4 0/3 0/7	0% Wk. 2 3 4 Total – 0/4 0/4 0/8	0% Wk. 2 3 4 Total – 0/5 0/4 0/9	0% 0/24
-5.00		0% Wk. 2 3 4 Total 0/5 0/5 0/3 0/13	0% Wk. 2 3 4 Total 0/5 – 0/4 0/9	0% 0/22
-7.50		0% Wk. 2 3 4 Total – 0/4 0/4 0/8		0% 0/8

*When winning offer equaled Monopoly Price.

CARTEL DETERRENCE EXPERIMENTS

Group 6, All Period Types

Percentage of Winning Offers Exceeding Winning Seller's Cost

Probability of Detection

Expected* Profit	0.1	0.25	0.5	Total
+2.00	100% Wk. 2 3 4 Total – 5/5 – 5/5			100% 5/5
+1.25	80% Wk. 2 3 4 Total – 4/5 – 4/5	100% Wk. 2 3 4 Total 4/4 – 2/2 6/6		91% 10/11
0	0% Wk. 2 3 4 Total – 0/5 0/6 0/11	67% Wk. 2 3 4 Total 4/4 1/5 3/3 8/12	92% Wk. 2 3 4 Total 4/4 4/5 3/3 11/12	54% 19/35
-1.25		0% Wk. 2 3 4 Total – 0/5 0/3 0/8		0% 0/8
-2.50	0% Wk. 2 3 4 Total 0/4 – 0/3 0/7	0% Wk. 2 3 4 Total 0/4 – 0/4 0/8	11% Wk. 2 3 4 Total 0/4 – 1/5 1/9	4% 1/24
-5.00		0% Wk. 2 3 4 Total 0/4 0/5 0/3 0/12	0% Wk. 2 3 4 Total – 0/5 0/4 0/9	0% 0/21
-7.50		0% Wk. 2 3 4 Total 0/4 – 0/4 0/8		0% 0/8

*When winning offer equaled Monopoly Price.