CPRC Discussion Paper Series

Competition Policy Research Center
Fair Trade Commission of Japan

An Experimental Study of Procurement Auctions with Leniency Programs

Yasuyo Hamaguchi

Faculty of Economics, Nagoya City University

Toshiko Ishikawa

Hitachi Research Institute

Masayuki Ishimoto

Japan Fair Trade Commission

Yuji Kimura

Competition Policy Research Center, Japan Fair Trade Commission

Tadanobu Tanno

Faculty of Management, Atomi University

CPDP-24-E January 2007

1-1-1, Kasumigaseki, Chiyoda-ku, TOKYO 100-8987 JAPAN

Phone:+81-3-3581-1848 Fax:+81-3-3581-1945

URL:www.jftc.go.jp/cprc.html

E-mail:cprcsec@jftc.go.jp

An Experimental Study of Procurement Auctions with Leniency Programs*

Yasuyo Hamaguchi†
Toshiko Ishikawa‡
Masayuki Ishimoto§
Yuji Kimura**
Tadanobu Tanno††

Abstract

We experimentally study repeated procurement auctions with leniency programs. Leniency programs give immunity from fines to cartel firms which report to the antitrust authorities about their illegal activities. In our experiments, subjects can freely communicate before bidding, through an online chat system. We investigated whether introducing leniency programs is more effective at deterring cartels than an institution which only imposes a fine against bid rigging. Our results show that leniency programs are only as effective at deterring cartels as the institution with the fine. In addition to that, our results show that leniency programs may be effective to dissolve pre-existing collusions and make the contract price lower, but they are not powerful enough to dissuade firms from forming a new cartel and raising the transaction price.

KEYWORDS: Procurement Auction, Leniency Programs, Bid Rigging, Communication, Experimental Economics.
JEL Classification Number: L4, D44.

---

* This paper is based on joint research at the Competition Policy Research Center (CPRC) of the Japanese Fair Trade Commission, which is downloadable at http://www.jftc.go.jp/cprc/reports/cr-0306..pdf. The names of the authors are alphabetically ordered, which does not indicate their degree of contribution to this research. We thank Sobei H. Oda and staffs at Kyoto Sangyo University for letting us use the laboratory and their generous support for this research. We also thank Bradley Ruffle for his helpful comments and encouragement. All views expressed in the paper are our own.
† Faculty of Economics, Nagoya City University. Email: yhamagu@econ.nagoya-cu.ac.jp
‡ Hitachi Research Institute. Email: toshiko.ishikawa.ka@hitachi.com
§ Japan Fair Trade Commission. Email: Masayuki_Ishimoto@jftc.go.jp
** Competition Policy Research Center, Japan Fair Trade Commission. Email: Yuji_kimura@jftc.go.jp
†† Faculty of Management, Atomi University. Email: tanno@atomi.ac.jp

1. Introduction

To detect and deter cartels, many antitrust authorities have adopted antitrust policies called leniency programs. Since one characteristic of cartel activity is secrecy, it is difficult to collect evidence of illegal cartel activities such as price-fixing. In order for antitrust authorities to cope with the secrecy and prosecute cartels, leniency programs grant reduction in or exemption from sanctions for firms which report their illegal antitrust activity to the antitrust authorities before they are detected. This sort of policy gives cartel firms a second chance to deviate from other cartel participants after price fixing, which makes their collusion unstable.

Various kinds of leniency programs have already been introduced in many countries.[1] There are two categories of leniency programs, defined by their objectives. One is a program that grants exemptions from criminal or civil prosecution, and the other is a program that grants exemptions from or reduction in administrative penalties. The U.S., Canada and Ireland have adopted the former type of leniency program, while the EU, the UK, Germany, France, Netherlands, and South Korea have adopted the latter. In Japan, a leniency policy on surcharge payments was introduced in January 2006 along with the amendment of the Japanese Antimonopoly Act.[2]

Many economists have investigated the usefulness and limitation of leniency programs from the viewpoint of theory and experimentation. In theoretical analyses

[1] The U.S was the first country to introduce the programs. After the U.S. reformed their leniency programs in 1993, their effectiveness against cartels became widely recognized.

[2] The outline of this policy is as follows. The first applicant before the JFTC's on the-spot inspection shall be exempted from a surcharge payment. The second applicant before the JFTC's on-the-spot inspection shall be given a 50% reduction in the surcharge amount. The third applicant before the JFTC's on-the-spot inspection shall be given a 30% reduction in the surcharge amount. The Applicant after the JFTC's on-the-spot inspection shall be given a 30% reduction in the surcharge amount as long as the number of corporations reported before the inspection is less than three.

Motta and Polo (2003) compare different schemes of leniency programs and suggest an optimal leniency program. Brisset and Thomas (2004) discuss the efficiency of leniency programs under a first-price sealed-bid procurement auction. Apesteguia, Dufwenberg and Selten (2003) analyze leniency programs theoretically and experimentally under the one-shot Bertrand competition model. In part of their experiments, they experimented with a leniency program which gives a whistle blower some bonus, and found such a policy actually promotes cartel formation, contradictory to its theoretical prediction. Hamaguchi, Kawagoe, and Shibata (2005) study group size effects and how many firms should be allowed to get leniency in a repeated coordination game. They found that leniency programs work more effectively against bigger cartel groups, and giving a reward to a reporter is effective in deterring cartel formation. Hinloopen and Soetvent (2005) investigated the effect of leniency programs in a repeated Bertrand competition model, which is an extended version of Apesteguia, Dufwenberg and Selten (2003). They conclude that these leniency programs lead to lower prices and have little effect on recidivism. In our experiments, we studied repeated procurement auctions with leniency programs. Our experimental design was inspired by the method of Hinloopen and Soetvent. We are chiefly concerned with how bid riggings are organized and how introducing leniency programs affects them. Before bidding, subjects in our experiment could freely communicate by chatting online.[3] Since our experimentation allows subjects to communicate any content through an online chat system, we observed bid rigging formation, breakdown of collusion schemes and the effects of leniency programs. Our results show that leniency programs are effective to dissolve pre-existing collusions and make the contract price

[3] Hinloopen and Soetvent (2005) allowed subjects to communicate only the price range they would like to transact not free context.

lower, but they are not powerful enough to dissuade firms from forming a new cartel and raising the transaction price.

The remainder of this paper is structured as follows. Section 2 presents our experimental design. Section 3 analyzes the outcomes of our experimentation. Section 4 examines how communication affects collusive agreement. Section 5 presents our conclusions.

2. Why does a cartel occur?

Suppose there is a public authority which has budget constraints and wants to build a new building at the lowest cost. There are only five contractors which can potentially make a contract with the authority.[4] A public auction is the best way to find which company can build the building at the cheapest price. Ahead of the auction, the authority publicly announces the reserve price (z), which is the maximum price it can pay to build the building. If the auction is perfectly competitive, which means that bidders do not communicate about price fixing before bidding, then the winner of the action would be the company which can build the building at the lowest cost. If the winning company bid a price equal to its marginal cost, however, this would not be a profitable outcome for the company. If such an auction is held often, and the same bidders try to win the auctions, they would get to know each other and have an incentive to discuss about price fixing and rotate the winner in the sequence of auctions. For simplicity, assume that all bidders have the same marginal cost ($c<z$)) to compete for a certain public task (for example, they are similar in construction skills, the number of

[4] In general, the number of construction companies which can build large scale public facilities is limited. In addition to that, it is common in Japan for a public authority, such as a local government, to limit the range of potential bidders to only local contractors.

workers, and the cost of buying resources). They know that the winning bid would be close to the marginal cost plus ε if they fell into a fierce competition.[5] Let us assume that when all bidders bid the same price, the winner would be chosen randomly. One price fixing scheme is for all bidders to bid the reserve price. That way, the winner can get the maximum profit of (z-c). Since each bidder can be the winner with a probability of 1/5, the expected profits for an auction is (z-c)/5. The other price fixing scheme is for bidders to decide who will win in the upcoming auction, and the chosen winner bids a slightly lower price (z-ε) than the reserve price, which the other bidders bid. In this scheme, the winner can get the profits of (z-ε-c). Therefore, each bidder can get the profits once in 5 auctions (rotating among 5 bidders). Assume that the bidders believe that their interdependent relationship will last for an unpredictably long time. Then, their incentive structure can be modeled as a infinitely repeated game. We derive the conditions that the collusive strategy is supported as an equilibrium as follows. For simplicity, we assume that players (companies) are committed to bid rigging in the beginning, and would continue colluding by bidding the reserve price together in every auction unless a player defects. Once a player defects, no players will collude again and they bid (c+ε), i.e. the profit is only ε if a player wins an auction. We assume that each player decides what strategy they will take in the beginning of the repeated game. We first compare the case where there is no penalty on bid rigging.

[5] ε is the minimum bid increase. If bidders can bid any continuous price, then the competitive equilibrium price for a one-shot game converges toward the marginal cost. We assume that bidders can bid only discrete prices (ε is strictly larger than zero). Therefore, there are two kinds of equilibrium, one where everybody bids c, and the other everybody bids c+ε. If bidders never commit bid rigging and continue playing competitively for infinite times, the expected profit each play can make is only ε/5(1- δ), which is strictly lower than the expected profits through bid rigging. The expected profits through bid rigging are explained in the rest of this section.

1) *The expected profits for collusion when there is no penalty:*

$$E\pi_C = \frac{(z-c)}{5} + \delta\frac{(z-c)}{5} + \delta^2\frac{(z-c)}{5} + \cdots = \frac{z-c}{5(1-\delta)}.$$

δ is the discount rate (0<δ<1).

2) *The expected profits for defection when there is no penalty:*

$$E\pi_d = (z-\varepsilon-c) + \delta\frac{\varepsilon}{5} + \delta^2\frac{\varepsilon}{5} + \cdots = (z-\varepsilon-c) + \frac{\delta\varepsilon}{5(1-\delta)}.$$

The condition that $E\pi_c$ exceeds $E\pi_d$ is $\delta > (4k-5)/(5k-6)$, where k equals $(z-c)/\varepsilon$. If δ is high enough, it is profitable for each bidder to maintain the collusion.

Next we compare the expected profits when the authority introduces a fine scheme against bid riggers. Suppose the probability that the authority can detect a bidder who won an auction by bid rigging is *p*. If they detect collusion, they impose a fine rate *q* over the guilty party's winning price. Assume that the fine is imposed only on winners of the three most recent periods. That is, the fine scheme imposes a penalty not only on the winner of the current period but also on the winners of the last two periods. Even if a winning bidder was not detected in the preceding periods, he might have to pay a fine in one of the following two periods. Once he has paid a penalty for a certain period t, he does not have to pay again for this same period even if he is detected in periods t+1 or t+2.[6] To calculate the expected profits in the case with penalty is rather complicated (for

[6] In other words, if a bidder has won in periods 1, 2, and 3, and is caught bid rigging in period 3,

example, we have to consider the timing, how many times a bidder is detected, the probability a bidder is caught three times in a row, and so on). We can tell at least that the expected profit for collusion would be somewhere between the optimistic expected profit (never getting caught) and the pessimistic expected profit (always getting caught). The optimistic profit ($\overline{E\pi}_{cf}$) is the same as $E\pi_c$, the profits for collusion when there is no fine against bid rigging. The pessimistic profit ($\underline{E\pi}_{cf}$) is derived as follows.

3) *The pessimistic expected profits for collusion when there is a fine against bid rigging:*

$$\underline{E\pi}_{cf} = \frac{(1-q)z-c}{5} + \delta\frac{(1-q)z-c}{5} + \delta^2\frac{(1-q)z-c}{5} + \cdots = \frac{(1-q)z-c}{5(1-\delta)}.$$

The expected profits for defecting is derived as follows:

4) *The expected profits for defecting when there is a fine against bid rigging:*

$$E\pi_{df} = [(1-p)(z-\varepsilon-c) + p\{(1-q)(z-\varepsilon)-c\}] + \frac{\delta\varepsilon}{5} + \frac{\delta^2\varepsilon}{5} + \cdots = \{(1-pq)(z-\varepsilon)-c\} + \frac{\delta\varepsilon}{5(1-\delta)}.$$

The condition that even the pessimistic expected profit exceeds the expected profit for defection is that δ is larger than $4\{(1-q)(z-\varepsilon)-c\}/[5\{(1-q)(z-\varepsilon)-c\}-\varepsilon]$. The

---

he has to pay a fine on periods 1, 2, and 3. If the same bidder is then caught again in period 4, he only has to pay a fine on period 4, and not on periods 2 and 3.

question is whether such δ exists between 0 and 1. If $(1-q)(z-\varepsilon)-c$ is larger than ε, then there exists a δ within the range. The expression $(1-q)(z-\varepsilon)-c$ is interpreted as almost the same as the profit if the bid rigging is not detected. If this value is larger than the value of the minimum increase bid, ε, then even a bidder who estimates the expected profit for collusion pessimistically may have an incentive to keep colluding.

Next we compare the expected profits when a leniency program is introduced. Assume other players will not defect in their bidding and will not report to the authority. Similar to the previous section, the fine rate is q, which is imposed on the winning price. We assume that the number of bid riggers who can be exempted from fines are only the first few reporters, and only the first reporter can get the full immunity (other early reporters can get immunity less than 100 %). In the case when the leniency program is introduced, there are four strategies for player *i*: "collude and not report"; "collude and report"; "defect and not report"; "defect and report." The expected profit for each strategy is explained below.

5) The expected profit for collude and not report ($E\pi_{cnr}$).

$E\pi_{cnr}$ is the same as $E\pi_{cf}$ which is some value between $\overline{E\pi}_{cf}$ and $\underline{E\pi}_{cf}$.

6) The expected profit for collude and report ($E\pi_{cr}$).

$$E\pi_{cr} = \frac{z-c}{5} + \delta\frac{\varepsilon}{5} + \delta^2\frac{\varepsilon}{5} + \cdots = \frac{z-c}{5} + \frac{\delta\varepsilon}{5(1-\delta)}.$$

7) The expected profit for defect and not report ($E\pi_{dnr}$).

$E\pi_{dnr}$ is the same as $E\pi_{df}$.

8) The expected profit for defect and report ($E\pi_{dr}$):

$E\pi_{dr}$ is the same as $E\pi_{d}$ since the player does not have to pay the fine by reporting.

Comparing these four expected profits, we can tell immediately that it is not rational to choose the "defect and not report" for player i since $E\pi_{dr}$ always exceeds $E\pi_{dnr}$.

To know in what condition the collusion would be maintained under the leniency program, we compare the expected profits of the remaining three strategies. First, compare $E\pi_{cnr}$ and $E\pi_{cr}$ to examine whether a player who did not defect in bidding has an incentive to report or not. If δ is larger than $qz/(z-\varepsilon-c)$, then it is more profitable for player *i* not to cheat the other players by reporting ($E\pi_{cnr} > E\pi_{cr}$). Assume that this condition is satisfied. Now we compare $E\pi_{cnr}$ and $E\pi_{dr}$ to examine whether player *i* should defect in bidding rather than maintain the collusion. As we mentioned before, it is rather difficult to calculate the exact value of $E\pi_{cnr}$, which is the same as $E\pi_{cf}$. We can tell only that $E\pi_{cnr}$ is larger than $\underline{E\pi}_{cf}$. Therefore, we use $\underline{E\pi}_{cf}$ to compare with $E\pi_{dr}$ and find a strict condition when keeping the collusion is more profitable than defecting. If δ is larger than $\{4(z-\varepsilon-c)+qz-\varepsilon\}/\{5(z-\varepsilon-c)-\varepsilon\}$, then $\underline{E\pi}_{cf}$ exceeds $E\pi_{dr}$, and it is more profitable for player *i* to maintain the collusion ($E\pi_{cnr}$ is strictly larger than $E\pi_{dr}$). The question is whether such a δ exists between 0 and 1. If $(z-\varepsilon-c)$ is larger than qz , then there exists a δ within the range. Since we already assumed that δ is larger

than $qz/(z-\varepsilon-c)$, $(z-\varepsilon-c)$ is surely larger than qz. Therefore, leniency programs are not sufficient to deter cartels. Especially if a fine rate is too low, it is very possible that the leniency program does not have any impact on firms which are involved in cartels.

3. Experimental Design and Procedure

Our experiments were run at the Kyoto Sangyo University Experimental Economics Laboratory on December 17, 20, and 21 in 2005. Three sessions were run in total, and one session was conducted on each day. 67 subjects were recruited in total from among undergraduates of various majors. They applied for the experiment voluntarily through the Internet. We tried to recruit subjects whose backgrounds (major, year in school) were not similar, so that most subjects would not be put into a group with someone they already knew.

All experiments were run by computers. All the treatments were programmed and conducted with the software z-Tree (Fischbacher (1999)). We also used a free software program called "Minna no chat" that allows subjects to communicate online before price bidding.

In each session, there were four six-member groups. Five of the members in each group played the role of seller, and one member played the role of an observer for his group.[7] The role of buyer was played by the computer, which determined who bid the

[7] The task of the observer of each group was to monitor who entered the online chat room. The reason why we had one observer for each group is that we needed one computer in each group as a server computer for the chat system. Although the chat system and the zTree program were at work simultaneously, the two systems were not connected mechanically. Therefore, somebody had to input the information on who entered the chat room from the chat software program to the zTree program manually. (When there were not enough subjects, experimenters played the role of observers and completed the task.) We instructed and assisted subjects who had to play the role of observer in the course of the experiments, so that they would not make

minimum price in each group. If there was more than one seller who bid the minimum price, only one of those sellers was chosen randomly by the computer to complete the transaction.

We ran four kind of treatments: *Benchmark, Communication, Antitrust, and Leniency.*[8] The values for the parameters in the theoretical model were: the reserve price, z, is 200, the homogeneous marginal cost, c, is 100, the minimum bid increase (or decrease), ε, is 10, the probability of investigation by the authority, p, is 0.15, the fine rate, q, is 0.10. In each treatment, the auction game was repeated for 15 rounds. Subjects were not told the exact number of repetitions at the beginning of experiments. We told them that the auction game would be repeated for at least 12 rounds, and after the 12th round, the computer would decide randomly whether the game would be continued or terminated. The details of each treatment are explained below.

Benchmark

This treatment is a competitive market where five sellers compete for one contract with one monopolistic buyer.[9] In this treatment, sellers are not allowed to communicate with each other before bidding. Each seller bids a price simultaneously and independently within the range of 100 to 200 with 10-unit increments.

---

any mistakes in inputting the information. They could see the content of the chatting in their group, but they were not allowed to participate in the conversation

[8] The names of these treatments follow those of Hinloopen and Soetvent (2005) [6]. The characteristics of each treatment are very similar to theirs.

[9] This treatment is similar to the posted-offer market experiment by Roth, Prasnikar, Okuno-Fujiwara, and Zamir (1991). In their experiments, there were 7 to 9 buyers and one seller. They found that transaction prices went down quickly to an equilibrium in the early rounds of the experiment, since the market was fairly competitive. Communication among subjects was not allowed.

Communication

In this treatment, sellers can communicate with each other before bidding. At first, they decide whether they will enter the chat room or not. Each subject could chat only with same-group members, and not with people in other groups. There is no penalty for chatting (or bid rigging). The chatting time is limited to three minutes. After the communication stage, each seller bids a price simultaneously and independently as in the Benchmark treatment.

Antitrust

This treatment is similar to the Communication treatment except that there is a fine against chatting. Each seller is investigated by a hypothetical antitrust authority (the computer) after the auction with the probability of 0.15. Similar to the Communication treatment, sellers first decide whether they will enter the chat room or not. If only one person entered the room, this was not considered as illegal communication. If more than one person entered the room, this was regarded as illegal communication and a fine was imposed on them if they were investigated. After the auction, the computer drew a lottery for each subject at the end of every round and determined who was investigated. If the investigated seller had entered the chat room and there was at least one other person in the room before bidding, he is given a fine. The fine is 10% of the winning price (gross profits) earned in the current round and any two rounds prior, only if the detected subject won the auctions. A fine can only be applied once per round. To illustrate, if a seller is found to be involved in collusion in the third round, then his fine is applied to his first, second, and third round profits. If the same seller is then found to

be in collusion in the fourth round, his fine is based only on his fourth round profits. As in the Communication treatment, the chatting time is limited to three minutes.

Leniency

This treatment adds to the Antitrust treatment the possibility for sellers to apply for a leniency program. Similar to the Antitrust treatment, subjects who communicated with other group members are fined if they are investigated. Those who entered the chat room can decide whether they will apply for the leniency program or not after the auction. The first applicant can get full immunity from a fine, the second applicant can get 50% immunity, and the third applicant can get 30% immunity. People who did not enter the chat room or people who entered the room alone, do not have to move on to the leniency application decision.

A summary of the characteristics of each treatment is shown in Table 1. In each session, two treatments were run in sequence. The combination of the two treatments in each session is explained in Table 2. In each session, the Benchmark treatment was implemented first, and then one of the other treatments followed. That is, subjects experienced the competitive auction without communication first, and then they experienced a treatment with communication.

The experimenter read aloud the instructions in front of the subjects, who read along using their own printed instructions.[10] Subjects went through three practice rounds to get used to how to use the mouse and keyboard and how to understand their computer screen. In the Benchmark treatment, subjects were not allowed to make free

[10] We used neutral words in the instructions. However, to make subjects understand the nature of the fine better, we used the word "surcharge." The instructions are available upon request.

decisions in the practice rounds (they typed a certain input which was given by the experimenter). In other treatments with communication, subjects practiced how to chat online. We instructed subjects to input certain content first and then allowed them to input messages freely in the later practice rounds.[11] [12] No subject was forced to enter the chat room in the real rounds.

In all the four treatments, subjects were informed whether they won the auction or not, and the winning price was revealed to all subjects after the auction. However, bidding prices of other subjects were not revealed, and each subject could not know who was the winner if he was not the winner. At the end of each round, subjects could see the information of his own profits for the round. He was not informed of the information related to other people's profits. In the Leniency treatment, subjects who applied for the leniency program were informed how much immunity they could get, but not about how much immunity other subjects got. Subjects who did not apply for the leniency program were informed via their computer screens whether any other member in his group applied for the leniency program. Each seller had their own ID ("member 1," "member 2," "member 3," "member 4," "member 5") in Communication, Antitrust, and Leniency. Their IDs were the same for each treatment throughout the experiments. The identities of the subjects were kept secret.

[11] It was possible for subjects to discuss about price fixing for the real rounds in the practice rounds. Actually some groups formed collusions within the practice rounds, and they decided not to enter the chat room in the real rounds, but bid the highest price together till the end of the experiment. Some researchers might think that this free communication in the practice rounds could contaminate the data in the real rounds. However, this free communication is also a part of our experimental design that reflects the reality. There is always a certain period from when a law passes the cabinet until the law is really enacted. The situation in the practice rounds was similar, i.e. potential bidders discuss what to do after the law is enacted. We will discuss about the effects of the pre-play bid rigging conversation in the practice rounds in the later section.

[12] There was no control on the content of communication except for the identity of each subject. We believe this free communication condition resembles a real face-to-face bid rigging situation.

The fine scheme was designed to be similar to the JFTC scheme. The immunity schedule is also similar to JFTC's leniency program.[13] [14]

Subjects were paid in cash individually after the experiment. The amount they were paid was determined by how much they earned in the experiment (the currency unit was called "dollar" in the experiment. One dollar was exchanged for 10 yen). The average earning was 2,873 yen (about 24.8 US dollars), the highest was 7,000 yen (about 60.3 US dollars), and the lowest was 2,500 yen (about 21.6 US dollars). Each session lasted about three hours.[15]

4. Predictions

In this section, we compare the expected profits according to the theoretical model discussed in Section 2. Since subjects were told that the experiment would be repeated at least for 12 rounds, subjects might plan what to do for the first 12 rounds and calculate the expected profits for those rounds. For simplicity, we assume that subjects do not discount their profits.

[13] However, we did not reflect some characteristics of the JFTC leniency program. The JFTC program allows firms to apply for the leniency program even after the investigation. If fewer than three firms applied for the program before the investigation, there is still a chance for firms to get some immunity. In addition, the surcharge is getting higher if a firm was found to be repeatedly involved in bid rigging in the last 10 years. Investigating the effects of those institutional factors is our future research plan.

[14] It is hard to know what is the real probability of being investigated by the authority, since it is not really random monitoring. Rather, the authority would investigate a company only when they have some crucial information. In this research, the probability of investigation (15%) is similar to the one Hinloopen and Soetevent (2005) used, so that we can compare our results with theirs.

[15] There were some subjects who earned less than 2,000 yen. We considered that they had to work for three hours and paid them 2,500 yen, which guaranteed approximately the standard hourly rate of a part-time job outside the campus (800 yen). Subjects were not told about this minimum payment until after the experiment.

In the Benchmark treatment, since subjects can not discuss bid rigging, it is predicted that they will bid 110 together from the early rounds. The expected profit for the first 12 rounds is just 24（=12*（110－100）/5）.

After experiencing such a competitive treatment, it is predicted that subjects will try to collude with each other in the next treatment (with communication) and bid 190 or 200. In the Communication treatment, if a subject does not enter the chat room (does not commit bid rigging) and bids 110 from the first period, the expected profits for this case for the first 12 rounds is the same as the one for the Benchmark treatment (=24).[16] If subjects commit bid rigging and bid the reserve price together and rotate the winner randomly in every round, the expected profits for this case is 240 (=12*(200-100)/5). The expected profit for defecting from the first period (a bidder bid rigged, but cheated the other bid riggers by bidding 190) is 112. Therefore, it is more profitable for each subject to commit bid rigging in the Communication treatment.

In the Antitrust treatment, the expected profit for collusion is 232.8, while the expected profit for defecting is 109.15. Therefore, it is still more profitable for subjects to maintain their collusion.

In the Leniency treatment, the expected profit for colluding and not reporting is 232.8 (the same as the expected profit for colluding in the Antitrust treatment). The expected profit for colluding and reporting is only 42. The expected profit for defecting and reporting is 112 (the expected profit for defecting and not reporting is always dominated by the expected profit for defecting and reporting. See the discussion in

[16] If there is any player who does not enter the chat room, it is rational for other members to predict that the outsider would try to win over them by bidding a lower price than the reserve price. Upon knowing that, everybody eventually bids c+ε and tries to win the auction with the probability 1/5. The expected profit in this case for each round is only 2 (=(110-100)/5).

Section 2). Therefore, it is still more profitable to maintain collusion under the leniency program.

However, in the prediction above, we assume that subjects would give up collusion immediately as soon as any player defects or reports to the authority to punish the defector (trigger strategy). This assumption might be too strong, and the prediction might not be the same as the real behavior of subjects. In addition to that, maintaining collusion is only one of the equlibria. It is also an equilibrium that nobody commits bid rigging. Since it is not costly at all for subjects to enter the chat room, and the content of communication did not bind subjects to a specific price to enter in the bidding stage, chatting can be considered as merely cheap talk. In this case, subjects might behave as competitively as in the Benchmark treatment. Moreover, subjects might use a different kind of bid rigging scheme.[17] Therefore, we believe that experimenting on this sort of situation in the laboratory is helpful in observing plausible bid rigging schemes and what form an optimal leniency program might take.

5. Analysis of Results

5.1 Bid Prices and the Rate of Bid Rigging[18]

Figure 1 compares the average winning prices of all treatments. The average winning price in Benchmark never went higher than 111, while the average winning price in the Communication treatment never went lower than 192.5. This shows the existence of fierce competition in the Benchmark treatment, and the existence of strong

[17] McAfee and McMillan (1992) discuss bid rigging schemes.
[18] The definition of "*bid rigging*" is a case in which more than one person entered the chat room, while the definition of "*non bid rigging*" is a case in which fewer than two people entered the chat room.

collusion in the Communication treatment. The average winning prices in Antitrust and Leniency are somewhere between the two extreme results.

Figure 2 compares the number of bid riggers in the three treatments in which pre-play communication was allowed. It shows that subjects almost always commit bid rigging in the Communication treatment (the only exception is that only one person did not enter the chat room in the 7th round). The lines of Antitrust and Leniency appear almost overlapping, and the average number of bid riggers in each group is from 2 to 3.

The top panel in Table 3 shows the average winning prices in five-round intervals. The bottom panel of Table 3 shows the statistical differences in winning prices among all the treatments (by using a Mann-Whitney U test). It shows that there is a strong difference between Benchmark and the other three treatments (the overall comparison: $z=-2.38$, $p=0.02$ for Benchmark and Communication; $z=-2.07$, $p=0.04$ for Benchmark and Antitrust; $z=-2.07$, $p=0.04$ for Benchmark and Leniency). This confirms that when communication among bidders is possible, the market becomes collusive and the transaction prices are significantly higher than the ones in a competitive market. One interesting finding is that the difference between Benchmark and Antitrust is no longer significant in the last interval (Round 11-round 15). This means that imposing fines against bid riggers dissolves cartels eventually. On the other hand, the difference between Benchmark and Leniency is consistently significant across all the intervals. However, there is no significant difference between Antitrust and Leniency in all the intervals (the overall comparison: $z=-0.15$, $p=0.88$). The differences between Communication and Antitrust, and between Communication and Leniency are also statistically significant (the overall comparison: $z=1.69$, $p=0.09$; $z=2.37$, $p=0.02$,

respectively). They indicate that introducing an antitrust policy is effective in lowering the transaction price.

The top panel of Table 4 shows the average number of bid riggers in five-round intervals. The bottom panel of Table 4 shows the statistical differences in the number of bid riggers among all the treatments in the same way as in Table 3. Similar to the statistical results in Table 3, the difference between Communication and Antitrust and between Communication and Leniency are significant (the overall comparison: $z=2.37$, $p=0.02$, $z=2.37$, $p=0.02$, respectively). On the other hand there is no significant difference between Antitrust and Leniency (the overall comparison: $z=0.29$, $p=0.77$).

Table 5 summarizes more results on the average winning price of bid riggers and those of non bid riggers, the rate of bid rigging, and the rate of using the leniency program. The rates of bid rigging in the Antitrust and Leniency treatments are 73.3% and 66.7% respectively. The average durations of bid rigging are also similar between Antitrust and Leniency (5.5 rounds and 5.7 rounds, respectively).

One puzzling finding for the Antitrust and the Leniency treatment in Table 5 is that the average winning price among bid rigging groups is lower than the one among non bid rigging groups. This contradictory result stems from the fact that some groups had already reached collusion in the practice rounds and decided to bid the reserve price and not to enter the chat room in the real rounds. On the other hand, subjects in the groups which could not reach a collusive agreement within the practice rounds started with lower prices. We will discuss the effects of pre-play bid rigging in the following section.

5.2 Pre-play Collusion in the Practice Rounds and Its Effects on Bid Prices

To analyze the puzzle raised in the previous section more closely, we classified groups by their chat content in the practice rounds of both Antitrust and Leniency. We use the term "pre-collusive" for those groups that made a collusive agreement within the practice rounds, and "non pre-collusive" for those groups which could not reach any collusive agreement within the practice rounds.[19] [20] Table 6 shows that the average winning prices among pre-collusive groups are much higher than non pre-collusive groups in both Antitrust and Leniency. Moreover, we divided the data according to whether more than one person entered the chat room (categorized as "bid rigging") or not (categorized as "non bid rigging") in the real rounds. It shows that "non bid rigging" subjects in pre-collusive groups kept their promise of price fixing in the real rounds and the wining price of those cases are even higher than those of "bid rigging" subjects in pre-collusive groups (the average winning prices of "non bid rigging groups" in Antitrust and Leniency are 180.2 and 182.5, respectively).

Except for one group, all the groups in Antitrust agreed on the price fixing scheme in which everybody bids the reserve price every round. One group kept their collusion until the last round (i.e. the winning price of this group was always 200). One other group maintained their collusion until the 5th round. However, the collusion of this group started breaking up in the 6th round. The winning price of this group went down to 110 in the 9th round and never went up again until the last round. The other pre-collusive group discussed the same price fixing scheme in beginning of the 1st round. However, their collusion was dissolved immediately, and the winning price of this group went down to 110 in the 2nd round and it never went up again until the last

[19] See also footnote 11.
[20] While players can exchange messages in our pre-auction chat practice, there are other pre-auction forms. One example is a first price pre-auction knock-out. See McAfee and McMillan (1992).

round. It seems difficult to keep the pre-play promise for a long time without having communication together again in the real rounds.

In the Leniency treatment, there were two groups which discussed price fixing within the practice rounds. One group maintained collusion until the 9th round, and the other maintained collusion until the 10th round. After those rounds, the groups started breaking up their collusions and ended up with winning prices of 110 or 100.

The other two groups started competitively. One of the groups went down to 110 from the beginning and never managed to reach any collusion. The other group started competitively from the first round and remained competitive until the 8th round (i.e. the winning price was 110 in all these rounds). However, all the members of this group started colluding from the 9th round. Although they did not collude until the 9th round, there were a number of people who entered the chat room who intended to form a collusion (3 subjects were in the chat room from round 1 to round 3, and 4 subjects were in the chat room from round 4 to round 8. Before the 9th round, subjects who had already entered the chat room agreed to bid 110 together by saying, “Let’s bid 110 together until the last subject enters the chat room.”) Their attempt was successful and the last subject finally entered the chat room at the 9th round. As soon as this subject joined the chat room, the group started bidding 200 together from the 9th round until 12th round. Although the winner in such a case is chosen randomly, 4 different subjects happened to win in the 9th to 12th rounds. The remaining subject who had not won in any of the rounds told about that in the chat room at the beginning of 13th round. The other members decided to make him win for sure by using another kind of price fixing

scheme, in which one bidder bid 190 and the others bid 200. They used this price fixing scheme from the 13th round until the end.[21]

Even if there was an agreement to repeat collusion within the practice rounds, or such collusion emerged in the middle of the real rounds, outsiders who do not enter the chat room make it difficult for insiders (bid riggers) to raise their bidding prices. [22]

Except for one group in Leniency, there was no group which managed to start collusion within the real rounds. Table 6 shows that the average winning prices of non pre-collusive groups are not high. Although the average winning prices of bid riggers in non pre-collusive groups in Leniency seem to be relatively higher than those of non bid riggers, it is due to the exceptional group which started collusion from the 9th round described above. Table 7 compares the rate of change in the winning price between pre-collusive groups and non pre-collusive groups, and between bid riggers and non bid riggers. It shows that the price decline in pre-collusive groups is higher than non pre-collusive groups, which confirms that the pre-play collusion did not last long and was dissolved eventually. The results of all cells in Table 7 are negative except for the cell of bid riggers in non pre-collusive groups. This reflects the behavior of the group which started collusion from the 9th round.

Overall, it seems that the fine and the monitoring rate introduced in this experiment threaten individual bidders strongly enough to deter cartels both in Antitrust and Leniency. Since we do not have enough samples, we are not yet sure whether the one

[21] In the 13th round, although this group tried to make one subject win the auction for sure, it seems that some subjects were confused about how to do it (to let the subject win the auction, they thought they should bid prices lower than 190). One bid 100 by mistake, and he won. He confessed about that in the chat stage of the next round. After this round, everybody seemed to understand the new price fixing scheme.

[22] Saijo, Une, Yamaguchi (1996) finds that the existence of outsiders is crucial to lower the transaction price in their experimental study.

emergence of collusion within the real rounds is just a rare case or whether it could happen often. Also, we are not sure whether this emergence is because the leniency program makes it easier for bid riggers to start colluding. It might be true that although leniency programs can intercept established collusions (pre-play collusions), they might be only partially effective in preventing newly generated bid rigging.

Without either bid rigging or pre-play collusion, we found that the average winning price was nearly the competitive price (109.1 and 109.3 in Antitrust and Leniency, respectively).

5.3 Who Used the Leniency Program?

Table 8 classifies what kind of bid riggers used the leniency programs. "Defect" means a bid rigger who cheated other bid riggers by bidding a price lower than 190. "Win" means winning an auction. 90% of "Defect and Win" bid riggers used the leniency program. The reason for this behavior is obviously to protect their profits. This observation is similar to the result in Hinloopen and Soetvent (2005).[23]

One interesting finding is that subjects who did not win the auction also frequently applied for the leniency program. There is no incentive for them to do that for their own profit. However, they could reduce the winner's profit if they applied for the leniency program faster than the winner. Subjects answered in the post-experiment questionnaire that they understood the experiment game before the experiment was started, so we believe that this behavior is not due to confusion.

5.4 Comparison of Collected Fines between Antitrust and Leniency

[23] The rate of using the leniency program in Hinloopen and Soetvent (2005) is about 50%.

Table 9 summarizes the comparison of the collected surcharges between the Antitrust treatment and the Leniency treatment. In the Antitrust treatment, the collected surcharges amounted to 191 overall, while those of the Leniency treatment came to 324. However, 191.1 out of 324 was exempted by the Leniency treatment; therefore, the net collected surcharges were 132.9 in the Leniency treatment. This means that the net surcharges imposed on subjects were actually less in the Leniency treatment than in the Antitrust treatment. The reason why the (gross) collected surcharges were much higher in the Leniency treatment than in the Antitrust treatment is because more bid riggings were revealed voluntarily by subjects who applied for the leniency program. That is, an antitrust authority is able to expose more bid riggings by introducing a leniency program. Since the probability of investigation was the same between the Antitrust treatment and the Leniency treatment, we can assume that the cost of investigation was the same between the two treatments.

Despite that fact that many bid riggings were exposed in the Leniency treatment, why was the average winning price not significantly lower than in the Antitrust treatment? Since the collected surcharges were less in the Leniency treatment than in the Antitrust treatment, it is possible that the penalty effect of surcharges was reduced by the leniency program. In general, a leniency program is expected to make collusion unstable by creating defectors in the collusive groups. However, subjects in our experiment might have thought that the policy was just a safety net to protect a winner's profits, which was rotated among group members.

The Japanese antimonopoly law imposes higher surcharges on firms which repeatedly commit bid rigging. Our experiment results suggest that such a policy against recidivism, and constant monitoring of those who were already caught are necessary to

make a leniency program work properly. The purpose of an antitrust authority is to promote competition among firms so that consumers can get the maximum benefits. To achieve this goal, the authority should design their policy carefully. They should not only promote internal defectors from bid rigging groups (carrots), but also maintain the penalty effect of surcharges (sticks).

5.5 Collusion Schemes Observed in Communication Records

As we discussed in the previous sections, we found two specific kinds of price fixing schemes in the subjects' chat logs. We discuss and examine those schemes more closely in this section. We use the term "Stochastic Winner Scheme" (SWS, hereafter) to refer to the scheme in which all bidders bid the reserve price and the winner is randomly chosen.[24] We call the other scheme "Bid Rotation Scheme" (BRS, hereafter), in which bidders decide who will win the upcoming auction for certain. In this scheme, the chosen winner bids a slightly lower price than the reserve price, while the other bid riggers bid the reserve price.[25] In our experiment setting, every bid rigger bid 200 in SWS. In BRS, the one bid rigger who was chosen to win bid 190, and the other bid riggers bid 200. Groups which choose BRS would rotate the winner by some trivial rule, such as the order of their ID numbers.

Table 10 shows the details of observed price fixing schemes in Communication, Antitrust, and Leniency. "Failure Groups" represents the groups which could not sustain their collusion until the last round. "Successful Groups" represents the groups which could sustain their collusion scheme to the last round.

[24] See "weak cartel" in McAfee and McMillan (1992) for more on this collusive scheme.
[25] See Aoyagi (2003) and Thomas (2005) for this collusive scheme. Thomas (2005) discusses which collusion schemes are stable.

Overall, it seems that SWS is used more frequently than BRS (only one group in Communication, and only one in Leniency used BRS. These groups also used SWS; therefore, they are categorized in the "Both" column). The group that adopted the BRS in the Communication treatment actually decided to follow SWS in the beginning. They practiced how to let a bidder win for sure within the practice rounds. There were some subjects who were confused about the scheme in the practice round (they thought that the chosen winner should bid 200 and the others should bid 190). They decided to follow BRS in the pre-play communication in the 1st round. They rotated the winner every round until the 8th round in the order of their ID numbers (member 1 won in the 1st round, member 2 won the second, and so on). However, one member started cheating by bidding 190 in the 8th round, but he could not win the round and the chosen winner luckily won by lottery. In the 9th round, the same cheater bid 190 and the promised winner in this round could not win the auction. The cheater pretended like he had not cheated, and nobody in the group could tell who was the cheater in the chat conversation. The other bid riggers gave another chance to the losing "winner" of the 9th round, and he won in the 10th round. From the 11th to the 14th round, this group still promised to follow BRS in the chat room. However, the cheater and another cheater did not let the last subject win the auction until the 14th round. In the 15th round, the group members discussed following SWS from that round, since everybody had won twice until then. All the members bid 200 in the 15th round except for the same cheater who started cheating in the 8th round (he bid 180).

In the Leniency treatment, the group that used BRS is the one which started with competitive pricing first, but managed to collude in the 9th round. They used SWS first (from the 9th round to 12th round). Since one subject could not win at all in those 4

rounds, others decided to let him win for sure by using BRS in the 13th round. There were several subjects who were confused about using BRS in the 13th round, and the subject who had never won the auction finally won in the 14th round. In the 15th round, the members of this group decided to continue using BRS and chose the member to be the winner of each round according the order of their ID numbers. We are not sure how long they would be able to maintain their collusion if the experiment was repeated for more than 15 rounds.

The reason why SWS is used more frequently than BRS might be because SWS is less complex than BRS. In BRS, bid riggers have to make sure who will win every round. To ensure that BRS is maintained correctly by all the members, bidders need to know publicly (through the chat room) the true information about who was the winner of the last round and who is supposed to win in the next round. On the other hand, for the players to ensure that SWS is maintained by all the members, the members can tell if there is a cheater or not by observing the winning price (=200). They do not have to decide who will win every round. This difference might be the reason why SWS is used more prevalently than BRS.

6. Concluding Remarks

In this experiment, we investigated the enforcement power of an antitrust policy with leniency programs. Although our results are only preliminary because of the shortage of samples, it seems that the effect of leniency programs on prices is ambiguous. Under the Antitrust treatment and the Leniency treatment, fewer members engaged in bid rigging in the real rounds. They reached pre-play bid rigging in the practice rounds, and decided to bid the reserve price and not to enter the chat room in every real round. Although

some of these pre-collusive groups maintained their collusion until the end, most of these sort of collusions were dissolved by defectors in the middle of the experiments. The results overall seem to indicate that when there is a penalty against bid rigging and the chance of being detected is high enough, the antitrust policy works well to deter cartels.

One notable result is that there was one group in the Leniency treatment that started competitively at first, and then started colluding in the middle of the experiment. This might indicate that the kind of leniency program we investigated might have limited power to prevent firms from starting new collusions.

We observed two kinds of price fixing schemes. Since price fixing behavior is closely related to fairness (giving an equal chance to get a high profit), to investigate what price scheme is more stable is an interesting issue. The size of a cartel might be a crucial factor in this stability issue since the degree of difficulty of rotating the winner depends heavily on the size of the cartel.

Collusions seem to be very robust phenomena. Subjects who had never talked to each other before manage to create collusion very quickly. We found that observing the real conversations of "bid riggers" in the laboratory is notably helpful to understand the nature of cartels and to design an optimal antitrust policy.

References

Aoyagi, M. (2003), "Bid Rotation and Collusion in Repeated Auctions," *Journal of Economic Theory,* 112, pp.79-105.

Apesteguia,J., M.Dufwenberg, and R. Selten (2003), "Blowing the Whistle" working paper.

Brisset, K. and L. Thomas (2004), "Leniency Program: A new tool of the Competition Policy to deter cartel's activity in Procurement Auctions," *European Journal of Law and Economics,*17, pp.5-19.

Brisset, K. and L. Thomas (2004), "Leniency Program: A new tool of the Competition Policy to deter cartel's activity in Procurement Auctions," *European Journal of Law and Economics,*17, pp.5-19.

Fischbacher, U. (1999), "z-Tree-Zurich Toolbox for Readymade Economic Experiments-Experimenter's Manual" working paper Nr. 21, Institute for Empirical Research in Economics, University of Zurich.

Hamaguchi,Y, T.Kawagoe, and A.Shibata (2005), "An Experimental Study of Leniency Programs," mimeo.

Hinloopen, J. and A. R. Soetevent, (2005), "An Experimental Investigation of the Eieffects of Leniency Programs for Antitrust Enforcement," mimeo.

McAfee, R. P. and J. McMillan (1992), "Bidding Rings," *American Economic Review,* 82(3) 579-599.

Motta, M. and M. Polo (2003), "Leniency Programs and Cartel Prosecution," *International Journal of Industrial Organization,* 21(3), pp.346-379.

Roth, A., V. Prasnikar, M. Okuno-Fujiwara, and S. Zamir (1991), "Bargaining and market behavior in Jerusalem, Ljubljana, Pittsburgh, and Tokyo: An experimental study," *American Economic Review*, 81, pp.1068-95.

Saijo, T., M. Une, and T. Yamaguchi (1996), "'Dango' experiments," *Journal of the Japanese and International Economies*, 10, pp. 1-11.

Thomas, C. J. (2005), "Using Reserve Prices to Deter Collusion in Procurement Competition," *Journal of Industrial Economics,* 53(3), pp.301-326.

Table 1. Characteristics of the Treatments

	Benchmark	Communication	Antitrust	Leniency
Communication	No	Yes	Yes	Yes
Rate of investigation	No	No	15 %	15%
Immunity of fines	No	No	No	Yes
Immunity for the 1st applicant				100%
Immunity for the 2nd applicant				50%
Immunity for the 3rd applicant				30%

Table 2. The Treatment Order and Number of Subjects

Session date	1st treatment	2nd treatment	The number of subjects
Session 1 (2005/12/17)	Benchmark	Communication	22
Session 2 (2005/12/20)	Benchmark	Antitrust	21
Session 3 (2005/12/21)	Benchmark	Leniency	24
	The total number of subjects		67

Table 3. Comparison between Treatments (Winning Prices)

	The Means of Winning Prices in Each Treatment			
	Round 1-5	Round 6-10	Round 11-15	Overall
Benchmark	108.3	106	108.3	107.8
Communication	197.5	197.5	195.5	196.8
Antitrust	157	141.5	132	143.5
Leniency	155	162	134	150.3
	Comparison of Means by Mann-Whitney U test (z-value (p-value))			
Benchmark vs. Communication (Ben.=0)	-2.38 (0.02)	-2.37 (0.02)	-2.38 (0.02)	-2.38 (0.02)
Benchmark vs. Antitrust (Ben.=0)	-2.34 (0.02)	-2.32 (0.02)	-1.62 (0.11)	-2.07 (0.04)
Benchmark vs. Leniency (Ben.=0)	-2.35 (0.02)	-2.02 (0.04)	-2.32 (0.02)	-2.07 (0.04)
Communication vs. Antitrust (Com.=0)	0.99 (0.32)	1.70 (0.09)	1.70 (0.09)	1.69 (0.09)
Communication vs. Leniency (Com.=0)	1.00 (0.32)	1.38 (0.17)	2.37 (0.02)	2.37 (0.02)
Antitrust vs. Leniency (Ant.=0)	0.32 (0.75)	-0.15 (0.88)	-0.89 (0.37)	-0.15 (0.88)

Table 4. Comparison between Treatments (The Average Number of Bid Riggers)

	The Means of the Number of Bid Riggers in Each Treatment			
	Round 1-5	Round 6-10	Round 11-15	Overall
Communication	5.00	4.95	5.00	4.98
Antitrust	2.40	2.40	2.90	2.57
Leniency	1.95	2.40	3.30	2.55
	Comparison of means by Mann-Whitney U test (z-value (p-value))			
Communication vs. Antitrust (Com.=0)	2.46 (0.01)	2.37 (0.02)	2.48 (0.01)	2.37 (0.02)
Communication vs. Leniency (Com.=0)	2.46 (0.01)	2.38 (0.02)	1.98 (0.05)	2.37 (0.02)
Antitrust vs. Leniency (Ant.=0)	0.58 (0.56)	0.15 (0.88)	-0.45 (0.66)	0.29 (0.77)

Table 5. Summary Results of Treatments

		Benchmark	Communication	Antitrust	Leniency
The average winning price		107.8	196.8	143.5	150.3
	Bid rigging	-	196.8	126.9	130.5
	Non-bid rigging	107.9	-	158.8	169.2
The rate of bid-rigging (%)		-	100	73.3	66.7
The rate of detection (%)	By chance	-	-	15.0	18.0
	By reporting	-	-	-	49.5
The rate of using the leniency program		-	-	-	33.0
The average duration of bid rigging (rounds)		-	15.0	5.5	5.7
The average number of bid riggers in each group		-	5.0	2.9	2.7

Table 6. Collusion in Practice Rounds and the Average Winning Prices

			Antitrust	Leniency
Pre-collusive groups	The average winning price		176.0	174.3
		Bid rigging	164.9	131.7
		Non-bid rigging	180.2	182.5
	% of subjects using leniency		-	9.3
Non pre-collusive groups	The average winning price		111.0	126.3
		Bid rigging	111.8	130.2
		Non-bid rigging	109.1	109.3
	% of subjects using leniency		-	56.7

Table 7: The Rate of Change in Winning Prices in Antitrust and Leniency

		Antitrust	Leniency
Pre-collusive groups	Bid rigging	-22.5%	-41.0%
	Non bid rigging	-18.4%	-18.4%
Non pre-collusive groups	Bid rigging	-3.3%	33.0%
	Non bid rigging	-0.0%	-0.01%

Table 8. Who Used the Leniency Program?

	Defect & Win	Defect & Not win	Not defect & Win	Not defect & Not win
Not used	11%	29%	38%	46%
Used	89%	71%	63%	54%
# of observations	19	73	8	46

Table 9. Comparison of Collected Fines

	Total surcharges	Total amount of immunity	Net surcharges
Antitrust	191.0	_	191.0
Leniency	324.0	191.1	132.9

Table 10: Comparison of Price Fixing Schemes

Treatments		SWS	BRS	Both
Communication	# of Adopting Groups	3	0	1
	# of Failure Groups	0	0	1
	# of Successful Groups	3	0	0
Antitrust	# of Adopting Groups	4	0	0
	# of Failure Groups	3	0	0
	of Successful Groups	1	0	0
Leniency	# of Adopting Groups	2	0	1
	# of Failure Groups	2	0	0
	# of Successful Groups	0	0	1

Figure 1. The Average Winning Price of Each Treatment

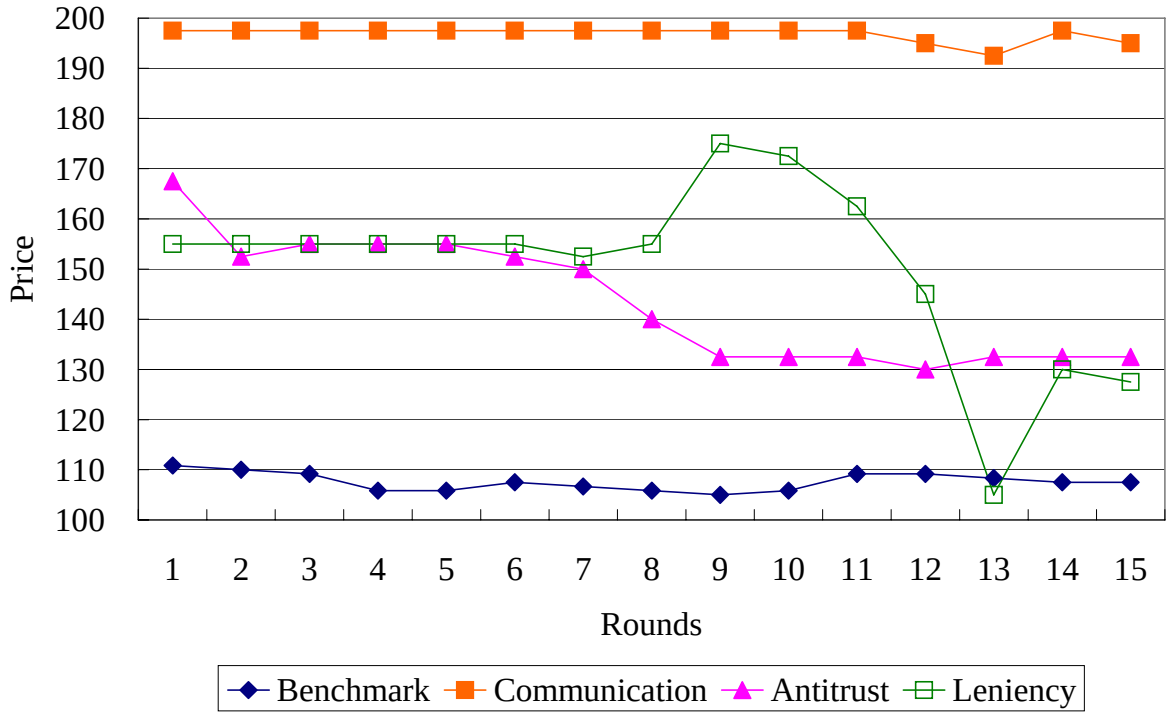

Figure 2. The Average Number of Bid Riggers