Group size effects on cartel formation and the enforcement power of leniency programs

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A R T I C L E   I N F O

Article history:
Received 21 June 2006
Received in revised form 21 May 2008
Accepted 21 May 2008
Available online 3 June 2008

JEL classification:
C92; D43; K21; K42

Keywords:
Leniency programs
Cartels
Collusion
Antitrust law
Experiment

A B S T R A C T

Antitrust authorities in many countries have been trying to establish appropriate competition policies based on economic analysis. Recently an anti-cartel policy called a “leniency program” has been introduced in many countries as an effective policy to dissolve cartels. In this paper, we studied several kinds of leniency programs through laboratory experiments. We experimentally controlled for three factors: (1) cartel size: the number of cartel members in a group, small (two-person) or large (seven-person), (2) fine schedule: the number of firms that are given leniency, and (3) degree of leniency: a partially reduced fine, a fully reduced fine, or a reward is given to self-reporting firms. The experimental results showed that (1) an increase in the number of cartel members in a group increased the number of cartels dissolved, (2) changing the fine schedule had no significant effect both in the two-person group size and in the seven-person group size, and (3) positive enforcement such as giving a reward for a self-reporting firm in a courageous leniency program has great impact on dissolving cartel activities.

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1. Introduction

Cartels, collusions among competing firms, harm the social welfare of consumers by restricting competition in markets. Such market restrictions include entry barriers, market-dividing activities, price fixing, and volume controlling. The major role of antitrust authorities (referred to hereafter as AA) is to restrain cartels. For example, the Japan Fair Trade Commission (JFTC) made recommendations for 19 cases of price fixing cartels and bid riggings in fiscal year 2005. Surcharges orders, which are legal means to confiscate excessive profits created by cartels, were imposed on 399 firms and the total amount of the surcharges amounted to 188.7 billion yen in fiscal year 2005.

An international trend is one of strengthening fines and surcharges. For example, JFTC submitted a major amendment to the Japanese Antimonopoly Act to the Diet in 2005 and the amendment was enacted in 2006. The essential features of the revisions are that the basic surcharge rate shall be increased from 6% to 10%, and that a leniency program shall accompany the surcharge system. Lowe (2003) describes EU’s future fine policy as follows:

The trend is clearly one of increasing fines, in order to achieve a genuine dissuasive effect on firms. In 2001, the heaviest individual fine yet, 462 million euros, was imposed against Hoffman–LaRoche in the Vitamins case. In 2002, the second highest amount ever, 250 million euros, was imposed against Lafarge for its participation in the Plasterboard cartel. Other significant fines were those imposed on the BPB, also in Plasterboard, 139 million euros and 118 million euros for Degussa for its role in the Methionine conspiracy.

In order to raise the probability of detecting cartels and to deter cartel activities, leniency programs have been implemented in many countries, such as the EU, the US, Canada, Australia, and Korea. They have proven that the program is a very effective device to detect cartels. A typical leniency program is carried out in the following way. If a member of a cartel group resigns from the cartel and reports himself to the AA with sufficient evidence of his cartel activity sooner

⁎ We would like to thank Sobei H. Oda and his assistants for their support and letting us use the Kyoto Sangyo University economics laboratory. We also thank Yoshio Ida for his advice on z-Tree programming. In addition, we thank Bram Cadsby, Martin Dufwenberg, Nicholas Felovich, Massimo Motta, Patrick Rey, Fei Song and Noriyuki Yanagawa for their helpful comments and encouragement. We would also like to thank the anonymous referee for comments on our first submission, which improved this paper significantly. This research was partially supported by the Ministry of Education, Science, Sports and Culture, Grant-in-Aid for Young Scientists. The views expressed here are the personal views of the authors and do not re

⁎⁎ According to the JFTC’s annual reports, a formal action means recommendations or surcharge payment orders without cease and desist orders preceding.

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than other cartel members, then his firm will be given full leniency and will be exempted from paying a fine at all. In the EU, which initiated its policy in 1996 and then made revisions in 2002, between 1996 and 2002, more than 80 firms cooperated with the EC Commission under the leniency program and out of a total of 24 decisions imposing fines, firms in 17 cases cooperated with the Commission under the leniency scheme.\(^2\) Under the 2002 Notice, 160 applications were filed in three and a half years.\(^3\) JFTC initiated a leniency program in 2006 and in fifteen months it received 105 leniency applications.\(^4\) That is, the number of leniency applications has been increasing dramatically.

There are some theoretical studies on various kinds of leniency programs using repeated game theory. Motta and Polo (2003) examined a leniency program in a repeated game setting and then they identified the equilibrium conditions to sustain collusion under the leniency program. In a similar model, Hinloopen (2002) theoretically analyzed European style leniency programs. In the European style leniency programs, a fine is considered to be proportional to gross annual sales of a firm (maximum fine up to 10% of total sales). Hinloopen showed that it is highly unlikely for a cartel member to report information to the AA unless the probability of detection and/or a fine are unrealistically high.

Brisset and Thomas (2004) obtained very similar results in the simplified first price auction settings. Compared with the European leniency program, Spagnolo (2000) proved that courageous leniency programs, which give rewards to self-reporting firms, may deter collusion completely and costlessly. Furthermore, Aubert et al. (2006) consider bounty mechanisms, which give a reward to an employee who informs his employer about illegal activity within their company.

Although these theoretical studies have been done, to the best of our knowledge, only a few experimental investigations have been carried out so far. Apesteguia et al. (2007) investigated leniency programs in a one-shot Bertrand competition framework theoretically and experimentally. They compared several variations of leniency programs including a courageous leniency program proposed by Spagnolo (i.e. giving a reward to applicants for the leniency programs) to determine whether or not the programs deter cartel activities. They found that the rate of cartel formation was the highest in the case that a reward was provided for the action of reporting, which contradicts the theoretical predictions. Hinloopen and Soetevent (2008) extended Apesteguia et al.'s study to the repeated game framework. They found that the leniency program is effective at lowering prices. However, they also found that the effect is not strong enough against cartel recidivism.

In many leniency programs, the first reporter has more advantageous immunity over later reporters. This might make cartel members rush to report to the AA to get the highest leniency. If this is true, then the leniency program which allows only the first reporter to get the highest leniency is more effective than the one which gives leniency to more than one reporter. In addition to that, the former policy might be more successful at finding cartel activities without increasing enforcement costs than the latter one. Since only unilateral deviations from the equilibrium are to be considered according to the Nash equilibrium concept, the equilibrium predictions in the two-person game models used in previous studies can be applied to the case where the game consists of more than two players and to the case where the fine schedule is limited only to the first reporting firm. However, we are not sure whether these predictions are true in real situations. If every firm involved in a cartel activity can give legally sufficient cartel information to the AA, the cartel can be dissolved easily just by one firm reporting. That might make each cartel member rely less on collusion as the number of cartel members increases. In addition, cartels might be dissolved much faster with the leniency program than without it, since if a firm reports the cartel information to the AA, they can avoid a considerable fine when the cartel is detected by the AA. Furthermore, such a deviation from collusion could be accelerated if only the first reporting firm can avoid the fine and others get a penalty. This tendency might be further reinforced by introducing a courageous leniency program which gives a reward for the reporting firm.

To investigate these institutional design issues, we must consider what the crucial variables that the AA can manipulate to prevent firms from forming cartels are. The variables the AA can control but firms can not are the probability of investigation and the level of the surcharge or fine. Those variables can greatly influence the incentive of firms for cartel formation. If the probability of being caught and the fine are very low (or high), firms believe that the expected profits that they could gain from the cartel would be greater (or smaller) than the expected losses from being caught.

Based on the considerations above, we experimentally controlled the following three factors to compare several institutional designs of leniency programs in a simplified oligopoly market:

1. **Carrot group size:** the number of cartel members in a group is either small (two members) or large (seven members).
2. **The fine schedule:** the number of firms that are given leniency is either only the first deviator (i.e. the first reporter of cartel information) or all deviators.
3. **Degree of leniency:** the firms that deviated from their cartel receive (1) partially reduced fines, (2) fully reduced fines or (3) rewards.

The model in our experiments is as follows. First, we assume that every firm has already reached collusive agreement and formed a cartel before the game (as the equilibrium condition mentioned later assures). They know the probability of being investigated by the AA, which is common knowledge among firms. Then, they voluntarily and independently decide whether or not to report the cartel information to the AA. If at least one player in a group reports the information, then the evidence of their collusion is revealed to the AA with certainty, and all but the players who reported the information suffer the full fine. The players who reported the information can receive either (1) exemption from penalty to some degree, (2) full exemption, or (3) reward. If no one in a group reports the cartel information, the collusion is detected by the AA with a certain probability, and every member of the group suffers the full fine if their collusion is detected.\(^5\)

Although it is very important to investigate whether people would collude in the beginning under the leniency programs, the issue we deal with here is limited only to how the leniency program works under the situation where firms have already committed to colluding with each other.\(^6\) There are a vast number of experimental studies on the prisoners' dilemma. Whether people are cooperative or not in the game is not the issue we deal with here.

The experimental results showed that (1) the large size cartel (7-person group) is more easily dissolved than the small size cartel (2-person group) under the leniency program; (2) the fine schedule (all reporters can get leniency or only the first reporter can) does not affect the likelihood of cartel continuation; (3) positive enforcement, such as giving a reward to reporters (courageous leniency program), has a greater impact on dissolving cartel activities.

The organization of the paper is as follows. The theoretical model we used in our experiment is explained in the next section. Our experimental design and procedures are explained in Section 3, and

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\(^2\) See Monti (2002).

\(^3\) EC’s Competition Policy Newsletter, Autumn 2005.

\(^4\) See Takeshima (2007).

\(^5\) We assume that the AA can verify the cartel information with certainty, if the firm they investigate is involved in a cartel.

\(^6\) This is the situation where each cartel member has been locked in a cartel, so they do not doubt whether other members are colluding with their firm or not. But they can decide whether to report their crime to get some immunity from the authority. By controlling the starting strength of the cartels, we can genuinely focus on the impact of the leniency program on hardcore cartels.
experimental results are discussed in Section 4. Finally, conclusions are given in Section 5.

2. Model

The theories on leniency programs are usually based on the prisoners’ dilemma game (e.g. Motta and Polo, 2003). The structure of this game is shown in Table 1.

Once a leniency program is introduced, one more stage is added to the prisoners’ dilemma game. Assuming that players commit to a cartel with certainty initially, we use the following two-stage game as a baseline game. It represents a simple oligopoly market.

Stage 1. Players collude with each other (choosing the “Cooperate” strategy in Table 1).

Stage 2. Without knowing whether the AA will investigate the players, they decide whether or not to report about their Stage 1 collusion to the AA. They make this decision independently and simultaneously without communication. Players only know the probability of investigation, $p (0 < p < 1)$.

Fig. 1 shows the game structure for the 2-person case in which a reduced fine is given to all reporters.

The incentive structure for the three degrees of leniency is as follows. If both players do not report to the AA in the second stage and the AA did not happen to investigate them, then each player can enjoy the cartel profits ($\pi_C$) for the round. If both players do not report but the AA investigates the players, then both players suffer a full fine ($F$) for the round. On the other hand, if a player deviated in the second

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<table>
<thead>
<tr>
<th>Player 1</th>
<th>Player 2</th>
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<tr>
<td>Cooperate</td>
<td>(\pi_C, \pi_C)</td>
</tr>
<tr>
<td>Defect</td>
<td>(\pi_D, \pi_D)</td>
</tr>
</tbody>
</table>
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Note 1: $\pi_D > \pi_C > \pi_{CD} > \pi_{DC}$.

Note 2: In the experiment, $\pi_{DC} = 60, \pi_C = 40, \pi_D = 20, \pi_{CD} = 10$. However, $\pi_{DC}$ and $\pi_{CD}$ were never realized since we did not allow subjects to deviate in Stage 1.

This game is repeated with probability of $\delta$ ($0 < \delta < 1$).

Fig. 1. The game tree for the case for immunity for all (2-person case).
stage (reported to the AA), that player can receive partial or full exemption from a fine (reduced fine = R (−F)), or get a reward (R'), while the other non-reporting player suffers the full fine for the round. The game is repeated with probability \(\delta (0 < \delta < 1)\), and the probability is high enough that maintaining collusion is profitable to players. If players can successfully maintain collusion (by not reporting) and the AA does not detect the collusion, then they go back to Stage 1 to commit collusion again and proceed to Stage 2 to decide whether to report or not. However, once the collusion is detected by the AA, or once at least one player deviated in the second stage, both players cannot collude anymore in the later rounds. They fall under the AA’s control, and are forced to choose to defect in the first stage in all the later rounds. They get only a competitive profit \((\pi_0)\) which is strictly lower than the cartel profits \((\pi_\text{C})\) in every later round.

In the reward case, the reward \(R\) for a reporting firm is a fraction of fines collected from not-reporting firms. The sum of fines collected from not-reporting firms is distributed to reporting firms, that is, 
\[
R = \frac{F \times N_{\text{R}}}{N_{\text{R}}},
\]
where \(N_{\text{R}}\) is the number of players who did not report, and \(N_{\text{R}}\) is the number of players who reported, and \(\alpha\) is a constant parameter.

Let us consider the incentive conditions for sustaining the collusion.

As we explained above, we assume that both players employ the following trigger strategy: each player maintains collusion (not reporting) as long as the other player does so. If one player deviates from the collusion (by reporting), the other player will never collude with the player again. Based on this trigger strategy, we can calculate the discounted payoffs for the not-reporting strategy and the reporting strategy in the standard repeated game analysis. In the following discussion, we examine whether a player has an incentive for unilateral deviation when his partner chooses the strategy of not reporting.

(1) The expected payoff for the not-reporting strategy \((\pi_{\text{NR}})\)

In this case, a player \(i\) does not defect from the collusion. However, if the pair is investigated by the AA (with probability \(p\)), both players suffer the full fine \(F\) and they can not collude again in all the rounds thereafter. On the other hand, if the pair is not investigated by the AA (with probability \((1-p)\)), they can continue to collude in the next round, too. Therefore, the expected payoff for this strategy with the discount factor \((\delta)\) of (probability of repetition of the game, \(\delta (0 < \delta < 1)\)) and \(p\) is as follows.

\[
\pi_{\text{NR}} = p \left[ (\pi_{\text{C}} - F) + \delta \pi_0 + \delta^2 \pi_0 + \cdots \right] + (1-p) \cdot \delta \pi_{\text{NR}}
\]

Rearranging,

\[
\pi_{\text{NR}} = \frac{p \pi_{\text{C}} - F + \delta \pi_0 + \delta^2 \pi_0 + \cdots}{1-\delta} + \frac{1-p}{1 - \delta} \pi_{\text{C}}
\]

(2) The expected payoff for the reporting strategy \((\pi_\text{R})\)

In this case, a player defects in the second stage. The defecting player receives a reduced fine \(R\) or a reward \(R'\), while the other player suffers the full fine \(F\). The pair can not collude in all the rounds thereafter. The expected payoff for the reduced fine case is as follows.

\[
\pi_{\text{R}} = (\pi_{\text{C}} - R) + \delta \pi_0 + \delta^2 \pi_0 + \cdots = (\pi_{\text{C}} - R) + \frac{\delta \pi_0}{1-\delta}
\]

From the calculations above, \(\pi_{\text{NR}} \geq \pi_{\text{R}}\) is the necessary and sufficient condition for each player to sustain the collusion as an equilibrium (if they follow the trigger strategy explained above). In our experimental setting, the payoff from one-shot collusion \((\pi_{\text{C}})\) is 40 and the payoff from not colluding is 20 \((\pi_0)\). The fine \((F)\) is equal to \(\pi_{\text{C}} (\approx 40)\), the partially reduced fine \((R)\) is 5, the discount factor \((\delta)\) is equal to 0.8, and the probability of investigation \((p)\) is 0.1, so \(\pi_{\text{NR}}\) is set equal to 157.14 and \(\pi_{\text{R}}\) is set equal to 115 for the case that the reduced fine is given to reporting firms. Obviously when a reporter gets full immunity from fine, \(\pi_{\text{NR}}\) is still slightly higher than \(\pi_{\text{R}}\) (120). For the case when the reward is given to reporting firms, \(\pi_{\text{NR}}\) is the same as in the reduced fine case (157.14) (since nobody gets any reward). The expected payoff for the reporting strategy is

\[
\pi_{\text{R}} = \left( \pi_{\text{C}} + \frac{F \times N_{\text{R}}}{N_{\text{R}}} \right) + \delta \pi_0 + \delta^2 \pi_0 + \cdots
\]

set equal to 144, where \(\alpha\) is 0.6 for the 2-person case. Therefore, firms have an incentive to maintain collusion in all these cases, and all of the leniency programs might be too weak to dissolve cartels.\(^7\)

These incentive conditions cannot exclude other equilibria. Our game can be reduced to a kind of stag-hunt game in which both the reporting strategy and the not-reporting strategy are equilibria. We set payoffs for the repeated game so that for both players not reporting is the payoff-dominant equilibrium.\(^8\) Therefore, we make the following hypothesis:

**Hypothesis 1.** None of these leniency programs can deter cartels at all.

Since only unilateral deviation from equilibrium is considered according to the Nash equilibrium concept, these conditions can be applied not only to the game which consists of two players but also to the game which consists of more than two players. Therefore, we make the following hypothesis.

**Hypothesis 2.** The not-reporting strategy is observed dominantly in both the two-person case and in the seven-person case.

Furthermore, the same equilibrium conditions can be applied to the case where leniency is limited to only the first reporting player. To pursue such an institutional design issue, we compared different fine schedules. One schedule is that only the first reporter can get a reduced fine or a reward. The other schedule is that all reporters can get the fully or partially reduced fine or the reward. We make the third null hypothesis as follows.

**Hypothesis 3.** The rate of collusion groups is close to 1 in both fine schedule cases (the first reporter is given leniency or all reporters are given leniency).\(^9\)

Although the difference of expected payoffs between the reporting strategy and the not-reporting strategy is smaller in the reward case than in the reduced fine case, the expected payoff for the not-reporting strategy is strictly higher than the one for the reporting strategy in both cases. Therefore, we make the fourth hypothesis:

**Hypothesis 4.** The frequency of collusion is close to one regardless of the degree of leniency.

However, the hypotheses above seem to be too tight and extreme. One may expect that the larger the number of colluding members, the

\(^7\) In the reward case, \(\alpha\) is set equal to 0.6 for two-person cases and 0.1 for seven-person cases so that the maximum reward for an individual firm is equal to the two-person and seven-person cases. A player can get a reward of at most 24 points in the seven-person case (=0.1×40×6/1), when the six other cartel members decided not to report.

\(^8\) When the fine is partially reduced \((8\times5)\), all players never reporting is a risk-dominant equilibrium as well. However, when the fine is fully reduced and even a reward is given to a reporter, the equilibrium is no longer risk dominant according to our parameter settings.

\(^9\) However, we can imagine a counter hypothesis to Hypothesis 3. The deviation from the collusion could be accelerated if only the first reporter can get a reduced fine or a reward because each player may rush to get them before the others can. The more uncertain subjects feel about other people’s rationality, the more this phenomenon would be observed.
larger the probability that at least one member of the group will deviate from the collusion under a leniency program (even if such a probability for each member is small). The equilibrium prediction above is based on the assumption that players can calculate the expected utility of each strategy correctly (without any bias), and they would follow the same logic to select the not-reporting equilibrium as the most plausible one. However, this assumption is not guaranteed in the laboratory.

We have a conjecture that if subjects believe that the other subjects are risk averse (too afraid of the prospect of loss in the case that their partner(s) cheat on them), they would not count perfectly on the idea that every other subject would focus on the not-reporting strategy with probability 1. Therefore, some other hypothesis is necessary in this case. We introduce the parameter setting as the degree of risk aversion, represented as \( \gamma \). This parameter becomes even bigger as the group size gets larger.

Suppose a group consists of \( n \) members and each player anticipates that they are matched with a player who chooses the not-reporting strategy with probability \( \gamma (0 \leq \gamma < 1) \). Then, with probability \( \gamma^{n-1} \), a player who chooses the not-reporting strategy will face the situation in which all the other players also choose the not-reporting strategy and can maintain the cartel. If at least one player deviates from the not-reporting strategy, this will occur with probability \( 1 - \gamma^{n-1} \). In this case, the player who chooses the not-reporting strategy should pay \( F \) and he can no longer form a cartel. Then, the expected payoff of that player for choosing the not-reporting strategy becomes,

\[
\pi_{NR} = \frac{1}{1-\gamma^{n-1}} \left( \gamma^{n-1} p + (1-\gamma^{n-1}) \right) \left( \frac{\partial \pi}{\partial \gamma} + \frac{\partial \pi}{\partial \theta} + \frac{\partial \pi}{\partial \gamma} \right) + \gamma^{n-1} (1-p) \pi_c
\]

This is a deceasing function of \( n \) since \( 0 < \delta, \gamma < 1 \) and \( \pi_c < \pi_n \) in our parameter settings. That is, the larger the number of players involved in the game, the less incentive the player has for choosing the not-reporting strategy. To be more specific, \( \gamma^{n-1} > 0.74786 \) is a necessary and sufficient condition for the not-reporting strategy to be the best response since \( \pi_c = 115 \) and \( \pi_{NR} = 10^{-\gamma^{n-1}} \) according to our parameter settings and Eqs. (2) and (4).

Given a large enough \( \gamma \), namely, a subject’s belief about the other subjects’ behavior is close to the one in the equilibrium, the not-reporting strategy is still the best response with respect to the player’s non-equilibrium belief \( \gamma \). In other words, \( \gamma \) has to become higher as the group size \( n \) becomes bigger to satisfy the condition \( \gamma^{n-1} > 0.74786 \). This means that members of a large size cartel must trust other members more strongly than in a smaller size cartel, if they have an incentive to maintain collusion. Therefore, we raise the following behavioral hypothesis.

**Hypothesis 5.** In the 7-person case, there are fewer groups which manage to keep collusion than in the 2-person case.

Based on the theoretical model and these hypotheses, we conducted a series of experiments. The details of the experiments are explained in the next section.

### 3. Experimental design and procedure

Experiments were conducted at Kyoto Sangyo University and Future University-Hakodate in 2004, 2005, and 2007. Subjects were recruited from among various majors. At Kyoto Sangyo University, subjects were recruited from the economics, business, law, foreign languages, cultural studies, engineering, and science departments. All subjects from Future University-Hakodate were computer science majors. We ran 17 sessions in total. Table 2 summarizes all the session details.

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<th>Leniency type</th>
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<td>All 1st</td>
</tr>
<tr>
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<td>Hokkaido</td>
<td>28</td>
<td>Reward</td>
<td>All 1st</td>
</tr>
<tr>
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<td>Hokkaido</td>
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<td>Reward</td>
<td>All 1st</td>
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<tr>
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<td>28</td>
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When the reward is introduced, the condition becomes stricter and \( \gamma \) has to be higher than approximately \( 0.94562 \) according to Eq. (3).
PARTIAL, the treatment with full reduction of fine, FULL, and the treatment with reward, REWARD.

17 sessions were run in total. The experimental procedures were programmed and conducted on z-Tree (Fischbacher, 1999) with computers with a network connection.

Except for the sessions for NO LENIENCY, each session consisted of two treatments in sequence and included some sort of leniency program. In most of those sessions, the first treatment was ALL, and the second treatment was ONE. To see the order effect, we reversed the treatment order for some FULL and REWARD sessions (see Table 2). In each treatment, subjects played with the same partner(s) for five trials in sequence.

To make our subjects understand that sustaining a cartel is the most profitable for them, they experienced the mutually cooperative outcome through a prisoners' dilemma game before the real experiment started. In this practice treatment, they could choose only cooperation for four to eight rounds.11 In addition to that, subjects practiced clicking their mouses according to the experimenter's directions to get used to how to manipulate the computers and how to understand the information shown on the screen for their decision making. They were not allowed to make any free decisions until the actual round started.

At the beginning of all treatments, each subject is asked to make a decision for Stage 1. There are two radio buttons (A or B) on the screen (see Appendix). A means collusion and B means defection. Subjects are restricted to choosing A. All subjects were restricted to colluding in Stage 1 in all treatments since we intentionally wanted to create a situation in which subjects committed to colluding with each other from the beginning.12 When all group members choose A, the computer screen in each booth shows the result of Stage 1 confirming that other member(s) of his or her group also chose A. Upon knowing that everybody in his or her group committed to colluding, they move on to Stage 2 to decide whether to deviate from the cartel. They see the computer screen asking to click one of two radio buttons, C or D. C means not to deviate (i.e. not to report) and D means to deviate (i.e. report). In other words, subjects know that when they choose C (and do not draw a payoff reduction lottery (= investigated by the AA with the probability of 0.1)), they can receive 40 points each. If they happen to draw a payoff reduction lottery, however, they get nothing for the round.

When at least one of them chooses D, on the other hand, all the people who chose D can receive leniency (in ALL treatments), the other member(s) get nothing for that round, and the group can never go back to choosing A in Stage 1 in later rounds. (In the ONE treatment, only the first subject to choose D can receive leniency for that round, while all the others get nothing. In the NO LENIENCY treatment, even if a subject chose D in Stage 1, there is no leniency for him or her.) In the case in which at least one member of the group chose D in Stage 2, all the members of the group are forced to choose B in later rounds, which means they get only 20 points per round until the end of the trial. This also happens in the case of drawing a payoff reduction lottery. When all the members of a group have finished making a decision for Stage 2, they see the result of Stage 2.13

At the end of each round, individual decisions of intra-group members were revealed to each player, plus whether the collusion activity in their group (choosing A in Stage 1) was found by the AA or not. Subjects could tell who deviated (chose D in Stage 2) at the end of a round. However, the identities of subjects and where they sat were kept confidential to guarantee anonymity among subjects.14 If everybody in the group chose C and the group was lucky not to get a payoff reduction lottery, then they go back to the screen for Stage 1 to continue the guaranteed collusion again (choose A again), and can move on to Stage 2. However, if the group members chose C in Stage 2, and they got a payoff reduction lottery, then all the members get nothing (paying the full fine (+40)). They will then be restricted to choosing B in Stage 2 in the subsequent rounds as long as the session is continued (+ with the probability of 0.8). There will be no decision making for Stage 2 since their collusion had been dissolved.

In addition to the fine schedule (ONE or ALL), there are three degrees of leniency. When the fine schedule is ONE, only the first

---

11 This practice treatment was not run in NO LENIENCY since in these treatments it is very easy to understand that maintaining the collusion is the most profitable. In the Hakodate sessions, the number of practice rounds was limited to four because of time constraints.

12 The reason why we did this is explained in footnote 6 in Section 1.

---

Table 3

The number of rounds in each session

<table>
<thead>
<tr>
<th>Date</th>
<th>Trial 1</th>
<th>Trial 2</th>
<th>Trial 3</th>
<th>Trial 4</th>
<th>Trial 5</th>
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<table>
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</tr>
<tr>
<td>2005/06/17</td>
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</tbody>
</table>
is shown in detail in Table 3.16 probability of 0.1. The number of rounds in each trial for each session treatments, each group was investigated by the AA with the number of repetitive rounds was not known beforehand. In all results of the treatments would be paid by choosing one of them by going to experience two treatments (ONE and ALL) and only one of the instructions for the case of ALL (2-person treatment, PARTIAL) are and rules of the experiment common knowledge among subjects. The instructions to the subjects and read them aloud to make all the parameters subjects during sessions. The instructor distributed written instructions to the subjects and made direct contact, i.e., by talking or making eye contact, with other sides of the desk in the laboratories. It was impossible for them to randomly assigned to a booth with partitions in front and on both members who chose D can receive leniency or a reward (in PARTIAL, the exemption is 35; in FULL, the exemption is 40; in REWARD, the exemption is 40 or more, depending on the number of members who defected in Stage 2).

The game depicted in Fig. 1 explains the above procedure for the 2 person case with PARTIAL, as an example. The game was repeated with a probability of 0.8 within each trial. Within each trial, the number of repetitive rounds was not known beforehand. In all treatments, each group was investigated by the AA with the probability of 0.1. The number of rounds in each trial for each session is shown in detail in Table 3.16

No subject participated in more than one session. Subjects were randomly assigned to a booth with partitions in front and on both sides of the desk in the laboratories. It was impossible for them to make direct contact, i.e., by talking or making eye contact, with other subjects during sessions. The instructor distributed written instructions to the subjects and read them aloud to make all the parameters and rules of the experiment common knowledge among subjects. The instructions for the case of ALL (2-person treatment, PARTIAL) are provided in the Appendix.

At the beginning of each session, subjects were told that they were going to experience two treatments (ONE and ALL) and only one of the results of the treatments would be paid by choosing one of them by lottery at the end of the course of the treatments. The experimenter read the instructions for each treatment at the beginning of each treatment, so subjects were not aware of the details of each treatment until just before the treatment began. Therefore, there was no incentive for subjects to sacrifice their profits in one treatment in order to make higher profits in a later treatment.

We did not allow subjects to have any communication before making their decisions. When cartel members decide whether to apply for the leniency program, they make the decision secretly from other cartel members. Therefore, we simply made subjects make their decisions without communication.

All sessions lasted about 2 h. Subjects were paid individually in cash according to their experimental results. During the experiments, subjects’ earnings were represented by points. They were told in the instructions that one point would be exchanged for five yen at the end of the experiment. The average payment for subjects in the two-person group experiment was 5014 yen (about 43 US dollars, $1=116 yen), and the average payment in the seven-person group experiment was 3232 yen (about 29 US dollars).

4. Results

4.1. Group-level analysis

Tables 4–5 summarize the average ratio of collusive groups in all treatments for the first rounds and the last rounds respectively.

15 The first round was realized with certainty.
16 We ran sessions for PARTIAL using a random stopping rule, so that the computer terminated each trial with the probability of 0.2. However, we realized that this would make the data unbalanced. Therefore, we ran sessions for FULL and REWARD with the same number of rounds in each trial, eliminating the random stopping rule. We set the number of rounds to equal those chosen by computer in PARTIAL sessions. We had to set the composition of rounds differently between ONE and ALL so that each subject could not guess the length of a trial in the second treatment.

17 Subjects were paid for the 8-round practice treatment (see footnote 7) as a show-up fee in all the treatments except for NO LENIENCY sessions. The treatments for NO LENIENCY were actually run first before 2 or 3 other kinds of treatments (which are not explained in this paper). Subjects in these treatments were paid for the results by chance (lottery).
18 The calculation of the average payments includes a show-up fee (=800 yen).
Figs. 2–5 show the time series data for the average ratio of collusive groups which sustained collusion during the treatments. They are (2-person, ONE), (2-person, ALL), (7-person, ONE), and (7-person, ALL) respectively. The figures compare the impact of the degree of leniency programs among NO LENIENCY, PARTIAL, FULL, and REWARD. The graphs depict only the ratio of groups which did not dissolve their cartels voluntarily, including the case in which none of the members of a group defected in Stage 2 but happened to get a payoff reduction lottery.\textsuperscript{19}

Each graph includes a line representing NO LENIENCY as the benchmark case. This line shows that no one was willing to dissolve cartels in both the 2-person case and the 7-person case. This indicates that subjects understood that there is no incentive to dissolve cartels in the treatment. The result implies that members of hardcore cartels, who do not doubt about their group members collusion, do not break up their cartels voluntarily when there is no leniency program (and when the frequency of the authority’s monitoring is low).

2-person treatment results (Figs. 2 and 3) show that the impact of leniency programs varies among PARTIAL, FULL, and REWARD. In most of the rounds, more than 50% of the groups tried to maintain their cartel in those treatments. The results also show that the impact of PARTIAL and FULL do not seem significantly different, while REWARD seems to promote more players to dissolve cartels voluntarily than PARTIAL and FULL as the experiments proceeded. 7-person treatment results (Figs. 4 and 5) show that most groups rarely managed to maintain cartels in all the treatments with leniency. The result indicates obviously that Hypothesis 1 is rejected.

To verify these observations and evaluate the hypotheses described in Section 2, we estimated a random effects logistic regression model using the data of PARTIAL, FULL, and REWARD. Whereas in Tables 4–5 and Figs. 2–5 we focus on the degree of successful collusion, in the following discussion we focus on the degree of cartel dissolution as the

\textsuperscript{19} Because of the experiment design, once a group luckily got a payoff reduction lottery (with the probability of 0.1), they were forced not to collude in later rounds. We simply assumed that those groups would have kept colluding if they were lucky.
dependent variable. The estimations were performed using Stata (version 8).

\[ \Pr(R = 1) = G(\beta_0 + \beta_1 \text{ONEorALL}_i + \beta_2 \text{Groupsize}_i + \beta_3 \text{Round}_t + \beta_4 \text{1stRound}_t + \beta_5 \text{Order}_t + \beta_6 \text{FULL}_i + \beta_7 \text{REWARD}_i + \text{Groupsize}_i(\beta_2 \text{ONEorALL}_i + \beta_3 \text{FULL}_i + \beta_4 \text{REWARD}_i) + \epsilon_i + \eta_t) \]  

\( G \) is the logistic cumulative distribution function. The estimated coefficients and other statistical information are shown in Table 6. In the above equation, \( i \) denotes the group and \( t \) denotes the round. Since subjects could interact with other subjects within their group, we employed group-level data in the analysis. \( R \) is a response variable that equals 1 when a cartel was dissolved voluntarily and 0 if the cartel was not dissolved voluntarily.

The variable \( \text{ONEorALL}_i \) is a dummy variable, which equals 1 for ALL and 0 for ONE; it changes across \( i \) and \( t \). The variable \( \text{Groupsize}_i \) is also a dummy variable, which is 1 for the seven-person group and 0 for the two-person group; it changes across \( i \) but does not change across \( t \), which is why it has no \( t \) subscript. The variable \( \text{Round}_t \) consists of \( \{1, 2, \ldots, 16\} \). The variable \( \text{1stRound}_t \) is a dummy variable that equals 1 when \( t = 1 \) and 0 otherwise; it does not change across \( i \), which is why it has no \( i \) subscript. The dummy variable \( \text{Order}_t \) is a dummy variable which equals 1 when \( t = 1, 2, \ldots, 8 \) (rounds for the first treatment) and 0 when \( t = 9, 10, \ldots, 16 \) (rounds for the second treatment); it does not change across \( i \). The variable \( \text{FULL}_i \) is a dummy variable which equals 1 for FULL and 0 for PARTIAL and REWARD; it does not change across \( t \), which is why it has no \( t \) subscript. The variable \( \text{REWARD}_i \) is a dummy variable which equals 1 for REWARD and 0 for PARTIAL and FULL; it does not have \( t \) subscript by the same reason for \( \text{FULL}_i \). Since group size was the same during each session, it can interact with \( \text{ONEorALL}_i, \text{FULL}_i, \text{REWARD}_i \). The term with \( \text{Groupsize}_i \) in the brackets contains the interaction terms of those variables. We assume that the unobserved group heterogeneity effect \( \epsilon_i \) is independent of all explanatory variables in all rounds.

---

**Fig. 4.** The average rate of collusive groups (group size: 7-person; leniency is given to the only the first deviator).

**Fig. 5.** The average rate of collusive groups (group size: 7-person; leniency is given to all deviators).
Following the above rules, we chose the data from sixteen particular rounds, which were the first round and the round as close to the last round that was common to both ONE and ALL. For example, in trial 2, ONE lasted 10 rounds, but ALL was terminated at round 6. Therefore, the sixth round is the round that is closest to the last round and common to both treatments. The rounds chosen for analysis are: for both ONE and ALL, the first round from trial 1, the first and the sixth rounds from trial 2, the first and the third rounds from trial 3, the first round from trial 4, and the first and the third rounds from trial 5.

The reason why we did not use all the data is that there are two possible factors that may mislead the statistical results, if we were to do so. One is that each trial has a different number of rounds because the number of rounds is determined probabilistically, i.e., they depend on the probability \( \delta \). For example, most sessions of ALL have only one round in trial 1. On the other hand, most sessions of ONE have 8 rounds in trial 1. For comparison, the data of the first round is the only fair candidate to compare the data between ALL and ONE. There is not enough data to know how subjects would have behaved if trial 1 had lasted 8 rounds in ALL sessions. Therefore, we chose the data for the first round of each trial since all sessions of all treatments have data for the first round. The second reason is that we also wanted to analyze data from rounds other than the first round. It is important to capture the characteristics of each treatment regarding how many groups ended up dissolving their collusion by the end of each trial. However, we should not simply compare the result of the last round of each trial. The longer a trial lasted, the more subjects were able to interact with each other. If we simply compared the results of the last round, a treatment which has more rounds in a trial might be evaluated as having more groups that dissolved the collusions. This comparison is not fair. Therefore, we chose the data of round 6 for trial 2, round 3 for trial 3, and round 3 for trial 5. (Since both ALL and ONE had only one round for trial 1 and trial 4, we compared the data of only the first round for those trials.)

From Table 6, one can see that the coefficient of Groupsize, is significantly positive, which indicates that the rate of cartel dissolution is significantly higher in the 7-person case than in the 2-person case.

Hence, Hypothesis 2 is rejected, and Hypothesis 5 is accepted. The coefficient for OneorALLi is not significant, which means that the fine schedule does not have a strong impact on people's behavior under our experimental parameters. Hence, Hypothesis 3 is accepted. From this result, we can conclude that limiting the number of firms which can enjoy the leniency program does not have a significant impact on the ability of collusive firms to maintain their collusion. The coefficient for 1st_round, is significantly negative, which indicates that even if groups dissolved their cartels in the previous trial they recovered their cartels in the new round of the next trial (see Figs. 2–5). That is, there existed cartel recidivism even when a leniency program was introduced. The coefficient for FULLi is insignificant, which means that the impact of leniency programs does not differ between the partial leniency and the full leniency treatments. On the other hand, the coefficient for Rewardi is significantly positive, which indicates that positive enforcement

<table>
<thead>
<tr>
<th>Table 6</th>
<th>Random effects logistic regression on cartel dissolution using group data</th>
</tr>
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<tbody>
<tr>
<td>Dependent variable: cartel dissolution (if a group dissolved its cartel=1, otherwise=0)</td>
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Note: ONEorALLi: if the only first deviator is given leniency=1; if all deviators are given leniency=1.

Groupsizei: two-person group=0, seven-person group=1.

Roundi: Round index consisting of {1, 2, ..., 16}.

1st_roundi: a dummy variable that equals 1 when i=1 and 0 otherwise.

Orderi: the treatment which was run first=1, the treatment run next=0.

FULLi: treatment with full leniency=1, otherwise=0.

REWARDi: treatment with reward=1, otherwise=0.

Therefore, we employed the random effects model. 20 The error \( \epsilon_{it} \) is the idiosyncratic (time-varying) error.

Roundi consists of \{1, 2, ..., 16\} in sequence. There are more than 16 rounds in total in each session (see Table 3). The reason why we did not use the data from all of the rounds is that the data after a group's cartel was dissolved was fixed automatically as either "dissolved" or "not dissolved" in our analysis. For example, if at least one member of a group defected in Stage 2 (=their collusion was dissolved voluntarily), they could never go back to colluding in Stage 1 (by choosing A), but had to choose not to collude (by choosing B) in later rounds. The status of such a group was set as "dissolved" until the trial ended. On the other hand, suppose that none of the members of a group defected in Stage 2, but got a payoff reduction lottery during some round. In this case also, they could never go back to colluding in Stage 1 (by choosing A), but had to choose not to collude (by choosing B) in later rounds. The status of such a group was set as "not dissolved" until the trial ended. We counted this case as "not dissolved" for analysis since they could have maintained collusion if they had been lucky. Since the status of the group was set as "not dissolved" artificially, we wanted to avoid counting the data of the later rounds as real action by subjects.

Originally we wanted to choose the first and last rounds of each trial for analysis. However, some trials have only one round; therefore, we simply could not choose two rounds from each trial. To compare ONE and ALL fairly, we established the following set of rules for analysis:

1. Use the data of the first round from each trial in each session
2. Select the data of one more round which represents the final outcome of each trial
3. The additional round has to be common to both ONE and ALL for fair comparison
4. If there is only one round in a trial in either ONE or ALL, compare only the first rounds for the trial.

20 We also ran the fixed effects model, and the results were consistent with those of the random effects model for the variables we were able to compare (i.e. ONEorALLi, Roundi, 1st_roundi, Orderi, and Groupsizei × ONEorALLi, which are all time-varying variables).

21 Although some coefficients for the interaction terms are significant, they do not reflect the characteristics of their marginal effects on cartel dissolution sufficiently. The software package we used (Stata) does give outputs on coefficients, but we should be careful about how to interpret them because they do not mean the marginal effects of the interaction terms in terms of both their signs and their magnitudes. See Ai and Norton (2003) for more on the correct interpretation of coefficients of interaction terms.
such as giving a reward for a self-reporting firm in a courageous leniency program has a great impact on dissolving cartel activities.\\(^{23}\) Thus, Hypothesis 4 is rejected. The coefficient for Round is not significant. Subjects could have gained enough experience and could have become familiar with our experimental environment as trials proceeded. After eliminating the first round effect, however, there was no significant tendency for the number of cartels to change as time proceeded. The coefficient for Order, was also not significant, which implies that the treatment order does not significantly affect the subjects’ behavior.\\(^{24}\) The main results of our experiments are summarized below.

Result 1: Leniency programs are useful to deter cartels even if they are not theoretically strict enough to eliminate the collusive equilibrium. This tendency is more salient in the case of a large cartel group than in the case of a small cartel group.

Result 2: The not-reporting strategy was observed more frequently in the two-person case than in the seven-person case.

Result 3: The rate of cartel dissolution was not significantly different between the case in which only the first reporter can use the leniency program and the case in which all reporters are allowed to use the program.

Result 4: The frequency of collusion dissolution is significantly higher when the courageous leniency program, which gives rewards, is used.

Having seen that rewarding reporters has a strong impact on deterring cartels, we next estimate the change in the predicted probability as it goes from Reward\(_i=0\) to Reward\(_i=1\) by

\[
\hat{C} = \hat{X} \hat{B} + \beta_{\text{REWARD}} + \beta_{\text{GROUPSIZE} \times \text{REWARD}} - \hat{C} \hat{X} \hat{B}.
\]

\(X\) is the vector of the average values for other variables, and \(\hat{B}\) is the vector of their estimated coefficients in Table 6. \(\hat{B}_{\text{REWARD}}\) and \(\beta_{\text{GROUPSIZE} \times \text{REWARD}}\) are the estimated coefficients for REWARD, and Groupsize\(_i \times \text{REWARD}\), in Table 6, corresponding to \(\hat{B}_7\) and \(\hat{B}_{10}\) in Eq. (5) respectively. The effect of reward was approximately 0.6423 (standard error = 0.057, \(p < 0.001\)). Therefore, about 64\% more cartels were dissolved by the leniency program with reward than the average level of other leniency programs without it in our experimental setting.\\(^{25}\)

### 4.2. Individual level analysis

By looking only at group-level data, we might have neglected some significant effect of leniency programs at the individual level. In addition, the insignificance of the effect of the fine schedule (ONE and ALL) may be due to individual subject social background characteristics (e.g., gender).

To see how robust the statistical result using group data is, we also analyzed individual level data including the gender information about each subject. Although most of the individual data is not independent since subjects interact with each other in the course of the experiments, we can use the data for the first rounds, before subjects interact. Strictly speaking, even the data of the first rounds in trials 2–5 are not independent, since subjects made decisions under the influence of the result of trial 1. However, Figs. 2–5 show that subjects seem to reset the result of the previous trial and recover their collusions in the first round of each trial. Therefore, we used the data of the first round of all the five trials. We employed the following random effects logistic regression model using the individual data of the first rounds among PARTIAL, FULL, and REWARD.\\(^{26}\)

\[
Pr(R(t) = 1) = H[\beta_0 + \beta_1 \text{ONEorALL}_i + \beta_2 \text{Groupsize}_i + \beta_3 \text{Trial}_i + \beta_4 \text{Order}_i + \beta_5 \text{Gender}_i + \beta_6 \text{Full}_i + \beta_7 \text{REWARD}_i + \text{Groupsize}_i \times \text{ONEorALL}_i + \beta_8 \text{Groupsize}_i \times \text{REWARD}_i + \text{Gender}_i \times \text{Full}_i + \text{Gender}_i \times \text{REWARD}_i + \text{Constant}_i + u_i].
\]

Similar to Eq. (5), \(H\) is the logistic cumulative distribution function. The estimated coefficients and other statistical information are shown in Table 7. In the above equation, \(i\) denotes the subject and \(t\) denotes the trial. \(R\) is a response variable that equals 1 when a subject dissolved their cartel voluntarily (by choosing D) and 0 if a subject did not dissolve the cartel voluntarily (by choosing C) in the first rounds.

The meanings of most explanatory variables are similar to those in Eq. (5). The variable Trial, consists of \(\{1, 2, \ldots, 10\}\). Since subjects went through 10 trials till the end of the session (5 trials each of ONE and ALL), there are 10 samples of the first rounds for each subject. The variable Order, is a dummy variable which equals 1 when \(t = 1, 2, \ldots, 5\) (the five first rounds for the first treatment) and 0 when \(t = 6, 7, \ldots, 10\) (the five first rounds for the next treatment). The variable Gender, is a dummy variable which equals 1 if a subject is male and 0 if a subject is female. We assume that the unobserved subject heterogeneity effect \(u_i\) is independent of all explanatory variables in all trials. Therefore, we employed the random effects model. The error \(u_i\) is the idiosyncratic (time-varying) error.

From Table 7, one can see that the coefficients for Constant, Groupsize\(_i\), Trial\(_i\), Order\(_i\), Gender\(_i\), REWARD\(_i\), are significant (\(p < 0.05\)). Similar to the result of the group-level data, the coefficient ONEorALL\(_i\) is not significant. The result means that the fine schedule does not

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23 We also compared FULL and REWARD separately. Significantly more cartels were voluntarily dissolved in REWARD than FULL (\(p < 0.001\)).

24 This result justifies combining the data from sessions in which ALL was run first and sessions in which ONE was run first.

25 The probability of the change could differ depending on which point we evaluate it from, and what kind of cumulative distribution function is employed. Although we can modestly say that a leniency program with reward has a great impact on cartel dissolution, the magnitude of this estimated probability should not be overstated.

26 We ran the fixed effects model for individual data, as well. The results were also consistent with those of the random effects model for the variables we were able to compare (i.e. ONEorALL\(_i\), Trial\(_i\), Order\(_i\), and Groupsize\(_i \times \) ONEorALL\(_i\), which are all time-varying variables).
have a strong impact even at the individual level. The coefficient of Groupsize, is significantly positive. The result indicates that the rate of applying for the leniency program is significantly higher in the 7-person case than in the 2-person case, confirming the group-level analysis. The coefficient for Trial, is significantly positive, which indicates that subjects dissolved their cartels more voluntarily as the experiment proceeded. However, this tendency is not strong enough in the analysis for the group-level data. The coefficient for Order, is significantly positive, which indicates that subjects dissolved cartels more in the first treatment than in the following treatment. This result implies that people would not necessarily give up colluding even if they experienced their cartels being dissolved by leniency programs before. The coefficient for Gender, is significantly negative, which indicates that fewer men dissolved their cartels than women. The coefficient for REWARD, is significantly positive, which is similar to the group-level analysis.27 The most interesting result from the above analysis is that the effect of the fine schedule (ONEorALLa) is still insignificant even after eliminating social background bias such as gender. That is, people do not change their behavior according to the fine schedule. The subjects’ behavior is more influenced by group size and the high reward.28

5. Conclusions

We studied several kinds of leniency programs through laboratory experiments. It is expected that the larger the group, the larger the probability of cartel dissolution will be. In addition, the deviation from collusion could be accelerated if only one firm is given a reduced fine. Based on the predictions above, under a simplified oligopoly market, we experimentally controlled the following three factors to compare several institutional designs of leniency programs; (1) group size: the number of members in a group, small group (two members) or large group (seven members), (2) fine schedule: only the first reporter of cartel information is given leniency, or all reporters are given leniency, and (3) degree of leniency: a partially reduced fine, a fully reduced fine, or a reward is given to self-reporting firm.

The experimental results showed that (1) the larger the number of cartel members in a group, the weaker their ability to maintain the collusion is, (2) changing the fine schedule does not have a significant impact on firms’ ability to maintain collusion (limiting the number of firms which can enjoy leniency does not make people rush to dissolve their collusion by reporting), and (3) positive enforcement, such giving a reward for a self-reporting firm in a courageous leniency program, has a great impact on dissolving cartel activities. That is, Spagnolo’s prediction that the AA should give rewards to firms to deter cartels was proved experimentally to be correct.29,30

Comparing our results with those of Apesteguia et al. (2007) and Hinloopen and Soetevent (2008), our findings complement and share some insights with their results.

In the result of the “Bonus” treatment in Apesteguia et al. (2007), which gives rewards to defectors based on the fines collected from non-defectors as in our REWARD treatment, they found that more cartels were formed and reported in the Bonus treatment than in other treatments without rewards. In their interpretation, subjects were attracted by the possibility of reward and therefore established collusions in order to receive the reward. (There was no reward for non-defectors.) They also pointed out that their one-shot game structure precluded the possibility of subjects learning not to enter cartels in the first place. They conjectured that if the game had been repeated for more than one round, subjects might have recognized that cartels are full of cheaters and that entering a cartel is not a rational choice. This conjecture might be optimistic. Although our experiment did not give subjects the option to collude or not in the first stage (they were forced to collude), our results across trials imply that subjects might still form cartels in a repeated game setting. Similar to the results of Apesteguia et al., our results also show that rewarding drastically increases the number of reporting firms. However, in the beginning of each trial of the REWARD treatment, most groups recovered their collusion almost to the level of the first round of the previous trial. If a leniency program with rewards strongly discourages cartel members to maintain collusion with each other, we should not have observed that subjects recovered their collusion level at the beginning of later trials. This implies that to receive a high reward, a player needs to make his partner(s) to believe that he is willing to collude in the first place. And then, the player will cheat others to collect high rewards before the others cheat him. That might be why subjects maintained collusion at the beginning of each trial in the reward treatment and then rushed to defect with the aim of receiving high rewards. That is, even a leniency program with rewards may still trigger the formation of new cartels or cartel recidivism in the short term. Furthermore, subjects might even consider rotating the receiver of the reward in the repeated game setting.

Hinloopen and Soetevent’s paper focused exclusively on the leniency policy that gives full immunity to the first applicant and a 50% reduction to the second applicant. Our FULL treatment with 2-person groups is the closest condition to their experimental setting. Our results seem much more collusive than theirs. There are many possible reasons for this difference since the experimental designs have many different aspects. We suspect the main reason is that their subjects had more strategies for cheating other subjects. In their experiments, subjects had only binary choices (report or not); therefore, each subject had only two extreme choices: collude or not collude. On the other hand, subjects in Hinloopen and Soetevent’s experiment could choose a price from 100 to 110. Other than the competitive price (=101), any other price choice is considered somewhat collusive in their interpretation. Even if subjects agreed on a high price range, such as {103, 110}, subjects are afraid of bidding a relatively high price because they might be cheated by other players who could bid a slightly lower price. This way of thinking leads subjects to bid a low price even when they were allowed pre-play communication.

We can provide several policy implications from our experimental findings. The average size of a cartel in the real world consists of about six firms.31 Therefore, the seven-player case in our experiments nearly corresponds to the real-world situation. We found

27 Although we had 112 subjects for the 7-person with REWARD treatment in total, we could collect the gender information from only 56 subjects. We did not collect the questionnaire on gender for the sessions on 2005/6/16 and the two sessions of 2005/6/17. Since we included Gender as an explanatory variable, the data which does not have the gender information is omitted.

28 The sign of coefficient for Groupsize, × REWARD, differs between the group data analysis and the individual data analysis (Tables 6 and 7). This puzzle was pointed out by the anonymous referee. We checked this contradiction by estimating the probability change from REWARD = 0 to REWARD = 1 separately for the 2-person group data and for the 7-person group data. It confirmed that the probability change is smaller in the 7-person case, which is consistent with the results represented in Table 4 and the result for the individual data in Table 7. The puzzle may have been created because of the difference between coefficients for interaction terms and their marginal effects, which we mentioned in footnote 21.

29 However, it is hard to believe that any society would be willing to introduce a system that gives criminals some reward for their confessions. Korea’s leniency program gives a reward to an “external” whistle blower.

30 Spagnolo (2000) stresses that the most effective leniency programs are those that give more than a full reduction to the first applicant and none to subsequent ones. Our experiment is partly in line with Spagnolo’s ideas.

31 Leniency programs set up in the EU in 1996 achieved some notable success in prosecuting cartels (see European Union’s Official Journal Legislation (OJL), 1998/121-2003/12/16). The data of 31 cartels prosecuted between January 21, 1998 and December 16, 2003 demonstrates that the average number of firms forming a cartel is about six, and by applying leniency programs the fines for cartel members are reduced by 10% to 100% according to evidence brought to AA.
that under the leniency programs, most seven-member groups easily terminated their collusion, while none of the groups dissolved their cartels at all when they were not given any leniency for self-reporting. Therefore, we can predict that the leniency program could be fairly effective for regular size cartel groups in reality. However, such a power must be relatively weak when a cartel group consists of only a few members.

Motta (2004) discusses the effect of cartel group size on collusion in a different setting from the one we created through our experimental design. He argues that when a market is dominated by only a few firms and they collude with each other, the immediate gain a firm can get by deviation (e.g. lowering a price) does not expand drastically since each firm already has a significant share of the market. Therefore, the immediate gain from reporting cannot attract those firms to dissolve their collusion voluntarily since the long run gain from collusion would be much higher. However, if a market is dominated by many firms and they collude with each other, each firm is strongly tempted to deviate from collusion since a firm can gain a drastically larger profit by deviating. That is, when a market consists of many firms, the immediate gain from deviation is much higher than the long run gain because the deviating firm can potentially get the whole market share temporarily. In experiment, the immediate gain from deviation and the long run gain from collusion do not differ in the two-person group case and the seven-person group case. However, the collusion in the latter case was rarely successful. We predict that it would be even more difficult for a large group to maintain a successful collusion in a market like the one Motta discusses.

Huck et al. (2004) analyze how the number of firms affects the level of competition in Cournot market experiments. They showed that a market with two firms can be collusive, while a market consisting of more than three firms is too competitive for the firms to form collusion. Their findings are consistent with our results.

The question of how many firms should be given a reduced fine or a reward is an issue to be discussed in policy making in many countries. In our experiments, we could not find any difference between ONE and ALL. This suggests that it might be better for the AA to give immunity only to the first reporter rather than all reporters from the viewpoint of the cost effectiveness of a public policy, since giving the immunity means a reduction in revenues from surcharges, thus increasing the cost of law enforcement. In most countries other than US, however, the immunity and/or a reduction in fines are given to more than the first reporter. One of the reasons is to increase information brought to the AA by accepting multiple reporters. This reduces investigation costs for the AA. This information aspect of a leniency program remains to be studied.

There are several possible explanations for the potential difference in the effectiveness of applying leniency only to the first reporter or to all reporters. If only the first reporter is given a reduced fine or a reward, member firms might race to get leniency before the others could do so. This intuition is also supported by researchers and the AA in the United States where only the first reporter is given maximum leniency and no leniency is given to other reporting firms. On the other hand, in the case of Japanese cartels, the members may hesitate to report the cartel evidence to the AA if only the first reporter would be given a reduced fine or a reward. This is because a member might fear retaliation from other members in the future. As the Japanese market is smaller than that of the United States and there is a tight connection between firms, Japanese firms fear being branded as a “cheater.” On the other hand, if all the reporters were given reduced fines or a reward, then they would less fearful of retaliation for applying for leniency since not doing so would be irrational and no one can be blamed for doing the right thing. In fact, under the Japanese leniency program, three firms can receive a reduced fine with different reduction rates, and in 2008 a new law is expected to be submitted to the Diet to expand the maximum number of eligible firms. Under the leniency program in the EU, even though its reduction rates differ, there is no restriction on the number of firms who are given a reduced fine.

We have to admit that our experiment focused on only a limited environment. Since our subjects were forced to collude in the beginning, we can not say anything about whether firms would newly form a cartel voluntarily where a leniency program is already introduced. However, we believe that the effect of leniency programs can be separated into two dimensions. One dimension is the kinds of leniency programs actually used by preexisting cartel groups. To create a situation in which firms count on their cartel partner(s) to collude with them with certainty in some illegal business activity (e.g. price fixing), we forced subjects to collude in the first stage. In addition to that, since the JFTC leniency program was just enacted in early 2006, we wanted to know what kind of leniency program would actually be effective to dissolve preexisting cartel firms. The other dimension is the kinds of leniency programs that would prevent firms from newly forming a cartel or would prevent cartel recidivism. Even if a leniency program is successful at dissolving preexisting cartels, such an effect could be temporary. The leniency program might be weak at preventing hard core cartels from reforming a cartel again. Investigation into this second dimension remains to be investigated. In addition, the effects of changing the amount of a fine and the investigation probability have not been investigated in this study. The effectiveness of a policy which rewards reporters should be investigated in terms of the magnitude of reward.

Appendix A. Instructions for Partial Leniency for ALL (2-person case)

This experiment consists of Stage 1 and Stage 2. You choose A in Stage 1. In Stage 2, you can select one of two choices, C or D. The summary of the procedure of this experiment is as follows.

1) Decision making in Stage 1 (Choose A)
2) Decision making in Stage 2 (Choose C or D)
3) Each group is given a lottery number. There is an unlucky lottery number, which reduces the payoffs of the members of the group. The possibility of each group drawing this lottery is 10% (therefore, your group won’t get this lottery at 90% of the time.)
4) Your payoff of the round is decided.

We will explain how your payoff will be decided by referring to the Payoff Tree in the next page.

Stage 1

Look at the Payoff Tree on the next page. You and your partner choose A in this stage and go on to Stage 2.

Stage 2

You are free to choose C or D. Your payoff will be different in the following cases.

1) You and your partner choose C and your group did not draw the payoff reduction lottery.
2) You and your partner choose C and your group drew the payoff reduction lottery.
3) Other cases: (You choose C, Partner chooses D), (You choose D, Partner chooses C), (You choose D, Partner chooses D).

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33 See Nippon Keidanren (2004).
In the following table, payoffs are shown in these cases.

<table>
<thead>
<tr>
<th>Case 1</th>
<th>Case 2</th>
<th>Case 3</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Payoff in the round</strong></td>
<td><strong>Payoff in the later round</strong></td>
<td><strong>Payoff in the later round</strong></td>
</tr>
<tr>
<td>You and your partner earn <strong>40</strong> points each.</td>
<td>In the next round, you choose A in Stage 1 for the next round and make a new decision in Stage 2. The payoff will be decided similarly to this round.</td>
<td>Regardless of the result of lottery, a person who chose C earns <strong>0</strong>, a person who chose D, earns <strong>35</strong>. After this round, you are forced to choose B in Stage 1 and your payoff will automatically be <strong>20</strong> in every later round.</td>
</tr>
</tbody>
</table>

---

From the second round, whether the session is ended or not is determined by lottery. The session is continued with probability of 80%. We do not know when the session is ended beforehand. (For example, the probability that the session is repeated for 5 rounds is \(0.8^5 \times 100\), which is about 33\%).
Now we explain how to manipulate the computer. When Experiment 2 is started, you will see this kind of screen.

In the top panel, it says,

Round
1/1.

Since we do not know how long the session is repeated, this means that this is the first round. The right top panel shows how many seconds left to make a decision.

The middle panel says,

Rule of Stage 1
You are paired with somebody. Your partner will remain the same until the end of experiment. Please choose A in Stage 1. If you have anything you don’t understand, please read the instructions.

In the bottom panel, the first line shows “Your ID.” This number is the same as your seat number. Please make sure whether they are the same. The next line is “Your decision.” On the right side, there are two choices, A and B. However, in this experiment, you are allowed to choose only A. When everybody is finished choosing A in Stage 1, your screen will change to this.
In this screen, you confirm what you and your partner did in Stage 1. Then, click the OK button in the lower right corner. When everybody has clicked buttons, you will see the following screen.

<table>
<thead>
<tr>
<th>number</th>
<th>1</th>
</tr>
</thead>
<tbody>
<tr>
<td>name of your partner</td>
<td>1</td>
</tr>
<tr>
<td>your decision</td>
<td>A</td>
</tr>
<tr>
<td>your partner's decision</td>
<td>A</td>
</tr>
</tbody>
</table>

---

あなたは、あなたとパートナーを確認します。実験の最後まで同じ人とパートナーになります。

第1段階では、Aを選んでください。

分からないことがあれば、実験後の質問を含めてください。
The middle panel says,

*The rule of Stage 2*

*Your partner is the same as in Stage 1. You will be matched with the same person till the end of the experiment. In Stage 2, please choose C or D. If there is anything you do not understand, please refer to the instructions.*

In the bottom panel, “your ID number” appears first and you make your decision for Stage 2. There are two radio buttons, C and D. **You can choose one of them freely.** For example, assume that you and your partner choose C, and the screen changes to the following.
Please look at the Top-left panel. It says:

Your payoff
You chose C, your partner chose C
If your group did not draw a payoff reduction lottery, you both get 40 points each (the probability not to draw this lottery is 90%). If your group drew the lottery, you both get 0 points each (the probability to draw this lottery is 10%).
You chose C, your partner chose D
You get 0 points, your partner get 35 points.
You chose D, your partner chose C
You get 35 points, your partner get 0 points.
You chose D, your partner chose D
You and your partner both get 35 points each.

All the cases except for the case that both you and your partner choose C, \textit{regardless of the result of the lottery}, a person who chose C gets 0, a person who chose D gets 35 points. That is, if either one of you chose D, the lottery does not affect your payoff.

Top-right panel says:

Your payoff from the next round
You and your partner chose C and your group did not draw a payoff reduction lottery, your group goes back to Stage 1 and choose A. Except for this case, you and your partner are forced to choose B in Stage 1 from the next round. Each of you gets automatically 20 points each.

The bottom panel describes:

Your ID number
Your payoff for this round
Your decision in Stage 1
Your partner's decision in Stage 1
Your decision in Stage 2
Your partner's decision in Stage 2
Whether your group drew a payoff reduction lottery or not

The example above is the case which your group did not draw a payoff reduction lottery and your payoff is 40 points. If your group had drawn a payoff reduction lottery, you get 0 points. Check the result in the following picture.
If you and your partner chose C and your group did not draw a payoff reduction lottery, your group goes back to Stage 1 to choose A and then can proceed Stage 2. However, in other cases as follows,

1. You and your partner chose C and drew a payoff reduction lottery
2. Either you or your partner chose D, or both chose D (Your choice, partner’s choice) = (C, D), (D, C), (D, D)

you and your partner are forced to choose B in Stage 1 and you automatically get 20 points in every later round. In this case, you do not see a screen for decision making, but you see the following result screen.
Please look at the bottom panel. Your payoff for this round is 20 points. The panel shows that your decision and your partner’s decision for Stage 1 are B. It also shows that your decision and your partner’s decision for Stage 2 are “no choice.” Also it shows that your group did not draw a lottery.

Now we will practice following 5 examples to understand how your payoff is determined. We will also practice how to manipulate the computers. First please fill in the blanks below. We will repeat 5 rounds for each question.

1. Both chose C and your group did not draw a payoff reduction lottery
   Your payoff points Your partner’s payoff points

2. Both chose C and your group drew a payoff reduction lottery
   Your payoff points Your partner’s payoff points
   Your payoff from the next round points
   Your partner’s payoff from the next round points

3. Both chose D
   Your payoff points Your partner’s payoff points
   Your payoff from the next round points
   Your partner’s payoff from the next round points

4. You chose C and your partner chose D
   Your payoff points Your partner’s payoff points
   Your payoff from the next round points
   Your partner’s payoff from the next round points

5. You chose D and your partner chose C
   Your payoff points Your partner’s payoff points
   Your payoff from the next round points
   Your partner’s payoff from the next round points

Next we will practice using the computers. Please raise your hand if you have a question, and the experimenter will come to your seat to explain.
References


