Fines, Leniency and Rewards in Antitrust*

Maria Bigoni†, Sven-Olof Fridolfsson‡,

Chloé Le Coq§, and Giancarlo Spagnolo¶

10th November 2011

Abstract

This article reports results from an experiment studying how fines, leniency and rewards for whistleblowers affect cartel formation and prices. Antitrust without leniency reduces cartel formation but increases cartel prices: subjects use costly fines as punishments. Leniency improves antitrust by strengthening deterrence but stabilizes surviving cartels: subjects appear to anticipate the lower post-conviction prices after reports/leniency. With rewards, prices fall at the competitive level. Overall our results suggest a strong cartel deterrence potential for well-run leniency and reward schemes. These findings may also be relevant for similar white-collar organized crimes, like corruption and fraud.

JEL codes: C73, C92 and L41

*Many thanks to our editor Judith Chevalier and to two anonymous referees as well as to Martin Dufwenberg, Tore Ellingsen, Nisvan Erkal, Magnus Johanneson, Dorothea Kuebler, Joe Harrington, Nathan Miller, Hans-Theo Normann, Sander Onderstaal, Charles Plott, Patrick Rey, Maarten Pieter Schinkel, Adriaan Soetevent, Jean Tirole, and Julian Wright for discussions and advice related to this project, and to audiences in Alicante (IMEBE 2008), Amsterdam (ENABLE Conference), Berlin (ESMT, and WZB), Boston (IIOC 2009), Copenhagen (U. of Copenhagen), Crete (CRESSE 2009), Frankfurt, Gerzensee (ESSET 2007), Gothenburg (NWBEE 2008), Mannheim (RNIC 2007), Norwich (UEA-CCP), Rome (Tor Vergata and EIEF), Toulouse, San Francisco (IOS-ASSA Meeting 2009), Singapore, Stockholm (Ifn, Stockholm School of Economics and Konkurrensverket), and Naples (U. Federico II) for comments and suggestions. We also gratefully acknowledge research funding from Konkurrensverket (the Swedish Competition Authority) that made this research possible. Chloé Le Coq thanks Jan Wallanders och Tom Hedelius Stiftelse for Financial support.

†University of Bologna, maria.bigoni@unibo.it

‡IfN, Sven-Olof.Fridolfsson@ifn.se

§SITE, Stockholm School of Economics, chloe.lecoq@hhs.se

¶University of Rome Tor Vergata, SITE, EIEF and CEPR, giancarlo.spagnolo@uniroma2.it

Key words: Cartels, Collusion, Coordination, Competition policy, Crime and Punishment, Deterrence, Law enforcement, Price-fixing, Whistleblowers.

1 Introduction

The last decades have brought a major innovation in antitrust law enforcement. In most OECD countries, leniency policies – schemes that reduce sanctions for self-reporting cartel members – are now the main tool for discovering and prosecuting cartels.[1] These policies are considered hugely successful, having increased dramatically the number of detected and convicted cartels. Yet, higher numbers of detected and convicted cartels alone are not necessarily good indicators of success.[2] As competition policy's main objective is increasing welfare, ideally a successful policy should reduce cartel formation and prices rather than increase convictions.

Compared to many other law enforcement policies, the deterrence effects of antitrust policies are particularly difficult to evaluate because the population of cartels and changes in it are unobservable. Recent indirect methods developed by Miller (2009) and Harrington and Chang (2009) address this problem, identifying empirically the likely effects of new antitrust policies using only changes in observables (such as the number of detected cartels or their duration).[3] Although highly valuable, these methods have limitations. They can only estimate the effects of policies actually implemented, not those of the many available alternatives, and they focus on cartel formation rather than on welfare.[4]

These features - common to other forms of white-collar crime, like corruption and fraud - make laboratory experiments particularly valuable. Experiments have obvious own limitations with firms represented by students who compete in highly stylized environments. Still, experiments allow us to observe policy induced changes, both in the

[1] Some jurisdictions (e.g. Korea, the UK) have also introduced rewards for whistle-blowers, following their successful use in fighting government fraud (US False Claim Act) and tax evasion. See Spagnolo (2008) for an overview.

[2] For example, an extremely lenient policy with substantial fine reductions to all cartel members may produce many leniency applications and greatly facilitate prosecution, but harm society by encouraging cartel formation and increasing prosecution costs.

[3] See also Brenner (2009). Brenner and Miller bring these methods to the data and find, respectively, no significant increase in deterrence following the 1996 introduction of the EU Leniency program, and a positive and significant increase in deterrence following the 1993 changes in the US Leniency policy.

[4] As argued by Whinston (2006), the relationship between communication in cartels and prices is not yet fully understood, hence the presumption that reduced cartel formation feeds back into lower prices and higher welfare cannot be taken entirely for granted. See also Sproul (1993) who finds in a sample of US cases that prices increased weakly after antitrust conviction; and McCutcheon (1997) who suggests that antitrust fines may stabilize collusive agreements by preventing agreements' renegotiation, but not their formation.

population of cartels and in prices, and to test different policy designs.

This article presents results from an experiment we designed to analyze the general deterrence and price effects of different antitrust policies. Subjects play a repeated differentiated goods Bertrand duopoly game and can decide, before choosing prices, whether to form a cartel by communicating on prices. Treatments differ in the presence of a cartel prohibition with positive expected fines for infringers, and in the possibility of obtaining either leniency or a reward by self-reporting before an investigation is opened. Most crucially – and unlike in previous experimental works – subjects can self-report both before and after price choices become public information, as in reality.

The main questions we ask using our experiment are: How do monetary fines with and without leniency or rewards for self-reporting whistleblowers affect cartel formation (deterrence), stability/break down (desistance), and recidivism? What are these policies' effects on prices (welfare), both inside and outside cartels, and after cartels are dismantled? Does it matter if self-reporting is possible before price choices (and hence defections) become public? Are leniency applications used as opportunities to defect and abandon cartels, as instruments to punish defectors and stabilize cartels, or both?

Our main findings are the following. Antitrust laws without leniency, as captured by fines following successful investigations, turn out to have significant deterrence effects; the number of cartels formed in our experiments are reduced. However, antitrust laws also have a significant pro-collusive effect. The prices of those cartels that do form increase. Indeed, the net welfare effect of antitrust laws appears negative, as prices increase on average relative to a laissez-faire regime in which antitrust laws are not enforced (but cartel agreements are not legally enforceable).

Introducing leniency for the first party reporting strongly improves welfare relative to antitrust without leniency. Leniency leads to lower average prices, primarily by further reducing cartel formation. However, we still do not find that this regime lowers prices relative to laissez-faire. This is primarily due to the fact that cartels that do form under the antitrust regime with leniency are more stable than cartels formed under laissez-faire.

In our experiments, we find a powerful role for whistleblower rewards. When rewards

for whistleblowers, financed by fines from competitors, are introduced, average prices fall to competitive levels. Although some cartels still form in this treatment, they are mainly attempts to cash the reward at the expense of the partner and are systematically reported.

The focus of current antitrust practice is deterring explicit cartel formation. Our results seem to give some weight to the concern that explicit cartel deterrence may not always feed back into low prices, the real goal of competition policy. The results also suggest that Miller's (2009) important finding, that the US Corporate Leniency Policy probably reduced cartel formation, may not yet be sufficient to confidently conclude that the policy was welfare-increasing.

The higher cartel prices with antitrust enforcement call for an explanation. We explore several possible ones, including selection and coordination effects. Our results suggest that the most important mechanisms differ under regimes with and without leniency. In the antitrust regime with fines but no leniency, our results suggest that using reports and fines as punishments against defectors allow cartels to sustain higher prices. In cartels with leniency, we find that reports are not used as punishments. Our results are consistent with the presence of an "enforcement effect". Subjects appear to anticipate that, after defecting (and reporting) under leniency, prices are particularly low.

More generally, post-conviction behavior reveals a significant ex post deterrence (desistance) effect of antitrust enforcement, as cartels do not re-form for several periods after being dismantled. This effect becomes much stronger under leniency when the cartel is detected because one party defected and self-reported. Then, the cartel is almost never reformed, so that leniency greatly reduces recidivism in our experiment, contrary to previous findings. And post-conviction prices on average are significantly lower after conviction than before, particularly with leniency.

We also perform a preliminary exploration of the effect of excluding the ringleader from the leniency program, as in the US leniency policy, finding that the deterrence effect of leniency is unaffected, though prices increase. This result should be taken as a very preliminary first benchmark, however, as our experimental set up was not designed to address this question and is particularly unfavourable to excluding ringleaders.

The article is organized as follows. The next section reviews the related literature. Section 3 describes the experimental design. Section 4 presents our hypotheses, which serve as a benchmark for our analysis. Section 5 presents and discusses our results and Section 6 concludes. Appendices discuss our empirical strategy and provide additional details about the experiment.

2 Related Literature

The theoretical literature on leniency policies in antitrust, initiated by Motta and Polo (2003) and surveyed in Rey (2003) and Spagnolo (2008), has shown that granting leniency to subjects reporting before the opening of an investigation can be very effective in deterring cartels but may also be used strategically by wrongdoers to punish defections. Many issues remain open therefore for empirical and experimental research. We mentioned earlier the important recent empirical studies by Miller (2009) and Brenner (2009), as well as their limited ability to observe prices and to evaluate policies that have not actually been implemented. Experiments are useful in this regard, and we are not the first to use them in this area. We build in particular on the work of Apesteguia, Dufwenberg and Selten (2007) and Hinloopen and Soetevent (2008), henceforth "ADS" and "HS", extending it along several dimensions and investigating unexplored issues important to the design and implementation of antitrust policy.[5]

ADS develop and implement in the lab a stylized theoretical framework. They augment a one-shot homogeneous goods discrete Bertrand triopoly game with the possibility to communicate before the price choice, and to be convicted by an antitrust authority afterwards if communication took place. They test four legal frameworks: *Ideal*, in which cartels are impossible (communication is not allowed); Standard, where communicating

[5] We are aware of two other previous experimental studies dealing with these issues, though in very different environments. Hamaguchi et al. (2009) perform an experiment where subjects are forced to collude, and look at the effects of leniency on the speed with which cartels are dismantled. Hamaguchi et al. (2007) study the effects of leniency in a repeated auction game, in which subjects have to decide who will win the auction. After our work other experimental studies have been performed in various environments, some of which confirm our finding that law enforcement policies based on leniency may have perverse effects on market prices (see e.g. Krajkova and Ortmann 2009a,b). There are, of course, many previous experimental studies of price competition that do not focus on the antitrust issues we analyze. See Holt (1995) for a review.

firms face fines equal to 10% of their revenue with positive probability and no fine reduction if they self-report; Leniency, in which self-reporting firms receive a fine reduction; and Bonus, in which they are rewarded with a share of the fines paid by other firms. Subgame perfect collusive equilibria (including the monopoly outcome) exist in Standard and Leniency, sustained by the credible threat of self-reporting after a price defection;[6] in Ideal and Bonus, the Bertrand outcome is the only equilibrium. They find Leniency to have a significant deterrence effect relative to Standard, although prices are higher with antitrust enforcement than without. Surprisingly, their results are inconsistent with the theoretical prediction that rewarding whistleblowers further increases deterrence. Our experiment differs from this pioneering study in many ways, including the dynamic approach, the scope for learning, the possibility to self-report both before and after price choices and the inclusion of fixed fines. This last feature accounts for fixed components of real antitrust fines, which do not disappear when the other party undercuts the collusive price as in ADS, and simplifies the decision problem. Our results confirm their observations of positive cartel deterrence effect of leniency and of possible perverse effects of standard antitrust enforcement on prices. On the other hand, we find that rewards perform much better in our dynamic set up (as ADS conjectured).

HS implement a repeated version of ADS's game (but for bonuses) in which subjects are matched into the same group of three throughout the experiment. They find that leniency reduces cartel formation and prices, and destabilizes non-deterred cartels (cartel members defect more often and more aggressively), but does not reduce cartel recidivism compared to standard antitrust. We find instead that leniency deters cartels but does not significantly reduce average prices relative to laissez-faire, as it stabilizes surviving cartels, although it substantially reduces recidivism. Our experiment, besides dealing with several different issues, also differs a lot in the design, which justifies the different results on the overlapping issues. Most crucially, in our experiment subjects can self-report both before price choices are observed by other subjects and after, as in reality. This possibility

[6] The threat of self-reporting to punish a price deviation is also credible in Standard because the competitors of the defecting firm face no cost of self-reporting; fines are a fraction of revenues, which equal zero in a homogeneous Bertrand game.

activates a deterrence channel – defections become more profitable under leniency – considered crucial by theorists and practitioners.[7] It also allows us to precisely disentangle and quantify reports linked to defections and to punishments. Other important aspects that distinguish our approach from HS are that in our set up self-reporting is possible even absent leniency; that our experiment is framed as a cartel game, as in ADS; that our subjects compete in duopolies rather than in triopolies, so that they do not refrain from punishing defectors out of reluctance to harm a third 'innocent' party (a concerned raised by Holt, 1995); that our subjects are re-matched in every period with an exogenous and constant probability, so that they face a constant continuation probability which also allows us to study in detail the differences between ex ante and post conviction deterrence. In addition, in HS fines are function of profits realized the last period before conviction. In a dynamic framework this makes it even more difficult to control for subject's expectations, because conviction may take place when prices (and fines) are high because the cartel is successful or when they are low because of a defection or a price war. We decided to opt for fixed fines to simplify the decision problem and have full control of subjects' perceived expected fines.

A drawback of both approaches is that sanctions are not sensitive to cartel duration and accumulated profits, like in most jurisdictions. Future experimental work should therefore try to introduce fines that increase with accumulated cartel profits, although this will further complicate subjects' decision problem.

3 Experimental Design

In our experiment, each subject represented a firm and played in anonymous two-person groups a repeated duopoly game. In every stage game, the subjects had to take three types of decisions. First, they had to decide whether or not to form a cartel by discussing prices. Second, they had to choose a price in a discrete Bertrand price game with

[7] This deterrence channel was named 'protection from fines effect' in Spagnolo (2004) and 'deviator amnesty effect' in Harrington (2008). Absent the possibility to report before prices are disclosed, reports are likely to work mainly as credible punishments under leniency, as highlighted by Spagnolo (2000) and Ellis and Wilson (2001).

differentiated goods.[8] Third, the subjects could choose to self-report their cartels to a competition authority. The attractiveness of this third opportunity depended on the details of the antitrust law enforcement institution, which were the treatment variables in our experiment.

The Bertrand game

In each period, the subjects had to choose a price from the choice set $\{0, 1, ..., 11, 12\}$. The resulting profits depended on their own price choice and on the price chosen by their competitor, and were reported in a profit table distributed to the subjects (see Table 1). This table was derived from the following standard linear Bertrand game. (The details of the Bertrand game were not described to the subjects.)

[Table 1 approximately here]

The demand function for each firm i was given by:

$$q_i(p_i, p_j) = \frac{a}{1+\gamma} - \frac{1}{1-\gamma^2}p_i + \frac{\gamma}{1-\gamma^2}p_j \tag{1}$$

where p_i (p_j) is the price chosen by firm i (firm j), a is a parameter accounting for the market size and $\gamma \in [0, 1)$ denotes the degree of substitutability between the two firms' products. Each firm faced a constant marginal cost, c, and had no fixed costs. The profit function, $\pi_i(p_i, p_j)$, was thus given by $\pi_i(p_i, p_j) = (p_i - c)q_i$. In the experiment, $a = 36$, $c = 0$ and $\gamma = 4/5$ and subjects' choice set was restricted $\{0, 2, ..., 22, 24\}$, yielding the payoff table. To simplify the table we relabeled each price by dividing it by 2 and rounded the payoffs to the closest integer. In the unique Bertrand equilibrium, both firms charge a price equal to 3, yielding per firm profits of 100. The joint profit-maximizing price (charged by both firms) is 9, yielding profits of 180. Note also that a firm would earn 296 by unilaterally and optimally undercutting the joint profit-maximizing price, i.e.

[8] We adopt differentiated goods Bertrand competition because we find it more intuitive and realistic for studying price-fixing agreements than Cournot, and to avoid that leniency applications could be inflated by the strong 'revenge' incentives the homogeneous good Bertrand model may generate given the extreme costs incurred by a subject when facing any price deviation.

by charging a price of 7. In this case the other (cheated upon) firm only earns a profit of 20. Similarly, there are gains from deviating unilaterally from other common prices as well as associated losses for the cheated upon firm; in the range of prices $\{4, ..., 8\}$, these gains and losses are smaller than when a subject deviates unilaterally from the joint profit-maximizing price.

Cartel formation

Throughout the experiment, the subjects could form cartels by discussing prices. At the beginning of every period, a communication window opened if and only if both subjects agreed to communicate. This communication stage, described in more detail below, was designed in a way to produce a common price on which to cooperate. The agreed price was non-binding so that subjects could subsequently undercut. Following HS, we adopt a highly structured communication protocol, which allows subjects to coordinate on collusive prices, but not on punishment strategies.[9] Whenever two subjects chose to communicate, they were considered to have formed a cartel. In this case, the subjects risked to be fined as long as the cartel had not been detected. Subjects could be fined therefore in a period even if no communication took place in that period, for example if they had communicated in the previous period without being detected. Once detected, a cartel was considered to be dismantled and in subsequent periods the former cartelists did not risk being fined unless they communicated again.

Antitrust law enforcement (Treatments)

We ran four lead treatments corresponding to different legal frameworks and each subject participated in a single treatment, a *between subjects* design. Depending on the treatment, a competition authority could detect cartels and convict its members for price fixing. Detection could occur in two ways. First, cartel members could self-report their cartel.

[9] A recent paper by Cooper and Kuhn (2010) shows that allowing for free-form communication may foster more stable and effective collusive agreements, for two main reasons. First, subjects can issue explicit threats of punishment, and second, verbal punishment is used as an inexpensive but highly effective substitute for price wars.

In this case the cartel members were convicted for price fixing with certainty and if so, the size of the fine depended on the treatment. Second, non-reported cartels were in every period detected with an exogenous probability, α, and, if detected, both cartel members had to pay an exogenous fine, F.[10]

The lead treatments are summarized in Table 2. The baseline treatment, L-FAIRE, corresponded to a laissez-faire regime: in this treatment, $\alpha = F = 0$ so that forming a cartel by discussing prices was legal. To simplify the instructions and to eliminate irrelevant alternatives, subjects were not allowed to report cartels. In the three other treatments, FINE, LENIENCY, and REWARD, the expected fine without reporting was strictly positive: $\alpha = 0.1$ and $F = 200$ (i.e. $2,5$ times the extra monopoly profit of $180 - 100 = 80$), yielding an expected fine $\alpha F = 20$; and cartel members were allowed to report their cartel. FINE corresponded to traditional antitrust laws without leniency: if a report took place, both cartel members (including the reporting one) had to pay the full fine F. LENIENCY corresponded to antitrust laws embedded with leniency: if the cartel was reported by one cartel member only, the reporting member paid no fine whereas the other paid the full fine, F; if instead both cartel members reported the cartel simultaneously, both paid a reduced fine equal to $F/2$. Finally, REWARD differed from LENIENCY in one respect only: if a single cartel member reported the cartel, he/she paid no fine and was rewarded with the full fine, F, paid by the other cartel member.

[Table 2 approximately here]

In addition we ran three other treatments, NOREPORT, REMATCH and RINGLEADER, which we review further below.

Timing and rematching procedure

At the end of each period, subjects were rematched with the same competitor with a probability of 85%. With the remaining probability of 15%, all subjects were randomly

[10] Repeated communication in real world cartels is likely to increase the probability of detection. We chose not to replicate this in our design to avoid adding further complexity to an already demanding set up. This simplification is unlikely to bias our results as increases in the probability of detection due to repeated communication should affect all policy treatments in the same way.

matched into new pairs. If so, subjects could no longer be fined for cartels formed in the previous match. After the first 20 periods, if the 15% probability event took place there was no more rematch, and the experiment ended. The subjects were also informed that the experiment would end as well if it lasted for more than 2 and 1/2 hours. This latter possibility was unlikely and did not occur. This re-matching procedure minimized problems with end game effects, pinned down subjects' expectations on the duration of matches for all contingencies, and allowed us to distinguish ex ante deterrence (communication decisions prior to the first time two subjects communicated) from post conviction deterrence (communication decisions after a first cartel was convicted). This procedure may also have facilitated learning insofar subjects were more willing to test alternative strategies after a rematch (e.g. sticking rather than undercutting an agreed upon collusive price).[11]

The timing of the stage game

With the exception of L-Faire, a stage game consisted of 7 steps. In L-Faire, steps 4, 5 and 6 were skipped. An overview of the steps is given in Figure 1.

[Figure 1 approximately here]

Step 1: Communication decision. Each subject was asked whether or not he wished to communicate with his competitor. If both subjects pushed the yes button within 15 seconds, the game proceeded to step 2. Otherwise the two subjects had to wait for 30 seconds before pricing decisions were taken in Step 3. In all periods, subjects were also informed whether or not a re-match had taken place.

Step 2: Communication. If both subjects decided to communicate in step 1, a window appeared on their computer screen asking them to state simultaneously a minimum

[11] Subjects' decisions in the current match may be influenced by the possibility of being re-matched with the same competitor in future matches. Compared to the procedure's benefits, however, this potential cost appears small. A subject faced a very low probability to be paired with the same competitor when a rematch took place, and even if two subjects were paired together again, they would not know it. Moreover, even if the prospect of future matches had a relevant impact on current behavior, the effect should not lead to biases as it is likely to affect all treatments in the same way. This is also why recent experimental work on repeated games increasingly relies on this type of re-matching (see e.g. Dal Bó and Fréchette, 2011, and Dreber et al., 2008).

acceptable price in the range $\{0, ..., 12\}$. When both had chosen a price, they entered a second round of price negotiations, in which they could choose a price from the new range $\{p_{min}, ..., 12\}$, where p_{min} equalled the minimum of the two previously chosen prices. This procedure went on for 30 seconds. The resulting minimum price was referred to as the agreed upon price.

Step 3: Pricing. Each subject had to choose his price from the choice set $\{0, ..., 12\}$. Price agreements in step 2 were non-binding. The subjects were informed that if they failed to choose a price within 30 seconds, then their default price would be so high that their profits became 0.

Step 4: Secret reports. If communication took place in the current period or in one of the previous periods and had not yet been detected, subjects had a first opportunity to report the cartel. Reports in this step are referred to here as 'secret'.

Step 5: Market prices and public reports. Subjects learned the competitor's price choice. If communication took place in the current period or in one of the previous periods without being discovered and no one reported it in step 4, subjects had a new opportunity to report the cartel. The crucial difference between this 'public' report and the secret one is that the subjects knew the price chosen by the competitor. In addition the subjects were informed about their own profits and the profits of their competitor, gross of the possible fine/reward.

Step 6: Detection. If communication took place in the current period or in one of the previous periods without being discovered or reported before (in steps 4 and 5), the cartel was detected with probability α.

Step 7: Summary of the current period. At the end of each period, all the relevant information about the stage game was displayed: the agreed upon price (if any), prices chosen by the two players, possible fines and net profits. When players were fined, they were also told how many players reported. This step lasted 20 seconds.

Experimental procedure

Our experiment took place in March, April, May and December 2007 at the Stockholm School of Economics (Sweden) and at Tor Vergata University (Rome, Italy). Sessions lasted on average 2 hours, including instructions and payment. The average payment was: (i) in Stockholm Euros 26.14, with a minimum of 12.54 and a maximum of 42.51 and (ii) in Rome Euros 24.22 with a minimum of 16.5 and a maximum of 31.5.[12] In every session we ran one treatment; the number of subjects per session ranged from 16 to 32, and the total number of subjects was 390. Details about each session including the number of subjects, when and where they were conducted as well as the number of periods and matches are reported in Appendix A.

The experiment was programmed and conducted using z-tree (Fischbacher, 2007). Subjects were welcomed in the lab and seated, each in front of a computer. They received a printed version of the instructions and the profit table. Instructions were read aloud to ensure common knowledge of the rules of the game. We then asked the subjects to read the instructions on their own and ask questions, which were answered privately. When everyone had read the instructions and there were no more questions (in each session, after about fifteen minutes), each subject was randomly matched with another subject for five trial periods. After these trial periods, participants had a final opportunity to ask questions. Then subjects were randomly rematched into new pairs and the real play started.

At the end of each session, the subjects were paid privately in cash. The subjects started with an initial endowment of 1000 points in order to reduce the likelihood of bankruptcy, an event that never occurred. At the end of the experiment the subjects were paid an amount equal to their cumulated earnings (including the initial endowment) plus a show-up fee of 7 Euros (50 Swedish kronor in Stockholm). The conversion rate was 200 points for 1 Euro (10 Swedish kronor in Stockholm).

[12] The subjects in Stockholm were paid in Swedish kronor (SEK). At the time of the experiment, 1 SEK=0.109 Euros.

4 Hypotheses

This section discusses possible effects of the different policies in our experiment. The purpose is to propose sensible and testable hypotheses. Specifically, these are formulated so as to be consistent with how the different policies are intended to work in reality.[13]

The joint profit-maximizing price can be supported as an equilibrium outcome in our four lead treatments. No hypotheses can thus be stated on the ground that collusive outcomes do not constitute an equilibrium in some of the treatments. Yet the participation (P-) and incentive compatibility (IC-) constraints, two necessary conditions for the existence of a collusive equilibrium, provide valuable insights about the possible effects of law enforcement institutions. All else equal, these constraints are tighter in some treatments and, under the standard assumption that tighter equilibrium conditions make it harder to sustain the equilibrium, they should also increase deterrence. Increased deterrence should also mean lower prices on average, at least if cartels charge the same prices across treatments.

The P-constraint requires that the gains from collusion should be larger than the expected cost. All else equal, it is tighter in the policy treatments than in L-Faire, because the expected cost (the risk of being fined) is 0 in that treatment. The IC-constraint requires that sticking to an agreement is preferred over a unilateral price deviation followed by a punishment. All else equal, it is (i) tighter in Reward than in Leniency (because the reward strengthens the incentives to deviate), (ii) tighter in Leniency than in Fine (because a deviation combined with a secret report provides protection against the fine) and (iii) tighter in Fine than in L-Faire (if subjects communicate more on the collusive path than on the punishment path). This reasoning leads to our first hypothesis.

Hypothesis 1 (cartel formation and prices) Cartel formation rates and prices are highest in L-Faire, followed in order of decreasing magnitude by Fine, Leniency and Reward.

[13] A simple equilibrium analysis based on Spagnolo (2004) underpins our hypotheses (see Bigoni et al, 2009). In line with experimental evidence (e.g. Crawford, 1998), this analysis presumes (among other things) that pre-play communication (cartel formation in our context) enhances subjects' ability to coordinate and charge high prices.

The previous equilibrium based reasoning implicitly presumes subjects to be risk neutral and fully rational, perfectly able to coordinate on any proposed equilibrium when communicating, and motivated only by monetary payoffs. None of these assumptions is realistic: subjects are likely both to undercut the agreed upon price and to report, and therefore differences across treatments in terms of cartel stability, cartel detection, cartel prices and so on are likely to arise. Still, the P- and IC-constraints highlight costs and benefits associated with price deviations and reports. As such they offer a valuable starting point for stating plausible hypotheses about subjects' behavior which, strictly speaking, is inconsistent with the equilibrium behavior.

Optimal price deviations are combined with secret reports in LENIENCY and REWARD, in effect hindering the use of public reports as a punishment against defectors. In FINE, both secret and public reports are costly. These incentives suggest the next hypothesis.

Hypothesis 2 (secret and public reports) Price deviations are combined with secret reports in LENIENCY and REWARD, but not in FINE. Public reports are used in none of the treatments.

Tighter IC-constraints may not only affect cartel formation but also cartel stability: price deviations may occur more frequently in treatments with tight IC-constraints, because the incentives to stick to a collusive agreement become weaker. By affecting cartel stability, tighter IC-constraints may also affect cartel prices: all else equal, cartel prices should be higher in treatments with low rates of price deviations. Finally, agreed upon prices may be higher in treatments with stable cartels; if cartels are re-formed after price deviations, subjects may attempt to collude on lower prices so as to relax the IC-constraint. The ranking in Hypothesis 1 thus suggests the following hypothesis.

Hypothesis 3 (cartel stability, cartel prices and agreed upon prices): Cartel stability, cartel prices and agreed upon prices are highest in L-FAIRE, followed in order of decreasing magnitude by FINE, LENIENCY and REWARD.

Cartel stability is also likely to affect the frequency of cartel detections, because optimal price deviations are combined with secret reports in LENIENCY and REWARD but

not in FINE. The ranking in Hypothesis 3 relating to cartel stability thus also suggests the following hypothesis.

Hypothesis 4 (cartel detection) Cartels are detected most frequently in REWARD, followed in order of decreasing magnitude by LENIENCY and FINE.

Secret reports may generate distrust and thereby increase ex post deterrence. Trust destruction following secret reports motivates our final hypothesis.

Hypothesis 5 (cartel recidivism) Convicted cartels are re-formed earlier in FINE than in LENIENCY and REWARD.

5 Experimental results

The success of our experiment hinges to a large extent on two factors. First, consistently with existing experimental evidence showing that pre-play communication enhances subjects' ability to coordinate (see the survey by Crawford, 1998), cartel formation should lead subjects to charge higher prices. Not surprisingly, our experiment validates this finding.

Second, the experiment works if subjects understand the incentives linked to self-reporting. Table 3 presents the rates of secret reports (given an own price deviation) and of public reports (possible only if the rival did not secretly report) in FINE, LENIENCY and REWARD. As expected, subjects almost never used secret reports in FINE, whereas in LENIENCY and REWARD price deviations usually were optimally combined with secret reports.[14]

The rates of public reports are more intriguing. Although public reports were costly in FINE, subjects used them as punishments against price deviators in almost one-third of the cases. We further explore the motive behind these costly reports in Section 5. The rates of public reports in LENIENCY and REWARD also are intriguing, as public reports were

[14] As subjects gained experience, the rates of secret reports rose gradually in both LENIENCY and REWARD. In LENIENCY (REWARD) these rates were approximately 0.6 (0.8) over the five first periods and exceeded 0.9 (equaled 1) over the five last periods.

not used systematically as a costless punishment against defectors that did not combine their price deviation with a secret report. One may hypothesize that subjects in this case were reluctant to use the public report for fear of reducing trust and jeopardizing future cooperation. Overall we view the rates reported in Table 3 as evidence that the subjects understood fairly well the incentives linked to reports.

[Table 3 approximately here]

Traditional and modern antitrust policies

This section reports our main experimental results, namely the effects of traditional policies, FINE, and modern ones, LENIENCY, both in relation to each other and to our benchmark, L-FAIRE. We postpone the discussion on REWARD to Section 5 (although to save space the tables and figures in the current section already include results from REWARD).

Cartel deterrence, detection and recidivism

Cartel deterrence Table 4 reports the two main measures for evaluating the success of the different policies in terms of deterrence: the fraction of subjects choosing to communicate (rate of communication attempts) and the fraction of pairs starting a new cartel (rate of cartel formation), provided that subjects are not already cartel members. The requirement that cartels are not formed is important; in effect an attempt at communicating is an attempt at forming a cartel, and not merely a decision to communicate at no cost. The table also reports the rates of communication attempts during the first period in a match – a measure of *ex ante* deterrence, which also has the advantage of being insensitive to the (random) length of matches.

Result 1 (Cartel deterrence) FINE and particularly LENIENCY are effective at deterring cartel formation.

[Table 4 approximately here]

Rates of communication attempts and of cartel formation are significantly lower in FINE, and much lower in LENIENCY, than in L-FAIRE. These deterrence effects are consistent with the experimental findings in ADS and HS as well as with Miller's (2009) empirical evidence that the US Corporate Leniency Policy reduced cartel formation. The deterrence effects of FINE and LENIENCY are thus consistent with Hypothesis 1.[15]

Cartel detection Table 4 also reports two measures of cartel detection: the rates of detection due to self-reporting, based either on reporting decisions in all periods a cartel was formed, or during the first period two subjects communicated. Both measures yield a ranking consistent with Hypothesis 4:

Result 2 (Cartel detection) LENIENCY substantially and significantly increase cartel detection due to self reporting.

Result 2 is not surprising given the high rates of secret reports in LENIENCY reported in Table 3. It is qualitatively consistent with Miller's (2009) finding that the US Corporate Leniency Policy significantly increased the rate of cartel detection.

Taken together, Results 1 and 2 imply a sizable deterrence effect of LENIENCY: cartels were present more than twice as often in FINE (in 58.3% of the periods) than in LENIENCY (where the figure drops to 26.4%).

Cartel recidivism The rates of communication attempts in the first period of a match are higher in FINE and LENIENCY than the rates of communication based on observations from all periods when a cartel was not formed. This pattern suggests that cartel detection may have affected subjects' decisions to re-form a cartel. Figure 2 shows for FINE, LENIENCY (and REWARD) the cumulative percentage of cartels (vertical axis) re-formed by convicted subjects in the five periods following the conviction (horizontal axis). The

[15] The rates of communication attempts during the first period of communication in each match largely confirm Result 1, although the difference between FINE and LENIENCY is insignificant. The difference becomes significant at the 5% level if we test it via a three-level logit regression, with no random effect at the city level. In fact, the difference is significant at the 1% level if we restrict the sample to data collected in Rome, where the rates are 0.708 in FINE and 0.219 in LENIENCY, whereas it is not significant for Stockholm, where the rates are 0.660 and 0.525, respectively.

plots underestimate this percentage number of re-formed cartels, as some matches ended before the five periods after the conviction occurred. Still, the data tells us quite a lot.

[Figure 2 approximately here]

First, history of play matters, as a large fraction of cartels are not re-formed after conviction even though the subjects faced the same expected fine, available actions and payoff functions as before the convicted cartel was formed. Second, ex post deterrence (desistance) in LENIENCY is higher than in FINE: close to 40% of convicted cartels are re-formed immediately in FINE, but not in LENIENCY.

Result 3 (Cartel recidivism) LENIENCY significantly reduce cartel recidivism.

Result 3 contrasts with HS who found no reduction in cartel recidivism linked to the introduction of leniency policies. The reason is probably that price deviations could not be combined with simultaneous secret reports in their experiment, whereas the lion's share of convictions in LENIENCY were due to secret reports. Such reports are likely to generate substantially more distrust than would a discovery by the competition authority, reducing subjects' willingness to re-form a cartel.

Prices, price deviations and post-conviction pricing

Prices The ultimate objective of antitrust law enforcement is to keep prices low. Table 5 presents price levels on average as well as average prices within and outside cartels and average agreed upon prices. The Table also reports the average cartel and agreed upon prices based on observations from periods when two subjects communicated for the first time. The first lesson to be drawn from this table is that cartel deterrence is desirable, as it reduces prices; in all treatments, prices are higher within cartels than outside them. This finding combined with the high cartel formation rates in L-FAIRE suggests that prices should be highest in that treatment. Our data contradicts this conjecture (and Hypothesis 1).

[Table 5 approximately here]

Result 4 (Average prices) FINE increases significantly prices on average whereas LENIENCY leaves them almost unchanged relative to L-FAIRE.

Thus in our experiment FINE appears to reduce welfare relative to L-FAIRE, whereas LENIENCY does not significantly improve it, even though it substantially reduces prices as compared to FINE. Interestingly, our finding that average prices in FINE are significantly higher than in LENIENCY is consistent with ADS and with HS. This may seem surprising as reporting is much costlier in our treatment FINE than HS's Antitrust treatment and in ADS's Standard treatment, where fines were (unrealistically) absent for 'cheated upon' subjects given they had no revenue.

Prices charged within cartels constitute the main explanation why average prices did not drop in FINE and LENIENCY relative to L-FAIRE despite the significant cartel deterrence effects associated with these policies.

Result 5 (Cartel prices) FINE and LENIENCY significantly increase cartel prices relative to L-FAIRE.

Both cartel prices and the prices charged in periods when newly matched subjects communicated for the first time are significantly larger in the policy treatments than in L-FAIRE. (The differences between LENIENCY and L-FAIRE are also significant at the 1% level.) These findings are inconsistent with Hypothesis 3 and contrast with HS where the antitrust and leniency treatments reduced cartel prices (though only significantly so in the latter treatment). As clarified in the literature review, our experimental design differs along many dimensions from HS and all differences may have contributed to the difference in results. However, we conjecture that subjects' ability to undercut price and report first with certainty, an option only present in our setting (and in reality), and the 'enforcement effect' this generates (discussed in depth in section 5) are the main drivers of these differences.

Table 5 also shows that cartel prices are significantly higher in LENIENCY than in FINE, yet this difference should not be over-emphasized. The reason is that our legal definition of a cartel artificially inflates cartel prices in LENIENCY relative to FINE. As subjects

usually (optimally) combined price deviations with secret reports in LENIENCY but not in FINE (see Table 3), price deviations in LENIENCY frequently led to the disruption of cartels. Price wars therefore often took place outside cartels in LENIENCY whereas in FINE they occurred frequently as cartels still were legally formed. The finding that the prices charged in periods when subjects communicated for the first time were significantly larger in FINE than in LENIENCY also suggests that cartel prices are artificially inflated in LENIENCY. We conclude that Hypothesis 3 cannot be rejected on the ground that cartel prices appear to be higher in LENIENCY than in FINE. Still the low cartel prices in L-FAIRE remain inconsistent with that hypothesis.

Interestingly the price levels for non cartel members appear to be higher in FINE and LENIENCY than in L-FAIRE. Thus the prices charged outside cartels also contributed to the high average prices in FINE and LENIENCY.[16] One possible interpretation of this pattern is that a refusal to communicate when it is costly to do so, does not clearly signal an unwillingness to cooperate. Thereby antitrust policies may result in tacit collusion substituting for explicit collusion.

Price deviations Finally, Table 5 reports the fraction of cartel members that undercut the agreed-upon price determined in the last period in which communication took place (the rates of price deviations) as well as this fraction restricted to periods when two newly matched subjects communicated for the first time. These deviation rates are consistent with the high cartel prices in FINE and LENIENCY, suggesting that antitrust policies may stabilize cartels that are not deterred.

Result 6 (Price deviations) Both FINE and LENIENCY significantly reduce the frequency of price deviations relative to L-FAIRE.

Post-conviction prices Figure 3 shows for FINE, LENIENCY (and REWARD) the price choices in cartels before and after conviction (conviction takes place at time 0), separately for the subjects that re-formed and did not re-form the convicted cartel. The stylized facts

[16] Given that cartels were almost formed systematically in L-FAIRE, this is not the main explanation for the high average prices in FINE and LENIENCY.

emerging from the figure are (a) prices after conviction are on average lower than in cartels before conviction, (b) when cartels are re-established after conviction, prices reach levels close to those prevailing in the period when the cartel was convicted, (c) when cartels are not re-established, prices fall substantially relative to the cartel price prevailing at the time of conviction, remaining low in LENIENCY and rising gradually in FINE and finally (d) post-conviction prices are higher in FINE than in LENIENCY when the convicted cartel is not re-formed.

[Figure 3 approximately here]

The difference arising between LENIENCY and FINE when convicted cartels are not reformed deserves further discussion (stylized fact d). The average price remains close to Bertrand in LENIENCY, whereas it increases in FINE as if – after having formed an explicit cartel and having paid the fine – some of the subjects tried to reach a tacit agreement on prices. A possible explanation for this finding is that detection resulting from investigations by the competition authority occurs more frequently in FINE than in LENIENCY, and that this form of detection does not disrupt trust between cartelists. In LENIENCY cartels are instead usually detected through secret reports combined with simultaneous deviations, in which case post-conviction tacit collusion may be harder to sustain.

Potential explanations for high cartel prices

Several forces may have contributed to the higher cartel prices in treatments with antitrust enforcement. We briefly explore here three non exclusive potential explanations: selection, coordination and enforcement.

Selection

The increase in cartel prices in FINE and in LENIENCY relative to L-FAIRE could in principle be explained by a selection effect in which only the weaker cartels, supporting lower prices, are deterred. To verify whether this effect is present in our data we can plot

the distribution of prices chosen by subjects in the first period they form a cartel, per every match (Figure 4).

[Figure 4 approximately here]

Figure 4 clearly shows that the left tail of the distribution is substantially thicker in L-Faire than in Fine and Leniency. In this baseline treatment, 27.07% of subjects chose a price lower or equal to 4 when they started a cartel, meaning that they decided to establish a price-setting agreement with the sole purpose of deviating immediately from it, and cash in the gains from defection. This proportion drops to 16.15% in Fine and 16.57% in Leniency. This first piece of evidence would be consistent with a selection effect. Yet, Figure 4 also highlights that the right-most part of the price distribution presents important differences across treatments. If we consider only subjects choosing a price equal or above 5, we notice that only 34.02% chose a price above 7 in L-Faire, whereas this figure rises to 65.33% in Fine and 43.84% in Leniency. This suggests that a selection effect due to deterrence cannot be the only or main explanation of the increase in cartel price we observe in treatments Fine and Leniency compared to L-Faire.

Coordination

In experiments where subjects pay to participate in a game, e.g. in an auction, their ability to coordinate on more efficient outcomes appears substantially enhanced.[17] Offerman and Potters (2006) recently found an analogous effect in an experiment where licence auctions are followed by dynamic oligopolistic interaction. In our context, the risk of being fined in Fine and Leniency after communicating similarly may have worked as a coordination device, with subjects coordinating on higher collusive prices thanks to the additional expected cost of cartel formation. Alternatively, the risk of being fined may have facilitated coordination by transforming the initial communication stage from pure 'cheap talk' to possibly more effective 'costly talk'.[18]

[17] See e.g. Van Huyck and Battalio (1993) and Cachon and Camerer (1996). Crawford and Broseta (1998) showed that this effect is partly due to forward induction considerations, and partly to learning and other forces.

[18] The effects of costly communication on coordination and collusion has been investigated experimentally in Andersson and Wengstrom (2007) and Andersson and Holm (2010), though with a very different

If these kinds of coordination effects were important in our experiment, one would expect higher agreed upon prices in FINE and LENIENCY than in L-FAIRE. The agreed upon prices in Table 5, based on all observations when subjects actually communicated, provide some support for a coordination effect. Yet the low agreed upon prices in L-FAIRE may reflect only high deviation rates. Subjects perhaps attempted initially to coordinate on a high price also in L-FAIRE, then experienced frequent price deviations and, to reduce the temptation to cheat, subsequently attempted to collude on a lower price. The agreed upon prices in Table 5, based only on the periods when two subjects communicated for the first time, were less sensitive to this problem. These agreed upon prices were virtually the same in L-FAIRE and LENIENCY, suggesting that improved coordination was not driving the high cartel prices in LENIENCY. However, it may have contributed to the high cartel prices in FINE, as the agreed upon prices in that treatment were significantly higher than those in L-FAIRE.

Enforcement

The high cartel prices in FINE and LENIENCY could also be explained by some enforcement effect. Subjects may have refrained from undercutting agreed upon prices for fear of harsher punishments. The scope for punishing defectors differed in FINE and LENIENCY: because subjects in FINE had no incentives to (and did not) use secret reports, they had access to the public report as an additional instrument for punishing deviators. For this reason, we discuss potential enforcement effects separately for the two treatments.

Enforcement effect in Fine The fact that some subjects in FINE used public reports as punishments (see Table 3) suggests that the threat of such reports may have enforced high cartel prices.[19] At first glance one might dismiss public reports as non-credible, but in fact, punishments involving costly reports are optimal: any collusive price can be sustained in equilibrium for any discount factor. The reason is that collusion is a subgame perfect

take.

[19] Dreber et al. (2009) implement experimentally a modified version of a repeated prisoners' dilemma where subjects can punish defectors. They find that "winners don't punish", i.e. subjects that fare better, do not use costly punishments. Still, the possibility to punish seems to discipline subjects.

equilibrium in the *stage* game. If *both* players' strategies stipulate that they report the cartel whenever one of them deviates unilaterally, then deviating is no longer profitable. Furthermore, costly public reports are credible: given that both players (including the deviating one) report the cartel following a deviation, both players are indifferent between reporting and not reporting. Thus reporting is an equilibrium in the reporting subgame. The weakness of this subgame perfect equilibrium is that the Nash equilibrium in the reporting subgame is in weakly dominated strategies. Yet, undominated strategies with the same flavor are constructed easily when the stage game is repeated infinitely (see the Appendix in Bigoni et al. (2009) for a proof of this claim).

We ran an additional treatment, NoReport, to test the hypothesis that the threat of public reports enforced high cartel prices in Fine. NoReport was identical to Fine except for the missing reporting possibility. The cartel prices in NoReport should be low if the public reports enforced the high cartel prices in Fine. On average cartel prices were 5.031 in Fine and 3.553 in NoReport, and this difference is significant at the 1% level.[20]

Result 7 (Cartel prices and public reports) The opportunity in Fine to punish defectors through costly public reports significantly increases cartel prices.

Result 7 suggests that subjects may have perceived the public reports as a credible threat. But it does not explain us why. Were the subjects so sophisticated that they understood the structure of such optimal punishments? Or did they use public reports to punish "altruistically", as often observed in public good experiments (Fehr and Gächter; 2000, 2002) and suggested by recent findings in the field of Neuroeconomics (de Quervain et al., 2004)? To discriminate between these two hypotheses, and in line with Fehr and Gächter (2002), we ran an additional treatment, ReMatch. The only difference from Fine was that subjects were paired with a new rival in every period.[21] In ReMatch, public reports were not credible unless subjects used them altruistically. Positive rates of

[20] Here we use only data collected in Rome, because NoReport (as well as the ReMatch treatment discussed below) was conducted only in Rome.

[21] ReMatch was a perfect stranger design so that two subjects were never paired twice, and the fixed number of periods was 25. This was emphasized in the instructions.

reports in ReMatch would thus suggest that subjects used public reports altruistically. These rates may even be larger in ReMatch than in Fine as price wars constituted an additional punishment tool in Fine; some reporting subjects in ReMatch could therefore have exchanged punishments through reports for price wars, had they participated in Fine instead. Provided that only one subject defected from the agreed upon price, the rates of public reports were indeed higher in ReMatch (0.324) than in Fine (0.197).

Result 8 (Public reports as altruistic punishments) Subjects used public reports as altruistic rather than optimal punishments.

Result 7 thus suggests that public reports can enforce high cartel prices, a finding consistent with ADS. This agreement with ADS may be viewed as puzzling, because reporting is costly in Fine, whereas in ADS's Standard treatment fines were costless for cheated upon subjects (as cheated upon subjects had no revenues). Result 8 resolves this puzzle by suggesting that even costly punishment may be credible as subjects appear willing to punish altruistically. Finally Result 7 also appears to explain why cartel prices were high in Fine and not in HS's Antitrust treatment; in the latter treatment, subjects were not allowed to report.

Enforcement effect in Leniency The high cartel prices in Leniency were probably not driven by the threat of public reports as punishments. Price deviations mostly were combined with simultaneous secret reports (See Table 3), effectively hindering the use of public reports as punishments. Yet our previous results are consistent with an enforcement effect. The post-conviction behavior documented earlier shows that price deviations combined with secret reports led to low post-conviction cartel formation rates, and thereby to long and costly price wars. As a result, subjects may have refrained from undercutting agreed-upon prices (as documented by the low rates of price deviations in Leniency) due to the threat of long and costly price wars. Interestingly, the rates of price deviation were higher in Leniency during periods when subjects communicated for the first time than on average when a cartel was formed (see Table 5). A possible interpretation of this pattern is that the enforcement effect in Leniency was more pro-

nounced when two subjects had already communicated once, particularly for cartels in which subjects initially stuck to the agreed upon price. Then trust may have emerged among subjects, perhaps enabling them to coordinate on even higher prices (as reflected in Table 5 both by the lower prices and the lower agreed upon prices during periods when subjects communicated for the first time than on average when a cartel was formed). Additional support for this interpretation obtains by looking at the profits of subjects who undercut the agreed upon price in the first cartel of a match, after having colluded for at least one period. We observe that their average profits in periods following the deviation are much lower in LENIENCY (118.8) than in FINE (159.5), and the difference is highly significant (p-value<0.001).

Unlike here, the experiments of ADS and HS yielded low cartel prices in their leniency treatments. This seems puzzling as ADS and HS only allowed for public reports after prices were revealed - which under leniency mainly work as costless punishments - whereas we also allowed for secret reports before prices were revealed, which encouraged price deviations with simultaneous reporting that removed the possibility to use the public report as a punishment. The divergence with ADS is probably explained by the fact that their subjects played a one shot game, so that the cartel prices in their sample reflected to a large extent price deviations and not prices charged repeatedly in successful cartels. A possible explanation for the divergence with HS is that their subjects were never rematched and thus competed with the same subjects throughout the experiment. Thereby, subjects in their sample may have been unable to overcome distrust generated by early price deviations and/or reports. By contrast, the subjects in our sample may have learned in early matches that price deviations combined with secret reports led to costly price wars and may therefore have tried other strategies (i.e. not deviate) in later matches.[22]

[22] Still, one may argue, the 'public' reports in HS should have disciplined cartel members. The reports in HS may however partly have worked as secret ones. In their design, only the subject pushing the report button first in a simultaneous reporting phase was granted leniency. Therefore an undercutting subject was probably more likely to be granted leniency as he/she was 'ready to push' and did not have to react upon new information on whether or not competitors had undercut the agreed upon price. Reports in HS may thus have been 'mixed', partly working as secret ones and partly as public ones. In this sense our design enabled us to distinguish secret and public reports without substantially increasing the incentives to deviate relative to HS.

Rewards

Although successful in deterring cartel formation, neither traditional (FINE) nor modern antitrust policies (LENIENCY) appear to reduce prices and increase welfare in our environment. This motivates the investigation of more powerful incentive schemes such as rewarding whistleblowers. Surprisingly the rates of communication attempts and of cartel formation reported in Table 4 are larger in REWARD than in LENIENCY (although insignificantly so for the latter rate). At first hand rewarding whistleblowers thus appears to at least weakly reduce deterrence. This finding contradicts Hypothesis 1 and appears in line with the one by ADS, albeit weaker (the rates of cartel formation in their *bonus* treatment were higher than in their *standard* treatment).

Despite the relatively poor performance of REWARD in terms of deterrence, the scheme nevertheless substantially and significantly increased cartel detection due to self reporting, both relative to FINE and LENIENCY. The rates of detection were indeed spectacular in REWARD as almost systematically at least one cartel member reported. In 118 out of the 120 cases a cartel was formed, it was reported *in the first period*. One of the remaining cartels was reported in the subsequent period. Only the subjects in the last cartel did resist the temptation to report, managing to collude successfully for the seven remaining periods of the match.

The subjects could exploit the reward system implemented in REWARD by communicating and taking turns in reporting and cashing in the reward.[23] Alternatively they may have formed cartels with the intent of fooling their competitor by undercutting the agreed upon price and simultaneously reporting the cartel so as to cash in the reward. Our experiment validates this latter hypothesis, initially proposed by ADS. In fact, no pair of subjects exploited the opportunity to take turns in reporting.[24] Instead, price deviations were immediate and frequent, significantly more frequent in REWARD than in

[23] The reward scheme is exploitable in the sense that the expected fine is 0 if cartel members take turns in self-reporting and cashing in the reward. Some practitioners have raised concerns that reward schemes could be exploited, although it is well known that it is always possible to design them non-exploitable by keeping rewards substantially below the sum of fines paid by other wrongdoers (see e.g. Spagnolo, 2004).

[24] This is consistent with Dal Bo's (2005) finding that efficient asymmetric (alternating) equilibria in a repeated prisoners' dilemma game are never played in the lab. This could change, of course, if subjects had available more open forms of communication than in our experiment, an interesting subject for future work.

L-Faire (although not reported in Table 5, the difference in the rates of price deviations between the two treatments is significant at the 1%-level).

This finding is all the more striking given that both Fine and Leniency instead reduced the frequency of price deviations relative to L-Faire (Result 6). Unlike Fine and Leniency, Reward thus destabilized cartels, leading to low prices, both within and outside cartels as well as on average. In particular, both cartel prices and prices on average were significantly lower in Reward than in L-Faire (although not reported in Table 5, these differences are significant at the 1%- and 5%-levels respectively). Thus Reward appears to be the only welfare enhancing policy in our experiment.

The puzzling contrast between the deterrence and price effects of Rewards disappears if we restrict the attention to cartels that sustain high prices at least in the first period (*successful cartels*), disregarding the somewhat implausible cases of subjects attempting to lure their opponent into a cartel only to then report and cash the bonus.[25] The rate of cartel formation is then also significantly lower in Rewards (0.017) than in Leniency (0.053), a difference significant at the 1% level. This indicates that if we exclude the implausible cartels only formed with the purpose of cashing in the prize, in Reward we almost achieve full deterrence, a possibility suggested by theory. [26]

To sum up, a clear picture emerges in Reward. As in ADS, most subjects formed cartels with the intent of fooling the competitor by simultaneously undercutting the agreed upon price and reporting the cartel so as to cash in the reward. If we disregard these cases, Reward leads to almost complete cartel deterrence. In any case, Reward leads to very low prices. The frequent price deviations substantially reduced cartel prices and, together with the systematic secret reports, likely generated distrust. The lower level of trust reduced post-conviction cartel formation and prices (see Figures 2 and 3), and weakened subjects' ability to collude tacitly. Reward thereby strongly reduced average prices relative to all other treatments emerging as the only strongly welfare-improving policy.

[25] We thank an anonymous referee for suggesting this analysis.

[26] Note that the reward is equal to about three periods of incremental profit from maximally colluding. The reward is therefore attractive but not excessively high. Yet it seems to have a powerful effect on behavior.

Additional result: Ineligibility for Cartel Ringleader

Under the US Corporate Leniency Policy, and unlike in the EU since the revision of the EU Leniency Notice in 2002, the cartel instigator (the ringleader) is ineligible for amnesty. Excluding the ringleader from the leniency program may increase deterrence – if firms wait for other firms to take the initiative of forming the cartel to keep the right to obtain leniency – or reduce it because ringleaders become more trustworthy for other cartel members reducing their incentives to rush to report. To evaluate the pros and cons of ringleader ineligibility, we ran one additional treatment. In our framework deterrence did not increase when the ringleader was ineligible for amnesty, but prices did. Excluding ringleaders from amnesty may thus reduce the effectiveness of leniency programs.[27] One important caveat, however, is that in our set up subjects competed in duopolies – the worst conceivable scenario for excluding the ringleader, as the ban leaves only one cartel member with the option to self-report. The incentive to "race to report" generated by the risk of somebody else reporting first is then eliminated by the ineligibility of the ringleader. Additional experimental research with more cartel members is needed to appropriately evaluate the effects of this policy.

6 Conclusions

Leniency policies are being introduced in more and more areas of law enforcement, though their effects on cartel formation and prices are hard to observe. This article reports results from a laboratory experiment designed to examine the effects of fines, leniency programs, and reward schemes for whistleblowers spontaneously reporting before an investigation is open on firms' decisions to form cartels (cartel deterrence) and on their price choices (welfare).

In our experiment traditional antitrust law enforcement without leniency has a significant deterrence effect (fewer cartels form), but also a pro-collusive effect (surviving cartels' prices grow) so that overall prices do not fall. This effect appears to be driven by agents'

[27] See our working paper Bigoni et al, (2009) for details.

strategic use of the law enforcement environment, and in particular of self-reporting and fines as punishment devices. Leniency programs further increase cartel deterrence, but also stabilize surviving cartels relative to a laissez-faire regime, so that welfare does not significantly increase. The reason appears to be subjects' anticipation that tacit collusion or a new cartel are much less likely after a price defection including self-reporting. When fines are used as rewards for self-reporting agents prices fall significantly and antitrust enforcement improves welfare.

As with any laboratory experiment, one has to be careful about which effects are likely to be of first order importance in reality and which are instead likely to be mainly a product of the laboratory environment. We believe that the threat of reporting as a punishment in the absence of leniency, the use of 'altruistic punishments' and the effects on tacit collusion are likely to be of second order importance for real world cartels, where ancillary sanctions (like disqualification) and the larger number of players should make such reports unattractive and tacit collusion difficult to sustain. On the other hand, we believe that the effects of leniency uncovered by our experiment, its ability to improve antitrust policy by reducing cartel formation and post-conviction prices are natural and likely to be relevant in reality. Similarly, the effectiveness of rewards in minimizing the pernicious effects of cartels on prices and welfare appears likely to be relevant also in the real world.

Our results also suggest that subjects are able to use antitrust law enforcement strategically up to a certain point, and that we should continue to evaluate its design both in terms of deterrence and price effects, as even when deterrence is achieved prices and welfare may not react in the intended direction. More experimental and empirical work in this area seems highly needed.

A Appendix

Data and empirical methodology

In each period, subjects had to take up to four types of decisions: (i) decide whether or not to communicate, (ii) determine an agreed upon price, (iii) choose a price and (iv) decide whether or not to report a cartel. These decisions yielded individual or duopoly-level data. For example, observations of a cartel being formed or being detected are duopoly-level data because they are identical for subjects belonging to the same duopoly. An attempt to communicate or a decision to undercut an agreed upon price are examples of individual level data.

The main challenge for testing differences across treatments lies in accounting for correlations between observations from the same individual, or from different individuals belonging to the same duopoly. In addition, the tests must also account for correlations among observations that result from potential session or cultural effects. To address this issue, we adopt multilevel random effect models. The following four- and five-level models are used to account for correlations between observations generated within the same duopoly:

$$y_{pdsc} = \beta_0 + \beta_1 TREAT_{pdsc} + \eta^{(2)}_{dsc} + \eta^{(3)}_{sc} + \eta^{(4)}_{c}, \quad (2)$$

$$y_{pidsc} = \beta_0 + \beta_1 TREAT_{pidsc} + \eta^{(2)}_{idsc} + \eta^{(3)}_{dsc} + \eta^{(4)}_{sc} + \eta^{(5)}_{c}. \quad (3)$$

The four-level model uses only duopoly-level data. A measurement occasion, p (one for each period), is nested in a specific duopoly, d, which in turn is nested in a session, s, and a city, c. $TREAT$ is a treatment dummy variable and equals 1 for one of the treatments and 0 for the other. $\eta^{(2)}_{dsc}$ is the second-level random intercept common to observations belonging to the same duopoly d in session s and in city c, $\eta^{(3)}_{sc}$ the third-level random intercept common to observations from the same session s in city c and $\eta^{(4)}_{c}$ the fourth-level random intercept common to observations from the same city c. Random intercepts are assumed to be independently normally distributed with a variance estimated through

our regression. The five-level model uses individual level data instead, so that there are two observations per period in a specific duopoly, one for each subject i in a duopoly.[28]

This model accounts for potential correlations among observations from the same duopoly. Observations from different duopolies may also be correlated however, because subjects participated in several duopolies. To address this problem, we also run several regressions using a single observation per individual and duopoly, adopting the following-four level random effect model:

$$y_{djsc} = \beta_0 + \beta_1 TREAT_{djsc} + \eta^{(2)}_{jsc} + \eta^{(3)}_{sc} + \eta^{(4)}_{c}. \tag{4}$$

In this case, a measurement occasion, d (one per subject and duopoly), is nested in a specific subject, j, which in turn is nested in a session, s, and a city, c. Note that this model does not account for possible correlations among (the two) observations belonging to the same duopoly. For this reason, we use only observations within a duopoly that can (reasonably) be viewed as independent. For example, as a measure for deterrence, we use only subjects' decision to attempt to communicate in the first period in a match. Similarly, as a measure for cartel prices, we use only the prices charged in the periods when two subjects communicated for the first time. These regressions can be viewed as a robustness check. In some cases, however, they also test for something different than when more observations from the same match are used. For example, using only subjects' attempts to communicate during the first period in a match in effect tests for *ex ante* deterrence only.

We run logit regressions to analyze the decisions to communicate and deviate and to test for the rates of cartel formation and detection, adopting instead linear regressions for prices and agreed upon prices. To estimate our models we use the GLLAMM commands in Stata (see Rabe-Hesketh and Skrondal, 2004 and http://www.gllamm.org).

[28] Adding a level substantially increases the time needed to run a regression. For this reason, we transform some individual level data into duopoly-level data. Specifically, we transform the individual price data into duopoly-level data by taking the average price charged by two subjects in a given period and duopoly as a single observation.

Experimental sessions

The table below provides additional details about each session: when and where they were conducted, the number of subjects in each session as well as the number of periods and matches.

[Table 6 approximately here]

References

Andersson, O. and Holm, H. "Endogenous Communication and Tacit Coordination in Market Entry Games - an Explorative Experimental study." *International Journal of Industrial Organization*, Vol. 28 (2010), pp. 477-495.

Andersson, O. and Wengstrom, E. "Do Antitrust Laws Facilitate Collusion? Experimental Evidence on Costly Communication in Duopolies." *Scandinavian Journal of Economics*, Vol. 109 (2007), pp. 321-339.

Apesteguia, J., Dufwenberg, M. and R. Selten. "Blowing the Whistle." *Economic Theory*, Vol. 31 (2007), pp. 143-166.

Bigoni, M., Fridolfsson, S., Le Coq, C. and Spagnolo, G. "Fines, Leniency and Rewards in Antitrust: an Experiment", CEPR Discussion Paper no. 7417. London, Centre for Economic Policy Research, 2009.

Brenner, S. "An Empirical Study of the European Corporate Leniency Program", *International Journal of Industrial Organization*, Vol. 27 (2009), pp. 639-645.

Cachon, J. and Camerer, C. "Loss-Avoidance and Forward Induction in Experimental Coordination Games." *The Quarterly Journal of Economics*, Vol. 111 (1996), pp. 165-194.

Cooper, D. J. and Kuhn, K. U. "Communication, Renegotiation, and the Scope for Collusion." Mimeo, Florida State University, 2010.

Crawford, V. "A Survey of Experiments on Communication via Cheap Talk." *Journal of Economic Theory*, Vol. 78 (1998), pp. 286-98.

Crawford, V. and Broseta, B. "What Price Coordination? The Efficiency-enhancing Effect

of Auctioning the Right to Play." *American Economic Review,* Vol. 88 (1998), pp. 198-225

Dal Bó, P., and Fréchette, G. "The Evolution of Cooperation in Infinitely Repeated Games: Experimental Evidence." *American Economic Review*, Vol. 101 (2011), pp. 411-429.

Dreber, A., Rand, D.G., Fudenberg, D. and Nowak, M.A. "Winners Don't Punish." *Nature*, Vol. 452 (2008), pp. 348-351.

Ellis, C. and Wilson, W. "Cartels, Price-Fixing, and Corporate Leniency Policy: What Doesn't Kill Us Makes Us Stronger." Manuscript, University of Oregon, 2001.

Fehr, E. and Gächter, S. "Cooperation and Punishment in Public Goods Experiments." *American Economic Review,* Vol. 90 (2000), pp. 980-994.

Fehr, E. and Gächter, S. "Altruistic punishment in humans." *Nature* Vol. 415 (2002), pp. 137-40.

Fischbacher, U. "Z-Tree: Zurich Toolbox for Ready-made Economic Experiments." *Experimental Economics,* Vol. 10 (2007), pp. 171-178.

Hamaguchi, Y., Ishikawa, T., Ishimoto, M., Kimura, Y. and Tanno, T. "An Experimental Study of Procurement Auctions with Leniency Programs." Competition Policy Research Center Discussion Paper Series, CRDP-24-E, 2007.

Hamaguchi, Y., Kawagoe, T. and Shibata, A. "Group Size Effects on Cartel Formation and the Enforcement Power of Leniency Programs". *International Journal of Industrial Organization*, Vol. 27 (2009), pp. 145-165.

Harrington, J. "Optimal Corporate Leniency Programs". *The Journal of Industrial Economics*, Vol. 56 (2008), pp. 215-246.

Harrington, J., and Chang, M. "Modelling the Birth and Death of Cartels with an Application to Evaluating Antitrust Policy", *Journal of the European Economic Association*, Vol. 7 (2009), pp. 1400-1435.

Hinloopen, J. and Soetevent, A. R. "Laboratory evidence on the effectiveness of corporate leniency programs." *RAND Journal of Economics*, Vol. 39 (2008), pp. 607-616.

Holt, C.A. "Industrial Organization: A Survey of Laboratory Research", n *The Handbook of Experimental Economics*, J. H. Kagel & A.E.Roth (eds.), Princeton University Press, 1995.

Krajkova, J. and Ortmann, A. "Testing Leniency Programs Experimentally: The Impact of Change in Parameterization." Working Paper, CERGE-EI, Prague, 2009a.

Krajkova, J. and Ortmann, A. "Testing Leniency Programs Experimentally: The Impact of a 'Natural' Framing." Working Paper, CERGE-EI, Prague, 2009b.

McCutcheon, B. "Do Meetings in Smoking-Filled Rooms Facilitate Collusion?" *Journal of Political Economy,* Vol. 105 (1997), pp. 330-350.

Miller, N. "Strategic leniency and cartel enforcement." *American Economic Review.*, Vol. 99 (2009), pp. 750-68.

Motta, M., and Polo, M. "Leniency Programs and Cartel Prosecution". *International Journal of Industrial Organization*, Vol. 21 (2003), pp. 347-379.

Offerman, T., and Potters, J. "Does Auctioning of Entry Licenses Induce Collusion? An Experimental Study." *Review of Economic Studies* Vol. 73 (2006), pp. 769-791.

de Quervain, D. J.-F., Fischbacher, U., Treyer, V., Schellhammer, M., Schnyder U., Buck, A. and E. Fehr. "The Neural Basis of Altruistic Punishment." *Science* Vol. 305 (2004), pp. 1254-1258.

Rabe-Hesketh, S. and Skrondal, A. *Generalized Latent Variable Modeling: Multilevel, Longitudinal and Structural Equation Models.* Boca Raton, FL: Chapman & Hall/CRC, 2004.

Rey, P. "Towards a Theory of Competition Policy," Chapter 3 of *Advances in Economics and Econometrics: Theory and Applications*, ed. M. Dewatripont, L.P. Hansen S.J. Turnovsky, Cambridge University Press, 2003.

Spagnolo, G. "Self-Defeating Antitrust Laws". F.E.E.M. Working Paper 52, 2000.

Spagnolo, G. "Divide et Impera: Optimal Leniency Programs". CEPR Discussion Paper No. 4840, 2004.

Spagnolo, G. "Leniency and Whistleblowers in Antitrust", in Buccirossi P. (Ed.), *Handbook of Antitrust Economics*, MIT Press, Cambridge, MA, 2008.

Sproul, M F. "Antitrust and Prices." *Journal of Political Economy*, Vol. 101 (1993), pp. 741-54

Van Huyck, J.B. and Battalio, R.C. "Asset Markets as an Equilibrium Selection Mechanism: Coordination Failure, Game Form Auctions, and Tacit Communication." *Games and Economic Behavior*, Vol. 5 (1993), pp. 485-504.

Whinston, M. D. *Lectures on Antitrust Economics.* MIT Press, 2006.

Tables

		your competitor's price												
		0	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	**11**	**12**
your price	**0**	0	0	0	0	0	0	0	0	0	0	0	0	0
	1	29	38	47	56	64	68	68	68	68	68	68	68	68
	2	36	53	71	89	107	124	128	128	128	128	128	128	128
	3	20	47	73	100	127	153	180	180	180	180	180	180	180
	4	0	18	53	89	124	160	196	224	224	224	224	224	224
	5	0	0	11	56	100	144	189	233	260	260	260	260	260
	6	0	0	0	0	53	107	160	213	267	288	288	288	288
	7	0	0	0	0	0	47	109	171	233	296	308	308	308
	8	0	0	0	0	0	0	36	107	178	249	320	320	320
	9	0	0	0	0	0	0	0	20	100	180	260	324	324
	10	0	0	0	0	0	0	0	0	0	89	178	267	320
	11	0	0	0	0	0	0	0	0	0	0	73	171	269
	12	0	0	0	0	0	0	0	0	0	0	0	53	160

Table 1: **Profits in the Bertrand game**

Table 2: **Treatments**

Treatment	**fine** (F)	**probability of detection** (α)	**report**	**report's effects**
L-Faire	0	0	No	–
Fine	200	0.10	Yes	pay the full fine
Leniency	200	0.10	Yes	no fine (half the fine if both report)
Reward	200	0.10	Yes	reward (half the fine if both report)

Table 3: **Self reporting**

	Fine	Leniency	Reward
Rate of Secret Reports (given an own price deviation)	0.002	0.704	.905
Rate of Public Reports (given only the rival deviated)	0.286	0.481	0.333

Note: the rates of secret reports (given an own price deviation) is the fraction of cartel members that made a secret report, provided they undercut the agreed upon price in the same period. The rates of public reports (given only the rival deviated) is the fraction of cartel members that made a public report, provided only the rival deviated without simultaneously making a secret report.

Table 4: **Cartel deterrence and detection**

	L-Faire		**Fine**		**Leniency**		**Reward**
Rate of comm. att.	0.835	$>^{***}$	0.566	$>^{***}$	0.377	$<^{***}$	0.484
Rate of cartel formation	0.716	$>^{***}$	0.315	$>^{***}$	0.178	$\approx$	0.220
Rate of comm. att. (1^{st} period)	0.925	$>^{***}$	0.684	$\approx$	0.437	$<^{*}$	0.481
Rate of reporting	–	–	0.092	$<^{***}$	0.507	$<^{***}$	0.937
Rate of reporting (1^{st} comm.)	–	–	0.136	$<^{***}$	0.761	$<^{***}$	0.983
Incidence of cartels	0.961	$>^{***}$	0.583	$>^{***}$	0.264	$\approx$	0.230

Note: In this and the following table, ***, ** and * indicate significance at the 1%, 5% and 10% levels. The **Rates of communication attempts** are computed using the binary individual decisions to communicate in all periods a cartel was not already formed (or in the first period in a match). The **Rates of cartel formation** are computed using a single observation per duopoly and period, indicating if a cartel was formed in that period. The **Rates of reporting** are computed provided that a cartel was formed, using a single observation per duopoly and period, indicating if a cartel was detected in that period because one or both subjects reported the cartel. The **Rates of reporting** during the first period two subjects communicated in a match are computed using the reporting decisions of each subject as a single observation. The **Incidence of cartels** is computed as the average per-period ratio of the number of cartels over the number of duopolies, using a single observation per duopoly and period. The differences across treatments are tested using multilevel random intercept logit regressions, as outlined in Appendix A.

Table 5: **Prices, agreed upon prices and price deviations**

	L-Faire		Fine		Leniency		Reward
Average price	4.917	$<^{*}$	5.349	$>^{***}$	4.845	$>^{*}$	3.973
Cartel price	4.971	$<^{***}$	6.144	$<^{***}$	7.024	$>^{***}$	5.339
Prices outside cartels	3.5	$<^{**}$	4.233	$\approx$	4.063	$\approx$	3.567
Agreed upon price	7.689	$<^{***}$	8.242	$\approx$	8.218	$\approx$	8.512
Rate of price dev.	0.564	$>^{***}$	0.424	$\approx$	0.373	$<^{***}$	0.782
Cartel price (1^{st} comm.)	5.929	$<^{***}$	6.990	$>^{***}$	6.663	$>^{***}$	5.483
Agreed upon price (1^{st} comm.)	7.881	$<^{***}$	8.129	$>^{*}$	7.886	$\approx$	8.100
Rate of price dev. (1^{st} comm.)	0.590	$>^{***}$	0.408	$\approx$	0.443	$<^{***}$	0.717

Note: the point estimates for the different price measures are computed using the average among the prices chosen in a period by the two members of a duopoly. **Average prices** are computed using all observations, whereas **average prices within (outside) cartels** only uses observations when a cartel is formed (not formed). **Average agreed upon prices** are computed using observations when subjects actually communicated. To test for differences across treatments, we run multi-level random intercept linear regressions as outlined in Appendix A. The average **cartel price** during the periods when two subjects communicated for the first time is computed and tested using individual price data. The **Rates of price deviations** are computed using the binary individual decisions to undercut the last agreed upon price, provided that no subject has not yet undercut that price. Differences across treatments are tested using a five level random intercept logit regressions, as outlined in Appendix A. We also check the robustness of our results using only observations from the first period two subjects communicated. In this case we run four level random intercept logit regressions, as outlined in Appendix A.

Treatment and date	City	N. Subjects	N. of Periods	N. of Matches
L-Faire				
26/03/2007	Stockholm	16	29	4
30/05/2007	Rome	32	23	4
07/11/2007	Stockholm	22	23	4
Fine				
26/03/2007	Stockholm	16	22	2
31/05/2007	Rome	32	26	6
09/11/2007	Stockholm	24	21	4
09/11/2007	Stockholm	22	23	3
Leniency				
28/03/2007	Stockholm	18	26	1
04/06/2007	Rome	32	25	2
08/11/2007	Stockholm	18	24	4
08/11/2007	Stockholm	14	27	5
Reward				
29/03/2007	Stockholm	16	23	4
12/12/2007	Rome	32	23	3
RingLeader				
08/06/2007	Rome	32	22	3
ReMatch				
13/12/2007	Rome	32	25	25
NoReport				
14/12/2007	Rome	32	27	5

Table 6: Sessions and treatments.

Figure 1: Timing of the stage game

Figures

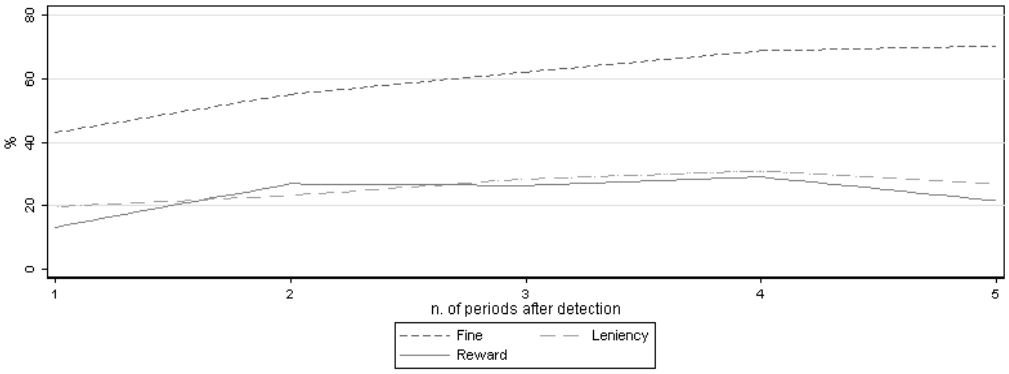

Figure 2: % of cartels re-established

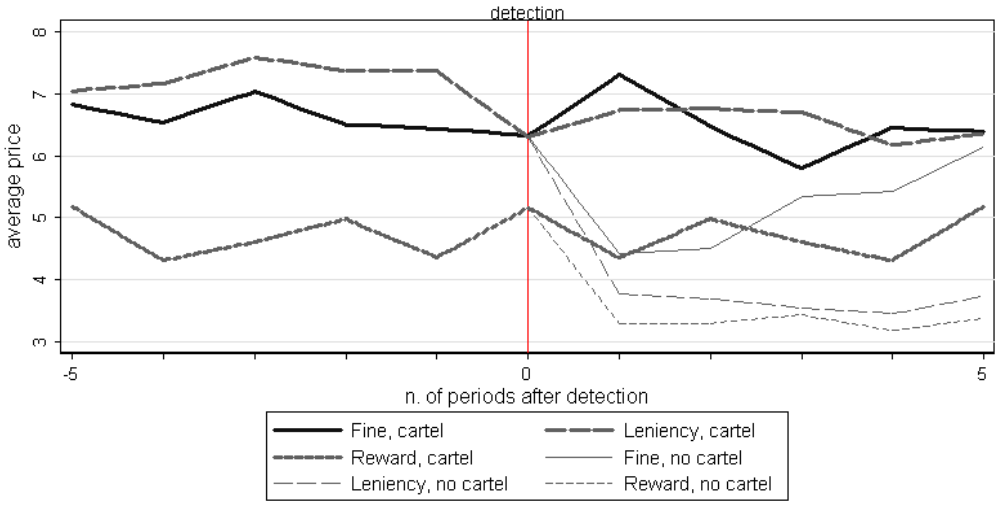

Figure 3: Price before and after detection