Fines, Leniency, Rewards and Organized Crime: Evidence from Antitrust Experiments*

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Abstract

Leniency policies and rewards for whistleblowers are being introduced in ever more fields of law enforcement, though their deterrence effects are often hard to observe, and the likely effect of changes in the specific features of these schemes can only be observed experimentally. This paper reports results from an experiment designed to examine the effects of fines, leniency programs, and reward schemes for whistleblowers on firms' decision to form cartels (cartel deterrence) and on their price choices. Our subjects play a repeated Bertrand price game with differentiated goods and uncertain duration, and we run several treatments different in the probability of cartels being caught, the level of fine, the possibility of self-reporting (and not paying a fine), the existence of a reward for reporting. We find that fines following successful investigations but without leniency have a deterrence effect (reduce the number of cartels formed) but also a pro-collusive effect (increase collusive prices in surviving cartels). Leniency programs might not be more efficient than standard antitrust enforcement, since in our experiment they do deter a significantly higher fraction of cartels from forming, but they also induce even higher prices in those cartels that are not reported, pushing average market price significantly up relative to treatments without antitrust enforcement. With rewards for whistle blowing, instead, cartels are systematically reported, which completely disrupts subjects' ability to form cartels and sustain high prices, and almost complete deterrence is achieved. If the ringleader is excluded from the leniency program the deterrence effect of leniency falls and prices are higher than otherwise. As for tacit collusion, under standard anti-trust enforcement or leniency programs subjects who do not communicate (do not go for explicit cartels) tend to choose weakly higher prices than where there is no anti-trust enforcement. We also analyze post-conviction behavior, finding that there is a strong expost deterrence (desistance) effect. Moreover post-conviction prices are on average lower than before even though the average prices within cartels are the same. Finally, we find a strong cultural effect comparing treatments in Stockholm with those in Rome, suggesting that optimal law enforcement institutions differ with culture.

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1 Introduction

The last decade has witnessed major innovations in the law enforcement against price cartels. Following the US example, leniency programs that reduce sanctions for cartel members that self-report to the competition authority have been introduced in most OECD countries and have become the main tool for cartel discovery and prosecution.[1] The European Competition Network, a forum including all European competition authorities also launched a "model" for the design of effective leniency programs.[2] In some jurisdictions (e.g. Korea) reward schemes for whistle-blowers that report a cartel have also been adopted, following their successful use in the fight of government fraud (US False Claim Act) and tax evasion.[3]

The introduction of leniency programs increased dramatically the number of cartels detected and convicted in the US and the EU, and this is why they are considered a tremendous success (see Spagnolo 2006 for details). A higher number of detected and prosecuted cases, however, is not always a good indicator of the effectiveness of Anti-trust policies.[4] For example, an extremely lenient policy that reduces fines to almost all parties of a discovered cartels in exchange for information will enormously facilitate prosecution and generate many spontaneous reports. Such a policy could well make a competition authority famous as a successful agency, but is likely to heavily damage society by at the same time (a) encouraging cartel formation through the drastic reduction in expected fines that such a overly lenient policy generates, and (b) increasing the cost of prosecution (by the higher number of prosecuted cartels, given that prosecution costs are a pure deadweight losses for society). Law enforcement's main objective is crime deterrence, i.e. prevention. An efficient and successful antitrust policy against cartels should have tough ex ante deterrence effects that keep low both the costs of prosecution and those of price fixing activities.

The purpose of this paper is to examine the deterrence effects of antitrust fines, leniency programs, and reward schemes for whistle-blowers. In particular we focus on how monetary fines, leniency programs, and reward schemes against price fixing cartels affect market participants' decision to form cartels (cartel deterrence) and their price choices. We also analyze how different design of antitrust policy may affect firms' ability to enter in tacit collusive agreements instead, since as Whinston (2006) recently reminded us, the final objective of competition policy is to keep prices at competitive level, not to deter explicit horizontal agreements per se. The main questions raised in this paper are, therefore, the following:

1. What are the effects of traditional antitrust law enforcement, fines following successful investigations of the competition authority but no leniency, on cartel formation and on pricing behavior in and outside the formed cartels?

2. What are the effects of introducing a leniency program when reporting the cartel? Does it make a difference if the report is secret or not? Do things improve when expected fines are higher, or when the ringleader is banned from leniency as in the US (but not in the EU)? Is the possibility to report used as improved opportunity to undercut the cartel, or as a threat to punish defectors and thereby stabilize cartels, or both?

3. What are the effects of rewarding the first party that applies for leniency with a bonus equal to the fines paid by the co-conspirators in that cartel? Also, do agents exploit the reward scheme taking turn in reporting and cashing the reward when the scheme is too generous and makes this a profitable option?

4. What are the effects of these different law enforcement instruments on agents' choice of collusive price and on their ability to sustain *tacit* collusion, given the importance of this issue in the recent debate?

The big problem with optimizing law enforcement policy by looking at its real world performance is that for cartels and analogous forms of organized crime (fraud, corruption, earning management, etc.) there

[1] The Antitrust Division of the US DoJ has had a leniency programs for cartels since the seventies but reformed the program in 1993 and 1994, introducing the novel Corporate and Individual Leniency Policy, and later on introduced the Amnesty Plus scheme. Analogously, the EC's DG Comp introduced a first Leniency Notice in 1996, and revised it in 2002.

[2] The ECN leniency "model" is substantially more lenient than the US program, as it allows to partially reduce fines practically to all members of a cartel, while the US leniency program is restricted to the first party that reports only.

[3] In December 2006 The US Congress strengthened the legislation that already allowed whistleblowers to cash as rewards 15 to 30% of taxes and fines recovered by the IRS thanks to their help, by making the payment of the reward almost automatic. A change that, by reducing the agency discretion on the payment of the reward, resembles the 1993 change in the US Corporate Leniency Program.

[4] This was often mentioned in the lively debate on the effectiveness of antitrust enforcement (see e.g. Crandall and Winston 2003, Baker 2003, Kwoka 2003, and Werden 2003, among others).

is precious little else to look at than discovered and prosecuted cases. Contrary to most other types of crimes, where there are conscious victims that denounce and thereby signal the frequency of the crime independently from the fraction of these crimes where the criminal is detected and convicted, victims of cartels and analogous forms of organized crime (corruption, fraud, etc.) are mostly not aware of them. This implies that we cannot directly observe the total population of cartels in society and how this changes with the introduction of new policies, though indirect methods offer partial indications (see e.g. Harrington, 2006; Miller, 2007).[5] This intrinsic lack of observability, accompanied by the fact that many design features of the proposed and theoretically analyzed schemes have never been implemented in reality, makes experimental investigation a crucial policy tool, an almost unique possibility to try measure the likely change in deterrence, prices and welfare caused by the many different possible designs of law enforcement policies.

We consider an experimental framework, as close as possible to the strategic situation agents face in an oligopolistic industry subject to current antitrust laws, in which subjects play a repeated Bertrand price game with differentiated goods. Subjects can decide to coordinate on price (and thus they form a cartel). We consider several treatments different in the probability of cartels being caught, the level of fine, the possibility of self-reporting (and not paying a fine), the existence of a reward for reporting. We are not the first to look at these issues experimentally. Apesteguja, Dufwemberg and Selten (2007) and Hinloopen and Soetevent (2006), for example, have already produced instructive pieces of work in this direction. However, as will be explained in depth in the next section discussing the literature, we found that both those previous experiments could be further improved in one way or another, and that they do not cover most of the important policy issues we wanted to deal with in our experiment. In particular we find new results on secret reports, reward schemes, the interaction between fines, leniency and deterrence and tacit collusion.

We found that traditional antitrust law enforcement, fines following successful investigations of the competition authority and no leniency, has a deterrence effect (reduces the number of cartels formed) but also has a pro-collusive effect (increases collusive prices). Leniency programs might not be more efficient than standard antitrust enforcement, since they do deter a significantly higher fraction of cartels from forming, but they also induce higher prices in cartels that are not reported. If the ringleader is excluded from the leniency program, as under the US leniency policy, the deterrence effect of leniency falls and prices are higher than otherwise. With rewards for whistle blowing, instead, cartels are systematically reported, disrupting completely subjects' ability to form cartels and to sustain high prices. Also, we find that when the reward scheme is 'wrongly designed' in the sense that can be exploited, in our case by completely eliminating the risk of being fined at no cost, subjects do not recognize the possibility to manipulate and gain from the scheme, a result in line with recent experiments in other fields (see e.g. Dal Bo, 2005). We also analyze post-conviction behavior, finding that there is a strong ex post deterrence (desistance) effect. Moreover post-conviction prices are on average lower than before even though the average prices within cartels are the same. If the ringleader is excluded from the leniency program, as under the US leniency policy, the deterrence effect of leniency falls and prices are higher than otherwise. As a first step for analyzing tacit collusion, we focus on subjects's behavior when they do not communicate (do not go for explicit cartels). We find that under standard antitrust enforcement or leniency programs, subjects who do not communicate choose significantly higher prices than where there is no anti-trust enforcement whatsoever. This is not the case anymore when reward schemes are introduced. Finally, we find a strong cultural effect comparing treatments in Stockholm with those in Rome, suggesting that optimal law enforcement institutions differ with culture.

The remainder of this paper is organized as follows. The next section discusses related literature, theoretical and experimental. Section 3 describes the underlying theoretical model and the experimental design, contrasting it with previous ones. Section 4 presents our results. Section 5 concludes, discussing implications for the theory and practice of designing deterrence mechanisms for cartels and similar forms of organized crime. An appendix contains the instructions for the experiment.

[5] Harrington (2006) develops a smart indirect method to estimate the likely changes in deterrence caused by the introduction of a new law enforcement instrument based on the observed changes in duration of the detected cartels. Miller (2007) further develops the approach and applies it to cartels detected in the US in the last decades, finding positive deterrence effects of cartel formation consistent with those we observe in our experiment. Unfortunately this work appears not to offer results or implication regarding price and welfare changes, which as our results show may not go in the expected direction (Sprouls 1993 offers empirical evidence that prices weakly increase after antitrust conviction, which is also consistent with our experimental results).

2 Literature review

2.1 Theory

Starting with the contributions of Motta and Polo (2003), Rey (2003), and Spagnolo (2000a,b), a theoretical literature has blossomed in the last decade that analyzes the optimal design of anti-cartel policies based on the provision of incentives to breach trust and to self-report.[6] Different effects of leniency and rewards are considered in this literature. The focus here is on the deterrence effects of the first part of the leniency policies, restricted to firms that self-report before an investigation by the competition authority has invested them. The most important effects identified by the literature in this respect are:

1. *The protection from fines effect.* Spagnolo (2000a, 2004) and Rey (2003) suggest that amnesty offered to the first firm reporting before an investigation is open may have deterrence effects by ensuring that if a cartel member wants to undercut the cartel, it can report and avoid paying the fine.[7]

2. *The reward effect.* Spagnolo (2000a, 2004), Buccirossi and Spagnolo (2001, 2006), Rey (2003) and Aubert et al. (2006) suggest that rewards could further increase deterrence by generating stronger temptations to undercut the cartel and cash the reward by reporting. Spagnolo (2000a, 2004) shows that such a mechanism can for the first time deliver the first best in a model a la Becker (1968), complete deterrence without investigation costs, provided that fines are sufficiently but finitely large, and that the reward is lower than total fines.

3. *The 'reporting as a threat' and 'what does not kills us makes us stronger' effects.* Spagnolo (2000b) and Buccirossi and Spagnolo (2001, 2006) show that when self-reporting becomes attractive thanks to leniency programs, the *threat* of self-reporting to punish an agent that did not behave as the cartel agreed upon may also become credible, and may be exploited to enforce cartels that would not be sustainable otherwise. Building on this idea, Ellis and Wilson (2001) obtain a related effect, showing that, for cartels that are not deterred, leniency programs have the effect of reinforce/stabilize collusion. The reason is that if a cartel is formed, then leniency induces cartel members to self-report after any defection from agreed collusive strategies, thereby strengthening the punishment for defections of an amount equal to antitrust fines.

4. *Tacit collusion and post-conviction pricing.* Antitrust doctrine agreed in the 50s that the focus should be restricted to 'explicit cartels', i.e. to conspiracies where firm managers meet or communicate with the explicit objective of coordinating on higher prices, and leave alone tacit collusion, i.e. cases where firms manage to coordinate on and sustain high prices without explicit communication. Whinston (2006) reopened the debate, arguing that what is important for welfare are prices, so that we should reflect more on how antitrust enforcement may affect firms' ability to sustain prices, even when high prices are sustained by *tacit* collusion. On a different but related stance, Buccirossi and Spagnolo (2007) suggest that antitrust fines might have the effect of inducing firms to increase collusive prices following conviction, either because they do not realize they are a 'sunk cost' and try to recover them through higher margins, or because paid fines may help firms coordinating on higher post-conviction prices sustained by tacit collusion.

We discuss such effects when we present the experimental results.

2.2 Experiments

Apesteguja, Dufwemberg and Selten (2007), and Hinloopen and Soetevent (2006) are the first to analyze experimentally the effects of leniency policies on cartel deterrence.[8]

[6] Other early pieces include Aubert et al. (2006), Buccirossi and Spagnolo (2001, 2006), Ellis and Wilson (2001), Harrington (2006) and Harrington and Chen (2007). See Spagnolo (2006) for a review of this growing theoretical literature.

[7] More recently Harrington (forthcoming) coined a perhaps nicer acronym for the same effect, *deviator amnesty effect*, but here we stick to temporal priority.

[8] We are aware of two other studies that deal with not exactly the same issues but are somewhat related. Hamaguchi and Kawagoe (2005) design an experiment where subjects are forced to collude. Most obviously, such a setup cannot address the

2.2.1 Apesteguja, Dufwemberg and Selten (2007)

Apesteguja et al. (2007) conducted the first experimental investigation of the effects of Leniency policies and rewards schemes on cartel deterrence. This elegant paper first develops a stylized but static theoretical framework that tries to capture the main points made in the theoretical literature mentioned earlier on the general deterrence effects of leniency policies, and then uses it to undertake an experimental analysis of these effects. The market game they focus on is a one-shot homogeneous discrete Bertrand oligopoly. This is embedded in four alternative legal frameworks: in *Ideal* there is no antitrust law, cartels are not possible (communication is not allowed), and colluding firms face neither full nor reduced fines; in *Standard* convicted firms face fines equal to 10% of their revenue and no reduction if they report; in *Leniency* firms that report a cartel they took part in receive a reduction in their fine; in *Bonus* reporting firms receive part of the fines paid by other firms as a reward. Strategically equivalent collusive subgame perfect equilibria, including one implementing the monopoly price, exist in both *Standard* and *Leniency*, sustained by the threat of reporting if a defection takes place. The reason is that if a firm defects in an homogeneous Bertrand game, its opponent will have no revenue, so even if there is no leniency self-reporting is costless for a party whose opponent defected and is therefore a credible threat that can sustain collusion in the one-shot game.

The experimental settings allows for pre-play communication and let subjects play in groups of three, and for the rest it follows closely the theoretical model just described.

The experimental results confirm that agents understand and use the threat of reporting to sustain collusion in the one-shot Bertrand game: prices are substantially higher in *Standard* and in *Leniency*, where collusive equilibria exist sustained by the threat to self-report if cheated upon, than in *Ideal* where no such threat is available and the only equilibrium is the Bertrand one. *Leniency* has a significant deterrence effect relative to Standard, although prices are much higher than in *Ideal*, without any antitrust. Surprisingly, the experimental results are inconsistent with the theoretical predictions that rewarding reporting firms should reduce cartel formation: the *Bonus* treatment has non-significant effects on collusion.

As also argued by the authors, this paper can be seen as a first exploratory step in the experimental analysis of cartel deterrence mechanism. The reason is that both theory and experiment make a number of simplifications that may affect the result in a non trivial way.

First, the game and experiment allow for only one round of decisions, leaving experimental subjects no way to learn. This may be a problem for the interpretation of the experimental results. The equilibria agents are choosing among in *Standard* and *Leniency*, and the difference with *Bonus* are not that easy to understand. Most recent experimental studies of one-shot games allow for some repetition precisely because it is well known from earlier work on public goods games that the first decisions are typically mistaken. In fact, it is possible that the surprising result on the ineffectiveness of *Bonus* is driven by subjects not fully grasping the situation.

In our experiment we try to improve on this point by having both a repeated game, and five initial rounds for subjects to experiment the game.

Second, the theoretical framework used for the experiment resembles closely that in Spagnolo (2000b), but for fines chosen equal 10% of firms' revenue. In this case, even without leniency programs in place (like in the *Standard* treatment), reporting becomes a credible threat. If a partner-cartelist undercuts then reporting is costless since the revenue on the punishment path is zero. In reality there is 10% revenue cap for EU fines but it is relative to firms' total yearly turnover in the last period the cartel is active. In an appropriately dynamic framework, therefore, fines would never be zero because of a defection. Moreover, it is hard to imagine a market where, if a firm undercuts the cartel, other firms have zero revenue for one full year. Firms are active in many markets and total business stealing appears impossible in reality, so that absent leniency policies, a firm that reports a cartel would always be subject to a positive fine. Given that reporting is costly, the multiplicity of equilibria in *Standard* disappear as after a defection reporting is dominated by not doing it (and avoiding the fine), and *Leniency* may then fare much worse than how depicted. Moreover, homogeneous good Bertrand competition is a degenerate case of price competition

issue of how different policies perform in terms of cartel deterrence. Hamaguchi et al. (2007) adapt the setup of Hinloopen and Soetevent but to a repeated procurement auction with leniency programs. They consider a different game since there is only one winner at each period, so when players are colluding, they have to decide who will win the auction. They found evidence of deterrence effects with Leniency programs as well as higher prices under leniency and antitrust than under communication.

with differentiated products, and the collusive equilibria in Standard would disappear with a little product differentiation.

To improve on these points, in our experiment we chose an infinitely repeated differentiated product Bertrand game, and fixed fines rather than fines that go to zero for some price choices.

2.2.2 Hinloopen and Soetevent (2006)

Hinloopen and Soetevent (2006) study experimentally general deterrence effect but also desistance effect of Leniency Programs. They use an infinite horizon set up that allows for communication before prices are chosen each period, and where the stage game is the same homogeneous Bertrand game used by Apesteguja et al. (2007). Subjects are matched in groups of three at the beginning of each treatment, and then play without re-matchings for at least 20 rounds, after which the continuation probability falls to 80%.

They embedded these oligopoly games in four different treatments: *Benchmark*, where subjects cannot communicate; *Communication*, where subjects can communicate before choosing prices; *Antitrust*, where subjects that communicated are exposed to a positive probability of being detected and fined; and *Leniency*, which differs from *Antitrust* by the possibility to self-report after the choice of price and before the random audit by the competition authority. In *Leniency*, therefore, subjects can only self-report after prices have been chosen and made public, so that subjects cannot both secretly report and secretly undercut the collusive price, as is possible in reality where competition authorities may keep the report secret to arrange for dawn raids allowing (or even asking to) the reporting firm the possibility to secretly undercut former cartel partners. Fines are also equal to 10% of revenue in the period of conviction, but there is a fixed cost of reporting under the leniency program. Such feature destroys Apesteguja et al.'s (2007) one-shot collusive equilibria sustained by the threat of reporting after defections.

These authors' main findings are that leniency: (i) increases cartel deterrence (fewer cartels are formed); (ii) reduces the duration of cartels that are not deterred (agents that form a cartel defect more afterwards); and (iii) makes agents defect more aggressively than in the absence of the Leniency Program. They do not find that the leniency program affects the likelihood that a detected and fined cartel forms again thereafter (no effects on recidivism).

Although Hinloopen and Soetevent (2006) is the experimental work which is closest to what we do, our experiments differ from theirs in several respects.

First, in each stage-game subjects can both self-report and set prices before any of these choices is observed by other subjects. This implies that subjects have the possibility to simultaneously secretly report and defect/undercut cartel partners, much like in reality, and then they will have also the possibility to self-report after observing price choices, if nobody reported before price became public. We consider this a major improvement towards the realism of the experimental set up that allows agents to defect from the cartel and avoid fines, as possible in reality. This allows us to disentangle and quantify reports linked to defections (the 'protection from fines' effect discussed in section 2.1) and reports that are made to punish defections from the cartel (the 'reporting as a threat' or 'what does not kill us makes us stronger' effects discussed in section 2.1). Note also that, as it is the case in reality, in our experimental design reporting to the Antitrust authority is always possible even if this leads to no reduction in fines.

Second, we have fixed fines. The main reason is that when fines are equal to 10% of the revenue of the period in which cartel is detected, as it is the case for the others two mentioned experimental studies, fines vary a lot with the outcome of the stage-game in which a cartel happens to be detected, so that it is not clear what the expected fine perceived by subjects actually is, which makes it impossible to cleanly analyze the role of fines and their interaction with leniency.

Third we follow Apesteguja et al. (2007) in framing the experiment explicitly as a cartel/antitrust game, rather than having a "neutral" frame as in Hinloopen and Soetevent, as we want to make sure that subjects do not misunderstand the situation, and we want to minimize the possible impact of social preferences on subjects' choices.

Fourth, we use a perhaps more realistic oligopoly model, a repeated differentiated product Bertrand game.

Fifth our subjects are rematched with positive and constant probability all along the treatments, so that

each supergame has a constant continuation probability, like in Dal Bó (2005), Dal Bó and Frechette (2007), and Blonski et al. (2007).

Sixth, we use duopolies rather than triopolies, to avoid that agents may be unwilling to punish defections too hard by the unwillingness to harm a third 'innocent' (non-defecting) party, as suggested by Holt (1995).

In addition, and again differently from Hinlopen and Soetevent, we consider other important effects, in particular the effects of rewards for whistle blowers. We are able, in an appropriately dynamic environment, to test the robustness of Apesteguja et al.'s (2006) surprising mixed finding about reward schemes

3 Experimental Design

Our experimental design is most closely related to the one by Hinloopen and Soetevent, but we introduce a number of crucial modifications. Our innovations are mainly relative to the timing when subjects can self-report, the fines' structure, the oligopoly game, and the framing of the experiment. We also consider two extra treatments, REWARD and LENRING, which will be discussed in detail below when we describe our experiment.

In our experiment, each subject represented a firm and played in anonymous two-persons groups a repeated duopoly game. In every stage game, the subjects had to take three types of decisions. First, they had to choose a price in a discrete Bertrand price game with differentiated goods. Second, they had to decide whether or not to form a cartel by discussing prices. Third, the subjects could choose to self report cartels to a competition authority. The attractiveness of this latter opportunity depended on the details of the antitrust law enforcement institution - the treatment variables of our experiment.

3.1 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.)

		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**

The demand function for each firm i was given by:

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

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 our experimental setup, we chose $a = 36$, $c = 0$ and $\gamma = 4/5$ and restricted the subjects' choice set to $\{0, 2, ..., 22, 24\}$. These parameters yield the payoff table distributed to each subject. To simplify the table we also 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 monopoly 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 monopoly price, i.e. 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 than the monopoly price as well as associated losses for the cheated upon firm; in the range of prices in between the Bertrand price and the monopoly price, i.e. in the range $\{4, ..., 8\}$, these gains and losses are smaller than when a subject deviates unilaterally from the monopoly price.

3.2 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, which is described in more detail below, was designed in such a way that it would result in a common price on which to cooperate. This agreed upon price was non-binding, however, and therefore each subject could cheat on the agreement by subsequently charging a price different from the agreed upon price.

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 competition authority had not yet detected the cartel. This implied that two subjects could be fined in a period even if no communication took place in that specific period; for example, two subjects could be fined in a period in which they did not communicate if they had communicated in the previous period and the competition authority had not detected the associated cartel in that period. Once a cartel was detected, however, it was considered to be dismantled and in subsequent periods, the former cartelists did not run any risk of being fined unless they communicated again.

3.3 Antitrust law enforcement (Treatments)

Whenever two subjects had formed a cartel, a competition authority could detect the cartel and convict its members for price fixing. Detection could happen in two ways. First, in every period, the competition authority detected cartels with an exogenous probability, α. If this happened, both cartel members had to pay an exogenous fine, F. Second, the cartel members could self-report the cartel, in which case the cartel members were convicted for price fixing with certainty. If this happened, the size of the fine depended on the details of the law enforcement institution.

We ran five types of treatments and we adopted a *between subjects* design, so that every subject only played the game under a single treatment. Each treatment corresponded to a specific type of antitrust law, that is our treatment variables were the different law enforcement institutions. The differences between the treatments are summarized in table 2.

Our baseline treatment corresponds to a laisser faire regime and is denoted COMMUNICATION: in this treatment, $\alpha = F = 0$ so that forming a cartel by discussing prices is legal. To simplify the instructions and to eliminate irrelevant alternatives, subjects were not allowed to report cartels. In the four other treatments, denoted ANTITRUST, LENIENCY, LENRING and REWARD, the expected fine (given that no reports took place) was strictly positive ($\alpha = 0.1$ and $F = 200$ yielding an expected fine $\alpha F = 20$) and cartel members were allowed to report cartels in which they participated. The ANTITRUST treatment corresponds to traditional antitrust laws without any leniency program: in case a report took place, both cartel members (including the reporting one) had to pay the full fine F. The LENIENCY treatment corresponds to current antitrust

Table 2: **Treatments**

Treatment	**fine (F)**	**probability of detection (α)**	**report**	**report's effects**
COMMUNICATION	0	0	No	–
ANTITRUST	200	0.10	Yes	pay the full fine
LENIENCY	200	0.10	Yes	no fine (half the fine if both report)
LENRING	200	0.10	Yes*	no fine
REWARD	200	0.10	Yes	reward (half the fine if both report)

*Only for the player who's the last to decide to communicate.

laws embedded with a leniency program: in case the cartel was reported by one of the cartel members only, the reporting member paid no fine while the other one paid the full fine, F; if instead both cartel members reported the cartel simultaneously, both paid a reduced fine equal to $F/2$. The treatment LENRING was identical to LENIENCY except that the first subject attempting to communicate was treated as the cartel's initiator - the so-called ringleader - and, as a result, was not eligible for the leniency program. (The way the ringleader was identified is described in more detail below). Finally, the REWARD treatment differed from LENIENCY in one respect only: if only one cartel member reported the cartel, his/her fine was not only reduced to 0; in addition, he was rewarded with the full fine, F, paid by the other cartel member.

In addition to these five treatments, we also ran a number of other treatments to check the robustness of our results to changes in α and F. First, we ran two additional antitrust and LENIENCY treatments with higher expected fines equal to 60 ($\alpha = 0.2$ and $F = 300$). These treatments were denoted ANTIHIGH and LENHIGH respectively. Second, we ran two additional reward treatments, both with an expected fine equal to 0 ($\alpha = 0$) but with different fines. The treatment denoted REWLOW had a relatively low fine ($F = 200$) while REWHIGH had a high fine ($F = 1000$).

3.4 The experiment's timing and the 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 matched into new pairs. When this happened, the history in the previous match did no longer matter; for example, a subject could no longer be fined for a cartel formed in a previous match. The subjects were also informed that the experiment would end if more than 20 periods had passed and the 15% probability event took place or if the experiment lasted for more than 2 hours and 30 minutes. This latter possibility was so unlikely that it never happened.

This re-matching procedure had several advantages. First, the subjects were playing truly *infinitely repeated games* without problems associated with end effects. Second, each subject played several repeated games against different competitors. Thereby we observed the subjects' behavior in a larger number of repeated games.

Before the experiment started, the subjects were paired with the same competitor for five practice periods. During these practice periods, subjects were assigned to different competitors than those that they faced in the first period of the 'true' (i.e. remunerated) experiment. Participants were informed about this.

3.5 The timing of the stage game

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

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 additional 30 seconds before pricing decisions were taken in Step 3. In all periods, subjects were also informed whether they were matched with the same opponent as in the previous round or if a re-match had taken place.

Figure 1: Timing of the stage game

In the treatment LenRing, the first subject to push the button within the time window of 15 seconds was treated as the ringleader. If instead only one of the subjects pushed the yes button, then this subject was treated as a ringleader even if the cartel was formed in later periods. In either case, both subjects were informed at the end of Step 1 about the identity of the (possibly only potential) ringleader.

Step 2: Communication. If both subjects decided to communicate in step 1, a window appeared on their computer screen asking them to simultaneously state a minimum acceptable price in the range $\{0, ..., 12\}$. When both of them 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} was defined as the minimum among the two prices selected in the previous negotiation round. This procedure went on until 30 seconds had passed. The resulting minimum price p_{min} was referred to as the agreed upon price.

Step 3: Pricing. Each subject had to choose his price from the choice set $\{0, ..., 12\}$. Possible price agreements reached in step 2 were not 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: First Reporting Decision. If communication took place in the current period or in one of the previous periods and had not yet been discovered by the competition authority, subjects had a first opportunity to report the cartel.

Step 5: Market prices and second reporting decision. Subjects learn the prices set by their opponent. If communication took place in the current period or in one of the previous periods and was not yet discovered by the competition authority and nobody has reported it in step 4, subjects have again the opportunity to report the cartel. The crucial difference between this second reporting opportunity and the first 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.

Step 6: Detection. If communication took place in the current period or in one of the previous periods and had not yet been discovered or reported in steps 4 or 5, the competition authority discovered the cartel 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: agreed upon price (if any), prices chosen by the two players, possible fines and net profits. In case players were fined, they were also told how many players reported. This step lasted 20 seconds

Note that with our experimental setup subjects have two opportunities to report the cartel: first at step 4, right after having set their price, then again at step 5, after having been informed about the price chosen by their opponent. In our design, reporting can thus be used for two different purposes: (i) deviating subjects may report to get protection against prosecution and (ii) cheated upon subjects may report to punish their opponents, if they have not reported before.

3.6 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). Session lasted on average 2 hours, including instructions and payment. The average payment was: (i) in Stockholm Euros 26.49, with a minimum of 15.3 and a

maximum of 36 and (*ii*) in Rome Euros 24.45 with a minimum of 16.5 and a maximum of 31.5. [9]We ran a treatment for every session; the number of subjects per session ranged from 16 to 32, and the total number of subjects was 194.

Subjects were welcomed in the lab and seated, each in front of a computer. When all subjects were ready, a printed version of the instructions and the profit table was distributed to them. Instructions were read aloud to ensure common knowledge of the rules of the game. The subjects were then asked to read the instructions on their own and ask questions, which were answered privately. When everybody had read the instructions and there were no more questions (which always happened after about fifteen minutes), each subject was randomly matched with another subject for the five practice rounds. After the practice rounds, participants had again a last opportunity to ask questions about the rules of the game. Again, they were answered privately. Then they 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. 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. The conversion rate was 200 points for 1 Euro.

3.7 Equilibrium set

In the games presented above, the equilibrium structure can be described in the following way. For each treatment and each price that the firms want to collude on, there exists a critical discount factor such that the firms can collude on the desired price level if and only if the firms' discount factor is larger than the critical discount factor. While it is not trivial to find these critical discount factors for each treatment, it is possible to rank them. Let $\delta_{Communication}$, $\delta_{Antitrust}$, $\delta_{Leniency}$, $\delta_{LenRing}$ and $\delta_{\mathrm{Re}\,ward}$ denote the critical discount factors for the, COMMUNICATION, ANTITRUST, LENIENCY, LENRING and REWARD treatments respectively. Provided that the probability of detection, α, and the size of the fine, F, are equal in all treatments, then it can be shown that

$$0 = \delta_{Antitrust} < \delta_{Communiction} < \delta_{Len} = \delta_{LenRing} < \delta_{\mathrm{Re}\,w} < 1.$$

The only surprising feature of this ranking is that collusion can be sustained for any discount factor in the ANTITRUST treatment ($\delta_{Anti} = 0$). The reason is simple: in the stage game, it is a subgame perfect equilibrium to collude. Indeed, if *both* firms' strategies stipulate that one should report the cartel whenever a firm unilaterally deviates from the collusive price, then it is no longer profitable to deviate due to the reports. Furthermore, the reports are credible: since both firms (including the deviating one) report the cartel following a deviation, both firms are indifferent between reporting and not reporting, and thus reporting is an equilibrium in the reporting subgame. Of course, the weakness of this subgame perfect equilibrium is that it is sustained through weakly dominated strategies. When the stage game is infinitely repeated, however, it is easy to construct strategies with the same flavor, which are not weakly dominated.

The key to the above observation is that in the ANTITRUST treatment, reports can be used as punishments against deviators. This is not the case in the other treatments. In the COMMUNICATION treatment, it is trivial that reports cannot be used. To see that reports cannot be used as punishments in the remaining treatments, note that optimal deviations involve secret reports. Thus cartels are dismantled after unilateral deviations and therefore reports cannot be used as punishments. For this reason, the ranking of the remaining critical discount factor is the expected one.

3.8 Empirical Methodology

A critical point in our analysis is how to control for repeated observations of the same subject or the same duopoly, when testing the significance of the observed differences across treatments. Before explaining more in detail the procedure we adopted, it is useful to introduce here some terminology. We call "individual-level"

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

data the data representing individual decisions of the subjects. On the contrary, we call "duopoly-level" data the data that refer to variables that always have the same value for the two members of a duopoly. Thus, for example, the presence of a cartel within a duopoly in a given period, or the fact that a given cartel is detected by the antitrust authority, are duopoly-level data, while the decision to communicate or not in a given period or the decision to unilaterally deviate from a collusive agreement are individual level data, as in every period they might take different values for the two members of a duopoly.

Given the structure of our game, we need to account for correlation between two observations from the same individual, as well as correlation between two observations from different individuals who belong to the same duopoly. Moreover, since we have run the experiment in two different cities, we also have to control for the possible correlation among observations collected in the same city. To this purpose, we adopted multilevel random effect models.

Since in our experiment a subject may take part in more than one duopoly during the game, the random effects at the subject level and at the duopoly level are not nested, which makes it difficult to estimate a model with a random effect at the duopoly level and a random effect at the subject level at the same time. To overcome this complicacy, when analyzing individual-level data, we assume that there is a random effect for every subject within any particular match – which accounts for the correlation among observations belonging to the same match. To take into consideration also the possible correlation among observations relative to the same subject in different matches, we hypothesize that there is a second random effect for every subject across different matches. Finally, we conjecture a third random effect at the city level.

To analyze duopoly-level data we make the assumption that correlation between observations belonging to the same subject but to different duopolies can be disregarded. We therefore hypothesize to have only a random effect at the duopoly level, nested with a random effect at the city level.

The only independent variable of our simple regressions is the treatment, as a dummy. To analyze individual-level data, we adopt a four-levels model of the following form:

$$y_{hijk} = \beta_0 + \beta_1 TREAT_{hijk} + \eta_{ijk}^{(2)} + \eta_{jk}^{(3)} + \eta_k^{(4)}$$

where h, i , j and k are indices for measurement occasions, subjects in matches, subjects across matches and cities, respectively. $TREAT$ is the dummy variable for the treatment. Since we always compare only two treatments at a time, this variable takes value 1 in correspondence of one of the two treatments, and value 0 in correspondence of the other one. $\eta_{ijk}^{(2)}$ represents the random intercept for subject j in match i, and in city k (second level), $\eta_{jk}^{(3)}$ represents the random intercept for subject j in city k (third level) and $\eta_k^{(4)}$ represents the random intercept for city k (fourth level). Random intercepts are assumed to be independently normally distributed, with a variance that is estimated through our regression.

The general three-levels model we adopt when looking at duopoly-level data has the following form:

$$y_{hlk} = \beta_0 + \beta_1 TREAT_{hlk} + \eta_{lk}^{(2)} + \eta_k^{(3)}$$

As above, h and k are indices for measurement occasions and cities, while l is the index for duopolies. $\eta_{lk}^{(2)}$ and $\eta_k^{(3)}$ represent random intercepts at the duopoly and city levels.

We ran logit regressions to analyze the decision to communicate, the decision to deviate, and the rates of cartel formation and of cartel detection; we adopted instead linear regressions for prices and agreed upon prices. To estimate our model we used an ordinary panel regression with random effect, when the number of considered levels was equal to 2, while we used GLLAMM (see Rabe-Hesketh and Skrondal, 2004 and http://www.gllamm.org) when the number of considered levels was equal or higher than three.

4 Results

4.1 Traditional and modern law enforcement

In this section we report the subjects' behavior in the COMMUNICATION, ANTITRUST and LENIENCY treatments. The purpose is to assess how traditional antitrust law (ANTITRUST) and more modern law enforcement institutions embedded with a leniency program (LENIENCY) perform relative to a laisser faire regime

(COMMUNICATION). Our primary interest is to document how these different policies perform in terms of ex ante deterrence and their implications for the subjects' price choices. In addition we also report post conviction deterrence and prices, that is whether cartelists, after having been convicted, are deterred from reforming the dismantled cartel. We postpone our analysis of the LENRING and REWARD treatments to two subsequent sections.

4.1.1 Cartel Deterrence

Table 3 reports the rates of communication attempts and of cartel formation provided that subjects are not currently cartel members. These rates are our main measures for evaluating the success of the different policies in terms of ex ante deterrence, that is the main objective of Antitrust policies.[10]

Result 1 (Ex ante deterrence) Traditional antitrust laws (ANTITRUST) are effective in deterring explicit cartel formation and modern antitrust laws (LENIENCY) even more so.

Table 3: **Deterrence effects**[11]

	Communication		**Antitrust**		**Leniency**
Rate of communication attempts	0.835	$>^{**}$	0.566	$>^{***}$	0.377
Rate of cartel formation	0.716	$>^{***}$	0.315	$>^{***}$	0.178
Rate of individual deviation	0.564	$>^{***}$	0.424	$\approx$	0.373
Rate of reporting	–	–	0.092	$<^{***}$	0.507

Note: In this table and in the following ones, the symbols ***, ** and * indicate significance at the 1%, 5% and 10% level, respectively.

Result 1 stems from the fact that the rates of communication attempts and of cartel formation are significantly lower in ANTITRUST than in COMMUNICATION. Moreover LENIENCY was even more successful in terms of ex ante deterrence since the rates of communication attempts and of cartel formation were significantly lower in LENIENCY than in ANTITRUST. Relative to COMMUNICATION, the rates of individual communication attempts decreased by 31% in ANTITRUST and by 56% in LENIENCY. These differences were even more striking for the rates of cartel formation, with a 55% and 77% decrease in ANTITRUST and LENIENCY respectively. This is line with Miller's (2007) estimate that leniency may be associated with a 52 percent decrease in the rate at which cartels form. These results are also (partly) consistent with previous experimental results. Apesteguja et al. do find a reduction in the percentage of formed cartels (from 67% to 50%) when Leniency is introduced (compared to the case when firms can report but there is no reduced fine). Hinloopen and Soetevent (2006) find a similar pattern as we do concerning the rate of cartel formation although they observe no significant differences between their antitrust and LENIENCY treatments with respect to the rate of communication attempts.

[10] For the **Rate of communication** the single observation is the binary individual decision to communicate of a subject in every single period. Similarly, for the **Rate of individual deviation** the single observation is represented by the individual decision to deviate from the last collusive agreement. Here we consider only the cases in which the subjects had previously formed an agreement on prices.

For the **Rate of cartel formation** we have a single observation per duopoly per period, which indicates if in that period a cartel has been formed within that duopoly. We consider only the cases in which no cartel pre-existed. For the **Rate of reporting** we consider only the cases in which a cartel exists. We have a single observation per duopoly per period, which indicates whether the cartel has been discovered in that period *because at least one of the two cartel members reported it to the antitrust authority*.

As explained above, we used a multilevel random intercept model to evaluate the significance of the observed differences. We adopted a four (three)-level random intercept logistic regression when using individual (duopoly)-level data. We ran a regression per each couple of treatments.

[11] Our results are not perfectly comparable with those of Hinloopen and Soetevent because they report the rate of communication for all periods while we report the rate of communication provided that no cartel has been formed previously.

Table 3 also reports the rates of detection due to self reporting by subjects - a first source of cartel instability. The rate of reporting is small in ANTITRUST while it increases substantially and significantly in LENIENCY. Hence:

Result 2 (Cartel stability and self-reporting) Modern antitrust laws (LENIENCY) reduce cartel stability due to self reporting.

Result 2 is in line with Miller's (2007) conclusion that leniency programs are associated with a 62% increase in the rate of detection, even though we observe an even higher increase. This is also consistent with Aspeteguja et al. who find an increase of 50% of the detection rate.

Our experimental design also allows us to distinguish between different motives behind reporting behavior. As already mentioned, subjects can either report in order to protect themselves against fines using the first reporting opportunity or they can report and punish their competitor after having observed the competitor's price choice. Clearly, we should expect to observe the former type of reports in LENIENCY only. By contrast, the latter opportunity to report in order to punish deviators may be observed both in ANTITRUST and LENIENCY, although one may argue that reports to punish should be rare in both treatments. In ANTITRUST, subjects may find it too costly to report and in LENIENCY, an optimal deviation involves a simultaneous secret report, implying that a cheated upon subject should not be able to punish by reporting.

Table 4 reports the probability of using the first and second reports in ANTITRUST and LENIENCY. As expected, the probability of using the first report in ANTITRUST is very low. However subjects in Antitrust used costly second reports to punish defectors in almost 30 % of the cases they were cheated upon. Thus some subjects were willing to take a quite large cost in order to punish deviators. Whether this reflected that these few subjects used optimal punishments or altruistic punishment as described by Fehr and Gätcher (2002) is an open question.[12]

Table 4: **Self reporting**

	Treatment	**Antitrust1**	**Leniency1**
Number of reports	(% of possibilities to report)	60 (4.7%)	195 (35.6%)
Number of First Reports	In total	4	168
	Simultaneous deviation	1	140
Number of Second Reports	In total	56	27
	At least one deviated	54	27
	Only rival deviated	46	13
	Rival deviated more	7	1

In LENIENCY, the probability to use the first secret reporting stage when simultaneous undercutting of the agreed upon price is slightly more than 70%. This is consistent with optimal deviations and the "protection gains fines motive". Still a non-negligible number of subjects in LENIENCY were reluctant to use optimal deviations or cheap punishments. In almost 30 % of the cases when subjects deviated, they did not simultaneously report. In more than 50 % of the cases a rival deviated without reporting simultaneously, the cheated upon subject did not punish the defector by reporting the cartel.

Finally, Table 3 reports the rates of deviation from agreed upon prices - a second source of cartel instability.

Result 3 (Cartel stability and rate of deviation) Both traditional (ANTITRUST) and modern (LENIENCY) antitrust laws increase cartel stability by reducing the rate of deviations from agreed upon prices.

[12] Fehr and Gätcher (2002) analyse a repeated one shot public good game experiment and argue that subjects are willing to bear the cost of punishing free riders. They explain that "Free riding may cause strong negative emotions among the cooperators and these emotions, in turn, may trigger their willingness to punish the free riders". Their experimental evidence gives stronger support to the hypothesis of altruistic punishments. In their experiment the individual punisher never meets the same subject again and thus the observed patten cannot be explained by optimal punishments.

Result 3 stems from the fact that the rate of deviation in both the ANTITRUST and LENIENCY treatments are significantly lower than in the COMMUNICATION treatment. This suggests that antitrust polices may generate trust among subjects, provided that none of the subjects have previously reported the cartel. As we will see when we next comment on the subjects' price choices, this observation implies that current antitrust policies are not unambiguously positive despite the fact they increase ex ante cartel deterrence (and, in the LENIENCY treatment also the probability of detection due to higher rates of self reporting).

4.1.2 Prices

The ultimate objective of antitrust law enforcement is to generate low prices. Table 5 presents for our first three treatments the average price, the average price within cartels, the agreed upon price and the average price given that subjects do not communicate. The first lesson to be drawn from this table is that cartel deterrence is desirable, since it reduces prices. Indeed, for each treatment in Table 5, prices within cartels are higher than the prices without communication. (Although not reported, these differences are statistically significant.) Combined with our earlier finding that ANTITRUST reduces the rate of cartel formation relative to COMMUNICATION and that LENIENCY further reduces that rate, it suggests that average prices should be highest in COMMUNICATION followed by ANTITRUST and lowest in LENIENCY. If anything, our data suggests the reverse:

Table 5: **Price levels**

	Communication		**Antitrust**		**Leniency**
Average price	4.913	$\approx$	5.348	$>^{***}$	4.844
Price within cartels	4.971	$<^{***}$	6.114	$<^{**}$	7.024
Agreed upon price	7.689	$<^{***}$	8.242	$\approx$	8.218
Price without communication	3.227	$\approx$	3.890	$\approx$	4.013

Note: For the **Average price**, a single observation is represented by the average among the prices chosen in a period by the two members of a duopoly. The same is true for **Price within cartels**, but here we only consider the cases in which the members of a duopoly have formed a cartel which has not been detected or reported yet. For **Agreed upon prices** we only consider the cases in which the subjects have communicated and found an agreement on the price to set. A single observation is given by the agreed upon price per duopoly, per period. For **Price without communication** we restrict our analysis to the cases in which no communication has taken place in the present period, and any possible previous agreement on prices has already been broken. A single observation is represented by the average among the prices chosen by the two members of a duopoly, in every single period. As explained above, we used a three-level random intercept linear model to compare the results across treatments since the analysis here concerns duopoly-level data. We ran a regression per each couple of treatments.

Result 4 (Average prices) Both traditional (ANTITRUST) and modern (LENIENCY) antitrust laws appear ineffective in reducing average prices.

Result 4 stems from the fact that average prices, relative to COMMUNICATION, are higher in ANTITRUST or only slightly lower (although not significantly so) in LENIENCY. This pattern thus suggests that both traditional and modern policies embedded with a leniency program are counter productive and might increase prices. The main driving force behind this result is that these policies appear to increase cartel stability (as noted in Result 3) and naturally this translates into higher prices within cartels (see Table 5).

Result 5 (Prices within cartels - what does not kill us makes us stronger) Both traditional (ANTITRUST) and modern (LENIENCY) antitrust laws increase cartel prices significantly.

Spagnolo (2000b), Buccirossi and Spagnolo (2001, 2006) and Ellis and Wilson (2001) suggested that antitrust policies embedded with a leniency program could have the effect of stabilizing those cartels that are not deterred. Their idea was that reporting could be used as a punishment against deviators, since reporting is less costly with a leniency program. This potential explanation for the high cartel prices in LENIENCY is not completely convincing in the context of our experiment; since we allowed for secret reports,

deviators could in effect protect themselves against such punishments. In fact, one may argue that reports as threats against deviators should be more relevant in ANTITRUST, since optimal punishments in that treatment involve reports. However, since we observed very few reports in ANTITRUST (although most of these few reports were used as punishments against deviators), it seems unlikely that reports as a threat against deviators were the main explanation for the high cartel prices in ANTITRUST.

In our view it seems more reasonable that antitrust policies generate trust among cartel members provided that the cartels are not reported. It is also interesting to note that cartel prices are significantly higher in LENIENCY than in ANTITRUST. This pattern suggests that the tougher the policy, the larger is the potential for generating trust among cartel members.

It is also interesting to note that the price levels for non cartel members appear to be higher (although insignificantly so) in the Antitrust and Leniency treatments than in the Communication treatments. One possible interpretation of this pattern is that a refusal to communicate when it is costly to do so, does not signal as clearly an unwillingness to cooperate. As a result, current antitrust policies may also facilitate tacit collusion. It should be emphasized, however, that because of higher deterrence, average prices overall are not significantly higher in the Antitrust and Leniency treatments.

4.1.3 High expected fines

To test the robustness of our findings to changes in α and F, we ran the two additional treatments, ANTIHIGH and LENHIGH with higher expected fines of 60 ($\alpha = 0.2$ and $F = 300$). Table 6 reports the rates of communication attempts and of cartel formation as well as average prices and prices within and outside cartels. These figures are compared with those for our original treatments, ANTITRUST and LENIENCY.

The first lesson from this table is that higher expected fines increase deterrence and reduce average prices under traditional antitrust laws but not under modern laws embedded with a leniency program. The reason is probably that the expected fine mostly increased through an increase in the probability of detection - this probability was doubled while the size of the fine increased by 50 % only - and that under LENIENCY, many cartels are reported irrespective of the probability of detection, thereby reducing subjects sensitivity to changes in that probability. Note also that the prices within cartels increased in the LENIENCY treatment but not in the antitrust treatment.

Table 6: **High expected fines**

	Antitrust		**AntiHigh**		**LenHigh**		**Leniency**
Rate of comm. dec.	0.590	$>^{**}$	0.452	$\approx$	0.435	$\approx$	0.344
Rate of cartel form.	0.316	$>^{***}$	0.195	$\approx$	0.163	$\approx$	0.146
Average price	4.34	$>^{*}$	4.00	$>^{*}$	3.65	$\approx$	3.93
Price within cartels	5.03	$\approx$	5.22	$<^{**}$	6.21	$>^{*}$	5.49
Price without communication	3.32	$\approx$	3.20	$\approx$	3.17	$\approx$	3.46

4.1.4 Post-conviction behavior

In this section we analyze agents' behavior after they are convicted and fined for a cartel they had formed before. This is interesting for at least two important and related reasons. The first reason is that any law enforcement policy should generate, beside a general ex ante deterrence, an ex post deterrence, some times called *desistance* in the antitrust literature: the policy should ensure that convictions stop the convicted wrongdoer(s) from committing the crime again. The crucial question here is, therefore, how do convictions, in general and in particular when generated by different law enforcement policies (presence of leniency, size of fines), affect agents' following decision whether to form another cartel and - whether or not a new cartel is formed - their price choices? The topic is particularly interesting for antitrust in light of Sproul's (1993) empirical finding that for a sample of US antitrust indictments prices often rose after antitrust conviction (see also the discussion in Whinston 2006).

The second related reason why post-conviction behavior is important is that a number of recent studies, theoretical and experimental, suggest that in oligopolistic industries the payment of a large sunk cost by competitors may lead to an increase in prices, either because the sunk cost acts as a coordination device for explicit or tacit collusion (e.g. Offerman and Potters 2006; Janssen 2006), or because agents are subject to a 'sunk-cost bias', that is, they use simple mark-up pricing rules of thumb to try to recover the costs sunk by charging a higher markup (see e.g. Baliga et al. 2006, who also describe how the best business administration textbooks in fact suggest these pricing rules based on average cost and mark ups as optimal ones). Buccirossi and Spagnolo (2007) noted that if these effects were present and significant in oligopolies, then the existing theory of optimal fines could not be applied to cartels as commonly done in the antitrust debate (it would be misleading): it should first be extended to incorporate these effects in the evaluation of the costs and benefits trade offs that lead to the optimal fines.

Ex post deterrence (or 'desistance') Figure 2 shows the cumulative number of new cartels (vertical axis) formed by convicted agents in the five periods following the conviction (horizontal axis), separately for our Antitrust and Leniency treatments. The plots are slightly 'optimistic', in the sense that some of the matches end before the five periods after conviction considered, possibly leading to a slight underestimation of the number of cartels that form again after conviction. Still, the data tell us quite a lot.

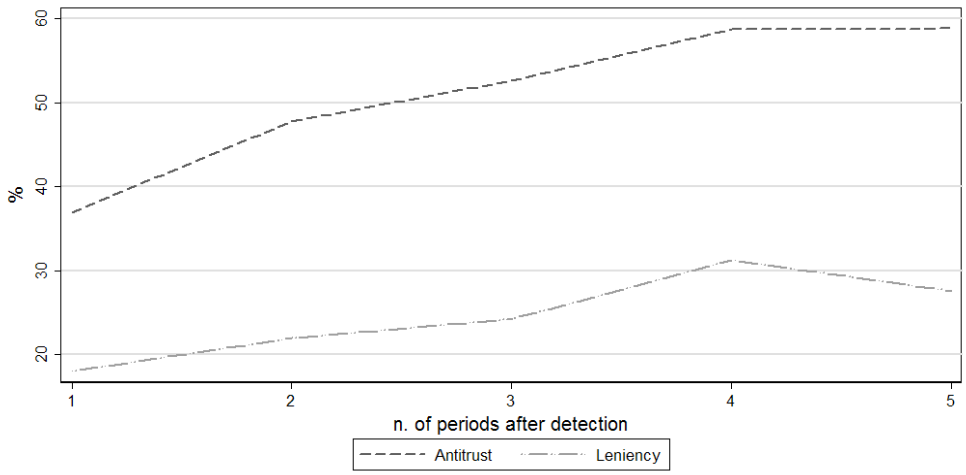

Figure 2: % of cartels re-established

First, there is a large fraction of agents that do not form new cartels after having been convicted and fined for a first cartel, even though our treatments are designed so that agents' situation in terms of expected fines, discount factors, available actions and payoff functions after a conviction is exactly identical to that before the first convicted cartel was formed. What differs after a conviction is only the history of play, as agents now have played several rounds, formed one or more cartels, and were convicted and fined. If history or agents' experience did not matter, so that 'bygones are bygones', in our stationary framework we would expect all 'rational' agents that chose to form a cartel a first time and were convicted and fined to form a new cartel the period after conviction. Instead, more than half of former cartelist did not form a new cartel periods days after the conviction. This suggests that history and experience matter a lot in our experiment.

Second, there is a strong difference between the ex post deterrence effects of Antitrust and Leniency: close to 40% of convicted cartels come to life again in Antitrust treatments, but not in Leniency treatments. In other words, in our experiment *the introduction of leniency policies produces a strong increase in desistance.* Leniency policies appear much better at reducing recidivism than standard antitrust policies without leniency. This result is in stark contrast with Hinloopen and Soetevent (2006), who in their experiment find no

improvement in desistance linked to the introduction of leniency policies. The reason behind their opposite result, in our view, is most likely due to their experimental set up not allowing for secretly reporting and simultaneously deviating from cartel agreement, as is possible in our experiment and in reality. As we have seen before, in our experiment most of convictions in Leniency treatments are linked to agents simultaneously undercutting cartel price and self-reporting. This joint action is likely to generate substantial more distrust among agents than a discovery by the competition authority, and thereby to make substantially more difficult for convicted agents to trust each other again.

Post-conviction prices In his paper on the effects of antitrust indictments on prices charged after the indictment - in the absence of an effective leniency program - Sproul (1993) finds that:

1. On average prices rise gradually after an indictment for price fixing.
2. The largest immediate drops in price after conviction are about 9-10 percent.
3. Post-conviction prices are negatively correlated with the severity of penalties.

Sproul suggests that some of the cartels he analyzes could involve efficiencies, and imputes the increase in average prices to a loss of these efficiencies. A comparison between his results and ours, particularly under Antitrust treatments (there was no serious leniency program at the time of the cartels studied by Sproul) might help to understand some aspects of the phenomenon under analysis.

Figure 3 shows price choices in cartels before conviction (conviction takes place at time 0) and after conviction, separately for convicted agents that have formed again a new cartel and by those that did not do it, and for Antitrust and Leniency treatments. The stylized facts that emerge from our experiment are the following:

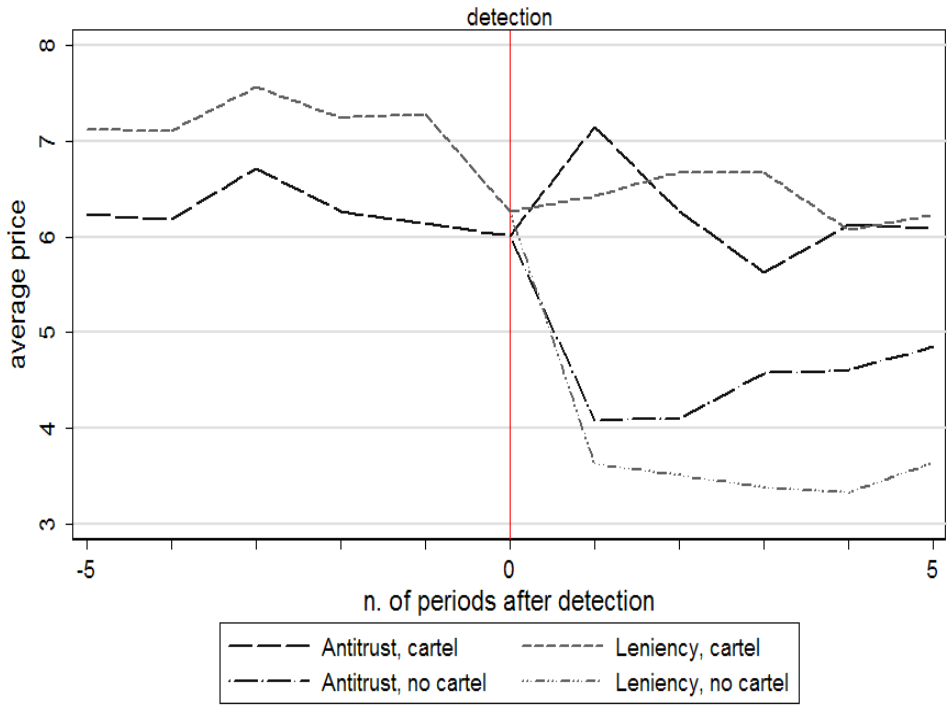

Figure 3: Price before and after detection

a) Prices after conviction are on average lower than in cartels before conviction.

b) When cartels are re-established after conviction, prices stabilize at levels close to that prevailing in the cartel the period when the cartel was convicted.

c) When cartels are not re-established, prices fall substantially with respect to the prevailing cartel price at the time of conviction, and remain low.

d) Post-conviction prices are higher in Leniency than in Antitrust treatments when a new cartel is formed after conviction, while the opposite happens when a new cartel is not formed after conviction.

The fact that average prices within cartels that are restored after conviction remain close to the level observed in the period in which the previous cartel was detected, whether leniency is granted to the whistleblowers or not, appears consistent with Sproul's (1993) findings given that in our framework there are no efficiencies linked to collusion. Somewhat in contrast to Sproul (1993) we also find that in the large number of cases where a novel cartel is not formed after conviction prices fall much below the level reached in the period in which detection took place, which drives down average post-conviction prices. True, this happens much more often in Leniency treatments than in Antitrust ones, and prices when new cartels do not form are much lower in Leniency than in Antitrust treatments, while at the time of Sproul's cartels an effective leniency policy was not in place. Still, even focussing only on Antitrust treatments, it appears that prices fall on average after conviction. On the other hand, to explain why in his sample prices do not fall after indictment, Sproul hypothesizes that "the government mainly prosecutes cost-reducing cartels". Such an interpretation is not questioned by our data, since in our experiment cartels have no effects on costs.

As for the effects of Leniency, it appears to have the novel effect of strongly increasing desistance thereby reducing average prices, even though prices are substantially higher in cartels that manage to form again after a conviction caused by a leniency application.

The difference that arises between Leniency treatments and Antitrust treatments when players decide not to form a new cartel after being detected is also interesting (stylized fact d)). While under Leniency the average price remains close to Bertrand and to the level observed before the (detected) cartel arose, under Antitrust average non-collusive prices after detection rise as if – after having formed an explicit cartel and having experienced the fine – some of the subjects try to reach a tacit agreement on prices. A possible interpretation of this effect is that under Antitrust detection does not affect trust between cartelists, while under Leniency detection and defection are often simultaneous, and the cartel is discovered because it is reported by the deviating player; therefore, post-conviction tacit collusion is more difficult to achieve under Leniency.

Size of the fine and post-conviction prices: 'sunk cost bias' and coordination effects The idea here is that antitrust fines might be viewed as a sunk cost by the convicted subjects. The literature has discussed the possible coordination role of sunk costs (e.g. Offerman and Potters 2006) and the existence of a 'sunk cost bias' in decision making, where agents try to recover sunk fixed cost by increasing a mark up on the average cost chosen when setting the unit price (Baliga et al. 2004). To distinguish the two effects, we hypothesized that the first effect should imply improved coordination in general, and therefore also in newly formed cartels. Table 7 reports post conviction prices from our experiment, in newly formed cartels and outside, and the level of the fines levied on convicted agents. Consistently with one of Sproul's finding, we observe a negative (though not always significant) correlation between the size of the fine and post-conviction prices. In our experiment this effect is somehow puzzling, since even before getting fined our subjects were informed about the size of the fine and the probability of detection, so if they were fully rational they should not change their behavior after detection. A deeper analysis is required to understand the reasons that lead to this finding.[13]

We observe that post conviction prices are generally lower when the fine (and the expected fine) is higher, both within cartels and outside cartels, whether leniency is granted to the "whistleblowers" or not. Consequently, our evidence seems to contradict the hypothesis of a sunk cost bias, which would affect prices of firms that choose not to re-establish a cartel after being fined; our results are also against the hypothesis of a coordination effect of the fine for cartels restored right after their detection.

To test the significance of the observed difference in post conviction prices between Antitrust and AntiHigh, and between Leniency and LenHigh, we estimated a three level random effect linear model using GLLAMM, following the procedure explained in section 3.6. As mentioned above, this procedure allows us

[13] To investigate this matter, we ran some other related experiments' whose results will be presented in a companion paper.

Table 7: Size of the fine and post-conviction pricing

Treatment	Fine	Prices outside cartels	Prices within cartels
Antitrust	200	4.418	7.297
		$\approx$	$\vee^*$
AntiHigh	300	3.310	5.750
Leniency	200	3.776	6.732
		$\approx$	$\approx$
LenHigh	300	3.181	4.700

to keep into account the correlation between observations from the same duopoly, and also the correlation between observations from the same city. We notice that the differences we observe are economically, but not statistically significant in most of the cases. According to our results, the difference in post conviction prices between LENIENCY and LenHigh is not significant, neither within cartels nor outside cartels. On the other hand, the difference between prices observed in Antitrust and AntiHigh is significant, but only outside cartels. This lack of statistical significance may be due to the sample size, which is very small since we restrict our analysis only to the cases in which a cartel was discovered and dismantled in the previous period.

4.2 Ineligibility for Cartel Ringleader[14]

Under the US Corporate Leniency Policy, a firm is ineligible for amnesty if it is the instigator of the cartel - the so called ringleader. In order to qualify for amnesty, the policy requires that the "corporation did not coerce another party to participate in the illegal activity and clearly was not the leader in, or the originator of, the activity" (Corporate Leniency Policy, *supra* note 58). By contrast, and following the revision of the EU Leniency Notice in 2002, also the ringleader is eligible for amnesty in the EU. Whether or not ineligibility of the ringleader has desirable consequences in terms of deterrence and prices is not clear cut. Excluding the ringleader from the leniency program may increase deterrence if each firm wait for some other firm to take the initiative of forming the cartel. As noted by Leslie (2006), however, extending amnesty to the ringleader may increase deterrence as well by ensuring that even the ringleader cannot be completely trusted, as it may also loose confidence and rush to report under the leniency program.[15]

Table 8: **Deterrence effects**

	Leniency		LenRing
Rate of communication attempts	0.344	$\approx$	0.290
Rate of cartel formation	0.146	$\approx$	0.135
Rate of individual deviation	0.472	$>^{***}$	0.230
Rate of detection	0.646	$>^{***}$	0.289

To evaluate the pros and cons of ringleader ineligibility, we ran the additional treatment, LenRing. Tables 8 and 9 compare the effects on deterrence and on price levels of eliminating the possibility of amnesty for the ringleader. Three features are striking in these tables. First, the LenRing treatment has no significant effect on cartel deterrence relative to the Leniency treatment. Second, cartels appear to become more stable and third the LenRing treatment increase prices significantly according to all our price measure. These findings are summarized in the next result [16].

[14] We thank Joe Harrington for suggesting this treatment.

[15] There are other arguments for and against the ineligibility of ringleaders. Extending leniency to the ringleader may be important to elicit self-reporting, as it may not be that clear to a firm considering whether to apply for leniency if it risks being regarded as ringleader. On the other hand, in an adversarial system, where testimony is crucial to persuade juries, testimony by a ringleader may not be convincing.

[16] Treatment LENRING was run only in Rome. For sake of consistency, in tables 8 and 9 we only consider observations gathered

Result 7 (Ringleader) If the ringleader is excluded from the leniency program, the deterrence effect of leniency falls and prices are higher than otherwise.

Result 7 thus suggests that the US practice of excluding the ringleader from the leniency program is unambiguously bad in our set up. While we find this result an interesting first step, that confirms some observers' concerns that excluding ringleaders may reduce the effectiveness of the leniency program, we should also emphasize one important caveat. In our experiment subjects were matched pairwise into duopolies to avoid social preferences effects towards non-defecting third parties.

Table 9: **Price levels**

	Leniency		**LenRing**
Average price	3.926	$<^{***}$	4.847
Price within cartels	5.494	$<^{***}$	7.284
Agreed upon price	7.099	$\approx$	7.833
Price without communication	3.457	$<^{***}$	3.912

This, however, is the worst conceivable situation for the US policy of excluding ringleaders, as the ban leaves only one cartel member with the option to self-report obtaining leniency, eliminating the incentives to "race to report" generated by the risk that another cartel member could do it before. With more than two firms, therefore, it is likely that LenRing treatment will show more desirable properties. Therefore, further experimental research with many cartel members is needed to attempt any policy conclusion on this feature.

4.3 Rewards

So far we have only considered policies that have been extensively implemented in reality. Given that none of these policies yielded fully satisfactory results, it is natural to turn attention to policies that have been advocated in the literature on optimal law enforcement. The type of policy that we consider here is one where the reporting subject gets rewarded by an amount equal to the fine paid by its rival.[17] Tables 10 and 11 compare the effects on deterrence and on price levels of introducing such reward schemes.

Result 6 (Ex post deterrence) Cartels are systematically reported in the Reward treatment.

This result is corroborated by Table 10 showing that the rate of detection due to reporting is almost equal to one in the Reward treatment. In fact, a simple inspection of the data in the Reward treatment reveals that almost every time a cartel was formed, at least one of the subjects reported it: out of the 120 times a cartel was formed, the cartel was reported during the first period in 118 cases. In one of the remaining cases, it was reported in the subsequent period, while there was only a single duopoly in which the players resisted the temptation of reporting and managed to sustain the collusive agreement for seven consecutive periods. This cartel ended because a re-matching took place.

Table 10: **Deterrence effects**

	Antitrust		**Reward**		**Leniency**
Rate of communication attempts	0.566	$>^{***}$	0.484	$>^{***}$	0.377
Rate of cartel formation	0.315	$>^{***}$	0.220	$\approx$	0.178
Rate of individual deviation	0.424	$<^{***}$	0.781	$>^{***}$	0.373
Rate of detection	0.092	$<^{***}$	0.937	$>^{***}$	0.507

in Rome for treatment Leniency as well.

[17] Korea is the only country we are aware of that adopts this kind of reward schemes for whistleblowers in antitrust; analogous schemes are however used in other fields of law enforcement, particularly in the US.

There are two potential explanations to this phenomenon. First the subjects could in principle exploit the reward system by taking turns in reporting and cashing in the reward. The second hypothesis, first proposed by Apesteguja et al, is that subjects form a cartel with the hope of fooling their competitor by undercutting the agreed-upon price and by reporting the cartel in order to cash in the reward. The next result confirms this latter hypothesis.

Result 7 (Cartel stability) The antitrust policy with rewards significantly reduces cartel stability.

This result is reflected by the fact that the rate of individual deviation increased substantially in the Reward treatment. Note also that at least one subject undercut the agreed upon price in 111 out of the 118 cartels that only lasted one period.

Table 10 also suggests that the antitrust policy with rewards is not more efficient in deterring cartels ex ante than the traditional policies. Indeed the rates of communication attempts and of cartel formation in the Reward treatment are not significantly different from corresponding rates in the Antitrust and Leniency treatments. Nevertheless:

Result 7 (Ex ante deterrence) The antitrust policy with reward strongly deters explicit cartel formation, the more the longer subjects play.

Result 7 is explained by the fact that the subjects eventually learned that it was not possible to form cartels for the purpose of cashing in the reward and, as a result, the number of formed cartels was reduced drastically. This appears clearly in Figure 4 showing that the number of cartels formed were reduced as subjects were re-matched.

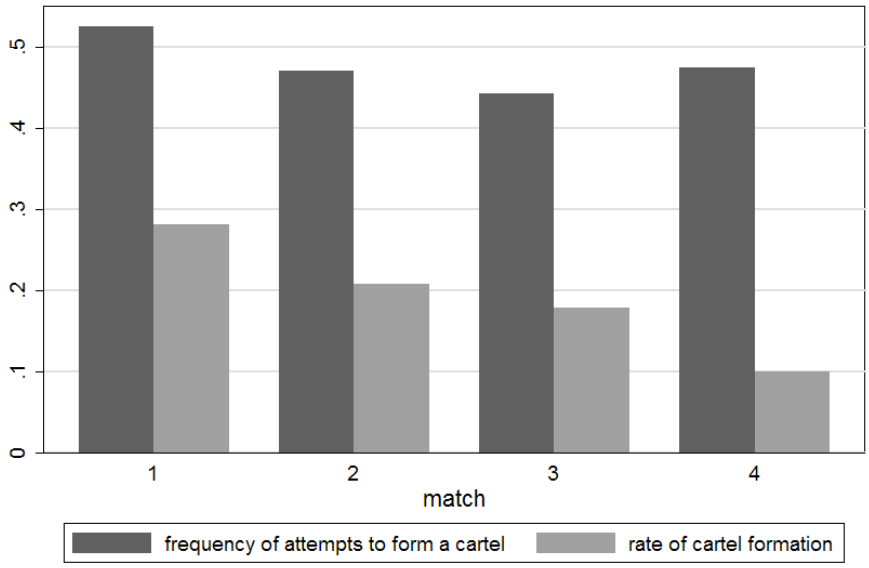

Figure 4: Rates of cartel formation and communication attempts in the four matches of the Reward treatment.

Result 8 (Prices) The antitrust policy with rewards strongly reduces both prices in explicit cartels and subjects' ability to collude tacitly.

Table 11: **Price levels**

	Antitrust		**Reward**		**Leniency**
Average price	5.348	$\approx$	3.975	$\approx$	4.844
Price within cartels	6.144	$>^{**}$	5.339	$<^{***}$	7.024
Agreed upon price	8.242	$\approx$	8.512	$\approx$	8.218
Price without communication	3.890	$\approx$	3.565	$\approx$	4.013

The systematic reports when subjects took part in cartels probably undermined trust among the subjects and, as a result, also prices dropped (though not significantly) in the Reward treatment. This is most striking in Table 11 where all measures of prices are the lowest in the Reward treatment. In particular, prices also dropped when subjects did not communicate. Thus prices were not only low because cartels were deterred from forming, giving further support for the claim that the systematic reports undermined trust. Rewarding whistleblowers appears therefore the only antitrust policy able to reduce price and increase welfare in our experimental set up.[18]

In theory, and contrary to any previous result in the economic literature on law enforcement, it is possible to achieve the first best of full deterrence with finite fines and no inspection probability using reward schemes (Spagnolo, 2000, 2004). To verify this we ran a further treatment RewLow, identical to Reward but for the probability of detection that we set at zero ($\alpha = 0$). The results showed that some agents still need to try a couple of times to induce others to form a cartel just to report and cash the bonus. After a couple of attempts they learn that with the whistleblower reward scheme everybody reports immediately once a cartel forms, so that entering a cartel is never profitable, and cartels disappear. The first best is therefore only achieved in asymptotic form. Indeed, after enough learning the subjects appear to converge to the first best. We also run a second treatment RewHigh, exactly equal to RewLow but for the fine, which was higher ($F = 1000$). In this last treatment we observed a further increase in deterrence (the rate of communication attempts dropped to 25.9%, and the rate of cartel formation was 7.8%). Yet, full deterrence was not achieved even in this treatment, because at least some of the players still tried to form a cartel and to fool their opponent by deviating from the agreement and simultaneously reporting it.

As a final remark we stress that the results of our treatments with rewards forcefully confirm the importance of learning in experimental settings with complicated/realistic games if we compare to those of Apesteguja et al. (2007). In particular, the results show that the preliminary and strange conclusion by Apesteguja et al. (2006) - that Rewards are not more effective than Leniency - was premature and due to the subjects' inability to learn in that set up, as these authors already conjectured.

4.4 Culture, Trust and Antitrust

We run our experiments in Stockholm and in Rome, two towns with quite distinct cultures. It is not obvious that one or the other culture should lead to more cartels given the legal framework, as our experiment was framed, cartel formation was presented as illegal, and Swedes are thought to be more law abiding than Italians. This would point at Italians colluding more. On the other hand, according to the World Values Survey (1999), there are important differences between Sweden and Italy that may point in a different direction. In particular, when they were asked whether "information to help justice should be given to the authorities", 40.2 % of the Italians strongly agreed when only 26% of Swedes do. Moreover a majority of

[18] Note that we designed the reward scheme in such a way that it could be exploited by cartel members: the scheme is such that if cartel members took turns in self-reporting and cashing the reward, the expected fine would be zero (Spagnolo 2004, 2006 makes it clear that - for this reason - in reality the whistleblower's reward should always be strictly smaller than the sum of fines paid by the other wrongdoers, so that there exist no possibility to manipulate/exploit the scheme). Still, none of our subject appear to have realized this opportunity, a result that confirm that some legal scholar's claims that reward schemes could be manipulated are unfounded empirically, besides being incorrect theoretically when the scheme is appropriately designed (by an economist). The result is consistent with Dal Bo's (2005) finding that asymmetric (alternating) actions cooperative equilibria in the repeated Prisoner's Dilemma are never played by experimental agents even when they are way more efficient than standard, stationary cooperative equilibria.

Swedes (63.7%) think that "people can be trusted" while only 31.8% Italian agreed. The difference in the answers to the first question suggests that leniency programs could be more effective in Italy. The difference in the answers to the second question suggest that Swedes are more confident in the cooperation of partners, so that they are more likely to coordinate on collusive/cooperative equilibria.

Separating treatments according to location we found results consistent with the differences in answers to the World Value Surveys: Swedes collude more often, coordinate on higher prices, and deviates much less often than Italians. In all treatments prices lower and cartels less frequent in Italy than in Sweden, and defection and applications to leniency are much more frequent in Italy. According to our results, nordic countries may be in more need of antitrust enforcement because of their 'cooperative' culture than southern ones.

5 Conclusions

This paper reports results from an experiment designed to examine the effects of fines, leniency programs, and reward schemes for whistleblowers on firms' decision to form cartels (cartel deterrence) and on their price choices. We consider an experiment in which subjects play a repeated Bertrand price game with differentiated goods, running several treatments different in the probability of cartels being caught, the level of fine, the possibility of self-reporting (and not paying a fine), the existence of a reward for reporting, and cartel leaders access to leniency. We find that fines following successful investigations but without leniency have a deterrence effect (reduce the number of cartels formed) but also a pro-collusive effect (increase collusive prices in surviving cartels). Leniency programs might not be more efficient than standard antitrust enforcement, since in our experiment they do deter a significantly higher fraction of cartels from forming, but they induce even higher prices in those cartels that are not reported, pushing average market price higher than without antitrust enforcement. With rewards for whistle blowing, instead, cartels are systematically reported, which completely disrupts subjects' ability to form cartels and sustain high prices. If the ringleader is excluded from the leniency program the deterrence effect of leniency does not increase while prices are higher than otherwise. As for tacit collusion, we find that under standard anti-trust enforcement or leniency programs, subjects who do not communicate (do not go for explicit cartels) choose weakly higher prices than where there is no anti-trust enforcement. We also analyze and post-conviction behavior, finding that after convictions caused by a report under the leniency program much fewer cartels form and prices are lower than when conviction takes place under standard antitrust policies without leniency. Finally, we find a strong cultural effect in the deterrence power of the various law enforcement regime when comparing treatments in Stockholm and Rome.

Our results have policy implications for general deterrence of organized crime similar to cartels, and as a test for the theoretical results mentioned in Section 2 (the protection from fines effect, the reward effect, the reporting as a threat and tacit collusion). Our results have only marginal implications for most of the many other theoretical papers, including cornerstone contributions to this literature.

For example, Motta and Polo (2003), the first economic analysis on leniency programs, focuses mainly on the effects of the second part of the leniency programs opened to firms already under prosecution. The only implication of our experimental results for that paper is that they do not support its two policy conclusions that (i) to have deterrence effects leniency programs must be opened to firms already under investigation (in our experiment they aren't), and that (ii) introducing a leniency program is a second best choice relative to standard antitrust law enforcement if there is a large enough budget. Analogous, Harrington (forthcoming) does not consider general deterrence but focuses on desistance effects, i.e. the ability of law enforcement mechanism to shorten the duration of cartels that were not deterred by the mechanism. It also introduces a stochastic movement in a law enforcement parameters to generate equilibrium applications of cartel members to the leniency schemes. The only implication of our experimental results for that paper is that our real world agents did form cartels and then apply for leniency in our fully deterministic, stationary oligopolistic environment. This suggests that deterministic theoretical analyses are perfectly OK, they are not at odd with the evidence that people form cartels and then report them.

Aubert et al. (2006) do focus on the first part of leniency programs, on general deterrence and on rewards,

but their contribution is about the costs and benefits of providing leniency and rewards to the individual employees of colluding firms. In our experiment we only have single decision makers, so we have no evidence relevant to that issue. This sounds, however, as an exciting topic for future experimental work.

Appendix 1: Instructions for the Leniency 1 treatment

Welcome to this experiment about decision making in a market. The experiment is expected to last for about 1 hour and 45 minutes. You will be paid a minimum of 7 Euros for your participation. On top of that you can earn more than 30 Euros if you make good decisions.

We will first read the instructions aloud. Then you will have time to read them on your own. If you then have questions, raise your hand and you will be helped privately.

In summary, the situation you will face is the following. You and one other participant referred to as your competitor produce similar goods and sell them in a common market. As in most markets, the higher the price you charge, the more you earn on each sold good, but the fewer goods you sell. And, as in many markets, the lower the price charged by your competitor, the more customers he or she will take away from you and the less you will sell and earn. It is possible, however, to form a cartel with your competitor, that is, you will have the possibility to communicate and try to agree on prices at which to sell the goods. In reality, cartels are illegal and if the government discovers the cartel, cartel members are fined. In addition members of a cartel can always report it to the government. The same happens in this experiment. If you communicate to discuss prices, even if both of you do not report, there is still a chance that the 'government' discovers it and if this happens, you will have to pay a 'fine'. If you report, and if you are the only one to report, you will not pay any fine but your competitor will pay the full fine. Conversely, if only your competitor reports the cartel, you will pay the full fine and your competitor will not pay any fine. If instead both of you report the cartel you will both pay 50% of the fine.

Timing of the experiment

In this experiment you will be asked to make decisions in several periods. You will be paired with another participant for a sequence of periods. Such a sequence of periods is referred to as a match. You will never know with whom you have been matched in this experiment.

The length of a match is random. After each period, there is a probability of 85% that the match will continue for at least another period. So, for instance, if you have been paired with the same competitor for 2 periods, the probability that you will be paired with him or her a third period is 85%. If you have been paired with the same competitor for 9 periods, the probability that you will be paired with him or her a tenth period is also 85%.

Once a match ends, you will be paired with another participant for a new

match, unless 20 periods or more have passed. In this case the experiment ends. So, for instance, if 19 periods have passed, with a probability of 15% you are re-matched, that is you are paired with another participant. If 21 periods have passed, with a probability of 15% the experiment ends.

When you are re-matched you cannot be fined anymore for a cartel formed in your previous match with your previous competitor.

The experimental session is expected to last for about 1 hour and 45 minutes but its actual duration is uncertain; that depends on the realization of probabilities. For this reason, we will end the experimental session if it lasts more than 2 hours and 30 minutes.

Before the experiment starts, there will be 5 trial periods during which you will be paired with the same competitor. These trial periods will not affect your earnings. When the experiment starts, you will be paired with a new competitor.

Prices and Profits

In each period you choose the price of your product. Your price as well as the price chosen by your competitor determines the quantity that you will sell.

The higher your price, the more you earn on each sold good, but the fewer goods you sell. Therefore your price has two opposing effects on your profit. On the one hand, an increase in your price may increase your profit, since each good that you sell will earn you more money. On the other hand, an increase in your price may decrease your profit, since you will sell less.

Furthermore, the higher the price of your competitor, the more you will sell. As a result, your profits increase if your competitor chooses a higher price.

To make things easy, we have constructed a profit table. This table is added to the instructions. Have a look at this table now. Your own prices are indicated next to the rows and the prices of your competitor

are indicated above the columns. If you want to know your profit if, for example, your competitor's price is 5 and your price is 4, then you first move to the right until you find the column with 5 above it, and then you move down until you reach the row which has 4 on the left of it. You can read that your profit is 160 points in that case.

Your competitor has received an identical table. Therefore you can also use the table to learn your competitor's profit by inverting your roles. That is, read the price of your competitor next to the rows and your price above the columns. In the previous example where your price is equal to 4 and your competitor's price is equal to 5, it follows that your competitor's profit is 100 points.

Note that if your and your competitor's prices are equal, then your profits are also equal and are indicated in one of the cells along the table's diagonal. For example, if your price and the price of your competitor are equal to 1, then your profit and the profit of your competitor is equal to 38 points. If both you and your competitor increase your price by 1 point to 2, then your profit and the profit of your competitor becomes equal to 71.

Note also that if your competitor's price is sufficiently low relative to your price, then your profit is equal to 0. The reason is that no consumer buys your good, since it is too expensive relative to your competitor's good.

Fines

In every period, you and your competitor will be given the opportunity to communicate and discuss prices. If both of you agree to communicate, you will be considered to have formed a cartel, and then you might have to pay a fine F. This fine is given by:

F = 200 points

You can be fined in two ways. First, you and your competitor will have the opportunity to report the cartel. If you are the only one to report the cartel, you will not pay any fine but your competitor will pay the full fine, that is 200 points. Conversely, if only your competitor reports the cartel and you do not, then you will have to pay the full fine equal to 200 points and your competitor will not pay any fine. Finally, if both of you report the cartel, you will both pay 50% of the fine, that is 100 points.

Second, if neither you nor your competitor reports the cartel, the government discovers it with the following probability.

Probability of detection = 10%.

Note that you will run the risk of paying a fine as long as the cartel has not yet been discovered or reported. Thus you may pay a fine in a period even if no meeting takes place in that period. This happens if you had a meeting in some previous period which has not yet been discovered or reported.

Once a cartel is discovered or reported, you do not anymore run the risk of paying a fine in future periods, unless you and your competitor agree to communicate again.

Earnings

The number of points you earn in a period will be equal to your profit minus an eventual fine or plus an eventual reward. Note that because of the fine, your earnings may be negative in some periods. Your cumulated earnings, however, will never be allowed to become negative.

You will receive an initial endowment of 1000 points and, as the experiment proceeds, your and your competitor's decisions will determine your cumulated earnings. Note that 20 points are equal to 1 SEK. Your cumulated earnings will be privately paid to you in cash at the end of the session.

Decision making in a period

Next we describe in more detail how you make decisions in each period. A period is divided into 7 steps. Some steps will inform you about decisions that you and your competitor have made. In the other steps you and your competitor will have to make decisions. In these steps, there will be a counter indicating how many seconds are left before the experiment proceeds to the next step. If you fail to make a decision within the time limit, the computer will make a decision for you.

Step 1: Pairing information and price communication decision

Every period starts by informing you whether or not you will play against the same competitor as in the previous period.

Remember that if you are paired with a new competitor, you cannot be fined anymore for cartels that you formed with your previous competitors.

In this step you will also be asked if you want to communicate with your competitor to discuss prices. A communication screen will open only if BOTH you and your competitor choose the "YES" button within 15 seconds. Otherwise you will have to wait for an additional 30 seconds until pricing decisions starts in Step 3.

Step 2: Price communication

After the communication screen has opened, you can "discuss" prices by choosing a price out of the range { 0, 1, 2, ... , 12 }. In this way you can indicate to your competitor the minimum price that you find acceptable for both of you. When both of you have chosen a price, these two prices are displayed on the computer screen. You can then choose a new price but now this price should be greater or equal to the smaller of the two previously chosen prices. This procedure is repeated until 30 seconds have passed. The screen then displays the smaller of the two last chosen prices, which is referred to as the agreed-upon price. Note, however, that in the next step, neither you nor your competitor is forced to choose the agreed-upon price.

Step 3: Pricing decision

You and your competitor must choose one of the following prices: 0, 1, 2, ... , 12. When you choose your price, your competitor will not observe your choice nor will you observe his or her price choice. This information is only revealed in Step 5. The experiment proceeds after 30 seconds have passed. If you fail to choose a price within 30 seconds, then your price is chosen so high that your profits will be 0.

The experiment proceeds to the first reporting decision in Step 4 if you communicated in Step 2 or if in previous periods you formed a cartel not yet discovered or reported. Otherwise you have to wait for 10 seconds until market prices are revealed in Step 5.

Step 4: First (secret) reporting decision

By choosing to push the "REPORT" button, you can report that you have been communicating in the past. As described above, if you are the only one to report, you will not pay the fine; the opposite happens if only your competitor reports; and if both of you report, you will both pay 50% of the fine.

If you do not wish to report, push instead the "DO NOT REPORT" button.

When you decide whether or not to report, your competitor will not observe your choice, nor will you observe his or her choice. This information is only revealed when market prices are revealed in Step 5.

If you do not reach a decision within 10 seconds, your default decision will be "DO NOT REPORT".

Step 5: Market prices and second reporting decision

In this step your and your competitor's prices and profits are displayed.

In case you have formed a cartel not yet discovered or reported, the screen will also display whether or not you or your competitor reported it in the first reporting step (Step 4). If not, you will get a new opportunity to report.

If you wish to report, push the "REPORT" button. If you do not wish to report, push instead the "DO NOT REPORT" button.

Again, if you are the only one to report, you will not pay the fine. On the contrary, f your competitor reports and you don't you will have to pay the fine and he will not. If both you and your competitor report, you will both pay 50% of the fine, that is 100 points.

Step 6: Detection probability

If this step is reached, you formed a cartel either in the current period or in previous periods. Furthermore the cartel has not yet been discovered or reported. The cartel can nevertheless be discovered. This happens with a probability of 10%. If the cartel is discovered, you and your competitor will have to pay the full fine of 200 points.

Step 7: Summary

In this step you learn the choices made in the previous steps: your and your competitor's price choices and profits, your eventual fine, your eventual reward and your earnings.

If you paid a fine in this period, you will also know whether your competitor reported the cartel or the government discovered it.

In case a cartel was detected or reported in this period, you will not run any risk of being fined in future periods, unless you and your competitor discuss prices again.

Step 7 will last for 20 seconds.

Period ending and ending of the experimental session

After Step 7, a new period starts unless 20 or more periods have passed and the 15% probability of pair dismantling takes place. In that case, the experiment ends.

The following time line summarizes the seven steps of each round.

Figure 5: Timing of the stage game

Throughout the experiment, a table will keep track for you of the history with your current competitor. For each previous period played with your current competitor, this table will show your price and profit, your competitor's price and profit as well as your eventual fine.

Payments

At the end of the experiment, your earnings in points will be exchanged in SEK. In addition you will be paid the show up fee of 50 SEK.

Before being paid in private, you will be asked to answer a short questionnaire about the experiment and you will have to handle back the instructions.

Please read now carefully the instructions on your own. If you have questions, raise your hand and you will be answered privately.

THANK YOU VERY MUCH FOR PARTICIPATING IN THIS EXPERIMENT AND GOOD LUCK!

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