Anti-trust and the 'Beckerian Proposition': the Effects of Investigation and Fines on Cartels

Subhasish M. Chowdhury
School of Economics, University of East Anglia

Frederick Wandschneider
School of Economics, University of East Anglia

CCP Working Paper 13-9

Abstract
In order to deter collusion and punish the infringement of competition law, anti-trust authorities run costly investigations and levy fines on detected and convicted wrongdoers. Across countries, the resources committed to anti-trust investigations and the fine level vary. According to Becker (1968) different combinations of magnitude of fine and likelihood of detection are substitutable in their deterrence effect. Since detection depends on costly investigation, it is optimal to minimize detection efforts and impose high fines. Recently the UK Office of Fair Trading faced a budget reduction that may affect detection efforts, while it simultaneously increased colluding firms fines from 10% to 30% of its annual turnover. Experimental support for the Beckerian Proposition is mixed in different contexts, and it is not known from a behavioural perspective how effective this type of policy design would be in a market. We address this issue through a market experiment to study the effects of magnitude and likelihood of fines on cartel activity, prices and collusive stability. We find that, in the absence of a leniency program, complying with the Beckerian Proposition, detection rates and fines are indeed substitutable. In the presence of a leniency program, however, a regime that embodies low rates of detection and high fines reduces the propensity to collude and lowers the overall incidence of cartelized markets significantly more than a high detection and low fine regime. This indicates that antitrust agencies can economize on enforcement costs and achieve a higher degree of deterrence by imposing higher level of fines.

ISSN 1745-9648

Anti-trust and the 'Beckerian Proposition': the Effects of Investigation and Fines on Cartels*

Subhasish M. Chowdhury† Frederick Wandschneider‡

School of Economics, Centre for Behavioural and Experimental Social Science,
ESRC Centre for Competition Policy, University of East Anglia,
Norwich NR47TJ, United Kingdom

October 9, 2013

Abstract

In order to deter collusion and punish the infringement of competition law, anti-trust authorities run costly investigations and levy fines on detected and convicted wrongdoers. Across countries, the resources commited to anti-trust investigations and the fine level vary. According to Becker (1968) different combinations of magnitude of fine and likelihood of detection are substitutable in their deterrence effect. Since detection depends on costly investigation, it is optimal to minimize detection efforts and impose high fines. Recently the UK Office of Fair Trading faced a budget reduction that may affect detection efforts, while it simultaneously increased colluding firms' fines from 10% to 30% of its annual turnover. Experimental support for the Beckerian Proposition is mixed in different contexts, and it is not known from a behavioral perspective how effective this type of policy design would be in a market. We address this issue through a market experiment to study the effects of magnitude and likelihood of fines on cartel activity, prices and collusive stability. We find that, in the absence of a leniency program, complying with the Beckerian Proposition, detection rates and fines are indeed substitutable. In the presence of a leniency program, however, a regime that embodies low rates of detection and high fines reduces the propensity to collude and lowers the overall incidence of cartelized markets significantly more than a high detection and low fine regime. This indicates that antitrust agencies can economize on enforcement costs and achieve a higher degree of deterrence by imposing higher level of fines.

Keywords: Experiment; Antitrust; Cartels; Deterrence; Illegal Behavior and the Enforcement of Law; Leniency.

JEL Classification: C92; D03; K42; L4.

*We appreciate the comments and suggestions of David Cooper, Stephen Davies, Richard Havell, Franco Mariuzzo, Stephen Martin, Russell Pittman, Adriaan Soetevent, Jack Whybrow, seminar participants at the University of East Anglia, and of participants at the 2013 World Meetings of the Economic Science Association (ESA) in Zurich, the 7th Competition Law and Economics European Workshop in Bergen and the 2013 CeDex-CREED-CBESS meeting in Amsterdam. We are grateful for financial support by the ESRC Centre for Competition Policy, University of East Anglia, and would like to thank Melanie Parravano for assistance in the lab. All opinions and errors are ours alone.

†E-mail: s.modak-chowdhury@uea.ac.uk

‡Corresponding author. E-mail: f.wandschneider@uea.ac.uk

1 Introduction

A central task for anti-trust authorities is to disincentivize and punish deliberate infringements of competition law by imposing sanctions on detected wrongdoers. In this study we examine by means of a market experiment, how the magnitude of the fine levied on a firm and the likelihood of anti-trust punishment, affect the choice to participate and engage in a (illegal) cartel.

Recently, anti-trust authorities in different countries experimented with finding an optimal punishment for anti-trust law infringements. For example, the Office of Fair Trading (OFT) in the United Kingdom, while facing a 5% year-on-year budget reduction that may well affect their ability to commit resources to costly investigations, increased the fine imposed on businesses in case of an infringement of competition law (OFT, 2013).[1] Until September 2012, collusive behaviour was punished with a fine of up to 10% of the business' relevant worldwide turnover in the last business year. Under the newly announced guidelines, firms face a maximum fine of up to 30% of the relevant worldwide turnover (OFT, 2012).

An implicit reasoning behind such policy movement can be that the anti-trust authorities view the level and likelihood of fines to be interchangeable. This relates to the prominent but hitherto untested within a market context '*Beckerian Proposition*' (Dhami and al-Nowaihi, 2013), that constitutes the background of this current study. The Beckerian proposition, relating to crimes, states that the magnitude and the likelihood of punishment are substitutes, as any offsetting change is supposed to achieve the same deterrence incentive.[2] In accordance with the supply of crime literature (Ehrlich, 1973), a

[1] Other jurisdictions in which changes in the fine levels are currently debated include the United States, where on July 8, 2013 the American Antitrust Institute raised voice towards the US Sentencing Commission to increase fines for antitrust offenses, and Germany where on June 25, 2013 the German Federal Cartel Office announced new guidelines for calculating fines that may effectively lead to higher fines.

[2] The rigorous economic analysis of law enforcement and deterrence begins with Becker (1968), who assumes that rational wrongdoers compare the expected returns from violating the law and complying to it. Polinsky and Shavell (1979) extend Becker (1968)'s reasoning to risk-averse agents, and show that the optimal fine may be less than the maximal possible one. Most recently Dhami and al-Nowaihi (2013) use a non-expected utility framework and show that Beckers' proposition holds under rank-dependent utility and cumulative prospect theory. For surveys of the theoretical literature on optimal law enforcement, see Garoupa (1997) and Polinsky and Shavell (2000).

firms' decision to deliberately infringe competition law is based on rational choice. Firms opt to engage in criminal misconduct when the economic gains from participating in an unlawful action exceed the expected costs of the illegal activity. As the economic gain, in general, lies outside the direct control of anti-trust legislation, policy makers are left with two ways to increase the expected cost: they can either increase the likelihood of detection, or they can increase the severity of the imposed punishment. With the components seen as substitutes, it intuitively follows from Becker's (1968) analysis that, in case the law enforcement is costly, the most cost efficient enforcement regime to deter wrongdoers combines the 'severest possible penalty with the lowest possible probability'. That is, enforcement agencies can economize on the cost of enforcement by committing fewer resources to the detection of crime, while achieving the same deterrence effect through an offsetting increase in the fines levied upon wrongdoers. Applying this reasoning to anti-trust enforcements, Becker's pioneering idea advocates minimal screening of markets, combined with huge fines.

The implication of the Beckerian proposition for anti-trust policy is not uncontested. Block and Sidak (1980), for example, provocatively propose that a simple solution for the problem of optimal anti-trust enforcement would be to 'hang a price fixer now and then'. Turning to an economic reasoning however, they question that an optimal anti-trust enforcement would consist of high financial fines and a low detection effort. They argue that such an enforcement regime is suboptimal as it discourages marginal deterrence,[3] leads to inefficient overinvestment in private law enforcement, and, most importantly, may lead to bankruptcy, which is harmful to society. The latter is especially a concern in the presence of judicial errors and has been widely accepted as a main argument against draconian sanctions in anti-trust enforcement.[4]

This study contributes to the ongoing debate on optimal enforcement mechanisms and the role of anti-trust institutions by exploring the Beckerian proposition within a market

[3] For a detailed discussion on marginal deterrence see Stigler (1970).

[4] Landes (1983) explores the optimal antitrust penalty for a variety of enforcement issues such as cartels, joint venture, or predatory pricing. He identifies the optimal penalty to be equal to the net harm times the inverse of the conviction probability.

experiment.[5] Using a market setting is important, as previous research has shown that a change in the experimental frames may or may not provide support for the Beckerian proposition, and thus results can not easily be transferred to the domain of anti-trust infringement. To the extent of our knowledge, the relative effectiveness of an increased likelihood versus an increased severity of punishment in deterring illegal collusion has not yet been studied in existing experimental work. At the heart of this study is therefore a market with three firms, competing in a repeated Bertrand game with inelastic demand and constant marginal cost (Dufwenberg and Gneezy, 2000). Firms can form a non-binding price cartel; however such collusion is illegal and, if detected, can result in anti-trust penalties. We vary the probability of detection and the level of the anti-trust fine in a controlled manner. We additionally include two treatments, which aim to reflect leniency programs (Motta and Polo, 2003). In these treatments, we allow subjects to self-report the existence of a price cartel in return for a reduction in fines.

The main finding of this study is that the Beckerian proposition of the substitutability of fines and detection rates may in general be supported in a market frame. As predicted by theory, different combinations of fine and detection rates with equal expected punishment achieve the same deterrence effect. However, this is only true in an environment without leniency. In the presence of a leniency program, the rate of firms favoring collusion is significantly lower under low detection probability and high fines. More importantly, a high fine and low detection rate under leniency decreases the overall incidence of cartels, which is the ultimate aim of an anti-cartel mechanism. One of the main contributions of this paper is that it provides empirical support for the policy move orchestrated by the OFT. We further experimentally investigate the concern that 'tougher punishment may lead to more severe crime' (Stigler, 1970), since criminals would try to compensate the anticipated loss. In terms of market and asking prices, we find no support for this claim. Finally we observe that deviation and reporting rates are independent of high or low fine combinations.

This study drew on existing experimental work from a number of distinct frames.

[5]For the benefits of a behavioral economic analysis of law see Jolls et al. (1998). Normann and Ricciuti (2009) demonstrate how laboratory experiments can be used for economic policy making.

Initially, the Beckerian proposition has been investigated in the experimental tax evasion literature. Friedland et al. (1978) increase either the tax rate or the fine level, while audit rates change accordingly to guarantee a constant expected punishment of tax evasion. Contrary to their theoretical expectations, they find mild (but not statistically significant) evidence that larger fines tend to be a stronger deterrent than more frequent tax audits. This finding appears to be a common characteristic of experiments on tax compliance. In a review of the experimental results in this area of research, Alm and McKee (1998) report the elasticity of tax evasion with respect to audit rates at ranging from 0.1 to 0.2, while the elasticity with respect to penalty rates is less than 0.1.

Block and Gerety (1995) use a sealed-bid auction in which subjects have the opportunity to illegally communicate and coordinate their bids. They observe the willingness to collude based on changes in either the fine level, the likelihood of detection, or both. Using students and prisoners as subjects, they observe that risk-loving prisoners are more responsive to the detection rates, while risk-averse students respond more to the fine level.

In a risk-taking framework employed by Baker et al. (2003), subjects can choose between an act that yields a moderate financial gain, or a risky choice in which a larger gain is combined with the risk of being caught and punished. The expected gain from choosing the risky option is always larger than the potential fine, which ensures that risk-neutral subjects should select the risky option. Baker et al. (2003) test the effects of uncertainty and risk in both fine level and detection probability. They find that subjects are averse to uncertainty, and the higher the uncertainty with respect to the fine and probability, the fewer the number of subjects who chose the risky option. Surprisingly, the difference in deterrence between risk and uncertainty in both fine and detection rates is small and only marginally significant. They, however, do not focus on the relative effect of a change of the level of the fine as compared to a change in the likelihood of detection.

Anderson and Stafford (2003) analyze the effectiveness of punishment in a public good experiment. For this, they incorporate a third party punishment for free-riders. The authors vary the probability and severity of being punished, and allow for one-shot or repeated interaction. Their results indicate that compliance is increasing in the expected fine, and that a larger fine has a stronger effect on compliance than a higher detection probability. Most importantly, subjects do not consider the probability and severity to be perfect substitutes. The marginal effect of penalty is about one third larger than that of the probability.

That an increase in the severity of punishment may exert a stronger deterrent effect than an increase in detection rate has also been shown in field experiments. Bar-Ilan and Sacerdote (2004) examine whether red light running decreases in response to an increase in the fine, while detection probabilities are being held constant. As predicted by theory, they observe a decrease in violations in response to an increase in fines, with an estimated elasticity of the crime with respect to the fine of -.20 to -.30. In an earlier version of their paper,[6] they further report results from a field experiment that varies the probability of detection while the fine level remains constant. They report the estimated elasticity of red light running with respect to detection likelihood as between -.15 and -.22.

Most recently, with another laboratory experiment, DeAngelo and Charness (2012) conduct an experiment in which the expected cost of a speed-limit violation is being held constant, whilst the probability of detection and the resulting fine are varied. At the beginning, subjects are unaware of the enforcement regime. They then allow subjects to vote on which regime will be enforced. Subjects prefer a high fine and low screening regime. However, once subjects know which regime they are in they do not behave differently to the subjects in the alternative regime.

A direct experimental test of Becker's deterrence hypothesis by Hörisch and Strassmair (2012) uses the context of stealing. Subjects play a mirrored dictator game as in List (2007), in which they can steal from a passive player. Treatments differ in the probability and fine level if stealing is detected. They find that high expected fines significantly reduce the stolen amount; however intermediate expected fines backfire and increase the average amount taken. They further find tentative evidence that detection rate and fine level are interchangeable, which contradicts some of the aforementioned experimental findings.

Table 1 places this study in context with the previous work in this area of literature.

[6] The earlier (2001) working paper is entitled "The response to fines and probability of detection in a series of experiments" and is available at http://www.nber.org/papers/w8638.

Table 1: Related experimental literature.

Authors	Frame	Method	Finding
Friedland, Maital and Rutenberg (1978)	Tax evasion	Variation in either audit rate or fine with constant expected fine	Larger fines are a stronger deterrent than frequent audits (although this is not statistically significant)
Block and Gerety (1995)	Sealed-bid auction	Variation in either detection rate or fine, as well as offsetting change in both	Risk loving (averse) subject are more (less) responsive to a change in detection rate than in fines
Bar-Ilan and Sacerdote (2001, 2004)	Red-light running	Variation in either detection rate or fine with constant expected fine	Elasticity of violation with respect to increase in fines (detection) is between -.20 and -.30 (-.15 and -.22)
Baker, Harel and Kugler (2003)	Risky choice lottery	Variation in the certainty of detection rate and fine	Relative effects not focus of study. Uncertainty increases deterrence
Anderson and Stafford (2003)	Free-riding on Public Goods	Variation in either detection rate or fine, both with increasing and constant expected fines	Marginal effect of an increase in fines is one third larger than of an increase in detection
De Angelo and Charness (2012)	Speeding	Uncertainty over the detection rate or fine. Subjects can vote for high (low) detection and low (high) fine regime	Preference for high fine and low detection regimes. No significant differences in speeding rates
Hoerisch and Strassmair (2012)	Stealing	Variation in detection rate and fine, including treatments with same expected fine	No difference in deterrence for equal expected fines. Only high expected fines deter

Hence, conclusions regarding the applicability of the Beckerian Proposition is mixed at the best. It is important to notice, also, that the insights of the literature to date have not been derived from a market frame.[7] As a result, without specific tests no definitive forecast can be made about the validity of the Beckerian Proposition in a market context either. Moreover, a market differs from the aforementioned frames in at least two dimensions. Whereas violating the law is a individual decision in areas such as tax evasion, speeding or stealing, it is a coordinated action in a market setting. Further, no definite conclusions can be drawn from other frames, as policy tools such as the ability to self-report in exchange for a reduction in fines (known as the 'leniency program') are unique to the market setting. The knowledge gained from using a market frame is likely to guide both legal and economic discussions of rule enforcement, and can help to achieve a richer understanding of how agents in a market react to incentives, in particular in situations where violators of the law are punished. A solid understanding of the deterrent effect of an exogenous increase in the likelihood of an enforcement action, compared to an increase in the severity of the anti-trust fines, is highly relevant to the academic discourse in Law and Economics as well as to anti-trust practitioners.

The remainder of this paper is structured as follows. Section 2 describes the details of the experiment. Results are provided in Section 3. Section 4 concludes.

2 Experiment

2.1 Experimental procedure

The experiment was carried out at one of the Centre for Behavioural and Experimental Social Science (CBESS) experimental laboratories at the University of East Anglia (UEA). Subjects were recruited from a large pool of UEA students via ORSEE (Greiner, 2004). A total of 180 students from various backgrounds and nationalities, without prior

[7] After conducting our experiment, we have been pointed at a recent working paper by Bigoni et al. (2012b), who examine how leniency creates distrust among cartel members. To the extend of our knowledge, it is the only experiment that varies detection rates and fines within a market frame - albeit investigating a very different question and using a very specific setting with duopoly producers of differentiated goods and rematching throughout the experiment.

experience in market experiments, took part. We employed a fixed matching in which every subject was matched with the same other two subjects for at least 20 periods. To avoid end-game effects we implemented a random stopping rule: at the beginning of period 21 and of each following period, there was a 20% chance that the experiment stopped.[8] Due to dependencies of observations between subjects, each group of three constitutes an independent observation. 36 subjects participated in each treatment; hence we have 12 independent observations from each treatment.[9]

The experiment was programmed and conducted using the z-Tree software (Fischbacher, 2007). Upon arrival, subjects were welcomed and seated in the laboratory at visually isolated computer terminals in order to avoid communication between them. The experiment consisted of two parts. In the first part, run on pen and paper, subjects individually had to choose between pairs of risky lotteries in a risk elicitation task (Holt and Laury, 2002). We implemented this to get an indication of the risk preferences of the subjects. A computerized dice throw determined the outcome, but subjects did not receive feedback about their earnings of this part of the experiment until the very end of each session. After completing the Holt and Laury risk elicitation task, subjects were provided with both computerized and printed instructions (reproduced in Appendix B) for the second part of the experiment, and were asked to read them on their own. A questionnaire was used to ensure understanding. After the questionnaire subjects could raise their hand and ask questions, which were answered privately. Finally, after the experiment finished, subjects were asked to fill out a demographics survey.

While earnings for the first part were denoted in British pound, earnings for the second part were recorded in terms of "experimental points", and converted to British pounds at a rate of 15p per point at the end of the experiment. The average payment was £11.41, including an initial endowment of £6 to cover potential losses. At the end of the experiment subjects were paid privately in cash. Sessions lasted between 45 and 60 minutes, and no subject was allowed to participate in more than one session.

[8] Dal Bo (2005) highlights the importance of a random-stopping rule to reduce opportunistic behavior in strategic games such as prisoner dilemma games.

[9] One observation had to be dropped, as two subjects went bankrupt. Treatment pL (see Table 2) hence has 11 independent observations.

2.2 Experimental design

Our experimental design is a modified version of the cartel formation game in Gillet et al. (2011). Subjects play the role of a firm with a constant cost of production of 90. They face a repeated homogeneous-goods discrete Bertrand oligopoly as in Dufwenberg and Gneezy (2000) with three firms operating in a given market. In each period firms have to simultaneously decide if they want to form a non-binding cartel. If all three competitors in a given market decide to collude, they are informed that they mutually promised to charge the highest possible price. Firms then simultaneously select a price p from the discrete choice set $\{90, 91, ..., 102\}$, but are not obliged to set their agreed-upon price. The firm charging the unique lowest price p^{min} earns the full market profit $p^{min} - 90$, while firms with a higher price receive no earnings. In case of ties, firms split the profit evenly.

In all but one treatment, reaching a price agreement comes at the risk of an anti-trust fine, which is levied upon firms by a fictitious Anti-trust Authority. The novelty of our design comes from the controlled variation of the likelihood of getting detected and the magnitude of fines between the treatments. The detection *probability* can be either "low" (henceforth indicated by a small p) or "high" (hereafter indicated by a capital P). Likewise, *fines* can be either "low" (from now on indicated by a small f), or "high" (henceforth indicated by a capital F). This allows us to experimentally distinguish the deterrent effect of fines and detection probabilities.

Further, two treatments allow firms to self-report the existence of a price cartel in return for a reduction in fines. This makes it possible to explore the robustness in the presence of an important policy that is unique to a market frame. The so called "leniency programs" offer cartel members the opportunity to report their illegal conduct in exchange for full immunity or a reduction of anti-trust penalties (Motta and Polo, 2003). Examining the validity of the Beckerian proposition with and without leniency does also allow us to give policy advice for countries with anti-trust enforcement that have not (yet) introduced a leniency policy, such as Indonesia or the Philippines. Similiar to Hinloopen and Soetevent (2008), self-reporting costs one experimental point. This is implemented in order to prevent firms to punish a deviating firm for free. Fines in these treatments with leniency are denoted with l and L, respectively.

In Table 2 we summarize the treatments:

Table 2: Treatment Table

Probability	Fine	without Leniency	with Leniency
10%	8	Low Detection rate, High Fine (pF)	Low Detection rate, High Fine (pL)
20%	4	High Detection rate, Low Fine (Pf)	High Detection rate, Low Fine (Pl)
0%	0	Baseline (B)	

We assume an infinite repose period in which the liability for the illegal collusion lasts until the agreement has been detected or revealed by means of a leniency application. This implies that a firm which stops colluding or deviates from its agreement, can still be fined for its previous misconduct. At the beginning of each period of the experiment, firms are informed whether or not they are liable for a previous agreement. While firms can renew their agreement, they can not end a potential previous liability.

The timing and information structure of the game with and without the leniency stage is summarized in Figure 1:

1. Each firm expresses its willingness to reach an agreement over prices by selecting the appropriate button. If all firms in a given market wish to collude, they enter a non-binding agreement to choose the joint profit maximizing price of 102. If at least one firm decides not to collude, firms are informed about each rivals' choice and competition takes place in the market.

2. Each firm chooses its price from the set $\{90, 91, \ldots, 101, 102\}$. Firms then observe all prices in their market, and learn whether their price is the lowest of the three submitted prices.

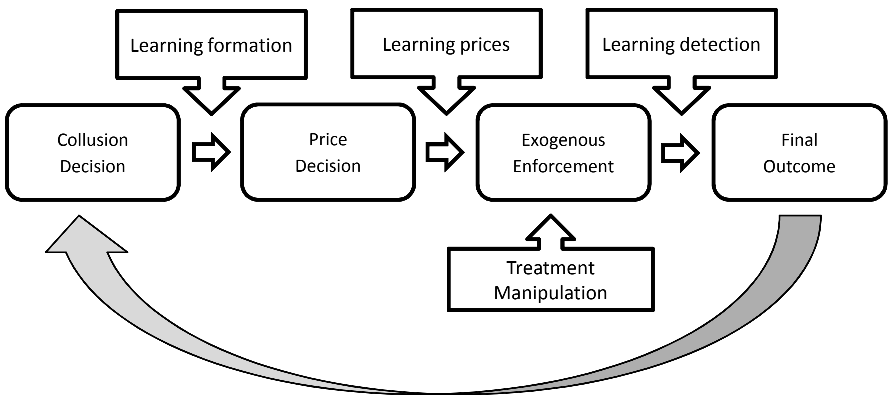
3. In the treatments with leniency, each firm can decide to reveal the existence of a cartel at the expense of one experimental point. If a firm is the sole self-reporter, it gains complete immunity from fines whereas the other firms have to pay the full fine. If two firms report, their fine is reduced by half. In case all three firms report, the fine is reduced by one-third. If no firm self-reports, the cartel may be detected by the Anti-trust Authority with the detection probability specified in the treatment.

4. In the last step, firms are informed about their final earnings, whether collusion was detected or not, and about the number (but not identity) of the whistleblowers.

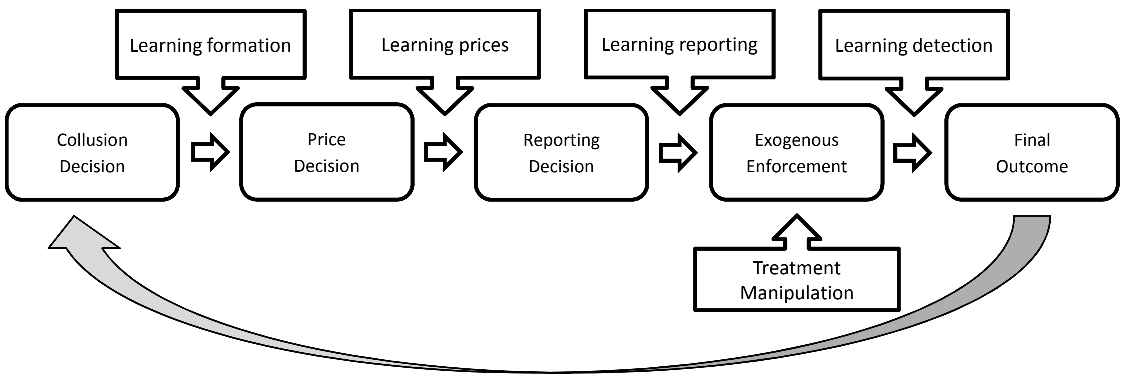
At the end of the experiment, the number of experimental points earned in each period minus the penalties paid is converted into cash. The earnings from the Holt and Laury task and the Bertrand game are then summed up and paid out in private.

Figure 1: Game Tree

a) without the leniency stage.



b) with the leniency stage.



2.3 Theoretical framework

Across all treatments the competitive Bertrand equilibrium is to select a price of 91. Any further reduction of the price to the level of the marginal cost of 90 does not provide an increase in expected earnings. Absent collusion, a firm thus realizes a competitive profit of $\pi^{comp} = (91 - 90)/3$. However, firms can coordinate on prices above the competitive equilibrium by choosing to collude. The joint profit maximizing price is 102, which yields per period collusive profits of $\pi^{coll} = (102 - 90)/3$.

Engaging in price fixing comes at the risk of anti-trust enforcement. Let ρ denote the probability that a fine is effectively imposed upon a colluding firm. We chose ρ based on the estimation of cartel detection rates by Bryant and Eckard (1991), who report rates between 13%-17%. Previous market experiments used either 10% (Bigoni et al., 2012a) or 15% (Hinloopen and Soeteven, 2008; Gillet et al., 2012). We select 10% and 20% in order to ease mental accounting and understanding for our subjects, while simultaneously selecting detection rates that can be observed in the real world.

Once a firms' engagement in an illegal cartel has been detected, the exogenous Anti-trust Authority levies a fine F upon firms. Set $F = \begin{Bmatrix} 4 \text{ if } \rho = 20\% \\ 8 \text{ if } \rho = 10\% \end{Bmatrix}$, where the "low" fine of 4 reflects a firm's one-shot profit from colluding, while a "high" fine of 8 equals twice the gain from colluding. It is imporant to note that the per-period expected fine ρF is constant across treatments. The net present value of the expected fine payments, given an infinite repose, is $\rho F + (1-\rho)\delta\rho F + (1-\rho)^2\delta^2\rho F + ... = \frac{\rho F}{1-\delta(1-\rho)}$. Finally, assume that collusion is enforced via grim-trigger strategies. A deviating firm then slightly undercuts the collusive price, and gains a one shot profit of $\pi^{dev} = 101 - 90$, followed by reversion to the competitive equilibria.

The incentive compatibility constraint (ICC) for the Baseline is then:

$$\frac{4}{1-\delta} > 11 + \delta\frac{1/3}{1-\delta} \tag{1}$$

Similarly, the ICC for a treatment absent leniency is given by:

$$\frac{4}{1-\delta} - \frac{\rho F}{1-\delta(1-\rho)} > 11 - \frac{\rho F}{1-\delta(1-\rho)} + \delta\frac{1/3}{1-\delta} \tag{2}$$

The left-hand side (LHS) of equation (1) and (2) consists of the net collusive profit, which is the infinite gain from collusion minus the expected fine payment. The right-hand side (RHS) of equation (1) and (2) is the one-shot profit from deviation plus the expected earnings from competition, minus the expected fine payment. Note that the critical threshold for the discount factor in (1) and (2) is identical. As in the framework of Becker (1968), the theoretical prediction would therefore not expect any significant differences between the treatments *pF* and *Pf.*

Furthermore, note that in the presence of leniency, the optimal deviation strategy is to report at the expense of $c^{report} = 1$. The ICC for a treatment with leniency is then:

$$\frac{4}{1-\delta} - \frac{\rho F}{1-\delta(1-\rho)} > 11 - 1 + \delta\frac{1/3}{1-\delta}. \tag{3}$$

Here, the LHS consists of the net gain from collusion, while the RHS consists of the one-shot profit from deviation and reversion to competition, minus the cost of a leniency application .

2.4 Hypotheses

Insights from the Law and Economics literature, existing experimental findings and the theoretical framework discussed previously offer predictions that we can examine within our experiment. The analysis will focus on three parameters: First, we seek to investigate cartel formation, which can be measured by observing either the propensity to collude (i.e., the rate at which firms favor collusion), or the actual incidence of collusive markets. The null hypothesis, which is supported by our theoretical model, is that severity and probability of punishment is substitutable (Becker, 1968). In our experimental setting, this translates that firms respond in the same way to an increase in the likelihood of an enforcement action while the magnitude of fines decreases, as to an increase in the severity of the anti-trust fines while the probability of detection falls. The alternative hypothesis is that higher fines have a larger deterrence effect (Anderson and Stafford, 2003), both with and without a leniency policy in place. Hence we expect less cartel activity in both pF and pL, as compared to Pf and Pl. Second, we consider the impact on asking and market prices. In our theoretical framework, the parameters of the enforcement regime do not influence the profit-maximizing price. Our null hypothesis is therefore that prices do not differ between the treatments. Stigler (1970) mentions that tougher punishment may well lead to a more severe crime. In a market frame, a more severe crime means that a firm charges higher prices, as firms aim to compensate an increased fine by higher gains from collusion (Jensen et al., 2013). Our alternate hypothesis is therefore that prices are higher when fines are large. Finally, we explore cartel stability, by observing how often firms within a cartel deviate from the joint maximization price, and how often firms self-report in case of leniency. As incentive constraints in our model are satisfied for all treatments, a colluding firm should stick to a collusive agreement and not apply unilateraly for leniency, independent of the detection rate and fine level. Our null hypothesis is therefore that there will be no difference between our treatments. We will test this against the alternating hypothesis that there will be no difference in the treatments without leniency, but more self-reporting and deviations in pL than in Pl. We expect this, because deviating firms will try to avoid high fines by reporting.

In summary, we have the following hypothesis which we test against the null hypothesis:

H0: There are no differences between pF and Pf and between pL and Pl in terms of:

a) communication attempts and cartel formation,

b) asking and market prices,

c) cartel stability.

H1A: With higher fines, firms are less likely to collude and there is a lower number of cartelized markets, both with and without leniency: $r_{pF} < r_{Pf}$ and $r_{pL} < r_{Pl}$.

H2B: Market and asking prices are higher in F and in L: $p_{pF} > p_{Pf}$ and $p_{pL} > p_{Pl}$.

H3C: There will be more self-reporting and deviations in pL than in Pl.

3 Results

The results are presented in three parts. First, we evaluate the effectiveness of different detection-fine combinations at deterring cartel activities. Next we compare the resulting prices and present a welfare analysis. Finally, we take a closer look at the stability of the cartel agreements. Throughout the paper all tests are performed with the entire sample, but restricting the analysis to observations from round 1 to 20 replicates the results. We do, however, use the restricted sample of 20 rounds to display dynamics over time, in order to avoid misrepresentations caused by the unbalanced number of observations in later rounds. Table 12 in Appendix B provides details about our sessions, including the number of rounds played. Unless stated otherwise, we use the non-parametric two-sided Mann-Whitney U-Test for the pairwise comparison of our treatments. Because of the dependency of observations within a market, the average statistic of all three firms constitutes one unit of observation. We further carry out a panel data analysis which will be reported in the first subsection.

3.1 Cartel activity

In this section we test whether combinations of detection rate and level of fines resulting in equal expected fines are equally successful in deterring collusion, or whether high fines act as a stronger deterrent (Hypothesis 1A). The experiment allows us to answer this question by means of two key indicators of cartel activity that are commonly used in the literature (Hinloopen and Soetevent, 2008; Gillet et al., 2011; Bigoni et al., 2012a). The first one is the *propensity to collude* - the percentage of firms in favor of cartel formation.[10] Our second indicator is the *rate of cartelized markets* - the percentage of markets in which a cartel exists, taking into account that undetected cartels carry over into later periods. In order to test for differences across treatment, we aggregate a firm's binary decision to favor collusion in Stage 1 of the game over all periods and markets.

[10] To check for robustness, we also conducted a similar analysis using a more restrictive notion of collusion attempts. In particular, we derived the propensity to collude using only observations from periods in which a firm was not already liable for collusion. We further investigated periods in which no cartel has been formed previously, and periods in which a previous cartel has been formed but has been detected/reported. The results do not differ qualitatively.

In the first step, we will focus on the propensity to collude for which Table 3 contains the descriptive statistics.

Table 3: Propensity to collude - Average results per treatment.

Probability	Fine	without Leniency	with Leniency
10%	8	50.74 (19.04)	53.84 (10.42)
20%	4	49.74 (16.97)	64.67 (12.33)
Baseline:		76.69 (16.99)	

Notes: Standard deviations are in parenthesis. The propensity to collude is computed using the binary firm decision to attempt collusion or not. We use the per-period average of a market as the unit of observation, and aggregate first over periods, then over all markets.

Not surprisingly, we note that in comparison with our baseline treatment, all antitrust sanctions effectively deter cartel formation. Of greater interest, however, is that the difference in the propensity to collude across treatments in which an enforcement regime is in place is statistically significant (Kruskal-Wallis test, $p < 0.01$).[11]

In order to get a grip on what drives the observed differences, we first focus on the existence (or absence) of a leniency policy. For a first statistical analysis we pool the observations of treatments with and without leniency. A comparison reveals that the propensity to collude is statistically significantly different in the presence of a leniency program (Mann-Whitney test, $p = 0.04$), with an average rate of 59.26% with and 50.24% without leniency. That the propensity to collude is higher if a leniency policy exists is not surprising, as it hints at the pro-collusive effect of leniency first described in Motta and Polo (2003). It is important to note that the propensity to collude only explains the intention to collude and not the actual rate of cartel formation. Next, we turn to a comparison of pF vs. pL and Pf vs. Pl, in order to test if the pro-collusive effect exists for both detection-fine ratios. A bivariate test yields no significant differences between the two treatments with low detection rate and high fines, but collusion attempts are significantly more frequent in the Pl than in the Pf treatment (Mann-Whitney test,

[11] We also investigated the attempt to collude at the very first period of the experiment, which can be seen as a measure of pre-deterrence. There is no significant difference between the enforcement regimes.

$p = 0.03$).

Our subsequent focus is on the second potential driver, i.e., the difference between the detection rates and fines. For that, we pool *pL* and *pF* and compare them with the pooled observations of *Pf* and *Pl*. We do not detect any statistically significant differences in the propensity to collude between different detection-fine regimes (Mann-Whitney test, $p > 0.1$). This result would suggest that the Beckerian proposition can be supported in a market frame, and that fines and detection rates are indeed substitutable. However, as treatments with and without leniency differ in their respective deterrence, we need to assess the substitutability of fine and detection rates for each policy regime separately. Table 4 documents the two-sided p-values of pairwise Mann-Whitney comparisons.

Table 4: Propensity to collude - Two-sided p-values of pairwise MWU-Test.

	pF	**Pf**	**pL**	**Pl**
Baseline	0.0039***	0.0016***	0.0081***	0.0325**
pF		1.0000	0.6225	0.0646*
Pf			0.7583	0.0282**
pL				0.0488**

Notes: * p<0.10, ** p<0.05, *** p<0.01

The table can be read in the following way: First, we obtain no statistically significant difference between the treatments without leniency. This is particularly interesting, as it supports the substitutability of fine and detection rates to achieve the same deterrence. However, the table also reveals that the difference in the propensity to collude between *pL* and *Pl* is statistically significant (Mann-Whitney test, $p = 0.05$). This finding is new to the experimental literature and questions if Becker's proposition holds for markets regardless of whether a leniency policy is in place. We summarize the findings described so far in the following outcome:

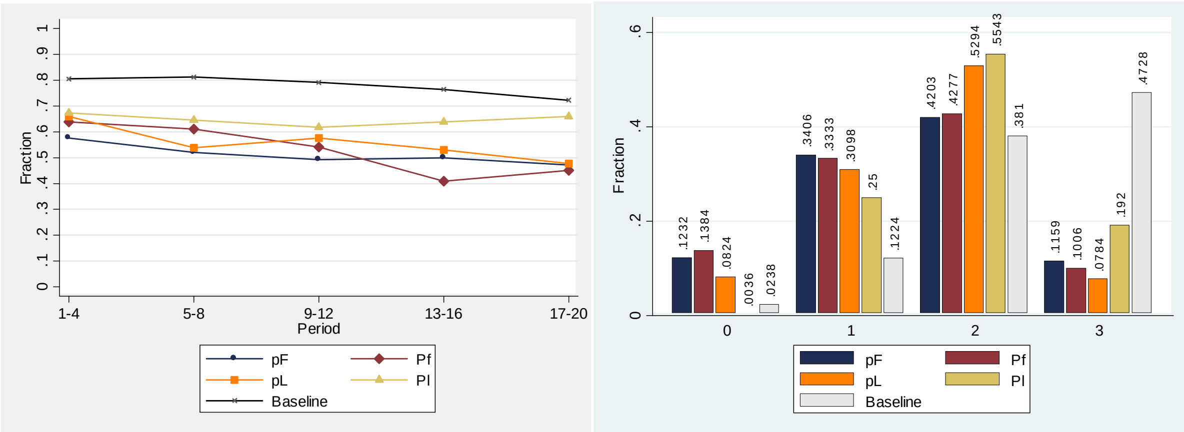
Outcome 1:

There is no statistically significant difference in the propensity to collude between pF and Pf, but between pL and Pl.

To attain a more concrete understanding of a firm's decision to favor collusion, we now consider the evolution of the propensity to collude over time. The dynamics of the fraction of firms that favor collusion are tracked on the left hand side of Figure 2, in which we have divided the time dimension into four blocks of four periods. The right hand side of Figure 2 depicts a histogram of the number of firms in a market that were willing to collude. For the former, note that collusion rates tend to decline mildly over time, with the exception of *Pl* which slightly converges towards the Baseline. For the latter, note that in our framework cartel formation is a unanimous decision: a cartel is only formed if all three firms expressed their willingness to collude. We observe that treatments with leniency have the highest number of "all-but-one" cases, which is in line with previous findings by Hinloopen and Soetevent (2008). Most importantly, the right hand side of the histogram depicts the rate at which cartels are being formed (i.e. all three firms agreed to collude, regardless of the existence of a cartel in previous periods).[12] A pairwise comparison of the rate at which cartels are being formed reveals no statistically significant differences between *pF* and *Pf*, but the observed higher rate in *Pl* than in *pL* is mildly statistically significant (one-sided t-test, $p = 0.05$). Note that the lowest rate of cartel formation is in the *pL* treatment. This is an indicator that in the presence of a leniency program, high fines and low detection rates seem most effective in deterring cartels.

[12] Bigoni et al. (2012a) report the rate at which firms start a new cartel, provided they are not already in an existing cartel. The equivalent rates in our experiment are: pF: 5.11; Pf: 5.66; pL: 7.36; Pl: 15.25. Restricting the analysis to observations without previous liability does not affect our results.

Figure 2: Evolution of the fraction of firms who wish to form a cartel (Left) and histogram of the number of firms willing to form a cartel (Right)



The preliminary evidence indicates that under traditional enforcement fine and detection rate are indeed substitutable, but a lower detection rate and higher fine regime is a better deterrent in the presence of a leniency program. The analysis so far is, however, incomplete as we aggregated the individual decision in each market and hence did not fully explain which factors explain this result at the firm-level. In the next step of our analysis, we therefore conduct a regression analysis in which we treat each firm as a unit of observation in order to better understand the behavioural forces that drive our initial findings. The model explains a firms' individual decision to engage in a cartel by means of a dynamic random-intercepts logit model where the dependent variable is the binary choice to attempt collusion. To account for potential random disturbances caused by the group composition, we employ the random-effect at the level of markets. In addition to treatment dummies, we define a period and period-squared variable to correct for a potential trend over time. Independent variables further include the lagged decision to collude in the previous period ($Decision\ to\ collude_{t-1}$), a dummy indicating whether or not a cartel has been successfully formed in the previous period ($Cartel\ formed_{t-1}$) and a dummy indicating whether a cartel has been detected ($Cartel\ detected_{t-1}$) or reported ($Cartel\ reported_{t-1}$) in the previous period. Further, we use a dummy which takes the value 1 if a cartel existed in the previous period and at least one member deviated from the optimal cartel price by charging a price below the collusive one. In a further set of estimations we also add other variables. In model 2, we control for individual risk preferences by including the number of risky choices that were made during the Holt and Laury task, as well as a dummy variable (*inconsistent preferences*) to control for subjects that expressed inconsistent risk attitudes by switching more than once between the safe and risky lottery option.[13] Finally, in model 3 we use the number of times a firm has so far been involved in a cartel, as well as the number of times its engagement in a cartel was detected or reported, as alternative explanatory variables. Table 5 displays the results of the regressions.

For the regressions in the three columns on the left-hand side, the pF treatment is used as a benchmark, represented by the constant term. On the right-hand side, we use the pL treatment as our benchmark in order to investigate the effect of a different detection-fine regime given the presence of a leniency program. The logit regression confirms our initial results from the non-parametric analysis. The coefficient of the treatment dummy Pf is not statistically significant, indicating no difference in deterrence, while the estimated coefficient Pl is of positive sign and significant at the 5% level. With respect to the other variables, we make the following observations. First, there is strong evidence that the previous periods decision to collude, represented by the $\textit{Decision to collude}_{t-1}$ dummy, is an important factor for the current decision. Second, we do not obtain a statistically significant effect of time between the treatments. Whether or not a price deviation occurred in the previous period also seems irrelevant. As undetected cartels carry over into the next periods, having formed a cartel in the previous period negatively affects the odds to decide to collude. Further, experiencing an anti-trust action has a deterrence effect by reducing the odds to collude. Interestingly, $\textit{Cartel formed}_{t-1}$ and $\textit{Cartel detected}_{t-1}$ does not turn significant in the regressions with leniency. The size and sign of the coefficient cartel reported indicate that experiencing self-reporting rather than exogenous detection is among the main factors that influence a firm's decision not to collude again.

[13] Controlling for socio-demographic characteristics such as age, gender and nationality does not affect the sign or significance of the estimated coefficients.

Table 5: Decision to collude - Random effects logistic regression.

	without Leniency (Base: pF)			**with Leniency** (Base: pL)		
	Model 1	Model 2	Model 3	Model 1	Model 2	Model 3
Decision to collude						
Pf	0.0411	0.165	0.236			
	(0.250)	(0.271)	(0.281)			
Pl				0.381^{**}	0.376^{**}	0.314^{**}
				(0.170)	(0.169)	(0.146)
Decision to collude$_{t-1}$	2.933^{***}	2.766^{***}	2.582^{***}	3.571^{***}	3.563^{***}	3.179^{***}
	(0.139)	(0.142)	(0.132)	(0.163)	(0.163)	(0.142)
Period	-0.00630	-0.00995	0.0321	-0.0166	-0.0182	0.0129
	(0.0390)	(0.0394)	(0.0412)	(0.0446)	(0.0446)	(0.0449)
Period^2	-0.000646	-0.000591	-0.00115	-0.000307	-0.000249	-0.000566
	(0.00140)	(0.00141)	(0.00141)	(0.00164)	(0.00165)	(0.00161)
Cartel formed$_{t-1}$	-0.764^{***}	-0.640^{***}		0.648	0.859	
	(0.220)	(0.233)		(0.580)	(0.595)	
Cartel detected$_{t-1}$	-0.625^{**}	-0.617^{**}		-0.298	-0.425	
	(0.288)	(0.303)		(0.480)	(0.487)	
Cartel reported$_{t-1}$				-2.278^{***}	-1.674^{**}	
				(0.583)	(0.686)	
Price deviation$_{t-1}$	-0.0355	-0.0870		-0.908	-0.891	
	(0.174)	(0.181)		(0.583)	(0.586)	
Risk choice		0.159^{***}	0.168^{***}		0.0321	0.0248
		(0.0270)	(0.0270)		(0.0291)	(0.0273)
Inconsistent preferences		-0.251	-0.279		-0.209	-0.0940
		(0.241)	(0.242)		(0.205)	(0.185)
# of times busted			-0.257			-0.409^{***}
			(0.163)			(0.151)
# of times colluded			-0.0216			0.320^{**}
			(0.0228)			(0.129)
Constant	-1.216^{***}	-1.898^{***}	-2.153^{***}	-1.309^{***}	-1.386^{***}	-1.609^{***}
	(0.302)	(0.335)	(0.334)	(0.288)	(0.309)	(0.299)
Observations	1710	1710	1710	1524	1524	1524

Standard errors are in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Let us now turn to the role of risk preferences in Model 2. While controlling for risk preferences does not change the sign or statistical significance of the previously mentioned coefficients, risk choice turns out significant in the comparison of the treatments without leniency, but not in the comparison with leniency. We offer a possible behavioral explanation for this finding in the Discussion section of this paper.

To include both fixed (at the firm) and random effects (at the market level), we further run an alternative random-intercept logistic regression, using a Generalized Linear Latent and Mixed model (GLAMM). GLAMM has previously been used in a similar statistical analyses by others such as Bigoni et al. (2012a) to account for correlations between observations from the same firm, and from different firms belonging to the same market. The estimates confirm the robustness of our results from our logit model, and are reported in the Appendix A. We thus summarize our findings in the following statement:

Result 1: Propensity to Collude

Absent leniency, fine and detection rate are substitutes with respect to their deterrence. When leniency exists, a lower detection probability and higher fine regime is significantly stronger in deterring firms to favor collusion than a higher detection and lower fine regime.

Up to now we have discussed the effects of different policy regimes on collusion attempts and formation. Of greater interest however is whether any policy regime is more successful in reducing the actual incidence of cartels. We address this question by observing the number of cartelized markets, meaning markets at which a non-binding cartel agreement was in place at the price decision stage.

Table 6: Cartelized Markets - Average results per treatment.

Probability	Fine	without Leniency	with Leniency
10%	8	44.69 (38.13)	08.27 (09.28)
20%	4	29.32 (21.29)	20.41 (21.26)
Baseline:		90.31 (28.56)	

Notes: Standard deviations are in parenthesis. The percentage of cartelized markets is computed using a dummy that takes the value 1 if a given market has an existing (newly formed or old) cartel, and 0 otherwise. We use the per-period average of a market as the unit of observation, and aggregate first over periods, then over all markets.

As can be seen in Table 6, anti-trust regimes differ greatly in the resulting number of cartelized markets. We find that with anti-trust enforcement between 8.27% and 44.69% of all markets were cartelized, while in a laissez-faire environment almost every market is collusive. At this stage it is wise to recall that a cartel that has been formed continues to exist until it got detected (or, for the leniency treatments, reported), which explains these extraordinary high rates of collusion in the baseline. At a first glance, the table also reveals that the rate varies along two dimensions. There seem to be fewer cartels with than without leniency, and there seems to be a difference between low fine and high detection and high fine and low detectiom regimes. Particularly interesting is the opposite trend between the latter: While our results indicate less cartels for high detection rates and low fines absent leniency, the opposite pattern emerges with leniency. The difference in the percentage of cartelized markets across treatments with anti-trust enforcement is statistically significant (Kruskal-Wallis test, $p = 0.04$).

Similar to the former analysis, we start by observing the effects of having a leniency policy in place by comparing the pooled observations of pF and Pf vs. pL and Pl. As expected, leniency is successful in reducing the incidence of cartels and displays statistically significantly lower rates of cartelized markets (Mann-Whitney test, $p = 0.01$). The mean rate drops from 37.01% in the absence of leniency to 14.34%. The relatively small difference between treatments with and without leniency in terms of the rate at which cartels form, presented in Figure 2, can not explain this result. Other than the effect on deterrence that we discussed previously, leniency must therefore also have a desistance effect - and this effect is well established in the literature (Bigoni et al., 2012a). A closer analysis reveals that the rates observed in Pf and Pl are not statistically significantly different. However, less cartels exist in pL than in pF (Mann-Whitney test, $p = 0.02$). This might indicate that the effect of leniency is jeopardized if the incentive to avoid fines is low.

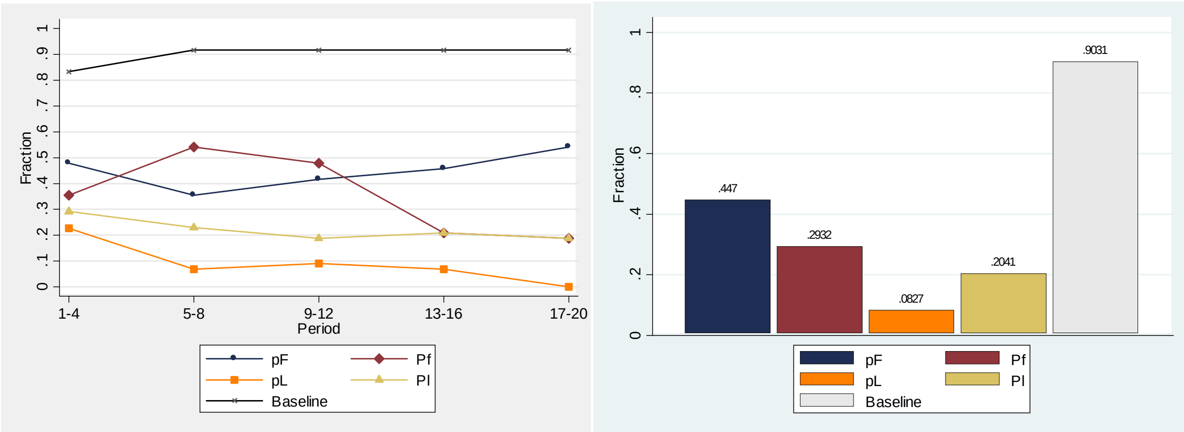
In the follow-up analysis, we pool low detection-high fine (pF and pL) and high detection-low fine (Pf and Pl) treatments, but observe no statistically significant difference between them. Finally, we compare pF vs. Pf and pL vs. Pl in order to test for the substitutability of detection rate and sanctions with and without leniency. We find support for Becker (1968), as we cannot reject our null hypothesis of equal population means for low detection rates and high fines without leniency (Mann-Whitney test, $p = 0.56$). However, there is mild evidence that a higher fine and lower detection regime reduces the number of cartelized markets in the presence of leniency (one-sided t-test, $p = 0.04$) which questions the general validity of the Beckerian proposition. We summarize:

Outcome 2:

There is no statistically significant difference in the number of cartelized markets between pF and Pf, but between pL and Pl.

To complete the analysis, we now turn to a graphical representation of the effects of different policy regimes on the rate of cartelized markets. The left hand side of Figure 3 depicts the fraction of cartelized markets over time as observed in our data. The right hand side of Figure 3 depicts the average fraction of collusive markets aggregated over all periods, and highlights the relative effectiveness of each treatment in reducing the occurrence of cartels.

Figure 3: Average fraction of cartelized markets (Left) and the average fraction over all periods (Right).



The figure reveals that at any moment in time, fewest cartels were operating in the pL treatment, followed in order by Pl and the two treatments without leniency.[14] We can now present our second result:

Result 2: Cartelized Markets

Absent leniency, fine and detection rate are substitutes with respect to the occurrence of cartels. When leniency exists, a lower detection probability and higher fine regime is significantly stronger in reducing the number of active cartels than a higher detection and lower fine regime.

3.2 Asking and Market Prices

The analysis so far has very much focused on the participation in the cartel, although a change in the level of fines and detection rates might also affect the price that colluding firms charge. An Anti-trust Authority that cares about consumer welfare will try to achieve lower prices as a result of a change in the enforcement regime, or at least it will try to prevent an increase in prices.

How a wrongly designed enforcement regime can provide incentives to commit a more severe crime was first discussed by Stigler (1970), and has recently been explored by Jensen et al. (2013). They show in a theoretical model that firms might react to higher

[14] Towards the last periods, all cartels in pL ceased to exist due to detection or self-reporting.

fines by increasing their prices. In this subsection, we experimentally examine this suggestion by comparing the resulting prices (and hence consumer welfare) under each anti-trust regime. We ask if higher fines and lower detection probabilities diminish consumer welfare (Hypothesis 2B).

We start to address this question by investigating the *asking price*, which is the average of the three stated prices in a given market in a particular period. Table 7 yields the asking prices for all treatments, and distinguishes between the price charged in rounds with and without a cartel. At a first glance, three main insights emerge from that table. Prices do not appear different when varying detection probability and magnitude of fines, but they appear higher in collusive than in competitive markets. Further, it is not obvious if prices are substantially different given the presence or absence of a leniency policy.

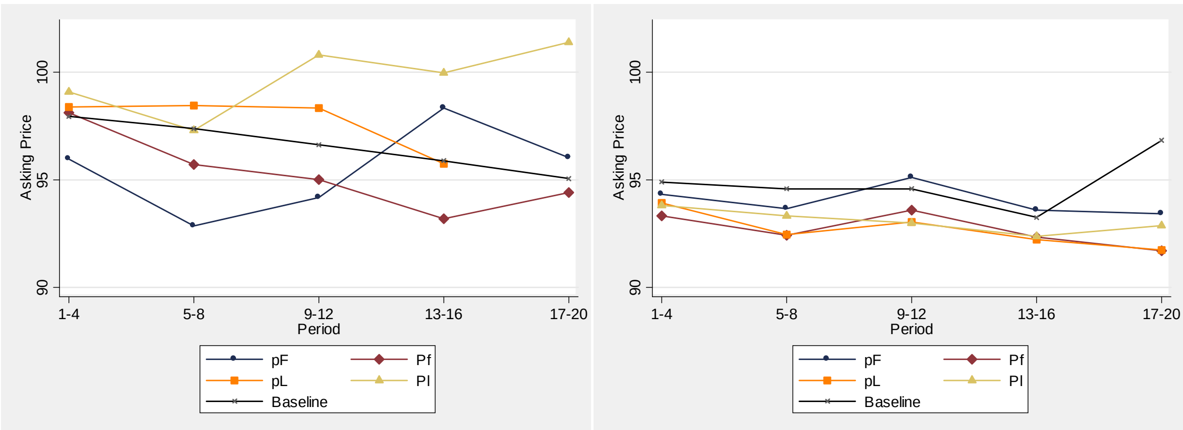
Table 7: Asking Prices - Average results per treatment.

		Collusive		Competitive	
Probability	**Fine**	**without Leniency**	**with Leniency**	**without Leniency**	**with Leniency**
10%	8	95.79 (2.90)	97.83 (2.11)	93.81 (1.92)	92.56 (1.59)
20%	4	95.66 (2.11)	98.06 (2.90)	92.36 (0.86)	93.01 (1.08)
Baseline:		96.23 (2.69)		95.96 (2.44)	

Notes: Standard deviations are in parenthesis. Asking prices are calculated using the average of the three stated prices in a market. We use the per-period average of a market as the unit of observation, and aggregate first over periods, then over all markets.

One may argue that the experimental design allows firms to tacitly collude to avoid detection, which would make it impossible to discuss consumer welfare. If firms were indeed tacitly colluding, one would expect no significant difference between asking prices within and outside of a cartel. Hence it is important to notice that there exists a clear gain from colluding, as asking prices are between 3 and 4 points higher in collusive than in competitive markets. These findings appear all the more remarkable as the gain from colluding exists even though the cartel agreement was not binding, and no actual communication by means of, for example, a chat took place. Further, we observe that asking prices from competitive markets are not statistically different across treatments (Kruskal-Wallis test, $p > 0.1$). This is intuitive, as absent collusion firms face identical decisions across our treatments. There is however mild statistical evidence that asking prices are different for collusive markets (Kruskal-Wallis test, $p = 0.09$). This in fact supports the findings of Bigoni et al. (2012a), who report statistically higher prices inside, but not outside of cartels. The difference can be visualized when we compare the price dynamics over time. Figure 4 depicts the per-period average asking prices for collusive and competitive markets. The figure reveals a tendency for more dispersed prices in collusive markets, while prices in competitive markets move almost parallel with little differences over time. Note that towards the end of the experiment there were no collusive markets in treatment pL, which explains the lacking prices for this treatment.

Figure 4: Asking prices for collusive (Left) and competitive markets (Right).



Turning to statistical tests, for which we focus only on collusive markets, we compare the asking price with and without leniency. We find that asking price are about 2 points higher in the presence of leniency, and this difference is statistically significant (Mann-Whitney test, $p = 0.01$). Higher cartel prices in treatments with leniency are also reported in Bigoni et al. (2012a), who emphasize that in the presence of a leniency program firms undercut the agreed-upon price and self-report. Hence they reason that any punitive price-war will be occur in competitive markets, while absent leniency the price war might take place within the cartel. A similar reasoning can be applied to our experimental design, which may artificially inflates prices in treatments with leniency.

In the next step, we check if this effect of leniency also exists independent of the fine-detection ratio. We find no statistically significant difference between the asking prices pF and pL, but for high detection rates and low fine there is mild evidence of a statistical difference between Pf and Pl (Mann-Whitney test, $p = 0.08$). A more detailed comparison shows that the difference between low detection rates and high detection rates is neither statistically significant for pL vs. Pl, nor for a comparison between pF and Pf. In other words, our analysis provides no statistical support for the suggestion that firms react to higher fines by raising their asking prices. We have to conclude that fine and detection ratios are indeed substitutable with respect to their effect on asking prices.

We summarize:

Outcome 3:

There is no statistically significant difference in the asking price across treatments.

To complete the analysis, let us now examine the *market price*, i.e. the lowest price charged by any firm in a market. Similar to our analysis of asking prices, Table 8 yields the market prices for all treatments, differentiated between the price charged in rounds with and without a cartel.

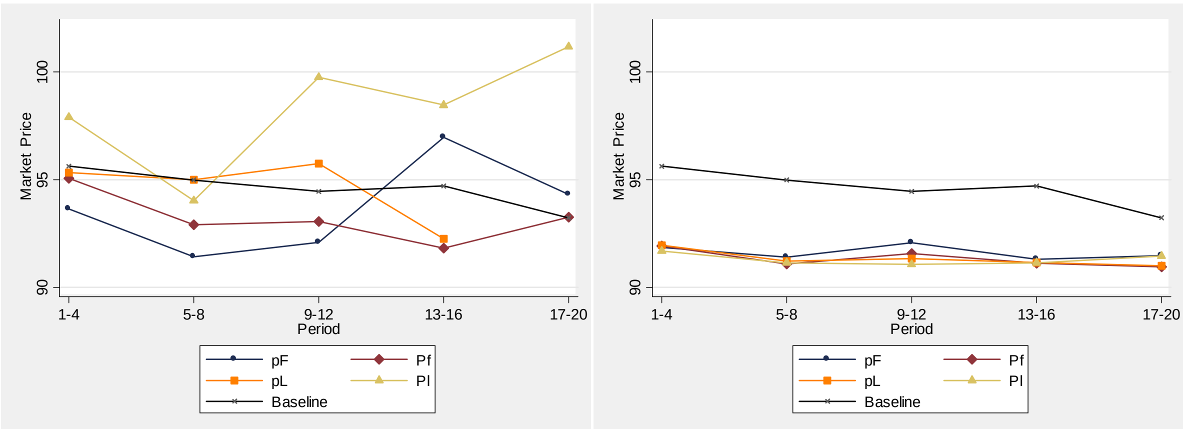
Table 8: Market Prices - Average results per treatment.

		Collusive		Competitive	
Probability	**Fine**	**without Leniency**	**with Leniency**	**without Leniency**	**with Leniency**
10%	8	93.82 (3.25)	94.61 (3.11)	91.56 (0.90)	91.33 (1.20)
20%	4	93.04 (1.99)	95.47 (4.37)	91.20 (0.32)	91.29 (0.68)
Baseline:		94.32 (2.68)		93.62 (1.25)	

Notes: Standard deviations are in parenthesis. Market prices are calculated using the minimum of the three stated prices in a market. We use the per-period average of a market as the unit of observation, and aggregate first over periods, then over all markets.

We make the following two observations. First, notice that market prices in collusive markets are about 3 points above the prices in competitive markets. This seems to support the very clean evidence for a gain from collusion that we identified previously. Second, notice that different enforcement regimes have essentially no effect on market prices in competitive markets. Prices absent collusion are close to the theoretical Bertrand equilibrium. Furthermore, the prices in collusive markets are about 8 points below the joint profit maximizing price which indicates the existence of price deviations. These regularities becomes easily recognizable in Figure 5, which reports the evolution of market prices over time, both for collusive and competitive markets.

Figure 5: Market prices for collusive (Left) and competitive markets (Right).



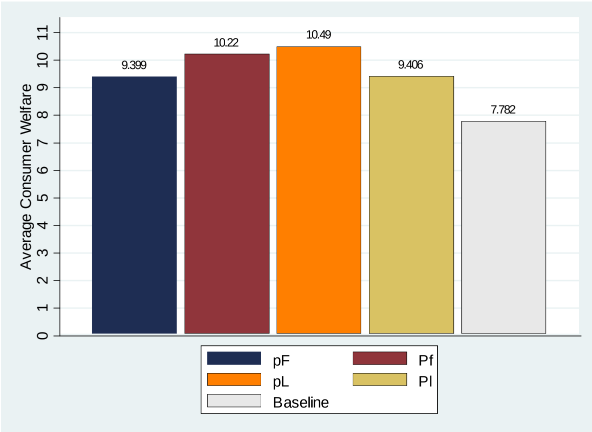
The observed patterns over time broadly support our initial intuition. In a further statistical analysis we find that prices in cartel groups may appear more dispersed than in competitive markets, but there is no statistically significant difference between either of them (Kruskal-Wallis test, $p > 0.1$). A pairwise comparison using Mann-Whitney tests in a similar manner as in our previous analysis confirms this. Summarising, we have seen no statistically significant evidence that suggest any validation of the claim that policy regimes will influence the severity of the commited crime. We can thus present our third result:

Result 3: Prices

With a constant expected fine, irrespectively of the the presence of a leniency program, asking and market prices remain the same.

To assess which policy regime is to be favored from a consumer's point of view, we now discuss the average *consumer welfare*, which is defined as the difference between the maximum willingness to pay of 102 and the actual market price. Averaging over all periods and groups, Figure 6 indicates that consumers are worst off without anti-trust enforcement. While this calls for enforcement actions, it is not immediately clear whether or not a leniency program is welfare improving (Kruskal-Wallis test, $p > 0.1$). In pairwise comparison of pF and Pf to pL and Pl, we find no significant difference. Furthermore, a comparison of pooled data for low and high detection rates also turns insignificant.

Figure 6: Average consumer welfare.



However, anti-trust authorities can choose both detection rate/fine level, and whether or not to employ a leniency program. Conditional on the existence of a leniency program, there is a mild statistical evidence that welfare is significantly higher if fines are high and detection rates are low (one-sided t-test, $p = 0.07$). Absent leniency, no such difference exists. We therefore conclude:

Result 4: Consumer Welfare

Consumer welfare is the highest for a higher fines and lower detection rates regime with leniency.

3.3 Cartel Stability

The final analysis will focus on successfully formed cartels in order to understand how they achieved prices above the competitive equilibrium. Specifically, we investigate *defection* and *self-reporting*, which can be understood as a proxy for the internal stability of a cartel. We measure *defection* by the percentage of firms within a cartel which select a price below 102 and hence deviate from the agreement. Table 9 provides the average defection rates for each treatment.

Table 9: Price Deviation - Average results per treatment.

Probability	Fine	without Leniency	with Leniency
10%	8	69.84 (27.70)	57.14 (18.35)
20%	4	74.54 (20.92)	53.78 (31.05)
Baseline:		66.97 (23.83)	

Notes: Standard deviations are in parenthesis. The rate of price defections (conditional on the existence of a cartel) is calculated using a dummy that takes the value 1 if a firm in a cartel chose a price below 102. We use the per-period average of a market as the unit of observation, and aggregate first over periods, then over all markets.

A first point to notice is that defection rates vary across treatments (Kruskal-Wallis test, $p = 0.09$). Firms undercut the agreed upon price more rigorously in the absence of leniency. In fact, the rate of price deviations is about 17% lower for the two leniency treatments, and this difference is statistically significant (Mann-Whitney test, $p = 0.02$). This finding is not surprising, as it has been often argued that firms can utilize the leniency program to punish deviators with reporting.[15] Of greater interest is the difference between *pF* and *Pf*, and between *pL* and *Pl*. Our analysis shows that the difference between the both rates is statistically not significant (Mann-Whitney test, $p > 0.1$). However, it is important to note that only about 9% of all markets in the pL treatment had a cartel. The number of observations that we can use for statistical tests is hence rather limited, so that we may lack the power necessary to find significant differences.

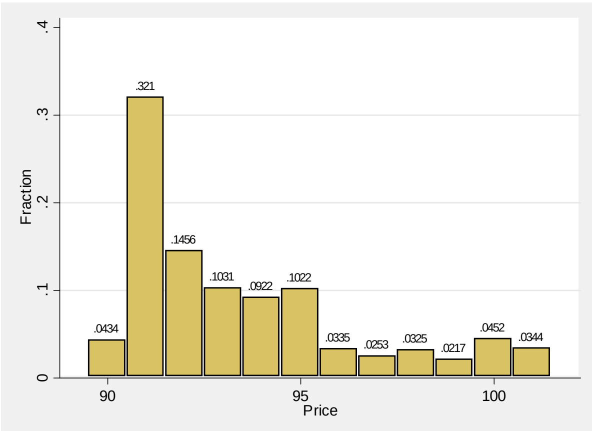
[15] Hinloopen and Soetevent (2008) report that the agreed-upon price is undercut in 97% of the cases with leniency, as compared to 75% without leniency.

Outcome 4:

Firms cheat less in the presence of leniency. However, this is statistically not significant.

How exactly do firms deviate? Theoretically, firms within a cartel should set the joint profit maximizing price of 102 and share their earnings, while a deviator should undercut and charge 101 to gain the entire market. Figure 7 depicts the distribution of chosen prices by deviating firms. It appears that the most common price is the competitve equilibrium price, but we also observe marginal cost pricing (which indicates punishment behaviour) as well as prices at the upper end of the price range.

Figure 7: Price distribution of deviating firms.



Now focus on the use of the leniency program by *self-reporting,* by which we mean that a firm reveals the existence of a cartel at the expense of one point, in order to avoid the possibility of anti-trust fines.Remember however that self-reporting does not guarantee immunity from fines. Similar to the design of leniency programs in the experimental literature so far, a reporting firm may still pay a (reduced) fine if more than one firm reports the cartel. Table 10 contains the average reporting rates for each treatment.[16] The rate of self-reporting using all observations is reported on the left side, while the right side of Table 10 provides the rate of self-reporting using only observations where a firm either deviated itself, or experienced a deviation from another cartel member.

[16] An alternative way to analyse the effect of leniency is to observe the fraction of established cartels that end due to reporting. In our *pL* treatment, 95.23% of the established cartels had at least one whistleblower, compared with 82.88% in *PL*. While on a first view these rates appear extremely high, they are not too different from the 78% reported in Hinloopen and Soetevent (2008).

Table 10: Reporting - Average results per treatment.

Probability	Fine	Reporting	...given own deviation	... given rival firm deviated
10%	8	54.34 (11.55)	34.12 (09.31)	53.17 (11.50)
20%	4	56.52 (30.10)	37.71 (26.66)	54.35 (32.26)

Notes: Standard deviations are in parenthesis. The rate of reporting is calculated using a dummy that takes the value 1 if a firm liable for a cartel self-reports. We use the per-period average of a market as the unit of observation, and aggregate first over periods, then over all markets.

We observe no statistically significant difference between the two treatments with leniency (Mann-Whitney test, $p > 0.1$). While this does not allow us to reject our null hypothesis in support of the alternative hypothesis H3C, we need to be aware that very few markets in pL are cartelized and that this limits the number of observations that we can draw conclusions from.

To observe if firms use the leniency program as part of a deviation strategy, we test for the percentage of cartel members that self-report after they have deviated from the collusive price. 34.12% and 37.71% of firms in pL and Pl use this strategy. Next, we are interested if firms self-report to punish deviators. Indeed 53.17% (54.35%) of firms in pL (Pl) report after deviations by others (conditional on sticking to the collusive agreement themselves). This pattern indicates that firms use the leniency program more often to punish deviators, than as part of their own deviation strategy.

However, as the difference between all observations is not significant we conclude:

Result 5: Stability

Deviation and Reporting rates do not differ significantly between different detection-fine combinations.

4 Discussion

This paper experimentally examines the *Beckerian Proposition*, according to which different combinations of the magnitude and the likelihood of punishment achieve the same deterrence effect. This key principle to the Law and Economics literature has been supported in existing laboratory experiments on speeding and stealing, but not in other experimental frames such as free-riding and tax evasion. The ambiguous evidence makes it difficult to draw conclusions for the design of optimal law enforcement mechanisms, which is of particular importance for the work of anti-trust authorities, who face a trade-off between economizing on costly enforcement actions and the potential adverse effects of a higher fine rate. Criminal activities in a market frame differ from all previously studied situations, as the violation of anti-trust laws is a coordinated rather than an individual action. Further, enforcement agents can utilize other policy tools such as leniency to disincentivize and punish wrongdoers. To date it is therefore unclear how firms will react if authorities vary either the likelihood of detection or the level of fines, but keep the expected fines constant. This experiment closes this gap by experimentally varying the probability of detection and the amount of anti-trust fines in a repeated Bertrand game with inelastic demand and exogenous anti-trust enforcement.

Based on the data retrieved from the experiment, our interpretation is that in general fines and detection rates can indeed be treated as substitutes. It is reassuring that, as predicted by theory, different combinations of the magnitude and likelihood of punishment seem to be interchangeable instruments to deter cartels. However, in addition to demonstrating that the Beckerian proposition can hold in a market frame, we also have clear indication that the deterrence effect of punishment is not maintained if a leniency policy exists. In the presence of leniency, a lower detection probability with higher fines significantly reduces the rate of firms which attempt to form a cartel. More importantly, a high fine and low detection policy under leniency decreases the overall incidences of cartels, which is the ultimate aim of a deterrence mechanism. We further experimentally validate the claim that 'tougher punishment leads to more crime', and find no support for this hypothesis in terms of asking and market prices. Finally, we observe that no fine-detection regime is superior in terms of its destabilization of cartels.

From a policy point of view, the experimental study has an important implication. The results indicate that society can not just economize on costs of enforcement, as postulated by Becker (1968), but actually achieve greater deterrence at lower costs. Consequently, the results give empirical support for the policy move towards higher fines as orchestrated recently by the OFT.

Two immediate questions arise: First, why does the Beckerian proposition hold absent leniency, but not when a leniency policy exists? And second: if detection rate and fine are not substitutable, why do we observe stronger deterrence in pL than in Pl?

A possible answer to these questions is that firms may assume a different likelihood of detection when a leniency program exists. While absent leniency the perceived detection probability is the exogenous given probability – and hence no statistically significant difference between pF and Pf exists - the perceived detection probability ($\tilde{p}$ or $\tilde{P}$) with leniency is a combination of the exogenous detection rate and the belief that other firms may self-report. The likelihood that another firm self-reports may well depend on the fine levels, as higher (lower) fines provide more (less) incentives to self-report, all else equal. If for high fines the perceived likelihood of detection is greater than the exogenous likelihood, this implies that firms perceive the expected fine as greater than the combination of exogenous detection and fine level. In other words, expected fines $\tilde{p}L > pL = Pl$. Contrary, for low fines firms are less likely to self-report, which reduced the perceived detection probability: $\tilde{P}l < Pl$. This in turn implies that $\tilde{p}L > \tilde{P}l$, which may explain our results.

An interesting direction for future research is whether firms anticipate higher fines, and react by spending socially wasteful resources on avoidance activities as proposed by Malik (1990). This idea has recently been investigated by Bayer and Sutter (2009) in the frame of tax evasion, but has not yet been tested for a market frame. It would also be interesting to test the role of uncertainty over both fine and detection level, and to investigate the effect of additional private class actions. These issues could be investigated in a similar experimental study, which we leave for future research.

References

1. Anderson, Lisa R. and Sarah L. Stafford. "Punishment in a Regulatory Setting: Experimental Evidence from the VCM", Journal of Regulatory Economics, 24(1), 91-110, (2003)

2. Andreoni, James. "Reasonable Doubt and the Optimal Magnitude of Fines: Should the Penalty Fit the Crime?", RAND Journal of Economics, 22, 385-395, (1991)

3. Baker, Tom, Harel, Alon and Tamar Kugler."Virtues of Uncertainty in Law: An Experimental Approach", Iowa Law Review, 89, 443-494, (2003)

4. Bayer, Ralph C. and Matthias Sutter. "The Excess Burden of Tax Evasion - An Experimental Detection-Concealment Contest", European Economic Review, 53, 527-543, (2009)

5. Bar-Ilan, Avner and Bruce Sacerdote. "Response of Criminals and Noncriminals to Fines", Journal of Law and Economics, 47(1), 1-17, (2004)

6. Becker, Gary. "Crime and Punishment: An Economic Approach", Journal of Political Economy, 76, 169-217, (1968)

7. Bigoni, Maria, Fridolfsson, Sven-Olof, Le Coq, Chloe and Giancarlo Spagnolo. "Fines, Leniency and Rewards in Antitrust", Rand Journal of Economics, 43(2), 1756-2171, (2012a)

8. Bigoni, Maria, Fridolfsson, Sven-Olof, Le Coq, Chloe and Giancarlo Spagnolo. "Trust and Deterrence", CEPR Discussion Paper No. DP9002, (2012b)

9. Block, Michael K. and Joseph G. Sidak. "The cost of antitrust deterrence: Why not hang a price fixer now and then?", Georgetown Law Journal, 68(5), 1131-1139, (1980)

10. Block, Michael K., Nold, Frederick C. and Joseph G. Sidak. "The Deterrent Effect of Antitrust Enforcement", Journal of Political Economy, 89(3), 429-445, (1981)

11. Block, Michael K and Vernon E. Gerety. "Some Experimental Evidence on Differences between Student and Prisoner Reactions to Monetary Penalties and Risk", Journal of Legal Studies, 24(1), 123-138, (1995)

12. Bryant, Peter G. and Woodrow Eckard. "Price Fixing: The Probability of Getting Caught", Review of Economics and Statistics, 73(3), 531-536, (1991)

13. Dal Bo, Pedro. "Cooperation under the shadow of the future: experimental evidence from infinitely repeated games", American Economic Review, 95,1591-1604, (2005)

14. DeAngelo, Gregory and Gary Charness. "Deterrence, expected cost, uncertainty and voting: Experimental evidence", Journal of Risk and Uncertainty, 44(1), 73-100, (2012)

15. Dhami, Sanjit and Ali al-Nowaihi. “An extension of the Becker proposition to non-expected utility theory”, Mathematical Social Sciences, 65, 10-20, (2013)

16. Dufwenberg, Martin and Uri Gneezy. “Price competition and market concentration: an experimental study”, International Journal of Industrial Organization, 18(1), 7-22, (2000)

17. Ehrlich, Isaac. “Participation in illegitimate activities: a theoretical and empirical investigation”, Journal of Political Economy, 81(3), 521-565, (1973)

18. Eide, Erling. “Economics of Criminal Behavior”, In: Bouckaert, Boudjwijn and Gerrit De Geest: Encyclopedia of Law and Economics, Vol. 5, Cheltenham, 345-389, (2000)

19. Friedland, Nehemiah, Maital, Shlomo and Aryeh Rutenberg. “A simulation study of income tax evasion”, Journal of Public Economics, 10(1), 107-116, (1978)

20. Fischbacher, Urs. “z-Tree: Zurich Toolbox for Ready-made Economic Experiments”, Experimental Economics, 10(2), 171-178, (2007)

21. Garoupa, Nuno. “The Theory of Optimal Law Enforcement”, Journal of Economic Surveys, 11, 267-295, (1997)

22. Garoupa, Nuno. “Behavioral Economic Analysis of Crime: A Critical Review”, European Journal of Law and Economics, 15(1), 5-15, (2003)

23. Gillet, Joris, Schram, Arthur and Joep Sonnemans. “Cartel formation and pricing: The effect of managerial decision-making rules”, International Journal of Industrial Organization 29, 126-133, (2011)

24. Greiner, Ben. “The Online Recruitment System ORSEE – A Guide for the Organization of Experiments in Economics,” In: Kremer, Kurt and Volker Macho: Forschung und wissenschaftliches Rechnen, Goettingen,79-73, (2004)

25. Hinloopen, Jeroen and Adriaan R. Soetevent. “Laboratory evidence on the effectiveness of corporate leniency programs”, Rand Journal of Economics, 39(2), 607-616, (2008)

26. Hoerisch, Hannah and Christina Strassmair. “An experimental test of the deterrence hypothesis”, Journal of Law, Economics, & Organization, 28(3), 447-459, (2012)

27. Holt, Charles A., and Susan K. Laury. “Risk Aversion and Incentive Effects”, American Economic Review, 92(5), 1644-1655, (2002)

28. Jensen, Sissel, Kvaloy, Ola, Olsen, Trond E. and Lars Soergard. “When tougher punishment leads to a more severe crime: an application to antitrust enforcment”, NHH Dept. of Economics Discussion Paper No. 04, (2013)

29. Jolls, Christine, Sunstein, Cass R., and Richard Thaler. “A behavioral approach to law and economics”, Stanford Law Review, 50, 1471-1550, (1998)

30. Landes, William M. “Optimal Sanctions for Antitrust Violations”, University of Chicago Law Review, 50(2), 652-678, (1983)

31. List, John A. “On the interpretation of giving in dictator games”, Journal of Political Economy, 115(3), 482-493, (2007)

32. Malik, Arun S. “Avoidance, screening and optimum enforcement”, Rand Journal of Economics, 341-353, (1990)

33. Motta, Massimo and Polo, Michelle. “Leniency programs and cartel prosecution”, International Journal of Industrial Organization, 21(3), 347-379, (2003)

34. Normann, Hans-Theo and Roberto Ricciuti. “Laboratory Experiments for Economic Policy Making”, Journal of Economic Surveys, 23(3), 407-432, (2009)

35. Office of Fair Trade. “OFT’s guidance as to the appropriate amount of a penalty”, OFT423 Press Release, 1-26, (2012)

36. Office of Fair Trade. “Office of Fair Trading Annual Plan 2013–14”, OFT1462 Press Release, 1-20, (2013)

37. Polinsky, A. Mitchell and Steven Shavell. “The optimal tradeoff between the probability and magnitude of fines”, American Economic Review, 69(5), 880-891, (1979)

38. Polinsky, A. Mitchell and Steven Shavell. “The Economic Theory of Public Enforcement of Law”, Journal of Economic Literature, 38(1), 45-76, (2000)

39. Stigler, George. J. “The optimum enforcement of laws”, Journal of Political Economy, 78, 343-355, (1970)

Appendix A

Table 11: Decision to collude - Generalized Linear Latent and Mixed Model

	without Leniency (Base: pF)			**with Leniency** (Base: pL)		
	Model 1	Model 2	Model 3	Model 1	Model 2	Model 3
Decision to collude						
Pf	0.0601	0.233	0.355*			
	(0.230)	(0.185)	(0.194)			
Pl				0.381**	0.376**	0.314**
				(0.170)	(0.169)	(0.146)
Decision to collude$_{t-1}$	2.928***	2.742***	2.540***	3.571***	3.563***	3.179***
	(0.139)	(0.142)	(0.132)	(0.163)	(0.163)	(0.142)
Period	-0.00752	-0.0140	0.0214	-0.0166	-0.0182	0.0129
	(0.0391)	(0.0395)	(0.0409)	(0.0446)	(0.0446)	(0.0449)
Period^2	-0.000612	-0.000467	-0.000953	-0.000307	-0.000249	-0.000566
	(0.00140)	(0.00142)	(0.00142)	(0.00165)	(0.00165)	(0.00161)
Cartel formed$_{t-1}$	-0.771***	-0.703***		0.860	0.859	
	(0.230)	(0.231)		(0.593)	(0.595)	
Cartel detected$_{t-1}$	-0.620**	-0.664**		-0.395	-0.425	
	(0.298)	(0.304)		(0.487)	(0.487)	
Cartel reported$_{t-1}$				-1.650**	-1.674**	
				(0.681)	(0.686)	
Price deviation$_{t-1}$	-0.0255	-0.0244		-0.908	-0.891	
	(0.170)	(0.170)		(0.583)	(0.586)	
Risk choice		0.163***	0.176***		0.0321	0.0248
		(0.0256)	(0.0263)		(0.0291)	(0.0273)
Inconsistent preferences		-0.215	-0.267		-0.209	-0.0941
		(0.229)	(0.230)		(0.205)	(0.185)
# of times busted			-0.308**			-0.409***
			(0.125)			(0.151)
# of times colluded			-0.00333			0.320**
			(0.0204)			(0.129)
Constant	-1.234***	-1.954***	-2.228***	-1.313***	-1.386***	-1.610***
	(0.294)	(0.304)	(0.300)	(0.289)	(0.309)	(0.299)
Observations	1710	1710	1710	1524	1524	1524

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix B - Instructions

We ran the following sessions:

Table 12: Session table.

Treatment	Date	Subjects	Rounds
Baseline	05/12/2012	18	25
	06/12/2012	18	24
Low Detection Rate, High Fine (pF)	05/12/2012	18	22
	05/12/2012	18	24
High Detection Rate, Low Fine (Pf)	05/12/2012	18	27
	05/12/2012	18	26
Low Detection Rate, High Fine with Leniency (pL)	14/11/2012	18*	27
	14/11/2012	18	20
High Detection Rate, Low Fine with Leniency (Pl)	15/11/2012	18	21
	15/11/2012	18	25

*: 2 subjects went bankrupt. The entire group observation was hence dropped.

An example of the experimental instructions that we used:

Welcome and thank you for taking part in this experiment. In this experiment you can earn money. How much money you will earn depends on your decision and on the decision made by other participants in this room.

The experiment will proceed in two parts. The currency used in Part 1 of the experiment is Pound Sterling (GBP). The currency used in Part 2 is experimental points. Each experimental point is worth 15 pence. All earnings will be paid to you in cash at the end of the experiment.

Every participant receives exactly the same instructions. All decisions will be anonymous. It is very important that you remain silent. If you have any questions, or need assistance of any kind, please raise your hand and an experimenter will come to you.

Instructions for Part 1

In the first part of the experiment you will be asked to make 15 decisions. For each line in the table in the next page there is a paired choice between two options ("Option A" and "Option B"). Only one of these 15 lines will be used in the end to determine your earnings. You will only know which one at the end of the experiment.

Each line is equally likely to be chosen, so you should pay equal attention to the choice you make in every line. At the end of the experiment a computerized random number (between 1 and 15) determines which line is going to be paid.

Your earnings for the paid line depend on which option you chose: If you chose option A in that line, you will receive £1. If you chose option B in that line, you will receive either £2 or £0. To determine your earnings in the case you chose option B there will be second computerized random number (between 1 and 20).

Instructions for Part 2

In this part of the experiment you will form a group with two other randomly chosen participants in this room. Throughout the experiment you are matched with the same two participants. All groups of three participants act independently of each other.
This part of the experiment will be repeated at least 20 times. From the 20th round onwards, in each round there is a one in five (20%) chance that the experiment will end.

Instruction:

You are in the role of a firm that is in a market with two other firms.
In each round, you will have to choose a price for your product. This price must be one of the following prices:
9*0, 91, 92, 93, 94, 95, 96, 97, 98, 99, 100, 101, 102.*
You will only sell the product if your price is the lowest of the three prices chosen by you and the other two firms in that round. If you sell the product, your earnings are equal to the difference between the price and the cost, which is 90:
Earnings = Price – 90.

If you do not sell the product, you will not get any earnings but you do not have costs either. If two or more firms sell at the same lowest price, the earnings will be shared equally between them. After your price choice, you will be told whether you have selected the lowest price as well as the price of the other firms. Before you choose your price, you can decide to agree with the other firms to set the highest price of 102 and share the earnings. This agreement is only valid if all three firms want to agree on it. However, the price agreement is not binding and firms are not required to set the agreed price.

The price agreement may be discovered by the computer. In that case, a fine of 8 points has to be paid. The computer can detect it in one out of 10 cases (a chance of 10%).
A price agreement remains valid – and can be discovered– as long as it has not been discovered in a previous round. Once this has happened, you will not be fined in the future, unless you make a price agreement again.

At the end of each round, you will be told

- the earnings you made in this round
- in case you agreed on a price if this agreement has been detected.

Final Payment:

At the beginning of the experiment you start with an initial endowment of 40 points = 6 GBP. The earnings you earned in each round minus any fine that you paid will be converted into cash. Each point is worth 15 pence, and we will round up the final payment to the next 10 pence. We guarantee a minimum earning of 2 GBP.