FOUR ESSAYS ON OPTIMAL ANTITRUST ENFORCEMENT

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Abstract

This thesis consists of four essays related to optimal antitrust enforcement. The first essay provides a case study of EC ringleader cartels and discusses by means of a theoretical model the effect of excluding ringleaders from leniency programmes on collusive prices. The second essay adds an experimental investigation of the former, and examines in particular the effects on cartel formation, prices and stability. The third essay experimentally explores the substitutability of antitrust detection rates and fines, and test whether different fine and detection rate combinations with constant expected fines achieve an equal level of deterrence. Lastly, the final essay discusses the role of antitrust enforcement on collusion when firms can engage in avoidance activities.

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Chapter 1

Introduction

The dissertation consists of four essays broadly related to cartels and antitrust enforcement. It is meant as a contribution to selected on-going debates among practitioners and academics about the optimal design of antitrust policy. While each essay can be read independently, the thesis consists of two sets of two related essays.

The first two essays (Chapter 2 and 3) address the design of an important instrument that antitrust authorities around the world have employed to fight cartels: Corporate leniency programmes provide cartel members the opportunity to receive a substantial reduction of (or even immunity from) their antitrust fines in exchange for providing evidence that helps to detect and convict a collusive agreement. While leniency programmes in different jurisdictions share many similarities, there exists a notable difference in the treatment of cartel ringleaders. Under the current U.S leniency policy, and different to the E.U. policy, centralised "cartel managers" are not eligible for amnesty or reduction in fines. Despite their prominence in case law, surprisingly little is known about cartel ringleaders in practice, and it yet remains a topic of debate as to which of the two alternating policies are to be preferred.

The main aim of Chapter 2: CARTEL RINGLEADERS AND THE CORPORATE LENIENCY PROGRAMME is to contribute to this debate by first exploring ringleaders descriptively and then by investigating the impact of ringleader exclusion from leniency on cartel prices by means of a theoretical model. The first part of the chapter, reports a survey of 75 European Commission cartel decisions and investigates common features and regularities of ringleader cartels. Several interesting results are derived. We find that approximately one in five cartels have an identified ringleader, which often is the largest firm in terms of market shares. Surprisingly, there often exists more than one ringleader with an average of about two ringleaders per cartel. The ringleaders are found to perform a striking variety of tasks, and are on average punished with a fine increase of about 42% due to their leading role. The second part of Chapter 2 is of a theoretical nature and has recently been published in a refereed journal.[1] We present a model of a price setting super game with capacity constraints, which we use to implement size asymmetry. In a fairly general setting, we analyse the effect of excluding a ringleader from leniency on the sustainable cartel price. The model derives the result that ringleader exclusion may allow a cartel to sustain higher prices, in particular when antitrust fines depend on individual cartel gains in a nonlinear fashion and the size distribution of members is sufficiently heterogeneous.

In contrast to the descriptive and theoretical methodology used in the previous chapter, Chapter 3: An experimental study of ringleader exclusion from leniency programmes applies experimental methodology to analyse how excluding the ringleader from leniency affects formation, prices and stability of cartels. Using laboratory experiments has the advantage that we obtain full control over all aspects of a firms' decision making process, and all observed differences between the outcomes of an (non-)exclusionary leniency policy is linked to the variation of specific features of the treatments, and not due to unobservable differences between the U.S. and EU cartels. Using a three-firm discrete Bertrand pricing game with the possibility of collusion, we implement two treatments which resemble the U.S. and E.U. leniency policy. Our findings indicate that ringleader exclusion does not lead to greater deterrence, as a firms' willingness to engage in collusive misconducts does not decrease. On the contrary, cartel formation may even be facilitated when the leader is not able to obtain leniency. While we further observe a significant increase in the number of price deviations, indicating a destabilising effect of ringleader exclusion, the reporting rate of regular cartel members drop, potentially jeopardising the overall effectiveness of the leniency programme.

In Chapter 4: Antitrust and the Beckerian Proposition: the Effects of Investigation and Fines on Cartels we take a closer look at the effects of

[1] Bos, I. and Wandschneider, F. (2013): A Note on Cartel Ringleaders and the Corporate Leniency Program, Applied Economic Letters, 20(11), 1100-1103.

different fine and likelihood of detection combinations with constant expected fines, in particular with respect to cartel formation, prices and collusive stability. The background to this chapter is a prominent claim in the law and economics literature by Becker (1968), according to which fine and detection likelihood are substitutes in their deterrence effect. This claim has influenced IO theory as well as the work of anti-trust authorities, leading to a recent increase in antitrust fines by the Office of Fair Trade. The rationale behind recent moves towards higher antitrust fines is, that a rational firm weighs the expected benefits of breaking antitrust law against the conjecture of detection rate and fine level. Hence, as screening markets for collusion is expensive, it is optimal to reduce the probability of detection and increase the magnitude of the fines levied upon wrongdoers. We design a cartel experiment, in which we vary the probability of detection and the level of the antitrust fine in a controlled manner, such that the expected fine remains the same. We find that, in the absence of a leniency programme, detection rates and fines are indeed substitutable. In the presence of a leniency programme, however, a regime that embodies low rates of detection and high fines reduces the propensity to collude and lowers the overall incidence of cartelised markets significantly more than a high detection and low fine regime. This indicates that antitrust agencies can economise on enforcement costs and achieve a higher degree of deterrence by reducing the probability of detection and increasing the severity of the fines.

Chapter 5: An Experimental Analysis of Antitrust Enforcement under Avoidance builds on the preceding chapter but directs attention to a prominent limitation of high cartel fines: As firms may react to higher fines by spending resources on avoidance, the overall welfare effects of reducing detection rates and increasing fines are not clear. To date, it has not been tested how allowing for avoidance activities influences insights from cartel experiments.We present a market experiment in which we allow firms to reduce their future potential antitrust fine. We observe that avoidance has a significant impact on cartel formation, as firms are more willing to collude when they are able to insure themselves against their future fines through avoidance. Further, as suggested in Jensen et al. (2012), firms that engage in avoidance activities achieve higher collusive prices. Finally, our results suggest that some firms utilise avoidance procedures as an alternative means to leniency in an attempt to avoid being punished for price deviations.

Lastly, a summary of our results appears in Chapter 6.

Chapter 2

Cartel Ringleaders and the Corporate Leniency Programme

> "The early bird catches the worm, but the second mouse gets the cheese"
> Unknown Author

2.1 Introduction

[1]Corporate leniency programmes offer cartel members the opportunity to report their illegal conduct in exchange for full immunity or a reduction of antitrust penalties. Although the various programmes across jurisdictions share many similarities, the treatment of undertakings that had a central role in establishing or organising the cartel differs markedly. Most notably, such "cartel ringleaders" are sometimes eligible for amnesty (e.g., in Europe), whereas in other jurisdictions they are excluded from the leniency programmes (e.g., in the U.S.). The purpose of this paper is to gain an understanding of cartel ringleaders and how ringleader exclusion may affect collusion.

Before proceeding, it is important to clarify what we mean by "cartel ringleader". Cartel ringleader is a term used for the centralised decision maker or "cartel manager" (Hovenkamp and Leslie, 2011) of a collusive network. While the concept of cartel leadership has so far widely been neglected in the IO literature[2], antitrust court rulings repeatedly declared the culpability and special responsibility of certain cartel members. In their antitrust fining guidelines, the European Commission

[1]This chapter is in parts based on joint work with Iwan Bos. See Bos and Wandschneider (2011, 2013).

[2]Related is the literature on collusive price leadership. For a recent contribution in this field see Mouraviev and Rey (2011).

names instigation and leadership as an aggravating role that justifies an increase in antitrust penalties. The former concerns the establishment or enlargement of a cartel, whereas the latter applies to its operation.[3] More specifically, an instigator is an undertaking that has persuaded or encouraged other firms to establish or join a cartel by taking the initiative to suggest collusion. A firm is classified as a leader if it was a significant driving force for the cartel. This may include giving a major boost to the performance of the cartel by being the first firm to implement the arrangement, taking voluntary initiatives to propel the cartel and, more generally, by taking responsibility for developing the illegal agreement. Likewise, the United States Sentencing Guidelines read that the seriousness of an offence is increased if a defendant was the "organizer", "leader", "manager" or "supervisor" of a criminal activity, and further clarifies that the court should consider "the exercise of decision making authority, the nature of participation in the commission of the offense, the recruitment of accomplices, the claimed right to a larger share of the fruits of the crime, the degree of participation in planning or organizing the offense, the nature and scope of the illegal activity, and the degree of control and authority exercised over others."[4]

Let us now have a closer look at the leniency policy regarding ringleaders in both the U.S. and Europe to clarify the term "ringleader exclusion". To begin, the 1993 U.S. guidelines on corporate leniency reads that a firm is only eligible for amnesty when it "did not coerce another party to participate in the illegal activity and clearly was not the leader in, or originator of, the activity".[5] The European leniency programme became effective in 1996 and initially followed the U.S. approach rather closely. Only a firm that "has not compelled another enterprise to take part in the cartel and has not acted as an instigator or played a determining role in the illegal activity" can obtain non-imposition or a (very) substantial reduction of fines.[6] This rule significantly restricted ringleaders by only allowing them to apply for a

[3] See side numbers 14-18 in Case T-15/02 BASF AG v. Commission, Summary of the Judgement, March 15, 2006.

[4] See Application Note to §2R.1.1, United States Sentencing Commission (2012), Guidelines Manual, as well as the relevant paragraph 3B1.1.

The U.S. Aggravating and Mitigating Role Adjustments Primer (2013) clarifies that the line between being a organiser/leader as opposed to a being a manager/supervisor is not clearly drawn. As a rule of thumb, organisers and leaders are engaged in the planning, developing, directing of the crime and thus are deemed more culpable than managers or supervisors.

[5] United States Department of Justice (1993), Corporate Leniency Policy, para A6. See also B7.

[6] European Commission (1996), Commission Notice on the non-imposition or reduction of fines in cartel cases, para B(e).

limited fine reduction in the range of 10 to 50%. However, and in contrast to the original 1996 leniency notice, the 2002 and 2006 revisions allow ringleaders to apply for full immunity provided that some standard requirements are met.[7]

In the ensuing analysis, we use the term "ringleader" broadly in that we do not make an explicit distinction between instigation and leadership. As to ringleader exclusion, we distinguish between a "non-discriminatory leniency programme" and a "discriminatory leniency programme". The former is used to describe the case where ringleaders have the same rights as regular cartel members, whereas the latter refers to a situation where ringleaders are not eligible for (full) immunity.

Understanding the impact of ringleader exclusion on collusion is not trivial. On the one hand, knowing *ex ante* which of the cartel members is not eligible for amnesty is likely to affect the level of trust among colluders. In particular, the fact that a ringleader has not much to gain from self-reporting makes it a trustworthy "partner in crime". Indeed, in a review of the U.S. corporate leniency programme, Leslie (2006) argues in favour of a non-discriminatory leniency programme as this would foster distrust among cartel participants. Moreover, ringleader exclusion increases the chance for regular cartel members to be the first to self-report, which in turn lowers the risk of a "race to the courtroom", all else equal. On the other hand, ringleader exclusion increases the expected antitrust penalty for ringleaders, thereby making collusion less attractive.[8] Firms are likely to have a stronger incentive to wait for others to take the lead, which makes the formation of cartels *ceteris paribus* less likely. Finally, ringleader exclusion introduces legal uncertainty as it may not always be clear when a firm is regarded a leader. This unclarity potentially leads to fewer confessions. It is therefore *a priori* unclear whether ringleader exclusion enhances deterrence or instead facilitates collusion.

In this paper, we seek to shed some light on how ringleader exclusion affects collusion. As not much is known about cartel ringleaders, we first provide a survey of 75 fining decisions taken by the European Commission between 2000 and 2010. In 14 cases, leadership is identified and explicitly mentioned as an aggravating circumstance. A detailed analysis of these cases reveals, among other things, that (i) cartels

[7]European Commission (2002), Commission notice on immunity from fines and reduction of fines in cartel cases, para A11(c) and Section B. For the rationale behind this see the relevant part in European Commission (2002), Question & Answer on the Leniency Policy, Memo./02/23.

[8]As mentioned earlier, apart from being (partially) excluded, ringleaders additionally face an increase of the basic fine. See European Commission (2006), Guidelines on the method of setting fines imposed pursuant to Article 23(2)(a) of Regulation No 1/2003, para A28 and United States Sentencing Commission (2010), Guidelines Manual, para 3B1.1.

often had more than one ringleader, (*ii*) the role of ringleaders was very diverse and (*iii*) ringleaders were typically the largest cartel members. Next, we theoretically analyse the impact of ringleader exclusion on the collusive price level. Specifically, we investigate a price setting supergame in which firms differ in terms of capacity stocks, which is taken as a proxy for firm size. Under the assumption that cartel profits as well as the (expected) antitrust penalty depend positively on firm size, we find that a discriminatory leniency programme leads to higher prices when (*iv*) the joint profit maximum cannot be sustained under a non-discriminatory leniency policy, (*v*) antitrust fines depend on individual cartel gains in a nonlinear fashion and (*vi*) the size distribution of members is sufficiently heterogeneous. We also consider the possibility of alternative profit allocation rules and establish that side-payments are *ceteris paribus* most likely when the intended ringleader is the smallest firm. Our overall findings therefore support the imposition of antitrust penalties proportional to firm size when ringleaders are excluded from the corporate leniency programme.

While empirical studies on cartels have identified that successful cartels create mechanisms to share information, organise decision making and align incentives (Levenstein and Suslow, 2006), traditionally IO economists have abstracted away from the internal organisation of collusive agreements.[9] Yet, attempts to unravel the "black box" have been made by sociologists, management scientists and lawyers. Starting with the premises that there exists a trade-off between the need to conceal the collusive agreement and the need to organise its tasks efficiently, Baker and Faulkner (1993) construct a network of all cartel participants of the Switchgear, Transformer and Steam Turbine Generators cartel using archival data on their social ties in order to show how the social structure influences the cartel conduct. They find that the need to process vast amounts of information and to make complex decisions lead to centralised networks in which the "core" participants are densely interconnected while few links between the regular cartel members exist. They further maintain that on an individual level, managers like to be members of the core to monitor and prevent competitors from treating their company unfairly. However, managers also want to avoid additional risk of being detected and sanctioned that comes with such a central position.

Another attempt to understand the organisation of collusion has been made by Pressey et al. (2010) who examine the formal managerial structure of four EC

[9] Scherer (1980) even maintains that conventional economic analysis does not allow to address the "relationship between an industry's informal and formal social structure and its ability to coordinate pricing" (p. 225).

cartel cases. They conclude that two modes of organising price-fixing cartels exist (a centralised "bilateral" network and a decentralised "multilateral" form), which both have different defining features. In particular, decentralised networks are described as being more integrated and comparable to Joint Ventures. Members tend to be more symmetric and have multiple communication links between them, which arguably makes them more visible to enforcement agencies. In contrast, "bilateral" networks are said to share properties of terrorist organisations, which are more difficult to detect and show greater flexibility to react to outside shocks.

Arguing from a lawyers perspective, Hovenkamp and Leslie (2011) discuss various decision-making schemata to explain why firms are willing to delegate decisions away to a centralised authority. They postulate that a "cartel manager" can help to reduce agency costs and align divergent preferences of cartel members.

As little is known about cartel ringleaders, there exist even less literature on the impact of ringleader exclusion on collusion.[10] To our knowledge, the only two other papers that explicitly and extensively analyses cartel ringleaders in relation to the corporate leniency programme are Herre and Rasch (2009) and Bigoni et al. (2012a). Herre and Rasch's study theoretically explores the deterrent effect of ringleader exclusion by considering variations in the probability of conviction. If there is a relatively small chance of being caught, then a non-discriminatory leniency policy is preferred as the additional information that a ringleader may provide can be essential for cartel prosecution. By contrast, if the probability of conviction is relatively high, then it is optimal to exclude ringleaders. The reason is that ringleader exclusion creates an asymmetry among firms, which makes sustainability of collusion ceteris paribus more difficult. The recent experimental study by Bigoni et al. finds that ringleader exclusion leads to higher prices, but hardly affects the formation of cartels.

This paper proceeds as follows. The next section provides a description and discussion of cartel ringleaders in antitrust practice. The model is introduced in Section 2.3. Section 2.4 concludes.

[10] Contributions in the area of leniency include, among others, Motta and Polo (1999, 2003), Spagnolo (2000), Aubert et al. (2006), Motchenkova and Leliefeld (2007) and Harrington (2008). For empirical analyses compare Brenner (2009) for the European leniency programme and Miller (2009) for the US leniency policy.

2.2 Case Study

Before analysing the impact of ringleader exclusion on collusion, we believe it is instructive to first examine cartel ringleaders in practice. We want to explore the frequency and characteristics, as well as the function and legal treatment of the cartel ringleader. Towards that end, we have conducted a survey of 75 European Commission cartel decisions taken over the last decade. Specifically, we have surveyed all prohibition decisions and press releases concerning the policy area "Cartels" between January 1, 2000 and January 1, 2011.[11] For each case, we first assessed whether the European Commission explicitly mentions the leading role of one or more cartel members and adjusted the fine accordingly.

Before we proceed, a word of caution is in order. We are aware that the findings presented here may be biased in several respects. First, and inherent to all empirical cartel studies, our sample of cartel cases may not be a good representation of the unknown pool of cartels. Second, our sample might be a fraction of the actual number of ringleader cases. As leadership typically results in a substantial increase of the antitrust fine (for our sample, the average fine increase is about 42%), the Commission is likely to explicitly refer to a leading role only when it has sufficient legal evidence available to win an appeal in court. Our sample is therefore likely to mark a lower bound as there may have been other cases in which the Commission, despite having some evidence of leadership, did not increase the basic fine. Finally, the vast majority of ringleader cartels operated in manufacturing industries producing more or less homogeneous goods; one of which included agreements between buyers and sellers (*Bitumen Nederland*). It can be argued that such a buyer - seller cartel has different structural features compared to horizontal cartel agreements.

This being said, we are confident that a discussion of some of the traits of known cartel ringleaders is informative and useful for current and future research.

[11] These dates were chosen rather arbitrarily. We do not expect results to be radically different for alternative time frames. For the selected period, we have studied all cases that were available at the Commission's online database at the end of March 2011 (See http://ec.europa.eu/competition/cartels/cases/cases.html).

2.2.1 Frequency of ringleader cartels

A ringleader was identified by the European Commission in 14 out of the total of 75 cases. This proportion of ringleader to no-ringleader cases is similiar to the findings in Ganslandt et al. (2010) who report identified ringleaders in 10 out of 43 observed cartel decisions. In more detail, the *Aggravating circumstances* paragraph of the prohibition decision refered to those ringleaders as a "leader" in 7 cases, as an "instigator" in 2 cases, and used both terms simultaneously in 5 cases. Figure 2.1 depicts the proportion of cartels with an identified ringleader over time. As can be seen, the majority of indentified ringleader cases appeared before or around the 2002 leniency revision.

Figure 2.1: Number of cartels and ringleader cases over time.

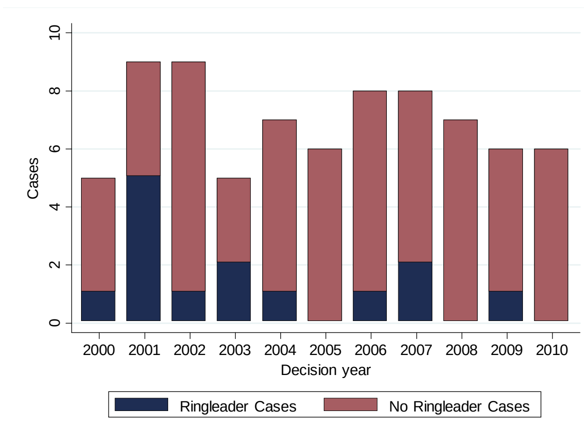


Table 2.1 provides an overview of the identified ringleader cases by stating the case number, case title and the name of the identified ringleaders. It further classifies the role of the leaders into either of the two categories, instigation or leadership, depending on the ringleaders' role defined in the prohibition summary by the European Commission. As can be seen in the second column of Table 2.1, a common property of the cartels in our sample is the existence of more than one ringleader. Specifically, in 10 out of 14 cases two or more undertakings shared the responsibility for establishing or leading the cartel.[12] Some cartels had multiple ringleaders operating

[12] For a discussion of the difficulties of determining a leading position in the context of multiple leaders see Case COMP/36.545/F3 Amino acids, paras 418 and 419. Interestingly, in the U.S. it holds that "in situations where the corporate conspirators are viewed as co-equals or where there are two or more corporations that are viewed as leaders or originators, any of the corporate participants will qualify" for amnesty under the U.S. guidelines. See U.S. Department of Justice (1998), The Corporate Leniency Policy: Answers to Recurring Questions.

simultaneously, whereas in other cases members took turns. Four cartels were led by a single undertaking and the majority had two ringleaders (the average number of leaders is 1.79). A special case is the cartel which had three leading participants. In *Gas Insulated Switchgear*, Siemens was the sole leader during most of the cartel's life. ALSTOM replaced Siemens during its temporary departure from the cartel and this firm was taken over at some point by AREVA, which continued the leading role.

Presumably, implementing or monitoring a collusive agreement among a large number of competitors is inherently difficult, as the complexibility to coordinate increases exponentially with number of firms (Scherer, 1980). As cartels might require a leader for the sole purpose of coordinating their activities, one might conjecture the presence of ringleaders in cartels with many participants. However, our sample suggests that the number of ringleaders does not increase with the number of cartel participants. For instance, in *Carbonless paper* there were eleven members and one ringleader, whereas in *Interbrew and Alken-Maes* there were four members and two leaders. The average number of firms involved in a cartel with one, two or three identified ringleaders is respectively 7.75, 7.22 and 11. These averages do not seem to be notably different from average cartel sizes reported in other studies (cp. Levenstein and Suslow, 2004b). Likewise, we observe no unusual difference in terms of the mean cartel duration. For our sample the average ringleader cartels lasted just about 92 month, or 7.6 years.

2.2.2 Ringleader characteristics

A defining feature of ringleaders is their market position. In 11 out of the 14 cases, the largest firm in terms of market shares was one of the ringleaders. This finding supports Ganslandt et al. (2010), which establish that ringleaders are frequently substantially larger than other firms. There are two cases for which this was not true. Yet, in one of these cases, *Interbrew and Alken-Maes*, the ringleaders were in fact the two largest brewers in Belgium, but not in the market segment where the cartel was active (Belgian private label beer). Likewise, the ringleaders in *Citric acid* were the world's biggest vitamin producers, but not in the relevant market segment. A third case, *Viandes Bovines Françaises*, had the association of farmers as a ringleader, which makes it difficult to use market shares as a measure of market power. Though our sample suggest a strong tendency to having the market leader as the ringleader, it is noteworthy that in *Gas Insulated Switchgear*, the largest firm left the cartel, which left space for a smaller producer to take the lead.

Table 2.1: European Cartel Ringleader Cases 2000 - 2010

Case Title	Identified Ringleader	Instigator	Leader
36545 - Amino Acids	Ajinomoto, Archer Daniels Midland		✓
36490 - Graphite electrodes	SGL Carbon, UCAR International	✓	✓
37512 - Vitamins	F. Hoffmann-La Roche, BASF	✓	✓
37614 - Interbrew and Alken-Maes	Interbrew, Alken-Maes	✓	
36604 - Citric acid	Archer Daniels Midland, F. Hoffmann-La Roche		✓
36212 - Carbonless paper	Arjo Wiggins Appleton	✓	✓
37667 - Specialty Graphite	SGL Carbon	✓	✓
38279 - Viandes Bovines Francaises	Federation nationale bovine	✓	
37370 - Sorbates	Hoechst, Daicel Chemical Industries		✓
36756 - Sodium Gluconate	Jungbunzlauer		✓
38456 - Bitumen Nederland	Shell, Koninklijke Volker Wessels Stevin	✓	✓
38899 - Gas Insulated Switchgear	Siemens, ALSTOM, AREVA		✓
38710 - Bitumen Spain	Repsol, Productos Asfalticos		✓
39406 - Marine Hoses	Bridgestone, Parker ITR		✓

2.2.3 Role of leader(s)

We identify a variety of tasks that a ringleader performs, and it is not always clear-cut how and for which reasons the Commission decided to evaluate a particular task into the categories of instigation vs. leadership. The role of ringleaders varied remarkably among the cartels and often consisted of a mixture of activities related to instigation and leadership.

Further, the line between active membership and leadership is not clearly drawn and the Commissions argumentation does not appear coherent. In *Amino acids*, the Commission finds it irrelevant whether these tasks were offered to the leader by the other participants or seized by the leader on its own initiative.[13] In *Graphite electrodes* however, Tokai was designated by the cartel to collect the volume information but besides its active engagement Tokai was not declared a leader by the Commission.

As to instigation, the most common activity was to encourage other companies to join the cartel. Specifically, in 6 out of 14 cases the ringleader approached other companies in order to persuade them to join the agreement. How the ringleader approached other companies varied. In some of these cases, the ringleader explained the mechanisms of the cartel and highlighted the added value of previous arrangements. In other cases, the ringleader invited to "club meetings" or offered the restructuring of an old agreement. Perhaps surprisingly, coercion seems to have played a minor role. In none of the cases the Commission increased the fines due to coercion. In only three cases, there was sufficient evidence that leaders (ab)used their dominant market position to discipline unwilling undertakings or to pressure firms into joining the cartel.[14]

Activities concerned with the operation of the cartel ("Leadership") can be categorized in four main tasks. First, in 10 out of 14 cases, leaders were the first to implement a collusive agreement or to announce price changes. For example, in *Vitamins*, BASF and F. Hoffmann-La Roche took turns in announcing price changes and then invited the other cartel participants to follow. Likewise, in *Carbonless paper*, the ringleader was responsible for selecting the cartel price.[15] This finding confirms recent theoretical predictions about collusive price leadership. For instance, Ishibashi

[13] See COMP/36.545/F3 Amino acids para 353.

[14] See Marshall, Marx and Samkharadze (2011) for a description and analysis of dominant-firm conduct by cartels.

[15] The reason why cartel members might want to cede their power over the price decision to a ringleader is discussed in Hovenkamp and Leslie (2011). They argue, among other things, that a participatory bargaining process increases the risk of detection and conviction, as each communication creates evidence against the cartel.

(2008) establishes that under price competition with capacity constraints larger firms have an incentive to move early. Moreover, Mouraviev and Rey (2011) show that price leadership can facilitate collusion by allowing for more effective punishments. Second, leaders exercised tasks intended to enhance the operation of the cartel. In 9 out of 14 cases, this includes manning the cartel secretariat, collecting commercial information and exchanging summaries, providing presentations about the state of the industry or allocating customers. In *Marine Hoses*, for example, Bridgestone allocated tenders to cartel members and provided bidding instructions and tables indicating the actual state of tender allocation, while in *Amino Acids*, Ajinomoto acted as the central office "to which each lysine producer would provide monthly sales figures. Ajinomoto's job would be to keep track of the figures so that the producers could make adjustments in their sales to limit the overall annual sales to the agreed maximums." (Case COMP/36.545/F3 - Amino Acids para 122).

Third in 9 out of 14 cases, the leader's task was to organize the actual communication. Among other things, this includes hosting, organizing or chairing meetings, paying bills and setting up the agenda. In *Sorbates*, for instance, Hoechst hosted, organized and payed for the European cartel meetings. Finally, in 6 cases, the ringleader acted as a representative or intermediary. In *Bitumen Nederland*, KWS and Shell were both representatives of either buyer or seller groups and often negotiated bilaterally to reach outcomes that were 'binding' for the other participants. In *Sodium Gluconate*, Jungbunzlauer negotiated between members in case of internal rivalry.

Other activities were more specific to the need of the respective cartel. For instance, in *Gas Insulated Switchgear* part of the leading role of Siemens was to provide European and Japanese fellow members with Siemens mobile phones equipped with encryption technology.

2.2.4 Adjustment to base fines

The European Commission rules that a firm that takes the role of a leader or instigator bears a special responsibility, which may result in an increase of the basic fine. We find that, once leadership or instigation was identified as an aggravating circumstance, ringleaders indeed received an increase of the basic fine in the range of 30% to 85%.[16] The average fine increase is about 42%. In *Specialty Graphite*, the

[16] In *Graphite electrodes*, the ringleader SGL was giving warnings to other companies of the forthcoming investigations and continued the infringement after the initial investigation, which

Commission increased the fine for SGL Carbon by 50%. SGL Carbon challenged this outcome as it felt that the weight given to its leading role was disproportionate. The Commission, however, justified its decision by stating that it has "no obligation to apply a mathematical formula nor is it generally bound by previous decisions".[17] In five cases, a ringleader asked for non-imposition or a substantial reduction ($> 50\%$) of its fine under the 1996 leniency notice. However, these requests were rejected with reference to their leading role. Overall, the leader received a significant reduction of the fine in the range of 10% to 50% in nine cases.

2.3 Model

In the remainder of this paper, we study a modified version of the price setting supergame with heterogeneous capacity constraints as presented in Bos and Harrington (2010). Consider a homogeneous good industry in which a fixed and finite set of firms, denoted by $N = \{1, \ldots, n\}$, interact repeatedly over an infinite, discrete time horizon. Producers have constant unit cost $c \geq 0$ and choose prices from $\{0, \epsilon, 2\epsilon, \ldots, c-\epsilon, c, c+\epsilon, \ldots\}$ with ϵ being small and positive. In each period, firms simultaneously make price decisions so as to maximize the expected discounted sum of their profit stream, where $\delta \in (0, 1)$ is the common discount factor. There is perfect monitoring in the sense that prices chosen in previous periods are common knowledge. The production capacity of firm $i \in N$ is denoted by k_i and we assume that $k_1 \geq k_2 \geq \ldots \geq k_n > 0$, which is without loss of generality. Total industry capacity is $K = \sum_{i \in N} k_i$ and capacity stocks remain fixed throughout the entire game.

Monopoly profit is $(p - c)D(p)$ and we assume that market demand $D(p)$ is twice continuously differentiable with $D'(p) < 0$ and $D''(p) \leq 0$, *i.e.*, monopoly profit is strictly concave. Moreover, $D(c) > 0$ and the monopoly price is denoted by p^m: $D(p^m) + (p^m - c)D'(p^m) = 0$. Consumers buy first from the cheapest supplier(s). Demand of firm i is then given by $D_i(p_i, \mathbf{p}_{-i})$, which depends on its own price p_i and the prices set by all rivals $\mathbf{p}_{-i}$. Firms produce to meet demand up to their capacity. Following Bos and Harrington (2010), we make the following simplifying assumptions on firms' capacity:

$$\textbf{Assumption 1: } k_i < D(p^m) \text{ and } \sum_{j \in N \setminus \{i\}} k_j \geqq D(c), \text{ for all } i \in N.$$

might explain a proportion of the high fine increase.

[17] Case T-71/03 Tokai Carbon and Others v. Commission, paras 310 and 315.

The first part of the assumption says that each firm has insufficient capacity to supply monopoly demand. Thus, the possibility of producers being very large in absolute terms is ruled out. Yet, they can be of significant size in relative terms. The second part states that any $n-1$ firms can meet competitive demand. Indeed, the second part implies that there are two (symmetric) Nash equilibria in the one-shot game. The static Nash equilibrium has all firms either pricing at c or at $c+\epsilon$. However, as results will be derived for sufficiently small ϵ, this difference is negligible. As a consequence, producers make zero profits absent collusion. A final implication of the assumption is that duopolistic market structures are excluded (*i.e.*, $n \geq 3$). Albeit somewhat restrictive, it holds for the majority of industries in which collusive behaviour has been observed.[18] For example, the cartels discussed in section 2.3 all operated in markets with more than three undertakings. Moreover, cartels comprising two members form a special case when considering ringleader exclusion as the presence of a ringleader would eliminate the "race to the courtroom".

In the following, we consider the possibility of an all-inclusive price-fixing cartel and assume a proportional profit allocation rule.[19] Given a common cartel price $p > c+\epsilon$, individual cartel profits are therefore given by

$$\pi_i(p) = (p-c)D(p)\frac{k_i}{K}, \text{ for all } i \in N. \tag{2.1}$$

Yet, by forming a cartel, firms expose themselves to antitrust enforcement. There is a risk of being caught after each collusive period and, for simplicity, it is assumed that conviction leads to a permanent breakdown of the collusive agreement. Let the probability of conviction be given by $\alpha \in (0,1)$. Leaving out the potential discount due to leniency (which we will introduce later), successful cartel prosecution has firm i paying a fine $F(k_i)$. We make the following assumption on the penalty function $F(k)$.

[18] A notable exception is the cartel agreement between auction houses Christie's and Sotheby's in the 1990s. See Case COMP/E-2/37.784, Fine Art Auction Houses.

[19] Bos and Harrington (2010) provide various rationales for a proportional profit sharing rule. Capacity may be taken as a proxy for market share and there exists evidence of cartels that based their profit sharing rule on the market share of members in years prior to the cartel. For example, in COMP/36.545/F3 Amino Acids the undertakings agreed to fix future quantities based on the sales of the previous year, while in COMP/E-1/36 604 Citric acid the basis of the market sharing quota was the average of the last three years' sales. Moreover, the proportional rule captures the idea that the largest firms (*i.e.*, often the ringleaders) have most to gain from collusion as their additional profit is highest. Hence, they have the strongest incentive to initiate an agreement.

Assumption 2: The antitrust penalty function $F(k)$ is continuously differentiable with $F(0) = 0$ and $F'(k) > 0$ at all $k \in (0, D(p^m))$.

This assumption states that larger firms incur a higher fine in case of conviction, all else equal. Moreover, as we assume a proportional allocation rule, fines are positively correlated with cartel gains. While many paper on leniency use lump-sum fines which are independent of size of the convicted firm, we assume that the magnitude of antitrust fines depends on a firms' share in the industry.[20] This seems to be a plausible assumption for many jurisdictions.[21] For instance, the European Commission's 2006 fining guidelines establish a link between fines and the value of sales to which the infringement relates. More precisely, the basic fine is computed as a proportion of the value of sales in the last business year before taxes while taking account of the gravity and duration of the infringement. In a similar vein, the U.S. penalty guidelines offer a way to determine the basic fine using the pecuniary loss due to the offense for which 20% of the volume of affected commerce (*i.e.*, total U.S. sales revenue) is used as a proxy.[22] As the precise determination of antitrust penalties is complex and varies among jurisdictions, we do not further specify the antitrust penalty function.[23]

2.3.1 Non-discriminatory Leniency: A Benchmark

In the next step, we introduce and analyse the cartel's problem under the assumption that a ringleader can apply for leniency like regular cartel members. We require collusion to be a subgame perfect Nash equilibrium outcome of the game and assume that firms adopt grim-trigger strategies to sustain supra-competitive prices. That is, every member of the cartel adheres to the collusive agreement until one firm deviates. In the event of defection, the coalition collapses with a one-period time lag and all

[20] Contributions that treat the antitrust fine as a parameter include, *e.g.*, Motta and Polo (1999, 2003), Spagnolo (2000, 2008) and Aubert et al. (2006). For a discussion of three levels of antitrust fine functions (fixed, proportional and more than proportional) and their impact on the profit-maximizing cartel price see Houba *et al.* (2010).

[21] The aim of the 2006 EU guidelines revision was to better reflect the perniciousness of an infringement as well as the share of each company involved. See Press Release IP/06/857, European Commission, Competition: Commission Revises Guidelines for Setting Fines in Antitrust Cases (June 28, 2006).

[22] See European Commission (2006), Guidelines on the method of setting fines imposed pursuant to Article 23(2)(a) of Regulation No 1/2003 and United States Sentencing Commission (2010), Guidelines Manual.

[23] For instance, in a recent study on European cartel fines, Veljanovski (2011) finds that many aspects of the fining procedure are unexplained and that the Commission displayed excessive secrecy in its reasoning.

firms set stage game Nash equilibrium prices in all periods following the period of defection.[24] For ease of analysis, we assume that all members ex ante have identical expectations about the amount of discount they might receive due to leniency. Let this expected discount be captured by a parameter $\beta \in (0,1)$. Specifically, higher values of β correspond to larger expected reductions of the antitrust fine, all else equal.[25]

Under this nondiscriminatory antitrust regime, the collusive value for firm i is defined recursively by

$$V_i(p) = (p-c)D(p)\frac{k_i}{K} - \delta\alpha(1-\beta)F(k_i) + \delta(1-\alpha)V_i(p), \tag{2.2}$$

which is equivalent to

$$V_i(p) = \frac{(p-c)D(p)\frac{k_i}{K} - \delta\alpha(1-\beta)F(k_i)}{1-\delta(1-\alpha)}. \tag{2.3}$$

If a cartel member deviates from the cartel agreement, then it will optimally set a price $p-\epsilon$. This is so, as by Assumption 2 the cheating firm produces up to capacity. Consequently, further lowering prices is unprofitable. We assume that once a firm deviates, it cannot be convicted for its former misconduct.[26] Therefore, given that the price grid is sufficiently fine, optimal deviating profits for firm i are approximately equal to $(p-c)k_i$.

The incentive compatibility constraint (ICC) under a nondiscriminatory leniency regime is then given by

$$\frac{(p-c)D(p)\frac{k_i}{K} - \alpha(1-\beta)\delta F(k_i)}{1-\delta(1-\alpha)} \geq (p-c)k_i, \text{for all } i \in N.$$

[24] Note that the grim-trigger strategy is the most severe credible threat in this setting. That is to say, whenever some level of collusion cannot be sustained by the threat of eternal competition, then it cannot be sustained by any other credible punishment strategy.

[25] One possible example for the expected discount β would be the expectations regarding an optimal leniency policy as described in Harrington (2008). There the expected discount is captured by $\frac{n-1+\theta}{n}$, where $\theta \in [0,1]$ gives the percentage of the penalty to be paid by the first firm to report. Hence, $\theta = 0$ corresponds to maximal leniency and $\theta = 1$ implies that the leniency program is ineffective or absent.

[26] It is in the legislators interest to induce defection in order to break down cartels, hence such behaviour should not be punished. However, allowing for antitrust enforcement after defection affects part of the analysis. In Bos and Wandschneider (2013) we allow the deviating firm to obtain leniency. Ringleader exclusion then affects both their collusive and deviating profits. Under such a setting, ringleader exclusion always loosens the incentive constraint of regular cartel members and tightens or loosens the incentive constraint of ringleaders.

Rearranging gives,

$$\lambda(p) \equiv (p-c)\left(\frac{D(p)}{K} - 1 + \delta(1-\alpha)\right) - \delta\alpha(1-\beta)\frac{F(k_i)}{k_i} \geq 0, \text{for all } i \in N. \quad (2.4)$$

Let us have a closer look at the ICC as given by (2.4). To begin, observe that the ICC is violated for $p = c$. Colluding on prices sufficiently close to unit production costs is therefore not feasible, which is due to the antitrust penalty. Next, the LHS of (2.4) is strictly concave:

$$\lambda''(p) = \frac{2D'(p) + (p-c)D''(p)}{K} < 0.$$

For collusion to be feasible, this implies that the first order condition at c must be positive. Taking the derivative of $\lambda(p)$ gives:

$$\lambda'(p) = \frac{D(p) + (p-c)D'(p)}{K} - 1 + \delta(1-\alpha).$$

Thus, a necessary condition for collusion is $\lambda'(p) > 0$ or $\delta > \frac{K-D(c)}{(1-a)K}$. Feasibility of collusion therefore requires that firms are sufficiently patient and that the probability of getting caught is not too high.[27]

As monopoly profit is strictly concave, (2.3) reveals that all cartel members agree to maximize the cartel price (not exceeding the monopoly price). Therefore, the cartel faces the following constrained maximization problem:

$$\max_p (p-c)D(p), \quad (2.5)$$

subject to

$$\lambda_i(p) \geq 0, \text{ for all } i \in N.$$

Clearly, the cartel will optimally set the monopoly price when $\lambda_i(p^m) \geq 0$ for all $i \in N$. However, the monopoly price may not be sustainable. To see this, notice that $\lambda_i'(p^m) = -1 + \delta(1-\alpha) < 0$ and therefore $\lambda_i(p)$ reaches its maximum at a price below the monopoly price. Consequently, collusion may be feasible only at a price below the monopoly price. In the following, let $p^* \equiv \min[\bar{p}, p^m]$ denote the solution to (2.5), where $\bar{p}$ is the constrained solution.

[27] Observe that the ICC's are satisfied when $\delta \to 1$ and $\alpha \to 0$.

Given that collusion is sustainable but not at the monopoly price, the issue to consider is which firm has the binding ICC. The next result shows that this effectively depends on the shape of the antitrust penalty function.

Lemma 1 *Suppose $k_j > k_i$.*
(i) If $F'(k) < \frac{F(k)}{k}$, then $\lambda_j(p) \ > \lambda i(p)$,
(ii) If $F'(k) = \frac{F(k)}{k}$, then $\lambda_j(p) \ = \lambda i(p)$,
(iii) If $F'(k) > \frac{F(k)}{k}$, then $\lambda_j(p) \ < \lambda i(p)$.

Proof. With a non-discriminatory leniency programme, the incentive compatibility constraint of each cartel member is of the following form:
$(p - c)\left(\frac{D(p)}{K} - 1 + \delta(1 - \alpha)\right) - \delta a(1 - \beta)\frac{F(k)}{k}$.
Taking the derivative with respect to k yields $\frac{k(-\delta a(1-\beta)F'(k))-(-\delta a(1-\beta)F(k))}{k^2} \lesseqqgtr 0$. Rearranging gives $F(k) - kF'(k) \lesseqqgtr 0$.
Hence, at any price p: (i) if $F'(k) - \frac{F(k)}{k} < 0$, then $\lambda(p)$ is highest for the largest firm(s), (ii) if $F'(k) - \frac{F(k)}{k} = 0$, then $\lambda(p)$ is equal for all cartel members, and (iii) if $F'(k) - \frac{F(k)}{k} > 0$, then $\lambda(p)$ is highest for the smallest firm(s). ■

Part (i) states that the smallest member has the tightest ICC when the antitrust penalty function is concave. Part (ii) shows that all firms face the same ICC when the fining structure depends on firm size in a linear fashion. Part (iii) states that the largest member has the tightest ICC when the antitrust penalty function is convex.

2.3.2 The Impact of Ringleader Exclusion

Next, we evaluate the potential impact of ringleader exclusion on the collusive price level and assess whether it provides incentives to compensate the ringleader for its loss in expected leniency discount. We will show that a discriminatory leniency programme leads to higher prices when (i) the monopoly price cannot be sustained under a non-discriminatory leniency policy, (ii) antitrust fines depend on individual cartel gains in a nonlinear fashion, and (iii) the size distribution of members is sufficiently heterogeneous. Additionally, we find that adopting a different profit sharing rule is most likely when the intended ringleader is the smallest firm. In light of available evidence from antitrust practice, we consider the use of side-payments to compensate the ringleader possible but rather unlikely.

Collusive Price Level

To begin, suppose the cartel has a ringleader that is not (or only partly) eligible to apply for leniency. An effect of such a discriminatory leniency policy, when compared to the benchmark, is that the expected discount for regular cartel members increases, whereas it decreases for the ringleader. This asymmetry is due to the fact that the ringleader is excluded from (full) immunity and therefore will be less eager to apply for leniency. In turn, this *ceteris paribus* increases the chance for regular members to win the "race to the courthouse", thereby increasing the expected discount. Let the expected discount for the ringleader and regular members under a discriminatory leniency programme be respectively given by β_l and β_m, with $0 \leq \beta_l < \beta < \beta_m < 1$. Thus, moving from a non-discriminatory to a discriminatory leniency programme loosens the ICC of regular cartel members and tightens the ICC of the ringleader, all else equal. This implies that in order to assess the impact of ringleader exclusion on the collusive price level it is sufficient to evaluate the change in the ICC of the ringleader.[28]

In the following, let l indicate the ringleader and let k_l denote its production capacity. The ringleader's ICC is thus given by

$$\lambda_l(p) \equiv (p-c)\left(\frac{D(p)}{K} - 1 + \delta(1-\alpha)\right) - \delta\alpha(1-\beta_l)\frac{F(k_l)}{k_l} \geq 0.$$

The next result shows under which conditions a discriminatory leniency policy leads to higher prices compared to a nondiscriminatory leniency policy. In stating this result, let p^{**} denote the optimal price of the cartel with a ringleader under a discriminatory leniency regime. Additionally, define the tightest ICC under a nondiscriminatory leniency policy as $\bar{\lambda}(p) \equiv \min\{\lambda_1(p), \ldots, \lambda_n(p)\}$ and let $\bar{k}$ be the capacity level for which $\bar{\lambda}(p^*) = 0$.

Theorem 2 *Suppose that there is a ringleader and that collusion is viable under a nondiscriminatory leniency policy.*

$p^{**} > p^*$ *if and only if* $p^* < p^m$ *and* $\frac{1-\beta}{1-\beta_l} > \frac{\frac{F(k_l)}{k_l}}{\frac{F(\bar{k})}{\bar{k}}}$.

[28] Notice that the same logic applies when there is more than one ringleader. In that case it would be sufficient to analyze the impact of ringleader exclusion on the ringleader with the tightest ICC under a nondiscriminatory leniency program.

Proof. First, notice that the cartel does not find it optimal to set a price in excess of the monopoly price. Consequently, if $p^* = p^m$, then $p^{**} \leqq p^*$. Suppose therefore that $p^* < p^m$, which implies $\lambda_l(p^*) = 0$. As ringleader exclusion loosens the ICC of regular members and tightens the ICC of the ringleader, p^* is sustainable under a discriminatory leniency regime when $\lambda_l(p^*) \geqq \bar{\lambda}(p^*)$. If $\lambda_l(p^*) < \bar{\lambda}(p^*)$, then $p^{**} < p^*$. Likewise, $p^{**} > p^*$ when $\lambda_l(p^*) > \bar{\lambda}(p^*)$, or $(p^* - c)\left(\frac{D(p^*)}{K} - 1 + \delta(1-a)\right) - \delta a(1-\beta_l)\frac{F(k_l)}{k_l} > (p^* - c)\left(\frac{D(p^*)}{K} - 1 + \delta(1-a)\right) - \delta a(1-\beta)\frac{F(\bar{k})}{\bar{k}}$. Rearranging gives $\frac{1-\beta}{1-\beta_l} > \frac{\frac{F(k_l)}{k_l}}{\frac{F(\bar{k})}{\bar{k}}}$. ■

The above result indicates that ringleader exclusion allows the cartel to sustain higher prices when three requirements are met. First, the optimal cartel price under a nondiscriminatory leniency policy must lie strictly below the monopoly price. Clearly, if the joint profit maximum can be sustained without ringleader exclusion, then the introduction of a discriminatory leniency policy will induce a weakly lower cartel price. Second, the antitrust penalty must depend in a nonlinear fashion on cartel gains. If fines are proportional to firm size, then the RHS of $\frac{1-\beta}{1-\beta_l} > \frac{\frac{F(k_l)}{k_l}}{\frac{F(\bar{k})}{\bar{k}}}$ is equal to one whereas the LHS is strictly smaller than one. Finally, the size distribution of firms must be sufficiently heterogeneous. This is because when all firms are of equal size the RHS is equal to one independent of the shape of the antitrust penalty function.

If we embrace the finding in Section 2.3 that ringleaders are typically the largest firms, then Theorem 2 supports a proportional or more than proportional fining structure. By Lemma 1, we know that the largest cartel member has the tightest ICC under a nondiscriminatory leniency programme when the antitrust penalty function is linear or convex. Thus, in these cases $\bar{k} = k_1$ and therefore $\frac{1-\beta}{1-\beta_l} > \frac{\frac{F(k_l)}{k_l}}{\frac{F(\bar{k})}{\bar{k}}}$ is violated when $k_l = k_1$. Yet, in this case ringleader exclusion may still lead to higher prices when the smallest member takes a leading role provided that the fining structure is nonlinear. Consequently, to prevent the potential adverse effect of ringleader exclusion, the above result suggests that an optimal antitrust punishment system prescribes the imposition of fines proportional to firm size.

Side-payments

Thus far, results have been derived for a given profit allocation scheme. However, it is important to note that adopting a different profit sharing rule can be beneficial for the cartel. Provided that the fining structure is nonlinear, it may pay for firms to reallocate profits so as to relax the tightest ICC. Specifically, when the antitrust penalty function is concave (convex) it can be profitable to adopt a less (more) than proportional profit sharing rule. One can imagine that this issue is more pronounced with ringleader exclusion. For example, potential cartel members may be hesitant to take a leading role under a discriminatory leniency policy and will only do so when sufficiently compensated for giving up the right to apply for (full) immunity.

Indeed, in the analysis above we have compared a nondiscriminatory with a discriminatory leniency policy under the assumption that there is a ringleader. Yet, an alternative way of looking at the benchmark is that it describes collusion under a discriminatory leniency regime absent ringleaders. This raises the question of whether and when having a ringleader is beneficial for all cartel members. Given the proportional profit sharing rule, firm $i \in N$ has an incentive to become a ringleader only when

$$(p^{**} - c)D(p^{**})\frac{k_i}{K} - \delta\alpha(1 - \beta_l)F(k_i) > (p^{*} - c)D(p^{*})\frac{k_i}{K} - \delta\alpha(1 - \beta)F(k_i). \quad (2.6)$$

Observe that (2.6) is violated for all $i \in N$ when $p^{**} \leq p^{*}$. That is, if the presence of a ringleader does not lead to higher prices, then none of the firms has an incentive to take a leading role. In this case, however, assigning a ringleader may still be profitable provided that cartel gains are allocated properly. The question of interest is therefore whether and when the presence of a ringleader generates a higher total cartel value in comparison with the nondiscriminatory benchmark.

To address this question, note that the total collusive value absent a ringleader is

$$V(p^{*}) = \frac{(p^{*} - c)D(p^{*}) - \delta\alpha(1 - \beta)\sum_{i\in N} F(k_i)}{1 - \delta(1 - \alpha)}, \quad (2.7)$$

whereas the total cartel value with a ringleader is given by

$$\begin{aligned} V(p^{**}) &= \frac{(p^{**}-c)D(p^{**})\sum_{i\in N\setminus\{l\}}\frac{k_i}{K} - \delta\alpha(1-\beta_m)\sum_{i\in N\setminus\{l\}}F(k_i)}{1-\delta(1-\alpha)} \\ &+ \frac{(p^{**}-c)D(p^{**})\frac{k_l}{K} - \delta\alpha(1-\beta_l)F(k_l)}{1-\delta(1-\alpha)} \\ &= \frac{(p^{**}-c)D(p^{**}) - \delta\alpha((1-\beta_m)\sum_{i\in N\setminus\{l\}}F(k_i) + (1-\beta_l)F(k_l))}{1-\delta(1-\alpha)} \end{aligned} \tag{2.8}$$

There exists a profit division rule for which assigning a leader makes all firms better off when $V(p^{**}) > V(p^*)$. Comparing (2.8) with (2.7) gives

$$V(p^{**}) > V(p^*) \Longleftrightarrow$$

$$(p^{**}-c)D(p^{**}) - (p^*-c)D(p^*) > \delta\alpha((\beta-\beta_l)F(k_l) - (\beta_m-\beta)\sum_{i\in N\setminus\{l\}}F(k_i)). \tag{2.9}$$

This condition reveals that whether or not side-payments are potentially beneficial essentially depends on the change in the cartel's objective and the change in the total expected antitrust penalty. Specifically, notice that if assigning a ringleader has no effect on the cartel price, then (2.9) is satisfied only when the decrease in expected discount for the ringleader is more than offset by the increase in expected discount for regular cartel members. Moreover, the RHS of (2.9) is maximal for $k_l = k_1$ and therefore is least likely to hold when the intended leader is the largest firm.

To gain some further insight, consider the extreme case where the ringleader and all regular members but the first to self-report are not eligible for leniency. Suppose further that the applicant receives full immunity (*i.e.*, $\beta = \frac{1}{n}$, $\beta_m = \frac{1}{n-1}$ and $\beta_l = 0$). If $p^{**} = p^*$, then $V(p^{**}) > V(p^*)$ only when the antitrust penalty of the ringleader is lower than the average fine:

$$F(k_l) < \frac{\sum_{i\in N}F(k_i)}{n}.$$

Thus, in this case compensation is a possibility when the ringleader is the smallest firm and not beneficial when the ringleader is the largest firm. In light of the empirical findings in Section 2.3, these results suggest that ringleader exclusion seems unlikely to create adverse effects by providing incentives to adopt more complicated profit allocation rules. Ringleaders that have been identified in antitrust practice were typically among the larger firms in the industry. Indeed, at least intuitively one would expect dominant firms to take a central position in the cartel. This also holds with respect to cartel formation as larger firms have usually more to gain from collusion. Furthermore, various empirical cartel studies show that bargaining complexities should not be underestimated. For example, Levenstein and Suslow (2004) state that "Bargaining problems were much more likely to undermine collusion than was secret cheating. About one quarter of the cartel episodes ended because of bargaining problems. Bargaining issues affected virtually every industry studied."

In sum, deviating from a simple and intuitive profit allocation rule is most likely to be beneficial when the intended ringleader is the smallest firm. However, evidence from antitrust practice does not offer much support for this possibility.

2.4 Conclusion

In this study, we have sought to shed light on cartel ringleaders in relation to the corporate leniency programme. Depending on the jurisdiction, a ringleader may or may not be eligible to apply for leniency. As not much is known about cartel ringleaders, we have first conducted a survey of recent European cartel cases to identify some common characteristics of ringleaders. The results of this survey reveal that (*i*) there is often more than one ringleader, (*ii*) the role of ringleaders is diverse and (*iii*) ringleaders are typically the largest cartel members. Our theoretical analysis shows that ringleader exclusion can create adverse effects. Specifically, disqualifying a cartel ringleader from obtaining leniency can lead to higher collusive prices when (*iv*) the joint profit maximum is unfeasible under a nondiscriminatory leniency policy, (*v*) antitrust fines depend on individual cartel gains in a nonlinear fashion and (*vi*) the size distribution of members is sufficiently heterogeneous.

These results are driven by two main factors. The first is quite general. In comparison to a nondiscriminatory leniency regime, ringleader exclusion tightens the ICC of ringleaders and loosens the ICC of regular cartel members. Given a particular profit allocation rule, the magnitude of these changes determine whether or not the cartel can sustain higher prices. The second is more specific. We have assumed that collusive profits and expected antitrust penalties depend positively on production capacity, which is taken as a proxy for firm size. This assumption finds support in antitrust practice. Yet, it is potentially profitable for a cartel to adopt a more complicated profit sharing rule. In this respect, we have shown that assigning a ringleader is most likely to create a higher total cartel value when the intended leader is the smallest firm. From a policy perspective, our overall findings suggest that it is optimal to impose antitrust penalties proportional to firm size when one is willing to exclude cartel ringleaders from the corporate leniency programme.

Chapter 3

An Experimental Study of Ringleader Exclusion from Leniency Programmes

> "Divide and rule, a sound motto. Unite and lead, a better one."
> Johann Wolfgang von Goethe (1814)

3.1 Introduction

Leniency programmes offer a cartel member the opportunity to self-report its illegal behaviour in exchange for non-imposition or reduction of antitrust penalties. These programmes, initially established in the U.S. in 1978 and introduced to the EU in 1996, are nowadays widely considered to be an effective means to fight cartels in both antitrust jurisdictions. Although they share many similarities, leniency programmes differ in their treatment of "cartel ringleaders", which is the term commonly used for a centralised decision maker that instigates or leads a cartel (cp. Ganslandt et al., 2008; Hovenkamp and Leslie, 2011; Bos and Wandschneider, 2011 and most recently Davies and De, 2013). Under the current U.S. leniency policy, and unlike to the EU legislation, a cartel ringleader is not eligible for amnesty. This potentially evokes substantial differences in the firms conduct.

In this paper we discuss the impact of a ringleader exclusion regime on cartel formation, prices and stability from an experimental viewpoint. The motivation for our study is the general lack of understanding about which legal treatment is to be preferred. Intuitively, arguments can be made both in favour of and against the

U.S. exclusionary policy. On the one hand, exclusion makes the leader a trustworthy 'partner in crime', which is likely to enhance cartel stability and foster collusion.[1] On the other hand, the leader faces a higher expected fine and firms may abstain from taking the initiative to lead, which in turn increases deterrence. Due to the lack of a robust theory, it remains an open question as to which effect dominates.[2] The likely effects of ringleader exclusion are further difficult to evaluate by the use of real world empirical testing, as the full population of deterred, detected or unknown cartels are unobservable. This paper seeks to remedy these problems by analysing the likely implications of both policy regimes in a simplified and controlled experimental world.[3]

At the heart of the experiment is a market with three firms, competing in a repeated Bertrand game with inelastic demand and zero marginal cost (cp. Dufwenberg and Gneezy, 2000). Firms can form a non-binding price cartel. However, such a collusive act is illegal and, if detected, can result in antitrust penalties. The novelty of our design is that we vary the possibility to self-report in exchange for a discount on the antitrust penalties in a controlled manner. In the baseline condition, which resembles the European leniency policy (henceforth labelled "*EU*"), all group members have the chance to report. By contrast, in the treatment condition, which corresponds to the discriminatory U.S. legislation (henceforth labelled "*US*"), we restrict the ability to report to two group members. This design encompasses the particularity of being a ringleader under the U.S. and EU legislation, and permits to evaluate the impact of this asymmetry in the firms' reporting abilities on cartel formation, pricing and stability.[4]

The study finds that a firms' willingness to engage in collusive misconducts does not decrease when the ringleader is excluded from leniency. On the contrary, taking into account that a cartel requires unanimous consensus of all three firms, the results of this study in fact indicate that cartel formation may even be facilitated when

[1] On the correlation between trust and cooperation see for example Gächter et al. (2004).

[2] For a non-formal discussion of both effects see Leslie (2006). More recently Herre and Rasch (2009) and the revised version Herre et al. (2012) analyse the effect of exclusion on cartel deterrence, while Bos and Wandschneider (2013) show that ringleader exclusion affects both sides of the incentive-constraints.

[3] The advantage of using experimental economics for researching competition policy designs are discussed in Normann and Ruffle (2011). Normann and Ricciuti (2009) assess the scope of laboratory experiments for economic policy making.

[4] It has been established that subjects in asymmetric experiments take significantly longer to reach an equilibrium and that asymmetric markets are less cooperative. See for example Mason, Phillips and Nowell (1992).

the leader is not able to obtain leniency. It can thus be suggested that ringleader exclusion is by no means a superior deterrence mechanism. The second finding of this study is that, contrary to the previous literature, we did not detect any evidence for higher prices in the exclusionary leniency regime. This can be seen as good news for American consumers.

Finally, we observe an ambiguous effect of ringleader exclusion on cartel stability. With ringleader exclusion there was a significant increase in the number of price deviations, indicating a destabilising effect of this policy. However, the reporting rate of regular cartel members dropped, potentially jeopardizing the race to the courtroom effect that leniency programmes aim to create.

Overall, taking into account that antitrust authorities may benefit in particular from the self-reporting of the ringleader, as arguably ringleaders hold most of the vital information that authorities need in order to successfully convict a cartel, our findings question the rationale behind excluding the cartel manager.

3.2 Related Literature

In recent years, there has been an interest in the optimal design of leniency programmes in both the theoretical (*e.g.*, Motta and Polo, 2003; Spagnolo, 2008; Harrington, 2008) and empirical (Miller, 2009; Brenner, 2009) literature. Additionally, to date several experimental studies related to leniency exist (*e.g.*, Apesteguia et al., 2007; Hinloopen and Soetevent, 2008; Hamaguchi et al., 2009; Dijkstra et al., 2011). However, a common feature of these experiments is, that the experimentalists' interest has concentrated on quantifying the desistance and deterrence effect of leniency as compared to a laissez-faire or traditional antitrust environment, rather than on the issue of ringleader exclusion.[5] To our knowledge, only two experimental studies address ringleaders. Bigoni et al. (2012a) investigate a variety of different antitrust frameworks in a two player Bertrand supergame, including a preliminary exploration of the effect of excluding a ringleader from the reporting phase. They maintain that excluding the ringleader has a small and insignificant effect on deterrence, but leads to higher market prices and less price deviations. The limitation of their study is, that leadership is implemented by treating the subject who first expresses the willingness to discuss prices (that is, presses a button on a computer

[5] Our decision to focus on the effect of ringleader exclusion and not to compare our treatments to a Laissez-Faire control or replicate existing findings is in parts motivated by the copious existence of experimental evidence on the overall effect of leniency.

screen) as the ringleader. In addition, their results rely too heavily on a duopoly setting, which can be argued to be inherently more effective in maintaining a collusive agreement (cp. Huck et al., 2004). It would thus be of interest to learn how ringleader exclusion affects collusion in an oligopoly.

In an attempt to overcome the previous limitation, a recent experimental study by Hesch (2012), which was conducted at the same time as this study, uses a three player game with a similar structure to the leniency experiment by Hinloopen and Soetevent (2008). Hesch identified that, given a low probability of detection, excluding a ringleader can stabilise the collusive agreement and lead to higher prices. The author further maintains that the opposite effect can be observed if the detection probability is high. We would like to point out that a drawback of this study lies in the modelling of the identity of the leader. The role of a ringleader is randomly assigned each period, and therefore is not a conscious choice by the participants. Subjects might hence anticipate leadership as an exogenous reduction in expected payoffs, rather than an endogenous decision.

This paper seeks to remedy this limitation by introducing a unique mechanism to select the leader in an oligopoly setting. Our experimental design, which will be described in the next section, is complementary to Hesch (2012)'s experiment by providing a meaningful role for the leader, so that subjects perceive its existence as non-trivial, unambiguous and realistic. A cartel ringleader in our setting is the subject whose suggested cartel price during the communication stage has been accepted by the two other group members. The novelty of this straightforward selection mechanism is that it ensures that the leader has a meaningful role: a leader is the subject which eases the difficulties of coordinating on the contract surface (cp. Osborne, 1976).[6]

[6] In a strict sense, leadership is not entirely endogenous, as the computer randomly picks a subject to make the first price suggestion. However, every subject has the chance to make a price suggestion which is rejected by the other subjects, and thereby the opportunity to avoid being the leader. It is the ability of group members to coordinate that in the end defines the leader.

3.3 Experiment

3.3.1 Experimental Procedure

The experiment was conducted in the laboratories of the Centre for Behavioural and Experimental Social Science (CBESS) at the University of East Anglia. Subjects were recruited from a large pool of university students via ORSEE (Greiner, 2004). In total, 114 students from various backgrounds and nationalities participated.

The experiment was fully computerised using the z-Tree software (Fischbacher, 2007), and the order of all choice buttons was randomised. Upon arrival, subjects were welcomed and seated in the laboratory at visually isolated computer terminals in order to avoid communication between them. Subjects were provided with both computerised and printed instructions, and were asked to read the instructions on their own. A questionnaire was used to ensure understanding. After the questionnaire subjects could ask questions, which were answered privately by the experimental supervisor. The presentation of the experimental instruction used a neutral language to avoid inducing behaviour by framing; no terms such as "price", "cartel" or "antitrust fine" were used throughout the experiment. However, for the sake of better readability, we will use the corresponding Industrial Economics terminology (e.g., "price" rather than "points") throughout this paper.

The experiment employed a fictional currency, labelled "experimental points", which was converted to British pound at the end of the experiment at a rate of $3.5p$ per point. Sessions lasted between 45 and 60 minutes including instruction time and subjects earned on average £11.08, including an initial endowment of £4. At the end of the experiment subjects were paid privately in cash. The maximum and minimum payments were £28.73 and £4.62, respectively.

3.3.2 Experimental Design

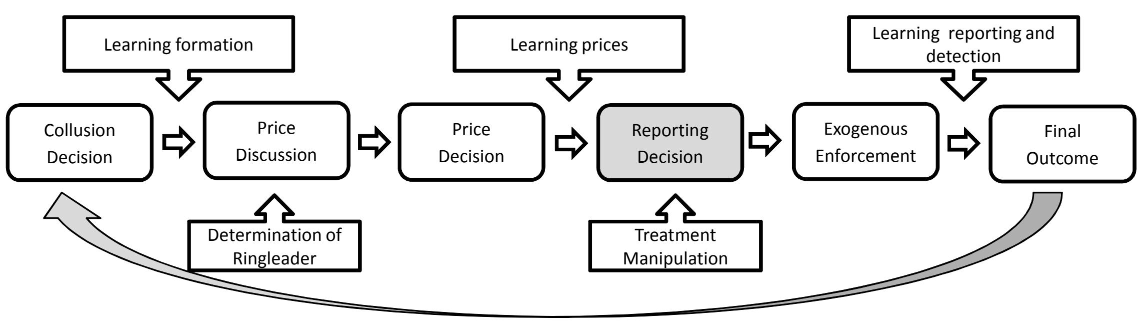
The core of the experiment is a repeated homogeneous product Bertrand game with zero cost of production as introduced by Dufwenberg and Gneezy (2000). Subjects play the role of a firm, which for 20 periods forms a market with two other randomly selected firms. The triopolies are fixed during the experiment and subjects have no knowledge of the identity of their counterparts. In each period, firms simultaneously and independently set integer prices that can range from 0 to 100. A buyer with a reservation price of $p^{max} = 100$, who is simulated by the computer, then demands one unit per period from the lowest pricing producer. The firm setting the lowest price earns a payoff equal to its price. In case of ties, the market demand is evenly split among the tied firms.

Prior to setting their price, firms can form a non-binding price agreement. Only if all three firms in a given market simultaneously choose to discuss prices, a cartel forms and firms can communicate in a restricted manner about their price choice. Price discussions come at the risk of an antitrust fine, which is levied upon the firms by a computerised Antitrust Authority. In the period in which a cartel exists, the Antitrust Authority has a 20% probability of discovering the agreement. As described in more detail below, firms further have the possibility to self-report the existence of a recent cartel in exchange for a reduction in their potential penalties. Convicted firms pay a fine equal to 10% of their chosen price, irrespective of if they are the lowest priced seller or not.[7]

We implement two treatments, corresponding to the two different legal frameworks discussed in the introduction. Treatments are identical in every aspect apart from their reporting stage. The first treatment ("*EU*") is our benchmark and resembled the European leniency programme in which all firms can self-report their cartel. The second treatment ("U*S*") tests for the effect of excluding the ringleader from leniency, and resembled the U.S. leniency policy in which all but the ringleader can report.

In more detail, in every period the following 6 steps are implemented:

Figure 3.1: Game Tree.



[7] Unlike Hinloopen and Soetevent (2008), we use a finite repose period in which firms can only be penalised in the period in which they communicated. Cartels do not carry further into later periods, so that firms had to decide whether or not to form a cartel at the beginning of each period. Arguably, the lack of liability for pre-existing cartels is a limitation of our design. It was a necessary simplification to avoid that subjects get confused over their liability, especially when a firm might be the ringleader in one old and undetected cartel, but regular member in another one.

1. Each firm simultaneously makes a binary choice to discuss prices. Individual decisions of firms in the same market are then revealed. If all three firms within a market want to discuss prices, communication takes place in a restricted manner in the next step. Otherwise the game proceeds at Step 3.

2. The computer randomly selects one firm in the market to suggest a (non-binding) cartel price. The two other firms can then either accept or reject the suggested price. If the decision to accept is unanimous, the experiment continues with Step 3. Else, the computer again randomly selects one of the three firms to suggest a price. This procedure continues until an agreement is reached or 2 minutes are over, in which case the experiment continues without an agreed-upon price. In our analysis, we consider firms which discuss prices as cartel members, even if no agreement is reached.[8] A crucial element of this stage is the selection of the ringleader, which is the firm whose price suggestion is accepted by the other firms in the market. How the leadership role is selected is common knowledge.[9] In the "*US*" treatment, firms are aware that the ringleader will later be restricted in its ability to obtain leniency in Step 4.

3. Each firm simultaneously sets a price from the discrete choice set $\{0, 1, \ldots, 100\}$. Price agreements from Step 2 are non-binding. The firm with the lowest price receives a net earning of $\frac{p}{L}$ points where L denotes the number of firm that choose the same, lowest price. Any firm with a higher price receives no earnings. Firms then learn the prices of the two other firms. In case a cartel exists, the experiment continues with the next step. Else the experiment continues with the Step 6.

4. Firms can self-report the existence of a cartel. We model leniency according to the procedure used by Apesteguia et al. (2007), but to follow current antitrust practice, which works on object rather than effect, we fine every firm involved in a cartel 10% of their chosen price, irrespectively if it has no revenues due to price deviations by other firms.

   Treatment "EU": In the "*EU*" treatment, all firms can report the cartel. If

[8] This notion of a cartel is common in the experimental literature. See for example Apesteguia et al., 2007; Hinloopen and Soetevent, 2008; Hesch, 2012.

[9] We feel that this is a more natural selection than a random determination of the ringleader by the computer as in Hesch (2012) or by a reaction task as in Bigoni et al. (2012a). The role of a ringleader in the real world, amongst others, is to assist with price coordination (see Chapter 2). This role is captured in our experimental design.

only one firm self-reports, it gains immunity from fines. If two (three) firms report, their fine is reduced by 50% (33%).[10]

Treatment "US": In the *"US"* treatment, the leader is excluded from reporting. If one member self-reports, it gains immunity from fines. If both members report, their fines are reduced by 50%.

5. If no firm self-reports, a computerised Antitrust Authority may detect the cartel with 20% chance.

6. Firms learn their final earnings, whether the cartel is discovered and the number of reporting firms (though not the individual reporting choice of each firm).

At the end of the experiment the initial endowment plus the number of points earned in every period minus the penalties paid are converted into cash.

3.3.3 Hypothesis

The insights from the law and economics literature and the existing experimental findings offer some predictions that we can examine within our experiment. We will test these hypotheses, described below, against the alternative hypothesis of no difference between our treatments:

Cartel Formation: Ringleader exclusion influences the participation constraints, which loosen for regular cartel member, as their expected fine is decreasing due to less pressure to "race to the courthouse", and tighten for the ringleader. While the overall effect on deterrence is hard to predict, experimental evidence by Bigoni et al. (2012a) finds no difference in the rate of communication attempts, while Hesch (2012) reports that with ringleader exclusion more firms are willing to join talks. As expected fines fall for two of the three firms, our first hypothesis is that the rate

[10] Our aim was to design a simple enough environment to guarantee understanding for our subjects, while also keeping it much in line with the existing E.U. legislation, which embodies an upper boundary of 10% of the sum of total annual turnover to the cartel fine. A 10% fine of firm revenues is used in parts of the literature (Apesteguia et al., 2007; Hinloopen and Soetevent, 2008 and Hesch, 2012), while Bigoni et al., 2012a and Dijkstra et al., 2011 use a lump-sum fine. The 20% detection probability slightly exceeds the estimation of cartel detection rates ($13\% - 17\%$) by Bryant and Eckard (1991). Previous experiments use either 10% (Bigoni et al., 2012a), 15% (Hinloopen and Soetevent, 2008) or different probabilities for each treatment (15% or 75% in Hesch, 2012; 20% probability of investigation and 75% probability of conviction (or the reverse) in Dijsktra et al., 2011). We select 20% in order to ease mental accounting and understanding for our subjects.

of communication attempts and the frequency of cartelised markets are lower in *US* than in *EU*.

Pricing: For prices, both existing experimental studies find higher prices. This is in line with Leslie's (2006) intuition that exclusion makes a suggested price more credible, which should in turns enable firms to collude on higher prices. For this reason, we conjecture that the conventional wisdom will hold and assume that prices are higher in *US* than in *EU.*

Cartel Stability: Leslie (2006) argues that exclusion may stabilise cartels by boosting trust between firms. This is experimentally confirmed by Bigoni et al. (2012a) and Hesch (2012), who find less price deviations (although no behavioural differences on reporting rates are reported). We predict that the frequency of reporting and deviation is lower in *US* than in *EU.*

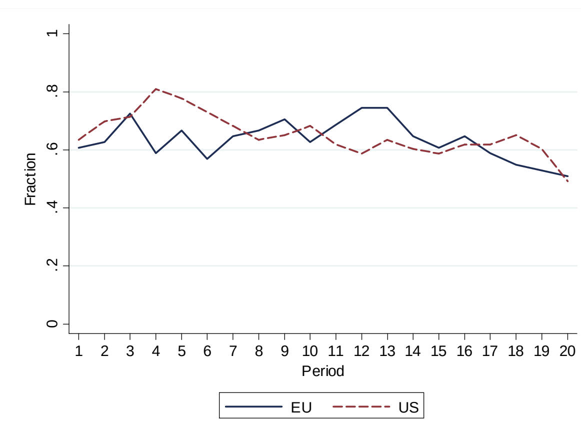
3.4 Results

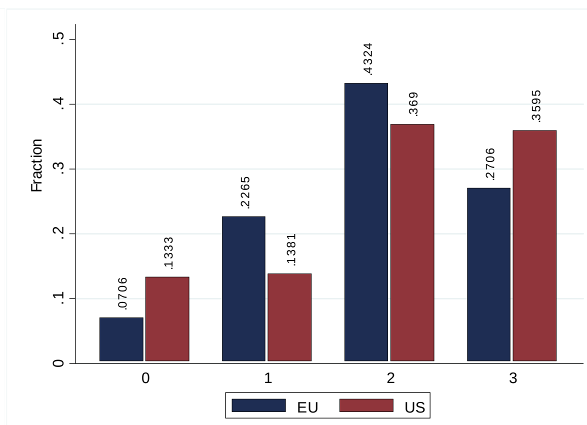
We present the results in three parts. This section begins by evaluating the effectiveness of both policy regimes at deterring cartel formation. The next section assesses the resulting prices, and finally we take a closer look at the stability of the cartel agreement. Unless stated otherwise, all non-parametric tests reported are two-tailed and have been conducted at the group level to control for the non-independence of observations within a market. 51 (63) subjects participated in the EU (US) treatment, generating 17 (21) independent observations.

3.4.1 Cartel Formation

Our data yields information on the success of both policy regimes, *EU* and *US*, in deterring cartel formation. In a first step we analyse the firms' individual intention to form a cartel, which is indicated by the binary decision to communicate in Step 1. The evolution of the fraction of firms willing to do so over time is depicted in the left side of Figure 3.2.

Figure 3.2: Evolution of the fraction of firms (Left) and histogram of the number of firms in a market (Right) who wish to form a cartel.





Throughout the experiment a majority of firms in both treatments decide in favour of collusion. While slightly more firms are willing to discuss prices in the *US* treatment (on average 65.16% as compared to 63.43%), the difference between the treatment is not statistically significant (Mann-Whitney test, $p > 0.10$).[11] Likewise,

[11]Potentially, the decision to attempt collusion is closely connected with experiences gained in the previous periods, in particular the existence (or lack) of a cartel in the preceding period. Con-

firms have no different intentions to communicate at the beginning of the experiment - a proxy of ex-ante deterrence. The overall results indicate that, consistent with Bigoni et al. (2012a), deterrence does not increase when the ringleader is excluded from leniency. This is confirmed in a regression analysis (Table 3.1), which was used to predict a firm's decision to collude by means of a random effects logit model. Random effects are introduced at the market (group) level to account for heterogeneity in group composition. We use a dummy variable representing the treatment as well as time variables and the *Lagged Decision to collude*. Further independent variables are *Lagged Deviation*, a dummy taking value of 1 if the firm experienced a deviation from the agreed-upon price in the previous period, and *Lagged Reporting*, a dummy indicating whether the firm experienced self-reporting in the previous period.

In our notion a cartel is only formed when discussing prices is an unanimous decision by the cartel members. We depict the number of firms per market which agreed to discuss prices in the right side of Figure 3.2. What is interesting in this data is that for both treatments the majority of cases is characterised by less than three firms in a market being willing to collude. In particular, *EU* has the highest fraction of *all-but-one* collusion attempts and the lowest number of unanimous decisions to form a cartel. Of particular interest is the last column, which provides the frequency at which firms achieve consensus to collude. Consistent with Hesch (2012), we find that ringleader exclusion leads to more established cartels (Mann-Whitney test, $p < 0.01$).[12] Further analysis of the evolution of the fraction of formed cartels reveals mild statistical evidence that much of the difference stems from a higher rate of cartel formation in the first 10 periods (Mann-Whitney test, $p = 0.06$) while rates do not differ in the second half of the experiment (Mann-Whitney test, $p > 0.10$).

In the next step we investigate the extend to which firms repeatedly form cartels, which indicates the strength of a policy to deter recidivism. Across all markets, in about one quarter of the cases a cartel was formed in the first period, followed by further collusion in later periods. The persistency with which firms attempt collusion is illustrated in the left side of Figure 3.3, which provides the cumulative distribution function (cdf) of the number of times a firm wanted to discuss prices.

sequently, we distinguish between observations in which firms competed / colluded in the previous period. In markets in which no cartel existed in the previous period, firms show a 10% higher willingness to form a cartel (Mann-Whitney test, $p = 0.06$). However, there is no statistically significant difference between the two treatments.

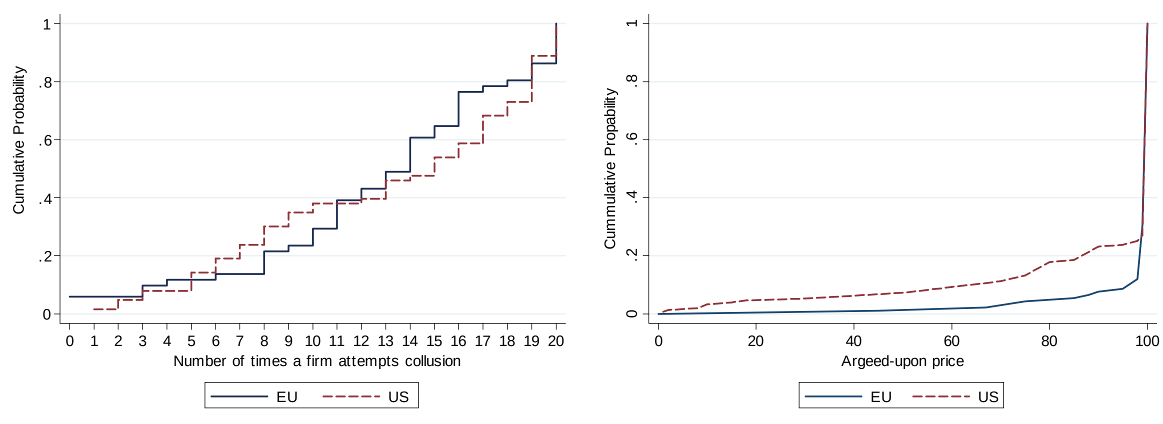
[12] Note that the histogram is using group data from all periods, whiile we use the per group 20-period averages to compute the Mann-Whitney test statistic.

Table 3.1: Random effects logistic regression on the willingness to collude.

Dependent Variable: Willingness to collude	Coefficient (Std. Error)
Constant	0.149
	(0.280)
US	0.0598
	(0.308)
Lagged Decision to collude	2.857***
	(0.141)
Period	-0.0881**
	(0.0395)
Period^2	0.00145
	(0.00185)
Lagged Deviation	-0.630**
	(0.287)
Lagged Leniency	-0.977***
	(0.282)
Observations	2280

Note: This table presents the estimated coefficients of a random effects logit model where the dependent variable is a firm's decision to join a price negotiation. Random effects are introduced at the market level. Firms without ringleader exclusion are used as the benchmark represented by the constant term.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 3.3: Cumulative distribution function of the number of times a firm attempted collusion (Left) and of the agreed-upon price (Right).



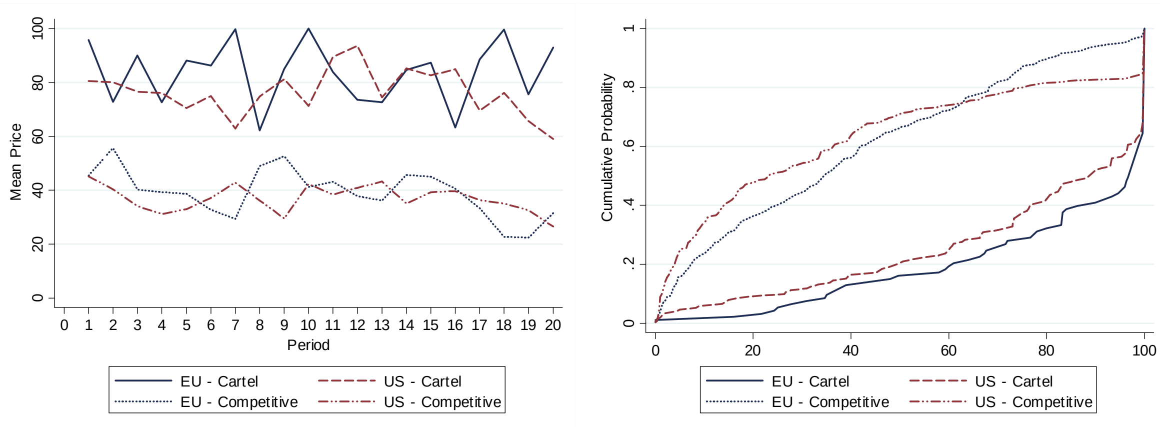
On average firms attempt to collude around 13 times in both treatments, but the higher rate of formation results in more cartel participation: under the *US* regime the average firm participates 7.19 times rather than 5.41 in a cartel.

The question remains on which price firms settle during the price negotiation stage. The cdf of the agreed-upon price is shown on the right side of Figure 3.3. Of the markets who formed a cartel, just over 70% agreed to charge the monopoly price of 100. In one incident, firms agreed to set a joint cartel price of 1 to reverse from a zero punishment phase in the earlier periods. In case the ringleader is excluded, the average agreed-upon price is about 9 points lower than in the EU treatment. However, no statistically significant difference in the outcome of the price discussion stage was found (Mann-Whitney test, $p > 0.10$).

3.4.2 Prices

In this section we consider the impact of ringleader exclusion on prices, focusing first on the *asking price*, which is the average of the three prices in a market. To analyse the development of the asking price, consider the left side of Figure 3.4 which plots the average asking price for rounds with and without cartels. From the graph we can see that there is no noticeable time trend in the data, and for a majority of the rounds both prices in the *EU* treatment were higher than in the *US* treatment. This finding is confirmed in the right side of Figure 3.4, which looks at the cdf of asking prices. The cdf of *US-Cartel* first-order stochastically dominates the cdf of *EU–Cartel*, which indicates that collusive prices may be lower if the ringleader is ineligible for leniency discounts.

Figure 3.4: Evolution of asking prices (Left) and cumulative distribution function (Right).



Further analysis showed that the average asking price in the *EU* (*US*) treatment is at 50.67 (51.10), with a median price of 41.61 (45.76). Further, prices within a cartel are on average 50% higher than in competing markets, and are 8 points higher in EU than US. Turning to statistical tests the difference proves statistically insignificant (Mann-Whitney test, $p > 0.10$). The price chosen by a firm was subjected to a random effects tobit regression analysis, whose outcome is presented in Table 3.2. The dependent variable is the firm's price in a given period. As before, we introduce random effects at the level of markets. Explanatory variables include three dummy variables indicating whether a firm is in a cartel or competing in the *EU* or *US* treatment, and a lagged regressor indicating the lowest price of the previous period. To control for the experience in a cartel during the previous period, we used *Lagged Leniency,* a dummy of value 1 if the firm experienced a leniency application in the previous period, and *Lagged Cheated Upon*, a dummy variable equal to 1 if a firm experienced undercutting of the agreed-upon price in the previous period. The comparative benchmark consists of firms in competitive markets and without ringleader exclusion.

Not surprisingly, firms set significantly higher prices the higher the lowest price in the previous period, and they respond to self-reporting or price deviations by reducing their prices. The coefficients of *EU Cartel* and *US Cartel* are positive and significant, indicating higher prices in cartelised markets. Testing the difference in the coefficients with a χ^2-test reveals that the difference is not statistically significant ($p = 0.35$). Further, in line with both Bigoni et al. (2012a) and Hinloopen and Soetevent (2008), prices tend to decline over time.

Table 3.2: Random effects tobit regression on firms' asking price.

Dependent variable: Asking Price	Coefficient (Std. Error)
Constant	41.60*** (7.117)
EU Cartel	55.15*** (3.208)
US Cartel	45.97*** (9.487)
US Competion	1.960 (9.322)
Lagged Market Price	0.453*** (0.0358)
Lagged Leniency	-15.60*** (2.590)
Lagged Cheated Upon	-8.148** (3.805)
Period	-0.829*** (0.141)
Observations	2280

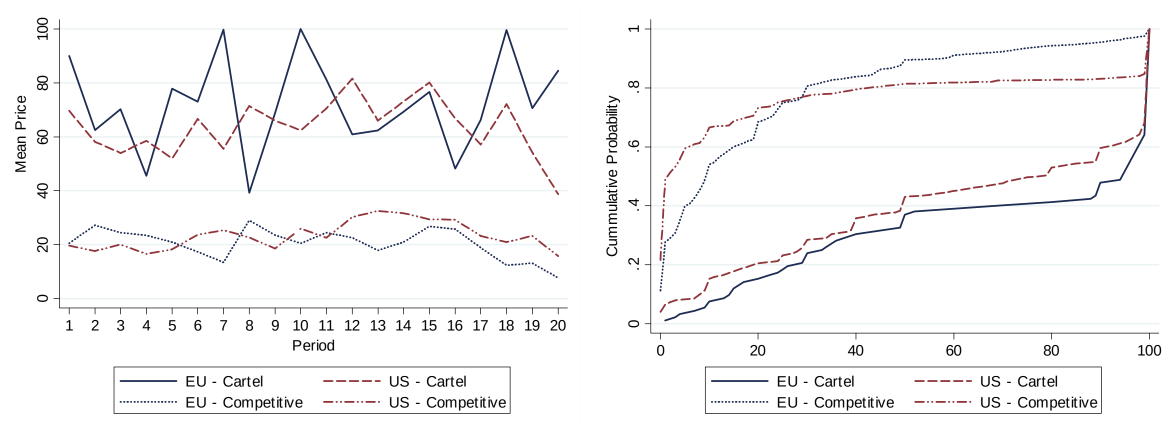
Note: This table presents the estimated coefficients of a random effects tobit model where a firms chosen price is the dependent variable. Random effects are introduced at the market level. Firms in competitive markets and without ringleader exclusion are used as the benchmark represented by the constant term. There were 105 left-censored and 666 right-censored observations.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

In the next step we focus on the *market price* which is the lowest of the three stated prices in a given market and period. Similar to our former discussion, the average market price for collusive and competitive markets is plotted in the left side of Figure 3.5. We note that market prices for markets in which a cartel has been established are more dispersed. The average collusive price is 65.37 (58.13) in EU (US) treatment, but the difference is not statistically significant.[13] In line with our

[13]Three groups in EU and one group in US never established a cartel, leaving us with 14 (20) independent observations.

previous findings, the difference between prices of colluding and competing markets for each treatment is highly significant (Mann-Whitney test, $p < 0.001$). To gain additional insights concerning the distribution of market prices, the right side of Figure 3.5 depicts the cdf for collusive and competing markets. As the figure shows, the majority of competitive (collusive) markets had prices in the lower (upper) end of the price range.

Figure 3.5: Market prices (Left) and cumulative distribution function (Right).



3.4.3 Stability

To investigate the effect of ringleader exclusion on cartel stability we first consider the defection rate, i.e. how often the agreed-upon price is undercut by one or more cartel members. As can be seen in the second column of Table 3.3, a majority of cartels breaks down due to price deviations. However, there is significantly less undercutting if the ringleader can apply for leniency (Mann-Whitney test, $p = 0.09$). A likely reason for this finding is that in *EU* a ringleader can still punish deviation by self-reporting, whereas he cannot do so in *US*.[14] In other words, ringleader exclusion destabilises agreements, as firms can deviate without the fear of punishment.

[14] The individual rate at which ringleader and regular cartel members deviate does not differ significantly.

Table 3.3: Cartel Stability - Average (Std. Dev.) results per treatment.

Treatment	Deviation	Reporting	...given own deviation	... given rival firm deviated
EU	52.17 (50.22)	80.86 (08.71)	42.03 (29.84)	88.52 (32.13)
US	62.91 (48.46)	43.87 (31.89)	25.35 (24.76)	80.35 (40.08)

Note: The rate of deviation is calculated using a dummy of value 1 if at least one firm uncercut the agreed-upon price. The rate of reporting is calculated using a dummy that takes the value 1 if a firm self-reports.

In fact, every cartel in *EU* had at least one firm reporting, while only 51.65% of cartels in *US* got reported, and this difference is highly significant (Mann-Whitney test, $p < 0.001$). However, reporting the fraction of cartels in which at least one firm self-reported provides a biased picture because fewer firms (two instead of three) were able to do so which naturally creates a lower fraction of reported cartels. To control for this potential bias, in the next column in Table 3.3 we therefore report how often a firm, which was able to report, did so. We find that eligible firms in *EU* are almost twice as likely to self-report than eligible firms when the ringleader is excluded (Mann-Whitney test, $p < 0.001$). To analyse what drives this result, we separate observations into rounds in which a firm deviated from the agreed-upon price and then self-reported, and rounds in which a firm sticked to the agreement, and self-reported to punish deviation by others. For the former, we find that if a firm deviated from the collusive agreement, it self-reported in 78.31% of the time in *EU*, but only *43.85%* of the time in *US.* In case other firms deviated from the agreement while a firm sticked to it, that firm self-reported in more than 80% of the cases in both leniency regimes. While this indicated the use of leniency as a punishment device, there is no statistically significant difference between firms (Mann-Whitney test, $p > 0.1$).

3.5 Conclusion

Ringleaders are eligible for leniency in the EU but are excluded in the U.S. The study was designed to determine the likely implications of this asymmetry on cartel formation, prices and collusive stability. In a repeated three-firm Bertrand experiment with the possibility of collusion, subjects were given the opportunity to self-report cartels in exchange for a reduction in antitrust penalties. In one treatment, all subjects were able to report, whereas the ringleader was excluded from reporting in the other treatment. An important modification to previous studies by Bigoni et al. (2012a) and Hesch (2012), who select the excluded firm by means of a quick reaction task or a random draw, is the mechanism by which we implement the selection of the ringleader. In our setting, the leader is the firm whose price suggestion has been accepted as a collusive price by the other firms in a voting procedure. This ensures that the leader is given a meaningful role in easing the cartel coordination problem.

A comparison of the two treatments of this study did not show any significant decrease in the firms' willingness to engage in collusive misconducts when the ringleader is excluded. However, these results need to be interpreted with caution as the intention to discuss prices does not necessarily mean that firms manage to reach consensus and actually form a cartel. With respect to the actual rate of cartel formation, our findings are in agreement with Hesch's (2012) findings which showed that actually more cartels are formed when the leader is not able to obtain leniency.

An interpretation of this result is that the decrease in expected fines for the two regular cartel members weighs stronger than the increase in fines for the leader. Instead of abstaining from leading a cartel, firms coordinate more often on collusion.

Another important finding concerns the effect on prices. This study has been unable to demonstrate that prices increase through ringleader exclusion as in the duopoly setting of Bigoni et al. (2012a).

The third finding is that ringleader exclusion destabilises the collusive agreement, as more firms deviate. A explanation for this might be that firms can deviate without fearing punishment by the leader. However, we do not observe evidence of significant differences in the way leniency is used to punish deviators. In fact, the only difference with respect to the reporting behaviour is that firms who deviated themselves self-report more often if the ringleader can obtain leniency. Further research should be done to investigate the cause of the increase in deviations. Especially on the conditions under which a leader is needed to ease coordination seems an interesting direction for future research.

3.6 Appendix

Instruction

Welcome to this experiment about decision making.

If you have any questions throughout the experiment, raise your hand and wait till the experimenter comes to your desk to answer the question in private.

You will form a group of three with two randomly chosen participants in this room. You will not be told who the two other participants in your group are. Each group is independent from others, and for 20 periods you will engage in the experiment with the same two participants.

In each period you can earn Points. Each Point is worth 3.5 Pence. How many Points you will earn depends on the decisions made by you and the others. You start with an initial endowment of 4 Pound Sterling (GBP). At the end of the experiment, all your Points will be converted into cash. If the experiment has to stop for any reason, or your final earnings are negative, you receive a showup fee of 2 Pound Sterling (GBP).

Generally, the number of points you can win depends on the following:

You will have to select a number between $0, 1, 2, ..., 98, 99, 100$. The participant who chooses the lowest number earns points equal to this number. If more than one participant chooses the lowest number, the points will be divided by the number of participants who also choose the lowest number. The other participants earn zero.

Instructions

Each period consists of the following 6 steps, which are the same in every period and for every participant:

Step 1: Every period starts with the question if you want to communicate with the other participants about which number to set. However, note that communication is prohibited and might lead to a penalty as described later. You can choose to "*Communicate*" or "*Not Communicate*" by selecting the choice and clicking OK. Only if all participants in your group wish to communicate, a communication screen will open (Step 2). If not, the experiment continues with Step 3.

Step 2: You are equally likely to be selected by the computer to make a suggestion which number to set. The other two participants can either "*Accept*" or "*Reject*" the suggestion by clicking on the button. If both participants indicate that they accept, the experiment continues with Step 3. Else, the computer again selects one of you to make a suggestion. This procedure repeats until you agree on a number, or until 2 minutes are over in which case the experiment also continues with Step 3. The participant that suggests a number which is accepted by both members of the group will have a different decision in Stage 5 as explained later.

Step 3: Each participant in your group must choose one number between 0,1,2,...98,99,100.

Step 4: In this step you learn the numbers set by all participants in your group. If you communicated in Step 2, the experiment continues with Step 5. If not, the experiment continues with Step 6.

Step 5: If you communicated in Step 2, and you have not been the participant that suggested the number your group agreed on, you must decide whether or not to report this. You can do so by clicking on the "*Report*" or "*Not Report*" button. If you are the participant that has suggested a number which the other participants agreed on, you will not have the option to report.
In case on or more group members report, each group member has to pay a penalty equal to:
Penalty = 10 % of the number you have chosen.

This penalty will be deducted from the points you earned. However, in case you report your penalty gets reduced as follows:

- If you are the only one to report, you will not pay the penalty but the others will pay the full penalty.
- If you report and the other participant also reports, then your penalty is reduced by 1/2. The remaining participant who suggested a number will pay his full penalty.

Further, if neither you nor your group members report, the computer can detect that you communicated, which leads to the point deduction penalty. The chance that the computer detects your group is 20%.

Step 6: In this step you learn the final amount of points you earned during the game. In case you communicated in Step 2, you further learn whether this was detected and how many participants of your group reported.

End of Experiment:

The experiment ends after 20 periods. You will be paid at the end of the experiment. The number of points you earned in each period minus the penalties you paid will be converted into cash. You start with an initial endowment of 4 Pound Sterling (GBP). If the points you earned minus the penalties you had to pay do not sum to a positive number, you will need to invest your initial endowment. We guarantee a minimum earning of 2 Pound Sterling (GBP). If it sums up to a positive number, you will receive this amount in cash plus the initial endowment.

Chapter 4

Antitrust and the Beckerian Proposition: the Effects of Investigation and Fines on Cartels

> "Whatsoever evil it is possible for man to do for the advancement of his own private and personal interest at the expense of the public interest, that evil, sooner or later, he will do, unless by some means or other, intentional or otherwise, prevented from doing it." Jeremy Bentham (1830)

4.1 Introduction

[1]A central task for antitrust authorities is to disincentivize and punish deliberate infringements of competition law by imposing sanctions on detected wrongdoers. In order to deter firms from engaging in criminal misconduct, a necessary condition is that the expected cost of the illegal activity exceeds the economic gains from participating in the same (cp. Ehrlich, 1973). As the economic gain, in general, lies outside the direct control of antitrust 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. 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 antitrust punishment affect the choice to participate and engage in a (illegal) cartel.

[1]This chapter is based on joint work with my supervisor, Subhasish M. Chowdhury.

One can observe how antitrust authorities in different countries recently experimented with finding an optimal punishment for antitrust 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).[2] An implicit reasoning behind such policy movement might be that the antitrust authorities economise on the cost of enforcement by committing fewer resources to the detection of crime, while aiming to achieve the same deterrence effect through an offsetting increase in the fines levied upon wrongdoers. This relates to the prominent (but hitherto untested within a market context) 'Beckerian Proposition', which, 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.[3]

This implication of the Beckerian proposition for antitrust policy, phrased by Kolm (1973) as "hang offenders with probability zero", is not uncontested. Block and Sidak (1980), for example, argue against draconian sanctions as they may discourage marginal deterrence (Stigler, 1970), lead to inefficient overinvestment in private law enforcement, and, most importantly, may lead to bankruptcy, which is harmful to society.

This study contributes to the ongoing debate on optimal enforcement mechanisms, recently brought to the attention of the general public by The Economist (2012), by exploring the Beckerian proposition with a market experiment.[4] At the heart of this study is 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 antitrust penalties. We vary the probability of detection and the level

[2] 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 lead to higher fines.

[3] The rigorous economic analysis of law enforcement and deterrence begins with Becker (1968) and is extended to risk-averse agents by Polinsky and Shavell (1979). 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).

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

of the antitrust fine in a controlled manner such that the expected fine remains the same. We additionally include two treatments, aiming to reflect leniency programs (Motta and Polo, 2003), in which we allow subjects to self-report the existence of a 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 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 programme, 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. Finally we observe that deviation and reporting rates are independent of high or low fine combinations and similar results are achieved.

Using a market setting is important, as previous research has shown that a change in the experimental frames may (*e.g.*, De Angelo and Charness, 2012; Hoerisch and Strassmair, 2012) or may not (*e.g.*, Friedland, Maital and Rutenberg, 1978; Anderson and Stafford, 2003) provide support for the Beckerian proposition, and thus results can not easily be transferred to the domain of antitrust infringement. Table 4.1 places this study in context with the previous work in this area of literature.

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.

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 0.20 to 0.30. In an earlier version of their paper,[5] 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 0.15 and 0.22.

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 compared 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

[5] 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.

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.

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 experimental work.[6] Hence, conclusions regarding the applicability of the Beckerian Proposition is mixed at the best. As a result, without specific tests no definitive forecast can be made about the validity of the Beckerian Proposition in a market context either. A market differs from the frames employed in previous studies in at least two dimensions. Whereas violating the law is a individual decision in areas such as tax evasion, speeding or stealing, it requires 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 programme') 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.

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.

[6] 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.

Table 4.1: 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)
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

4.2 Experiment

4.2.1 Experimental Procedure

The experiment was conducted at the Centre for Behavioural and Experimental Social Science (CBESS) at the University of East Anglia (UEA). Subjects were 180 UEA students without prior experience in market experiments. 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.[7]

The experiment consisted of two parts. In the first part, run on pen and paper, subjects took part in a risk elicitation task (Holt and Laury, 2002). A computerised 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 risk elicitation task, subjects were provided with both computerised and printed instructions (reproduced in the Appendix) for the second part of the experiment (programmed and conducted using z-Tree (Fischbacher, 2007)). A questionnaire was used to ensure understanding. Finally, after the experiment finished, subjects were asked to fill out a demographics and feedback survey.

For the first part, earnings were denoted in British pound. For the second part, they 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.

4.2.2 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 triopoly as in Dufwenberg and Gneezy (2000). 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

[7]Dal Bo (2005) highlights the importance of a random-stopping rule to reduce opportunistic behavior in strategic games, e.g. prisoner dilemma.

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 antitrust fine, which is levied upon firms by a computerised Antitrust Authority. The novelty of our design comes from the controlled variation of the likelihood of detection 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 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 programmes" offer cartel members the opportunity to report their illegal conduct in exchange for full immunity or a reduction of antitrust penalties. Examining the validity of the Beckerian proposition with and without leniency does also allow us to give policy advice for countries with antitrust enforcement, such as Indonesia or the Philippines, that have not (yet) introduced a leniency policy. 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. 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 (three) firms report, their fine is reduced by half (one-third). Fines in these treatments with leniency are denoted with l and L, respectively.

In Table 4.2 we summarise the treatments:

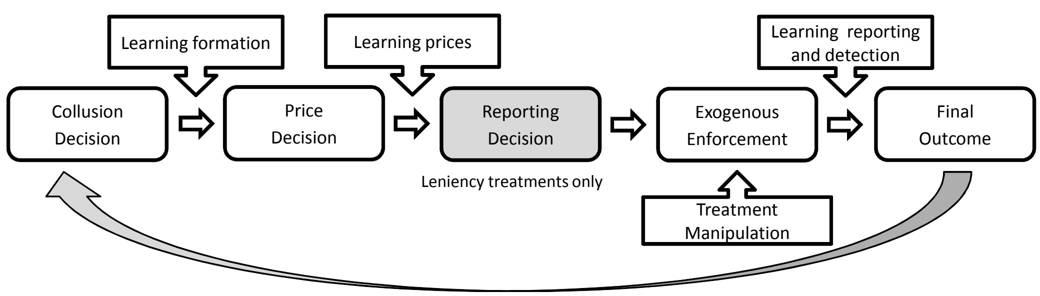
Table 4.2: Classification of treatments.

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 is summarised in Figure 4.1:

Figure 4.1: Game Tree.



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 then enter a non-binding agreement to choose the joint profit maximizing price of 102. If at least one firm decides not to collude, then firms are informed about each rivals' choice and competition takes place in the market.
2. Each firm chooses its price from the set $\{90, 91, \ldots, 102\}$. Firms then observe all prices in their market, and learn whether their price is the lowest submitted price.

3. In the treatments with leniency, each firm can decide to reveal the existence of a cartel at the expense of one experimental point.
4. If no firm self-reports, the cartel may still be detected by the Antitrust Authority with the detection probability specified in the treatment.
5. 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 risk elicitation task and the Bertrand game are then summed up and paid out in private.

4.2.3 Theoretical Framework

Across all treatments the competitive Bertrand equilibrium is to select a price of 91 with 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 antitrust 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%. Several previous market experiments used a rate of 15% (Hinloopen and Soeteven, 2008; Gillet et al., 2011). We select 10% and 20% in order to ease mental accounting and understanding for the 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 Antitrust Authority levies a fine $\digamma$ upon firms. Set $\digamma = \begin{Bmatrix} 4 \text{ if } \rho = 20\% \\ 8 \text{ if } \rho = 10\% \end{Bmatrix}$, 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 important to note that the per-period expected fine $\rho\digamma$ is constant across treatments.

The net present value of the expected fine payments, given an infinite repose, is $\rho\digamma + (1-\rho)\delta\rho\digamma + (1-\rho)^2\delta^2\rho\digamma + ... = \frac{\rho\digamma}{1-\delta(1-\rho)}$. Finally, when collusion is enforced via grim-trigger strategies, a deviating firm 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 equations (1) and (2) consists of the net collusive profit, which is the infinite gain from collusion minus the expected fine payment, whereas the right-hand sides (RHS) are 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:

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

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

4.2.4 Hypotheses

Insights from the Law and Economics literature, existing experimental findings and the theoretical benchmark discussed above offer predictions that we can examine within our experiment. The analysis will focus on three parameters: (i), 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, supported by our theoretical benchmark, 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 as to an increase in the severity of the antitrust fines while keeping the expected fine the same. 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. (ii), we consider the impact on asking and market prices. In the theoretical benchmark, 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 alternative hypothesis is therefore that prices are higher when fines are large. (iii), 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 unilaterally for leniency, independent of the detection rate and fine level. Our null hypothesis is therefore that there will be no difference between the 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 cartelised 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.

4.3 Results

In synchronisation with the hypotheses, results are presented in three parts. Throughout the paper all tests are performed with the entire sample, but restricting the analysis to observations from round 1 to 20 replicates the same 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. Due of the dependency of observations within a market, the average statistic of all three firms constitutes one unit of observation. 36 subjects participated in each treatment; hence we have 59 independent observations.[8]

[8] One observation in pL had to be dropped, as two subjects went bankrupt.

4.3.1 Cartel Formation

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.[9] Our second indicator is the *rate of cartelised markets* - the percentage of markets in which a cartel exists, taking into account that undetected cartels carry over into later periods.

Propensity to Collude

In the first step, we focus on the propensity to collude. Table 4.3 contains the descriptive statistics. Not surprisingly, we note that in comparison with the baseline treatment, all antitrust sanctions effectively deter collusion attempts. Further, the difference in the propensity to collude across treatments in which an enforcement regime is in place is statistically significant (Kruskal-Wallis test, $p < 0.01$).

Table 4.3: Propensity to collude - Average (Std. Dev.) results per treatment.

Probability	Fine	without Leniency	with Leniency
10%	8	50.74 (19.03)	53.84 (10.42)
20%	4	49.74 (16.97)	64.67 (12.33)
Baseline:		76.69 (16.99)	

Note: The propensity to collude is computed using the binary firm decision to attempt collusion or not.

[9] To check for robustness, we also conducted similar analyses using more restrictive notions 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, available upon request, do not differ qualitatively.

In order to get a grip on what drives the observed differences, we focus on the existence (or absence) of a leniency policy.[10] A comparison reveals that the propensity to collude is about 9% higher in the presence of a leniency programme, and this is statistically significant (Mann-Whitney test, $p = 0.04$). This hints at the possible pro-collusive effect of leniency first described in Motta and Polo (2003), according to which firms use self-reporting as a punishment against defectors. 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, $p = 0.03$).

Our subsequent focus is on the second potential driver, i.e., the difference between the detection rates and fines. We pool *pF* and *pL* and compare them with *Pf* and *Pl*. We do not detect any statistically significant differences (Mann-Whitney test, $p > 0.1$), which would suggest that fines and detection rates are indeed substitutable. However, as we showed that 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 4.4 documents the p-values of pairwise two-sided Mann-Whitney comparisons.

Table 4.4: Propensity to collude - p-values of pairwise two-sided 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**

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

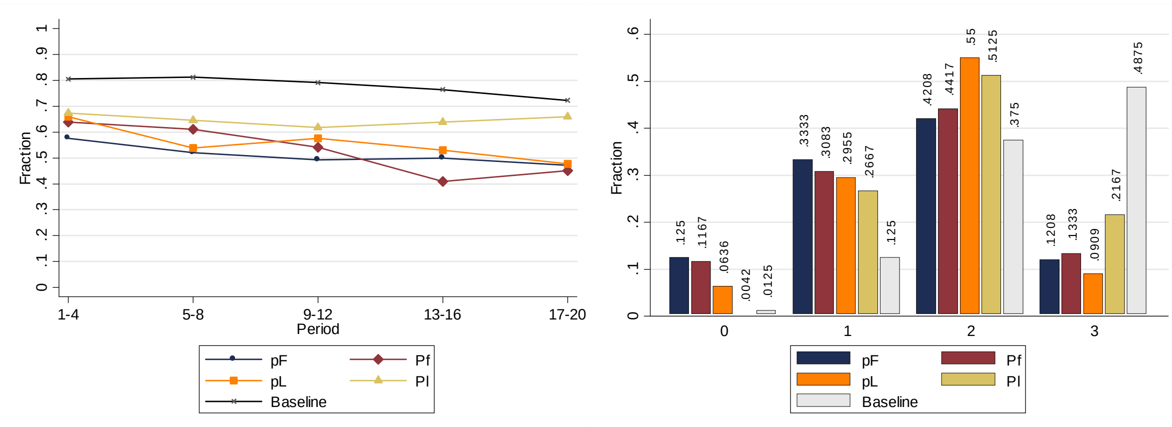
[10] We also investigated the attempt to collude at the very first period of the experiment, which can be seen as a measure of pre-deterrence. There is no significant difference between the enforcement regimes.

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.

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 4.2, in which we have divided the time dimension into four blocks of periods. The right hand side of Figure 4.2 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).[11] 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, indicating that in the presence of leniency, high fines and low detection rates seem most effective in deterring cartels.

[11] 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 4.2: Evolution of the fraction of firms who wish to form a cartel (Left) and Histogram of the number of firms in a market that want to collude (Right).



The analysis so far is, however, not complete as we aggregated the individual decisions 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.[12] 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 4.5 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 programme. 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 antitrust action has a deterrence effect by reducing the odds to collude. $\textit{Cartel formed}_{t-1}$ and $\textit{Cartel detected}_{t-1}$ does not turn significant in the regressions with leniency. The size and sign of the coefficient $\textit{Cartel reported}_{t-1}$ 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]

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

[13] 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.

Table 4.5: Random effects logistic regression on the decision to collude.

	without Leniency (Base: pF)			**with Leniency** (Base: pL)		
	Model 1	Model 2	Model 3	Model 1	Model 2	Model 3
Decision to collude						
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)
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)	
Risky choices		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)
Observations	1710	1710	1710	1524	1524	1524

Standard errors are in parentheses

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

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 (Rabe-Hesketh et al., 2005). GLAMM has previously been used in similar statistical analyses by other researchers, including 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. We thus summarise our findings in the following statement:

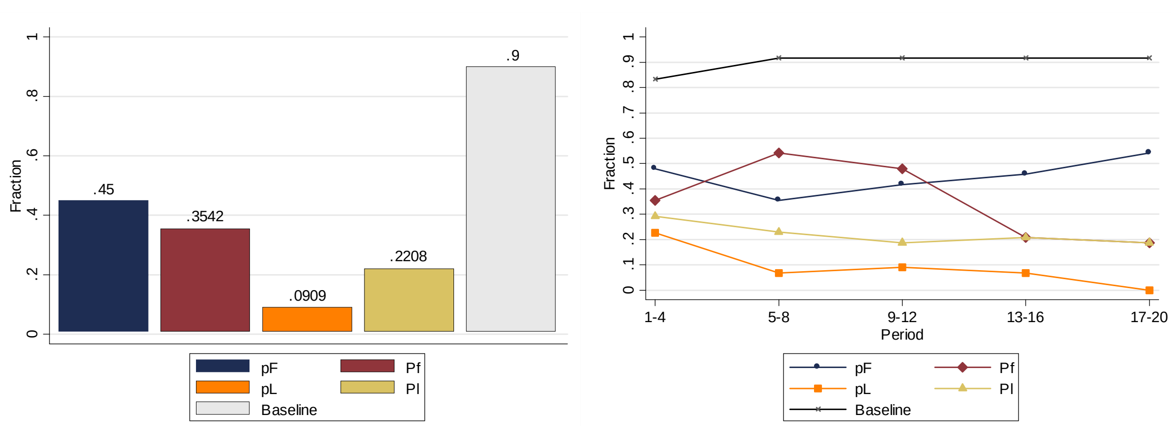
Result 1: Propensity to Collude

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.

Cartelised Markets

Of greater interest than the effects of different policy regimes on collusion attempts is whether any policy regime is more successful in reducing the actual incidence of cartels. We address this question by observing the number of cartelised markets, meaning markets at which a cartel agreement was in place at the price decision stage. The LHS of Figure 4.3 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 4.3: Average fraction of cartelised markets (Left) and the dynamics over all periods (Right).



As can be seen, antitrust regimes differ greatly in the resulting number of cartelised markets. We find that with antitrust enforcement between 8.27% and 44.69% of all markets are cartelised, while in a laissez-faire environment 9 out of 10 markets are collusive. 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 detection 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 cartelised markets across treatments with antitrust enforcement is statistically significant (Kruskal-Wallis test, $p = 0.04$).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 cartelised markets in the presence of leniency (one-sided t-test, $p = 0.04$) which again questions the general validity of the Beckerian proposition.

To complete the analysis, we now turn to a graphical representation of the effects of different policy regimes on the rate of cartelised markets. The RHS of Figure 4.3 depicts the fraction of cartelised markets over time as observed in our data. 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. We can now present our second result:

Result 2: Cartelised 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.

4.3.2 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 Antitrust 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 fines by increasing their prices. In this subsection, we compare the resulting prices (and hence consumer welfare) under each antitrust regime. We ask if higher fines and lower detection probabilities diminish consumer welfare (Hypothesis 2B).

Asking Prices

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. We start to address this by investigating the *asking price*, the average of the three stated prices in a given market in a particular period. Table 4.6 yields the asking prices for all treatments, and distinguishes between the price charged in rounds with and without a cartel.[14] 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. Furthermore, it is not obvious if prices are substantially different given the presence or absence of a leniency policy.

[14] Arguably, subjects will self-select into collusive and competitive markets. A pairwise comparison of asking prices without distinguishing between collusive and competitive markets reveals no statistically significant difference between prices with leniency. Absent leniency, prices are higher if fines are high and detection rates are low.

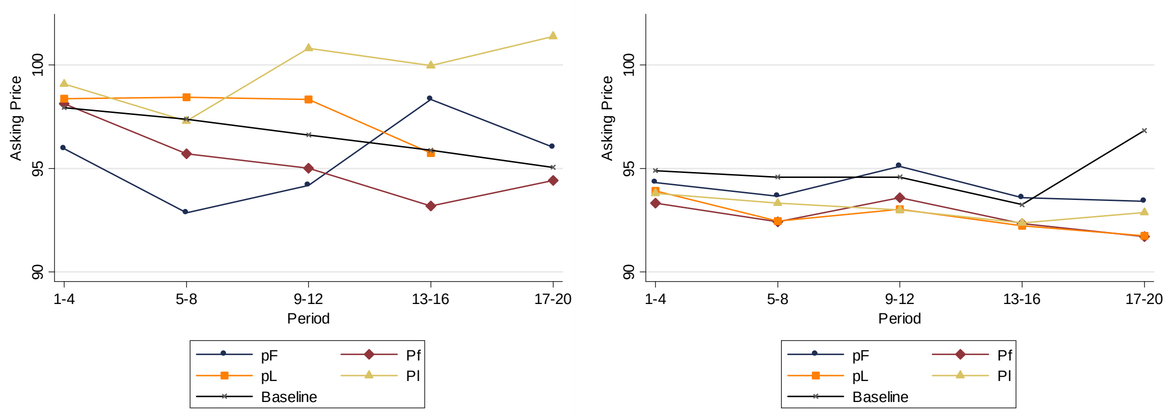
Table 4.6: Average (Std. Dev.) asking price per treatment.

		Collusive		Competitive	
Probability	**Fine**	**without Leniency**	**with Leniency**	**without Leniency**	**with Leniency**
10%	8	95.75 (2.90)	97.89 (2.10)	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)	

Note: Asking prices are calculated using the average of the three stated prices in a market.

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 4.4 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.

Figure 4.4: 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 programme firms undercut the agreed-upon price and self-report. Hence they reason that any punitive price-war will 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 inflate 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 summarise that *there is no statistically significant difference in the asking price across treatments.*

Market Prices

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 4.7 yields the market prices for all treatments, differentiated between the price charged in rounds with and without a cartel. We make the following two observations. First, market prices in collusive markets are about 3 points above the prices in competitive markets. This seems to support the gain from collusion that we identified previously. Second, 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.

Table 4.7: Average (Std. Dev.) market price per treatment.

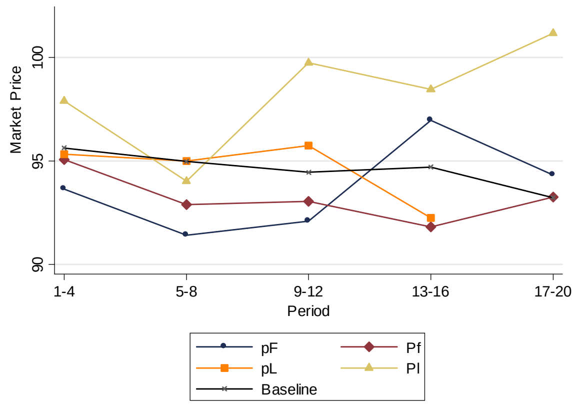
		Collusive		Competitive	
Probability	**Fine**	**without Leniency**	**with Leniency**	**without Leniency**	**with Leniency**
10%	8	93.82 (3.25)	94.61 (3.11)	91.56 (0.90)	91.33 (1.20)
20%	4	93.04 (1.99)	95.47 (4.37)	91.20 (0.32)	91.29 (0.68)
Baseline:		94.32 (2.68)		93.62 (1.25)	

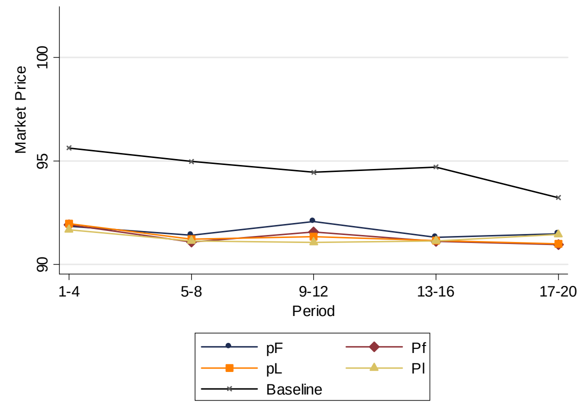
Note: Market prices are calculated using the minimum of the three stated prices in a market.

In a further Kruskal-Wallis test 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 ($p > 0.1$).[15] A pairwise comparison using Mann-Whitney tests in a similar manner as in our previous analysis confirms this. Summarising, we observe no statistically significant evidence that suggest any validation of the claim that policy regimes will influence the severity of the committed crime. The above mentioned regularities becomes easily recognizable in Figure 4.5, which reports the evolution of market prices over time, both for collusive and competitive markets.

[15] This result holds also for a pairwise comparison of market prices without distinguishing between collusive and competitive markets.

Figure 4.5: Market prices for collusive (Left) and competitive markets (Right).





The observed patterns over time broadly support our initial intuition. We can thus present our third result:

Result 3: Prices

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

To assess which policy regime is to be favored from a consumer's point of view, we further investigated the average *consumer welfare*, which is defined as the difference between the maximum willingness to pay of 102 and the actual market price. It is not immediately clear whether or not a leniency programme is welfare improving (Kruskal-Wallis test, $p > 0.1$). In pairwise comparisons of pF and Pf to pL and Pl, we find no significant difference.

4.3.3 Stability

In the final analysis we focus on successfully formed cartels in order to understand how they achieve 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 4.8 provides the average defection rates for each treatment.

Table 4.8: Price Deviation - Average (Std. Dev.) 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)	

Note: 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.

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 utilise the leniency programme to punish deviators with reporting. 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.

Of greater interest is the difference between pF and Pf, and between pL and Pl, which both are 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.

Now focus on the use of the leniency programme 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 antitrust fines.Remember however that self-reporting does not guarantee full immunity from fines. Similar to the design of leniency programmes in the experimental literature so far, a reporting firm may still pay a (reduced) fine if more than one firm reports the cartel. Table 4.9 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 4.9 provides the rate of self-reporting using

[16] Alternatively, we can 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).

only observations where a firm either deviated itself, or experienced a deviation from another cartel member.

Table 4.9: Reporting - Average (Std. Dev.) 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)

Note: The rate of reporting is calculated using a dummy that takes the value 1 if a firm liable for a cartel self-reports.

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 cartelised and that this limits the number of observations that we can draw conclusions from.

To investigate if firms use the leniency programme 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 programme 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 4: Stability

Firms deviate less often in the presence of leniency, and report more often if fines are low. However, rates do not differ significantly between different detection-fine combinations.

4.4 Conclusion

We experimentally examine 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 by antitrust 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 antitrust laws is a coordinated rather than an individual action. Further, enforcement agents can utilise 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 antitrust fines in a repeated Bertrand game with inelastic demand and exogenous antitrust enforcement.

Based on the data retrieved from the experiment, we find that in general fines and detection rates may 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 find effect of different detection-fine combinations in terms of asking and market prices. Finally, we observe that no fine-detection regime is significantly 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 economise 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 or suggested by Germany and the US.

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 programme 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 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. Contrary, for low fines firms are less likely to self-report, which reduced the perceived detection probability and may explain our results.

An interesting direction for future research is whether firms anticipate higher fines, and react by wastefully spending 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. This could be investigated in a similar experimental study, which we leave for future research.

4.5 Appendix

Table 4.10: Random intercept logistic regression on the decision to collude.

Dependent variable: Decision to collude	**without Leniency** (Base: pF)			**with Leniency** (Base: pL)		
	Model 1	Model 2	Model 3	Model 1	Model 2	Model 3
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)
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)	
Risky choicec		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)
Observations	1710	1710	1710	1524	1524	1524

Standard errors in parentheses

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

Instruction

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 computerised 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 computerised 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:

90, 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.

Chapter 5

An Experimental Analysis of Antitrust Enforcement under Avoidance

> "The worse the ill that confronts them, the more men are driven to evade it. The very savagery of a punishment has this effect, and to avoid the penalty for the one crime they have committed, men commit other crimes". Cesare Beccaria (1764) after: Sanchirico (2006, p.1350)

5.1 Introduction

It is well established that firms that deliberately infringe competition law often use a multitude of avoidance procedures to reduce their potential antitrust fines or the likelihood of detection. Examples of such avoidance activities include consulting with antitrust experts to litigate the reduction of any potential fines, destroying or covering up of incriminating evidence (Agisilaou, 2011), lobbying for favourable policy guidelines, and the restructuring of a firm's finance to qualify for an inability-to-pay reduction (Stephan, 2006). Such avoidance activities have particular implications for the design of public enforcement policies, as they affect both the deterrence and the social cost analysis. From a welfare perspective, avoidance expenditure cause an additional cost to firms (Tabbach, 2010) and are socially wasteful as labour and capital is diverted from more productive activities (see Sanchirico (2006) for a discussion of the empirical importance of avoidance costs). Moreover, all else equal, avoidance

[0]This chapter is based on joint work with my supervisor, Subhasish M. Chowdhury.

activities reduce the level of deterrence as expected fines decrease (Ehrlich, 1972). Overall, after accounting for the need to counter this effect by increasing enforcement expenditures as well as for the wasteful avoidance costs, it is not generally possible to say if the standard results obtained without including the possibility for avoidance still remain valid (Malik, 1990).

Besides the prominence of avoidance in case law, so far there has been little discussion about it in the academic literature of antitrust. In particular, to date it is unclear how the possibility of avoidance expenditures may affect the choice to participate and engage in a (illegal) cartel. The purpose of this study is to gain insights into firms' avoidance activities in a controlled laboratory environment. A key advantage of using experimental methodology is the lack of the sample-selection bias inherent to any empirical study on antitrust infringements, which by their nature need to rely on the sample of detected and successfully prosecuted cartels.[1] The laboratory avoids this limitation and offers full control and transparency over all aspects of the firms' decision, in particular over the arguably in the real world often unobservable avoidance activities.

Central to this paper is a three player Bertrand game with inelastic demand and constant marginal cost (Dufwenberg and Gneezy, 2000). Firms have the possibility to collude, however, by doing so they expose themselves to the risk of antitrust penalties. The innovation in the design is to add an avoidance option, in which we allow firms to reduce their potential fine. We then compare the resulting market outcome with a benchmark where firms cannot do so.

As mentioned before, arguably many avoidance activities lower the likelihood of detection rather than the fine. However, allowing for a reduction in the detection rate, which then benefits the whole cartel, may cause free-riding on avoidance activities by other group members. To avoid this additional complexity, while keeping the basic intuition of the real world avoidance activities that occur in markets, we keep detection rates constant and instead lower the fine load.

Further, in two treatments we allow firms to self-report in exchange for a leniency discount, which allows us to address how avoidance influences the effectiveness of leniency programmes. This is of particular interest for countries that have not (yet) introduced a leniency policy, such as Indonesia or the Philippines, as recent research by Innes (2001) suggests that an optimally designed leniency policy can prevent firms

[1] See Jolls et al., 1998 for a discussion of the benefits of behavioural economics in analysing law and economics. For examples see Normann and Hinloopen, 2009.

from spending avoidance costs.

In summary, we investigate whether wrongdoers indeed use resources to avoid sanctions, and how the possibility to avoid influences cartel formation, prices and collusive stability. The findings of our study have implications for the optimal design of antitrust policies, but are also of general interest for academic research that often treated firms as spectators which, apart from using leniency, do not react to the exogenous antitrust regime.

The following conclusions can be drawn from the present study. First, firms are more willing to collude when they have the option to avoid. This effect, however, holds only absent leniency. Furthermore, the increased likelihood to collude translates into a higher rate of cartel formation. Taken together, these results suggest that including a more realistic frame in experimental studies on antitrust enforcement, which allows firms to be more than mere spectators, has significant impact on cartel formation that is intuitive and straightforward. In particular, it indicates that avoidance can be used as a form of insurance against antitrust fines, which may cause more risk-averse firms to collude. This hypothesis is confirmed by a regression analysis, which reveals that a firm's decision to opt for avoidance expenditures is driven by its risk attitude as well as by its past experience of antitrust enforcement. Supporting the intuition in Jensen et al. (2012), we find that avoiding firms charge higher prices. Further, while in general the possibility to use avoidance reduces the rate of price deviations, we observe that firms that engage in avoidance deviate more than twice as often when a leniency programme exists. Additionally, there is evidence that some firms utilise avoidance as an alternative means to avoid being punished for price deviations by other self-reporters.

While experimental research to date has abstracted away from the implications of avoidance activities on law enforcement, a limited body of related theoretical work exists. The seminal work by Malik (1990) debates the trade-off between the detection rate and the magnitude of punishment if criminals spend resources to avoid detection. Likewise, Langlais (2008) discusses how an increase in the magnitude of fines has an indirect adverse effect by providing incentives to avoid detection which decreases the probability of getting caught. Langlais finds that which effect dominates depends on the sensitivity of avoidance activities with respect to fine level and detection rate. Nussim and Tabbach (2009) arrive at the same result, and argue that subsidizing legal alternatives rather than punishing crime can help to reduce crime and wasteful avoidance. If punishment is costly (e.g. the cost of running a prison), Tabbach (2010) shows that from a welfare perspective offenders should be encouraged to avoid punishment, as costly avoidance can be a substitute for costly public sanctions. However, a common limitation of these theoretical models is that the severity of crime and the degree of avoidance are not endogenised. Most recently Jensen et al. (2012) show that if it is possible for the firms to expend resources to reduce the likelihood of getting caught, then it is more profitable to commit a more severe crime. To the extent of our knowledge, we are the first to address avoidance from a behavioral perspective, and our results indicate that many of the above mentioned theoretical predictions are confirmed in an experimental setting, while a few critical results turn out to be different in the data.

The remainder of this paper is organized as follows. Section 2 describes the experimental design and procedure. Results are provided in Section 3, and Section 4 concludes.

5.2 Experiment

5.2.1 Experimental Procedure

The experiment was conducted at the Centre for Behavioural and Experimental Social Science (CBESS) at the University of East Anglia (UEA), and a total of 141 students from various backgrounds and nationalities participated.

The first part of the experiment, run on pen and paper, employed the design by Holt and Laury (2002) to measure individual risk preferences. Subjects then received instructions (see the Appendix for copies of the instructions) for the second part of the experiment, which was fully computerised using the experimental software z-tree (Fischbacher, 2007). Following this, subjects completed a questionnaire, and could ask further questions to the experimental supervisor, which were answered privately. Finally, after the experiment finished, subjects were asked to fill out a demographics and feedback survey.

The earnings for both parts of the experiment were computed at the end of each session and made privately and in cash. 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 £9.35, including an initial endowment of £6 to cover potential losses. Sessions lasted between 45 and 60 minutes.

5.2.2 Experimental Design

In the experiment, subjects were told that they represent a firm, operating in a market with two other competitors.[2] The market was characterised by one consumer with inelastic demand, and firms experienced a constant cost of production of 90. As in Dufwenberg and Gneezy (2000), the firm charging the lowest price earned its price, whereas other firms earned nothing. In case of ties, the profit of (price-90) was shared. The experiment was implemented as a repeated game with fixed matching, and subjects participated for at least 20 periods. For each period after the 20th period, a random stopping rule with 1/5 chance was employed.

In total the experiment consisted of four treatments (Table 5.1). We employed a 2x2-factorial design, in which we manipulated the availability of avoidance acts vs. the availability to obtain a leniency discount.[3] While the former allows us to investigate how the option to avoid influences market outcomes, the latter is of particular interest as research suggest that an optimally designed leniency policy can reduce the likelihood of such avoidance activities (Innes, 2003).

Table 5.1: Classification of treatments.

	without Avoidance	**with Avoidance**
without Leniency	Fine	Fine and Avoidance
with Leniency	Leniency	Leniency and Avoidance

At the beginning of each period, firms had to simultaneously decide if they want to mutually agree to charge the highest possible price and share the market. Only if all three firms in a market agreed on price-fixing, they were informed about their non-binding agreement. Such an agreement could be detected by a computerised Antitrust Authority with 10% chance, which would then levy a fine of 8 points upon the firms.[4] Firms were liable for the illegal agreement until the agreement has been detected (or revealed by means of a leniency application), so they could get fined even in periods in which previous collusion took place and was not detected.

[2] Using three firms is important as arguably a duopoly is inherently more collusive (Huck et al., 2004).

[3] The treatments without the option to spend avoidance expenditures are in line with the study of Chowdhury and Wandschneider (2013), which is similar to Gillet et al. (2011).

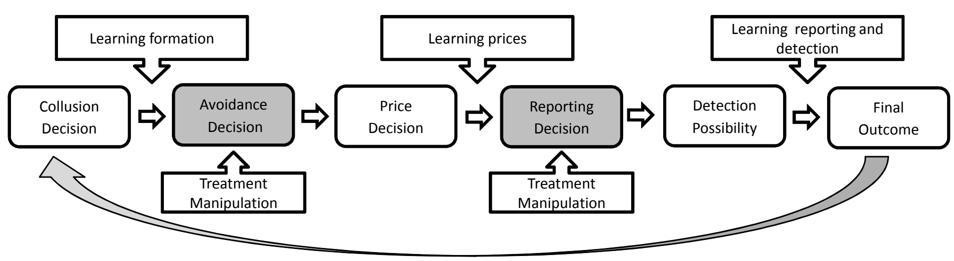
[4] Note that the cartel detection rate of 10% is slightly below the empirical estimation by Bryant and Eckard (1991) of 13%-17%, and that the fine of 8 points represents twice the collusive gain from being in a cartel.

In two treatments there was a decision stage in which firms were informed about the possibility to reduce their potential fine by half at the expense of paying 1 point. The cost of avoidance was implemented in order to prevent firms to reduce their potential fines for free, and to reflect the costs, such as legal cost or consultancy fees, of the real-world avoidance activities. Note that avoidance inevitably lowered expected punishment – and following the rational choice of crime (Ehrlich, 1973) this might affect deterrence. The reduction in the potential fine was also valid in future rounds, but its effect stopped once the agreement got detected or reported. After that, any new agreement was fined the full fine, unless an individual firm paid again 1 point to reduce its potential individual fine.

Firms then selected a price p from the discrete choice set $\{90, 91, ..101, 102\}$. and learned the market outcome. In two treatments firms could decide to reveal the existence of a cartel at the expense of one experimental point (cp. Hinloopen and Soetevent, 2008). If a firm was the sole whistleblower, it received a full fine reduction. In case two (three) firms self-report, their fine was reduced by half (one-third). In the last stage, firms were informed about their final earnings, whether collusion was detected or not, and the number (but not identity) of any whistleblowers. In case their agreement has not been detected, they were further informed whether or not they had already engaged in avoidance activities to reduce their potential future fine.

The timing and information structure of the game is summarised in Figure 5.1:

Figure 5.1: Game Tree.



1. Each firm is asked whether or not it wishes to attempt collusion. In case of a unanimous wish to collude, firms proceed with a non-binding agreement to set the joint profit maximizing price of 102. If at least one firm decides against collusion, firms are informed about each rival's choice and competition takes place in the market.
2. *In the treatments with avoidance*, each firm can decide to reduce its future

potential fine by 50% at the expense of one point.

3. Each firm sets a price from the set $\{90, 91, \ldots, 101, 102\}$. Firms then learn their competitors' price choice, and whether their price is the lowest of the three submitted prices.

4. *In the treatments with leniency*, each firm has the opportunity to reveal the existence of a cartel at the expense of one experimental point.

5. In the treatments without leniency, or if no firm self-reports, the cartel may be detected by the Antitrust Authority with 10% probability.

6. 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. In case a firm has avoided, it is informed if its potential fine is still reduced in the next period.

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.

5.3 Results

The result section is divided into four parts. The first part deals with the effect of avoidance on cartel formation, while the second part describes how often firms actually use avoidance. Part three assesses the effect on prices and part four the effect on cartel stability. The last part concludes with a discussion of total welfare. Due to the dependency of observations over time and within each market, we conduct non-parametric tests using the aggregated observations of each group. In total we have 47 independent observations from 141 subjects. Unless reported otherwise, all reported test statistics are two-tailed.

5.3.1 Cartel Formation

We start by examining the *propensity to collude*, which refers to the binary choice to attempt cartel formation in Step 1. Behaviorally, we expect that a risk-averse firm which otherwise would not have joined a cartel is now more likely to join, since avoidance acts like a form of insurance.

Table 5.2: Propensity to collude - Average (Std. Dev.) results per treatment.

	without Avoidance	with Avoidance
Fine	50.74 % (19.03)	57.22 % (24.52)
Leniency	53.84 % (10.42)	52.33 % (13.32)

Note: The propensity to collude is computed using the binary decision of a firm to attempt collusion or not.

For each treatment, Table 5.2. reveals the means and standard deviations of the rate at which firms aim to collude. It is apparent that absent leniency, the propensity to collude is about 7 % higher with than without avoidance. Moreover, the average rates in the leniency treatments are about equal. In the next step, we test for significance of the difference between treatments by means of a random intercept panel logistic regression with clustering at the group level to control for the potential dependency of decision by firms within the same market. The dependent variable is the decision to collude or not in Step 1. As independent variables we use treatment dummies, the lagged decision to collude or not from the previous period, and lagged regressors indicating if a firm experienced deviation from an existing cartel (*Lagged Deviation*), got detected (*Lagged Detection*) or reported (*Lagged Reported*). Further, we control for potential time effects (*Period*), risk attitudes (*Risky Choices*) and socio-demographics.

To test if the effects are statistically significant we estimate them separately for *Fine* and *Leniency* by means of a generalized linear latent and mixed model (Rabe-Hesketh et al., 2005). Details of the regression result are presented in Table 5.3.

The regression analysis indeed supports our initial observation. The marginal effect of the *Fine and Avoidance* dummy is significant at the 1% level, while there is no significant effect in the leniency treatment. The regression results further indicate that firms which attempted to form a cartel previously are more inclined to do so in the following period, and are less willing to do so if their previous cartel got detected or reported. Finally, we observe mild evidence that the propensity to collude is declining over time.

Result 1:

Given that no leniency programme exists, the willingness to collude is significantly higher if firms are able to avoid.

Table 5.3: Random intercept logistic regression on the decision to collude.

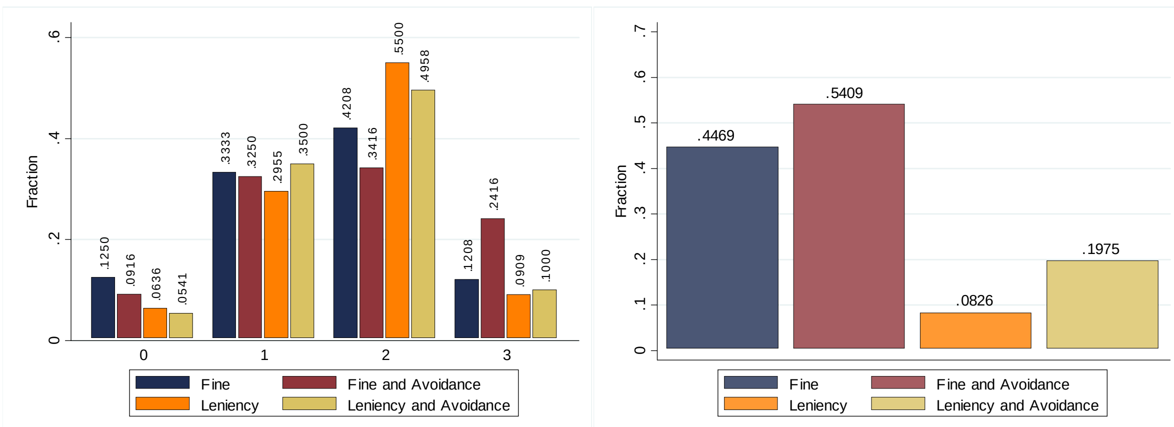
Dependent Variable: Decision to collude	Base: Fine	Base: Leniency
Constant	-1.077*	-0.139
	(0.644)	(0.606)
Fine and Avoidance	0.760***	
	(0.260)	
Leniency and Avoidance		0.268
		(0.315)
Lagged Decision to collude	1.245***	2.103***
	(0.222)	(0.315)
Lagged Cartel Deviation	-0.326	-0.806*
	(0.281)	(0.427)
Lagged Cartel Detected	-0.713*	
	(0.384)	
Lagged Cartel Reported		-0.872**
		(0.356)
Period	-0.0370**	-0.0823***
	(0.0157)	(0.0203)
Risky Choices	0.104	0.00129
	(0.0699)	(0.0560)
Age	-0.275***	-0.112***
	(0.0660)	(0.0282)
Male	-0.0959	1.305***
	(0.305)	(0.272)
British	0.697***	0.0808
	(0.263)	(0.245)
Observations	1620	1575

Standard errors in parentheses

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

A higher likelihood of collusion attempt raises questions about how often a cartel actually forms, which is the second main measure used in the literature to evaluate deterrence (Hinloopen and Soetevent, 2008; Bigoni et al., 2012). In each period, none, one, two or all of the firms in a market could opt for collusion. The distribution of the number of firms that agree to collude is provided in the left hand side of Figure 5.2. As is immediate from the inspection of the right column, which indicates how often all three firms in a market wished to collude, most of the time firms did not manage to reach consensus to establish a cartel.

Figure 5.2: Histogram of the number of firms in a market that want to collude (Left) and of the number of cartelised markets given that cartels carry over (Right).



What is striking though, is how the higher willingness to collude in *Fine and Avoidance* translates into an almost 50% higher rate of cartel formation. We test for statistical differences by a two-tailed Mann-Whitney test and find that the fraction of cartels reaching consensus is significantly different at the 1% level.[5] No similar difference in the Leniency treatments is detected.

Result 2:
Given that no leniency programme exists, firms form significantly more cartels if they are able to avoid.

Finally, taking into account that undetected cartels carry over into the next period, we compare the average fraction of markets that had a collusive agreement in place. As can be seen in the right hand side of Figure 5.2, treatments vary in their effectiveness to reduce the occurrence of cartels (Kruskal-Wallis test, $p = 0.01$). In

[5] We run the non-parametric test using one observation per market and period. Aggregating the data further over time yields a p-value of 0.13.

particular, both leniency treatments prove successful in reducing the occurrence of cartel, with about 35% fewer cartels existing than in the treatments absent leniency. The difference is significant (Mann-Whitney test, $p < 0.01$). While markets have a 10% higher rate of cartelization in the treatments with avoidance, this proves insignificant (Mann-Whitney test, $p > 0.10$). Likewise, a pairwise comparison between *Fine* and *Fine and Avoidance*, and *Leniency* and *Leniency and Avoidance* does not yield significant differences (Mann-Whitney test, $p > 0.10$).

5.3.2 Avoidance

Before proceeding to examine prices, it will be necessary to establish how often firms that are in a cartel actually decide to reduce their liability to fines through avoidance. For this, we observe the outcome of the binary decision to opt for avoidance in each period in which a firm was given the choice to do so. Recall, that once a firm decided to engage in avoidance, the reduction in the fine load carries over into future period until the cartel gets detected. That means a firm which already has reduced its potential fine cannot decide to spent avoidance expenditures again, unless the previous cartel is detected. However, a firm that decides not to spent avoidance resources can decide again in the subsequent periods, as long as the cartel has not yet been discovered.

The average percentage at which firms engage in avoidance is presented in the left column of Table 5.4. It can be seen from the data that firms in *Leniency and Avoidance* opt for avoidance more often than firms in *Fine and Avoidance.* The mean rate of avoidance with leniency is about 12% higher, which suggests that firms use avoidance procedures more often when a leniency programme exists.[6] However, this might be the result of an inherent bias: a firm that decided not to engage in avoidance is asked again in the subsequent periods, conditional on that the cartel still exists. Whether a cartel continues into future periods may well be affected by the existence of a leniency programme. In particular, there may exist longer 'chains' of repeated observations of value 0 from a firm that does not wish to spend avoidance expenditures in the *Fine and Avoidance* treatment. A solution to this potential bias is to consider only the decision in the first period in which a cartel has been formed. The adjusted mean rate is then 63% in *Fine and Avoidance* as compared to 58% in

[6]We can not test the significance of this difference by means of a non-parametric test, as the individual decisions to engage in avoidance or not create an unbalanced panel within and between groups, making the comparison of group level observations unfeasible.

Leniency and Avoidance.

As the reduction in the potential fine carries over into future periods, it is of additional interest to examine how often a firm in a cartel was facing a reduced, rather than the full fine. On average, a firm in a cartel was liable for the reduced rather than the full fine in 66% of the cases in *Fine and Avoidance*, as opposed to 50% in *Leniency and Avoidance.*

Table 5.4: Avoidance - Average results (Std. Dev.) per treatment.

	Firms using avoidance procedures	**Firms facing reduced fine**
Fine	22.47 % (41.85)	66.43 % (47.27)
Leniency	34.73 % (47.86)	50.29 % (50.14)

Notes: The percentage of firms using avoidance procedures is computed using the individual binary decision of a firm to avoid or not. The percentage of firms facing a reduced fine is computed taking into account that fine reductions carry over into future periods.

To better understand the motivational drivers behind the decision to engage in avoidance activities, we employ a random-effects logistic regression on the unbalanced panel. The dependent variable is the binary decision to conduct avoidance activities or not, given that a firm is not already engaging in such activities. We use firms in the *Fine and Avoidance* treatment as our comparative benchmark. Explanatory variables are a treatment dummy (*Leniency and Avoidance*), and the number of times a firms has been fined for being in a cartel in the past. We further control for risk-preferences and demographics. The results, shown in Table 5.5, indicate that there is indeed no statistically significant effect of the existence of a leniency regime on the decision to opt for avoidance. Instead, the decision to avoid is largely driven by having experienced cartel enforcement in the past. Also, the more risk loving a experimental subject is, the less likely the subject will reduce the potential fine. This supports our previous intuition that avoidance acts a an insurance, which in turn allows more risk-averse firms to collude. We conclude:

Result 3:

The decision to avoid is driven by a firms' risk attitude as well as by its experience of antitrust enforcement.

Table 5.5: Random effects logistic regression on the decision to engage in avoidance.

Dependent Variable: Decision to avoid	Coefficient (Std. Error)
Constant	-3.768*
	(2.118)
Leniency and Avoidance	0.299
	(0.632)
# of times detected so far	0.528***
	(0.161)
Risky Choices	-0.195**
	(0.0804)
Age	0.165*
	(0.0946)
Male	-1.130**
	(0.507)
British	0.884**
	(0.444)
Observations	273

Notes: This table presents the estimated coefficients of a random effects logit model where the decision to engage in avoidance is the dependent variable. Random effects are introduced at the market level. Firms in the Fine and Avoidance treatment are used as the benchmark.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

5.3.3 Prices

We now turn to the experimental evidence on prices, where we distinguish between the *asking price*, which is the average of the three prices in a given market, and the *market price*, which is the lowest of the three stated prices. Before turning to a regression analysis, we discuss some key statistics. Table 5.6 displays average asking and market prices, distinguishing further whether a market was competing or colluding when reaching the price decision stage. As can be seen from the top-left part of the table, prices in competing markets are a few points above the Bertrand equilibrium of 91 and vary only slightly between treatments. Further, there exists a clear gain from colluding as average prices are about 4.5 points higher than competitive ones. In line with Bigoni et al. (2012), cartel prices are higher in treatments with leniency. Crucially, collusive prices in treatments with the possibility of avoidance appear higher than without the option to use avoidance.

A similiar picture emerges when we compute the average market price. The results, shown in the lower part of Table 5.6, indicate that prices in competing markets fall almost to marginal cost level, while the average market price in colluding groups is around 95. The summary statistics may lead to the conclusion that average collusive market prices are higher in treatments with than without avoidance procedures.

Table 5.6: Average Price (Std. Dev.) per treatment.

		Competing Markets		**Colluding Markets**	
		without avoidance	with avoidance	without avoidance	with avoidance
Asking	Fine	93.79 (3.97)	92.14 (2.63)	95.75 (4.74)	97.67 (4.80)
	Leniency	92.38 (3.00)	93.29 (3.92)	97.85 (4.29)	100.57 (3.04)
Market	Fine	91.56 (0.90)	91.22 (0.25)	93.82 (3.25)	95.58 (4.22)
	Leniency	91.33 (1.19)	91.93 (1.90)	94.60 (3.10)	96.78 (4.46)

Notes: The asking (market) price is computed using the average (the lowest) of the three stated prices per period and market.

For a more detailed investigation of prices we use a random effects tobit regression with the price choice of an individual firm as the dependent variable (cp. Gillet et al., 2011).[7] We use firms in the *Fine* treatment without a cartel as the comparative benchmark, and introduce random effects at the market level. Independent variables include various treatment dummies, divided between whether a firm is in a collusive or competitive market, as well as a control for potential time effects. Table 5.7 shows the outcome of this regression, which widely confirms the initial remarks.

First, note that being in a cartel leads to a strong increase in the chosen prices across all treatments, which is in line with the increase in prices observed in Table 5.6. While of different magnitude, the coefficients of all treatment dummies for cartels

[7] There were 164 left-censored and 658 right-censored observations.

Table 5.7: Random effects tobit regression on firms' chosen price.

Dependent Variable: Chosen Price		Coefficient (St. Error)
	Constant	94.42***
		(1.000)
Cartel	Fine	2.765***
		(0.438)
Cartel	Fine and Avoidance	5.935***
		(1.419)
Cartel	Leniency	4.648***
		(1.528)
Cartel	Leniency and Avoidance	8.903***
		(1.465)
No Cartel	Fine and Avoidance	-0.156
		(1.423)
No Cartel	Leniency	-1.382
		(1.421)
No Cartel	Leniency and Avoidance	-0.0629
		(1.393)
	Period	-0.0514***
		(0.0115)
Observations		3195

Notes: This table presents the estimated coefficients of a random effects tobit model where the chosen price is the dependent variable. Random effects are introduced at the market level. Firms in the Fine treatment without cartel are used as the benchmark. Independent variables: *Fine*, *Fine and Avoidance*, *Leniency*, *Leniency and Avoidance* = dummy variable equal to 1 if price is set in a given treatment, distinguished between markets with (*Cartel*) and without (*No Cartel*) cartel. *Period* = round number.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

are positive and significant. Pairwise testing the difference in the coefficients with a χ^2-test reveals that when there exists a cartel, the difference between *Fine and Avoidance* and *Leniency and Avoidance* is mildly significant ($p = 0.05$). This further confirms that prices in the *Leniency and Avoidance* treatment are higher than in *Fine and Avoidance*. More importantly, the difference between *Fine* and *Fine and Avoidance* ($p = 0.02$) and between *Leniency* and *Leniency and Avoidance* ($p < 0.01$) are statistically significant, supporting higher collusive prices in the treatments with the option to engage in avoidance.

However, there is a potential inconsistency with this argument. As we discussed earlier, not all firms that are in a cartel actually use avoidance. Hence, prices in the *Fine and Avoidance* and *Leniency and Avoidance* treatment do not truly reflect the price decision of those firm which avoid, but are a combination of prices from two sets of firms: those that engage in avoidance and those who do not.

Further, we have to take into account that a potential fine reduction carries over into later period and might affect the price decision. In order to assess whether the higher prices in *Leniency and Avoidance* than in *Fine and Avoidance* still hold, we split the data from Table 5.5 into observations where a firm in a cartel is facing a fine, and observations where a firm in a cartel is facing the full fine. The corresponding price is shown in Table 5.8. The results indicate that prices between firms that engaged in avoidance and those who did not are about equal in *Leniency and Avoidance*, but are lower in *Fine and Avoidance*. Potentially, this is due to more frequent price deviations, which we will analyse in more detail later on.

Table 5.8: Average Chosen Price (Std. Dev.) per treatment.

	Chosen price if a firm in a cartel is...	
	facing reduced fine	facing full fine
Fine and Avoidance	96.91 (4.89)	99.19 (4.27)
. Leniency and Avoidance	100.55 (3.21)	100.58 (2.88)
Observations	367	227

Finally, we investigate the effect of avoidance on prices by means of a random effects tobit regression analysis in which we again use the firm's price in a given period as the dependent variable. As before, random effects are introduced at the market level. Explanatory variables include two dummies, indicating whether a firm is in a cartel and whether it used avoidance expenditures in the current period. Further regressors are the market price of the previous period, a dummy indicating if the firm experienced a price deviation in the previous period, as well as a control for round effects. The results are presented in Table 5.9.

Not surprisingly, firms set significantly higher prices when they are in a cartel. They also respond to higher prices in the previous period by increasing their current price, and react to price deviations by reducing their price. Of greater interest, however, is that they also increase their price when they have spent avoidance expenditures. This supports the intuition in Jensen et al. (2012). We can therefore conclude:

Result 4:
Firms that engage in avoidance activities charge higher prices.

5.3.4 Stability

For the next stage of the analysis we focus on successfully formed cartels and investigate their stability, which is measured in two ways. First, we explore how often a firm in a cartel undercuts the joint profit maximizing price, and second, we examine how often a firm self-reports its collusive agreement. Note that a firm's avoidance activity is private information, and competitors never knew whether a firm insured itself against potential fines. Also recall that avoidance activities only reduce a firms individual fine, and are not a public good. Given that a firm has paid avoidance expenditures, which carry over into future periods, we would expect it to stick to the collusive agreement in order to gain joint cartel profits rather than one-shot deviation profits.

Table 5.10 indicates the rate of price deviations for each treatment. From this data we can see that indeed fewer firms deviate in the treatments with avoidance, which also matches with the findings of higher market prices in Table 5.6. For the purpose of understanding how the individual rate of defection translates into cartel stability, we compute the fraction of cartels with at least one price defection in a given period. Showing a similar trend as the individual deviation rates, the fraction of cartels with at least one firm deviating is around 86% in *Fine*, and 72% in *Fine and Avoidance*. Likewise, the rates for *Leniency* and *Leniency and Avoidance* are 96% and 63% respectively. While the observed difference is intuitive and large, we find no statistically significant difference between either the rate of price deviations

Table 5.9: Random effects tobit regression on firms' chosen price.

Dependent Variable: Chosen Price	Coefficient (St. Error)
Constant	50.14***
	(3.083)
Firm is in cartel	5.481***
	(0.289)
Firm used avoidance	2.592***
	(0.726)
Lagged Market Price	0.474***
	(0.0331)
Lagged Deviation in earlier cartel	-2.892***
	(0.280)
Period	-0.0307***
	(0.0116)
Observations	3054

Notes: This table presents the estimated coefficients of a random effects tobit model where the chosen price is the dependent variable. Random effects are introduced at the market level. Independent variables are : *Firm is in cartel* = dummy variable taking value 1 if a firm is in a cartel. *Firm used avoidance* = dummy variable taking value 1 if a firm spent avoidance expenditures. *Lagged Market Price* = the market price of the previous period. *Lagged Deviation in earlier cartel* = dummy variable taking value 1 if firm experienced a price deviation in the prevous period. *Period* = round number.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

or the fraction of cartels (Mann-Whitney test, $p > 0.10$). This may be due to a limited number of independent observations, as only 8% (19%) of the markets in *Fine and Avoidance* (*Leniency and Avoidance*) were collusive.

Table 5.10: Price Deviations - Average results (Std. Dev.) per treatment.

	without Avoidance	**with Avoidance**
Fine	69.84 % (27.70)	57.14 % (18.34)
Leniency	50.25 % (34.97)	41.56 % (35.61)

Notes: The rate of deviation (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.

In the next step, the relationship between the decision to avoid and the decision to deviate is tested. Across treatments the mean square contingency coefficient which measures the correlation between the two binary dummy variables avoidance and deviation yields -0.04 and indicates no statistically significant relationship between the two variables. Despite, given that a firm decided to avoid, 22.5% of the firms deviate in *Fine and Avoidance*, as compared to 54.54% in *Leniency and Avoidance*, the difference being significant at the 1% level.

Result 5:
Given that a firm uses avoidance, it deviates more than twice as much when a leniency programme exists.

A likely explanation for this finding is that a firm in the *Leniency and Avoidance* treatment can reduce its potential fines to 4 points by spending avoidance expenditures, and it can further reduce its fine through self-reporting. This might make price deviations more attractive. To conclude the result section, we therefore have a detailed look at the self-reporting decision.

The importance of a firm's avoidance decision for the effectiveness of corporate leniency has recently been discussed in Innes (2001), who argues that under an optimally designed leniency programme self-reporting firms will not spend resources on avoidance. The first column of Table 5.11 shows the average rate of reporting per leniency treatment. We can observe that every second firm self-reports in *Leniency*, while one in three self-reports in *Leniency and Avoidance*. A closer look at the reporting rate reveals how the decision to deviate and the decision to self-report are interlinked. The second column of Table 5.11 reports how many firms that deviated from the collusive agreement also self-reported, while the third column focuses on those firms that experienced deviation by another cartel member, which is a proxy for using leniency as a punishment mechanism. For both rates, the same pattern can be observed: there is a lower rate of self-reporting in the treatment with avoidance. However, as discussed previously, not every firm decided to avoid when it has the opportunity to do so. Hence, drawing conclusions without distinguishing between those firms that do and do not avoid will lead to biased results. In the next step we therefor observe the percentage of firms that self-report, given their avoidance decision.

Given that a firm spent resources on avoidance, 54.54% of the firms self-report as compared to only 13.76% if a firm did not opt for avoidance. This indicates that firms which avoid are also more likely to self-report. A correlation analysis between the decision to avoiding and self-report supports this findings, as the decision to avoid and self-report is weakly positively related, with a correlation coefficient of 0.34.[8] This indicates that in the presence of a leniency programme, some firms use a combination of avoidance, price deviation and self-reporting to cheat on their counterparts.

Overall, we therefore summarise:

Result 6:
Firms that opt for avoidance do tend to self-report more often.

Table 5.11: Reporting - Average results (Std. Dev.) per treatment.

	Reporting	given own deviation	given other deviation
without Avoidance	54.36 % (11.54)	34.12 % (09.31)	53.17 % (11.50)
with Avoidance	36.63 % (35.35)	22.13 % (24.42)	29.68 % (36.54)

Notes: The rate of reporting is calculated using a dummy that takes the value 1 if a firm liable for a cartel self-reports.

[8] As a word of caution, the majority of observations in the *Leniency and Avoidance* treatment are categorized by a firm neither avoiding nor reporting. Only 18 observations exits in which a firm avoided and then reported, limiting the scope for statistical analysis.

5.3.5 Welfare

Finally, let us assess how avoidance influences *total welfare*. Recall that engaging in avoidance and self-reporting were costly activities. As any fine transfer from firms to the Antitrust Authority is welfare neutral, total welfare is defined as the sum of producer surplus, consumer surplus, and the cost of engaging in avoidance and self-reporting.

While *producer surplus* is the difference between the price of the lowest seller and the marginal cost of 90, the average *consumer surplus* is defined as the difference between the maximum willingness to pay of 102 and the actual market price. As was indicated earlier in Table 5.5, consumers are worse off when firms can engage in avoidance, and this hold with and without the presence of a leniency programme.

In terms of total welfare, Table 5.12 provides the total amount spent on avoidance and leniency. Of course, the amount spent on avoidance and self-reporting partly depends on the number of successfully formed cartels as well as the stability of the cartel agreement.

Table 5.12: Social Cost per treatment.

	Leniency	Fine and Avoidance	Leniency and Avoidance
Avoidance Cost		40	33
Self-reporting Cost	32		37
Total:	32	40	70

5.4 Conclusion

This study reports a first attempt to experimentally investigate the effects of antitrust avoidance activities on market outcomes. antitrust practitioners widely acknowledge that firms react to antitrust enforcement by spending costly avoidance efforts in order to reduce their expected fine. Such avoidance expenditures are of particular importance to policy makers, as avoidance costs influence the social cost analysis which underlies the design of optimal enforcement regimes (cp. Malik, 1993). To date, it has not yet been tested how allowing for such activities influences insights from cartel formation experiments.

Using a repeated three firm Bertrand game with the possibility of collusion, we allow firms to reduce their potential antitrust fine by spending avoidance resources. Additionally, we impose two treatments which resemble a corporate leniency programme as an alternative means of reducing ones fine.

We demonstrate that firms are more willing to collude when they are able to avoid, and that allowing for avoidance increases cartel formation. This supports our initial hypothesis that avoidance can be used as a form of insurance against antitrust fines, which may cause more risk-averse firms to collude. This findings is supported by a regression analysis on the firms avoidance decision, which finds that the main drivers behind the decision to avoid are the risk-attitudes and past experiences of antitrust enforcement.

With respect to consumer welfare, we observe that firms that engage in avoidance activities charge higher prices, which confirms the intuition in Jensen et al. (2012). Finally, with respect to collusive stability we observe that while the possibility to avoid in general leads to fewer price deviations, a firm which has decided to avoid deviates more than twice as often when a leniency programme exists. This indicates that in the presence of a leniency programme, some firms utilise avoidance as an alternative means to avoid being punished for price deviations. Supporting this, firms that engage in avoidance procedures also tend to self-report more often.

Overall, the results call for more refined research into the effect of avoidance procedures on the effectiveness of corporate leniency programmes. In particular, it is of interest if an optimally designed leniency policy as in Innes (2001) indeed reduced the avoidance expenditures. In future research, one should also investigate the robustness of this result by allowing for avoidance activities which reduce the detection rate, rather than the fine level, as the additional group dynamics introduced by the free-riding problem may well destabilise the collusive agreement.

5.5 Appendix

Instructions

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 computerised 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 computerised 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 continue for another round.

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:

90, 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 has to be paid. The computer can discover the agreement in two ways. First, the computer can detect it in one out of five cases (a chance of 20%). Second, any of the firms can report the agreement at the expense of 1 point.
If you or either of the other two firms report, the agreement gets detected with certainty. However, your fine gets reduced as follows:

- If you are the only firm to report, you will not pay the fine but the others will pay the full fine.
- If you report and one other firm also reports, then you pay 1/2 of the fine. The remaining firm will pay the full fine.
- If you report and both other firms report, then you will pay 2/3 of the fine.
- If you do not report and at least one other firm report, then you will pay the full fine.

A price agreement remains valid – and can be discovered or reported – as long as it has not been discovered or reported in a previous round. Once this has happened, you will not be fined in the future, unless you make a price agreement again.
Normally the fine is 4 points, but there is a possibility to reduce your fine. After you made a price agreement, or in any later round as long as the agreement has not yet been detected, you can reduce a potential fine to 2 points at the expense of paying 1 point immediately. The reduction in the potential fine will also be valid in future rounds -e.g. if you renew an agreement-, but it stops if the agreement gets detected. After detection, any new agreement will be fined the full 4 points, unless you pay again 1 point to reduce your potential fine.
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 or reported.

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 or reporting cost 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.

Chapter 6

Summary

This thesis consists of four contributions to specific policy issues related to optimal antitrust enforcement. Chapter 2 and 3 contribute to on-going debate on whether cartel ringleaders should be eligible for corporate leniency. Chapter 4 takes a more fundamental look on the functioning of antitrust enforcement by investigating if the magnitude of antitrust fines and the likelihood of having them imposed on firms are indeed substitutable in their deterrence effect. Finally, Chapter 5 discusses antitrust enforcement when firms can engage in costly avoidance efforts to reduce their expected fines.

The case study presented in Chapter 2 is the first scientific investigation of cartel ringleaders. Using a sample of 75 European cartels, we identify the traits and characteristics of 14 ringleader cartels. Several interesting observations are made: First, cartels often had more than one ringleader. Second, the role of ringleaders is very diverse and third, ringleaders were typically the largest cartel members. Chapter 2 further presents a model, analysing the effect of ringleader exclusion on collusive prices. It is shown that under fairly general conditions, prices can be higher when the ringleader is excluded. In particular, this can be the case if antitrust fines depend on individual cartel gains in a nonlinear fashion.

Chapter 3 investigates the likely effects of ringleader exclusion on cartel formation, prices and stability by means of a laboratory experiment. By comparing treatments where the ringleader could (not) self-report a cartel in exchange for a leniency discount, we observe that excluding the ringleader does not provide additional deterrence. While an exclusionary policy destabilised cartels, it also reduces the 'race to the courtroom effect', resulting overall in fewer reported cartels.

In the fourth chapter, we explore the effect of investigations and fines on cartels, and in particular investigate if different combinations of detection rate and cartel fine are substitutes in their deterrence effect. In the absence of a leniency programme, detection rates and fines are indeed substitutable, but when a leniency programme exists, firms are more deterred by low rates of detection and high fines. In particular, such a regime lowers the overall incidence of cartelised markets significantly more than a high detection and low fine regime. Overall, our research suggests that antitrust agencies can indeed economise on enforcement costs and achieve a higher degree of deterrence by reducing the probability of detection and increasing the severity of the fines.

Lastly, Chapter 5 explores the effect of avoidance activities on a cartel by means of a market experiment. It shows that the possibility to avoid may trigger more risk-averse firms to collude, which translates into a higher rate of cartel formation. Supporting the intuition in Jensen et al. (2012), we find that avoiding firms charge higher prices. Further, while in general the possibility to use avoidance reduces the rate of price deviations, we observe that firms that engage in avoidance deviate more than twice as often when a leniency programme exists. Additionally, there is evidence that some firms utilise avoidance as an alternative means to avoid being punished for price deviations by other self-reporters.

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> “Ronald [Coase] said he had gotten tired of antitrust because when the prices went up the judges said it was monopoly, when the prices went down they said it was predatory pricing, and when they stayed the same they said it was tacit collusion.”

Source: William Landes in "The Fire of Truth: A Remembrance of Law and Econ at Chicago", JLE (1981), p193.