Experimental Economics and Policy Design

How to Deter Cartelization, Impede Collusion and Suppress Illegitimate Behavior

Zur Erlangung des akademischen Grades eines
Doktors der Wirtschaftswissenschaften
(Dr. rer. pol.)
von der Fakultät für Wirtschaftswissenschaften des
Karlsruher Institut für Technologie (KIT)

genehmigte

DISSERTATION

von

Dipl.-Oec. Michael Hesch

Karlsruhe, 2012

Referent: Prof. Dr. Siegfried K. Berninghaus
Korreferent: Prof. Dr. Kay Mitusch
Tag der mündlichen Prüfung: 29. Oktober 2012

Acknowledgements

First and foremost, I wish to thank my supervisor, Prof. Dr. Siegfried K. Berninghaus. Ever since I joined his chair at Karlsruhe Institute of Technology (KIT), Siegfried Berninghaus has always supported and encouraged me academically. He provided unique opportunities to improve my skills in Industrial Organization and I was able to research in an excellent working atmosphere in Karlsruhe.

I am particularly grateful to my second supervisor, Prof. Dr. Kay Mitusch, whose advice I benefited greatly from, as well as to Jun.-Prof. Dr. Marten Hillebrand and Prof. Dr. Martin Klarmann for joining the examination board. Furthermore, I am indebted to Prof. Dr. Ulrich Schwalbe, as his lectures on Industrial Organization and competition policy at the University of Hohenheim are the reason for my career in the field of law and economics. I would also like to express my thanks to Prof. Dr. Max Albert for helpful comments and suggestions as well as for the hospitality at his chair at the Justus Liebig University Giessen. On numerous occasions I enjoyed working in the highly productive environment, on and off campus, in Giessen.

Moreover, I thank my current colleagues at the Bundeskartellamt, together with former colleagues at the chair of Professor Berninghaus. I am especially grateful to Andreas Hildenbrand, Eike B. Kroll and Stephan Schosser for helpful and stimulating comments on my work, as well as, figuratively speaking, a shot in the arm when needed. I greatly benefited from intense discussions with Gianni Carlton, Nadja König, Athanasios Saitis, Ben Meyer-Schwickerath and Frederick Wandschneider. A big thank you to those who helped to realize all projects: Nadine Fleischer, Philipp Flösser, Rita Frank, Julia Kaftan, Thorsten Rüger, Ralf Stegmüller, Philip Maximilian Stroisch, Renate Wegner and last but (definitely) not least, Klaus Welle. I also gratefully acknowledge the financial support of the Karlsruhe House of Young

Scientists (KHYS) for research visits to the Directorate General for Competition (European Commission) in Brussels, the University of East Anglia in Norwich and the Research Institute of Industrial Economics in Stockholm.

I owe a special debt of gratitude to my family: to my brother Marco Hesch, whose dedication and effort was always an inspiration to me, and to my parents, Ingrid and Kurt Hesch. Without their unconditional support, none of this would have been possible. Many thanks!

There is no IO without U - *Sesame Street*

Karlsruhe, October 2012

Michael Hesch

Contents

Preface **1**

1 Introduction **2**

2 The Effects of Ringleader Discrimination on Cartel Deterrence and Stability **5**

2.1 Introduction 5
2.2 Related literature 8
2.2.1 (A)symmetries and collusion 8
2.2.2 Leniency experiments 9
2.2.3 Cartel ringleader discrimination 11
2.3 Hypothesis 12
2.4 The experiment 14
2.4.1 Game design 14
2.4.2 Experimental design 17
2.4.3 Experimental procedure 19
2.5 Results 19
2.5.1 Pricing 20
2.5.2 Cartel activity 22
2.5.3 Cartel stability 23
2.5.4 High probability of detection 25
2.5.5 50% reduction of fines 26
2.6 Conclusion 27

3 Regulatory Price Interventions in Retail Gasoline Markets **29**

3.1 Introduction 29

3.2 Industrial and political background on gasoline market price regulation 31
3.2.1 The Austrian rule in Austria 31
3.2.2 The Austrian rule in Germany 34
3.2.3 Price ceiling (Luxembourg) 37
3.2.4 24 hour rule (Western Australia) 39
3.2.5 Price transparency and the margin squeeze issue 40
3.3 Related literature 43
3.3.1 Gasoline markets 43
3.3.2 Price-regulatory mechanisms 44
3.3.3 Price ceilings as focal points 46
3.4 The experiment 47
3.4.1 Game design 47
3.4.2 Treatments 48
3.5 Hypothesis 50
3.6 Experimental procedure 52
3.7 Results 53
3.7.1 Chosen prices and initial price increases 53
3.7.2 Volatility and level of prices 56
3.7.3 Endgame effects and (un)certainty 58
3.8 Conclusion 60

4 Ambiguity, Induced by Strategically Investigating Authorities 62
4.1 Introduction 62
4.2 Game design 65
4.2.1 Stages of the game 66
4.2.2 Formal predictions 67
4.2.3 Influence of strategy profile on p 68
4.3 Hypotheses 69
4.4 The experiment 70
4.4.1 Experimental design 71
4.4.2 Experimental procedure 73
4.5 Results 73
4.5.1 Changing the selection mechanism 75

4.5.2 Raising the degree of ambiguity . . . 75
4.6 Conclusion . . . 77

5 Summary and Conclusion **79**

Bibliography **81**

A Addenda to Chapters 2 and 3 **95**
A.1 Ringleader Experiment (Chapter 2) . . . 95
A.2 Gasoline Experiment (Chapter 3) . . . 96

B Experimental instructions **99**
B.1 Ringleader Experiment (Chapter 2) . . . 99
B.2 Gasoline Experiment (Chapter 3) . . . 103
B.2.1 Payoff-Table . . . 103
B.2.2 Instructions . . . 104
B.2.3 Questionnaire . . . 106
B.2.4 Post-experimental questionnaire . . . 107
B.3 Ambiguity Experiment (Chapter 4) . . . 109
B.3.1 Basic Game . . . 109
B.3.2 Extended Game . . . 110

List of Figures **113**

List of Tables **114**

List of Abbreviations **115**

Preface

First, I would like to point out that the single-authored analysis of the effects of cartel ringleader discrimination in Chapter 2 is published in the *Journal of Advanced Research in Law and Economics, Vol. 3, Issue 1(5), pp. 26-42.* Subsection 2.2 (Related literature), however, is not part of the publication. Further, the results presented in Chapter 3 (effects of a price-regulatory intervention in retail gasoline markets) have partly been published in German as *Berninghaus, S., Hesch, M. & Hildenbrand, A., 2012. Zur Wirkung regulatorischer Preiseingriffe auf dem Tankstellenmarkt. Wirtschaftsdienst 92(1), pp. 46-50.*

Second, the analysis of decision-making under ambiguity in Chapter 4 is in large parts identical to a current research paper written with Siegfried Berninghaus and Stephan Schosser, researcher at the Otto-von-Guericke University Magdeburg. The same goes for the joint work with Siegfried Berninghaus and Andreas Hildenbrand, researcher at the University of Giessen, in Chapter 3. I am thankful for my coauthors' permission to use our joint work as a part of this thesis. For reasons of consistency, I will use first-person plural personal pronouns throughout all analyses.

Further, I am careful to point out my current position at the Bundeskartellamt (German Federal Cartel Office). The views expressed in this thesis are mine alone, and do not necessarily reflect the views of the Bundeskartellamt. All analyses are based on economic experiments conducted at Karlsruhe Institute of Technology (KIT), Germany.

1 Introduction

> "The time for more experiments has come, to explore the imperfection of markets." Kenneth S. Rogoff in Handelsblatt (2012b)

Quite recently, in January 2012, Harvard professor Kenneth Rogoff urged academia to critically review its research methods and to stronger rely on evidence from the field of experimental economics. As "there are problems which cannot be solved mathematically", Rogoff makes the point that economists needed to take into account various aspects from other disciplines in social sciences, such as politics or history (see Handelsblatt (2012b)).

Likewise, and focusing exclusively on the domain of Industrial Organization (IO), Normann and Ruffle (2011) emphasize the advantages of economic experimentation in the introduction to one of the latest special issues of the *International Journal of Industrial Organization* (IJIO, Vol. 29(1)). They report classroom experimentation, theory testing and trials and simulations for decision-making in policy as suitable areas of application in IO. As a general rule, since any difference in the data is solely due to the variation of specific characteristics between the treatments, experiments allow for clear-cut results and distinct causality explanation. Moreover, Normann and Ruffle (2011) ascribe experimental methods to even go beyond confirming or refuting a theory as results often suggest extending or modifying existing theory. By addressing the possibility of observing "behavioral equilibrium selection" in game-theory models with multiple equilibria, the authors argue in favor of economic experimentation under controlled laboratory conditions in IO.

Experimental IO is also increasingly recognized by governmental institutions. The U.S. Federal Communications Commission, for example, conducted an experiment to shed light on the effect of changes in horizontal concentration among cable operators on the flow of programming to consumers (see Bykowsky et al.

(2002)). Further, the Bundeskartellamt (BKartA, German Federal Cartel Office) took into account the results of various oligopoly experiments in its recent decision on the merger case B9-13/10 "Magna/Karmann" (see Bundeskartellamt (2010, pp. 107ff.)). Moreover, numerous competition economists and competition law experts who attended the 2011 Conference on Antitrust Law, hosted by the Bundeskartellamt in Bonn, encouraged research into the effectiveness of price regulatory interventions in retail gasoline markets by means of economic experimentation (see Bundeskartellamt (2011c)).

In this thesis, we pick up on all the advantages mentioned above as we apply experimental economics to three specific policy and regulatory issues which we draw from the field of law and economics, especially from the rich domain of IO. We run a simulation for decision-making in policy in our first study, followed by an experimental analysis of a unique price regulatory mechanism established in Austria. Third, we take into account cognitive psychology as we evaluate the results of our third experiment on decision-making under ambiguity in an IO context.

The chapters of this thesis are organized as follows: We start off in Chapter 2, where we analyze the effects of discriminating cartel ringleaders from leniency application. Ringleaders make significant contributions to enabling illegal collusive agreements to function. According to U.S. legislation, they are excluded from corporate leniency programs. Since 2002, under EU regulations, ringleaders may qualify for a reduction of fines. To date, both antitrust laws treat cartel ringleaders differently. Following a brief introduction of EU and U.S. antitrust law, we analyze cartel agreed-upon prices, resulting market prices, cartel stability and deterrence by evaluating the results of our first experiment.

In Chapter 3, we investigate pricing behavior under the so called Austrian rule. Since the revised Gasoline Price Regulation Act came into effect on January 1, 2010 in Austria, increases in gasoline prices by gas station operators have only been possible once a day at 12 pm (noon). Price reductions, however, are permitted at any time of the day. As the beneficial character of the Austrian Rule was challenged by competition experts and the professional public, we analyze the regulation scheme theoretically and experimentally.

We present our third experiment in Chapter 4, as we analyze decision-making under ambiguity. In economic literature, authorities are primarily represented as a constant probability of detecting offenders. We introduce an explicit role for a strategically acting authority and investigate the impact on misbehavior (corruption, by way of example) in an experiment. We implement treatments with either a random selection mechanism or selection by direct human choice, thus turning a risky situation into an ambiguous one. This setup is then extended by making the risk of being detected depend inversely on the ratio of offenders.

In the final Chapter 5, we summarize the results of all presented analyses in this thesis.

2 The Effects of Ringleader Discrimination on Cartel Deterrence and Stability

2.1 Introduction

By offering immunity from fines to the first cartel defector who continuously co-operates with the authorities, so called leniency programs aim to generate distrust among the participants of illegal collusive agreements. Every undertaking has an incentive to cheat on one's collaborators before anyone else benefits while everyone else is fined. The term "leniency" refers to immunity from fines as well as a reduction in fines that would otherwise have been imposed (European Competition Network, 2006).

The theoretical literature is still ambiguous about the impact of leniency programs. While Chen and Harrington (2007) and Harrington (2008) support the destabilizing effects for the reason mentioned above, Spagnolo (2000) and Motta and Polo (2003) also find stabilizing effects on collusive agreements. In his recent contribution, Miller (2009) addresses this issue and points out that all the models depend on specific parameters that are unknowable theoretically. Miller (2009) himself provides empirical evidence on the basis of reports issued over a 20-year time span. He concludes that leniency programs enhance deterrence and positively affect detection capabilities. Yet what is the perception of practical leniency application? Scott D. Hammond, Deputy Assistant Attorney General at the Antitrust Division of the Department of Justice (DOJ), finds leniency programs to be most effective: "It is, unquestionably, the single greatest investigative tool available to anti-cartel enforcers" (Hammond, 2001). Although antitrust laws in the European Union (EU) and the United States (U.S.) rely on the same principles to generate incentives to defect from an agreement, legislation still differs in some parts. See Feess and Walzl (2005), Aubert et al. (2006) and Wils (2008) for an overview of distinctive features of EU and U.S. leniency programs. In this contribution we pay focused attention to the different treatment of cartel ringleaders. Bos and Wandschneider (2011) discuss specific features that define (and thereby classify an undertaking as) a cartel ringleader. By organizing meetings and being the driving force of the conspiracy, ringleaders have a determining role within the arrangement and thereby provide a platform for the cartel to work. Therefore, they are mentioned exclusively by numerous antitrust laws. According to the U.S. Corporate Leniency Policy, an undertaking may be granted leniency if...

> "...the corporation did not coerce another party to participate in the illegal activity and clearly was not the leader in, or the originator of the activity."[1]

The EU adopted this ringleader exclusion rule when setting up the first Leniency Policy in 1996[2]. However, in contrast to the radical U.S. approach, EU legislation has always recognized the cooperation of ringleaders and allowed for a fine reduction of up to 50%. For example, in the *Pre-Insulated Pipe Cartel*[3] the European Commission identified ABB Asea Brown Boveri Ltd. as the ringleader and main instigator of the cartel (see recital 121) and it was granted a 30% fine reduction (see recitals 173-174). Further, despite its determining role, Interbrew was granted the "full" 50% fine reduction in the *Interbrew and Alken-Maes*[4] case (see recital 357).

Hammond (2002) clarifies that the U.S. exclusion from immunity only applies for *the* single organizer or single ringleader of a conspiracy. Likewise, the wording of the first EU leniency program in 1996 was rather ambiguous. It left room

[1]See U.S. Department of Justice (1993), section A.6.
[2]European Commission (1996).
[3]European Commission, Decision of 21 October 1998 (COMP IV/35.691/E.4 — Pre-insulated pipes, OJ L 24, 30/01/1999, pp. 1-70).
[4]European Commission, Decision of 5 December 2001 (COMP IV/37.614/F3 — Interbrew and Alken-Maes, OJ L 200, 07/08/2003, pp. 1-58).

for interpretation as to who exactly may be granted leniency. Therefore, the European Commission clarified the requirements for leniency applications to create legal certainty in the 2002 Notice[5]. In doing so, the 2002 revision has become increasingly lenient to defectors and even allows cartel ringleaders to participate in the amnesty program (see Stephan (2008) and Billiet and Verma (2009) for discussions and comparisons of the 1996 and 2002 legislation). Provided one is the first one to come forward, the main requirement for qualifying for any immunity is "not to have taken steps to coerce other undertakings to participate in the infringement". As the conditions for immunity and reduction in fines were revised in 2006, we refer to the current legislation as the program is still open for ringleader applications.

> "An undertaking which took steps to coerce other undertakings to join the cartel or to remain in it is not eligible for immunity from fines. It may still qualify for a reduction of fines if it fulfils the relevant requirements and meets all the conditions therefor."[6]

In comparison, both jurisdictions agree not to grant immunity from fines to undertakings that did coerce other participants to join the illegal activity. However, cartel ringleaders are explicitly excluded only in the U.S. Corporate Leniency Policy, but not according to EU legislation. Even within the EU member states, ringleaders are treated differently by the national competition authorities (see Schroeder and Heinz (2006)). Vennström (2010) provides insights into cartel enforcement in Scandinavia. She points out that the national competition authorities of all nordic countries may grant full immunity from fines to cartel ringleaders (also provided they did not coerce other undertakings to join the infringement). In Germany on the other hand, sole ringleaders and coercers alike are explicitly not eligible for immunity from fines. However, according to Notice no. 9/2006 on the immunity from and reduction of fines in cartel cases, a reduction of fines may be granted to a cartel participant who does not meet the authority's conditions for immunity. See Bundeskartellamt (2006) for further details.

[5]See European Commission (2002), section A recital (11) c) and the European Commission statement IP/02/247.
[6]See European Commission (2006a), section II A recital (13).

We abstain from cartels with multiple ringleaders and analyze the impact of ringleader discrimination (exclusion from leniency programs) in an economic experiment using a repeated Bertrand game. Given a low probability of detection through an authority, we find that excluding ringleaders from leniency (asymmetric case) facilitates cartel formation, stabilizes collusion and leads to significantly higher market prices. The opposite holds for a high probability of detection. Here, the results are in line with the symmetric case when every player can apply for leniency.

2.2 Related literature

Only very little is known about the treatment of cartel ringleaders and the effects of ringleader discrimination, not to mention the (near) lack of contributions that provide theoretical and empirical or experimental analyses. The literature provides opinions and results which are rather mixed and argumentative. Our study is the first experimental analysis which deals exclusively with this issue.

In this section, we will first briefly discuss the literature regarding the impact of (a)symmetry on collusion and cooperation in 2.2.1. Then, we will provide an overview of contributions presenting experimental evidence on leniency programs in 2.2.2 before we study the literature on cartel ringleaders to its full extent in 2.2.3.

2.2.1 (A)symmetries and collusion

The economic literature deals with the interaction of symmetry and collusion in various ways. Mason et al. (1992) investigate (a)symmetric duopoly behavior in an economic experiment. They design a cournot duopoly game where asymmetries are characterized by differing costs (expressed by different payoff tables). They point out that "asymmetry is currently regarded as a positive but highly subjective attribute of market structures by the Department of Justice (DOJ)" (Mason et al., 1992, p. 670) as collusion becomes more difficult in increasingly asymmetric markets. Fonseca and Normann (2008) vary the distribution of industry capacity in a price-setting oligopoly experiment with constant demand. They compare symmetric and asymmetric markets and find symmetric markets to yield higher prices. This result is consistent with further experimental studies by Compte et al. (2002) (capacity constraints), Kühn (2004) (differentiated products) and Vasconcelos (2005) (cost asymmetries), as they all agree, that asymmetries hinder collusion.

The heterogeneity of firms in cartels is addressed by Rothschild (1999). Theoretically, he analyzes cartel stability and the use of trigger strategies in a framework with linear demand and n firms, where each firm operates subject to one of n possible cost functions. Rothschild (1999, p. 729) finds that "the effectiveness of the strategy as a deterrent to deviation depends crucially, and in a potentially quite complex way, upon the relative efficiencies of the deviant and the loyalists". However, he also points out that all results “depend to a large extent on the particular form of the demand and cost functions” which are employed. There are also more recent contributions relating to this approach by each implementing either different (1) capacities, (2) marginal costs or (3) products (Baranowska-Prokop (2010), Bos and Harrington (2010) and Paha (2010)).

2.2.2 Leniency experiments

There are several contributions investigating the effectiveness of leniency programs by means of economic experimentation. Apesteguia et al. (2007) use a one-shot Bertrand game and do not implement investigations by an antitrust authority, e.g. a certain probability of detection. Thus, the duopoly cartel can only be detected if one player applies for leniency (as punishment for the deviating collaborator). In case both players apply for leniency, the fine reduction is split between both of them. Apesteguia et al. (2007) find a decrease in prices when a leniency program is in place. However, cartel activity in the leniency treatment does not differ compared to their standard treatment, which was set up to capture antitrust law prior to the introduction of leniency clauses.

Hinloopen and Soetevent (2008) report their results to be complementary to those presented by Apesteguia et al. (2007), as they also play a homogeneous good Bertrand pricing game. Yet, their experiment differs fundamentally in various ways. First, Hinloopen and Soetevent (2008) analyze the results of an experiment with repeated interaction. Second, the authors introduce an antitrust authority. Groups that discuss prices are (successfully) investigated with a probability of 15 %. Third, they extend the stage game introduced in Apesteguia et al. (2007) by one player and capture the order of leniency applications. In this way, they induce a "race to report", as only the first reporting player is granted full immunity from fines. Hinloopen and Soetevent (2008) argue in favor of leniency programs as their results suggest fewer cartels (a deterrent effect) and lower market prices if reporting price discussions is possible.

While Hinloopen and Soetevent (2008) restrict price discussions to electronically exchanging accepted (market-)price ranges, Dijkstra et al. (2012) allow for unrestricted communication in their leniency experiment. Moreover, they offer cartel members the possibility to apply for leniency before or after an investigation has been opened. By implementing leniency treatments called "Profound" and "Superficial", the authors allow for either a small number of profound investigations (probability of investigation: 20%, probability of detection: 75%) or a large number of superficial investigations (probabilities inverted). Like Hinloopen and Soetevent (2008), Dijkstra et al. (2012) find leniency programs hinder cartel formation. Average market prices decrease in their experiment, too. The authors find more instances of reporting if investigations are carried out profoundly.

Bigoni et al. (2009) adopt differentiated goods Bertrand competition in their leniency experiment. The authors investigate the effects of the level of fines in different leniency programs on pricing behavior and cartel deterrence. They also study the outcome in case rewards are given to reporting cartel members. This type of a somewhat courageous leniency program is suggested by one of the coauthors in Spagnolo (2000). Based on the results of the experiment, Bigoni et al. (2009) suggest that antitrust enforcement without leniency programs reduces cartel formation, but increases cartel prices. Rewards successfully provide incentives to self-report, which, according to the authors, seems to be the only welfare enhancing policy. As the experiment was run in Stockholm and Rome, the authors also report on the influence of cultural differences. Bigoni et al. (2009) analyze the effects of excluding cartel ringleaders from leniency application in their ringleader treatment. We discuss the outcome below.

In a recent leniency study, Hamaguchi et al. (2009) force subjects to collude as they analyze leniency programs in a situation where firms have already committed to an agreement. The authors report most groups to quickly terminate collusion when they are given to possibility to report the infringement. They find that distrust increases with group size as larger groups have more difficulties in maintaining collusion. Like Bigoni et al. (2009), the authors find rewards to positively affect instances of reporting and cartel breakdown.

For reasons of completeness, we mention the studies by Hamaguchi et al. (2007) and Hinloopen and Onderstal (2011). Both contributions use a first price auction framework to analyze collusion and leniency in an economic experiment.

2.2.3 Cartel ringleader discrimination

Although each respective approach differs slightly, all experimental studies reviewed above draw a conclusion and basically confirm the effectiveness of leniency programs. Despite the amount of research that was recently conducted, only very little is known about the effects of excluding cartel ringleaders from leniency application. Almost all statements are rather argumentative. Leslie (2006), for example, describes ringleaders to be trustworthy partners in crime. As they have less (or nothing) to gain by reporting the cartel, they are highly unlikely to reveal the agreement to the authorities. If they were, however, eligible for immunity, uncertainty and distrust would rise among other cartel members. This is a positive feature for antitrust authorities as, according to Leslie (2006, p. 480), "fear deters cartelization".

Herre and Rasch (2009) take into account the role of a ringleader in their theory model. Given homogeneous goods and constant marginal costs, the authors first assume cartelists ($n \geq 3$) share the collusive profit equally in a game with an infinite horizon. As one player is endogenously (randomly) chosen to be the ringleader who is excluded from leniency application, players face asymmetric profits. An antitrust authority is in place and reviews the industry with a given probability. On the one hand, the authors find ringleader exclusion sustains cartel stability if the industry is reviewed with a low probability. Thus, they argue that (in this case) ringleaders should be able to participate in the program. On the other hand, if the authority commits to a high review probability, ringleader exclusion decreases cartel sustainability as ringleaders face higher fines (asymmetry) if they are convicted.

In a fully revised version of the aforementioned working paper, the authors pay focused attention to the amount of evidence a ringleader holds in Herre et al. (2012). Under the plausible assumption that a ringleader holds more evidence about the agreement than an ordinary cartel member, Herre et al. (2012) deduce an increased probability of successfully convicting the cartel if the ringleader cooperates with the authorities. On the basis of their theory model, the authors find a deterrent effect of ringleader discrimination when the ringleader's evidence is relatively low. However, ringleader discrimination does not to matter when the ringleader's evidence is relatively high. Then the cartel situation compensates the ringleader in a way which does not give an incentive to blow the whistle.

In their recent contribution, Bos and Wandschneider (2011) provide a survey of identified ringleaders in European cartel cases and analyze the effects of ringleader ineligibility in a theoretical model. They conclude their formal analysis with the indication that under some conditions, disqualifying ringleaders from leniency tends to yield higher prices.

Solely Bigoni et al. (2009) provide experimental evidence on cartel ringleader discrimination. In their ringleader treatment, the player who initiates a price discussion is treated as the ringleader. Compared to their leniency treatment, they do not find lower rates of communication attempts. Moreover, cartel groups present themselves to be rather stable. In addition, cartels successfully charge higher market prices in the ringleader treatment.

2.3 Hypothesis

Despite numerous studies that agree on asymmetries to hinder collusion, we follow the literature (as mentioned above) that is more closely related to cartelization and antitrust policy design. We provide hypotheses regarding the effects of ringleader discrimination on market prices and cartel activity as well as cartel stability.

In the Ringleader Treatment of their leniency experiment, Bigoni et al. (2009) find an insignificant deterrence effect and comparatively higher market prices. Bos and Wandschneider (2011) also conclude their formal analysis with the indication that under some conditions, disqualifying ringleaders from leniency tends to yield higher prices. We take these results into account and formulate two hypotheses:

H1: Market prices are higher in an asymmetric setting than in a symmetric setting.

However,

H2: Cartel formation does not differ between an asymmetric setting and a symmetric setting.

This means that cartels that do form in the asymmetric setting are rather stable and successfully realize higher market prices. Indeed, the literature suggests that cartels are more stable when excluding ringleaders from leniency application. For example, Bigoni et al. (2009) provide experimental evidence for cartel stabilization. Ringleader eligibility is also discussed by Leslie (2006). He explains that the "success" of the cartel lies in the trustworthiness of the player who is not able to report. Vice versa, he argues that antitrust enforcement would benefit from another player that might expose the cartel. Non-discriminatory policies increase distrust, and thus, instances of reporting. With regard to cartel stability we hypothesize:

H3: There is more deviation from agreed-upon prices in a symmetric setting than in an asymmetric setting.

H4: There are more instances of reporting in a symmetric setting than in an asymmetric setting.

In the cartel experiment conducted by Hinloopen and Soetevent (2008), non-binding price discussions (cartel situations) lead to higher market prices than in situations when players do not discuss prices (non-cartel situations, competition). No matter the (a)symmetry, we expect to obtain the same results and therefore formulate the hypothesis:

H5: Groups that do have non-binding price discussions realize higher market prices than groups that do not discuss prices.

Non-discriminatory leniency policies allow for one more player to report, defect and reveal the cartel. According to Herre and Rasch (2009), this increases the (otherwise relatively low) probability that an antitrust authority prosecutes a cartel (see Hypothesis H4). They postulate, that this reasoning (recognizing cartel ringleaders for leniency application) may not hold if the authority (already) audits the industry with a high probability of detection. The asymmetries regarding different profits then (negatively) outweigh the positive effect of increased detection capabilities through instances of reporting. We hypothesize:

H6: There is no difference between a symmetric setting with a low probability of detection and an asymmetric setting with a high probability of detection.

2.4 The experiment

2.4.1 Game design

Our design shares the basic structure of the game used in the leniency experiments conducted by Bigoni et al. (2009) and Hinloopen and Soetevent (2008). However, we make some minor modifications which will be discussed below. A repeated Bertrand game (as introduced by Dufwenberg and Gneezy (2000)) is played in groups consisting of three players $i \in \{1, 2, 3\}$. By individually choosing an integer of the choice set $\{101, 102, ..., 110\}$, the group enters Bertrand competition as only the player with the lowest price receives net earnings of

$$\frac{p_{min} - 100}{L} \tag{2.1}$$

where L is the number of players that choose the same (lowest) price at the same time. The deduction of 100 can be interpreted as a constant cost of production. p_{min} is considered to be the market price. Any price $p > p_{min}$ will not yield any earnings. $p^N = 101$ is the Nash equilibrium of the one-shot game. We allow non-binding price discussions before market price submissions. While every price $p > p^N$

is considered to be noncompetitive, we rely on a price discussion to take place in order to form a cartel. Spagnolo (2004) and Aubert et al. (2006) describe the antitrust authority's need for hard evidence to charge undertakings for cartelization. Apesteguia et al. (2007) and Hinloopen and Soetevent (2008) apply this notion in their cartel experiments, as do we.

In more detail, the game can be divided into three stages, namely (1) communication phase, (2) market phase, and (3) reporting phase. This timing of the stage game is widely accepted in the theoretical literature on cartel formation (Spagnolo, 2004). In a computerized experiment, participants play this game repeatedly. One period is structured as follows:

Stage 1 (communication phase): All group members simultaneously decide whether to join price discussions, or not. If at least one player does not participate, the group faces standard Bertrand competition and all players simultaneously set (market) prices without a possibility of reporting or the risk of being audited by an authority. Hence, a cartel is established if all three players agree to participate in the price discussion. Thereby, we do not allow the formation of partial cartels. The decision to participate is private information. Players only learn if a cartel is established, or not. If there is unanimous agreement to join talks, a communication screen opens which allows for price discussions. All players simultaneously choose a price range from the choice set $\{101, 102, ..., 110\}$. Individually, they enter the minimum and maximum price of the price range they accept. As suggested by Haan et al. (2009), players are only able to submit prices (in numbers) and nothing else. The intersection of all three price ranges is instantly shown to all players and they may again enter minimum and maximum prices. If the same number is submitted as both the minimum and maximum value, the price is locked. We allow price discussions until either (1) a single agreed-upon price is reached, (2) one player decides to quit the discussion, or (3) 30 seconds have passed. In both cases (2) and (3), the discussion is terminated without an agreement, the communication screen closes and no further coordination is possible for all players. We declare a cartel to be formed anyway, as coordination was possible and "hard evidence" is created.

Stage 2 (market phase): The agreed-upon price (Stage 1) is non-binding. All players now simultaneously set a price from the choice set $\{101, 102, ..., 110\}$. The lowest of the three prices is considered to be the market price which is then announced to every player.

Stage 3 (reporting phase): If a cartel is established in Stage 1, players have the possibility to report the collusive agreement. Every report costs one ECU (experimental currency unit), regardless of whether leniency is granted or not. This resembles administrative costs, legal fees, et cetera and prevents "free punishment". If reported, all players (except the reporting one) face a reduction of 10% of current period revenues. In case no player reports the participation in price talks, an authority audits the group with a probability of detection of $\delta \in \{0.15, 0.75\}$. The players will also face a reduction of 10% of current period revenues if the audit is successful and the cartel is detected.

Total earnings are updated every period. They are visible at all times. In a final step, all players (individually but simultaneously) learn all relevant information about the period of the game: (1) submitted price, (2) market price, (3) current period revenues, (3) revenue deduction, (4) reporting costs, and (5) current period net earnings.

Compared to the experiment by Hinloopen and Soetevent (2008) we do not pay attention to the sequence of reports after the first report is made. While they grant fine reductions for second and third reports, we only grant fine reductions to the first reporter, where a 100% reduction is immunity from fines. Our approach is sufficient to induce a race to report, figuratively to be the first one in the court room. In addition, we abstain from the possibility of fining subjects for cartelization in previous periods. Hinloopen and Soetevent (2008) allow this notion (the fine liability is carried over to the next period) but cartels do not last longer than one period as they break down by either defection, report or audit in their Leniency Treatment. Bigoni et al. (2009) also study a Ringleader Treatment. The ringleader role is assigned to the subject who first joins a discussion (initiates it). The authors do, however, point out that they eliminate the race to report as only one subject is allowed to report in their duopoly game. Our game is designed for three players and the ringleader role is assigned randomly each period. Like Hinloopen and Soetevent (2008), we use a detection probability of $\delta = 15\%$ which can be ascribed to the empirical analysis by Bryant and Eckard (1991). Although numerous studies estimate the cartel overcharge to be significantly higher than 10% (OECD (2002), Connor and Lande (2005), Veljanovski (2007)), the imposed fine corresponds to current EU legislation[7] which allows a maximum of 10% of the sum of the total turnover. According to the Guidelines Manual of the U.S. Sentencing Commission (USC)[8], the U.S. doubled the 10% to increase deterrence (Connor and Lande, 2005). For an overview of fines loads in EU, U.S. and other countries, see Connor (2010). For reasons of comparability with other experimental studies on cartel deterrence, we fine cartelists $f = 10\%$ of their revenue.

2.4.2 Experimental design

To analyze the effects of ringleader discrimination, we implement a total of four treatments and pick up the analysis where Hinloopen and Soetevent (2008) left off. The structure of their Leniency Treatment resembles our Symmetric Treatment (SYM), where all players may communicate and have the possibility to report the cartel. Also, an antitrust authority is in place, auditing the group with a detection probability of 15%. The fine load upon detection or report is $f = 10\%$ of current period revenues. Only the reporting player receives immunity from fines. In the Asymmetric Treatment (ASYM), only one aspect differs in comparison to SYM. Cartel ringleaders may not report the collusive agreement. In addition we run the AsymmetricHIGH Treatment (ASYMhigh). Like in ASYM, cartel ringleaders are unable to report. If none of the remaining players reports in ASYMhigh, the authority investigates with a high probability of detection (75%). $\delta = 0.75$ corresponds to the probability of detection used by Dijkstra et al. (2012) to simulate profound governmental cartel investigations. The structure of the Asymmetric50 Treatment (ASYM50) is half way between SYM and ASYM. Here, cartel ringleaders may report the cartel. However, they are not granted a full fine reduction as in SYM, but a 50% fine reduction ($f_{50} = 0.5 \times f$), provided the ringleader is the first player to come forward. We present an overview of the treatments in Table 2.1. The fine reductions shown in Table 2.1 apply only for first reporting cartel-ringleaders. All

[7]See European Commission (2006b), recital 32.
[8]See U.S. Sentencing Commission (2010), § 2R1.1(d)(1).

other first reporting players are granted immunity in any case.

	Symmetric	Asymmetric	
Fine reduction	100%	50%	X
Low probability of detection (15%)	SYM	ASYM50	ASYM
High probability of detection (75%)			ASYMhigh

Table 2.1: Classification of treatments

We ran a computerized experiment which was programmed and conducted with the z-Tree application developed by Fischbacher (2007). We used neutrally worded instructions (for a between-subject design) and avoided "hard" terms like antitrust, cartel, fines, et cetera (see Appendix B.1). However, the discussion about prices and the collusive character of the game was completely understandable. Also, the differentiation of the roles was obvious. As mentioned above, our experiment is very closely related to the experiment by Hinloopen and Soetevent (2008). Thus, our leniency program is non-exploitable, too. Per-period cartel profits are higher than per-period profits when applying for leniency. Simplifying $(110 - 100)/3 - \delta \times f \times 110/3 > (110 - 100)/3 - 1 - 0.5 \times f \times 110/3$ reduces the inequality to $(\delta - 0.5) \times f \times 110/3 < 1$, which is always true. The inequality holds for $\delta = 0.15$ in SYM, ASYM or ASYM50 and $\delta = 0.75$ in ASYMhigh, as well as $f = f_{50}$ in ASYM50. Remember that the deduction of one ECU stands for reporting costs.

Because of the possible reduction of current period revenues, subjects can also realize negative earnings: Assume a cartel is established. Now, one player sets her market price ($p_{individual} = 108$) below the agreed-upon cartel price ($p_{cartel} = 109$) and does not apply for leniency (whereas another player reports). She would then earn $(108 - 100) - 0.1 \times 108 = -2.8$ ECU in this period.

With regard to the race to report, participants quickly learn the position of the report button and might already place the courser before the actual report screen is shown. We randomly switch the order of the report and non-report buttons in our computerized design. Hence, subjects still have to make their decision consciously, if not even more consciously.

To avoid possible endgame effects, we end sessions probabilistically. After 20 periods, the session ends with a probability of 20% or another period starts. As for the results, we only analyze the first 20 periods.

2.4.3 Experimental procedure

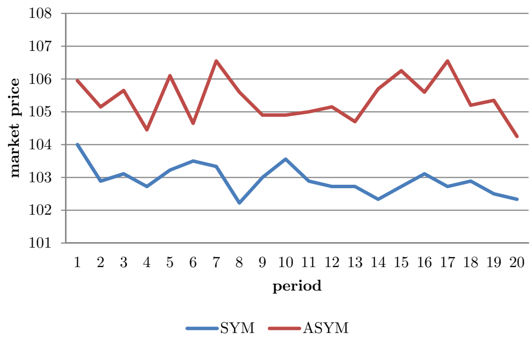
The experiment was conducted at the Laboratory for Experimental Economics at Karlsruhe Institute of Technology (KIT). Subjects were recruited via ORSEE (Greiner, 2004) and a total of 183 students across all fields of study took part in the experiment. There were 61 groups of three participants. We have 18 groups (observations) in SYM, 20 in ASYM, 11 in ASYMhigh and 12 in ASYM50. After welcoming the subjects to the laboratory, participants were randomly assigned to visually isolated computer terminals. The instructions were read aloud and all questions were answered privately. Participants were matched into groups of three and had to play five practice periods before the experiment started. All groups were re-matched afterwards and all participants knew that they would play within the same group until the end of the actual experiment. Earnings of the practicing periods were not added to the cumulative earnings of the experiment. Subjects were paid anonymously at the end of each session. All sessions lasted between 60 and 90 minutes. With a total of €1776.98 earned, average earnings were €9.71 with a minimum payment of €2.01 and a maximum of €23.83, respectively. All payments include a €5.00 show-up fee which was available during the experiment to avert possible losses. Nevertheless, two participants (1.1%) realized negative overall earnings (€1.04 in a SYM session and €1.35 in an ASYM session). Both of them paid the respective amount to the experimenter voluntarily and said they were always aware of the risks of their actions. Therefore, we include those participants in the overall sample of subjects and recognize the outcomes for our analysis.

2.5 Results

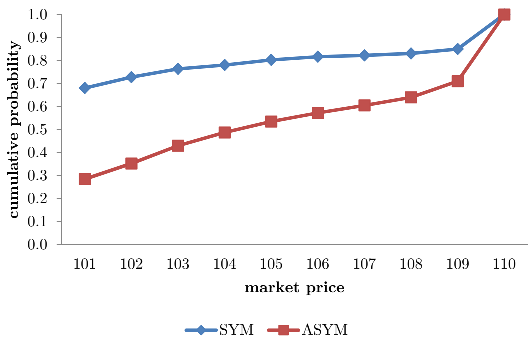
The results are divided in three subsections on (2.5.1) pricing, (2.5.2) cartel activity and (2.5.3) cartel stability. We evaluate Hypotheses H1-H5 by analyzing the data from the SYM- and ASYM-Treatments (18 observations in SYM and 20 observations in ASYM) before we present additional evidence from our ASYMhigh- and ASYM50-Treatment in subsection 2.5.4 and 2.5.5, respectively.

2.5.1 Pricing

We first analyze resulting market prices to evaluate Hypothesis H1. Average market prices per period are shown in Figure 2.1a. While average market prices in all treatments of the experiment by Hinloopen and Soetevent (2008) decrease over time, participants in our experiment succeed in maintaining a certain level (standard deviations are $\sigma = 0.447$ in SYM and $\sigma = 0.668$ in ASYM). Making one player ineligible for immunity from fines in ASYM (and thereby maximizing the asymmetry) appears to result in higher average market prices. The non-parametric Mann-Whitney U (MWU) test confirms that average market prices in SYM are significantly lower than average market prices in the ASYM Treatment ($p < 0.01$).



(a) Average market price per period



(b) Cumulative distribution function of market prices

Figure 2.1: Market prices

Figure 2.1b appends to this finding as we have a look at the plot of the cumulative distribution function (cdf) of market prices. We consider the distributions of market prices in the SYM and ASYM Treatments, characterized by the cdfs F_{sym} and F_{asym}, respectively. The stochastic dominance between the cdfs holds if $F_{sym}(y) \geq F_{asym}(y)$, for any market price $y \in \{101, 102, ..., 110\}$. Obviously, the cdf of market prices in the ASYM Treatment holds first-order stochastic dominance over the cdf of market prices in the SYM-Treatment. In Figure 2.1b we find the cdf of the

ASYM Treatment always below the cdf of the SYM-Treatment. This means that prices are higher in ASYM. We test for first-order stochastic dominance and take as independent observations the average market prices over all 20 periods per group. A one-sided MWU test then confirms the assessment (one-sided, $p < 0.01$).

To analyze the possible success of cartelization (Hypothesis H5), Figure 2.2 illustrates the cdfs of market prices divided into cartel and non-cartel groups. As expected, cdfs of market prices of cartel groups (CART-SYM and CART-ASYM) have unambiguous, first-order stochastic dominance over the corresponding non-cartel cdf. The MWU test confirms the difference with $p < 0.01$ for both, the symmetric and asymmetric treatment. Because of that and regardless of the (a)symmetry, groups that engage in discussions are more successful in realizing higher market prices than groups that do not discuss prices before individually setting a price in the market stage. Moreover, comparing both cartel cdfs, groups that form a cartel in ASYM are able to charge higher prices than cartels in SYM. The cdf of market prices of SYM cartel groups is first order stochastically dominated by the cdf of market prices of ASYM cartel groups (one-sided MWU test, $p = 0.015$).

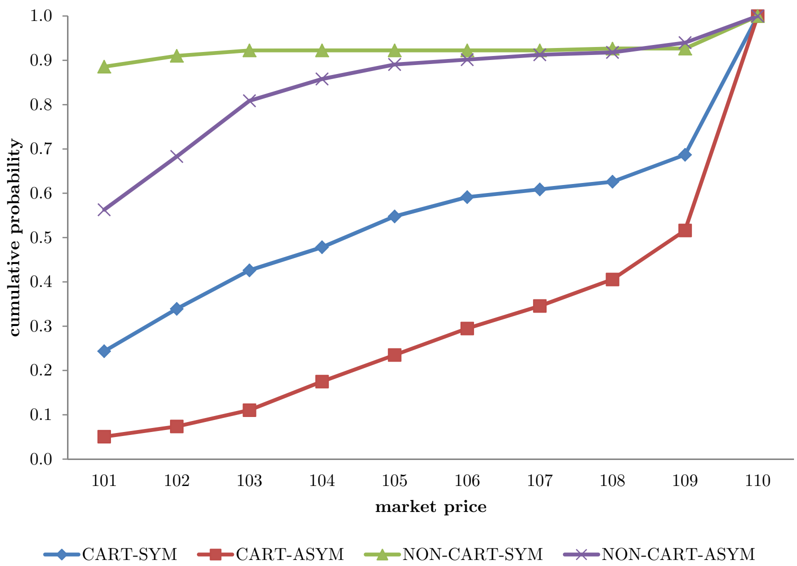


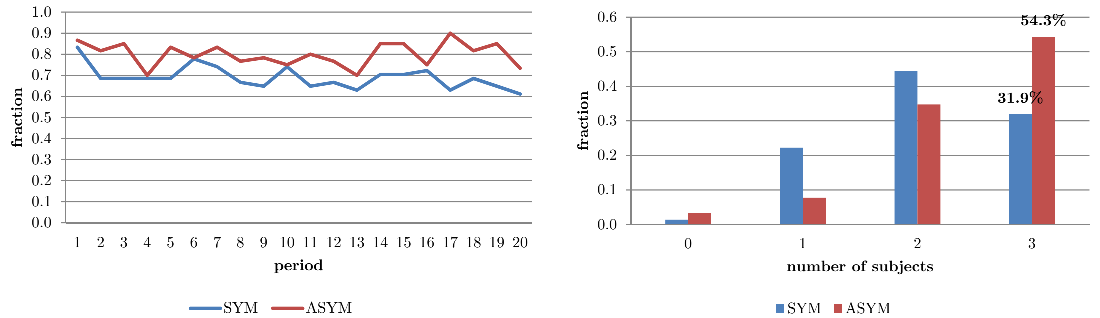
Figure 2.2: Cumulative distribution function of market prices in cartel and non-cartel groups

Non-cartel groups in the symmetric treatment charge prices closest to the Nash equilibrium with nearly 90% of all chosen prices being 101. Looking closer at the

NON-CART-SYM cdf, the 7.3% increase from 109 to 110 can be ascribed to one group only. Six times they managed to realize the highest market price of 110 through tacit collusion. After one player deviated, they never coordinated to 110 again, as all trust was gone. In summary, we find significantly higher average market prices in the asymmetric treatment. Even by only looking at cartel prices, groups in ASYM are able to realize higher prices than SYM cartels. It seems that players distrust each other less in a cartel where one player is unable to report the conspiracy.

2.5.2 Cartel activity

The overall fraction of cartels is 31.9% in SYM and 54.3% in ASYM. According to our research Hypothesis H2, there is no difference between SYM and ASYM with respect to cartel formation. Considering 18 groups in SYM and 20 groups in ASYM for a 20-period analysis, a one-sided MWU test confirms that cartel formation in ASYM is significantly higher than in SYM (one-sided MWU test, $p < 0.01$). Therefore, we reject Hypothesis H2.



(a) Average fraction of subjects who wish to join talks

(b) Number of group members agreeing to discuss prices

Figure 2.3: Cartel activity

Figure 2.3a shows the fraction of subjects who wish to form a cartel. With 20-period means of 68.9% in SYM and 80% in ASYM, we find a high willingness to participate in price talks. However, as illustrated in Figure 2.3b, there are fewer cartels in SYM because in the majority of the SYM groups, only two participants want to discuss prices. Since, according to our notion, a cartel is formed when three players unanimously agree to discuss prices, we find the fractions of cartels (31.9% and 54.3%) in the right columns of Figure 2.3b. The fractions, of course, add up to 1 in each treatment.

The plot of Figure 2.4 illustrates the cdfs of the number of times subjects want to join a discussion. Whereas in ASYM, subjects want to discuss prices at least four times, in SYM, 3.7% of all subjects do not want to discuss prices at all. Furthermore, the cdf of the individual willingness to join talks in SYM is first-order stochastically dominated by the cdf to join talks in ASYM. This means that subjects in ASYM individually want to join talks more often than in SYM. This result is contrary to the findings of Bigoni et al. (2009), as there, compared to their Leniency Treatment, the rates of communication attempts in the Ringleader Treatment are insignificantly lower.

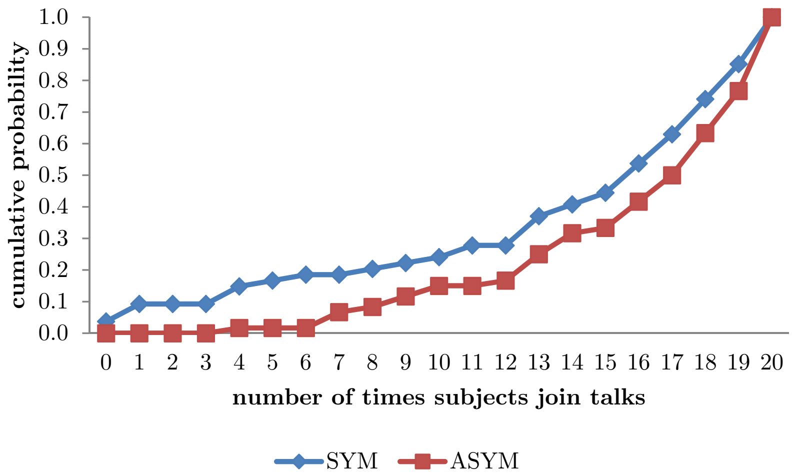


Figure 2.4: Cumulative distribution function of the number of periods individual subjects want to collude

2.5.3 Cartel stability

For the analysis in this subsection, we focus on cartel groups only. In case a cartel is formed, 92.6% of the groups in SYM coordinate to an agreed-upon price of 110. So do 95.7% of the groups in ASYM. See Figure A.1 on page 95 for the the cdfs of the agreed-upon prices by cartel groups. There is no significant difference in the outcome of price discussions (two-sided MWU test, $p = 0.508$). However, in subsection 2.5.1 we observe that SYM cartels charge significantly lower market prices than ASYM cartels. This indicates that subjects undercut prices in SYM more often than in ASYM. As there are more reasons why cartelization must not always be successful, we evaluate three causes for cartel breakdown: (1) a player deviates from the agreed-upon price to keep all the earnings of the current period, (2) at least one player reports the participation in price talks, and (3) the cartel is detected by the authority with probability $\delta = 15\%$. Table 2.2 summarizes the causes of cartel breakdown.

	Fraction of reasons for break down		
	Deviation	Reporting	Detection
SYM	0.67	0.58	0.04
ASYM	0.51	0.41	0.08

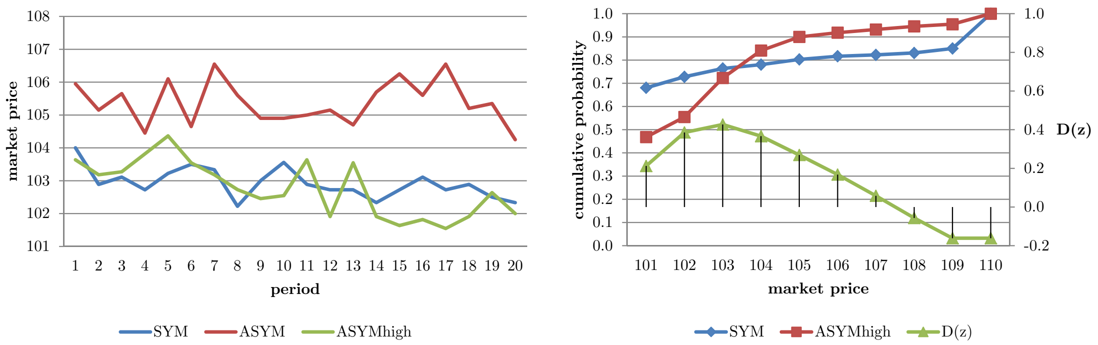
Table 2.2: Causes of cartel breakdown

The majority of all cartels break down because one player deviates from the agreed-upon price. However, there is significantly less undercutting in ASYM (51%) than in SYM (67%) (one-sided MWU test, $p = 0.09$). We obtain similar results regarding the breakdown because of reports. In SYM, 58% of all cartels are reported. A report happens for three reasons: (1) because a player deviates from the agreed-upon price (punishment or self protection for the undercutting player), or in case the cartel survives (with the market price being the agreed-upon price), because of the fear (2) another player might report first or (3) the authority might uncover the conspiracy. Compared to ASYM (41%), subjects in SYM report price discussions more often (58%), which indicates a high level of distrust among cartelists. However, behavioral differences are not significant (two-sided MWU test, $p > 0.10$). Our result confirms Hypothesis H3, but we reject H4. Nevertheless, cartels in ASYM, where cartel ringleaders are excluded from leniency application, seem to be more stable. Asymmetry appears to stabilize the cartel as the ringleader is trustworthy when not being able to report. The right column of Table 2.2 shows, that only very few cartels are detected after players neither deviate nor report the cartel.

2.5.4 High probability of detection

Concluding their formal analysis, Herre and Rasch (2009) suggest that ringleader exclusion does increase the sustainability of collusion. Our results confirm this view as we find less deviation from the agreed-upon price as well as a lower fraction of instances of reporting in the ASYM Treatment than in the SYM Treatment (see Table 2.2). Moreover, Herre and Rasch (2009) postulate that the opposite holds if the authority audits with a high probability of detection. Hamaguchi et al. (2009) present various studies that deem the probability of detection to be unrealistically high in order for cartel members to even report a cartel.

In this subsection, we analyze the results of the ASYMhigh-Treatment where we raise the detection probability from $\delta = 15\%$ in ASYM to $\delta = 75\%$ in ASYMhigh. Figure 2.5b shows that the cdf of prices in ASYM is stochastically dominated in the second-order sense by the cdf of prices in SYM as the cumulative difference ($D(z)$ for all $z \in \{101, .., 110\}$) between the cdfs drops below zero. However, the difference in average market prices over all 20 periods is not significant (two-sided MWU test, $p = 0.379$). The MWU test also confirms that there is no difference in average market prices per period (Figure 2.5a) between SYM (18 observations) and ASYMhigh (11 observations) as we obtain a p-value of 0.367 (two-sided).



(a) Average market price per period

(b) Cumulative distribution function of market prices

Figure 2.5: Market prices

Cartel formation in ASYMhigh is not significantly different from cartel formation in SYM (two-sided MWU test, $p = 0.231$). Furthermore, the fraction of subjects that deviate from an agreement (66%) is about the same as in SYM. 78% of those groups that do have price talks are reported afterwards, whereas only 4.9% of all cartels are detected by the authority. Still, this is 22.2% of the cartels that were not reported. All results of this subsection are in line with our Hypothesis H6.

2.5.5 50% reduction of fines

So far, our analysis has focused on a 100% fine reduction for cartel ringleaders. Now, we again assume a (leading) defector that fully cooperates with the authority. She therefore receives a fine reduction of 50% which is in line with the current practice of the European Commission. Furthermore, a fine reduction - but no immunity from fines - may be granted to ringleaders by the German Federal Cartel Office. The amount of the reduction is based on the value of the contribution to uncovering the illegal agreement as well as the sequence of the applications from all cartel members (Bundeskartellamt, 2006). Bos and Wandschneider (2011, Table 1) provide an overview of ringleader cases and fine reductions between 2000-2010. To analyze the effect of a 50% fine reduction for cartel ringleaders, we look at the data from the ASYM50 Treatment. Adapting both hypotheses H1 and H2 to this situation, resulting market prices and cartel formation should be inbetween the SYM and ASYM outcomes. At least, we do not expect to obtain the same results as in ASYM, as a 50% fine reduction is offered to the first reporting player and thereby gives an incentive to report.

With a total of 12 observations we find that average market prices are not significantly different from SYM (two-sided MWU test, $p = 0.396$). Surprisingly, cartel formation is even lower than in SYM (one-sided MWU test, $p = 0.014$). This result may be driven by the reluctance of cartel ringleaders to discuss prices. As they realize negative per-period earnings even upon reporting, not participating in talks seems to be the better option as $0.3\bar{3} > 0$. Then again, if there were enough trust among the group, cartelization would be the best option, but subjects undercut prices most often in this treatment. In 75% of all cartel groups, the agreed-upon price is not identical with the market price. For further details on pricing behavior in ASYM50, we refer to Figure A.2 in Appendix A.1 on page 95. In ASYM50, 75% of all cartels are reported, while 7.1% are detected by the authority. That is every second cartel which was not reported.

With market prices in ASYM50 being equally low and cartel formation being even lower than in SYM, one could argue in favor of the current practice of the European Commission. The EU implemented a non-discriminatory leniency policy in 2002, making cartel ringleaders eligible for leniency application and thereby raising distrust among cartelists. However, ringleaders have not yet been granted full immunity upon reporting, but they have been granted fine reductions of up to 50%. The results of our experiment show that even a fine reduction of only 50% has the same tendency as immunity from fines. Market prices and cartel formation are significantly lower than in ASYM (one-sided MWU test, $p < 0.01$).

2.6 Conclusion

In this essay, we analyze the impact of the different treatment of cartel ringleaders. In an economic experiment, subjects are given the possibility to report a cartel before an investigation takes place. In case of a report, the player is granted a fine reduction of up to 100%. In a symmetric treatment (SYM), all players may report the conspiracy, whereas the cartel ringleader is not allowed to report in the asymmetric treatment (ASYM). Despite the non-binding nature of price discussions, groups that do communicate are able to realize higher market prices than groups that do not discuss prices. This result is in line with similar experimental studies on cartelization. The focus of the experiment in this contribution lies on the effects of ringleader discrimination (exclusion from leniency programs). Our results suggest that antitrust policy should be designed for making cartel ringleaders eligible for leniency application. We find a strong deterrence effect as, compared to ASYM, cartel formation is significantly lower in non-discriminatory designs (SYM). Likewise, resulting market prices are significantly lower because cartelists cheat on each other more often. Compared to the ASYM Treatment, we also find a higher fraction of reports in SYM. When every player has the possibility to report the conspiracy, the cartel ringleader is no longer a trustworthy partner in crime as he has similar rights to anyone else. This strengthens the distrust among cartelists and vitalizes the race to report. We also provide evidence from a treatment where we implement a high probability of detection through an authority (ASYMhigh). The treatment yields about the same results as SYM. We do not find significant differences in either cartel formation or resulting average market prices. Subjects exhibit similar behavior regarding deviation and reporting. As an overall conclusion we argue in support of a non-discriminatory leniency policy. However, granting full immunity to cartel ringleaders might be hard to justify, as the driving force of the conspiracy then remains virtually unpunished. Since the results in ASYM50 show a similar effect as in SYM, a fine reduction upon reporting seems to be sufficient. This more desirable approach corresponds to the leniency program by the German Federal Cartel Office and is in line with the current leniency practice of the European Commission as to date, no ringleader has yet been granted full immunity from fines.

3 Regulatory Price Interventions in Retail Gasoline Markets: An Experimental Analysis of the Austrian Rule

3.1 Introduction

Since the Bundeskartellamt (BKartA, German Federal Cartel Office, henceforth FCO) took up a sector inquiry in the market for gasoline in May 2008, this specific market increasingly draws competition experts' attention. With the goal to analyze the proper functioning of the retail gasoline market in Germany, the FCO thereby finds a dominant position of five companies (Aral (BP), Shell, Jet (Conoco Philips), Esso (Exxon Mobil) and Total). As market entry is virtually impossible, no other player is able to effectively constrain the market power of the above-mentioned oligopoly. Furthermore, the market is characterized by a high level of transparency: every competitor has knowledge about all market prices at any given time and is also able to estimate production costs and sales prices of the other oligopolists. According to the FCO, the observed market behavior of price parallelism in the gasoline retail market is caused by this market structure. Evidence for explicit collusion, however, was not found.

In the final report, published in May 2011, the FCO attests the market an anticompetitive environment. Moreover, it discusses price-regulatory mechanisms which are current practice in neighboring countries and in Western Australia. However, the FCO remains skeptical about the effectiveness of the Austrian, the Luxemburgish and the Western Australian price regulation rules in Germany. Likewise, the point of view of numerous competition experts who attended the 2011 Conference on Antitrust Law in Bonn was skeptical, too. Furthermore, the associated working paper (Bundeskartellamt, 2011a) specifically mentions the "risk of worsening (instead of improving or solving) the situation if a potentially incorrect regulatory rule was implemented". Nevertheless, especially the Austrian rule attracted attention and was the subject of a vivid, yet constructive discussion at the conference. Since the majority of the participants was not convinced about the utility and the applicability of the Austrian rule, competition experts encouraged to further research the effectiveness of price-regulatory interventions by means of economic experimentation (see Bundeskartellamt (2011c)).

Basically, the Austrian rule dictates the timing of price increases. Gasoline station operators are only allowed to increase prices once a day at 12 pm (noon). Price reductions, however, are permitted at any time of the day. Thus, the rule restrains from setting prices at will and aims to counter price volatility. At first glance, such price-regulatory intervention seems attractive and consumer-friendly: As the rule is simple to comprehend, it creates transparency and certainty. However, the same goes for gasoline retailers and oil companies. The intended consumer advantage might therefore result in a consumer disadvantage. The rule regulates pricing behavior and reduces the suppliers' choice of actions. Thereby, pricing behavior becomes more predictable and, for retailers and oil companies, uncertainty about competitor's pricing decreases. This scenario provides ideal conditions facilitating price coordination and collusion.

For these reasons, we are also skeptical about the beneficial character of a price regulatory intervention along the lines of the Austrian rule. We therefore model interaction (constraints) under the Austrian rule and analyze pricing behavior in an economic experiment. We investigate the functioning of the mechanism and evaluate its suitability as a price-regulatory scheme in retail gasoline markets. As a result, we find the Austrian rule to support coordination and, thereby, to facilitate collusion in an experimental market with infinitely repeated interaction. Moreover, the regulated experimental market yields less volatile but significantly higher market prices compared to our unregulated experimental market.

In the following section, we start off by presenting a detailed print media coverage to shed some light on the political discussion about the implementation of a regulatory mechanism in the sense of the Austrian rule. We then introduce further price-regulatory schemes which are established law in neighboring countries and Western Australia in section 3.2. The related literature is reviewed in section 3.3. After introducing the experiment in section 3.4, we present our hypothesis in section 3.5. We describe the experimental procedure in section 3.6 before we evaluate all hypotheses by discussing the results in section 3.7. A summary of all findings is presented in section 3.8.

3.2 Industrial and political background on gasoline market price regulation

By covering print media from August 2009 until April 2012, we give a detailed understanding of the point of view of various policymakers and industry insiders in Austria and Germany. Furthermore, we introduce and discuss other price-regulatory mechanisms in this context.

We first show the continuous increase in gasoline prices in Germany. Because mineral oil companies are found to collude tacitly (see Bundeskartellamt (2011b)), competition authorities are not able to initiate appropriate legal proceedings. Therefore, the FCO merely pays focused attention to improve competition by strengthening independent gasoline stations. As prices increase further, politicians are called upon to rectify the situation. By evaluating different approaches, German politicians take into account various price regulation schemes which are established law in neighboring European countries and in Western Australia (see Die Welt (2012b) or Süddeutsche Zeitung (2012a) besides countless other newspaper articles). We discuss the Austrian rule in subsections 3.2.1 and 3.2.2, the Luxembourgish gasoline price ceiling in 3.2.3 and the 24 hour rule (Western Australia) in subsection 3.2.4.

3.2.1 The Austrian rule in Austria

The Austrian rule, as introduced above and established law today, is the result of a number of legislations that were adopted in Austria over the past years. In order to increase transparency and to counter highly volatile gasoline prices, the Bundesministerium für Wirtschaft, Familie und Jugend (BMWFJ, Federal Ministry of Economy, Family and Youth, henceforth FMEFY) introduced a regulation scheme which allows for price increases to take place once a day only. The Gas Price Regulation Act came into effect on July 1, 2009 (see BMWFJ (2009)). From then on, an increase in gas prices was only possible at 12 am (midnight) or upon opening of the gas station in the morning. Price reductions, however, were permitted at any time of the day. On the one hand, consumers quickly adapted to the new circumstances. According to an online-survey conducted by the Österreichischer Automobil-, Motorrad- und Touring-Club (ÖAMTC, Austrian Automobile, Motorcycle and Touring Association, henceforth AAMTA) one month after the price regulation came into effect, 60% of all consumers who took part in the survey changed their demand for gasoline: 21% of the participants no longer refuel during the day, when prices are comparatively high, but in the evening, when prices tend to be lower. Apparently with good reason, as the AAMTA observes price reductions at 10 am, 1 pm and 4 pm. However, already in August 2009, the AAMTA took the line, that prices are systematically set too high in the morning (see DiePresse.com (2009b)). On the other hand, shortly after the introduction of the 2009 price regulation, consumers also faced problems that might not have been properly taken into consideration by the FMEFY. The 2009 regulation, for example, differentiated between (1) gas stations that offer services during the day, (2) those with 24 opening hours and (3) automated gas stations, which can be operated without any personal all day and night. Each respective type of station could increase gas prices at (1) the opening of the gas station, (2) midnight and (3) by no later than 8:30 am. BP Austria also recognized the unequal treatment and announced to file an individual application with the Constitutional Court to repeal the regulation (see DiePresse.com (2009a)).

As this inconsistency rather increased uncertainty instead of transparency and fostered confusion among consumers, the regulation was revised. The new regulation scheme, commonly known as the Austrian rule, came into effect on January 1, 2011 (see BMWFJ (2010)). The only difference to the old regulation scheme was (and is to date) the point of time when prices may be increased. While the new regulation also only allowed for one price increase per day, the time was set to exactly[1] 12 pm (noon) for all kinds of gas stations. This way, the ministry followed the request of, inter alia, the AAMTA, who argued in favor of a single point of time for price increases (see ÖAMTC (2010)). In case of a violation of either the old or the new regulation, authorities may impose penalties according to the Gewerbeordnung (GewO, Trade Regulation Act, henceforth TRA) and the Preisauszeichnungsgesetz (PreisauszeichnungsG, Pricing Act, henceforth PA). See Table 3.1 for more details and a comparison of the old and new Austrian gasoline pricing regulation.

	Regulation old	**Regulation new**
came into effect	July 1, 2009	January 1, 2011
effective until	December 31, 2010	December 31, 2013
price increase	once at 12 am (midnight) or upon opening of the gas station, automated gas stations by no later than 8.30 am	once at 12 pm (noon), consistent for all kinds of gas stations
price reduction	at any time	at any time
penalty	up to EUR 2180 (TRA) and up to EUR 1450 (PA)	

Table 3.1: Comparison of the old and new Austrian gasoline pricing regulation

By implementing the revised Gas Price Regulation Act (which from now on, we solely refer to by the Austrian rule), the FMEFY, or, more precisely, Reinhold Mitterlehner (Minister for Economy and Energy) intended to further increase competition among gas station operators and to further increase transparency for consumers (see DerStandard.at (2010)). However, the AAMTA did not observe significant price reductions in retail gasoline markets. In urban areas, gasoline prices changed four times a day, two times in rural areas. The strategy still would be a major increase ("as high as possible") at noon and a step by step reduction during the day (see ÖAMTC (2011)). Using data from the European Commission's Oil bulletin, Dewenter and Heimeshoff (2012) apply an econometric model and do not

[1]The FMEFY expects an instant change in prices without undue delay. The ministry however recognizes the various practices at gas stations and further defines instant change as "in accordance with the technical requirements of labeling", which, according to the FMEFY takes about 5 to 10 minutes if the process is not automated.

find conclusive results: Gasoline retail prices may have decreased because of the Austrian rule, however, the effect is not found to be significant.

In April 2012, after an intense discussion about price-regulatory mechanisms in Germany (see below) and the evaluation of different schemes, Reinhold Mitterlehner announces further (possible) regulatory measures to the Austrian press. As for now, only the timing of price increases is regulated in Austria, Mitterlehner also takes "price corridors" into consideration (see DerStandard.at (2012)).

3.2.2 The Austrian rule in Germany

Gasoline prices is also a very sensitive topic in Germany. Due to an increase in price of about 10 Euro-Cent per liter in December 2010 (see Figure 3.1), in January 2011 the Auto Club Europe (ACE, Automobile Club Europe) requested Rainer Brüderle (Federal Minister for Economy at the time) to bring forth a regulation according to the Austrian rule in Germany (see Tankstellenmarkt.com (2011a)).

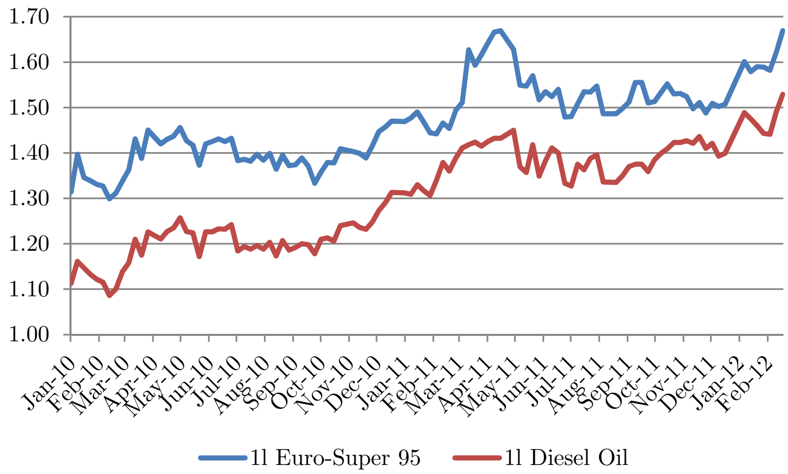


Figure 3.1: Retail Gasoline prices (with taxes in EUR) in Germany (January 2010 to February 2012), data from Europe's Energy Portal (2012)

Likewise, the Allgemeiner Deutscher Automobil Club (ADAC, General German Automobile Association, henceforth GGAA) often demanded for governmental intervention with respect to high gasoline prices, however, it did not take position as to which mechanism ought to be implemented. In May 2011, the FCO published the final report on the sector inquiry in the gasoline market in Germany. In the report, the FCO concludes that gasoline prices are above competitive pricing, but there is no explicit collusion among mineral oil companies. However, there seems to be price leadership, as the data shows that one company increases prices while others systematically and reliably follow on a regular basis. The collusive behavior is obvious but not within the scope of jurisdiction of the FCO. There is found no explicit coordination because the companies observe market behavior and adapt their own strategy, i.e., their pricing. Wernhard Möschel (former chairman of the German Monopoly Commission) deems this to be normal competitive behavior (see Die Welt (2011b)). By analyzing the data collected during the sector inquiry, the FCO finds a pattern that, based on trust and the functioning over time, does not require for any agreement. Thus, the FCO, too, requests politics to deal with the problem at hand.

The Federal Minister for Economy, Philipp Rössler (appointed as Minister in May 2011), immediately argued in favor of the Austrian rule and announced that "the ministry would test if the mechanism was appropriate" (see Rheinische Post (2011)). Numerous politicians were backing Rössler's steps towards the Austrian rule in summer 2011. Among the supporters at that time was Ilse Aigner (Federal Minister for Consumer Protection). Both, the Bundeswirtschatsministerium (BMWi, Federal Ministry for Economy, henceforth FME) and the Bundesministerium für Ernährung, Landwirtschaft und Verbraucherschutz (BMELV, Federal Ministry for Consumer Protection) repositioned themselves in November 2011, as the Austrian rule (apparently) allowed for price increases that are higher than necessary in order to reduce them step by step over the day (see Bundestag (2011) and Der Westen (2011)). To our best knowledge, the study or the investigation they base their decision on, is not publicly available. Yet, Berninghaus et al. (2012) were published online as a working paper.

Needless to say, most syndicates refuse to accept regulatory interventions, even more so when it comes to a pricing mechanism that resembles the Austrian rule (see Tankstellenmarkt.com (2011b) for more details and the positioning of each respective syndicate or institution). In addition to that, Andreas Mundt, president of the FCO, expressed his doubts as to the effectiveness of the Austrian rule for the same reasons mentioned above. So did the majority of the competition experts who attended the 2011 Conference on Antitrust Law in Bonn (see Bonner General-Anzeiger (2011) and Bundeskartellamt (2011c)). Likewise, the GGAA was skeptical about this kind of regulatory intervention. GGAA spokesman Klaus Reindl drew attention to the structural differences between the Austrian and the German gasoline market as he questioned the application of the Austrian rule in Germany (see Der Westen (2011)).

Just when everyone seemed to agree on *not* to pursue the Austrian rule as an appropriate way to reduce gasoline prices in Germany, the state of Thuringia issued an official request within the Federal Council on December 22, 2012 (see Bundesrat (2011)). The request aimed to bring the government to implement a price-regulatory mechanism along the lines of the Austrian rule in Germany. As the issue was not finally decided on at the 892nd meeting of the Federal Council on February 10, 2012, the request was forwarded to the expert committees for further analysis (see Bundesrat (2012c)). Three expert committees were chosen, namely the Wirtschaftsausschuss (Wi, Economic Committee, henceforth EC), the Ausschuss für Agrarpolitik und Verbraucherschutz (AV, Committee for Agricultural Policy and Consumer Protection, henceforth CAPCP) and the Finanzausschuss (Fz, Financial Committee, henceforth FC). On February 16, 2012, EC and CAPCP recommended the Federal Council to "implement a price regulatory mechanism along the lines of the Austrian rule or to further review alternative mechanisms". FC abstained from giving a recommendation to the Federal Council (see Bundesrat (2012b)). Michael Boddenberg, Hesse Minister of State, appended his request for price regulatory intervention at the 895th meeting of the Federal Council on March 30, 2012. He argued in favor of the 24 hour rule (Western Australia) to be implemented (see Bundesrat (2012d)). On the same day, the Federal Council took into account the advice given by the expert committees CAPCP and EC, and forwarded its decision to the Federal Government (see Bundesrat (2012a)).

Meanwhile, in March 2012, Andreas Mundt clarified the opinion of the FCO in Bonner General-Anzeiger (2012) and Wirtschaftswoche (2012). As companies in Germany already increased prices (only) once a day, the Austrian rule would, in his opinion, "not improve the situation much". Instead, Mundt brought up the 24 hour rule, which (to date) is current practice in Western Australia. Yet, he took the line that there was no price regulatory mechanism in the world which could identically be adapted to the German gasoline market (see Der Westen (2012)). Because of that, Mundt requested for more research to be conducted on possible regulatory mechanisms which lead to more competition in the market for gasoline. We introduce further price regulatory mechanisms in the following subsections.

3.2.3 Price ceiling (Luxembourg)

In Luxembourg, the maximum retail price for gasoline is set by the government. The price consists of the moving average of wholesale gasoline prices (Rotterdam) and a margin for costs set in negotiation with the Groupement Pétrolier Luxembourgeois (GPL, Association of Luxembourgish Oil Companies). A regulative mechanism according to this model was also discussed in the media with respect to governmental intervention in Germany (see Rheinische Post (2011) and Die Welt (2011b)).

In their economic survey on Luxembourg, OECD (2010, p. 50) on the one hand acknowledges that a fixated maximum price may effectively counter monopolistic pricing. On the other hand, the OECD finds price ceilings to be an imperfect substitute for effective competition. According to OECD (2010), price ceilings in Luxembourg possibly represent obstacles to price competition with the imminent risk of collusive behavior. OECD (2010) even recommends removing price ceilings for gasoline retailing as soon as the Luxembourgish competition authority has sufficient capacity (there were only four members of staff at the time the study was published). Sen et al. (2011) provide empirical evidence on retail gasoline price ceilings in Canada and find such regulation to be significantly correlated with higher prices.

Price ceilings may serve as focal points (see Schelling (1960)). This logic can be ascribed to the literature of evolutionary game theory where the relevancy of focal points emerges in games with multiple equilibria (see subsection 3.3.3 for further details). If there is a given maximum price, it seems only natural for retailers to make use of this offer. If one player assumes all other players to act in the same way, the outcome could result in an equilibrium by everyone charging the highest price possible. The collusive equilibrium might therefore constitute only due to the fact, that the ceiling price was proposed and communicated by the government or the authority (see also Scherer and Ross (1990, pp. 265-268)). Nevertheless, Lünemann and Mathã (2005) note that such price regulation must not per se lessen price volatility. They argue that gasoline prices in Luxembourg reveal a high frequency of price change, which is contrary to the point of view in Die Welt (2011b). In addition, OECD (2010) complements that prices, set by large gasoline retailers, are as a general rule undercut by independents who account for about one-third of the market in Luxembourg. A similar logic might hold for pricing behavior under the Austrian rule. Although no (numerical) ceiling price is set, all market participants might quickly learn the existence of a certain level of pricing that emerges every day at 12 pm (noon): for example, the monopoly price. Relating to this concern, the AAMTA study mentioned above has already shown a major, possibly higher than necessary or justified, price increase in Austria.

In the style of the government announcing a ceiling price, the industry also takes advantage of price announcements on its own. In late August 2011, the German media announced BP/Aral's strategy to charge higher prices (see Die Welt (2011a)). Information was made public that Aral intended to internally implement a compensation model which ties the commission for gasoline station operators to the duration they charge high retail prices. Thereby, the oil company provides incentives for retailers to set rather high than low prices. Further, it is beneficial to retailers to maintain high prices, once they are in place. In practice this has no effect on consumers as long as there are sufficient gas stations in the area that (1) are run by other oil companies or (2) are independent. As long as other oil companies do not follow Aral's example, setting similar incentives for retailers, these branded and independent retailers would be likely to provide gasoline cheaper than Aral retailers. Consumers will, however, face high prices across the whole area if all other oil companies adopted this approach. The danger of such pricing announcements has been shown by Andreoli-Versbach (2012) with regard to information exchange on intended future behavior. On the basis of an empirical analysis, the author provides evidence for an intense retail price parallelism subsequent to the public announcement of an individual price policy change by the Italian market leader Eni.

3.2.4 24 hour rule (Western Australia)

In late 2000, the Western Australian Government passed the Petroleum Products Pricing Amendment Act 2000 concerning a number of arrangements relating to gasoline prices. With the goal to deliver (1) greater competition on wholesale and retail levels, (2) a more transparent petroleum market, (3) a reduction of volatility of retail prices and (4) a reduction in the differential between city and country gasoline prices, one of the arrangements provided by the Act, was the requirement for retail prices being fixed for a 24 hour period. The arrangement came into effect on January 2, 2001.

As the legislation did not require retailers to move to the previously announced price, the government closed this loophole under the Petroleum Products Pricing Amendment Regulations (No. 2) 2001 from August 24, 2001. From then on, retailers had to fix prices of all grades of petrol, diesel and liquefied petroleum gas for each calendar day between 6 am to 6 am. All prices of the following day had to be announced to the West Australian Department of Consumer and Employment Protection (DOCEP) until 2 pm of the current day (see ACCC (2008)).

The 24 hour rule has always been a part of the discussion about price-regulatory mechanisms that could be implemented in Germany. In 2011, Peter Ramsauer (Federal Minister of Transport) promoted the pricing scheme in the media. Back then, Justus Haucap (Chairman of the Monopoly Commission and Professor for economics) spoke out against the realization of the 24 hour rule in Germany. He expressed his doubts with respect to lower gasoline prices and deduced state aided cartelization. "As prices had to be changed less often under governmental regulation, one could think of facilitated collusion" (see Die Welt (2011b)). Recently, Haucap and Müller (2012)) provide evidence on the effectiveness of the 24 hour rule on the basis of an economic experiment. As a main result of the paper, they suggest that the regulation implemented in Western Australia at least does, from a welfare point of view, not harm consumers. However, positive effects on welfare could not be expected either. The analysis by Haucap and Müller (2012) will further be discussed in section 3.3.2.

Associated with all the discussion, is, however, the understanding that the market structure of Western Australia might not be applicable to the respective market in Germany (or elsewhere). Western Australia is a huge state which accounts for one-third of the area of the whole continent. With 10% of the national population, Western Australia has a population density of 0.91 per km^2 with the majority of the population concentrated in the cities at the coastline (Perth, for example). In 2010, the population density in Germany was 220 per km^2 (99 per km^2 in Austria), whereas the distribution is rather uniform.

In their final report of the sector inquiry Bundeskartellamt (2011b), the FCO also picks up the 24 hour rule and refers to documents published by the Australian Competition & Consumer Commission (ACCC). Surprisingly, the authority itself finds a lessening of competition at both the wholesale and retail levels subsequent to the introduction of the 24 hour rule. According to ACCC (2002), this results from the inability of independent gas station operators to quickly respond to competitors. However, according to ACCC (2008), there apparently is no evidence of an increase in prices. Moreover, ACCC (2008) even concludes by finding a small overall price decrease. This result cannot be confirmed by Wang (2009), who assesses the 24 hour rule empirically. See section 3.3.2 for further details on his findings. The ACCC (2008) analysis is also heavily challenged by Harding (2008b), who deems the econometrics behind the ACCC (2008) conclusion to be deeply flawed. In his recent contribution, Harding (2008a) even contradicts some of the results. Notice the author's creativity as he refers to the Western Australian "Fuelwatch"-program by titling his work as responses to the "Foolwatch"-program.

3.2.5 Price transparency and the margin squeeze issue

According to Andreas Mundt in Wirtschaftswoche (2012), "one needs to think about how to disturb the peace of the gasoline oligopoly". We use this quote as an opportunity to digress, as we discuss the interaction between market (in)transparency and facilitated collusion as well as further mechanisms to "disturb the peace" in an excursus.

As mentioned above, the FCO has brought forth doubts regarding the effectiveness of the Austrian rule and warned that a rash price regulatory intervention might also backfire (see Bundeskartellamt (2011a)). A splendid example of a governmental intervention gone wrong is presented by Albaek et al. (1997). In 1993, in an attempt to "[..] promote competition and, thus, strengthen the efficiency of production and distribution of goods and services, etc., through the largest possible transparency of competitive conditions [..]", the Danish antitrust authority decided to gather data on transaction prices for two grades of ready-mixed concrete for publication on a regular basis. In the first year subsequent to the initial publication, average prices of both grades increased by 15 to 20 percent. The authors analyze the situation by taking into account business upturns and capacity constraints and suggest the increase in transparency to be the reason for the reduced intensity of oligopoly price competition.

When it comes to transparency, there is always the question of its effect on competition. Every economic textbook basically suggests markets and prices to be competitive if there is full transparency about everything. Besides the requirement for some other assumptions characterizing perfect competition, perfect information about prices and product quality are always mentioned (see, e.g., Carlton and Perloff (1999, p. 57)). The structure of this sort of economic interaction is often understood to be identically applicable to any market, but reality proves "us" wrong in more than just one case (or market). Because prices are key strategic variables, any company monitors the market in order to take into account the strategy which is currently pursued by one's competitors. Setting prices below competitor's prices requires for exact calculation and therefore knowledge about current market prices, too. In general, there is nothing wrong about positioning oneself in a "competitive" market. While some competition experts assess this behavior as thoroughly competitive behavior (Wernhard Möschel, former chairman of the German Monopoly Commission in Germany, for example, see Die Welt (2011b)), the possibility of observing current sales prices in highly transparent oligopoly markets poses a problem for antitrust authorities. This issue is also discussed by the members of the policy round table in OECD (2001). They suggest that a negative impact of increased price transparency is especially likely in markets already prone to anticompetitive coordination.

This logic may easily be adapted to the retail gasoline market in Germany. With five major companies in place (Aral (BP), Shell, Jet (Conoco Philips), Esso (Exxon Mobil) and Total), the German Federal Cartel Office finds a highly transparent market with concentrated supply by vertically integrated oil companies. Gasoline prices in Germany (with taxes) are known to be rather above than below EU average (see Europe's Energy Portal (2012)) and consumers regularly suggest cartelization as a reason for such high retail prices. However, the German antitrust authority did not find explicit agreements among oil companies. Yet, Andreas Mundt does not withhold the admonitory position of the authority in numerous interviews and speeches (see Bonner General-Anzeiger (2011) and Bonner General-Anzeiger (2012)) as the investigation (sector inquiry 2008-2011) has revealed implicit price coordination. Oil companies make use of a behavioral pattern where one company increases sales prices and all others follow on a regular basis. This coordination, over time, does not require for any kind of explicit agreement (see Bundeskartellamt (2011b)).

Because Philipp Rössler (FME) and Andreas Mundt (FCO) reject both, the Austrian rule and the Western Australian 24 hour rule (see Augsburger Allgemeine (2012)), they focus the effort of the competition authority to promote competition by strengthening independent gasoline stations, which (in terms of quantity) account for one-third of the gasoline retail market (see Frankfurter Rundschau (2012) and Süddeutsche Zeitung (2012a)). It has come to the FCO's attention that all, or at least some, of the five major (highly vertically integrated) oil companies might discriminate against independent gasoline stations. The authority accuses the oligopolists to charge higher wholesale gasoline prices to independent gasoline station operators than they charge their own (branded) gasoline station operators. This so called margin squeeze is prohibited according to Article 20 section 4.3 of the Gesetz gegen Wettbewerbsbeschränkungen (GWB, Act Against Restraints of Competition, henceforth ARC). The German government recognizes the effectiveness of this tool to fight discrimination by law, as legislature intends to adopt Article 20 section 4 ARC for an indefinite period of time. At the moment, its legal force is meant to end on December 31, 2012.

As numerous complaints were filed with the FCO, in early April 2012, the authority mailed detailed questionnaires to all major oil companies, demanding to explain and (if necessary to) justify their apparent illegal behavior. While some recipients declare to already have implemented surveillance mechanisms, all of them criticize the questionnaires after having already been investigated in the sector inquiry for over three years (see Süddeutsche Zeitung (2012b), Süddeutsche Zeitung (2012c) or Die Welt (2012a)). According to Handelsblatt (2012a), Norbert Röttgen, Federal

Environment Minister at that time, supports the intervention of the FCO as he describes the behavior of major oil companies as "an unacceptable and shameless abuse of power". However and despite all efforts, Süddeutsche Zeitung (2012b) expects a further increase in prices, since the margin squeeze approach does not affect high price transparency of the retail gasoline market.

3.3 Related literature

There is virtually no contribution which exclusively deals with the Austrian rule. We therefore study the literature that is closely related to gasoline markets (3.3.1) and price regulatory mechanisms in general (3.3.2). We append a brief insight into equilibrium selection with respect to the aforementioned focal point theory (3.3.3).

3.3.1 Gasoline markets

While various studies empirically analyze the speed of adjustment of retail prices to upstream prices (wholesale, crude oil prices) to determine whether increases in sales prices are due to market power and collusive behavior (see Borenstein and Shepard (2002) or Deltas (2008) and Kirchgässner and Kübler (1992) for an analysis of the German gasoline market), there are also experimental studies on gasoline markets. In one of them, Deck and Wilson (2008) investigate the competitive effects of "zone pricing" on consumers in a vertical model of gasoline markets. They find that prices in smaller and isolated regions are higher than in regions where there are more gas stations. They further analyze vertical interaction between refiners and retailers as, based on their data they suggest, that under uniform wholesale pricing by refiners, retailers extract surplus from the consumers by differentiating retail prices regionally (isolated and center area). Wilson (2007) discusses the advantages of experimental economics to research antitrust and policy issues as he explains the procedure of the above mentioned gasoline market experiment conducted by Deck and Wilson (2008).

3.3.2 Price-regulatory mechanisms

Ever since the media brought attention to the doubtful effectiveness of the Austrian rule, the study by Berninghaus et al. (2012) is the first contribution which analyses the Austrian rule by means of an economic experiment. We will not review the contribution at this point, as we discuss all results presented in Berninghaus et al. (2012) in section 3.7. In addition, we then have a closer look at the data and report further results.

In order to analyze the effects of all previously discussed price-regulatory mechanisms, Haucap and Müller (2012) build on the model by Deck and Wilson (2008) in their recently conducted economic experiment. Haucap and Müller (2012) use a quadratic (10×10)-Hotelling-city with for sellers (gasoline stations) and 10.000 uniformly distributed robot buyers. As they divide one period into four rounds (morning, noon, afternoon, night), they additionally model demand fluctuations. Under every regulatory mechanism (Baseline (free competition), Austria, Luxembourg, Western Australia), sellers set prices while also taking into account wholesale prices. Wholesale prices are communicated to be random, but a given price path is implemented in the experiment. Using a within-subject design, Haucap and Müller (2012) implement each two regulatory schemes in any of their treatments. They find the Austrian as well as the Luxembourgish approach to have a negative effect on consumer welfare that comes along with higher retail prices as in the Baseline setting. As a main result, the authors argue in favor of the Western Australian mechanism, as this regulation, based on their experimental data, at least does not seem to harm consumers.

In contrast to the simple experiment by Berninghaus et al. (2012) allowing for clear-cut results and testing the behavioral features that come along with a regulatory price intervention in the sense of the Austrian rule, the experiment by Haucap and Müller (2012) is more of exploratory nature: Because of a whole bunch of potentially influencing variables, there is less control. Participants decisions cannot exclusively be traced back to the price-setting rule. For example, Haucap and Müller (2012) recognize demand fluctuation. This is well-meant, but it is not controlled for the effect of this variation. In addition, it is inaccurately implemented: The authors allow for price increases in the first period of their experiment, when demand is high. This is not in line with the Austrian rule where an increase in price has to take place at 12 pm (noon), when, according to the authors' model, demand is relatively low. Haucap and Müller (2012) find a strong initial increase in prices under the Austrian rule while it is absolutely unclear if this effect can be attributed to the high demand in that first period (which would be self-explanatory, as the level of demand is communicated to all subjects) or the possibility of a sole increase, after all. The interpretation of the results would be more precise under the assumption of constant demand. Moreover, the authors are aware of the utmost importance of independent gasoline stations in Müller (2012), yet, they do not recognize this key market feature in their experimental analysis.

Dewenter and Heimeshoff (2012) apply difference-in-differences methods in order to analyze the impact of price-regulatory mechanisms on retail prices in Austria and Western Australia. For studying the Austrian rule, they use data for all 25 European countries. The Austrian rule is found to have a negative impact on gasoline and diesel prices. The effect is statistically significant. For examining the 24 hour rule in Western Australia, information on 107 Australian areas and cities are used. No statistically significant effect is found. Hence, the Austrian rule outperforms the Western Australian 24 hour rule. However, both the Austrian and the Western Australian estimation suffer from serial correlation. Therefore, the significance level may be smaller than estimated, resulting in insignificant effects in Austria, too.

Hilgers (2012) reviews the process of implementing a price-regulatory mechanism in Germany as he discusses (gasoline) price transparency and the margin squeeze issue in the light of the 8th Amendment to the ARC (8. GWB-Novelle). The author reports the German government's rejection of the Austrian rule and describes the FCO's position in favor of extending the margin squeeze prohibition for an indefinite period of time. Hilgers (2012) suggests to stronger rely on the Bundesnetzagentur's (BNetzA, Federal Network Agency) expertise regarding regulatory measures in other markets.

Wanting to introduce further literature related to the 24 hour rule, we realize that there is as much economic literature on advance, non-binding price announcements (see Holt and Scheffman (1987), Rotemberg and Saloner (1990) and Blair and Romano (2001) for example) as there is a lack of literature on (simultaneous) binding price announcements. There is one experimental study by Grether and Plott (1984). The authors analyze behavioral aspects that might emerge in markets characterized by the structural features of the lead-based antiknock compound industry (Ethyl Case[2]). One of the three practices challenged by the Federal Trade Commission was, that suppliers agreed to give at least a thirty-day notice of price increases. The binding announcements were either communicated to the buyers directly or made public in print media. The results of the experiment suggest that only a combination of (1) the specific market structure and (2) facilitating practices might account for prices and profits above the competitive equilibrium. However, in neither the real, nor the experimental market did suppliers make their binding announcements simultaneously. Other firms (in the real market) had "several days to respond". Hence, we do not deem this study crucially relevant to our context of the 24 hour rule as the simultaneity is missing.

Wang (2009) uses a rich data set that tracks price changes of nearly every gasoline station in the Perth area. He is, thereby, able to analyze pricing strategies before and after the implementation of the 24 hour rule in Western Australia. He finds strong evidence for a pricing behavior which is characterized by the Edgeworth price cycle equilibrium (see Maskin and Tirole (1988)) before and after the implementation of the 24 hour rule. This concept, which is also applicable to the German gasoline retail market (see (Bundeskartellamt, 2011c)), describes a pricing pattern that features short run price commitments. Wang (2009) partly confirms the findings of ACCC (2008), who also observes cyclical pricing behavior. Yet, Wang (2009) does not find any significant influence of the 24 hour rule on the level of retail gasoline prices.

3.3.3 Price ceilings as focal points

As already mentioned in subsection 3.2.3, we are concerned about the evolution of a focal point that, under a price-regulatory scheme in the fashion of the Austrian rule, might consolidate prices which remain on a higher level than in unregulated markets. Such situations emerge when individuals coordinate their behavior by

[2]Ethyl Corporation, E. I. du Pont de Nemours and Company, PPG Corporation and Nalco Chemical Corporation, Docket no. 9128. Federal Trade Commission.

referring to a practice or embarking on a strategy that is particularly striking or salient. This theory of focal points originates from Schelling (1960). In such situations, preferring a efficient equilibrium (everyone charges high prices) over a risk-dominant equilibrium (everyone charges competitive prices) would be in line with the axiomatic theory of equilibrium selection by Harsanyi and Selten (1988). Sen et al. (2011) suggest this notion to be applicable for price ceilings in gasoline markets, while the theoretical and experimental literature does not find proof for the focal point hypothesis. The results of two seminal contributions by Isaac and Plott (1981) and Smith and Williams (1981) on collusion in double-auctions do not suggest higher prices due to price ceilings, quite the contrary. Engelmann and Müller (2010) report on more laboratory experiments which have failed to find a collusive focal point effect of price ceilings. The authors take the line that previous studies have made such collusive effects rather difficult. However, in their newly designed experiment, which even increases the likelihood of a focal-point effect, the results again fail to provide supportive evidence. When it comes to empirical studies, there are more analyses besides Sen et al. (2011) which confirm price ceilings as focal points. Knittel and Stango (2003), for example, use data on (regional) state-level price ceilings on credit card charges to investigate the effect. Their results suggest that credit card companies made use of price ceilings as focal points in order to collude tacitly and thereby to sustain higher prices.

3.4 The experiment

3.4.1 Game design

In order to analyze the behavioral features along the lines of the Austrian rule, we design an experiment with a simple model of Bertrand competition: a symmetric duopoly with two homogeneous firms, A and B. Firms compete in prices $p \in P$, with $P = \{1, 2, 3, 4\}$. Assume both firms to choose their prices simultaneously without the possibility of communication. Ranking the corresponding payoffs correctly, we obtain an extended version of the classic textbook prisoner's dilemma (PD), where each firm has an incentive to always defect if the game is played once ($t = 1$ round) or finitely often (t rounds with $t \in \{1, ..., T\}$). As firms reason backwards in time and thereby determine their sequence of optimal actions, (1,1) is the unique subgame-perfect Nash equilibrium. We introduce a 4 × 4 matrix in the sense of an extended PD game in Table 3.2.

		firm B			
	number	4	3	2	1
firm A	4	28|28	15|30	10|34	5 |38
	3	30|15	22|22	16|27	10|30
	2	34|10	27|16	20|20	13|22
	1	38| 5	30|10	22|13	14|14

Table 3.2: 4×4 payoff matrix

The asymmetric payoffs of the symmetric game originate from the assumption, that the firm which sets a comparatively lower price attracts more demand and therefore earns more. However, the demand is still assumed to be positive, for example, because of transportation costs.

In an infinitely repeated game ($t \to \infty$ rounds), multiple equilibria emerge. As long as the corresponding payoffs are not lower than the one-shot equilibrium, every price combination might result as a subgame-perfect Nash equilibrium. Our 4×4 game even allows for more strategic interaction than a 2×2 PD game does. In case one firm (firm A) were to defect from the collusive equilibrium (4,4) or (3,3) by setting a price $p_A^t = p_A^{t-1} - 1$, the other firm (firm B) still has the possibility to punish by deviating even further ($p_B^{t+1} = p_B^t - 2$). Since the FCO finds the German retail gasoline market to be highly transparent (see Bundeskartellamt (2011b)), we adopt this market characteristic as we append the payoff Table 3.2 to every experimental instruction (see Appendix B.2.1).

3.4.2 Treatments

We ran a computerized experiment and implemented four treatments to (1) test our hypothesis and to (2) thereby analyze behavioral aspects with regard to a regulatory price intervention following the Austrian rule. Subjects play the game by two and independently choose a price $p \in \{1, 2, 3, 4\}$. They each receive payoffs according to Table 3.2 in ECU (experimental currency unit). Subjects play the game repeatedly for at least 31 rounds: round 0, 1, ..., 30 and so on. In the first round, round 0, the decision is made automatically. The price $p = 3$ is chosen for all subjects. Thus, they start off with a lump sum payment of 22 ECU each and a history of the game is generated before the first personal decisions take place. After the 30th decision made by each subject, the session ends with a probability of 1/5, or another round starts. This way, the end of the repeated game is unknown. We introduce this termination rule to avoid possible endgame effects (see Selten and Stoecker (1986) as well as Normann and Wallace (2012)). In our case, the likelihood of a continuation ($\delta = (1 - \frac{1}{5}) \approx 0.833$) can be considered as the discount factor of the game. $\delta \approx 0.833$ is sufficiently high to allow all aforementioned equilibria to emerge in an infinitely repeated game. For each subject, earnings per round are accumulated and visible during the whole session. As for the results in section 3.7, we only illustrate the first 31 rounds.

We first distinguish two treatments in a way, that in one setting, prices can be chosen freely, i.e. the choice of action of both subjects is not limited. We call this treatment NREG, as subject behavior or the game itself is "not regulated". In the second treatment, the choice of actions may be limited or the game might be understood as "regulated", hence the name REG. Like in NREG, the price $p = 3$ is chosen automatically in round 0. From then on, in round t, subjects are only allowed to choose the price that was chosen in round $t-1$, or any price below. The full price range is freely selectable in rounds 5, 20, 35 and so on. Only in these rounds subjects are able to increase prices. A price reduction on the other hand is possible in every round.

In the treatment REG(FI), subjects play the regulated game repeatedly (as introduced above) but not infinitely. We restrict the duration of the interaction to 31 rounds, which accounts for 30 decisions made by each subject. In REG(FI), besides the finite timeframe, the choice of actions is limited in the same way as it is in the REG treatment.

We deem both treatments with an infinite time horizon (REG and NREG) to be more fitting with respect to retail gasoline markets. No oil company conveys the impression to leave the market or to go out of business any time soon. Even if companies took into account the limitation of the resource (fossil) oil, their forecast period for gasoline retail markets would still be way shorter: by implication, they expect to sell gasoline for a rather long time than to end business soon. Nevertheless, for reasons of completeness, we additionally implement a finitely repeated NREG-Treatment, called NREG(FI). The classification of all treatments can also be understood from Table 3.3.

		Choice of Action	
		Limited	**Unlimited**
Duration of the game	**Ininite**	REG	NREG
	Finite	REG(FI)	NREG(FI)

Table 3.3: Classification of treatments

The experiment was programmed and conducted with the z-Tree application developed by Fischbacher (2007). We used neutrally worded instructions (see Appendix B.2) to avoid any indication to the gasoline market, the Austrian rule or the behavioral implications of the game. Hard terms like "regulatory", "limited actions" or "cooperation" were not used. Even participants were not referred to as firms, but simply "participants" and prices were merely "numbers". As we implemented a between-subject design, every participant took only part once in the experiment. She therefore had no knowledge of any other treatment.

3.5 Hypothesis

We base our hypotheses on the theoretical predictions of the 4×4 PD game introduced in subsection 3.4.1. As we deem gasoline markets to rather be long-lasting or even never-ending, we focus on the infinite horizon Treatments REG and NREG in Hypotheses H1 to H4. Additionally, we draw inferences from the finite horizon Treatments REG(FI) and NREG(FI) in H5 and H6.

Due to the symmetry of the game, we expect participants to coordinate to an equilibrium where they charge the same prices and split overall profits equally.

Thus, we formulate our first hypothesis:

H1: Both participants in one market are likely to charge the same prices.

Since we chose a likelihood of a continuation of the experiment ($\delta \approx 0.833$) which is higher than each critical discount factor δ of any subgame, the infinite horizon allows for multiple equilibria to emerge in REG and NREG. Now, even (4,4) may result as a subgame-perfect Nash equilibrium. Keeping in mind Hypothesis H1 and recognizing efficiency, it seems plausible for participants to rather choose the price combination (2,2) over (1,1), (3,3) over (2,2) and ultimately the price combination (4,4) over (3,3). Thus, participants ought to coordinate to the payoff dominant equilibrium (4,4). At least we expect participants to realize Pareto superiorities and hypothesize:

H2: Participants choose prices 3 and 4 more often than prices 1 and 2.

As the aforementioned logic holds for both the regulated and the unregulated market, we do not expect to observe behavioral differences.

H3: Participants in REG behave in the same way as participants in NREG.

Nevertheless, transparency in a regulated market is higher than in an unregulated market as price increases are only possible in certain rounds. Due to the restriction of actions, each participant's behavior is more predictable, which might facilitate collusive outcomes. As in the unregulated market uncertainty prevails, we hypothesize:

H4: There is less price variation in REG than in NREG.

The theoretical predictions change drastically if we look at the finitely repeated games in REG(FI) and NREG(FI). We obtain the only static Nash-Equilibrium

(1,1) by backwards induction. As in an infinitely repeated game, given our δ, even the strategy when both players continuously choose $p = 4$ constitutes a Nash equilibrium, we hypothesize:

H5: Average prices are higher in an infinitely repeated game than in a finitely repeated game.

Relating to Hypothesis H2, we formulate our last hypothesis:

H6: Participants choose prices 3 and 4 more often in an infinitely repeated game than in a finitely repeated game.

3.6 Experimental procedure

In order to test our hypotheses, we gather data from the economic experiment which we conducted at the Laboratory for Experimental Economics at Karlsruhe Institute of Technology (KIT) in August 2011. A total of 122 students across all fields of study took part in the experimental sessions. We ran eight sessions which lasted about 30 to 45 minutes. Subjects were recruited via ORSEE (Greiner, 2004). After welcoming the subjects to the laboratory, participants were randomly assigned to visually isolated computer terminals. The instructions were read aloud and all questions were answered privately. Prior to the beginning of the experiment, participants were asked to fill in a computerized questionnaire (see Appendix B.2.3) that checked their proper understanding of the instructions of each respective treatment. Unlike in REG and NREG, the number of rounds was limited to 31 rounds (30 decisions) in the REG(FI)- an NREG(FI)-Sessions. On average, subjects played 31.875 rounds. Participants' earnings per round were accumulated and paid anonymously at the end of each session. Average payments amounted to €15.96 with a minimum payment of €10.52 and a maximum of €18.49, respectively. All payments include a €5 show-up fee.

We ran the experiment with 17 groups in REG, 16 groups in NREG, 15 groups in REG(FI) and 13 groups in NREG(FI), respectively. Data from one group (2 participants) in the REG(FI)-Treatment are not part of the analysis. As one participant revealed an unintentional action taken during the session in the post-experimental questionnaire, we do not recognize the data for our results. This leaves us with 28 participants in the REG(FI) treatment and 14 groups, respectively. We take groups as independent observations and provide an overview of all treatments and sessions in Table 3.4, where we highlight the updated information for the REG(FI)-Treatment in Session 6.

treatment	REG		NREG		REG(FI)		NREG(FI)	
session	1	2	3	4	5	6	7	8
participants	16	18	16	16	16	14 **(12)**	12	14
independent observations	8	9	8	8	8	7 **(6)**	6	7
periods played	33	35	35	32	30	30	30	30

Table 3.4: Session information

3.7 Results

The conclusions which can be drawn from the REG and NREG treatment have partly been published in Berninghaus et al. (2012). We include these published results and provide further insights by analyzing the REG(FI) and NREG(FI) treatments. We present all results based on the data of the first 31 rounds. As the decision in round 0 is made automatically, we use data from rounds 1 to 30 (or rounds 5 to 30, if mentioned explicitly) to analyze descriptive and non-parametric statistics.

3.7.1 Chosen prices and initial price increases

Table 3.5 illustrates the percentage of prices chosen by all participants per treatment. Overall, we find the highest price 4 to be chosen most often in all of our treatments. The comparatively high percentage of $p = 3$ in REG and REG(FI) is due to the automated initial action, as participants were not able to choose prices above $p = 3$ for four rounds. Prices 2 and 3 are virtually not chosen in Treatment

NREG(FI). Average prices per treatment are 3.64, 3.22, 3.04 and 3.61 in Treatment REG, NREG, REG(FI) and NREG(FI), respectively.

treatment	REG	NREG	REG(FI)	NREG(FI)
price 1	6,37 %	20,94 %	24,17 %	12,31 %
price 2	2,45 %	5,52 %	6,67 %	0,77 %
price 3	11,76 %	3,23 %	9,88 %	0,77 %
price 4	79,41 %	70,31 %	59,29 %	86,15 %

Table 3.5: Percentage of prices per treatment

We find highly cooperative results when we look at price combinations chosen by participants. According to Hypothesis H1, participants are likely to charge the same prices. In 82.9% of all rounds, participants in NREG succeed to collaborate by setting the same prices. Participants in NREG(FI) succeed in 92.8% of all rounds. We even find slightly more cooperation in our regulated experimental markets. In REG, participates choose the same price in 94.1% of all rounds, so do participants in 93.8% of all rounds in REG(FI). While the Nash outcome (1,1) is played most often in REG(FI), collusion with the highest price possible prevails in NREG(FI). In Table 3.5, this information is divided by treatment. For a more detailed illustration (e.g. data by group) see Table A.1 in Appendix A.2.

treatment	REG	NREG	REG(FI)	NREG(FI)
price combination (1,1)	3,73 %	14,38 %	21,67 %	9,23 %
price combination (2,2)	0,00 %	1,25 %	5,48 %	0,26 %
price combination (3,3)	11,57 %	0,83 %	9,05 %	0,26 %
price combination (4,4)	78,82 %	66,46 %	57,62 %	83,08 %

Table 3.6: Cooperative results per treatment

Participants choose price combinations (3,3) and (4,4) in more than 2/3 of all rounds in any treatment. Actually, in the REG (which implies the behavioral features of the Austrian rule), participants cooperate in 91% of all rounds by setting prices in the upper range. According to Hypothesis 2, participants choose prices 3 and 4 more often than prices 1 and 2. The same goes for our assumption in

Hypothesis H6, where we expect participants in the infinite game to cooperate in high prices. Based on our results, we cannot reject Hypotheses H2 and H6 either.

Figure 3.2 illustrates average earnings per round. Since the price combination (4,4) is chosen predominantly in each treatment (see Table 3.5), it is not surprising to observe average earnings to manifest above the initial average earning of 22 (price combination (3,3)) at some point.

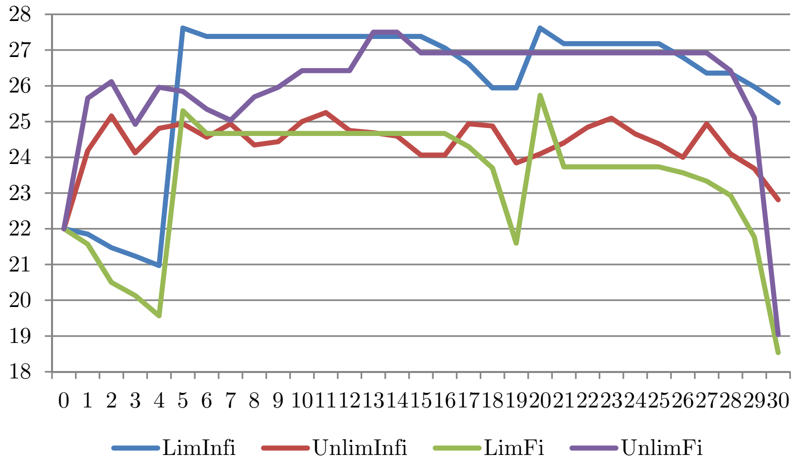
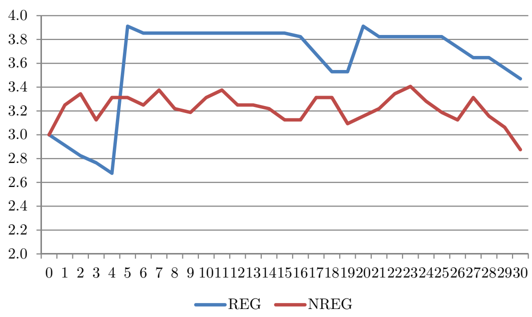


Figure 3.2: Average earnings per round

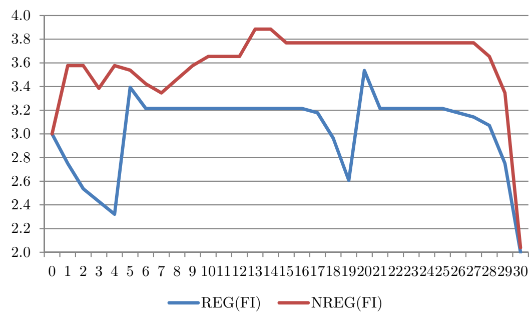
The situations where subjects' average earnings are below 22 are on the one hand the first 4 rounds in both regulated treatments REG and REG(FI) and the downturn in round 19 in REG(FI). This drop can be explained by the restrictions of the Austrian pricing rule, which in certain rounds only allows participants to set the same price as before, or any price below. On the other hand, we find a strong endgame effect in both finite treatments REG(FI) and NREG(FI), as nearly all participants switch to the Nash equilibrium outcome (1,1) towards the end of the game. Thus, the drop in earnings is somewhat self-explanatory. As for the REG- and REG(FI)-Treatment, we find a sudden and massive increase in earnings in rounds 5 and 20, which suggests that participants make use of the possibility to increase prices drastically, when the opportunity occurs. This observation seems to confirm the assumption of numerous economists, automobile associations and competition experts who claim, that a regulation according to the Austrian rule would lead to an unproportional (and maybe unjustified) increase in prices.

3.7.2 Volatility and level of prices

Separating the illustration in Figure 3.2 by the duration (finite or infinite) of the treatments and transforming average earnings into price paths, respectively, we obtain group average prices per round in Figure 3.3a and in Figure 3.3b.



(a) Infinite duration of the session



(b) Finite duration of the session

Figure 3.3: Group average prices per round

According to Hypothesis H3, participants exhibit the same behavior in REG as in NREG. We evaluate Hypothesis H3 by analyzing group average prices per round. We unambiguously find a pricing pattern in the regulated experimental markets REG and REG(FI), no matter the duration of the game. Using a non-parametric Mann-Whitney U (MWU) test, we compare the distributions of average prices per round. With regard to the infinite-horizon Treatments, we find significantly higher prices in REG than in NREG (one-sided MWU test, $p = 0.000$). The statistically significant difference still holds when we only use the data from rounds 5 to 30. Therefore, we reject Hypothesis H3. We obtain opposite results for the finitely repeated game, where prices in NREG(FI) are significantly higher than in REG(FI) (one-sided MWU test, $p = 0.000$).

We take a look at the respective standard deviation σ in Table 3.7 to analyze price volatility and thereby evaluate Hypotheses H4, according to which we expect less price variation in REG than in NREG.

	REG	NREG	REG(FI)	NREG(FI)
σ	0.81	1.24	1.27	1.00

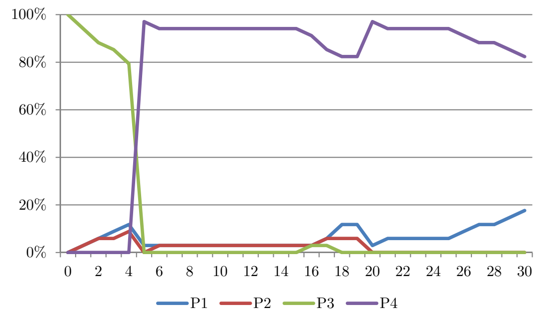
Table 3.7: Standard deviations of prices per treatment

As we take into account all chosen prices, we find prices in Treatment REG(FI) to be most volatile. NREG also exhibits high price volatility with $\sigma = 1.24$. Figures A.3 to A.6 in Appendix A.2 illustrate group average prices per round for all treatments conducted. If we compare Figures A.4 and A.5, it clearly shows that the volatility in REG(FI) can simply be attributed to the (extremely) distortive price drops and increases while still a pricing pattern emerges in every group. A regulation in line with the Austrian rule seems to hinder price decline until shortly before the next possibility to increase prices. Pricing in NREG, however, is constantly changing throughout all rounds which is due to the wide range of actions and the uncertainty about other participants' actions that comes along. Price volatility in Treatments REG and NREG(FI) is moderate with $\sigma = 0.81$ and $\sigma = 1.00$. Notice that prices are least volatile in the REG-Treatment, which implies the behavioral features of the Austrian rule in an infinitely repeated game. As we observe strong price fluctuations in the regulated markets particularly in rounds shortly before an increase in prices is possible, whereas we observe fluctuations through all rounds in our unregulated markets, Hypothesis H4 cannot be confirmed.

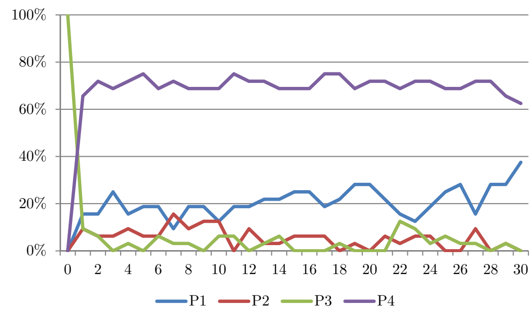
The results presented in previous paragraphs strikingly show the importance of the duration of the game. The horizon does matter for our results and plays a crucial role for the outcome of our experiment: Firstly, average prices per round in the infinite regulated Treatment (REG) are significantly higher as in the finite regulated Treatment (REG(FI)) (one-sided MWU test, $p = 0.000$). This result is solely due to the introduction of a finite horizon, where participants behave more cautiously in our regulated experimental market. We may deduce this conclusion from Figure A.5 on page 98. There is always a certain percentage of participants who choose the price $p = 1$ in REG(FI). Secondly, after a massive increase in prices in the regulated market of the infinite game (Figure A.3), prices in REG on average remain above the average prices chosen in the infinite unregulated market NREG. The opposite holds for the game with a finite duration. Here, average prices in the unregulated market are always above prices chosen in the regulated market. As the results from the finite-horizon Treatments might seem somewhat surprising at first, we address this outcome in the second part of the following subsection.

3.7.3 Endgame effects and (un)certainty

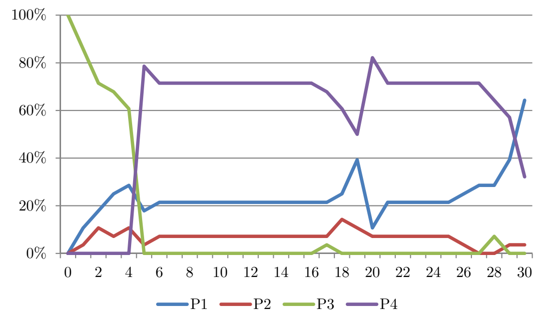
Slightly altering the plot and further splitting up both Figure 3.3a and Figure 3.3b, we obtain the percentages of chosen prices per round for each of our four treatments in Figures 3.4a to 3.4d.



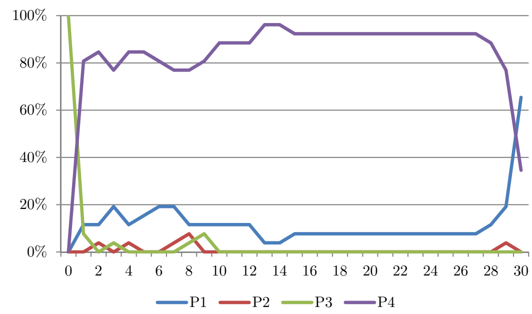
(a) REG



(b) NREG



(c) REG(FI)



(d) NREG(FI)

Figure 3.4: Percentage of chosen prices per round

The illustrations clearly show that participants immediately choose higher prices, as soon as they are given to possibility to do so in a regulated market. Prices then remain on a high level for a rather long time before decreasing towards the next possibility to increase prices in round 20. We find a strong endgame effect in

REG(FI). Likewise, we find a reduction in average prices in REG although the experiment continues with a probability of 80%. As the experiment lasted longer than 35 rounds in one REG-session, we are able to provide further evidence by analyzing subject behavior in rounds 34 and 35. With a total of 8 independent observations, 72% of all participants chose $p = 4$ in round 34. So did 89% in round 35, when an increase in prices was possible once again. This result strongly suggests that this pattern continues to emerge in longer experiments.

As mentioned above, we find average prices in the unregulated market to be always above average prices in the regulated market when the game is played finitely (see Figure 3.3b). This result is somewhat surprising, yet, it is not uncommon. There is a large amount of experimental literature investigating cooperation in finitely repeated PD games (see Andreoni and Miller (1993), Dal Bó (2005) and Normann and Wallace (2012)). In an early contribution, Kreps et al. (1982) report on experimental evidence that (contrary to theoretical predictions) finds subjects to being able to achieve some measure of cooperation, even if the game has an unique Nash equilibrium path (defection in each round of a finite $2{\times}2$ PD game). Their conclusion is as simple as it is plausible. Since there is incomplete information about the types of players, initial cooperation can be consistent with rational behavior. Adopting a Tit-for-Tat strategy, for example, may result in subjects successfully playing the collusive outcome for at least some part of the game. This so called sequential equilibrium reputation hypothesis has been investigated experimentally by Andreoni and Miller (1993). The authors find subjects to be rather altruistic as cooperation prevails until the first defection, even if the end of the game nears. While the price-volatility in the first 15 rounds of our NREG(FI)-Treatment might be considered as a sequential equilibrium learning process as suggested by Selten and Stoecker (1986), the results from our finitely repeated treatments appear to be in line with the findings by Andreoni and Miller (1993).

To finally evaluate Hypothesis H5, we take as independent observations average market prices over all 30 rounds per group. According to Hypothesis H5, average prices are higher in an infinitely repeated game than in a finitely repeated game. Having a closer look at the cumulative distribution functions (cdf) of prices in Figure 3.5 partly confirms this assessment.

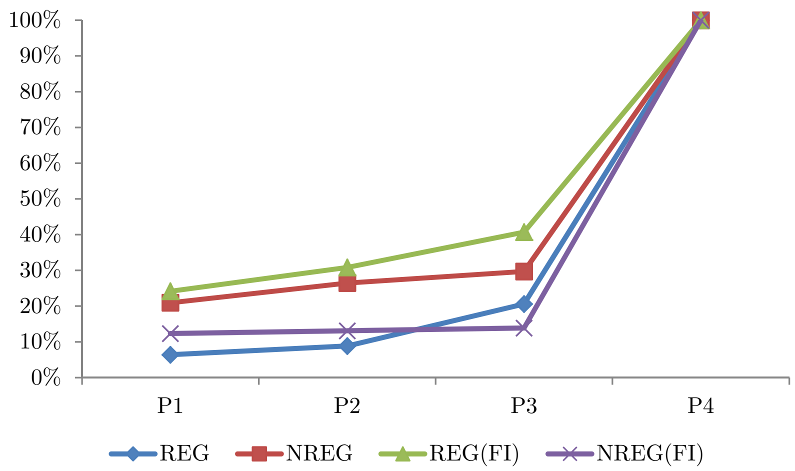


Figure 3.5: Cumulative distribution function of prices

The cdf of prices in REG holds first-order stochastic dominance over the cdf of prices in REG(FI) (one-sided MWU test, $p = 0.0115$). This result appends to the analysis of average prices per round in subsection 3.7.2. We find them to be higher in REG with a p-value of 0.000 (one-sided MWU test). Thus, we cannot reject Hypothesis H5. However, we do not find statistically significant results comparing NREG and NREG(FI) as we obtain a p-value of 0.361 (one-sided MWU test).

3.8 Conclusion

The skepticism of numerous governmental organizations and competition experts towards the implementation of a price regulatory intervention along the lines of the Austrian rule in Germany seems to be justified. Based on the results of our experiment, the Austrian rule tends to facilitate collusion as we find significantly higher prices in the regulated market (REG) compared to prices in the unregulated market (NREG). The absence of regulation appears to make collusion comparatively difficult. The variety of prices chosen (see Figure A.4 on page 97) suggests the insecurity of each subject, based on the uncertainty about the opponent's strategy. Analogous, the regulated market yields less volatile prices. This observation can be attributed to the increased level of transparency that comes along with the price-regulatory mechanism. By regulating the market and thereby once allowing an increase in prices at a specific time, the strategy of any player is more predictable as the choice of action is restricted in the majority of all rounds.

We additionally provide results from two finite horizon treatments. Contrary to the theoretical prediction, we find highly cooperative outcomes in the unregulated market. This result is likely to be driven by the sequential equilibrium reputation hypothesis. Like in REG, we find a massive increase in prices as soon as the opportunity arises in REG(FI). No matter the time horizon, the level of prices is then successfully kept upright until a sudden decrease shortly before the next possibility to increase prices again.

In the view of these facts, we expect a price-regulatory mechanism in the sense of the Austrian rule to be associated with a reduction of welfare: prices appear to be higher. Thus, the intervention would be counterproductive if maximizing welfare was the aim of the regulation. On the other hand, one might confirm its functionality if a reduction of price changes was intended. Then, the loss in welfare would stand for the costs of bringing forth less volatile prices. One would need to judge whether the utility gained from the implementation outweighs its costs, or not.

4 Ambiguity, Induced by Strategically Investigating Authorities

4.1 Introduction

The economic literature on law enforcement provides important insights into how rules need to be established to effectively work against offenders. By threatening potential criminals with penalties, a norm is set up, signalizing, that infringements are not tolerated and will be punished. This general deterrence is the most basic tool for any authority. Yet, misbehavior is still prevalent. One among other reasons is that offenders do not think they are going to get caught. For them, there is a trade-off between gains for offending and fines weighted with the probability of detection.

Regarding the question whether to commit a crime or to engage in any kind of illegitimate activity has always been subject to the risk of getting caught. Since the analysis of Becker (1968) on optimal law enforcement, numerous subsequent models provide a solid theoretical basis on the trade-off between probability and severity of punishment (see Posner (1985) for an overview of the related literature on law enforcement). However, most seminal theoretical models use a fixed probability for authorities to detect misbehavior (Becker and Stigler, 1974; Ehrlich, 1973; Polinsky and Shavell, 1979).

Lately, an increasing number of contributions focus on specific areas of crime using endogenous detection probabilities. In those models, the probability of detection depends on the proportion of corrupt officials (Lui, 1986), the level of agreed upon cartel prices (Harrington, 2008) or the amount of reported income (Allingham and Sandmo, 1972; Sandmo, 2005).

To further analyze decision-making regarding misbehavior and deterrence in legal settings, experimental methods of investigation have become increasingly popular. For example, experimental evidence on corruption is provided by Abbink et al. (2002). They find a significant decrease in corruption due to the threat of penalties. Hinloopen and Soetevent (2008) analyze the effect of fines, leniency and rewards on cartel formation and market prices in a Bertrand pricing game. With a focus on taxes, Anderhub et al. (2001) observe that higher income encourages evasion while higher tax rates do not. Those studies have one thing in common: All of them apply a computerized mechanism with a constant probability of being detected to represent an authority institution. Yet, very recent experiments on tax evasion endogenize detection probabilities. Coricelli et al. (2010) make the probability of an audit depend on the median income report of the group. In the tax experiment conducted by Kleven et al. (2010), the probability of detection is a decreasing function of reported income. However, none of them have an explicit role for an authority in the experiment.

Hinloopen and Soetevent (2008) and Haan et al. (2009) give rise to the question whether authority institutions are represented appropriately in economic experiments. They suggest to model the role of an authority separately and to endogenize detection probabilities in experiments. We support this view as in practice authorities do not decide to start investigations by an exogenously given probability. Instead, they base their decisions on experience and history as caught offenders for example are monitored more thoroughly in the future and some industries (or individuals) are perceived to be more likely to engage in illegitimate activity than others. Simply put, authorities act strategically.

With fines and gains either being known or expected, the probability of detection is always perceived individually. The presence of and the general awareness about an authority actively monitoring the environment are two relevant factors of influence. Therefore, we will investigate decision-making with regard to the degree of uncertainty in the presence of an authority being able to act strategically. By endogenizing the probability of detection, we influence the perceived probability of detection and observe a significant decrease in the prevalence of misbehavior.

Cognitive psychology suggests several heuristics and biases (Tversky and Kahneman, 1974) when judging the likelihood of uncertain outcomes. A perceived probability can therefore differ largely from a given objective probability. Kahneman (1972) introduces the representativeness heuristic. With judgment by representativeness, the decision maker's perceived probability is influenced by the degree of correspondence between occurrence and the generic feature of the underlying process.

According to the distinction of Knight (1921), decision makers in risky situations know an objective probability for every possible outcome. Under uncertainty, decision makers cannot assign any numerical probabilities. Following the ambiguity concept by Ellsberg (1961), decisions depended on (1) a perceived probability and on (2) the vagueness of the event in question. In other words, people rely their judgment on the reliability, credibility, or adequacy of the information they have (Ellsberg, 1961). Hence, „ambiguity is uncertainty about probability, created by missing information that is relevant and could be known.“(Frisch and Jonathan, 1988). An analogous definition is provided by Eichberger and Kelsey (2009):„[..] where some or all of the relevant information about probabilities is missing“. Ambiguous situations are therefore neither characterized by risk nor by complete uncertainty.

Tversky and Wakker (1995) affiliate decision-making not only with the degree of uncertainty, but also with the source of uncertainty. Empirical and experimental evidence for this claim is provided by Abdellaoui et al. (2011) as they take into account different sources of uncertainty in an Ellsberg-like urn experiment. Changing the source of uncertainty can create some kind of ambiguity if a decision maker does only have limited knowledge about the process that generates the outcome (Einhorn and Hogarth, 1985). This makes the concept similar to the application of the representativeness heuristic by Kahneman (1972), where people judge the probability or frequency of an event by the characteristics of the process by which it was generated.

By implementing an authority with strategic selection by human choice instead of random selection, the situation any decision maker faces is no longer only risky. All decision makers are required to judge under ambiguity. In contrast to random detection, the intended strategy, the decision-making process, of a human authority is unknown to all other players and probabilities are hard to determine. Although given the same objective probability of being selected in both cases (e.g. one out of X decision makers will be investigated, only the mechanism differs), this new situation is neither subject to risk, nor to complete uncertainty. Hau et al. (2010) recognize the influence on risky choices and differ between (1) a priori probabilities and (2) statistical probabilities. While numerical values are known in (1), probabilities cannot be calculated exactly in (2). Yet, they can be assessed empirically. This creates the ambiguous situation.

Considering these arguments, we investigate decision-making with an authority being present in an economic experiment and analyze behavioral differences between random selection and selection by direct choice. Despite of the diversity of all offenses mentioned above, we will abstain from focusing on a specific area of crime. Ehrlich (1973) describes the essential common properties of participation in illegitimate behavior as an increase of the offender's wealth, the psychic well-being, or both. Detection on the other hand results in a severe reduction of these attributes. This basic view of misbehavior will be kept throughout this chapter. Due to the risk of detection, participation in illegitimate behavior requires decision-making under uncertainty. Since we only focus on the native risk of getting caught, we abstain from external effects to third parties, leniency-programs of any kind, the possibility of whistleblowing and administrative or transaction costs for either offenders or authorities.

As a result we find that the presence of a strategically acting authority positively affects deterrence and due to the increase of the degree of uncertainty decreases the prevalence of misbehavior significantly.

4.2 Game design

In this section we first describe the stages of our game. We then calculate formal predictions concerning possible outcomes of the game before we have a closer look at the influence of strategy profiles on the probability of successful detection p.

4.2.1 Stages of the game

The following stages represent the respective steps for one of $n = |I|$ decision makers or an authority during one period in our repeated game.

		Alternative A		Alternative B	
(1)	Decision makers´ temporary payoff	600		1000	
(2)	Authority conducts an investigation	Yes	No	Yes	No
(2.1)	Probability of successful detection	p	-	p	-
(3)	Decision makers´ payoff	600	600	0	1000
(3.1)	Authority´s payoff	0	0	1000	0

Table 4.1: Stages of the game

Stage 1 *(decision maker):* Each decision maker chooses between a certain alternative (Alternative A with payoff 600) or a risky alternative (Alternative B, generating the payoff 1000 if the investigation is not successful with probability $(1 - p)$ or the payoff 0 if the investigation is successful with probability p). After choosing, all decision makers receive the respective amount for the moment. Choices are private information and unknown to all other players.

Stage 2 *(authority):* The authority now chooses whether to investigate one decision maker to reveal her alternative choice, or not.

- If the authority decides against an investigation, all decision makers keep the payment received in Stage 1. The authority receives nothing and the game continues with Stage 3.
- If the authority decides in favor of an investigation, one decision maker is selected, the suspect. All other decision makers, i.e. all decision makers but the suspect, keep the payment according to their choice in Stage 1. The investigation of the suspect is successful with probability p. In case of an unsuccessful investigation the suspect keeps the payment received in Stage 1 and the authority receives nothing. Otherwise, the authority finds out the alternative chosen by the suspect in Stage 1. If the suspect chose Alternative A, she can keep the reward. If the suspect chose Alternative B, she loses the

risky payoff and ends up with nothing. The authority then receives the lost amount (1000) and the game continues with Stage 3.

Stage 3 *(payment and feedback)*: After (final) payments are carried out, every player is finally given the information (1) whether the authority decided to investigate (2) the player that was selected (3) success or failure of the investigation and (4) the individual payoff.

See Table 4.1 for an overview of the game. In summary, while the decision maker choosing the risky alternative, Alternative B, can lose her payoff upon successful investigation, she always keeps the payoff when choosing the certain alternative, Alternative A. The authority has an incentive to always conduct an investigation.

4.2.2 Formal predictions

For our theoretical analysis, we focus on decision makers only. We do not formally represent the authority in our model. We assume a set of I risk-neutral players, i.e decision makers. The number of players is $n = |I|$. We assume that per period, only one decision maker can be selected for investigation. Hence, the objective probability of selection results in $\delta = \frac{1}{n}$. In Stage 1 of the game, every player i chooses her strategy $\sigma_i \in \Sigma_i$ with $\Sigma_i = \{'A';'B'\}$. Further, we assume that the probability of successful detection $p(\sigma)$ depends on the strategy profile all decision makers play. We closer analyze this aspect in subsection 4.2.3.

The payoff function is given by $H_i(\sigma_i, \sigma_{-i})$. As the payoff from choosing the sure Alternative A can be kept in any case, the player receives 600 as shown in formula 4.1.

$$H_i('A', \sigma_{-i}) = 600 \tag{4.1}$$

The payoff for choosing the risky Alternative B is

$$H_i('B', \sigma_{-i}) = (1-\delta)1000 + \delta[p(\sigma) \times 0 + (1-p(\sigma)) \times 1000] \tag{4.2}$$

In formula 4.2 we obtain the expected payoff considering the two scenarios when decision maker i is not selected for investigation with probability $(1-\delta)$ as well as the case in which the decision maker is selected for investigation with probability

δ. After being selected, the probability $p(\sigma)$ determines wether the investigation is conducted successfully, or not $(1 - p(\sigma))$.

Player i chooses Alternative A, if $H_i('A', \sigma_{-i}) > H_i('B', \sigma_{-i})$. Transformation of this inequality results in

$$p(\sigma) > \frac{2}{5\delta} = 0.4n$$

Hence, in a game with three or more risk neutral decision makers, choosing Alternative B is better than choosing Alternative A.

4.2.3 Influence of strategy profile on p

In this subsection, we consider two different methods to calculate $p(\sigma)$: (1) $p(\sigma)$ is exogenously given and (2) $p(\sigma)$ depends on the number of participants playing the risky strategy, i.e. Strategy B. We call the game in line with (1) Basic Game and assume that $p^B(\sigma) = 1$ holds for the Basic Game, while we call (2) Extended Game.

Introducing the Extended Game is motivated by Lui (1986). There, the probability of successful detection $p(\sigma)$ monotonically decreases with the number of decision makers choosing Alternative B. This reduces the likelihood of an authority auditing the suspect successfully because of the greater prevalence of misbehavior. Lui (1986) uses corruption by way of example, as do we. Formally, we let $p^E(\sigma)$ in the Extended Game depend on the number of decision makers $i \in \{1, .., n\}$ who have chosen Alternative B in Stage 1 of the game. The probability of successful detection is:

$$p^E(\sigma) = 1 - 0.05 \times (i - 1) \qquad \text{for } i \in \{1, .., n\} \text{ and } n \leq 21$$

In case all decision makers have chosen Alternative A (hence $i = 0$ players chose Alternative B), a search is conducted with $p^E(\sigma) = 1$ anyway.

Results can differ between Basic Game and Extended Game as the probability of successful detection depends on the other players' alternative choices in the latter scenario. Assume in a game with 21 decision makers, all decision makers choose the risky Alternative B. Then, even the investigated player will receive 1000 since no investigation is successful with $p^E(\sigma) = 0$. With an increasing number of players choosing Alternative A, the payoff decreases until only one decision maker (i.e. player i) solely chooses Alternative B. In this case, the probability of successful detection $p^E(\sigma)$ is 1. Figuratively speaking, player i is the only corrupt player in the population and therefor easy to detect for an authority.

Consider that being the only one choosing Alternative B results in equivalent formal predictions as for the Basic Game, as $p^E(\sigma) = p^B(\sigma) = 1$ holds. All other players $\{1, .., i-1, i+1, .., n\}$ receive the sure payoff 600. If the game is played with three or more decision makers, we can see that the risky Alternative B is more attractive in the Extended Game than in the Basic Game. The expected payoff from choosing the risky Alternative B in the Extended Game then is always higher or at least equal to the expected payoff of the Basic Game for any decision maker $i \in \{1, .., n\}$ with $n \leq 21$. We obtain this formal result although the situation itself is more ambiguous as every decision maker lacks necessary information as compared to the Basic Game. The other players' alternative choices are unknown but crucially relevant for the individual payoff in the Extended Game. In the Basic Game, this information is not needed.

4.3 Hypotheses

Introducing the role for an authority into an economic experiment allows us to implement different detection mechanisms while maintaining the same exogenously given probability of suspect selection $\delta = \frac{1}{n}$. With our mechanisms for suspect selection either being (1) random or (2) by direct choice of a human authority, we vary the degree of uncertainty. Whereas random selection with a fixed probability is a risky situation, selection by an authority is perceived to be ambiguous. Implementing those two selection mechanisms creates two comparable scenarios. In (1), suspect selection is computerized and random whereas in (2) the human authority chooses directly by a certain strategy. We are able to observe and analyze behavioral aspects regarding decision-making under risk (random selection) and decision-making under ambiguity (selection by choice).

Allais (1953) and Ellsberg (1961) challenged expected utility for decision-making under uncertainty. Since then an increasing amount of literature on behavioral aspects regarding risky, ambiguous and uncertain events has emerged. Camerer and Weber (1992) provide an excellent overview of subsequent studies on Daniel Ellsberg's findings and Wakker (2010) reviews commonly applied theories of choice. In case of uncertain events Abdellaoui et al. (2011) and Tversky and Wakker (1995) point out that not only the degree, but also the source of uncertainty matters for decision makers. With respect to this source dependence, we hypothesize:

H1: The underlying selection mechanism affects the decision makers' choices.

The concept of ambiguity aversion originates from the experiments of Ellsberg (1961) in which decision makers prefer bets on events with known probabilities. Camerer and Weber (1992) also suggest that missing information is upsetting and scary and therefore makes people prefer risk (known probabilities) to ambiguity (unknown probabilities). As the amount of missing information can be understood as the degree of ambiguity (Camerer, 1999) we formulate two hypotheses:

H2a: The ratio of risky choices is lower in situations that are subject to ambiguity.

H2b: The higher the degree of ambiguity, the lower the ratio of risky choices.

In both, Hypothesis H2a and H2b, we increase the degree of ambiguity. However, the initial situation differs. While we have a risky (unambiguous) situation in H2a to begin with, Hypothesis H2b postulates some level of ambiguity to be already in place.

4.4 The experiment

In this section, we first present the experimental design to introduce our four treatments. Then we provide some background information about the experimental procedure in subsection 4.4.2.

4.4.1 Experimental design

We chose treatment-specific variables with regard to the detection mechanism. We implement an overall of four treatments which differ (1) in the way suspects are selected and (2) whether the probability of successful detection is determined by the strategy profile of all decision makers (strategic uncertainty), or not. The suspect selection mechanism of a participant in the role of the authority is either "random" (RANDOM) or "by choice" (BY CHOICE). Therefore, the differences between the treatments are as follows:

- RANDOM: In case the participant representing the authority decides to have an insight into the alternative choice of one decision maker, the suspect is selected randomly by a computer.
- BY CHOICE: Here, upon her decision to conduct a search, the participant representing the authority chooses one decision maker directly.

The difference between the subject selection mechanisms is embedded in the instructions by only changing the sentence "The subject is selected randomly." in (RANDOM) to "The subject is chosen by the authority directly." in (BY CHOICE) (see Appendix B.3 for instructions).
The influence of strategic uncertainty is reflected in differences between the treatments as follows:

- Without strategic uncertainty (RANDOM or BY CHOICE): The investigation of every selected suspect is successful with probability $p^B(\sigma) = 1$. Every decision maker who chooses the risky Alternative B and is investigated receives 0. In other words, these treatments represent the Basic Game.
- With strategic uncertainty (RANDOMgroup and BY CHOICEgroup): After a suspect is selected for investigation, the success of the investigation depends on the number of other decision makers choosing Alternative B, i.e. $p^E(\sigma)$. These treatments represent the Extended Game.

That leaves us with four treatments (see Table 4.2 for an overview). According to the classification distinctions for decision-making under uncertainty, we categorize the treatments as shown in Table 4.2 as static or dynamic and as risky or ambiguous.

<table>
<tr><td colspan="2" rowspan="2"></td><th colspan="2">structure of the game [group dependence]</th></tr>
<tr><td>BASIC</td><td>EXTENDED</td></tr>
<tr><th rowspan="2">suspect selection mechanism [authority]</th><td>RANDOM</td><td>RANDOM
static|risky</td><td>RANDOMgroup
dynamic|ambiguous</td></tr>
<tr><td>BY CHOICE</td><td>BY CHOICE
static|ambiguous</td><td>BY CHOICEgroup
dynamic|ambiguous</td></tr>
</table>

Table 4.2: Classification of treatments

By "static" we describe the decision-making independent from other decision makers in the group ($p^B(\sigma) = 1$) and with "dynamic" we capture the dependence on the other decision makers for the generation of $p^E(\sigma)$. Given the known probability of selection in RANDOM, this is a risky situation. Handing over the subject selection to the authority in BY CHOICE, the decision is no longer made probabilistically and the situation cannot be expressed by a probability in the sense of Knight (1921). However, the likelihood of being selected is (objectively) equally probable with $\delta = \frac{1}{n}$. Both situations exhibit ambiguity over *outcomes* but changing the underlying selection mechanism results in some missing information in BY CHOICE: The strategy of the authority. Therefore, BY CHOICE exhibits ambiguity over *probability*. These degrees of ambiguity are fundamentally different (Camerer and Weber, 1992). In contrast to the treatments of the Basic Game, RANDOMgroup and BY CHOICEgroup both have a selection mechanism that passes through two stochastic processes. First, the selection probability $\delta = \frac{1}{n}$ and second, the probability of successful detection $p(\sigma)$, which depends on the strategy profile of all decision makers. Hence, decision makers also have to form beliefs about the other decision makers' actions, as they are relevant for the individual expected payoff. In addition to this strategic uncertainty, selection by direct human choice in BY CHOICEgroup raises the degree of ambiguity yet again.

Having a closer look at Table 4.2, the results of the experiment can be compared regarding ambiguity in the way subjects are being selected by the authority (RANDOM versus BY CHOICE) and the additional increase of uncertainty (Basic Game versus Extended Game) for each two treatments.

4.4.2 Experimental procedure

As the application of our research focus is not necessarily bound to one specific setting of illegitimate activity, we run an experiment using neutrally worded instructions. Thereby, we are also able to abstain from moral standards and focus on behavioral aspects only.

The experiment was conducted at the Laboratory for Experimental Economics at Karlsruhe Institute of Technology (KIT). There were 9 groups of 7 participants in each of the 4 treatments. Thus, one group consisted of one authority and $n = 6$ decision makers. Subjects were recruited via ORSEE (Greiner, 2004) and a total of 252 students across all fields of study took part in the experiment. Earnings were recorded in ECU (experimental currency unit) with a conversion rate of ECU100 being €0.05. On top of their cumulative earnings, each participant was rewarded a €5 show-up fee. Students were paid anonymously at the end of each session. Experimental sessions lasted about 30 minutes and average earnings were €10.98. Maximum and minimum payments were €12.5 and €7, respectively. The experiment was conducted under laboratory conditions. Upon arrival, participants were randomly assigned to visually isolated computer terminals and matched to groups with 6 other participants for all 15 periods. The instructions were read aloud and all questions were answered privately. The experiment was programmed and conducted with the z-Tree application developed by Fischbacher (2007).

4.5 Results

To analyze our research hypotheses we focus on the average share of risky Alternative B choices per group (see Table 4.3).

First, we compare treatments with different underlying selection mechanisms and study whether this difference in the source of uncertainty influences the players' behavior (Hypothesis H1). Then we evaluate Hypothesis H2a and H2b as we investigate the ratio of risky Alternative B choices with respect to the degree of uncertainty.

observation/ group	RANDOM	BY CHOICE	RANDOM group	BY CHOICE group
1	84.4%	66.7%	74.4%	55.6%
2	90.0%	53.3%	60.0%	77.8%
3	73.3%	37.8%	78.9%	75.6%
4	88.9%	71.1%	65.6%	56.7%
5	58.9%	64.4%	73.3%	58.9%
6	88.9%	75.6%	67.8%	55.6%
7	94.4%	54.4%	82.2%	74.4%
8	72.2%	70.0%	55.6%	74.4%
9	76.7%	74.4%	67.8%	61.1%
ø	80.9%	63.1%	69.5%	65.6%

Table 4.3: Relative shares of Alternative B choices per group

The average shares of Alternative B choices in Table 4.3 can also be understood from Figure 4.1.

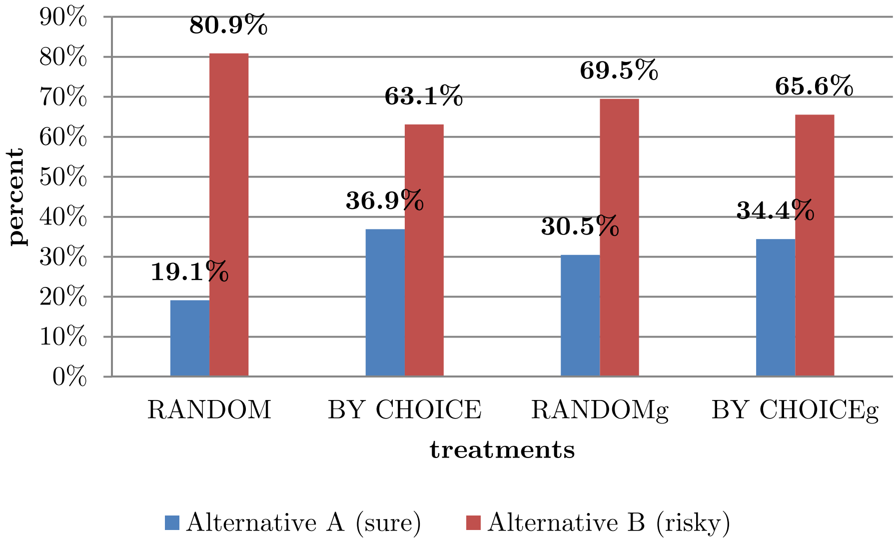


Figure 4.1: Alternative choices

4.5.1 Changing the selection mechanism

According to our research Hypothesis H1 we expect the underlying selection mechanism to affect the decision makers' choices. Having a closer look at the results of the Basic Game (Treatment RANDOM and Treatment BYCOICE) in Table 4.3, we observe that the average share of participants playing the risky choice (Alternative B) is 17,8% lower in Treatment BY CHOICE than in Treatment RANDOM. Apparently, the suspect selection mechanism does influence the players' behavior. A two-sided Mann-Whitney U (MWU) test confirms on a significance level of 1% ($p = 0.007$) that the average choice of risky Alternative B in Treatment RANDOM is different from the average choice in Treatment BY CHOICE.

Comparing both treatments with strategic uncertainty (Treatment RANDOMgroup and Treatment BY CHOICEgroup) we also observe a reduction in choices of risky Alternative B. A binomial test confirms the significance of the reduction (one-sided, α=0.1). With regard to Hypothesis H1 we conclude that there is a difference between random suspect selection and selection by direct choice of an authority. However, the overall decrease of 3.9% in average choice of the risky Alternative B in the Extended Game is rather small compared to the same effect (an overall reduction of 17.8%) in the Basic Game.

4.5.2 Raising the degree of ambiguity

According to our research Hypothesis H2a, the ratio of risky choices is lower in situations that are subject to ambiguity. To analyze this hypothesis we take into account the treatments where ambiguity is introduced. On the one hand we compare Treatment RANDOM and Treatment BY CHOICE because of the ambiguity due to change in the selection mechanism (see subsection 4.5.1). On the other hand, we compare Treatment RANDOM and Treatment RANDOMgroup. Here, having random selection in both treatments, Treatment RANDOMgroup exhibits ambiguity due to strategic uncertainty. We find a significant decrease in risky choices (Alternative B) in both cases (RANDOM/BY CHOICE: one-sided MWU test, $p = 0.0035$; and RANDOM/RANDOMgroup: one-sided MWU test, $p = 0.0235$). These results confirm our Hypothesis H2a.

Participants in Treatment RANDOMgroup show a significantly higher security orientation than in participants in Treatment RANDOM (one-sided MWU test, $p = 0.0235$) with an overall reduction of more than 11% in risky choices. The suspect selection mechanism is the same (random selection). However, the probability of getting caught in Treatment RANDOMgroup is always lower or equal to Treatment RANDOM, regardless of the number of participants choosing the risky Alternative B. Despite this fact, a smaller fraction of participants chooses the risky Alternative B in every round (see Figure 4.2). Because of that, we ascribe this outcome to the increased degree of ambiguity due to strategic uncertainty in Treatment RANDOMgroup.

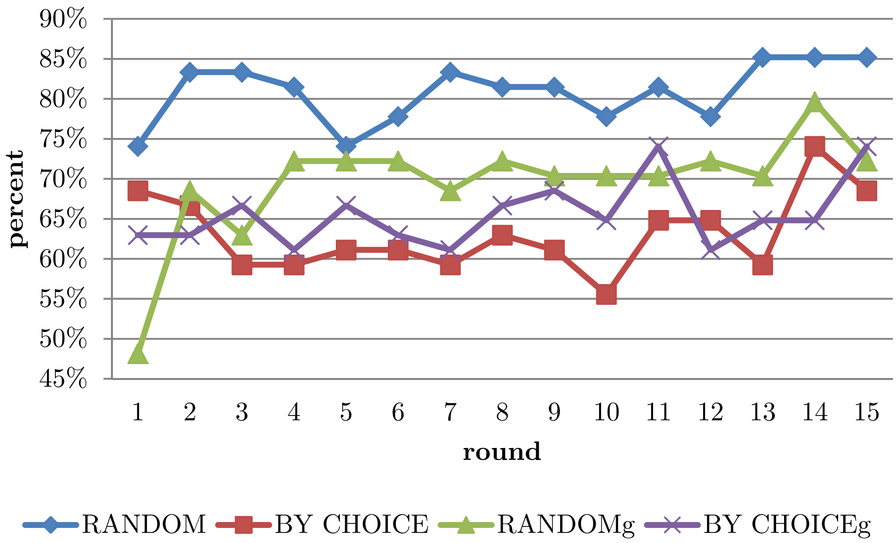


Figure 4.2: Alternative B choices per round

A recent study by Serra (2012) on the combination of top-down and bottom-up accountability in a bribery lab experiment shows similar results. Serra (2012) compares treatments with (1) no monitoring, (2) top-down auditing (only) and (3) a combined accountability system (top-down and bottom-up). Contrary to her prediction, she finds no statistically significant differences between (1) and (2) but between (1) and (3). She suggests conjunction fallacy in probability judgement (Tversky and Kahneman, 1983) as a possible reason for the outcome. While Serra (2012) finds these results for small probabilities of being detected (0% to a maximum of 4% in Treatment (2) top-down auditing), our results provide evidence for the same effect with rather high probabilities of detection (12.5% to a maximum of 16.7% in Treatment RANDOMgroup).

Finally we evaluate Hypothesis H2b. As we further increase the degree of ambiguity, we expect to see an increasingly lower ratio of risky choices (Alternative B). By making the probability of successful detection depend on the strategy profile of all decision makers in Treatment RANDOMgroup and Treatment BY CHOICEgroup, we raise the degree of ambiguity since less information is available than in Treatment RANDOM and Treatment BY CHOICE, respectively. Treatment BY CHOICE exhibits ambiguity about probability as the decision makers lack information about the authority's intended strategy. Treatment RANDOMgroup exhibits ambiguity due to strategic uncertainty through dynamic decision-making. In either case, the degree of ambiguity is still lower than in Treatment BY CHOICEgroup, where both pieces of information are missing. Comparing these three treatments we do not find statistically significant behavioral differences (BY CHOICE/RANDOMgroup/BY CHOICEgroup: two-sided MWU test, $p > 0.1$). Apparently, increasing uncertainty further does not affect the players behavior, if yet a certain degree of uncertainty is already prevalent. Therefore, we reject Hypothesis H2b.

4.6 Conclusion

Our study contributes to the experimental literature on decision-making under uncertainty by taking into account the explicit role of an authority in an experimental design. By applying different detection mechanisms, we are able to (1) vary the source of uncertainty and to (2) implement different degrees of uncertainty. Comparing decision-making in risky situations to decision-making under ambiguity, we find significant differences in the observed behavior. These results are consistent with the idea of ambiguity-aversion (Ellsberg, 1961) where people decide in favor of events with "better" known probabilities. While in Ellsberg (1961) the ambiguous situation is created by missing information about the composition of the ambiguous urn, we use a different approach to analyze decision-making under ambiguity. We model an explicit role in the experimental design and have a strategically acting authority present. Although the objective probability of detection is common knowledge, the intended strategy of investigation is unknown to all decision makers and generates the ambiguous situation.

Introducing ambiguity, we find behavioral differences when the underlying suspect selection mechanism is randomized (RANDOM vs. RANDOMgroup). Here, even objectively better alternatives turn out to be less desired by the experimental subjects. Increasing the degree of ambiguity further, we do not find behavioral differences regardless of the source of uncertainty (BY CHOICE and RANDOMgroup vs. BY CHOICEgroup). This suggests that an even higher degree of ambiguity has only little to no influence on decision makers' behavior if yet a certain level of uncertainty is already in place. With random detection being the treatment where misbehavior is most prevalent in our experiment, we appreciate the advantages of endogenizing detection probabilities in new theoretical and experimental contributions studying deterrence mechanisms.

5 Summary and Conclusion

In this thesis, we provide new evidence on three specific policy and regulatory issues by means of economic experimentation. Based on our results, we are able to gain further insights in the areas of (1) competition policy design, (2) price regulation and (3) decision-making under ambiguity in an IO context.

We run a cartel simulation in Chapter 2 where we analyze the effects of discriminating cartel ringleaders from leniency application. By implementing different leniency programs, we find that recognizing cartel ringleaders for full immunity from fines significantly deters cartel formation, increases instances of reporting and ultimately leads to lower market prices. Thus, our results suggest that antitrust policy should be designed to make cartel ringleaders eligible for leniency application. Since we find similar results for either a leniency program with immunity from fines or a 50% fine reduction, the latter mechanism appears to be sufficient to accomplish the objectives and, therefore, should be favored over granting immunity upon reporting. In case of a 50% reduction in fines, even a ringleader is given an incentive to report the infringement, while, the offender does not get off completely unpunished.

In Chapter 3 we have a closer look at the functioning of a specific price-regulatory mechanism, the so called Austrian rule. Austria adopted the revised Gasoline Price Regulation Act which came into effect on January 1, 2010. Since then, increases in gasoline prices by gas station operators have only been possible once a day at 12 pm (noon). Price reductions, however, are permitted at any time of the day. As the beneficial character of this rule was challenged by competition experts, we investigate the regulation scheme in an experiment. Our results suggest that the Austrian rule supports coordination and thereby facilitates collusion: Our regulated experimental market yields less volatile but significantly higher market prices compared to our unregulated experimental market. We are able to observe a distinct pricing pattern which can be attributed to the increased level of transparency, as competitors' actions are more predictable under the Austrian rule. We deem a regulatory mechanism along the lines of the Austrian rule to be counterproductive if maximizing welfare is the aim of the intended regulation.

In our third experiment (Chapter 4), we analyze decision-making under ambiguity. We introduce an explicit role for a strategically acting authority and investigate the impact on misbehavior experimentally. We implement treatments with either a random selection mechanism or selection by direct human choice, thus turning a risky situation into an ambiguous one. Based on economic literature on corruption, we extend this setup by making the risk of being detected depend inversely on the ratio of offenders. We find that the presence of a strategically acting authority positively affects deterrence, despite the same objective probability of detection compared to being randomly investigated. Due to an increase in the degree of uncertainty, the prevalence of misbehavior decreases significantly.

Bibliography

Abbink, K., Irlenbusch, B. and Renner, E.: 2002, An Experimental Bribery Game, *Journal of Law, Economics, and Organization* **18**(2), 428–454.

Abdellaoui, M., Baillon, A., Laetitia, P. and Wakker, P. P.: 2011, The Rich Domain of Uncertainty, *American Economic Review* **101**(2), 699–727.

ACCC: 2002, Terminal gate pricing arrangements in Australia and other fuel pricing arrangements in Western Australia.

ACCC: 2008, Petrol - Further Econometric Analysis Undertaken by ACCC.

Albaek, S., Mollgaard, P. and Overgaard, P. B.: 1997, Government-Assisted Oligopoly Coordination? A Concrete Case, *The Journal of Industrial Economics* **45**(4), 429–443.

Allais, M.: 1953, Le Comportement de l ' Homme Rationnel devant le Risque: Critique des Postulats et Axiomes de l ' Ecole Americaine, *Econometrica* **21**(4), 503–546.

Allingham, M. G. and Sandmo, A.: 1972, Income tax evasion: A theoretical analysis, *Journal of Public Economics* **1**, 323–338.

Anderhub, V., Giese, S., Güth, W., Hoffmann, A. and Otto, T.: 2001, Tax Evasion with Earned Income - An Experimental Study, *FinanzArchiv* **58**(2), 188–206.

Andreoli-Versbach, P.: 2012, Collusion through delayed pricing: Evidence from the Italian petrol market, *working paper*.

Andreoni, J. and Miller, J. H.: 1993, Rational Cooperation in the Finitely Repeated Prisoner's Dilemma: Experimental Evidence, *The Economic Journal* **103**, 570–585.

Apesteguia, J., Dufwenberg, M. and Selten, R.: 2007, Blowing the Whistle, *Economic Theory* **31**(1), 143–166.

Aubert, C., Rey, P. and Kovacic, W.: 2006, The impact of leniency and whistle-blowing programs on cartels, *International Journal of Industrial Organization* **24**(6), 1241–1266.

Augsburger Allgemeine: 2012, Mineralölkonzerne: Rösler will mehr Wettbewerb, *Augsburger Allgemeine* **March 26**, 1.

Baranowska-Prokop, E.: 2010, Cartel Formation and Stability for Heterogeneous Firms, *Proceedings of the International Conference On Applied Economics 2010*, pp. 623–629.

Becker, G. S.: 1968, Crime and punishment: An economic approach, *Journal of Political Economy* **76**, 169–216.

Becker, G. S. and Stigler, G. J.: 1974, Law Enforcement, Malfeasance, and Compensation of Enforcers, *Journal of Legal Studies* **3**(1), 1–18.

Berninghaus, S., Hesch, M. and Hildenbrand, A.: 2012, Zur Wirkung regulatorischer Preiseingriffe auf dem Tankstellenmarkt, *Wirtschaftsdienst* **92**(1), 46–50.

Bigoni, M., Fridolfsson, S.-O., Coq, C. L. and Spagnolo, G.: 2009, Fines, Leniency and Rewards in Antitrust: An Experiment, *CEPR Discussion Papers No. 7417*.

Billiet, P. and Verma, P.: 2009, Why would cartel participants still refuse to blow the whistle under the current EC leniency policy?, *in* I. Kalozymis, M. Maci and A. Themelis (eds), *Global Antitrust Review*, Interdisciplinary Centre for Competition Law and Policy (ICC), London, pp. 1–20.

Blair, R. D. and Romano, R. E.: 2001, Advance Price Announcements and Antitrust Policy, *International Review of Law and Economics* **21**, 435–452.

BMWFJ: 2009, 190. Verordnung in BGBL. II Nr. 190/2009.

BMWFJ: 2010, 484. Verordnung in BGBl. II Nr. 484/2010.

Bonner General-Anzeiger: 2011, Bundeskartellamt: Berlin soll bei Spritpreisen eingreifen, *Bonner General-Anzeiger* **November 23**, 8.

Bonner General-Anzeiger: 2012, "Ein ausgeklügeltes System des Abguckens und Nachmachens", *Bonner General-Anzeiger* **March 20**, 3.

Borenstein, S. and Shepard, A.: 2002, Sticky Prices, Inventories and Market Power in Wholesale Gasoline Markets, *RAND Journal of Economics* **33**, 116–139.

Bos, I. and Harrington, J. E.: 2010, Endogenous Cartel Formation with Heterogeneous Firms, *RAND Journal of Economics* **41**(1), 92–117.

Bos, I. and Wandschneider, F.: 2011, Cartel Ringleaders and the Corporate Leniency Program, *CCP Working Paper No. 11-13*.

Bryant, P. G. and Eckard, E. W.: 1991, Price fixing: the probability of getting caught, *The Review of Economics and Statistics* **76**(3), 531–536.

Bundeskartellamt: 2006, *Notice no. 9/2006 of the Bundeskartellamt on the immunity from and reduction of fines in cartel cases*.

Bundeskartellamt: 2010, *Decision on the merger casee B9-13/10 "Magna/Karmann"*, Bonn.

Bundeskartellamt: 2011a, Benzinpreise - Marktmacht, Preissetzung und Konsequenzen.

Bundeskartellamt: 2011b, *Fuel Sector Inquiry - Final Report May 2011*, Bonn.

Bundeskartellamt: 2011c, *Press release Oct, 11: Wettbewerbsexperten diskutieren in Bonn bei der Tagung des Arbeitskreises Kartellrecht*, Bonn.

Bundesrat: 2011, *Antrag des Freistaats Thüringen - Entschließung des Bundesrates für faire und transparente Preise bei Kraftstoffen - Drucksache 870/11*.

Bundesrat: 2012a, *Beschluss des Bundesrates - Entschließung des Bundesrates für faire und transparente Preise bei Kraftstoffen - Drucksache 870/11(B)*, Berlin.

Bundesrat: 2012b, *Empfehlungen der Ausschüsse - Drucksache 870/1/11*, Berlin.

Bundesrat: 2012c, *Plenarprotokoll 892 - Stenografischer Bericht*, Berlin.

Bundesrat: 2012d, *Plenarprotokoll 895 - Stenografischer Bericht*, Berlin.

Bundestag: 2011, *Press release Dec, 14: Bundesregierung will nicht in Benzinpreisbildung eingreifen*, Berlin.

Bykowsky, M. M., Kwasnica, A. M. and Sharkey, W. W.: 2002, Horizontal Concentration in the Cable Television Industry: An Experimental Analysis.

Camerer, C.: 1999, Ambiguity-Aversion and Non-Additive Probability.

Camerer, C. and Weber, M.: 1992, Recent developments in modeling preferences: Uncertainty and ambiguity, *Journal of Risk and Uncertainty* **5**(4), 325–370.

Carlton, D. W. and Perloff, J. M.: 1999, *Modern Industrial Organization*, third ed. edn, Addison-Wesley, Massachusetts.

Chen, J. and Harrington, J. E.: 2007, The Impact of the Corporate Leniency Program on Cartel Formation and the Cartel Price Path, *in* V. Ghosal and J. Stenneck (eds), *The Political Economy of Antitrust*, Emerald Publishing, pp. 59–79.

Compte, O., Jenny, F. and Rey, P.: 2002, Capacity constraints, mergers, and collusion, *European Economic Review* **46**, 1–29.

Connor, J. M.: 2010, Price Fixing Overcharges: Revised 2nd Edition.

Connor, J. M. and Lande, R. H.: 2005, How High Do Cartels Raise Prices? Implications for Reform of the Antitrust Sentencing Guidelines, *Tulane Law Review* **80**.

Coricelli, G., Joffily, M., Montmarquette, C. and Villeval, M. C.: 2010, Cheating, emotions, and rationality: an experiment on tax evasion, *Experimental Economics* **13**(2), 226–247.

Dal Bó, P.: 2005, Cooperation under the Shadow of the Future: Experimental Evidence from Infinitely Repeated Games, *American Economic Review* **95**(5), 1591–1604.

Deck, C. A. and Wilson, B. J.: 2008, Experimental gasoline markets, *Journal of Economic Behavior & Organization* **67**, 134–149.

Deltas, G.: 2008, Retail gasoline price dynamics and local market power, *The Journal of Industrial Economics* **61**, 613–628.

Der Westen: 2011, Berlin will den Sprit, *derwesten* **November 2**.
URL: *http://www.derwesten.de/politik/berlin-will-den-sprit-id6101599.html*

Der Westen: 2012, Kartellamt hält Gesetz gegen Spritpreisanstieg für nutzlos.
URL: *http://www.derwesten.de/nachrichten/kartellamt-haelt-gesetz-gegen-spritpreisanstieg-fuer-nutzlos-id6525120.html*

DerStandard.at: 2010, Spritpreis-Verordnung in Begutachtung.
URL: *http://derstandard.at/1289609439388/Spritpreis-Verordnung-in-Begutachtung*

DerStandard.at: 2012, Mitterlehner prüft Preisregulierung für Benzin.
URL: *http://derstandard.at/1334368950239/Regulierung-Spritpreis-Mitterlehner-prueft-Preiskorridor*

Dewenter, R. and Heimeshoff, U.: 2012, Regulatory Interventions in Retail Gasoline Markets: Empirical Evidence from Austria and West Australia, *working paper*.

Die Welt: 2011a, Aral will Spritpreise länger hoch halten, *Welt online* **August 28**.
URL: *http://www.welt.de/wirtschaft/article13570026/Aral-will-Spritpreise-laenger-hoch-halten.html*

Die Welt: 2011b, Ökonomen verreißen Regulierung von Benzinpreisen, *Welt online* **Juli 1**.
URL: *http://www.welt.de/wirtschaft/article13406589/Oekonomen-verreissen-Regulierung-von-Benzinpreisen.html*

Die Welt: 2012a, Kartellamt knöpft sich Ölmultis vor, *Die Welt* **April 5**, 9.

Die Welt: 2012b, Morgens ist das Benzin besonders teuer, *Die Welt* **March 27**, 9.

DiePresse.com: 2009a, BP klagt gegen Spritpreisverordnung beim VfGH.
URL: *http://diepresse.com/home/wirtschaft/502184/*

DiePresse.com: 2009b, Verordnung wirkt: Österreicher tanken am Nachmittag.
URL: *http://diepresse.com/home/wirtschaft/501529/*

Dijkstra, P. T., Haan, M. A. and Schoonbeek, L.: 2012, Leniency Programs and the Design of Antitrust: Experimental Evidence with Unrestricted Communication, *working paper*.

Dufwenberg, M. and Gneezy, U.: 2000, Price Competition and Market Concentration: An Experimental Study, *International Journal of Industrial Organization* **18**, 7–22.

Ehrlich, I.: 1973, Participation in Illegitimate Activities: A Theoretical and Empirical Investigation, *Journal of Political Economy* **81**(3), 521–565.

Eichberger, J. and Kelsey, D.: 2009, Ambiguity, *in* P. Anand, P. Pattanaik and C. Puppe (eds), *The Handbook of Rational and Social Choice*, Oxford University Press, Oxford.

Einhorn, H. J. and Hogarth, R. M.: 1985, Ambiguity and Uncertainty in Probabilistic Inference, *Psychological Review* **92**(4), 433–461.

Ellsberg, D.: 1961, Risk, Ambiguity, and the Savage Axioms, *Quarterly Journal of Economics* **75**(4), 643–669.

Engelmann, D. and Müller, W.: 2010, Collusion through price ceilings? In search of a focal-point effect.

European Commission: 1996, *Commission Notice on the Non-Imposition or Reduction of Fines in Cartel Cases, OJ C 207, 18/07/1996, pp. 4-6.*

European Commission: 2002, *Commission Notice on Immunity from Fines and Reduction of Fines in Cartel Cases, OJ C 45, 19/02/2002, pp. 3-5.*

European Commission: 2006a, *Commission Notice on Immunity from Fines and Reduction of Fines in Cartel Cases, , OJ C 298, 08/12/2006, pp. 17-22.*

European Commission: 2006b, *Guidelines on the Method of Setting Fines Imposed Pursuant to Article 23(2)(a) of Regulation No 1/2003, OJ C 210, 01/09/2006, pp. 2-5.*

European Competition Network: 2006, *ECN Model Leniency Programme.*

Europe's Energy Portal: 2012, Historical Fuel Prices.
URL: *http://www.energy.eu/#Prices*

Feess, E. and Walzl, M.: 2005, Quid-Pro-Quo or Winner-Takes-It-All? An Analysis of Corporate Leniency Programs and Lessons to Learn for EU and US Policies.

Fischbacher, U.: 2007, z-Tree: Zurich toolbox for ready-made economic experiments, *Experimental Economics* **10**(2), 171–178.

Fonseca, M. A. and Normann, H.-T.: 2008, Mergers, Asymmetries and Collusion: Experimental Evidence, *The Economic Journal* **118**, 387–400.

Frankfurter Rundschau: 2012, Langfristig steigt der Preis weiter, *Frankfurter Rundschau* **March 28**, 2.

Frisch, D. and Jonathan, B.: 1988, Ambiguity and rationality, *Journal of Behavioral Decision Making* **1**(3), 149–157.

Greiner, B.: 2004, The Online Recruitment System ORSEE 2.0 - A Guide for the Organization of Experiments in Economics, *in* K. Kremer and V. Macho (eds), *Forschung und wissenschaftliches Rechnen 2003. GWDG Bericht 63, Göttingen : Ges. für Wiss. Datenverarbeitung*, number 10, University of Cologne, pp. 79–93.

Grether, D. M. and Plott, C. R.: 1984, The Effects of Market Practices in Oligopolistic Markets: An Experimental Examination of the Ethyl Case, *Economic Inquiry* **22**(4), 479–507.

Haan, M. A., Schoonbeek, L. and Winkel, B. M.: 2009, Experimental results on collusion, *in* J. Hinloopen and H.-T. Normann (eds), *Experiments and Competition Policy*, Cambridge University Press, chapter 2, pp. 9–33.

Hamaguchi, Y., Ishikawa, T., Ishimoto, M., Kiruma, Y. and Tanno, T.: 2007, An Experimental Study of Procurement Auctions with Leniency Programs, *Competition Policy Research Center Discussion Paper Series No. CRDP/24/E.*

Hamaguchi, Y., Kawagoe, T. and Shibata, A.: 2009, Group size effects on cartel formation and the enforcement power of leniency programs, *International Journal of Industrial Organization* **27**(2), 145–165.

Hammond, S. D.: 2001, When Calculating the Costs and Benefits of Applying for Corporate Amnesty, *speech delivered at the Fifteenth Annual National Institute on White Collar Crime, San Francisco, CA* .

Hammond, S. D.: 2002, A Review of Recent Cases and Developments in the Antitrust Division's Criminal Enforcement Program, *paper presented at the The 2002 Antitrust Conference: Antitrust Issues In Today's Economy, New York, NY* .

Handelsblatt: 2012a, Mineralölkonzerne kritisieren Verfahren des Kartellamts, *Handelsblatt* **April 5**, 15.

Handelsblatt: 2012b, Sehr elegant, aber sehr, sehr erfolglos, *Handelsblatt* **January 23**, 22.

Harding, D.: 2008a, FoolWatch - Further Discussion of Econometric Analysis Undertaken By ACCC, *MPRA Paper No. 16048.*

Harding, D.: 2008b, FoolWatch: A Case study of econometric analysis and evidenced-based-policy making in the Australian Government, *MPRA Paper No. 16041.*

Harrington, J. E.: 2008, Optimal corporate leniency programs, *The Journal of Industrial Economics* **56**(2), 215–246.

Harsanyi, J. C. and Selten, R.: 1988, *A General Theory of Equilibrium Selection in Games*, The MIT Press, Cambridge.

Hau, R., Pleskac, T. and Hertwig, R.: 2010, Decisions from experience and statistical probabilities: Why they trigger different choices than a priori probabilities, *Journal of Behavioral Decision Making* **23**(1), 48–68.

Haucap, J. and Müller, H. C.: 2012, The Effects of Gasoline Price Regulations: Experimental Evidence, *DICE Discussion Paper No. 47*.

Herre, J., Mirma, W. and Rasch, A.: 2012, Excluding ringleaders from leniency programs, *working paper*.

Herre, J. and Rasch, A.: 2009, The deterrence effect of excluding ringleaders from leniency programs, *working paper*.

Hilgers, H. A.: 2012, Die 8. GWB-Novelle in Deutschland und die Benzinpreistransparenz in Österreich, *Wirtschaft und Wettbewerb* (5), 467–474.

Hinloopen, J. and Onderstal, S.: 2011, Going Once, Going Twice, Reported! Cartel Activity and the Effectiveness of Leniency Programs in Experimental Auctions, *Tinbergen Institute Discussion Paper No. 09-085/1*, University of Amsterdam.

Hinloopen, J. and Soetevent, A. R.: 2008, Laboratory evidence on the effectiveness of corporate leniency programs, *RAND Journal of Economics* **39**(2), 607–616.

Holt, C. A. and Scheffman, D. T.: 1987, Facilitating Practices: The Effects of Advance Notice and Best-Price Policies, *RAND Journal of Economics* **18**, 187–197.

Isaac, R. M. and Plott, C. R.: 1981, Price Controls and the Behavior of Auction Markets: An Experimental Examination, *The American Economic Review* **71**(3), 448–459.

Kahneman, D.: 1972, Subjective probability: A judgment of representativeness, *Cognitive Psychology* **3**(3), 430–454.

Kirchgässner, G. and Kübler, K.: 1992, Symmetric or asymmetric price adjustment in the oil market, *Energy Economics* **14**, 171–185.

Kleven, H. J., Knudsen, M. B., Kreiner, C. T. and Saez, E.: 2010, Unwilling or unable to cheat? Evidence from a randomized tax audit experiment in denmark, *Nber Working Paper 15769* .

Knight, F. H.: 1921, *Risk, Uncertainty, and Profit*, Boston, MA: Hart, Schaffner & Marx; Houghton Mifflin Co.

Knittel, C. R. and Stango, V.: 2003, Price Ceilings as Focal Points for Tacit Collusion: Evidence from Credit Cards, *The American Economic Review* **93**(5), 1703–1729.

Kreps, D. M., Milgrom, P., Roberts, J. and Wilson, R.: 1982, Rational Cooperation in the Finitely Repeated Prisoners' Dilemma, *Journal of Economic Theory* **27**, 245–252.

Kühn, K.-U.: 2004, The coordinated effects of mergers in differentiated product markets, *The John M. Olin Center for Law & Economics Working Paper No. 34*.

Leslie, C. R.: 2006, Antitrust Amnesty, Game Theory, and Cartel Stability, *Journal of Corporation Law* **31**, 453–488.

Lui, F. T.: 1986, A dynamic model of corruption deterrence, *Journal of Public Economics* **31**(2), 215–236.

Lünemann, P. and Mathã, T. Y.: 2005, Consumer Price Behavior in Luxembourg, *European Central Bank Working Paper Series No. 541.*

Maskin, E. and Tirole, J.: 1988, A Theory of Dynamic Oligopoly, II: Price Competition, Kinked Demand Curves and Edgeworth Cycles, *Econometrica* **56**, 571–599.

Mason, C. F., Phillips, O. R. and Nowell, C.: 1992, Duopoly Behavior in Asymmetric Markets: An Experimental Evaluation, *The Review of Economics and Statistics* **74**(4), 662–670.

Miller, N. H.: 2009, Strategic Leniency and Cartel Enforcement, *American Economic Review* **99**(3), 750–768.

Motta, M. and Polo, M.: 2003, Leniency programs and cartel prosecution, *International Journal of Industrial Organization* **21**(3), 347–379.

Müller, H. C.: 2012, Es liegt nicht nur am Öl.

Normann, H.-T. and Ruffle, B.: 2011, Introduction to the special issue on experiments in industrial organization, *International Journal of Industrial Organization* **29**(1), 1–3.

Normann, H.-T. and Wallace, B.: 2012, The Impact of the Termination Rule on Cooperation in a Prisoner's Dilemma Experiment, *International Journal of Game Theory* (forthcoming).

ÖAMTC: 2010, ÖAMTC-Erfolg: Spritpreisverordnung wird verlängert und klarer formuliert.
URL: *http://www.oeamtc.at/?id=2701,1142605,,*

ÖAMTC: 2011, Tankstellen in Ballungszentren ändern im Schnitt viermal täglich Preise.
URL: *http://www.oeamtc.at/?id=2500,1357750,,*

OECD: 2001, *OECD Policy Roundtables 2001: Price Transparency.*

OECD: 2002, *Report on the Nature and Effect of Cartels.*

OECD: 2010, *OECD Economic Survey of Luxembourg, May 2010.*

Paha, J.: 2010, Endogenous Cartel Formation with Heterogeneous Firms and Differentiated Products, *working paper.*

Polinsky, A. M. and Shavell, S.: 1979, The optimal tradeoff between the probability and magnitude of fines, *The American Economic Review* **69**(5), 880–891.

Posner, R. A.: 1985, An Economic Theory of the Criminal Law, *Columbia Law Review* **85**(6), 1193–1231.

Rheinische Post: 2011, Tankstellen sollen Spritpreise nur einmal ändern, *RP Online* **May 26**.

URL: *http://www.rp-online.de/auto/news/tankstellen-sollen-spritpreise-nur-einmal-aendern-1.1286243*

Rotemberg, J. J. and Saloner, G.: 1990, Collusive Price Leadership, *Journal of Industrial Economics* **39**, 93–110.

Rothschild, R.: 1999, Cartel stability when costs are heterogeneous, *International Journal of Industrial Organization* **17**, 717–734.

Sandmo, A.: 2005, The Theory of Tax Evasion: A Retrospective View, *National Tax Journal* **58**, 643–663.

Schelling, T. C.: 1960, *The strategy of conflict*, Harvard University Press, Cambridge, MA.

Scherer, F. M. and Ross, D.: 1990, *Industrial Market Structure and Economic Performance*, 3rd edn, Houghton Mifflin, Boston.

Schroeder, D. and Heinz, S.: 2006, Requests for leniency in the EU: Experience and legal puzzeles, *in* K. J. Cseres, M. P. Schingel and F. O. Vogelaar (eds), *Criminalization of competition law enforcement: economic and legal implications fo the EU member states*, Northamton, pp. 161–176.

Selten, R. and Stoecker, R.: 1986, End Behavior in Sequences of Finite Prisoner's Dilemma Supergames, *Journal of Economic Behavior and Organization* **7**(1), 47–70.

Sen, A., Clemente, A. and Jonker, L.: 2011, Retail Gasoline Price Ceilings and Regulatory Capture: Evidence from Canada, *American Law and Economics Review* pp. 532–564.

Serra, D.: 2012, Combining Top-down and Bottom-up Accountability: Evidence from a Bribery Experiment, *Journal of Law, Economics, and Organization* (forthcoming).

Smith, V. L. and Williams, A. W.: 1981, On Nonbinding Price Controls in a Competitive Market, *The American Economic Review* **71**(3), 467–474.

Spagnolo, G.: 2000, Optimal Leniency Programs, *FEEM Working Paper No. 42.2000.*

Spagnolo, G.: 2004, Divide et Impera: Optimal Deterrence Mechanisms Against Cartels and Organized Crime, *CEPR Discussion Papers No. 4840.*

Stephan, A.: 2008, An Empirical Assessment of the European Leniency Notice, *Journal of Competition Law & Economics* **5**(3), 537–561.

Süddeutsche Zeitung: 2012a, 1:0 für Ölkonzerne, *Süddeutsche Zeitung* **March 23**, 23.

Süddeutsche Zeitung: 2012b, Nur bellen, nicht beißen, *Süddeutsche Zeitung* **April 5**, 19.

Süddeutsche Zeitung: 2012c, Teurer Sprit, *Süddeutsche Zeitung* **April 5**, 19.

Tankstellenmarkt.com: 2011a, Deutschland soll sich Österreich zum Vorbild nehmen.
URL: *http://www.tankstellenmarkt.com/deutschland-soll-sich-oesterreich-zum-vorbild-nehmen-999189.html*

Tankstellenmarkt.com: 2011b, Zu hohe Benzinpreise sollen gesetzlich verboten werden.
URL: *http://www.tankstellenmarkt.com/zu-hohe-benzinpreise-sollen-gesetzlich-verboten-werden-1082523.html*

Tversky, A. and Kahneman, D.: 1974, Judgment under Uncertainty: Heuristics and Biases, *Science* **185**(4157), 1124–31.

Tversky, A. and Kahneman, D.: 1983, Extensional versus intuitive reasoning: The conjunction fallacy in probability judgement, *Psychological Review* **90**(4), 293–315.

Tversky, A. and Wakker, P. P.: 1995, Risk Attitudes and Decision Weights, *Econometrica* **63**(6), 1255–1280.

U.S. Department of Justice: 1993, *Corporate Leniency Policy.*

U.S. Sentencing Commission: 2010, *Guidelines Manual*.

Vasconcelos, H.: 2005, Tacit collusion, cost asymmetries, and mergers, *RAND Journal of Economics* **36**(1), 39–62.

Veljanovski, C.: 2007, Cartel Fines in Europe, *World Competition* **30**(1), 65–86.

Vennström, J.: 2010, Leniency in the Nordic Countries, *European Competition Law Review* **31**(1), 8–15.

Wakker, P. P.: 2010, *Prospect Theory for Risk and Ambiguity*, Cambridge University Press, Cambridge.

Wang, Z.: 2009, (Mixed) Strategy in Oligopoly Pricing: Evidence from Gasoline Price Cycles Before and Under a Timing Regulation, *Journal of Political Economy* **117**(6), 987–1030.

Wils, W. P.: 2008, *Efficiency and Justice in European Law Enforcement*, Hart Publishing, Portland.

Wilson, B. J.: 2007, Experimental Economics and Antitrust: What Can We Learn From Laborytory Markets?, *Antitrust* **21**(2), 54–56.

Wirtschaftswoche: 2012, Die Ruhe stören, *Wirtschaftswoche* **March**(1), 24–25.

A Addenda to Chapters 2 and 3

A.1 Ringleader Experiment (Chapter 2)

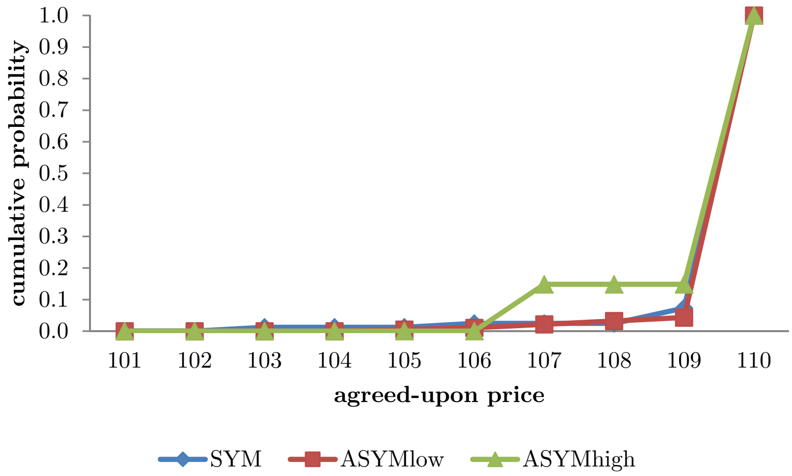
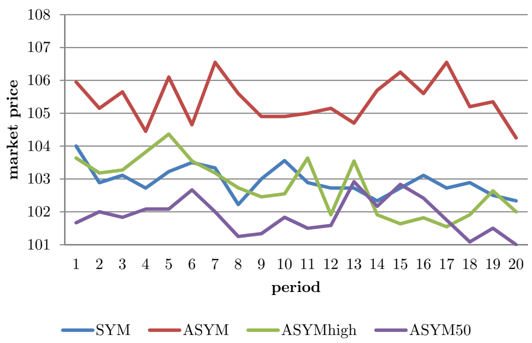
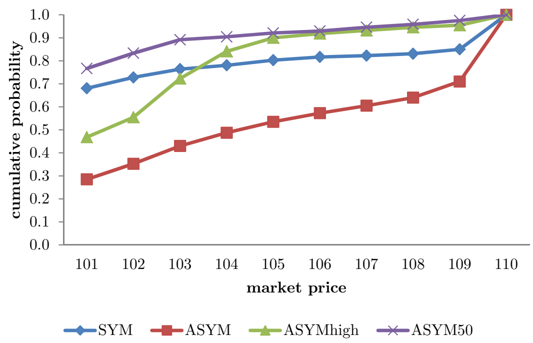


Figure A.1: Cumulative distribution function of agreed-upon prices



(a) Average market price per period



(b) Cumulative distribution function of market prices

Figure A.2: Market prices

A.2 Gasoline Experiment (Chapter 3)

Treatment	group/observation	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	Sum	Rate
REG	price comb. (1,1)	4	0	1	10	0	0	0	0	0	0	0	4	0	0	0	0	0	19	3.73 %
	price comb. (2,2)	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0.00 %
	price comb. (3,3)	4	4	4	0	4	4	2	4	4	3	4	0	4	4	4	4	4	57	11.18 %
	price comb. (4,4)	21	26	24	0	26	26	26	26	26	22	26	23	26	26	26	26	26	402	78.82 %
	REG choices	58	60	58	20	60	60	56	60	60	50	60	54	60	60	60	60	60	956	**93.73 %**
NREG	price comb. (1,1)	17	7	0	0	0	0	26	0	1	9	0	0	0	0	9	0		69	14.38 %
	price comb. (2,2)	1	2	0	0	0	0	0	0	0	3	0	0	0	0	0	0		6	1.25 %
	price comb. (3,3)	0	2	0	0	0	0	0	0	0	2	0	0	0	0	0	0		4	0.83 %
	price comb. (4,4)	0	0	30	29	30	30	0	30	22	0	30	29	30	25	5	29		319	66.46 %
	NREG choices	36	22	60	58	60	60	52	60	46	28	60	58	60	50	28	58		796	**82.92 %**
REG(FI)	price comb. (1,1)	0	28	0	0	1	0	26	1	0	2	10	1	0	22				91	21.67 %
	price comb. (2,2)	0	0	0	0	0	0	0	1	0	0	0	16	0	6				23	5.48 %
	price comb. (3,3)	4	0	4	4	4	4	0	4	4	2	4	0	4	0				38	9.05 %
	price comb. (4,4)	26	0	25	26	23	26	0	20	25	23	14	9	25	0				242	57.62 %
	REG(FI) choices	60	56	58	60	56	60	52	52	58	54	56	52	58	56				788	**93.81 %**
NREG(FI)	price comb. (1,1)	0	0	2	1	0	6	0	1	25	1	0	0	0					36	9.23 %
	price comb. (2,2)	0	0	0	0	0	1	0	0	0	0	0	0	0					1	0.26 %
	price comb. (3,3)	0	0	0	0	0	1	0	0	0	0	0	0	0					1	0.26 %
	price comb. (4,4)	30	28	19	27	29	20	29	25	0	29	30	29	29					324	83.08 %
	NREG(FI) choices	60	56	42	56	58	56	58	52	50	60	60	58	58					724	**92.82 %**

Table A.1: Cooperative results by group per treatment

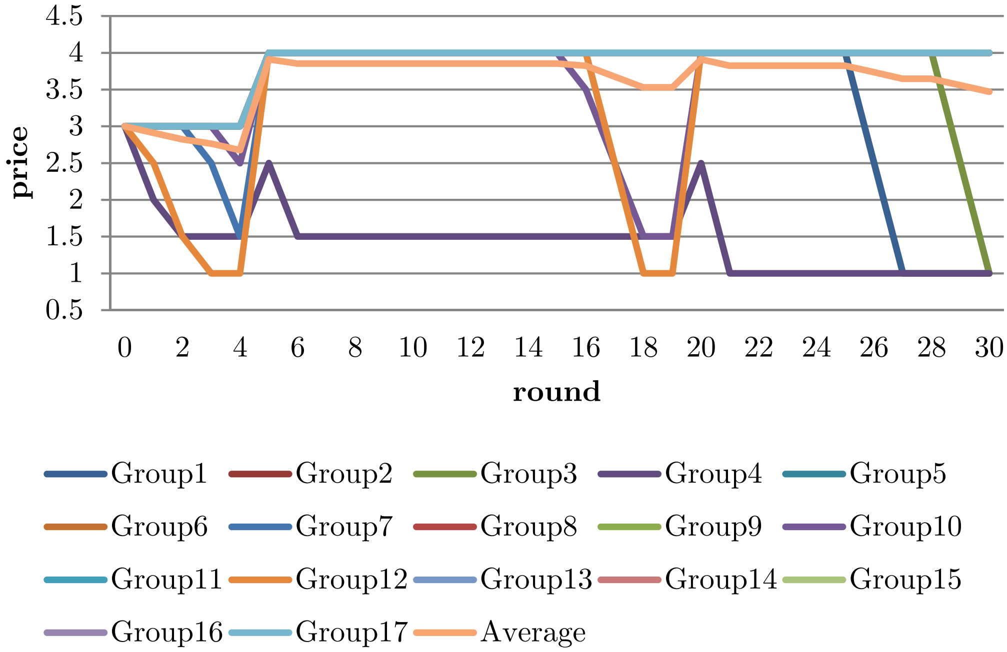


Figure A.3: Group average prices per round in REG

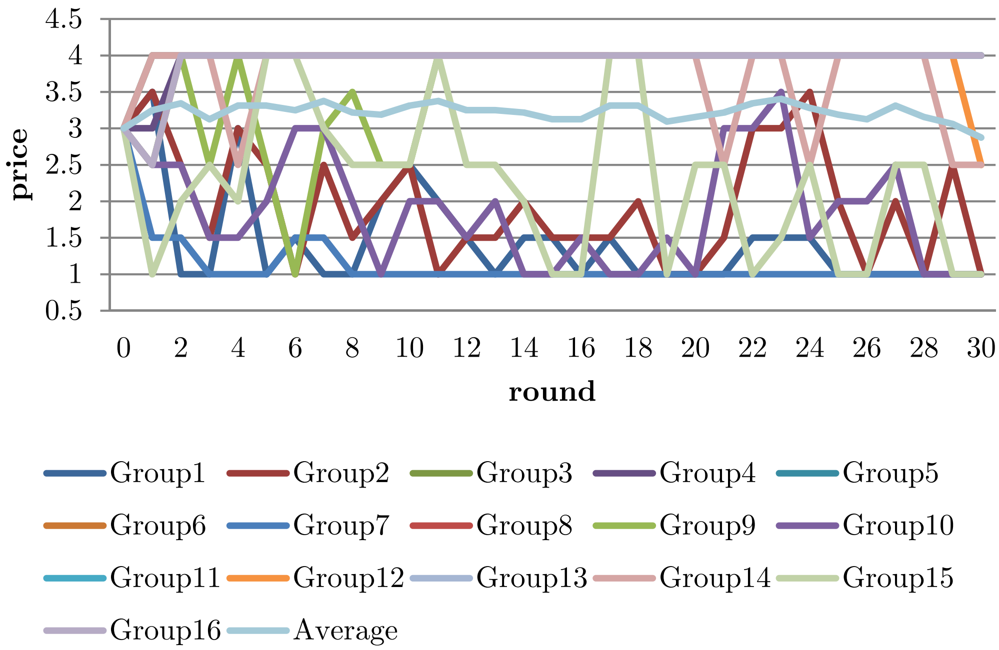


Figure A.4: Group average prices per round in NREG

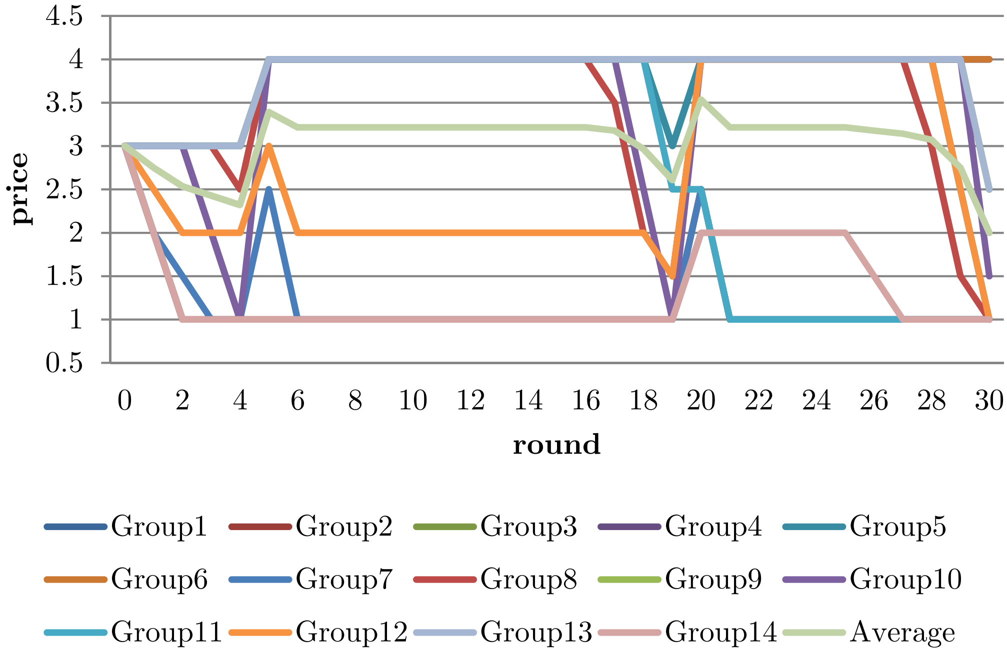


Figure A.5: Group average prices per round in REG(FI)

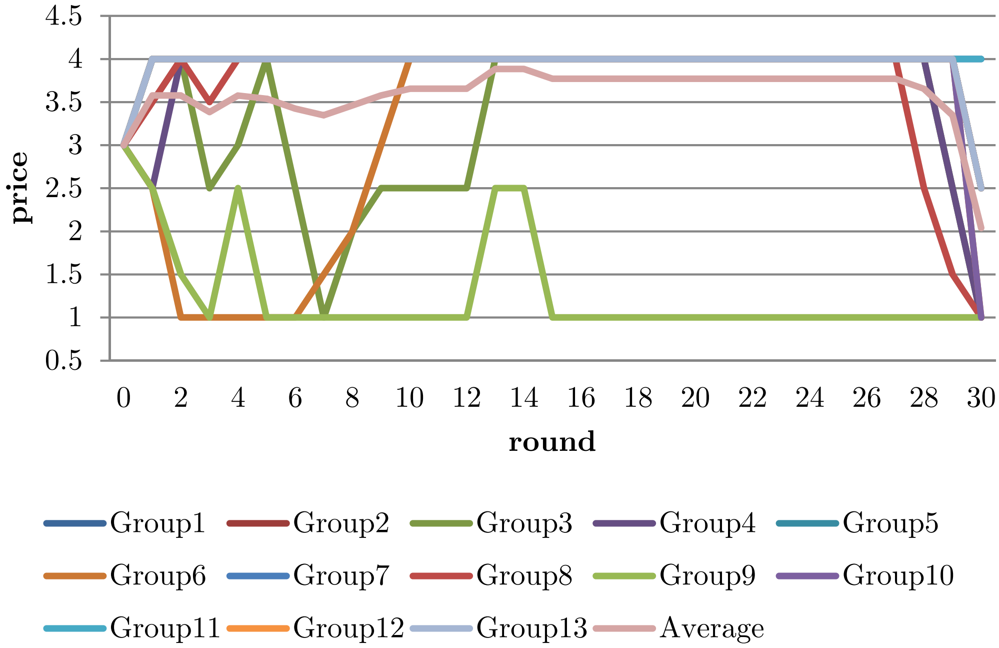


Figure A.6: Group average prices per round in NREG(FI)

B Experimental instructions

Since we performed the experiments at Karlsruhe Institute of Technology (KIT), Karlsruhe, Germany, the original instructions were written in German. Instructions in the original formatting and language can be obtained from the author. In the following, we append translated instructions for each of the three experiments we described above. Additionally, formatting is changed to save some space. We highlight the differences between the respective Treatments.

B.1 Ringleader Experiment (Chapter 2)

You are taking part in an economic experiment. During the experiment you can earn money. How much you earn depends on your decisions and on the decisions of other participants you are matched with. Every participant operates a visually isolated computer terminal and decides on her own. Communication between participants is not allowed.

In the experiment, you are a member of a group of three participants. Group assignments are carried out randomly at the beginning of the experiment, and the composition of the group stays the same for all periods. You don't know the identity of your group members, neither do they know yours. Each group consists of two "participants" and one "organizer". The roles are randomly assigned at the beginning of each period and are visualized on the computer screen at any time of the experiment.

Each of the 3 group members produces the same product and wants to sell one unit for a price p, with $p \in \{101, 102, 103, 104, 105, 106, 107, 108, 109, 110\}$.

Only the player choosing the lowest price (market price) has current period earnings according to $\pi = (p - 100/L)$, where L is the number of players who have simultaneously chosen the lowest price p: in case more than one player chose the same (lowest) price, earnings are split up equally (divided in half, or by three). Players with a price higher than the market price do not have earnings in this period (earnings equal 0).

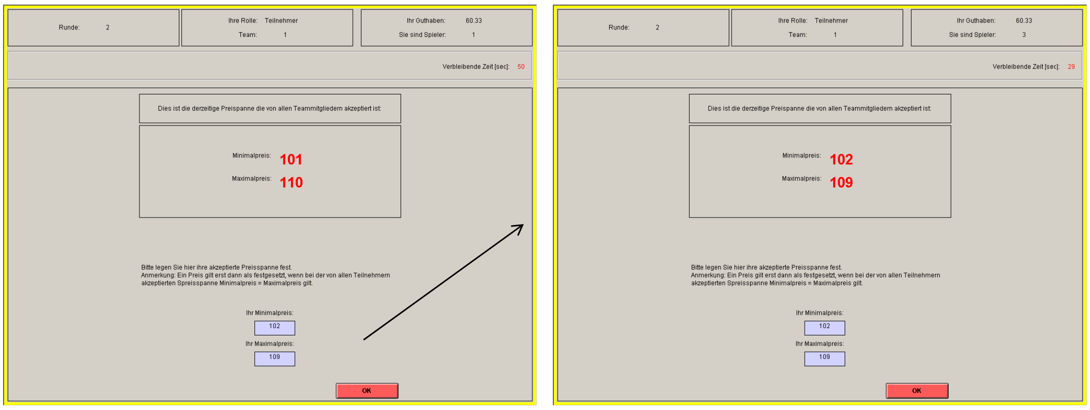
Every period of the experiment can be structured in **5 stages**:

Stage 1: Agree or disagree to join price discussions

Every player can decide whether to join or not to join a non-binding price discussion, before individually sets a market price. The discussion only takes place if all 3 group members unanimously agree to join the price discussion.

If no discussion takes place, every group member individually chooses a market price according to Stage 2.

If a discussion takes place, all group members begin a non-binding price discussion, before individually setting a market price. In the discussion, every player may submit an individually accepted minimum- and maximum price of the price range $\{101, 102, ..., 110\}$. A submitted price range has to be within the price range that is (at the moment) accepted by all group members. This accepted price range is visualized as two large, red numbers in the center of the screen and is updated in real time (see screenshots). If a submitted price range is not within the accepted price range, the player is asked to decide whether to (1) quit the discussion or (2) submit a new price range. In case (1), the discussion ends without an agreed-upon price, but the discussion has taken place. In case (2), the discussion continues. If only one value is submitted as both, a minimum- and maximum price, this price cannot be changed any further. Non-binding price discussions are possible for 30 seconds until there is no more price range available, but one agreed-upon price. The discussion screen closes after 30 seconds, even if there is no agreed-upon price.



(a) Input (b) Real-time output

Stage 2: Individual pricing decision

Every player individually chooses a price p out of $\{101, 102, ..., 110\}$. The choice is private information. Prices are simultaneously chosen by all group members and only the lowest of the three prices chosen (the market price) will then be public information.

Depending on all decisions made in Stage 1, the experiment continues with

Stage 3, if a discussion took place, or in
Stage 5, if no discussion took place.

Stage 3: Reporting

Stage 3 and Stage 4 only occur, if all players unanimously agreed to join a discussion in Stage 1.

[SYM, ASYM50: Every group member] [ASYM, ASYMhigh: Both participants, but not the organizer,] may report their participation in the discussion. The report is private information; no other player will learn who reported the participation. Reporting always costs 1 ECU (experimental currency unit). After a report, each player's earnings (π) are reduced by 10% of current period revenue.

For players choosing the market-price, current period revenues are (p_{market}/L). Net earnings therefore are $\pi - 0.1 \times (p_{market}/L)$. Players that chose a price above the market price in Stage 2, do not have any revenue and will therefore experience no reduction $(0.1 \times 0 = 0)$. Their net earnings still equal 0.

[SYM: The one group member who first reports the discussion will not experience a reduction in earnings.] [ASYM, ASYMhigh: The one participant who first reports the discussion will not experience a reduction in earnings.] [ASYM50: The reduction in earnings of 10% of current period revenues does not apply for the player who reports first. If a participant reports first, her earnings will not be reduced. Net earnings are $\pi - 0 \times 0.1 \times (\boldsymbol{p}_{market}/L)$. If the organizer reports first, earnings will be reduced by 5% of current period revenue. Net earnings are $\pi - 0.5 \times 0.1 \times (\boldsymbol{p}_{market}/L)$.]

Stage 4: Investigation
This stage only occurs, if the discussion has not been reported in Stage 3. The participation of all 3 group members in a discussion will be detected with a probability of **[SYM, ASYM, ASYM50: 15%] [ASYMhigh: 75%]**. In case of a successful detection, earnings of all players are reduced by 10% of the current period revenue. The agreement, to join a price discussion may therefore (Stage 3 and Stage 4) result in negative net earnings. If, however, no discussion takes place after Stage 2, both Stage 3 and Stage 4 will not occur in the respective period.

Stage 5: Information

You are told all relevant information of the current period. Your net earnings will be added to your overall earnings.

Every period of the experiment follows the logic of these 5 Stages. In the experiment, there are at least 20 periods in total. After period 20, another period begins with a probability of 80%, or the experiment ends with a probability of 20%.

Payment

Each player's per period earnings will be accumulated. You start the experiment with an amount of 24 ECU, which will be available throughout the experiment. In case a (possible) reduction of current period earnings is larger than current period earnings themselves, the negative value (loss) will be offset against your overall balance and you will be informed about it. Overall earnings in ECU will be converted into Euro, where 1 ECU is worth 0.25 Euro. Payment will be carried out individually and anonymously after the experiment.

Before the beginning of the experiment, there will be 5 practice periods. All earnings gained in these 5 periods will not be added to the overall earnings of the experiment. After the practice periods, all questions will be answered privately. Subjects and groups will then be rematched and the experiment begins.

B.2 Gasoline Experiment (Chapter 3)

The same Payoff-Table and its explanation (Appendix B.2.1) was handed out to all participants in every treatment. Regarding the computerized questionnaire mentioned in subsection 3.6, we provide, by way of illustration, the REG(FI)-Treatment comprehension questions in subsection B.2.3.

B.2.1 Payoff-Table

		the other participant			
	number	4	3	2	1
you	4	28|28	15|30	10|34	5 |38
	3	30|15	22|22	16|27	10|30
	2	34|10	27|16	20|20	13|22
	1	38| 5	30|10	22|13	14|14

The numbers which can be chosen by you are displayed in the head of each row. The numbers which can be chosen by the other participant are displayed in the head of each column. The payments associated with a combination of numbers are shown in the corresponding cell. The entry on the left side of the vertical bar corresponds to your payment. The entry on the right side of the vertical bar corresponds to the other participant's payment.

B.2.2 Instructions

Welcome to our experiment!

Please read the instructions carefully. Do not talk to your neighbors during the entire experiment. Raise your hand if you have questions. We will come around and answer your question privately. Your participation in the experiment will be rewarded. Depending on your behavior and the behavior of other participants you are matched with, you receive lower or higher monetary rewards in EUR.

You interact with another participant. Both participants each make one decision per round. Making a decision means choosing a number. The resulting combination of numbers is associated with a payment for each participant. The payments associated with each combination of numbers are shown in the attached table and on the screen.

The numbers which can be chosen by you are displayed in the head of each row. The numbers which can be chosen by the other participant are displayed in the head of each column. The payments associated with a combination of numbers are shown in the corresponding cell. The entry on the left side of the vertical bar corresponds to your payment. The entry on the right side of the vertical bar corresponds to the other participant's payment. The payments are quoted in ECU (experimental currency unit). The exchange rate between ECU and EUR is 1/75. That is, 75 ECU are exchanged for 1 EUR.

How are the decisions made?

You and the other participant decide simultaneously. That is, you choose your number (pick a row), and the other participant chooses his number (picks a column). If both decisions are made, you and the other participant will be informed about the choices. With that, a round is finished. That is, at the end of a round, each participant knows (i) his number, (ii) the other participant's number, (iii) his payment, and (iv) the other participant's payment. Decisions are communicated electronically. Items (i) to (iv) are automatically recorded and displayed. The sum of your payments from all previous rounds is visible during the whole experiment.

[REG: There are at least 31 rounds: round 0, round 1, . . . , round 30, and so on. After round 30, the probability of an additional round is 5/6. That is, the probability of continuation is about 83 percent from round 30 onwards. Decisions are automatically made in round 0. For both of you, number 3 is chosen. You and the other participant decide on your own in the following rounds. Both of you have to follow the following decision rule: In each round, you can choose the same number as in the preceding round, or you can choose a lower number. You are only allowed to choose a higher number in rounds 5 and 20, 35, and so on.]

[NREG: There are at least 31 rounds: round 0, round 1, . . . , round 30, and so on. After round 30, the probability of an additional round is 5/6. That is, the probability of continuation is about 83 percent from round 30 onwards. Decisions are automatically made in round 0. For both of you, number 3 is chosen. You and the other participant decide on your own in the following rounds.]

[REG(FI): There are 31 rounds: round 0, round 1, . . . , round 30. Decisions are automatically made in round 0. For both of you, number 3 is chosen. You and the other participant decide on your own in the following rounds. Both of you have to follow the following decision rule: In each round, you can choose the same number as in the preceding round, or you can choose a lower number. You are only allowed to choose a higher number in rounds 5 and 20.]

[NREG(FI): There are 31 rounds: round 0, round 1, . . . , round 30. Decisions are automatically made in round 0. For both of you, number 3 is chosen. You and the other participant decide on your own in the following rounds.]

You do not know the participant you interact with. You will be randomly assigned to a participant in round 0. This assignment remains fixed throughout all rounds. Anonymity among participants and towards experimenters is preserved. Your decisions can only be traced back to your identifier. Your personal data will not be associated with your identifier.

The sum of your payments from all rounds determines the variable part of your monetary reward in EUR. In addition, you will receive a fixed amount of 5 EUR. Your monetary reward will be paid out in private. That is, the other participants will not learn about the amount of your monetary reward.

B.2.3 Questionnaire

Raise your hand if you have problems answering the questions. We will come to you and answer your question privately.

Question 1

Decisions are automatically made in round 0. For both of you, number 3 is chosen. What is your payment and what is the other participant's payment in round 0?

My payment is . . . **[22]**. . . ECU.

The other participant's payment is . . . **[22]**. . . ECU.

Question 2

Assume you choose the number 4 whereas the other participant chooses the number 1. What is your payment and what is the other participant's payment in this round?

My payment is . . . **[5]**. . . ECU.

The other participant's payment is . . . **[38]**. . . ECU.

Question 3
Assume you decide to choose the number 1 whereas the other participant chooses the number 4. What is your payment and what is the other participant's payment in this round?

My payment is ... **[38]**... ECU.

The other participant's payment is ... **[5]**... ECU.

Question 4
Decisions are automatically made in round 0. For both of you, number 3 is chosen. Which numbers may be chosen by you and by the other participant in round 1?

I may choose the number(s) ... **[3, 2 or 1]**... .

The other participant may choose the number(s) ... **[3, 2 or 1]**... .

Question 5
Assume you decide to choose the number 2 in round 1 whereas the other participant chooses the number 1. Which numbers may be chosen by you and by the other participant in round 2?

I may choose the number(s) ... **[2 or 1]**... .

The other participant may choose the number(s) ... **[1]**... .

Question 6
Which numbers may be chosen by you and by the other participant in rounds 5, 20, 35, and so on?

I may choose the number(s) ... **[4, 3, 2, or 1 (all)]**... .

The other participant may choose the number(s) ... **[4, 3, 2, or 1 (all)]**... .

B.2.4 Post-experimental questionnaire

Please answer the following questions:

Questions regarding the experiment

(1) Did you choose your actions carefully?

☐ yes
☐ no
Comments to (1): ....

(2) Are you satisfied with your decisions?
☐ yes
☐ no
Comments to (2): ....

(3) How do you evaluate the comprehensibility of the instructions?
☐ easy to comprehend
☐ difficult to comprehend
Comments to (3): ....

Personal questions

(4) What sex are you?
☐ male
☐ female

(5) How old are you?
...... years

(6) Which faculty are you affiliated with?
☐ 01 Architecture
☐ 02 Civil Engineering, Geo- and Environmental Sciences
☐ 03 Chemistry and Biosciences
☐ 04 Chemical and Process Engineering
☐ 05 Electrical Engineering and Information Technology
☐ 06 Humanities and Social Sciences
☐ 07 Informatics
☐ 08 Mechanical Engineering
☐ 09 Mathematics

☐ 10 Physics
☐ 11 Economics and Business Engineering
☐ 12 other: ....

Thank you!

B.3 Ambiguity Experiment (Chapter 4)

B.3.1 Basic Game: RANDOM and BY CHOICE

You are taking part in a decision experiment. During the experiment you can earn money. How much you earn depends on your decisions and on the decisions of the other participants. Every participant makes his decisions on his own at his computer box. Communication between participants is not allowed.

In this experiment you are a member of a group of seven participants. This group consists of six decision makers and one authority. The role and group assignments are carried out randomly at the beginning of the experiment. The composition of the group stays the same for all 15 periods. In every period the decision makers are faced with the situation of step 1:

Step 1: Every decision maker decides between a secure payoff (Alternative A) and a risky payoff (Alternative B). The choice is private information and only known to the decision maker. Payoffs are calculated in ECU (experimental currency unit). Depending on the respective alternative choice, payoffs are being credited (for the moment).

	Alternative A		Alternative B	
Decision makers´ temporary payoff	600		1000	
Authority conducts an investigation	Yes	No	Yes	No
Decision makers´ payoff	600	600	0	1000
Authority´s payoff	0	0	1000	0

Step 2: After all payoffs have been credited, the authority decides whether to reveal one alternative choice, or not. If the authority decides in favor of an investigation, only one decision maker is selected to be investigated. **[RANDOM: The subject is selected randomly.][BY CHOICE: The subject is chosen by the authority directly.]** The selected decision maker can keep his payoff if he has chosen Alternative A in step 1. If he has chosen Alternative B, the payoff is being deducted and the authority is rewarded with this payoff. According to that, the decision maker earns nothing in this period.

Payments: Total earnings are calculated as a sum of the payoffs of all 15 periods. In addition to the earnings, participants receive a fixed participation fee that does not depend on the decisions. This participation fee is ECU10000. At the end of the experiment all earnings are converted into Euros. The fixed exchange rate is ECU100 = €0.05. The total earnings will be handed out individually and anonymously.

B.3.2 Extended Game: RANDOMgroup and BY CHOICEgroup

You are taking part in a decision experiment. During the experiment you can earn money. How much you earn depends on your decisions and on the decisions of the other participants. Every participant makes his decisions on his own at his computer box. Communication between participants is not allowed.

In this experiment you are a member of a group of seven participants. This group consists of six decision makers and one authority. The role and group assignments are carried out randomly at the beginning of the experiment. The composition of the group stays the same for all 15 periods. In every period the decision makers are faced with the situation of step 1:

Step 1: Every decision maker decides between a secure payoff (Alternative A) and a risky payoff (Alternative B). The choice is private information and only known to the decision maker. Payoffs are calculated in ECU (experimental currency unit). Depending on the respective alternative choice, payoffs are being credited (for the moment).

	Alternative A		Alternative B	
Decision makers´ temporary payoff	600		1000	
Authority conducts an investigation	Yes	No	Yes	No
Probability of successful detection	$p(\sigma)$	–	$p(\sigma)$	–
Decision makers´ payoff	600	600	0	1000
Authority´s payoff	0	0	1000	0

Step 2: After all payoffs have been credited, the authority decides whether to reveal one alternative choice, or not. If the authority decides in favor of an investigation, only one decision maker is selected to be investigated. **[RANDOMgroup: The subject is selected randomly.][BY CHOICEgroup: The subject is chosen by the authority directly.]** The success of the investigation (with $p(\sigma)$) depends on the number of decision makers that have chosen Alternative B in step 1.

Number of decision makers that chose Alternative B in step 1	**Probability of successful detection** $p(\sigma)$
1	100%
2	95%
3	90%
4	85%
5	80%
6	75%

If the investigation fails based on the probability of successful detection, the authority is not able to reveal the alternative choice of the selected decision maker. All participants keep the payoff according to their respective alternative choice. The authority gets nothing.

If the the investigation is successful, the decision maker is allowed to keep the (previously) credited payoff only if he chose Alternative A in step 1. If the investigated decision maker chose Alternative B in step 1, the payoff is being deducted and the authority is rewarded with this payoff. According to that, the decision maker earns nothing in this period.

Payments: Total earnings are calculated as a sum of the payoffs of all 15 periods. Additionally to the earnings, participants receive a fixed participation fee that does not depend on the decisions. This participation fee is ECU10000. At the end of the experiment all earnings are being converted into Euros. The fixed exchange rate is ECU100 = €0.05. Total earnings will be handed out individually and anonymously.

List of Figures

2.1 Market prices in SYM and ASYM 20
2.2 Cumulative distribution function of market prices in cartel and non-cartel groups (SYM and ASYM) 21
2.3 Cartel activity in SYM and ASYM 22
2.4 Cumulative distribution function of the number of periods individual subjects want to collude in SYM and ASYM 23
2.5 Market prices in ASYMhigh 25

3.1 Retail Gasoline prices (with taxes in EUR) in Germany 34
3.2 Average earnings per round 55
3.3 Group average prices per round 56
3.4 Percentage of chosen prices per round 58
3.5 Cumulative distribution function of prices 60

4.1 Alternative choices 74
4.2 Alternative B choices per round 76

A.1 Cumulative distribution function of agreed-upon prices 95
A.2 Market prices in all treatments 95
A.3 Group average prices per round in REG 97
A.4 Group average prices per round in NREG 97
A.5 Group average prices per round in REG(FI) 98
A.6 Group average prices per round in NREG(FI) 98

List of Tables

2.1 Classification of treatments 18
2.2 Causes of cartel breakdown 24

3.1 Comparison of the old and new Austrian gasoline pricing regulation 33
3.2 4×4 payoff matrix 48
3.3 Classification of treatments (Gasoline Experiment) 50
3.4 Session information 53
3.5 Percentage of prices per treatment 54
3.6 Cooperative results per treatment 54
3.7 Standard deviations of prices per treatment 57

4.1 Stages of the game 66
4.2 Classification of treatments (Ambiguity Experiment) 72
4.3 Relative shares of Alternative B choices per group 74

A.1 Cooperative results by group per treatment 96

List of Abbreviations

AAMTA ...... Austrian Automobile, Motorcycle and Touring Association, see ÖAMTC
ACCC .... Australian Competition & Consumer Commission
ACE .... Auto Club Europa
ADAC .... Allgemeiner Deutscher Automobil Club
am .... ante meridiem (before midday)
ARC .... Act Against Restraints of Competition, see GWB
ASYM .... Asymmetric Treatment
ASYM50 ...... Asymmetric50 Treatment
ASYMhigh .... AsymmetricHIGH Treatment
AV .... Ausschuss für Agrarpolitik und Verbraucherschutz des Bundesrats
BKartA .... Bundeskartellamt
BMELV .... Bundesministerium für Ernährung, Landwirtschaft und Verbraucherschutz
BMWFJ ...... Bundesministerium für Wirtschaft, Familie und Jugend
BMWi .... Bundeswirtschatsministerium
BNetzA .... Bundesnetzagentur
BYCHOICE ... By Choice Treatment
BYCHOICEg .. By Choice Treatment (group dependence)
CAPCP .... Committee for Agricultural Policy and Consumer Protection, see AV
cdf .... cumulative distribution function
DOCEP .... West Australian Department of Consumer and Employment Protection
DOJ .... Department of Justice
e.g. .... exempli gratia (for example)

EC .... Economic Committee, see Wi
ECU .... experimental currency unit
EU .... European Union
EUR .... Euro
FC .... Financial Committee, see Fz
FCO .... German Federal Cartel Office, see BKartA
FEMFY .... Federal Ministry of Economy, Family and Youth, see BMWFJ
FMCP .... Federal Ministry for Consumer Protection, see BMELV
FME .... Federal Ministry for Economy, see BMWi
Fz .... Finanzausschuss des Bundesrats
GewO .... österreichische Gewerbeordnung
GGAA .... General German Automobile Association, see ADAC
GPL .... Groupement Pétrolier Luxembourgeois
GWB .... Gesetz gegen Wettbewerbsbeschränkungen
i.e. .... id est (that is)
IJIO .... International Journal of Industrial Organization
IO .... Industrial Organization
KIT .... Karlsruhe Institute of Technology
MWU .... Mann-Whitney U
NREG .... Unregulated Treatment (infinite horizon)
NREG(FI) .... Unregulated Treatment (finite horizon)
ORSEE .... Online Recruitment System for Economic Experiments
PA .... Pricing Act, see PreisauszG
PD .... prisoner's dilemma
pm .... post meridiem (after midday)
PreisauszG .... österreichisches Preisauszeichnungsgesetz
RANDOM .... Random Treatment
RANDOMg ... Random Treatment (group dependence)
REG .... Regulated Treatment (infinite horizon)
REG(FI) ...... Regulated Treatment (finite horizon)
SYM .... Symmetric Treatment
TRA .... Trade Regulation Act, see GewO
U.S. .... United States

USC .... United States Sentencing Commission
Wi .... Wirtschaftsausschuss des Bundesrats
z-Tree .... Zurich Toolbox for Readymade Economic Experiments
ÖAMTC ...... Österreichischer Automobil-, Motorrad- und Touring Club