Tacit Collusion in Oligopolies and Regulated Industries

Zur Erlangung des akademischen Grades eines
Doktors der Wirtschaftswissenschaften

(Dr. rer. pol.)

von der Fakultät für
Wirtschaftswissenschaften
am Karlsruher Institut für Technologie (KIT)

genehmigte

DISSERTATION

von

Niklas Horstmann B. Sc.

Tag der mündlichen Prüfung: 20. Mai 2016
Referent: Prof. Dr. Jan Krämer
Korreferent: Prof. Dr. Karl-Martin Ehrhart
Karlsruhe, 2016

Acknowledgements

I am deeply grateful to my advisor Prof. Dr. Jan Krämer who always knew when to encourage and when to challenge me. He has not only been my mentor but also has become a dear friend. I am also indebted to Prof. Dr. Christof Weinhardt for the opportunity to pursue my scientific ambitions. My sincere thanks go to my co-advisor Prof. Dr. Karl-Martin Ehrhart for inspiring comments as well as to Prof. Dr. Kay Mitusch and Prof. Dr. Martin Ruckes for serving on the board of examiners.

My sincere appreciation goes to all my colleagues. In particular, I thank Marc Adam for satirical references, Christoph Flath for informative exchanges on politics, Felix Fritz for working weekend chats, Johannes Gärttner for entertaining conversations, Anuja Hariharan for exciting insights into Indian culture, Philip Köhler for his infectious cheerfulness, Tobias Kranz for passionate debates, Ewa Lux for making my time at the institute even more memorable and for proofreading this thesis, Marius Müller for diverting discussions, Claudia Niemeyer for her joviality, Thomas Setzer for his dry sense of humor, Daniel Schnurr for close and long-term collaboration, Alexander Schuller for his cooperative nature, Philipp Ströhle for brief glimpses into other research areas, Timm Teubner for constructive feedback, and Lukas Wiewiorra for helpful advice.

I thank all colleagues with whom I had the pleasure to collaborate in academic self-administration. Moreover, I gratefully acknowledge financial support from Deutsche Forschungsgemeinschaft and Karlsruhe House of Young Scientists. I thank Prof. Dr. Marc Bourreau for fruitful cooperation during my research stay at Télécom ParisTech.

Finally, I express my heartfelt gratitude to those who are most important to me: my family and friends. They encourage me in everything I do, help me to reflect myself, and remind me of the multiplicity of life. I owe them more than words can describe.

Abstract

Detection and prevention of collusive behavior are primary concerns of regulatory and antitrust authorities. This includes the exposure of explicit collusion, i.e., cartels, as well as the avoidance of tacit collusion in order to create competitive markets. In an effort to acquaint regulators with market structures and firm behavior that facilitate implicit coordination among competing firms, this thesis investigates principle characteristics of oligopolistic markets with respect to their propensity to collude tacitly. These main features are (i) strategic interactions of few competitors, (ii) multimarket contact between those firms, and (iii) vertically related upstream and downstream markets.

The market characteristics are investigated theoretically and empirically with a focus on economic laboratory experiments. Considering the lack of field data on tacit collusion due to the difficulties in detecting collusive behavior, experimental economics constitute a complementary research method which bridges the gap between theory and field evidence. In particular, economic laboratory experiments are well suited to systematically study the factors that may lead to tacit collusion in spite of the restrictive assumptions of theory or the deficiency of internal validity in real-world data.

As groundwork for the investigation of oligopoly characteristics, part one of the thesis reports on analyses of the experimental methodology itself. Its potential with respect to regulatory policy advice in general is assessed in a literature review that results in guidelines for corresponding experimental designs. In particular, the survey spans the spectrum of experiments by classifying them into those aiming for external validity by implementing regularities of a specific industry and those targeting internal validity by considering the most simplistic laboratory environment. A key design element of any experiment is the mode of timing. The vast majority of experiments consider discrete

time although continuous time may be argued to be a more realistic alternative. Therefore, both modes of timing are evaluated in an oligopoly competition experiment in which tacit collusion is found to be higher under discrete time than under continuous time. This finding emphasizes the implications of an experimenter's choice of timing.

Part two of the thesis is dedicated to theoretical and empirical analyses of the interactions of market structures and tacit collusion. First, the effect of the number of firms on tacit collusion is investigated by a meta-analysis of extant oligopoly experiments and by two experiments with symmetric and asymmetric firms. By systematic variation of the number of firms as well as the mode of competition, all analyses show that contrary to prominent belief the competitiveness of an industry does not strictly increase with the number of competitors. In fact, triopolies and quadropolies are found to be equally competitive, which bears important ramifications for merger control and ex ante regulation of oligopolies. Second, a theory of conglomerate firms' price setting behavior is developed that explains under which circumstances multimarket contact facilitates tacit collusion compared to single market contact. The theory builds on firms' ability to communicate collusive intentions solely through their price setting behavior and on the conjecture that such price signaling can be conducted more efficiently under multimarket contact. The findings suggest that limiting conglomerate firms' possibility to engage in price discrimination across geographically segmented markets may effectively reduce tacit collusion. Third, tacit collusion is also investigated in an environment of vertically related markets. In an industry in which wholesale access for a non-integrated reseller is provided by two vertically integrated firms, a laboratory experiment reveals that wholesale competition may facilitate tacit collusion, yielding wholesale and retail prices even above the monopoly level. Whereas regulation preventing a margin squeeze fails to decrease prices, a simple price commitment rule at the wholesale level is found to substantially reduce tacit collusion. These results indicate that consumers may be worse off under a wholesale duopoly with unregulated competitive wholesale provision relative to the case of a wholesale monopoly. Altogether, these findings highlight the vital importance of research on competition policy and of regulation itself in safeguarding competition.

Contents

Chapter 1

Introduction

"If we want our regulators to do better, we have to embrace a simple idea: regulation isn't an obstacle to thriving free markets; it's a vital part of them."

James Surowiecki (2010)

DETECTION and prevention of anti-competitive behavior are among the primary concerns of regulatory and antitrust authorities. This precept is based on the rationale of welfare economics that effective competition results in a market outcome which maximizes total surplus and that is hence considered to be efficient. The most prominent mode of anti-competitive behavior by more than a single firm is *collusion*. In general, this term refers to "an agreement among firms to divide the market, set prices, or limit production" (O'Sullivan and Sheffrin, 2003, p. 171). More specific, two forms of collusion are differentiated in antitrust policy with respect to the process of how such an agreement is concluded: *explicit collusion* and *tacit collusion*. While the former is at the core of what constitutes a cartel, the latter refers to conduct that results in implicit coordination among firms without the need of explicit communication. Although procedurally different they may lead to similar distortions of competition and thus both the exposure of explicit collusion as well as the prevention of tacit collusion are integral components of competition authorities' remit. Whereas the formation of a cartel is illegal conduct in most countries, e.g., according to the United States (US) Sherman Act or Article 101 of the Treaty on the Functioning of the European Union (EU), tacit collusion is by definition informal and therefore lacks the ground for prosecution of an identifiable illegal action. Therefore, ex ante regulation and likewise ex post competition policy warrant expertise on how to safeguard competition and hinder tacit collusion. Consequently, a substantial amount of economic research is devoted to characterize market conditions that facilitate tacit collusion among competing firms, which can in turn aid authorities in identifying anti-competitive market structures and deciding on appropriate regulatory remedies. This body of research may broadly be categorized into theoretical, empirical, and experimental approaches.

First, with respect to game-theoretic analyses of non-cooperative games—apart from few exceptions—predictions of collusion are attained only in infinitely repeated games, i.e., an environment in which the same game is repeated endlessly. For instance, a common assumption in the theoretical literature on collusion is that a firm behaves according to a grim trigger strategy which provides that the firm (i) plays the collusive action until one of its competitors deviates from this action and (ii) in case of a deviation punishes the deviating rival from that point on by infinite play of the competitive action. If all competing firms follow the grim trigger strategy stable collusive play occurs on condition that all firms sufficiently value future profits compared to present profits. In other words, competitors engage in joint profit maximization (JPM) if and only if they discount future profits at least as much as according to the critical discount factor. Therefore, standard economic theory predicts collusion only if the rationale of backward induction and likewise profitable deviation does not apply due to a time horizon that is not finite but infinite. Ivaldi et al. (2003) provide a comprehensive summary of the theoretical evidence on tacit collusion.

Second, with regard to empirical research, it is notoriously hard to obtain robust evidence for the existence of tacit collusion as it lacks a formal agreement that is observable in the field. More general, a benchmark for competition, which is required to assess whether a market outcome is collusive or not, is difficult to attain in empirical data. However, even if such a benchmark can be defined, it still remains to be judged whether supra-competitive prices should be attributed to a cartel and hence, explicit collusion, or to idiosyncrasies of the market structure and thus, tacit collusion. Consequently, the insights that can be retrieved from empirical analyses on causal effects in this context are limited. The few extant field studies on the identification of tacit collusion are reviewed by Feuerstein (2005).

Third and lastly, with respect to experimentation in economics, laboratory environments are well suited to systematically analyze the factors that facilitate tacit collusion. In contrast to empirical analyses of field data, a benchmark for competition is readily given by the game implemented in the experiment and hence tacit collusion can be measured and compared across varying market structures. The result is a high level of internal validity due to controlled variation as well as randomization of uncontrollable input variables, which is, however, accompanied by a potential lack of external validity. In this vein, regarding the economics of tacit collusion, laboratory experiments may be used as a testbed for the competitiveness of alternative market structures and thereby constitute a complementary research methodology that bridges the gap between theory and the field. Recent experimental studies on tacit collusion are surveyed by Potters and Suetens (2013). Engel (2007, 2015) further contributes to a formalization of the experimental evidence on tacit collusion by means of meta-analyses.

In a nutshell, the combined research indicates that (i) a low number of firms and (ii) multimarket contact are two of the main drivers of tacit collusion. Both of these market properties are distinctive of oligopolies. An oligopoly is by definition characterized by a fewness of competitors. Worldwide examples are the airliner market, which is effectively a duopoly with *Airbus* and *Boeing*, and the video game console market that is shared among *Microsoft*, *Nintendo*, and *Sony*. Moreover, it can be observed that oligopolies frequently arise in industries of natural monopolies, i.e., in industries with subadditive cost functions so that "multi-firm production is more costly than production by a monopoly" (Baumol, 1977, p. 810). In order to allow for competition in these industries, they are often regulated. Obvious examples for natural monopolies are network industries, which are characterized by both high sunk infrastructure investments and strict regulation. More specific, energy and telecommunications markets in many countries are dominated by a low number of firms. Furthermore, these industries also exhibit multimarket contact between conglomerate oligopolistic competitors. For instance, the end consumer market for fuel is concentrated in many countries with few firms controlling most of the gas stations. At the same time, consumers cannot move their demand for fuel arbitrarily. Obviously, demand for fuel is geographically tied and therefore multimarket contact occurs. An even more distinct example for a regulated oligopoly are mobile and fixed telecommunications markets in Europe.

As detailed in Section 1.1, the consolidation in and fragmentation of European telecommunications markets reduces the number of network operators and increases multimarket contact—whilst at the same time the European Commission (EC) targets a single market for telecommunications and digital industries across the EU. After this motivating example, Section 1.2 derives the research questions in the context of tacit collusion in oligopolies and regulated industries that underlie this thesis. Section 1.3 introduces the structure of the remainder of this thesis.

1.1 Motivation

In 2010, the EC formulated the *Digital Agenda for Europe* as one of the seven pillars of its *Europe 2020 Strategy*. The agenda's main objective is to develop a *digital single market (DSM)*, which is also among the ten priorities set out by the current Juncker Commission and assumed to boost the European gross domestic product by up to EUR 415 billion (European Commission, 2015c). The strategy towards the implementation of a DSM builds on three pillars, one of which is to create "the right conditions for digital networks and services to flourish" (European Commission, 2015c, p. 3). In particular, this pillar is primarily aimed at reviewing the telecommunications regulation to "ensure that markets operate more competitively and bring lower prices and better quality of service to consumers and businesses, while ensuring the right regulatory conditions for innovation, investment, fair competition and a level playing field" (European Commission, 2015b, p. 3). Scholars, authorities, and industry professionals (Pelkmans and Renda, 2011; Parcu and Silvestri, 2014; European Commission, 2013b; European Policy Center, 2010; van Gorp et al., 2011) agree that the EU is still fragmented into 28 national telecommunications markets based on indicators that are believed to reflect the state of cross-border merging. In essence, a connected DSM is pictured as an EU-wide market in which (i) consumers purchase telecommunications services across member states at sufficiently homogeneous prices, (ii) online trade of goods and provision of services is ubiquitous also across national borders, (iii) levels of broadband coverage and subscriptions are high and converged, and (iv) firms operate in a continent-spanning digital economy. However, a host of barriers to the DSM is discerned in the literature which can be summarized into four categories: (i) economic differences between member states, (ii) cultural differences between populations, (iii) consumers' concerns with respect to data privacy and protection, and (iv) heterogeneity in regulatory approaches—the latter of which is unanimously viewed as the key impediment of the evolution of a DSM. This notion is summarized trenchantly by Alexander Italianer (2015), then Director-General of the EC's Directorate General for Competition: "We have an open telecom market, but we don't have a European telecom market."

Therefore, the goal that is pursued with the above-mentioned pillar of EC's DSM strategy is a convergence of telecommunications regulation and eventually of telecommunications markets across the EU. However, this process is likely to facilitate two developments which can already be discerned in national European telecommunications markets today and which are related to anti-competitive effects: (i) a consolidation of telecommunications network operators and (ii) an increase in multimarket contact between telecommunications service providers. With respect to the *mobile* telecommunications industry, the ongoing consolidation is highlighted by an increasing concentration of (national) markets. In particular, as a consequence of horizontal mergers the number of mobile network operators recently decreased from four to three in Austria, Ireland, and Germany (Walle and Wambach, 2014). Yet the industry is also concentrated on the European level. Across member states, the four biggest mobile network operators combine approximately 60% of all subscribers in the EU; again the two biggest of these companies, *Vodafone* and *Deutsche Telekom*, operate directly or through subsidiaries in twelve and eleven EU member states, respectively (Vestager, 2015). Seven of these countries overlap so that both firms meet each other in these national markets. Other mobile network operators or mobile virtual network operators are present in more than a single country as well. These examples demonstrate that several telecoms are conglomerate firms that meet the same competitors in multiple national markets. In other words, multimarket contact is pervasive in the European mobile telecommunications industry.

Although in a different vein, this also applies to the *fixed* telecommunications industry. In many European countries, the fixed broadband infrastructure is almost exclusively owned by a single firm, i.e., the incumbent, which originates from the fact that most of the fixed telephone networks were built by state-owned enterprises, which were later privatized—in conjunction with the network infrastructure. Therefore, to allow for competition nevertheless, access to these infrastructures is regulated in most countries. However, the increasing transmission of content via the Internet Protocol, which is independent of the underlying infrastructure, makes cable networks more popular as their operators offer telecommunications services that are equal (or even superior) in quality to those of traditional fixed telecommunications operators. In fact, whereas almost 94% of EU households have a digital subscriber line (DSL) connection at their disposal, more than 44% are connected to a cable infrastructure so that at least a third of all households can choose between these two network types when subscribing for broadband (European Commission, 2015a). Due to consolidation movements among cable operators by conglomerate mergers over distinct geographic areas, several regions of the EU are not only governed by infrastructure competition between traditional telecoms and cable operators, but also exhibit increasing multimarket competition within national borders. Since demand for telecommunications services over fixed infrastructure is arguably only to a very limited extent geographically transferable, it may be argued that the market for fixed broadband is geographically segmented at the sub-national level.

In conclusion, although the European telecommunications industry features increasing multimarket contact of conglomerate firms due to ongoing consolidation within national markets, no single network operator is present in at least half of all EU member states—a fact that nourishes doubts that the connected DSM will be completed in the near future. Yet, further convergence of European telecommunications markets will only reinforce the developments detailed above. Albeit in consequence of different market dynamics, a set of similar issues arises in fixed and mobile telecommunications markets with respect to regulation and competition. First, whereas the number of firms in many mobile telecommunications markets decreases from four to three, fixed telecommunications markets exhibit infrastructure duplication and thus the transition from a monopoly to a duopoly. Therefore, in both types of markets combined regulatory and competition authorities are faced with the question of what constitutes a sufficient number of firms to ensure effective competition even in the absence of regulation. Second, increasing multimarket contact—either supra-national or sub-national—requires scrutiny regarding its potential anti-competitive effects. Third, a characteristic of infrastructure-based industries such as the telecommunications sector is that access to the infrastructure and provision of a service over the infrastructure constitute two different markets within the same value chain. Therefore, attention is also warranted with respect to whether the regularities of competition in a single (or multiple horizontally connected) market(s) carry over to vertically related markets in which the required input on the upper part of a value chain, i.e., access to a fixed or mobile telecommunications network, is controlled by few or even only a single firm.

1.2 Research questions

The three issues with respect to effective competition derived for the telecommunications industry apply likewise for other oligopolies in general and network industries in particular. Therefore, they constitute the starting point for this thesis. These issues are directly connected to research questions that are derived and listed in the following. Regarding the relation of the number of competitors in a market to the market's competitiveness, a prominent notion in the economic literature is that the competitiveness of a market increases monotonically with the number of competitors. Apart from the fact that this notion itself may be challenged, competition authorities reviewing merger cases and regulatory bodies examining the justifications for existing regulation have to judge what is the minimum number of firms in a market to prevent tacit collusion and ensure effective competition so that they thus arrive at a decision to clear or refuse a merger and regulate or deregulate a specific market, respectively.[1] As highlighted by the example of telecommunications markets, yet carrying over to oligopolies in general, the critical number of firms is assumed to lie between two and four. With this in mind, the following research question tackles the relationship between the fewness of firms and anti-competitive behavior.

RESEARCH QUESTION 1. *How many competitors are enough to ensure effective competition in a market that is governed by an oligopoly?*

The issue of multimarket contact and its potential anti-competitive effects are examined in the economic literature ever since Edwards's (1955) conjecture that two firms meeting in more than a single market will refrain from undercutting each other in any market because they fear to be punished by their rival in all markets. The expected consequence is stronger tacit collusion among the conglomerate firms than if the firms would meet only in a single market. Explanations that corroborate this conjecture hinge on specific assumptions about firm characteristics or the market structure. However, a collusive effect of multimarket contact cannot be explained by existing theory in a situation of profit-maximizing symmetric firms and markets. More importantly, the process according to which tacit collusion emerges in single markets and how this translates to

[1] Obviously, beforehand authorities have to decide on what constitutes effective competition in an oligopolistic market.

multiple markets is widely understudied. The next research question therefore directly addresses this.

Research Question 2. *Does multimarket contact between conglomerate firms facilitate tacit collusion more than single market contact? If yes, according to which price setting process does tacit collusion emerge?*

Whereas the previous issues deal with generic factors that are pervasive in oligopolies, the next issue is more specific for those oligopolies that are infrastructure-based industries. Apart from the telecommunications industry this applies to all network industries as well as several other industries that feature an essential facility at a higher level of their value chain. More specific, if an input good at an upstream market is required for the production of a downstream good, control over the former may be related to strong market power. This is assumed to be most severe in the case of a monopolistic upstream market. Therefore, in many network industries regulators oblige the incumbent of a bottleneck infrastructure to provide access to its downstream rivals in order to allow for competition at the downstream market. If, however, the upstream market is not composed of a monopolistic access provider but instead of two firms providing access, the rationale for regulated access should be reviewed. The following research question is therefore concerned with the interplay of competition at the upstream market and the regulation of access to the upstream good.

Research Question 3. *Is a duplication of essential infrastructure at an upstream market beneficial for firms at the downstream market and for consumers? How can regulation at the upstream market safeguard competition?*

As already pointed out, laboratory economic experiments are a promising research methodology when investigating tacit collusion and when evaluating regulatory institutions. However, the potential of the experimental methodology in economics with respect to examining regulatory issues is still widely unknown due to a lack of reviews of the extant literature. Especially with respect to how experiments can serve as a complementary research methodology in advising regulators, design and procedural guidelines are missing. Therefore, the promotion of the experimental methodology towards regulators requires to formulate a set of guidelines to be followed by researchers when conducting an experiment for regulatory policy advice. The next research question is aimed at fulfilling this task.

Research Question 4. *How and to what extent can economic laboratory experiments provide a testbed for regulatory institutions to advise regulatory policy?*

One feature of experimental design that gains recent attention is the mode of timing that is implemented to resemble the nature of an experiment's underlying repeated game. So far, most experiments employ the concept of discrete time, i.e., a one-shot game is repeated in separate periods in which each subject in an experiment makes a single decision as specified by the theoretical model underlying the experiment. An alternative that is considered to be closer to decision making in the real world is continuous time. This refers to an experiment that runs in (almost) real time so that subjects can make and adapt decisions at any point in time. The consequences of such a mode of timing are largely unknown and both theoretical and experimental analysis in this regard is scarce. Therefore, the implications of continuous time for competition in oligopolies are also unknown. The following research question targets this research gap.

Research Question 5. *Does continuous time in experiments on oligopoly competition facilitate tacit collusion more than discrete time?*

1.3 Structure of this thesis

The remainder of this thesis is organized in two parts. Part I deals with the methodology of experimental economics in regard to regulatory and competition policy and thereby constitutes the basis for the applications of the experimental methodology in Part II. In particular, Part I comprises Chapters 2 and 3. Chapter 2 addresses Research

Question 4 and provides a review of the extant experimental literature on issues of regulatory policy and derives guidelines for the design and procedures of experiments aimed at advising regulators from a qualitative analysis. Thereby, laboratory experiments are differentiated into those aiming for a maximum of external validity of their findings with respect to a specific industry and those targeted at internal validity of the test of a generic regulatory institution that is not limited to a specific industry context. Chapter 3 accompanies the qualitative analysis in the previous chapter. It reports on an investigation of the mode of timing as one specific experimental design element and thereby deals with Research Question 5.[2] In an experimental oligopoly environment, discrete time and continuous time are investigated in regard to their propensities towards tacit collusion next to market structure controls in an effort to obtain robust empirical evidence. Together these two chapters constitute the first part of the thesis and lay the methodological ground for the subsequent experimental investigations of certain features of market structures and their effect on tacit collusion.

Part II encompasses three chapters that all focus on a (rather) generic market characteristic and its effect on tacit collusion by means of both theoretical analysis and experimental investigation. Chapter 4, which relates to Research Question 1, is an extensive experimental analysis of number effects in oligopolies and composed of three distinct studies.[3] Starting with a meta-analysis of existing oligopoly experiments that systematically vary the number of competitors in a market, two comprehensive laboratory experiments are reported that investigate the effects of a varying number of firms under several market structures and thereby cover a wide range of oligopoly scenarios. Chapter 5 deals with price discrimination across geographic markets as one of the key features of multimarket contact in a laboratory experiment and thereby constitutes the groundwork for an investigation of Research Question 2.[4] Building on the findings from this experimental analysis, in Chapter 6 a behavioral theory is derived that targets to answer Research Question 2 by suggesting a process according to which tacit collu-

[2]Chapter 3 is based on joint work with Jan Krämer and Daniel Schnurr (Horstmann et al., 2015b).
[3]Chapter 4 is based on joint work with Jan Krämer and Daniel Schnurr (Horstmann et al., 2015a).
[4]Chapter 5 is based on joint work with Jan Krämer (Horstmann and Krämer, 2013).

sion can be reached in single and multiple markets.[5] More specific, the theory is able to determine the conditions under which multimarket contact facilitates tacit collusion more than single market contact, and vice versa. The predictions of the theory are validated by an economic laboratory experiment. Chapter 7 takes a focus on vertically related markets and tests a theory of upstream competition and regulation in the laboratory.[6] More specific, the scenario comprises two firms that are vertically integrated over an upstream market and a downstream market as well as a firm that operates solely in the downstream market but requires the upstream good as an input to supply its good to consumers. Within this context, Research Question 3 is addressed by systematic variation of the level of competition and regulation in the upstream market.

Finally, Chapter 8 concludes this thesis with a summary of findings with respect to the research questions formulated above. Thereby, the policy and managerial implications of the obtained results are discussed as well as the limitations connected to them. Furthermore, propositions for future work are derived from the limitations of this thesis and likewise from further open questions that arise from the reported findings.

[5] Chapter 6 is based on joint work with Jan Krämer (Horstmann and Krämer, 2016).
[6] Chapter 7 is based on joint work with Jan Krämer and Daniel Schnurr (Horstmann et al., 2016).

Part I

Regulation in the Lab

Chapter 2

Experimental Economics for Regulatory Policy Advice

THE governments of the United Kingdom (UK), the US, and Germany all seek counsel by behavioral scientists to improve the effectiveness and efficiency of policy programs and decisions. In 2010, Prime Minister David Cameron announced the *Behavioural Insights Team* as the first government institution dedicated to the application of behavioral research (McSmith, 2010). President Barack Obama followed in 2014 when he launched the *Social and Behavioral Sciences Team* to help "government programs better serve the nation while saving taxpayer dollars" (Shankar, 2015). A few months later, with a comparable intention, Chancellor Angela Merkel decided to establish a group of advisors for behavioral research (Plickert and Beck, 2014). Recently, also the Austrian government chimed in and announced that it will incorporate insights from behavioral economics into their policy (Weißensteiner, 2015). These examples emphasize the increasing influence of behavioral research on policy and society. Yet, the launchings of all the aforementioned government institutions were predominantly motivated by *nudging*, i.e., a concept of policy making based on "any aspect of the choice architecture that alters people's behavior in a predictable way without forbidding any options or significantly changing their economic incentives" (Thaler and Sunstein, 2008,

p. 6).[1] Or, as the Behavioural Insights Team (2015, p. 6) phrases it more bluntly as one of their objectives: To enable "people to make 'better choices for themselves' ".[2]

This chapter takes a different angle on the role of experimental economics for policy advice. Instead of analyzing how behavioral science may help governments in nudging the population to make better decisions, the aim is to investigate how experimental economics can advise decision-making in economic regulatory policy.[3] Remarkably, the potential of the experimental methodology for policy making in economic regulation of markets is far less emphasized and exploited. However, already several decades ago experiments have been used to address the potential consequences of political decisions on the economy. Therefore, this chapter builds upon previous related literature overviews (e.g., Davis and Wilson, 2002; Normann, 2008; Ricciuti, 2008; Normann and Ricciuti, 2009) and has two main contributions. First, an extensive review of economic experimental studies with specific implications for economic policy making in regulated industries is compiled. Second, key experimental design elements are derived from the reviewed literature in an effort to provide scholars with a documentation of the consensus (and dissent) among experimentalists regarding the design of laboratory experiments for regulatory policy advice. In particular, the focus is on experimental industrial organization (IO) research in the laboratory with implications for industry regulation. After Section 2.1 states the research objectives of this literature review and sets its scope, the key variables are introduced in Section 2.2. The actual review of the extant literature is conducted in Section 2.3. Subsequently, Section 2.4 summarizes observations from the previous review and thereby derives a conceptual framework of how to design an economic laboratory experiment for regulatory policy advice. Finally, in Section 2.5, the potential of the experimental methodology is discussed.

[1] A yet broader definition not limited to economic incentives is proposed by Hausman and Welch (2010, p. 126): "Nudges are ways of influencing choice without limiting the choice set or making alternatives appreciably more costly in terms of time, trouble, social sanctions, and so forth. They are called for because of flaws in individual decision-making, and they work by making use of those flaws."

[2] Lunn (2014) discusses the influence of behavioral economics on policy and summarizes related experimental work.

[3] Also outside the scope of this chapter are the implications of behavioral economics on potential biases in the decision-making process of regulatory authorities. See Cooper and Kovacic (2012) for a formal model of a regulator serving as an agent to a political supervisor.

2.1 Objectives

One may argue that it is among the main goals of economists to support policy makers in making better informed decisions which will be beneficial for the society as a whole. In this vein, many scholars draw policy conclusions of their research. However, comparably few economic studies have been explicitly motivated by a policy problem, analyzed its components, and subsequently proposed a solution. Diverse areas of economic research may provide valuable insights to policy makers. The scope of the present literature review is limited to economic laboratory experiments that provide insights for regulatory policy. In the following, this scope is motivated both in terms of content and method.

Questions on the economic regulation of firms, markets, and economic processes are closely connected to IO. Although Tirole (1988, p. 3) "would actually like to avoid giving a precise definition of the field, as its frontiers are fuzzy", he nevertheless suggests that there are two different perspectives on the field. The first takes an insider's point of view and is thus concerned with the internal organization and market behavior of a firm, which constitute its business strategy. The second takes an outsider's point of view and encompasses assessments of market efficiency. In welfare economics, market efficiency refers to a Pareto optimal allocation of resources, i.e., a situation in which it is impossible to make any individual better off without making at least one individual worse off. Pareto efficiency is a necessary condition for a market outcome that maximizes total welfare—however, the concept does not consider whether an allocation is socially desirable or not. The first fundamental theorem of welfare economics states that any competitive (Walrasian) equilibrium at which goods are sold at marginal cost results in a Pareto efficient resource allocation (see, e.g., Mas-Colell et al., 1995). Yet, most real markets, e.g., oligopolies, are imperfectly competitive and will thus also deviate from the maximum of social welfare. Thus, following Tirole's outsider's perspective, IO may be defined as "the theory and empirical evidence of imperfectly competitive markets" (Tremblay and Tremblay, 2012, p. 3). Moreover, IO analyzes whether and how government intervention may help to improve market outcomes in imperfectly competitive markets. Joskow and Noll (1994, p. 367) differentiate two types of regulation:

> "Economic" regulation controls profits, sets prices, and determines who can participate in a market or use a particular resource. "Social" regulation controls polluting by-products of production, sets health and safety standards for products and workplaces, restricts the content of information provided by sellers through advertising and other means of describing products to consumers, and establishes requirements to protect buyers from fraudulent, discriminatory, or incompetent behavior by sellers.

In this vein, the contextual scope of this literature review are executive and legislative forms of *economic regulation* and thus encompass interventions and rules determining firms' behavior on and the functioning of regulated markets. Issues of *social regulation* go beyond the constraints of this review.

The methodological scope encompasses economic laboratory experimentation. Whereas (laboratory) experiments have been an established research methodology in the natural sciences for hundreds of years, its application to economics, i.e., a social science, is a comparably new idea. Chamberlin (1948) is considered to have conducted the very first market experiments. In his classroom experiments, Chamberlin studied whether theoretical predictions of posted offer markets prevail in the laboratory environment with his students representing buyers and sellers. He found that the experimental markets were imperfectly competitive, which prompted him to conclude: The advantage of experimentation is "to study in isolation and under known conditions", whereas "the data of real life are necessarily the product of many influences other than those which it is desired to isolate" (Chamberlin, 1948, p. 95). These findings have stimulated the emergence of a new economic methodology. It is situated between the stringent propositions of theory—which are yet based on abstracting assumptions—on the one hand and the externally valid evidence of empirical research—which yet lacks explanatory underpinning—on the other hand.

Between these poles the application of economic laboratory experiments for regulatory policy may be fruitful for three reasons. First, experiments can test a specific theory. Within the controlled environment of the laboratory, the assumptions of a theory, e.g., non-cooperative behavior, may be induced by design so that theoretical predictions are tested by comparison to the behavior of real human subjects making real decisions under real (e.g., monetary) incentives. By systematic variation, this allows not only to test the validity of theorems, but also to test the robustness of underlying assumptions. In this vein, experiments may provide support for or against a theory's assumptions and predictions and thus "evaluate the internal workings of a theory" (Holt, 1995, p. 353). For instance, in Chapter 7 the theoretical prediction of cost-based pricing in a model of upstream competition in vertically related markets is examined in an experimental environment subject to different regulatory regimes. Second, experiments can test specifics of the real world. In case an experiment is not motivated by theory, but rather empirical observation, the laboratory environment may be designed to closely resemble the regularities of the real-world counterpart. In particular, an experiment's environment and institution (Smith, 1982) may be formally constructed according to the economic agents, commodities, messages, allocation rules, cost functions, and process rules inherent in the real-world example. By systematic variation of the environment's or institution's characteristics, findings from the laboratory may help to reveal underlying systematics in the real world. For instance, in Chapter 4 the number of providers needed in the mobile telecommunications industry to ensure effective competition is determined in a laboratory experiment by controlled variation of the number of firms, everything else being equal. Third, experiments may test the potential effects of regulatory policy legislation. By definition, empirical data on the effectiveness and potential side-effects of new, unimplemented regulatory measures is not available. Even if it is, e.g., from other countries or related markets, appropriate counterfactuals of a regulatory intervention are notoriously hard to find. Furthermore, findings from field data lack internal validity so that their application to a different context ignores potential interaction effects. However, regulatory intervention, and likewise non-intervention, can have tremendous effects on the economy, including business strategies, prices, and innovation. Due to these (opportunity) cost of regulation, policy makers depend on evidence that sheds light on the potential effects of a regulatory institution in order to make informed decisions. In this regard, laboratory experiments can serve, above and beyond theoretical predictions, as an inexpensive ex ante *test bed* for alternative regulatory institutions before incurring the costs of full-scale field trials. More specific, experiments may point to flaws in an intended regulatory policy, detect unforeseen problems, and discover design-objective tradeoffs that have not previously been considered. For instance, in Chapter 5 the effects of a uniform price regulation on conglomerate firms meeting the same rivals in several distinct geographic markets are analyzed in comparison to the unregulated case allowing for price discrimination. Because the high internal validity of findings from laboratory experiments is accompanied with a potential lack in external validity, e.g., through real-world factors (deliberately or unintentionally) neglected in the experimental design, results from experiments cannot be directly transferred to the field. However, if a regulatory measure fails in the controlled laboratory environment, there is little reason to believe that it will perform in the field (Plott, 1987). Taken together, experimentation allows to test *regulation in the lab*.

Naturally, experimentation in economics has limitations and is a complementary research methodology that may compensate for shortcomings of other methodologies, as vice versa. The criticism on experiments—which is predominantly directed towards the external validity of experimental evidence—has been extensively discussed by several scholars (see, e.g., Plott, 1989; Siakantaris, 2000; Smith, 2002; Falk and Heckman, 2009). Among the most common objections are the use of student subject pools, learning behavior, framing from instructions, too small payoffs, social preferences, and simplistic designs. Falk and Heckman (2009) discuss and reject these objections against experimentation in the social sciences altogether.

Following the motivation outlined above, the subsequent literature review focuses on experiments with distinct implications for regulatory policy. Although the boundaries of different streams of experimental economics tend to vanish, in the classification language of Davis and Holt (1993, p. 5–9) this review is centered around market experiments instead of game or individual-choice experiments. As a further limitation in scope, only experiments conducted in the laboratory are reviewed. Not only does a majority of new experimental evidence still stem from the laboratory, but regulatory measures of competition among firms can hardly be tested in the field at all. However, some remarkable exceptions (e.g., Lunander and Nilsson, 2004) exist. See Harrison and List (2004) as well as Carpenter et al. (2005) for a thorough introduction to and conceptualization of field experiments in economics, i.e., experiments conducted in a real-world context exposed to treatment conditions. Dolan and Galizzi (2014) provide a recent review of policy-relevant field experiments. In the same vein and with respect to the aim of this review, also macroeconomic laboratory experiments are not considered (see Ricciuti, 2008, for a comprehensive overview).

In a nutshell, the review encompasses economic laboratory experiments in which subjects represent firms making strategic decisions on a market and treatment variables resemble direct instruments or indirect consequences of economic regulation. The first research question addresses the evidence stemming from this body of literature. As previous overviews on (market) experiments for policy making (Davis and Wilson, 2002; Normann, 2008; Normann and Ricciuti, 2009) provide an excellent source of information on early experiments, this review is limited to works from the new millennium.

RESEARCH QUESTION 2.1. *What has been learned from economic laboratory experiments of the current millennium for regulatory policy?*

Based on the essence of the reviewed experimental evidence, qualitative and quantitative observations are derived to provide guidelines for researchers in conducting laboratory experiments for advice of policy makers in economic regulation of firms and markets. Thereby, key elements of the experimental design and procedures (see Section 2.2 for a listing) are addressed as indicated by the second research question.

RESEARCH QUESTION 2.2. *What are key design and procedural elements of an economic laboratory experiment testing a regulatory institution?*

After reviewing and analyzing previous relevant works, this chapter concludes with an assessment of the research potential of economic laboratory experiments and its contribution to regulatory policy making as captured by the third research question.

RESEARCH QUESTION 2.3. *Which regulatory policy problems are suited to be tested in the laboratory?*

The treatment of these research questions by means of a qualitative literature review is intended to provide researchers, politicians, and practitioners with a summary of experimental evidence on regulatory policy and additionally with guidelines on the design of experimental investigations in this regard.

2.2 Key variables

In accordance with the research questions, the literature review is conducted along four key variables: (i) The regulatory policy problem motivating the research, (ii) the key aspects of the experimental design to test the regulatory institution(s), (iii) the empirical results of the experiment, and (iv) the specific policy implications of these findings. Each reviewed article is investigated according to these variables and is thus analyzed on two different levels of abstraction. Whereas the first and last variable are only concerned with the real-world application and therefore independent of the research methodology, the remaining two variables cover the purely experimental part of each study and bridge between methodology and application. Thereby, a systematic overview not only of implications for regulatory policy making, but also of experimental design features of testing regulatory institutions is provided. Moreover, each of the four variables may be analyzed across the body of literature not only in connection but also independently.

The first variable on the regulatory policy problem underlying an experimental analysis is one of the two dimensions used to group the literature. Regulatory institutions are initiated by different government institutions (e.g., the parliament, departments, and other regulatory or competition authorities) and applied to a certain set of firms and/or markets. Accordingly, the policy problems discussed in the experimental literature are organized according to specific industries or regulatory policy domains.

The second variable encompasses central experimental design elements that are essential for the regulatory problem at hand and also used to arrange the literature. In terms of Smith's (1982) theory of experimentation in microeconomics, this includes the economic agents and commodities of the environment as well as the institution's messages, allocation rules, cost functions, and process rules. For the purpose of manageable length, the focus is on distinguishing design features of an experiment that separate it from other studies. Thereby, most experimental designs fall in between two extremes. First, the experiment may be designed to maximize internal validity as it tests a theoretical prediction, i.e., a hypothesis on the functioning of a regulatory institution. In this regard, the underlying model's assumptions on actions, markets, and timing are directly transferred to the laboratory. Second, the design may aim at maximum external validity as it replicates most aspects of a real-world scenario. The literature review points to characteristic features of each experimental design and tries to locate it within the above mentioned extremes.

The first two variables together are used to organize the literature. Thereby, a hierarchical approach is applied. First, in the vein of Schram (2005), experimental studies are categorized according to whether their design is mostly concerned with external validity, i.e., empirical regularities, or internal validity, i.e., theory testing. Second, experiments are classified into subcategories of industry or regulatory policy domains.

The third variable provides a sketch of an experiment's results. This is merely a summary of the empirical findings, while their implications for regulatory policy are relegated to the fourth key variable. This separation allows to assess how authors transform empirical evidence from laboratory to field. If the authors of an article suggest specific policy implications and the study is therefore considered as policy-relevant, its implications are replicated with utmost precision. If an experimental study touches on a topic of regulatory policy and is therefore also considered as policy-relevant, but lacks to provide concrete implications, they are drawn from the empirical results in the context of the corresponding regulatory policy.

2.3 Literature review

To compile the relevant literature basis for this review, a structured approach was followed (cf. Webster and Watson, 2002). First, leading economic journals publishing research on IO and laboratory experiments have been thoroughly searched. The keyword prefixes "experiment" and "market" were used (separately and in conjunction) to identify relevant articles in these journals. Second, the literature cited in previous overviews on experimental markets that are (at least remotely) related to policy making (Plott, 1982, 1989; Holt, 1995; Davis and Wilson, 2002; Wilson, 2007; Normann, 2008; Götte and Schmutzler, 2009; Normann and Ricciuti, 2009; Wilson, 2011; Potters and Suetens, 2013; Friesen and Gangadharan, 2013; Müller and Normann, 2014) has been included.

With the body of literature accruing from the first two steps, further relevant articles were thirdly identified by backward and forward citation browsing. While the former is a means to find prior important work in references, the latter allows to identify more recent relevant studies by searching through articles citing one of the previous relevant articles. Forward searching was conducted using *Google Scholar*.[4] As a fourth and last step, keyword searches in *Google Scholar* were conducted to find relevant articles in other journals that have not previously been considered as well as in conference proceedings. In the following, therof all articles published since the beginning of the millennium in a peer-reviewed outlet (journal or conference proceeding) are considered and analyzed according to the key variables.

[4]Since its release in 2004, *Google Scholar* has repeatedly been compared to similar services such as *Thomson Reuters' Web of Science* or *Elsevier*'s *Scopus*. In a comparative analysis, Meho and Yang (2007) find that *Google Scholar* reports more citations than its competitors as it also considers conference publications, dissertations, theses, and books. Kousha and Thelwall (2007, p. 1055) reach comparable results and conclude that *Google Scholar* is "more comprehensive for social sciences".

2.3.1 Laboratory experiments maximizing external validity

The first major category encompasses all economic laboratory experiments specifically designed to resemble a real-world market scenario as closely as possible. These studies were motivated by empirical regularities and suggest potential real-world implications of different regulatory institutions in specific markets. The authors of these works thus aim at maximizing the external validity of their experimental results. Naturally, this comes at the cost of reduced generalizability of the effects of a regulatory institution due to the uniqueness and specificity of experimental markets' designs. The list of industries covered by this category of experiments ranges from network industries such as energy markets, telecommunications markets, and railway markets via health care issues to mechanisms of environmental protection such as emission trading markets and water allocation.

Energy

Studies addressing regulatory policy with respect to the energy industry range across different energy sources, infrastructures for the transport of energy, and energy markets. In fact, some of the very first market experiments designed in reference to a specific industry cover regulatory issues of (then recently) liberalized energy markets. For example, in a series of experiments partly sponsored by the US Federal Energy Regulatory Commission, McCabe et al. (1989, 1990, 1991) and Rassenti et al. (1994) address the coordination problem of separate contracts for purchase and transportation of natural gas and suggest that centrally controlled uniform price sealed-bid double auctions may result in a more efficient, i.e., welfare-enhancing, allocation of resources. An early version of the mechanism was in fact adopted by a pipeline owner. Even earlier than that, Grether and Plott (1984) studied and found evidence for anti-competitive effects of price announcements and best price guarantees in the industry for lead antiknock gasoline additives after the US Federal Trade Commission had filed a case against the four major antiknock compound manufacturers. These and other early works are comprehensively reviewed by Rassenti and Smith (1998), Kiesling (2005), Staropoli and Jullien (2006), and Normann and Ricciuti (2009).

Surprisingly, more recent experimental work from the current millennium on the regulation of energy markets, as summarized in Table A.1, is exclusively located in the electricity industry.[5] Many of those studies are directly evoked or indirectly motivated by the debate on reforming the electricity market in England and Wales between 1997 and 2001 or by the 2000/2001 electricity crisis in California and subsequent regulatory interventions. In this vein, many of these experimental works are funded by regulatory authorities and their perennial regulatory policy issues are the anti-competitive effects of market power and other detrimental effects evoking allocative inefficiencies in oligopolistic wholesale electricity markets. In particular, experiments are used to assess the potential of various auction mechanisms, seller concentration, price caps, and demand-side management in mitigating the market power of electricity producers and thus increasing efficiency.

All experimental designs are motivated by empirical phenomena of electricity markets and capture intricacies of their real-world counterparts. Some of the designs resemble the real world in great detail. Considered electricity market regularities include the differentiation between baseload, midload, and peakload, cost asymmetries of power generation, supply outages and generator failures, power flows in a specified grid of nodes, transmission losses and constraints, demand uncertainty and shocks, and minimum capacity requirements. Consequently, the list of simplifying assumptions apart from those common to laboratory economic experiments in general is comparably short. Most researchers assume that all buyers in their experimental markets are electricity service providers and that all sellers are electricity producers which do not sell to other producers. Some scholars assume that there are no transmission constraints or that demand is certain. Each of the experiments employs an auction mechanism to allocate supply and demand. As multiple units of electricity are auctioned, most of the mechanisms are based on multiunit auctions. The discriminatory price variant extends the

[5] All tables referred to in this chapter are relegated to Appendix A.

first price single-unit auction and winners are paid the prices of their offers. Instead, in the uniform price variant, which is analogue to the second price single-unit auction, all winners are paid the same price regardless of their actual offers. Sellers of electricity are controlled by human participants in all experiments. However, designs differ in whether the demand-side is computerized (one-sided auction) or controlled by human subjects as well (double auction). With respect to the complexity and high level of detail incorporated in these experimental designs, it is not surprising that a group of researchers at Cornell University developed *PowerWeb*, an experimental software designated to conduct electricity markets in an environment closely resembling real-world characteristics (Zimmerman et al., 1999; Zimmerman and Thomas, 2004). An interesting deviation from standard experimental procedures, which follows from the complexity of the mechanisms studied and emphasizes their policy relevance, is that several studies rely on previously-experienced students or even industry professionals to portray sellers and/or (wholesale) buyers of electricity.

The key results of the experiments indicate clear implications for regulatory policy. First, a uniform price sealed-bid auction is suggested to be most effective in mitigating market power in oligopolistic wholesale electricity markets as it is connected with higher efficiency than a uniform price continuous auction (Denton et al., 2001b,a), a discriminatory price auction (Abbink et al., 2003; Rassenti et al., 2003b), or hybrid forms of uniform and discriminatory price auctions (Thomas et al., 2002; Mount et al., 2002; Vossler et al., 2009).[6] Second, demand-side bidding is found to dampen the exertion of market power better than one-sided auctioning (Weiss, 2002; Rassenti et al., 2003a); a result that also applies to retail markets and auction participation by consumers, i.e., demand-side management (Adilov et al., 2005). Double auctions have proven to be very robust in the sense that they lead to competitive outcomes in a number of circumstances. Third, forward contracting of capacity (Mount and Maneevitjit, 2008; Brandts

[6]Results from agent-based simulation models support these findings. Bower and Bunn (2001) simulate the England and Wales electricity market of 1999 with seller agents adopting a strategy based on reinforcement learning. The discriminatory price auction is found to result in higher market prices than the uniform price auction because bid and market prices are not publicly available so that agents with a large market share gain a significant informational advantage. Yet, Nicolaisen et al. (2001) find high levels of market efficiency in a discriminatory price double auction with reinforcement learning agents, but do not compare their results to a uniform price auction.

et al., 2008; van Koten and Ortmann, 2013) or financial transmission rights (Kench, 2004) intensify competition and may further reduce allocation inefficiencies. However, this finding does not carry over to dynamic efficiency with respect to investments in monopolistic network capacity (Henze et al., 2012). Fourth, price caps implemented by the regulator, either based on reference offers (Kiesling and Wilson, 2007; Shawhan et al., 2011) or fixed exogenously (Le Coq and Orzen, 2012; Henze et al., 2012), may also effectively mitigate market power. Fifth and lastly, the implications of seller concentration are controversial. Whereas some studies suggest that electricity markets with three or four firms may already lead to effective competition (Denton et al., 2001a; Weiss, 2002; Bernard et al., 2005), other experiments indicate that under some circumstances six sellers may be required (Denton et al., 2001b) or that prices may even be supra-competitive in a market with as many as 24 firms (Chapman et al., 2004).

Telecommunication

Laboratory experiments resembling empirical regularities of the telecommunications industry and drawing specific regulatory policy implications are exclusively found in the design of auction mechanism for licensing of radio spectrum.[7] As all rules of spectrum auctions are controlled by national regulatory authorities, spectrum auctions are conducted in a highly controlled environment specifically designed for its purpose. Therefore, spectrum auctions in the real world conform to the laboratory conditions of experimentation and thus, laboratory experiments are particularly suited to assess, evaluate, and conduct *wind tunnel* tests of alternative designs and rules of spectrum auctions. Only few other real-world processes allow for such an accurate replication in the laboratory, which ensures exceptionally high external validity of experimental findings. Consequently, experimental studies have been particularly influential in the design of spectrum auctions in many countries, in particular for the British 3G/UMTS

[7]Other laboratory investigations specifically designed to study real-world issues in telecommunications markets do not address regulatory policy. See, e.g., Friedman and Huberman (2005) for software agent and human real-time behavior in congested broadband networks and Kaskiris et al. (2006, 2007) for applications of combinatorial auctions to allocate (excess) broadband capacity as a commodity good.

auction in 2000 (Abbink et al., 2005) and for the US 4G/LTE auction in 2008 (Goeree and Holt, 2010).

Table A.2 lists all studies in the telecommunications category and allows to identify two broad streams of experimental literature in spectrum auction design since the beginning of the current millennium. Early works until 2005 study predominantly the efficiency of simultaneous multiround auctions, largely used in 2G and 3G spectrum auctions. More recent studies, published since 2010, try to engineer more efficient, less error-prone alternatives and seek these in combinatorial multiround auctions mostly applied in recent 4G spectrum auctions.

Spectrum auctions are multiunit auctions. Their purpose is to allocate multiple licenses for the (exclusive) use of a block of radio spectrum in a given geographical region (e.g., national or sub-national) for a limited period of time. Auction formats used in early spectrum auctions are generalizations of the single-unit ascending (also known as English) auction, in which each bid has to be greater than the bid that preceded it. The auction's duration is endogenous as it ends only if no higher bid follows. The most common multiunit extension of this procedure for spectrum auctions is the simultaneous multiround ascending auction, according to which bidders may place single-item bids on any license block in a subset of all offered licenses. The auction runs in multiple discrete rounds and, analogously, ends if no bidder places a higher bid on any of the offered licenses. However, this auction format entails an exposure problem, which refers to the risk for a bidder to make a loss in case of winning only a fraction of the desired license bundle. Although bids may be placed on multiple licenses, each single bid entails only a single license and may not be placed on a bundle of licenses. In case a bundle of licenses is valued more than the sum of individual licenses, the bidder may place bids on single licenses exceeding the valuation for the single license. Therefore, winning not the whole bundle may result in losses, e.g., due to overbidding for single licenses. Such value synergies for a bundle of licenses, also referred to as value complementarities or supperadditivity, may arise from geographic adjacency, e.g., as a mobile network operator is already operating in a neighboring area, or the number of licenses, e.g., due to consumers valuing larger networks or due to shared fixed costs that have to be incurred prior to extracting value from any acquired license in a bundle. Combinatorial auctions avoid this exposure problem by allowing for bids on indivisible bundles—but at the cost of complexity. Bidders in combinatorial auctions may place bids on any bundle of items in the auction. While this package bidding allows auction participants to bid precisely according to their own valuations for single items and various bundles of items, the number of potential bids is substantially higher than in a simultaneous multiround auction and exponentially increasing in the number of items in the auction. In particular, a bidder who is targeting two licenses but also values each license individually would have to place a total of three bids, i.e., two bids on the single licenses and one on the bundle of both licenses. Numerous variants of the combinatorial auction were suggested by researchers, practitioners, or regulators and have been investigated in the literature. First, prices of single licenses or license bundles may either increase by submitted bids as, e.g., in the combinatorial multiround ascending auction or automatically by a clock as in a combinatorial clock auction. With both auction formats, the auction usually ends if no higher bid is placed on any item or bundle. Second, complexity for bidders may be reduced by restricting the variety of bundles they can bid on. Third, bidders may be allowed to have only one provisionally winning bid in any given round as they can bid on any self-specified bundle. This is referred to as a XOR (read: exclusive or) bidding rule to distinguish it from an OR bidding rule, according to which multiple winning bids are allowed at any time. In all multiunit formats for spectrum auctions studied in the laboratory, eligibility rules, activity obligations, and increment requirements may be imposed to spur bidding on the one hand and avoid jump bidding on the other hand. Some experimental designs mirror the rules used in previous spectrum auctions or considered for prospective ones by national regulatory authorities even more closely and thereby derive a precise understanding of properties of these auction rules. In a nutshell, spectrum auction experiments may be viewed as a means of "applied mechanism design" (Banks et al., 2003, p. 314). A software suite specifically designed for the purpose of auction experiments is *jAuctions*, developed by Jacob Goeree.[8]

[8]See `http://people.hss.caltech.edu/~jkg/jAuctions.html`. Accessed January 29, 2016.

In 1994, the US Federal Communications Commission conducted its first spectrum auction and utilized the simultaneous multiunit ascending auction format. Banks et al. (2003) find that strong eligibility rules restricting bidding behavior may hinder the assignment efficiency of licenses in this auction format and conclude that an alternative application of a combinatorial multiround ascending auction may increase efficiency, but also auction length. Motivated by the 3G spectrum auctions in the UK and Germany, Abbink et al. (2005) and Seifert and Ehrhart (2005) compared the performance of previously considered auction formats.[9] They find that the simultaneous multiround ascending auction leads to high efficiency and is not outperformed by hybrid auction mechanisms that add an additional single round sealed-bid auction or that split the auctioning of licenses in two phases. In the vein of mechanism design, newer experimental studies try to increase the efficiency of allocation methods recently used for spectrum auction. Their key findings can be summarized in two major policy implications. First, the assumed nature and magnitude of license value synergies matters greatly for the performance comparison between combinatorial and simultaneous auction formats. If license value synergies are high or distributed such that they are easy to exploit, combinatorial multiround ascending auctions are more efficient than simultaneous multiround ascending auctions, irrespective of whether prices increase by submitted bids or automatically by a clock driven by excess demand (Brunner et al., 2010; Kagel et al., 2010, 2014). In case the opposite holds and license value synergies are low or complex, this result may be reversed (see also Bichler et al., 2013). Second, bidders' preselection of a small number of packages appears to be the main source of allocative inefficiencies in combinatorial spectrum auctions. Reducing the number of possible bundles through predetermination of license packages by the regulator (Goeree and Holt, 2010; Scheffel et al., 2012) or simplifying the bid language and the payment rule (Bichler et al., 2014) increases efficiency in combinatorial multiround auctions significantly. In summary,

[9] The 3G spectrum auctions in the UK and Germany are considered the highest-grossing auctions ever raising almost EUR 39 billion and EUR 51 billion, respectively (Binmore and Klemperer, 2002). Results from the 2000 UK 3G spectrum auction are reported at `http://www.ofcom.org.uk/static/archive/spectrumauctions/auction/auction_index.htm`. Final prices from the 3G spectrum auction in Germany during the same year may be found at `http://www.bundesnetzagentur.de/SharedDocs/Pressemitteilungen/DE/2000/000818UMTS-Versteigerung.html`. Both sources accessed January 29, 2016.

regulators appear to face a trade-off between the exposure problem of the simultaneous multiround auctions and the complexity of combinatorial multiround auctions as both negatively impact allocation efficiency.

Transportation

The third and last category of network industries in this review encompasses the regulation of mechanisms to allocate limited capacity in transportation systems. Allocation problems arising in transportation industries are characterized by uncertainty and indivisibility of demand as well as an inelastic supply due to fixed capacities (Banks et al., 1989). Pre-millenium experimental investigations on transportation systems include bulk commodity transportation on inland waterways (Hong and Plott, 1982), take-off and landing slots on airports (Grether et al., 1981; Rassenti et al., 1982), space shuttle payload resources (Ledyard et al., 1994; Plott and Porter, 1996; Ledyard et al., 2000), and railway track capacity (Brewer and Plott, 1996; Nilsson, 1999). Again, these early works are thoroughly reviewed in previous literature overviews.

Newer experimental work on the regulation of transportation systems from the current millennium, as summarized in Table A.3, has exclusively added to the literature on deregulated and vertically separated railway industries. There are also more recent laboratory experiments on transportation systems (e.g., Holguín-Veras et al., 2011) that, however, deal rather with the interaction between shippers and carriers of freight rather than the regulation of a (state-owned) transportation network infrastructure. Deck and Smith (2013) provide a recent review of these works.

Due to its network properties, experimental designs of regulated railway industries share several characteristics of electricity markets in the laboratory. This applies, first and foremost, to capacity constraints of each edge in the network. These are aggravated by the fact that a single track may only be used in one direction at the same time. Therefore, positive and negative externalities from potential conflicts between diverging preferences of train operators arise. In contrast to other network industries, the allocation of track capacity not only requires the assignment of usage rights, but also a scheduling of trains that accounts for the time a train needs to travel between two stations and the path dependencies arising from this. In other words, a train that has traveled from A to B may obviously not subsequently travel from A to C before it has previously returned to A. The experiments discussed here have carefully captured these regularities. In particular, they consider a state-owned railway network with two or more interconnected nodes and study the effects of different auction mechanisms to allocate rights of use on the network between private train operators providing passenger rail service.

Railway privatization started in the US, Japan, and Sweden in the 1980s and continued in several European and American countries in the 1990s. Irrespective of the subsidization of passenger railway services in many countries, two broad streams of privatization strategies may be differentiated. The first stream encompasses full privatizations of vertically integrated railway companies as in the US or Canada with duopolistic railway infrastructure competition. The second stream refers to access regulation or vertical separation introducing competition among train operators on a monopoly railway infrastructure as in several European countries.[10] The experimental studies reviewed here consider this latter stream. In a study commissioned by the Dutch Ministry of Transport, Cox et al. (2002) show that the allocation of regional monopoly usage rights of routes with regulated minimum schedules leads to higher consumer and total surplus than auctioning each time-slot on every route individually. Isacsson and Nilsson (2003) put a stronger focus on designing an efficient auction mechanism for individual train connections and find that multiround ascending auctions and a second-price sealed-bid auction are similarly efficient. Recent working papers (de Jong, 2012; Dixit et al., 2015) provide a more comprehensive review of experimental evidence in transportation industries and are not limited to issues of regulatory policy.

[10]Qualitative and empirical ex post assessments of railway privatizations attest mixed success of both streams of privatization strategies (see, e.g., Pittman, 2005; Alexandersson, 2009; Boardman et al., 2013).

Emissions

Cason (2010), Friesen and Gangadharan (2013) as well as Noussair and van Soest (2014) provide recent overviews of experimental evidence on environmental markets, including emission permit trading institutions (this subsection), conservation auctions (next subsection), and water markets (next but one subsection). Most of the experimental endeavors they summarize address (economic) regulatory issues and many do so by means of an experimental design that resembles specific regularities of a real-world market. The existence of these recent overview articles on experimental environmental markets highlights that issues of environmental protection are a vivid area of experimental economics. In fact, none of the afore-stated industries has evoked as many experimental publications on regulatory policy as environmental markets in recent years. For this reason, with respect to the existing literature overviews, the review in this and the following two subsections deviates from the scope set out in the introduction and is limited to the most recent experimental works published during this decade.

To cut the ongoing pollution is one of the greatest challenges of this century. In order to avert global warming, several countries or multinational political unions have imposed regulatory measures aimed at cutting back emissions. Often, a cornerstone of these measures is emission permit trading, a market-based mechanism that relies on financial incentives to abate emissions. Its rationale is to prohibit a firm to emit a certain greenhouse gas unless it holds a corresponding number of allowances, i.e., emission permits. First, a regulator sets a total cap on the amount of emissions or the permit price and distributes or auctions a specified number of permits. Then, firms that do not have enough permits at their disposal may either acquire permits from other firms on a regulated market or invest in technology to abate their own emissions. Thereby, this cap and trade mechanism is intended to evoke an efficient allocation of emission permits and abatement investments. In 2005, the *EU Emission Trading Scheme* was launched as the first large-scale emissions trading program (Ellerman and Buchner, 2007).[11] The

[11]To date, it covers 45% of all greenhouse gas emissions in the EU and is the world's largest emission permit trading market. See the EC's fact sheet on the EU Emission Trading Scheme provided at

laboratory experiments reviewed in Table A.4 investigate design features of this and other emission trading institutions with regard to static allocative efficiency, dynamic investment efficiency, permit prices, and compliance with the regulation.

Two principles may be differentiated in the initial allocation of emission permits, i.e., firms either acquire them in an auction held by the regulator or receive a free permit allotment corresponding to their current emission levels, a process referred to as grandfathering. Experimental designs capture both not only in the pure form but also in hybrids. Auction formats utilized for the initial allocation are exclusively multiunit (sealed-bid or multiround ascending clock) auctions with a uniform pricing rule, which highlights a recent popularity of uniform price auctions compared to the earlier experimental investigations discussed in previous subsections that also covered discriminatory pricing rules. Following the initial allocations firms trade permits among each other in a double auction that either dictates price developments by a multiround ascending clock or runs continuously. Across auction formats price controls such as ceilings and floors or mechanisms fixing the total quantity of permits may be enacted. Similar to regulated industries discussed earlier, experimental emission permit auctions feature strong asymmetry and uncertainty in parameters over firms and time, e.g., in grandfathered permit endowments, investment costs in abatement technology, marginal abatement costs, and thus permit valuations. Such asymmetries are especially inherent in double auctions. For example, ceteris paribus, a firm with high (low) marginal abatement cost and low (high) initial permit endowment is assumed to bid for (offer) emission permits. Further regularities of real-word emission trading programs are transferred to the lab, including banking of emission permits for future use, emission reports and compliance audits, and penalties for excess emissions. With up to 20 auction participants interacting simultaneously, experimental emission permit auctions are among the biggest laboratory experiments in terms of cohort size.[12] Student sub-

http://ec.europa.eu/clima/publications/docs/factsheet_ets_en.pdf. Accessed January 29, 2016.

[12] In the context of experimental economics, a cohort is referred to as a group of subjects which interact with each other but do not encounter any other subjects outside their cohort. Therefore, a cohort effectively constitutes the data aggregation level of independent observations.

jects depict either emitters subject to emission compliance regulation, non-compliance speculators, or intermediaries.

Experimental findings point to systematic market outcome effects of design features in emission permit trading programs.[13] First, with regard to initial permit allocations, the uniform price sealed-bid auction is superior to other allocation mechanisms. It is more efficient than grandfathering (Grimm and Ilieva, 2013) and also more effective in aggravating cheap-talk collusion between bidders than the related multiround ascending clock auction (Mougeot et al., 2011), which is not advantageous over grandfathering (Camacho-Cuena et al., 2012). Evidence on whether emission permits should be allocated all at once or rather reserves should be available to provide additional supply after previous emission trading is ambiguous (Grimm and Ilieva, 2013; Shobe et al., 2014; Perkis et al., 2015). Second, continuous trading among firms after initial permit allotments improves allocative efficiency more than clocked trading over multiple rounds (Camacho-Cuena et al., 2012), although it may even decrease efficiency compared to an initial grandfathering allocation (Grimm and Ilieva, 2013). Third, depending on the political goal, regulators can make use of specific permit trade rules such as price controls and banking or rely on emission report audits. Whereas permit banking and a price cap at which an unlimited amount of permits may be acquired from the regulator limit price volatility and thus the risk for emitters connected to permit trading at the cost of higher emission volatility, a supply cap limits the total amount of emissions but raises permit prices (Stranlund et al., 2014; Perkis et al., 2015). Analogously, high audit probabilities cut emissions and emission report violations at the cost of higher permit prices (Stranlund et al., 2011). Fourth, investments in greener technologies are effectively incentivized through emission trading (Camacho-Cuena et al., 2012), however, permit prices do not necessarily decrease as predicted by associated emission reduc-

[13] All experiments listed in Table A.4 consider the productions of emitting firms only in a highly abstracted manner. One remarkable exception is Dormady (2014) who utilizes a very rich experimental design to simulate an emission trading program in parallel to an electricity market. Allowing for numerous real-world regularities he considers a scenario with asymmetric types of electricity producers giving rise to market power and allowing for explicit collusion between dominant firms via a chat to a symmetric no-communication control treatment. Results indicate that dominant firms rather engage in strategic capacity withholding in the energy market to reduce demand in the emissions market than the other way around. As the article does not address regulation as a treatment, it is not included in the main text of the review.

tions (Taschini et al., 2014). Fifth, allowing banks, speculators, or eco-friendly firms that are not subject to environmental regulation to participate in permit auctions intensifies competition and adds liquidity which reduces excess emissions by compliance firms (Mougeot et al., 2011; Taschini et al., 2014). Sixth and lastly, Cason and Gangadharan (2011) glance at the future of global emission reduction goals and conclude that an internationalization of emission trading programs allowing for cross-country trading would be welfare-enhancing as efforts of emission reduction could be allocated between high and low abatement cost regions.

Conservation

Conservation programs are adopted by regulators to introduce land to environmental friendly use or to establish a habitat for endangered species, ultimately serving ecological policy goals. If the land is in private hands, the public sector may buy the land directly from landowners, which is, however, connected to high cost for the land as such and for personnel to negotiate bilateral agreements. Alternatively, a regulator may specify a fixed compensation payment for voluntary land conservation, oblige landowners to conserve a share of their land for a fixed reimbursement, or, yet more flexible, set up a conservation program based on an inverse auction mechanism that financially incentivizes landowners to offer their land for ecosystem service provision.

A conservation auction may be viewed as a specific form of a call for tenders in which the traditional roles of sellers and buyers in an auction are reversed. A government agency acts as the buyer and seeks to procure conservation land use projects from private individuals. Landowners make offers for this environmental service and compete with each other for a compensation for foregone profits from agricultural use of their land. At the close of the auction the regulator will choose a combination of offers according to its preferences which generally depend on the estimated environmental benefits and the price. Recent laboratory experiments, as summarized in Table A.5, study compensation schemes in conservation programs that are either based on an inverse auction mechanism or a fixed payment and voluntary or mandatory participation.[14]

The decisive and differentiating feature of experimental conservation markets compared to other laboratory-based mechanism designs is the careful attention of geographic positioning of land that allows to control for adjacency and proximity of land parcels. Landowners are arranged either along a circle or in a rectangular area. This arrangement of landowners to one another is crucial as contiguous conservation areas are beneficial for environmental goals, e.g., for the creation of a habitat for endangered species. For this reason, regulators may provide an agglomeration bonus for conservation areas that stretch over several land parcels, which again rewards landowners if they select adjacent land parcels for conservation or even coordinate with each other to form an even bigger coherent area. This additional level of complexity, which is included in all recent experimental investigations of conservation programs, reinforces the interdependence of landowners' decisions as the value of a conservation offer or choice depends on offers or choices from adjacent landowners. The idiosyncrasies of the real world are extensively captured in the laboratory. Conservation cost, environmental benefits from conservation, and agricultural value vary between up to 12 landowners, but also across land parcels from the same landowner. The environmental service of conservation itself is either auctioned or imposed on landowners. In the latter case conservation requirements may subsequently be traded and re-allocated among landowners.

The majority of recent conservation program experiments considers an obligation to conserve (Banerjee et al., 2012, 2014; Parkhurst et al., 2015) instead of competitive auctioning of conservation activities (Banerjee et al., 2015). With respect to conservation

[14]There are two related strands of experimental literature on regularities of environmental markets, which, however, are more directed towards individual human behavior. The first strand is a subset of laboratory experiments on conservation auctions that study the trustworthiness of landowners in committing to sold conservation activities (e.g. Vogt et al., 2013; Vogt, 2015). The second strand sheds light on how the regulation or self-governance, e.g., in the absence of a regulator, of natural resources (e.g. Janssen et al., 2010; Janssen, 2015) or compliance with international environmental treaties (e.g. Cherry and McEvoy, 2013; McEvoy et al., 2015) may account for the role of human behavior and interaction in commons dilemmas. See Janssen et al. (2014) for a discussion of experimental software suites with regard to behavioral research in social-ecological systems.

requirements, a subsidized agglomeration bonus for adjacent conserved land parcels leads to better spatial coordination between landowners so that more coherent conservation areas emerge, resulting in a more efficient land use (Parkhurst et al., 2015). Coordination may be further facilitated by dissemination of information on program participation of other landowners (Banerjee et al., 2014). Similarly, allowing firms to trade initially allotted conservation requirements in a continuous double auction increases allocative efficiency due to more coherent conservation areas (Parkhurst et al., 2015). However, the number of participating landowners is crucial, with more landowners hampering regional coordination, and thus regulators can increase the likelihood of desired coordination by deliberate determination of the territory for a conservation program (Banerjee et al., 2012). If environmental services are not imposed but rather auctioned by the regulator in the first place, landowners cannot only coordinate spatially but also on prices so that an agglomeration bonus may not necessarily be beneficial. Instead, a regulator who discloses its preferences for agglomeration allows landowners to collude tacitly on higher prices whilst allocative efficiency remains unaffected compared to a situation of unknown regulator preferences (Banerjee et al., 2015).

Water

In 2010, the United Nations recognized the human right to water.[15] In most countries water is supplied by public or private utilities or directly extracted from sources such wells, springs, or rivers for individual use. Some arid regions, however, such as Australia and the west of the US, rely on water trading schemes to allocate scarce water. In these areas, water is distributed according to water rights entitling their holders—often farmers—to a certain amount or share of water. By leasing or selling these rights in an auction water may be allocated to the highest bidder. The ongoing growth in world-wide water consumption, which is paralleled by an increasing number of peo-

[15]General Assembly resolution 64/292, The human right to water and sanitation, A/RES/64/292 (3 August 2010), available at http://undocs.org/A/RES/64/292.

ple affected by water scarcity highlights the relevance of research on the regulation of water supply.[16]

Recent experimental studies contribute to the design of market mechanisms that allocate water from one or more sources to agricultural and private consumers. In particular, prioritization of water rights, water options, alternative auction formats, and other allocation mechanisms are considered and tested on their potential to increase efficiency in water trading schemes. See Table A.6 for a summary.[17]

Water markets capture features from both environmental markets as well as network industries. They allocate usage rights of a scarce resource and additionally account for physical constraints of water flow such as its unidirectional flow in canals. Designs utilized in recent water markets experiments consider the roles of water suppliers, water demanders, and, if applicable, water rights holders depending on whether the laboratory resembles a market with water provided by monopolistic or oligopolistic firms or a trading mechanism with water allocated by water rights. In the latter case, up to ten farmers holding water rights are considered to allow for comparison to catchment areas in reality. Asymmetry in water extraction cost and water rights allocations as well as uncertainty in water supply, water demand, and crop values, e.g., due to weather risk, are implemented for better relation to reality or even parametrized with real-world biophysical data. With regard to water rights allocation, alternative trading institutions such as auctions, bilateral bargaining, and regulated fees are compared. The experimental software suite *VeconLab* provides a program especially designed to test externalities arising along a water canal.[18]

[16]Statistics and estimates on water scarcity are provided by the United Nations at `http://www.un.org/waterforlifedecade/scarcity.shtml`. Accessed January 29, 2016.

[17]Other related experiments exhibit great parallelism with real-world water markets but do not formally benchmark and test alternative regulatory institutions. Zetland (2013) suggests a new auction format for the redistribution of initial water allocations but the experiment does not allow for a systematic comparison to other regulated auction formats. Broadbent et al. (2014) simulate the water leasing market in the Middle Rio Grande basin in New Mexico, US, in great detail and find that capital crop farmers benefit most from trading. Furthermore, Cook and Rabotyagov (2014) conduct discrete-choice experiments, i.e., conjoint analysis, on water leasing markets with real water rights holders. Lastly, preliminary results from a laboratory experiment on coordination among municipalities under an environmental subsidy policy for water quality are reported by Šauer et al. (2015).

[18]See `http://veconlab.econ.virginia.edu/`. Accessed January 29, 2016.

In line with findings from other industries, trading among farmers in a uniform price sealed-bid double auction leads to highly efficient allocations of water rights that are not inferior to a continuous double auction (Tisdell, 2011). This superiority holds also over bilateral bargaining and cheap-talk agreements in a scenario of priority usage rights on water that arise from different locations of farmers along a canal of unidirectional water flow (Holt et al., 2012). The efficiency of allocations from continuous double auctions may, nonetheless, be improved by trade of priority-differentiated water rights. Thereby, the risk of exclusion from water provision is implicitly allocated as high security water rights provide priority water access over their low security counterparts in case of water scarcity. However, the efficiency gain from this additional layer of complexity can only be realized with low transaction cost (Lefebvre et al., 2012). Comparably, more efficient allocations result from continuous double auctions if, additionally to water rights, options for water rights are auctioned, allowing their holders to purchase water at a fixed price when it is needed (Hansen et al., 2014). Moreover, water options help to equalize gains from trade across water sellers in a water market monopsony with a single buyer of water, e.g., a big city. In case of strong market concentration on the supply side it may be assumed that water prices are announced by the supplier(s) rather than determined by a double auction. In line with common knowledge, if water is supplied by a privately owned monopolist households and farmers pay higher prices than in a duopoly—provided explicit collusion can be prevented—but consumers are best off in terms of both price and water quality with a monopolistic public utility acting as a social planner (García-Gallego et al., 2012a,b). However, this benefit for consumers comes at the cost of overexploitation of limited water resources.

Miscellaneous

Robust empirical evidence and success of laboratory experiments on specific industries and markets has stimulated the methodology to spread into other specialized markets as well. Deck and Wilson (2008) investigate anti-competitive firm behavior and corresponding regulation in a gasoline market. The experimental design captures four refiners and four retailers, all of which are controlled by humans, as well as computerized customers preferring one of the four gasoline brands with equal probability. Retailers and customers are organized on a 7×7 grid with retailers operating at two locations: the center and one of the four edges of the network. Regulation either permits geographic wholesale price discrimination by refiners between center and corner retailer locations (so called zone pricing) or not, which is reminiscent of a uniform price constraint. Uniform pricing in the wholesale market is found to increase retail transaction prices in the clustered center area but not the isolated edge areas as well as retailers' but not refiners' profits. Furthermore, retail transaction prices are lower throughout if each retailer is vertically integrated with a refiner. These findings lead the authors to suggest against a uniform wholesale price constraint as well as obligations against refiner-owned gasoline stations.[19]

Tisdell and Iftekhar (2013) and Iftekhar and Tisdell (2015) assess the efficiency of simultaneous and combinatorial single round auction formats to allocate geographical individual fishing quotas. These experiments share several design aspects of spectrum auctions and demonstrate comparably that the combinatorial variant is superior in terms of efficiency and auctioneer revenue. However, bidder valuations across regions of fishing quotas greatly impact allocative efficiency of both auction formats as there distribution may impact the exertion of market power. Regulators should thus scrutinize the distribution of potential value synergies.[20] The same authors (Iftekhar and Tisdell, 2014) also test auctioneer's project selection criteria and bidders' bidding flexibility in an auction for a wildlife corridor between two regions—a mechanism related to the inverse auctions of land conservation programs. Whether the regulator selects offers according to net benefits or the benefit to cost ratio does not impact the outcome. Instead, limiting bidders flexibility by allowing only a single bid per bidder reduces the total installation cost of the wildlife corridor.

[19] In a recent working paper, Haucap and Müller (2013) use a similar experimental setup as Deck and Wilson (2008) but neglect the wholesale market and instead investigate alternative measures of price volatility regulation implemented in real-world retail gasoline markets.

[20] As quota management alone may not be sufficient to resolve assignment problems in fisheries, Emery et al. (2015a,b) investigate the potential of alternative business models such as fishery closures and cooperatives to mitigate economic rent dissipation caused by allocation inefficiencies. See also Tanaka et al. (2014) for an experimental investigation of fishermen's decision on vessel size.

2.3.2 Laboratory experiments maximizing internal validity

Laboratory experiments in IO often study the effect of exogenous features of a market structure—determined by nature or chosen to match idiosyncrasies of an industry—on prices, quantities, investments, and product quality. These works contribute to our understanding of the underlying systematics of (imperfectly competitive) markets, help to identify anti-competitive firm behavior, demonstrate when markets require regulatory intervention, and are thus highly relevant to regulatory policy makers. However, these experiments rather test predictions from a theory across particular market structures than the functioning of a regulatory instrument itself. A smaller share of laboratory market experiments explicitly addresses the impact of a regulatory institution or (in)direct regulatory intervention. Experimental efforts on identifying determinants of anti-competitive firm behavior and intricate details of the market structure under which authorities should permit, intervene, or prohibit are condensed in recent reviews: By Engel (2007) with an emphasis on tacit and explicit collusion; with a broader view on static and dynamic efficiency by Potters and Suetens (2013); and, more recently, in a working paper by Engel (2015) with the explicit goal of assessing factors that are deemed anti-competitive according to merger guidelines in the EU and the US.

This second major category of the review of laboratory experiments for regulatory policy advice takes a very different view. It does not tackle the question on when to intervene, but how to do so. Thereby, it covers predominantly oligopoly and antitrust experiments and contributes to the problem of selecting an appropriate instrument to resolve the regulatory policy problem at hand. The experiments surveyed here differ in several aspects from those reviewed in the previous subsection. Foremost, the experimental design abstracts from a specific industry and is instead chosen to create a universal market setting such that it is likely not to have a significant effect on the findings. Consequential, the underlying theoretical models are, in general, less complex and have less parameters to be calibrated. Parameters are not chosen to resemble a real-world market but to create a market environment that allows for a consistent test of a regulatory institution. At the extreme, the experimental design is stripped of all elements that are not influenced by the regulatory intervention. In a nutshell, the purpose of these experiments is an assessment of the effects of a regulatory instrument prior to its implementation and/or to study it in an environment in which its pure effect may be clearly differentiated from confounding variables.

Price controls

The direct assessment of prices is undoubtedly one of the strongest instruments at hand of regulators. Prominent examples of such exogenous price determination are termination fees for interconnection in mobile telecommunications networks or transmission charges in electricity grids. The welfare effects connected to such regulatory conduct however are a matter of field studies as it leaves no room for business decision-making and thus no occasion for experimental investigation. Instead, experimentalists have tackled the impact of weaker price control mechanisms such as price floors, price ceilings, price volatility restrictions, and uniform pricing constraints. All these regulatory instruments have in common that they are aimed at safeguarding competition in markets with imperfectly competitive structures.

Table A.7 summarizes experiments on price controls. The majority of experimental designs aims at simplicity in an effort to reduce the risk of interactions between the market structure and the regulatory instrument under investigation. The prevalent competition model is one of price competition in homogeneous goods in which the lowest-pricing firm receives all demand, thus yielding a unique equilibrium of marginal cost pricing that is referred to as the Bertrand (1883) paradox. Alternatively, with price competition in differentiated goods equilibrium prices may be above marginal cost. For simplicity, both forms of price competition are referred to as Bertrand competition. Related to these modes of competition is a posted offer institution, which aims at a more realistic representation of retail markets and was already used in the (allegedly) first-ever market experiment by Chamberlin (1948). The most notable differences to homogeneous Bertrand competition are that the quantity that may be sold by each firm is constrained and that consumers have heterogeneous valuations so that they may also be embodied by human subjects in the laboratory. The experimental designs reported here, however, all computerize the demand side. Price controls are tested by implementing and abstaining from the regulation according to a between-subject or within-subject design.

The experimental findings are predominantly in favor of price controls due to their beneficial effect in fostering competition and may be summarized as follows. First, price floors above production cost ensuring firms a positive profit even at the minimum possible price increase competition rather than weaken it (Dufwenberg et al., 2007).[21] Second, price ceilings below the monopoly price likewise intensify or at least do not relax competition in contrast to the alternative hypothesis that they might constitute a focal point for tacit collusion (Engelmann and Normann, 2009; Engelmann and Müller, 2011). Third, restricting price choices by conglomerate firms to uniform prices across geographic markets exacerbates tacit collusion compared to a situation of price discrimination (Horstmann and Krämer, 2013). Fourth and notwithstanding, a restriction of the time of price increases effectively reduces the volatility but at the same time increases the level of prices by facilitating collusive behavior (Berninghaus et al., 2012).

Leniency programs

One of the most important yet toughest tasks of competition authorities is to detect cartels based on explicit coordination between firms which is deemed anti-competitive behavior detrimental to consumer and social welfare. According to antitrust law, e.g., the Sherman Act in the US or Article 101 of the Treaty on the Functioning of the EU, price-fixing agreements in a competitive environment are illegal. If a cartel is detected by a competition authority, its members are fined with regard to the damage done to

[21]Notably, Maart-Noelck et al. (2013) make a somewhat related finding in a very different setting. In their experiment, subjects do not strategically interact but choose individually when to make an investment whose net present value is increasingly uncertain over time. With a minimum value ensured as a subsidy, which the authors refer to as a price floor, investment decisions are not different than without the subsidy or price floor.

consumers and other firms. Since the beginning of this millennium, every year the EU revealed four to 10 cartels and imposed a yearly total of up to EUR 1.9 billion of fines on them.[22] A competition authority can learn about a cartel in three different ways: First through own investigation, second by a report from a third party, and third through self-reporting cartel members. The experimental studies reviewed here disregard the first two possibilities. Detecting cartels through own investigation without specific suspicion is notoriously hard. Therefore, the primary purposes of antitrust policy are not only to facilitate the exposure of cartels but also to prevent the formation of cartels in the first place.

Nowadays, many countries have included corporate leniency programs into their antitrust legislation to increase the likelihood of cartel detection by creating an incentive for cartel members to report the illegal price-fixing and thereby to deter firms from forming cartels altogether. There are three major forms of leniency for self-reporting cartel members: First a reduction in the fine that will be imposed on the cartel members, second amnesty from prosecution—which is essentially a reduction of the fine to zero—or third a financial reward or bonus from the fines payed by the other cartel members. The specific jurisdiction for a reduction or reward may be sensitive to the cartel's duration or on how cooperative self-reporting firms are. One may conjecture that these three forms of leniency are sorted by reporting incentive in ascending order and thus that the reporting of cartels and the deterrence of their formation follow accordingly. However, experimental investigations as summarized in Table A.8 suggest a more nuanced evaluation.[23]

The experimental markets are modeled either by a single round or multiround auction, by Bertrand competition in homogeneous or differentiated goods, or by a prisoner's dilemma. In case cartel formation is endogenous, all two to three firms in the market have to uniformly choose to communicate explicitly for a cartel to be established. Only then firms may communicate either via a chat or a more structured messaging protocol

[22]See the continuously updated cartel statistics of the EC at `http://ec.europa.eu/competition/cartels/statistics/statistics.pdf`. Accessed January 29, 2016.
[23]Due to access restrictions the study by Hesch (2012) is not included.

to form a non-binding price-fixing agreement. Depending on the implemented antitrust policy, firms can report a cartel to the authorities before and/or after prices are chosen. Finally, resembling investigation efforts by competition authorities some experiments implement a 10% to 15% probability for established cartels to be detected by the authority in case they have not yet been reported. Three benchmark cases for leniency programs are conceivable. First, traditional antitrust policy that relies solely on cartel detection by authority investigations without a leniency program. Second, an environment in which cartels are permitted and there is no antitrust policy in effect, i.e., the laissez-faire approach. Third, a situation in which cartels cannot be formed as explicit communication between firms is impossible and therefore price coordination may only occur tacitly.[24] The latter two cases may alternatively be thought of a situation without a leniency program and a cartel detection probability of 0% and 100%, respectively. Note that all the experimental designs allow only for market-wide cartel formation and thus the effect of price-fixing agreements on competitors are not investigated.

All experiments unisono support the notion that antitrust policy with a leniency program is more effective in reducing prices, deterring cartel formation, and facilitating cartel dissolution than the laissez-faire approach to cartel formation. On the level of different forms of leniency and their comparison to traditional antitrust policy, however, findings are more nuanced. First, an antitrust policy that is purely based on fines may even result in higher prices than laissez-faire (Bigoni et al., 2012). Second, allowing cartel members to report their activities and apply for leniency often improves but never worsens the deterrence effect on cartel formation compared to traditional antitrust policy without leniency (Apesteguia et al., 2007; Hinloopen and Soetevent, 2008; Bigoni et al., 2012; Hinloopen and Onderstal, 2014; Bigoni et al., 2015b). Third, with respect to the effect of leniency alternatives on prices, cartel formation, and cartel dissolution, experimental findings are ambiguous. Amnesty and fine reduction may be superior to a reporting reward (Apesteguia et al., 2007), it may be exactly the other way around

[24]Cheap-talk communication between firms and its collusion effect is a separate issue in experimental economics with multiple aspects, among them whether communication is costly (Andersson and Wengström, 2007), structured or free-form (Waichman et al., 2014), exogenously controlled (Fonseca and Normann, 2012), or endogenously formed (Fonseca and Normann, 2014).

(Hamaguchi et al., 2009), or there may be no significant difference (Bigoni et al., 2012). Fourth, granting leniency only to the first reporting firm and not also to firms that report on a cartel later but before an investigation is made public is, if any, beneficial to the goals of antitrust policy (Hinloopen and Soetevent, 2008; Hamaguchi et al., 2009). Fifth, leniency programs have less pronounced effects if applied to auctions (Hinloopen and Onderstal, 2014).

Mergers

Mergers and acquisitions involving large firms are subject to an inspection by competition authorities.[25] The purpose of this procedure is to assess the potential anti-competitive effects of such an undertaking. In other words, a competition authority opening a merger case aims at estimating the market power of the post-merger conglomerate and the corresponding impact on competition. The controversial practical issue of market definition and the delineation in terms of product and geography is not regarded in the experimental literature (see, e.g., Kaplow, 2010; Coate and Simons, 2012). Instead, the controlled environment of the lab allows to control for a clear market definition and investigate the isolated welfare effects of mergers. At least three types of mergers may be differentiated (Moeller and Brady, 2014). First, in a horizontal merger two firms are combined that are competitors and produce substitutes (e.g., the merger between *Telefónica* and *E-Plus* in Germany). Second, in a vertical merger two firms are combined that operate in the same value chain but on different stages of production and may be in a buyer/seller relationship (e.g., the merger between *Time Warner* and *AOL* in the US). Third, in a conglomerate merger two firms are combined that operate in different markets and have no business relationship (e.g., the merger between *General Foods* and *Philip Morris* in the US). Experimental investigations of horizontal mergers are comprehensively reviewed in Chapter 4.[26] The little attention conglomer-

[25]Although mergers in the narrow sense, i.e., between equally sized entities to form an entirely new company, are rare compared to a take over of smaller firms by larger firms, for simplicity and in line with the economic literature any consolidation of two or more firms is referred to as a merger.

[26]These experiments analyze the number effects in oligopolies, i.e., the effect of an exogenously differing number of firms on competition. Other experiments that consider merger formation as an event over

ate mergers receive in economic experiments predominantly focus on multimarket contact, which is the subject of Chapter 6. Therefore, only experiments on vertical mergers are reviewed here.

Table A.9 summarizes the extant experimental studies on the regulation of vertical mergers. They specifically address a merger between two firms that operate on different levels of the same value chain and scrutinize the effect on competitors. Thereby, the laboratory environment is utilized to control for a market structure with and without the merger. The effect on consumer welfare is not in focus. There are related experiments on anti-competitive firm behavior under vertical integration (e.g., Elliott et al., 2003) that do not perform a formal test of regulatory intervention and are thus not included here.

The experiments employ the most simplistic design of a vertical merger with just two levels of a supply chain constituting an upstream and a downstream market. The upstream market is either controlled by a monopolist making price-quantity offers or follows the logic of homogeneous Bertrand competition with the underlying assumption that downstream firms value upstream input goods equally. The downstream market is either computerized or characterized by firms competing in homogeneous Cournot (1838) competition according to which firms choose the quantity of their good instead of its price. In contrast to the Bertrand paradox, competition à la Cournot does not yield a market price at marginal cost in equilibrium even with homogeneous goods.

Vertical integration resulting from a merger between a downstream and an upstream firm raises prices (Normann, 2011) and reduces output (Martin et al., 2001) at the upstream level, both of which are indicators of a tendency to foreclose downstream competitors. However, in case the upstream market is monopolistic, public instead of secret offers to downstream firms are similarly foreclosing as vertical integration (Martin et al., 2001). Also in a duopolistic upstream market, foreclosure is facilitated as vertical

time (Lindqvist and Stennek, 2005; Huck et al., 2007; Fonseca and Normann, 2008) do not benchmark to a non-merger scenario so that these studies may not easily be adduced to compare the effects of stopping a horizontal merger or not. Furthermore, Davis (2002) and Davis and Wilson (2005, 2006) investigate a tool that is used by authorities to identify anti-competitive merger cases.

integration of one firm may serve as a collusion device for both upstream firms, which then raises prices in the downstream market, what is harmful for consumers (Normann, 2011).

Bundling

Bundling is referred to as the conduct to sell two (or more) products together at a single price. Everyday examples of its pure form are shoes and gloves: consumers cannot buy a single but only a pair of them. The individual products of bundles like these can be considered as perfect complements, i.e., a left shoe has (almost) no value without the corresponding right shoe. However, if bundled products are not perfect complements but independent, bundling is a strategic decision which may be detrimental to consumers and competitors. Thereby, a crucial factor is whether a firm sells the bundle as an additional offer to the single products or sells the bundle exclusively, thereby refusing to sell one product unless a buyer also takes another product. Whinston (1990) shows that the latter case, referred to as tying, enables a firm to leverage market power from the market of the tying product to the market of the tied product, which gives rise to foreclosure and exclusion of competitors and eventually relaxes competition to the detriment of consumers.[27] In case a competition authority investigates a bundling case and concludes that the behavior constitutes anti-competitive tying, it may impose a fine on the tying firm. As a recent example, Edelman (2015) briefly summarizes tying cases in the US and the EU before reviewing *Google*'s product bundling and concluding that it constitutes tying under antitrust law.

There are two distinct experiments that explicitly address the decision faced by competition authorities to intervene in situations of bundling (see Table A.10). By systematic variation of firms' possibility to engage in bundling, the welfare effects of bundling can directly be inferred. Thereby, the product markets are considered to be separate and

[27] Another strand of experimental literature (Landeo and Spier, 2009, 2012; Smith, 2011; Boone et al., 2014) examines the related concept of exclusivity contracts between sellers and buyers but does not consider regulatory intervention into this conduct.

independent of one another, such that without bundling demand in one market does not impact demand in the other market. Aloysius et al. (2012) conduct a related experiment without regulation and instead compare bundling to sequential pricing of two products. Their results indicate that the latter is more harmful to consumers although it is not preferred by sellers.

The experimental designs consider the most simple case of bundling of two products. Only one firm is in a dominant position as it produces both products and there may be one or more competitors. The firms compete either in a posted offer institution with limited capacity, in a homogeneous Cournot market setting, or à la von Stackelberg (1934), whose model is a modification of Cournot competition with sequential instead of simultaneous quantity decisions which results in a first-mover advantage for the so called Stackelberg leader—in this case the dominant multi-product firm that may engage in bundling.

Findings based on the two different experimental designs similarly indicate that the anti-competitive effects of bundling are nuanced.[28] On the one hand, if bundling decisions are endogenous, there are no adverse welfare effects overall as bundling occurs rarely (Caliskan et al., 2007; Muris and Smith, 2008; Hinloopen et al., 2014). On the other hand, instances of product bundling that do occur endogenously or are imposed exogenously diminish total welfare and competitors' profits (Hinloopen et al., 2014) and deter market entry into the tied market, irrespective of the existence of a competitive fringe that reduces prices in the tying market (Caliskan et al., 2007). In conclusion, there seems to be no clear ground to prohibit bundling per se, but competition authorities should scrutinize instances of product bundling on a case by case basis.

[28]See Greenlee et al. (2011) for a critical review of the experimental findings of Caliskan et al. (2007) reported in Muris and Smith (2008) with respect to the eligibility of the experimental design to test existing product bundling theory. See Muris et al. (2011) for a rebuttal.

Transparency and information

Companies and consumers seek to make informed business or purchase decisions, respectively. The real degree and value of information at hand of buyers and sellers in a market is hard to estimate. Although regulators acknowledge that information has an impact on the functioning of markets per se, the evaluation of additional information and information exchanges among firms with respect to competition is controversial (Kühn and Vives, 1995). Experimental economics can provide competition authorities with advice to whether and how they should control for the information flow in markets by assessing the competition and welfare effects of different (regulated) sets of information.

Table A.11 lists experimental studies that investigate the several instruments at hand of authorities to regulate the availability and exchange of information in markets, auctions, and industries. Information regulation instruments can be divided into three categories: Publication of aggregate industry or firm-specific data, announcement of winning and/or losing bids in state-run auctions, and an obligation of transparency about product quality. While the former two tackle the exchange of information among competitors, the latter considers information provided by firms to their consumers.

The experimental designs aim at simplicity. Market models include homogeneous as well as differentiated variants of Bertrand and Cournot competition. Two experiments are framed as an (inverse) first-price sealed-bid auction, which effectively is homogeneous price competition. One experimental design on transparency regulation is more comprehensive and endogenizes product quality. With a focus on the transparency of product quality to consumers it is, remarkably, the only study reviewed in this subsection on internal validity focused experiments that does not (fully) rely on computerized consumers. Alternative information regulations are implemented by a disclosure or concealment of prices, quantities, profits, winning bids, losing bids, and product quality of competitors in the same market or firms in other related but not competing markets. A notable difference to the market experiments reviewed above is that a majority of the information experiments employ a random matching of subjects over periods instead of a fixed matching. The usual motivation for this design choice is to avoid learning effects as much as possible.

The sum of findings suggests that there is no monotonic relationship between the amount of information and competitive intensity. First, concerning the distribution of information on firms' behavior within a single market, price and quantity competition lead to different results. With Cournot competition, providing firms with their competitors' individual output choices and profits intensifies competition compared to a situation when firms are only informed about the average or total output choice (Huck et al., 1999, 2000). However, an information status in between with firms knowing about firm-specific output choices but not profits may relax the competitive intensity and facilitate tacit collusion (Offerman et al., 2002). With Bertrand competition, the effect of different degrees of information exchange in a market may be much smaller (Huck et al., 2000). In a nutshell, instead of monotonicity, experimental evidence suggests a U shaped relationship between competition and information with competition being strongest with very little information or full transparency on competitors' actions and outcomes. Second, information on outcomes of similar but independent, separate markets can have a positive effect on coordination and is thus conjectured to work as a signaling device. With Cournot competition, informing firms about the average profit across other parallel markets drives down outputs and is thus detrimental to competition compared to both a situation with detailed knowledge about competitors in the same market or no information on other firms at all (Altavilla et al., 2006). Likewise with Bertrand competition, competition is more fierce if no information from other related but independent markets is provided (Dufwenberg and Gneezy, 2002; Bruttel, 2009). However, this finding may depend on either homogeneity of goods or a random matching of firms, as other experimental work indicates that with a differentiated Bertrand and fixed matching firms make highest profit if they have no information on competitors at all (Altavilla et al., 2006). Third, in contrast to information exchange among firms, transparency towards consumers about product quality has clear cut positive effects (Henze et al., 2015). In particular, product quality of an experience good—and thus, welfare—is monotonically increasing if consumers have less uncertainty about the quality of products offered to them.

Miscellaneous

Beyond the articles reviewed above there are further scattered experiments which investigate a particular economic regulatory instrument.[29] Offerman and Potters (2006) collect laboratory data on restricted market entry institutions. Their experimental industry has four firms that want to enter a market. Either the regulator distributes the two market entry licenses randomly—in which case the selected firms may have to incur a fixed cost—or they are allocated in a first-price sealed-bid auction. Upon market entry, five periods of duopolistic price competition in horizontally differentiated goods follow. The findings indicate that fees from restricted market entry lead to higher prices and facilitate collusion in the market per se, irrespective of whether these entry cost stem from an auction mechanism or are fixed by the regulator. In case there is only a single entry license, the post-entry monopolistic firm charges the monopoly price with entry fees as well as without.

Also related to cost but more reminiscent of a price control measure is yardstick competition, a mechanism suggested by Shleifer (1985) in which the price a firm is allowed to charge in a regulated industry is determined not only by its own cost but also by the cost of similar firms. Potters et al. (2004) compare two regulatory variants of yardstick competition between two local monopolists: Either the regulated price of each monopolist depends on the average cost of both firms or only on the other firm's cost. Thereby, firms are assumed to choose, i.e., report, their cost. Their profits are an initially increasing but concave function of these reported cost. In both cases of price determination cost are found to be above socially optimal levels. Yet, perfect tacit collusion is even

[29]The vast majority of the remaining experimental IO literature, however, is rather concerned with the anti-competitive effects of a specific oligopolistic market structure. As an exaggerated example, see the explorative (and unsuccessful) search by Gomez et al. (2008) for market characteristics that may evoke the allegedly anti-competitive conduct of predatory pricing.

more frequent and consequently prices are higher if a monopolist's price depends only on the other firm's cost.

2.4 Observations

Drawing on existing principles for economic laboratory experiments from seminal textbooks on the methodology (Davis and Holt, 1993; Kagel and Roth, 1995), this section treats the chapter's second research question and derives *additional* guidelines for the design of laboratory experiments that specifically examine issues of the economic regulation of markets from the exhaustive literature analysis in the previous section. Attention is attributed to all intrinsic and common design elements of a market experiment, including the allocation rules, demand and supply specifications of the microeconomic system, the economic agents and their action space, the parametrization of market variables, the treatment scheme, the mode of repetition and matching rules, the type of participants, and the framing of instructions. As these elements vary greatly in detail across experiments, the following conceptual framework, which is organized by observations, focuses at design and procedural issues on which the recent experimental literature as clustered in the review reveals a consensus. Matters in difference, especially between the two categories of experiments identified in the review, are clearly indicated. They highlight the ramifications of an experimenters decision to tackle an issue of regulatory policy either in a specific industry context or in a stylized but generic environment. The issue of whether a research question is suited for experimental examination is relegated to Section 2.5. Hence, for the scope of this section it is assumed that the regulatory policy problem subject to investigation is already specified. Before concentrating on the experimental design itself, the necessity of a clear identification of performance measures for the assessment of the regulatory policy is discussed.

OBSERVATION 2.1. *Quantifiable performance measures should be defined in accordance with the policy goals connected to a regulatory institution as an intermediate step between the identification of the regulatory policy problem and its experimental investigation.*

A consistent experimental evaluation of a regulatory institution requires the identification of and commitment to performance measures for a quantitative assessment. For an unbiased investigation, it is vital that performance measures are reasoned and chosen prior to designing—let alone carrying out and statistically analyzing—the experiment. The underlying general principle of ex ante hypothesis formulation, which applies to (non-exploratory) research in general, is especially relevant in the social sciences and even more so if research findings are supposed to advise policy makers. For the same reason, experimenters should point out how the choice of economic measures for the performance of a regulatory institution relates to a single or multiple policy goals such as, e.g., welfare maximization, consumer protection, or effective competition.[30] Non-transparent handling of policy goals and research objectives may give rise to doubts about the scientific nature of an experiment. Therefore, good experimental work on regulatory policy should start with (i) a clear identification of policy goals connected to the regulation at hand and (ii) precise definitions of the performance measures for a laboratory assessment of the regulation. The qualitative review reveals that all studies state the objectives of their experiment, however, only few particularly discuss how policy goals of the investigated regulation relate to these research objectives. Furthermore, experiments aimed at external validity appear to be less specific in naming and itemizing performance measures for the laboratory assessment of the regulation than experiments aimed at internal validity. As a consequence, hypotheses in studies from the former category—if specifically formulated at all—are often undirected.

In particular, the majority of experiments aimed at internal validity focus on a single performance measure. They exhibit a clear tendency towards individual or aggregated behavior with price and quantity choices being the predominant proxies. Experiments aimed at external validity are less homogeneous in this regard. Performance measure choices vary greatly across experiments, yet mostly across industries. In general, these

[30] The relevance of a transparent formulation of policy goals, research objectives, and hypotheses is further highlighted by Myrdal (1953, p. vii) who argues that it is impossible to avoid value judgments in economics and other social sciences alike: "There is an inescapable a priori element in all scientific work. Questions must be asked before answers can be given. The questions are an expression of our interest in the world, they are at bottom valuations. Valuations are thus necessarily involved already at the stage when we observe facts and carry on theoretical analysis, and not only at the stage when we draw political inferences from facts and valuations."

studies consider multiple performance measures which are not only related to firm choices but also to how these affect market outcomes.[31] Two types of studies may be broadly differentiated among experiments aimed at external validity: Those that focus on efficiency, i.e., total surplus, provided that its maximization is perceived to be the priority of regulation, and those which are rather concerned with the distribution of total surplus, in particular consumer surplus, firm profits, or public revenue (e.g., in a state-run auction). Apart from few exceptions, the performance indicators across all experiments on regulatory policy measure some form of static efficiency as dynamic efficiency inevitably has to deal with path dependencies in choice sequences which are notoriously hard to adequately control for in experiments with human subjects.

OBSERVATION 2.2. *Parallelism to the real world (salience and generalizability) is the pivotal principle of designing an economic laboratory experiment on a regulatory policy problem which is aimed at external (internal) validity of its findings.*

Naturally, the design of an experiment on an issue that is idiosyncratic to a regulated industry captures several regularities of this industry, which eventually results in an experimental environment that is more reminiscent of reality than the design of an experiment that addresses a generic regulatory instrument which is not restricted to a certain industry. In turn, an experiment of the latter type is guided by salience yielding a design as simple as possible to minimize the risk of interaction with the regulation and to obtain generalizable findings. Internal validity of laboratory data is negatively correlated with the complexity of the experimental design, whereas the opposite holds with respect to external validity of experimental findings provided that the additional complexity is a consequence of increasing parallelism to reality. Naturally, the two types of experiments outlined here are endpoints of a continuum. Yet, a comparison of experimental designs between the two broader categories reveals major differences. Generally, the commitment to internal or external validity is to a lesser extent conscious

[31] By evaluating this difference between experiments focused on internal and external validity, one has to take into account that in simple IO models, which are pervasive in the former, market outcomes are linear in aggregate choices. For instance, with homogeneous Bertrand competition consumer and producer surplus can be directly inferred from the market price, rendering an additional surplus analysis uninformative.

than implicit in the regulatory problem at hand, i.e., whether the research question is predominantly focused on a specific regulated industry or the functioning of a generic regulatory instrument.

OBSERVATION 2.3. *The microeconomic system of experiments aimed at external (internal) validity is predominantly composed of extensively regulated market mechanisms such as auctions (basic IO competition models à la Bertrand or Cournot) for which theoretical predictions may be ambiguous (are well established).*

A vast majority of 90% of articles aimed at external validity utilize auctions in their experimental designs—a finding that is likely to be caused by the controlled environment of auctions also in the real-world. Only 15% in the category of internal validity-centered experiments do so. More specifically, experiments in the former category frequently encompass more than a single allocation mechanism and thereby closely resemble the structure of a strongly regulated real-world market. Occasionally, this comes at the cost of ambiguous theoretical predictions or even the absence of analytical theoretical predictability. Instead, experimental designs in the latter category are almost exclusively limited to a single allocation mechanism—although the competition model itself is often considered a treatment variable, whereby the most common mechanisms are Bertrand competition (45%), Cournot competition (25%), and posted offer institutions (15%). This observation indicates that extensive regulation in a real-world market yields a detailed experimental environment with complex procedures. In other words, the more comprehensive a real-world market's regulation the lesser the experimenter's uncertainty in deciding on details of the microeconomic system. Also, however, it is reasonable to hypothesize strategic choices and market outcomes to be more volatile the more complex the experimental design. This provides in turn a rationale for simplistic designs of experiments which test generic regulatory theory: Any market feature requires parametrization for which, however, there is no source in lack of a specific target market. In this vein, Holt (1995, p. 361) provides a more general guideline to market experiment design: "One key to good experimental work on IO issues is to introduce the right simplifying conditions, without losing the essential features of the market environment." Clearly, Holt's principle is directed towards experimenters who aim for internal validity of their results. Acknowledging the classification of experiments accruing from the previous review, this highlights that an experimental assessment of regulatory policy requires an active consideration of the trade-off between a highly detailed or very stylized experimental design.

OBSERVATION 2.4. *Economic agents are predominantly firms embodied by human participants. In experiments aimed at external (internal) validity, the number of strategic decision-makers is chosen to resemble the corresponding real-world market (to be as low as possible). Instead, the demand side of a market is almost exclusively computerized.*

By definition with respect to the scope of this review, economic agents in each experiment are firms deciding on, e.g., the selling price or quantity to produce of their product, bids in an auction, or the quality of a product. As these firms are strategic decision makers, they are embodied by human participants in the experimental laboratory. In few exceptions, some but never all of the firms are controlled by software agents. The number of firms in market experiments on regulatory policy varies considerably depending on whether the laboratory environment is designed to resemble a specific real-world industry or not. Among experiments focusing on internal validity the distribution of the number of firms across the studies in this review has the 25th, 50th, and 75th percentiles, i.e., quartiles, $\{P_{25}, P_{50}, P_{75}\} = \{2,2,4\}$. Hence, in line with a stylized microeconomic system, the predominant (theoretically) competitive market structure of these experiments is a duopoly. The corresponding quartiles for experiments aimed at external validity are $\{P_{25}, P_{50}, P_{75}\} = \{4,6,8\}$, which clearly indicates a larger market size in terms of the number of firms. However, the medians of the number of firms across experiments in a specific regulated industry vary considerably and range between 2 and 12. In conclusion, depending on whether the experimenter aims for internal or external validity the number of economic agents in a market experiment on regulation should be chosen as low as possible or to resemble the number of competitors in the corresponding real-world market, respectively. Across both categories, the other sides of the markets under experimental investigation, i.e., the consumers in retail markets or the public authorities in state-run auctions, are almost entirely computerized. A prominent exception with a strategic demand side in experiments are wholesale markets, in which firms embodied by participants purchase a good as an input for the production of a good in another market. Yet, with regard to business-to-consumer markets all but one experiments computerize the demand side.

OBSERVATION 2.5. *The parametrization of market structure variables should always be deduced from field data. Experiments aimed at external (internal) validity utilize data which is narrow (broad) in scope but deep (shallow) in detail and consequently predominantly asymmetric (symmetric) across firms and often uncertain as well as volatile (certain and time-invariant).*

The parametrization strategies for experiments aimed at internal validity and those focused on external validity are widely different. Whereas a parametrization in the former category is stylized from the real world, it is based on regularities of a specific industry in the latter category; some of these studies indicate great efforts in capturing and processing field data to utilize it for experiment parametrization. This basic distinction has several implications for details of the parametrization. First, in 85% of experiments aimed at external validity firm-specific parameters such as endowments, cost or valuations are asymmetric across competing firms, whereas the same share of experiments focused on internal validity exhibit symmetric parameters and hence, firms. Second, firm-specific as well as market parameters that manipulate demand and supply vary over repetitions in some experiments centered at external validity, but are time-invariant in all experiments from the internal validity category. Third, the uncertainty in real-world industries about market parameters is captured in corresponding experiments, whereas complete information is always ensured in more stylized, i.e., internal validity-centered experiments. Fourth, as a consequence of the previous remarks, the choice of conducting an experiment which is aimed at maximizing either its internal or its external validity directly impacts whether the effect of parameter differences can only be evaluated in a relative manner or also—to some extent—at an absolute level. Taken together, as the purpose of any economic laboratory experiment is to make statements about real-world markets, parameter choices should always be based on related field data. The key distinction is which data and how this data is utilized for parametrization of an experiment, i.e., whether the data is taken from a specific industry and reflected in the experimental design in detail or whether the data is an aggregate over different industries sharing a certain feature and applied in the most simplistic way.

OBSERVATION 2.6. *The treatment design ideal is a full-factorial between-subject application of experimental conditions with atomic differences. For experiments aimed at external validity, design alterations across treatments can be greater to capture alternative regulations considered in reality.*

The ideal treatment design captures each combination of treatment variables (i.e., full-factorial) and applies them to separate groups of randomly selected subjects (i.e., between-subject). The rationale behind this gold standard of treatment design is that a simultaneous variation of multiple treatment variables does not allow to infer a causal effect of regulation as observed statistical differences in a market outcome may be evoked by a single treatment variable, i.e., monocausal, or caused by a combination of treatment variables, i.e., an interaction. A comparison of treatment schemes reveals that all experiments aimed at internal validity cover at least two treatments between which the only difference is the implementation of a single regulatory institution that is not an aggregate of multiple regulatory measures. The difference in the experimental design between these two treatments is thus not further separable, i.e., atomic. In contrast and in analogy to the complexity of the experimental design itself, differences between treatment conditions are less likely to be incremental in experiments focused on external validity than those aiming for internal validity. A regulatory policy problem in a specific industry may elicit alternative proposals that, by their nature, vary considerably. Due to budget and other constraints, an experiment designed to assess these alternative regulations consequently cannot exclusively rely on treatments with incremental differences but rather captures the extremes—a design strategy which is more generally utilized in case of continuous treatment variables, since the smaller an economic effect the stronger it has to be to show statistical significance.

Observation 2.7. *The standard mode of repetition is a fixed number of discretely separated periods that is common knowledge for all participants. Half of all experiments are composed of no more than 20 periods.*

The median regulation in the lab experiment runs for 20 periods and participants are informed about this length of the experiment. A period is an independent and repeatable part of an experiment which is interchangeable with other instances of itself. The number of periods in an experiment is a proxy of its length. Depending on how an experiment's ending is determined, the number of periods may be compared across experiments as a design element. More specific, this is only possible if the end of an experiment is imposed exogenously and publicly known. This includes both fixed and random ending rules provided that participants are informed about the fixed or minimum number of periods, respectively. If instead the end of an experiment is determined endogenously, the number of periods is not independent from the other features of the experiment and thus cannot be compared across different experimental designs. Within both categories of experiments, about 80% of all studies use a fixed ending rule. The remaining experiments in the category of internal or external validity-centered studies end after a random exogenous number of periods or an endogenous amount of time, respectively.[32] Regarding all experiments with an exogenous termination rule, the number of periods is similar across experiment categories with quartiles $\{P_{25}, P_{50}, P_{75}\} = \{12,20,30\}$ for studies aimed at external validity and $\{P_{25}, P_{50}, P_{75}\} = \{10,20,40\}$ for articles focused at internal validity. As only studies from the latter category apply exogenous random ending rules and as the minimum number of periods is used to compare the lengths of experiments, the actual number of periods may be higher in these experiments. Within each period, decisions are made simultaneously except for cases in which a specific sequence of decision-making is a direct consequence of a feature in the real-world market.

[32] All experiments with an endogenous ending investigate the regulation of spectrum auctions which run until an ending criterion depending on the bidding activity is met.

Observation 2.8. *Fixed matching with a permanent assignment of participants to cohorts over the entire time horizon is the most common matching procedure.*

Across periods, firms are matched into fixed groups in the majority of experiments, i.e., participants are initially assigned to a cohort and stay within that same cohort throughout a session. The advantage of such a matching procedure is the independence of different cohorts which facilitates subsequent statistical analysis. Depending on the research question, an associated downside may be strong learning effects due to repeated interaction with the same subjects. To carve out learning effects under a fixed matching, experimenters often cut off parts at the beginning and end of time series data. Alternative sample matching procedures are random matching, i.e., subjects are randomly rematched every period, and perfect stranger matching, i.e., each subject encounters any other subject at most once. The main purpose of these matching schemes is to reduce learning effects as participants cannot expect to meet the same fellow participants in subsequent periods or even know with certainty that they will encounter different participants every period. Consequently, the downside is that observations from different cohorts are not independent from each other. To sum up, fixed and random matching are counterparts as one turns a disadvantage of the other into an advantage and vice versa. Experiments aimed at external validity rely exclusively on fixed matching, which is arguably more reminiscent of real-world industries in which the same competitors in a market interact repeatedly (leaving out market entry and exit). Among experiments focused on internal validity, fixed matching is also prevalent with 55% of the studies utilizing such a scheme. A minority of 30% applies a random matching, which is also known as stranger matching. None of the experiments uses a perfect stranger matching. Instead, the remaining 15% treat the matching procedure as a treatment variable and apply both fixed and random matching in a between-subject manner.

Observation 2.9. *The standard and preferred subject pool with respect to the internal validity of experimental findings are students who are neither intentionally primed nor trained. The external validity of implications regarding regulatory policy can be ascertained by engaging industry professionals to participate in the lab.*

Implications of an experiment are drawn from decisions made by human participants. Demographic and other characteristics may influence the behavior of participants. Consequently, the process according to which an experiment's participants are selected is crucial to its success. The literature on economic laboratory experiments differentiates broadly between students, experts, and the general public as common subject pools. Common designations for experts in IO experiments are managers, practitioners, or industry professionals, indicating the decision makers who make the real-world decisions depicted in the laboratory. Experimental assessments the behavior of different subject pools have not eventuated in a complete set of systematic similarities and differences but call for a case by case evaluation. A majority of experimental IO studies that compared managers' and students' (e.g., Waichman et al., 2014) or industry professionals' and students' (see Chapter 7) behavior find similar behavior in the laboratory environment. Yet, concerning experiments on regulatory policy and with respect to the external validity of experimental findings, a subject pool of industry professionals appears to be most preferable as a random sample would be representative of the population of decision makers in firms. However, industry professionals can be primed, e.g., if they contemplate on the experiment's purpose and adapt their behavior to elicit a market outcome which they assume to result in more favorable regulation. Moreover, recruiting industry professionals for participation in an experiment may proof difficult in case the purpose of the experiment has to remain unknown. Regarding the internal validity of laboratory experiments, the appropriate subject pool is yet less clearly identifiable since industry professionals and students alike may be framed when making decisions in the lab. Just like students could be influenced by their field of study, professionals' behavior is likely to be affected by the industry they work in. Reviewed experiments aimed at internal validity rely exclusively on students, which is in line with the purpose of generic results as students who are inexperienced with the mechanisms of the experimental market are less likely to be primed than industry professionals. Subject pools in 75% of studies focused on external validity are also constituted of inexperienced students. In 10% of the articles industry professionals are engaged, which is motivated by a conjectured positive effect on the external validity. The remaining 15% of the experiments report to use experienced students, i.e., student subjects who have been extensively trained in the mechanics of the experiment—often for multiple hours. This last subject pool alternative is commonly reasoned by high complexity of the experimental design that makes training necessary. However, it is likely that deliberately induced experience of student participants affects their behavior during the experiment itself due to potential path dependencies from training sessions. Therefore, a subject pool of experienced students may combine the disadvantageous of both engaging inexperienced students or industry professionals without making use of the respective advantageous.

OBSERVATION 2.10. *There is no general rule on framing in IO experiments on regulatory policy. Yet, a majority of experiments aimed at external (internal) validity is framed in the corresponding industry context (in industry-unspecific economic terms).*

Framing refers to the context that is provided to the participants. Its purpose is to contribute to an environment in which participants can relate to the real-world significance of their decisions in the lab. So far, no clear standard has evolved in experimental economics regarding the framing of laboratory environments. This holds likewise for the subset of experiments on regulatory policy. Authors refer to different kinds of framing in their studies, the most common being context framing, economic framing, and neutral framing. The first usually refers to experiments in which participants are confronted with special vocabulary to induce a certain real-world context, e.g., a specific regulated industry. The second broadly covers experiments relying on economic terms such as 'market', 'firm', and 'price' without mentioning a specific industry. The third and last type of framing is predominantly stated by experimenters to describe that they abstained from economic terms in their experimental instructions altogether. Naturally, context framing, i.e., a framing within a specific industry, applies to a majority of experiments aimed at external validity, whereas experiments focused on internal validity fall rather into the categories of economic or neutral framing. However, these terms of framing types are used inconsistently so that the categories are too fuzzy to allow for a serious quantitative assessment of different kinds of framing above and beyond the qualitative evaluation. This observation is in line with a claim by Loewenstein (1999, p.

30) regarding neutral, i.e., abstract and context-free, framing: "The context-free experiment is, of course, an elusive goal. An egg-shaped cage provides the same amount of context, albeit somewhat more alien, as any other environment." In other words, there is no absence of framing, but merely different framing. For instance, instead of providing participants with the information that they are forming a cartel which is subject to a cartel detection probability, Hamaguchi et al. (2009, p. 150) tell their participants that they participate in a "payoff reduction lottery". Clearly, this term constitutes framing as well—although a very different one than cartel detection. The impossibility to avoid framing in designs and instructions of laboratory experiments on regulatory policy calls for extensive research on the effect of different types of framing.

2.5 Discussion

Experimental economics contributes substantially to issues of economic regulation. The extensive review conducted in this chapter provides an overview of regulatory policy problems that can be investigated in the laboratory. Moreover, treating the experimental design as a separate key variable in the review allows for a specific analysis of commonalities and differences in design features across studies. This resulted in the 10 observations listed in the previous section and represents the state of the art of regulation in the lab. This section concludes the chapter with a discussion on the potential of experimentation in economics to examine regulatory policy problems and to advise policy makers according to implications deduced from experimental findings.

An analysis of policy problems addressed by the reviewed experiments points to two broad categories: allocation problems and issues of antitrust—with experiments aimed at external validity falling into both categories, whereas the strand of articles focused on internal validity is largely covered by the second category. Moreover, a majority of external validity-centered experiments employs auctions, which are used in strongly regulated environments and simulate upper parts of the value chain, i.e., they rarely consider retail markets with consumers as buyers. These are in turn common for internal validity-centered experiments. In sum, experimental examination is not limited to certain parts of the value chain and is especially useful for issues of economic regulation that address the allocation of a scarce resource or the mitigation of market power and anti-competitive behavior. It is virtually impossible to assess the true implications of experimental work on regulatory policy, however, there is anecdotal evidence regarding the experiments resembling intricacies of a specific real-world market: Several of these experiments are funded by government institutions and many of them are conducted in countries or by authors from countries in which the regulation of the respective industry was undergoing drastic change by the time of the experiment.

Over and beyond the scope of this review, laboratory experimentation is utilized not only to examine issues of economic but also of social regulation. The range of policy problems addressed by the experiments reviewed in this chapter indicate six generic application possibilities which apply likewise to all fields of regulatory policy. First, laboratory experimentation is a test bed for regulatory policy and thus provides further insight into whether a policy will work as predicted in the real world or not. Second, experiments can provide complementary evidence with respect to existing economic theory on a regulatory measure. Third, two competing theories can be compared ceteris paribus in a controlled laboratory environment. Fourth, an already implemented regulatory institution can be reassessed in the laboratory in case its impact is hard to assess in empirical data. Fifth, experiments can support the design of completely new, regulated markets (e.g., emission permit auctions). Sixth, owing to its necessary simplicity, experiments may even be a tool to communicate and canvass for new regulation.

However, laboratory experiments are merely a complementary methodology in research on regulatory policy—not only compared to theoretical models but also with respect to empirical methodologies. In comparison to other empirical methodologies in economics, Griliches (1986, p. 1466) states: "If the data were perfect, collected from well-designed randomized experiments, there would hardly be room for a separate field of econometrics." This remark implicitly points out the impossibility of controlled experimentation in the real world. Instead, high internal validity of an empirical research methodology is generally paralleled by a low external validity, and vice versa. In particular, laboratory experiments may be ranked top on a scale of internal validity of empirical research methodologies, but at the same time at the bottom with respect to external validity. Following this logic, empirical alternatives to laboratory experimentation are—in order of increasing external and decreasing internal validity—field (also real or natural) experiments based on controlled variation in the field, quasi-experiments inferring causal relations by e.g., instrumental variables, and computational experiments using simulations calibrated with field data. Angrist and Pischke (2010, p.23) criticize this view on external validity and argue that "empirical evidence on any given causal effect is always local, derived from a particular time, place, and research design."

In conclusion, laboratory experiments have already examined issues from all major policy fields. A juxtaposition of common governmental departments and experimental literature shows that selective issues in all departments have already been studied in the lab. Over and beyond a review on and description of the state of the art of *regulation in the lab*, this calls for further investigation into the potential of *public policy in the lab*.

Chapter 3

Sequences of Decision Making in Experiments and Tacit Collusion

DECISION making is, by its nature, a continuous process. Individuals but also organizations monitor their environment continuously and may act or react according to their observations at any time. Especially in electronic markets, e.g., online retail or financial markets, sellers and buyers may react promptly to decisions by other market participants. However, the reaction time by decision makers can also be chosen strategically (e.g., strategically delayed), or actions may be taken only for a very short period (e.g., to send a signal to a competitor or to retaliate a rival's action). In continuous time, the reaction time or duration of an action is chosen endogenously by the decision makers and thus offers a richer set of strategies than if actions can only be taken at fixed points in time.

However, most economic laboratory experiments—including the studies on regulatory policy reviewed in the previous chapter—employ a discrete time framework and thus rely on the assumption that players move simultaneously or sequentially in a predefined and ordered sequence. Consequently, subjects in the experiment have a given (limited or infinite) amount of time to decide on their actions. By this means, these experiments abstract from the decision makers' choice with regard to the timing of an action and thus implicitly restrict the space of potential strategies.

Therefore, this chapter scrutinizes the different assumptions of discrete and continuous time in economic laboratory experiments. Section 3.1 outlines the research question and implications of this study, before the extant literature on experiments in non-discrete time is reviewed and a framework of timing in experiments is suggested in Section 3.2. Section 3.3 introduces an experiment designated for a laboratory assessment of discrete and continuous time and describes its design and procedures. Empirical results of the experiment are derived in Section 3.4. Finally, Section 3.5 concludes and discusses the results' methodological and policy implications.

3.1 Motivation

An increasing number of experimental studies on repeated games deviates from the classical mode of discrete time and instead employs a real-time setting that runs continuously and in which participants, per definitionem, hold onto their actions until they change them explicitly. Such continuous time frameworks have been used, e.g., in prisoner's dilemma games (Bigoni et al., 2015a; Friedman and Oprea, 2012) as well as in Hotelling (1929) location model (Kephart and Friedman, 2015a). Also experiments on continuous auction design naturally allow for bids and offers in continuous time—however, not as a means to repeat a one-shot game over independent periods. Instead, each transaction inherently depends on the transactions preceding it and thus continuous auction experiments are not considered in this chapter.

To the best of the author's knowledge no systematic investigation on the effect of continuous versus discrete time on price and quantity competition in oligopoly experiments exists to date. It is thus unknown whether discrete and continuous time competition affect price and quantity setting behavior on imperfectly competitive experimental markets, i.e., whether one of the two time frameworks facilitates cooperation and the ability to tacitly collude more than the other. To answer this research question is precisely the aim of this chapter.

The contribution to the literature is twofold. First, this study provides an examination of non-cooperative game settings in continuous time, a time framework which arguably captures more properties of time in reality than discrete time but for which theoretical analysis is scarce and ambiguous. Second, the experiment constitutes a formal test for differences in tacit collusion between discrete time and continuous time. Thereby, it provides first evidence for a reassessment of the real-world implications of discrete time oligopoly experiments in regard to the real-world context of continuous time.

For this study, the two time frameworks of discrete (synchronous-move) and continuous (real time, i.e., asynchronous-move) time are applied to a laboratory experiment of oligopoly competition. In particular, both price (Bertrand) and quantity (Cournot) competition in duopolies and triopolies, respectively, are considered. Taken to the lab, continuous time implies that the length of a period in a repeated game is so small that subjects cannot observe distinct periods, i.e., the reaction time of the experimental software is lower than the human reaction time. This study is not the first to employ a non-discrete time framework. Also, comparisons of discrete and continuous time in lab experiments exist for specific contexts (see the following section for an overview). However, this experimental study is the first concerned with the emergence of tacit collusion under oligopolistic competition in continuous time and the first to systematically investigate the differences in outcomes between continuous and discrete time oligopoly experiments.

The key insights are as following. Irrespective of the underlying competition model (Bertrand or Cournot) and the number of firms (two or three) competitors coordinate better on collusive outcomes under discrete time than under continuous time. This is in contrast to the experiment by Friedman and Oprea (2012), who find higher levels of coordination in (repeated) continuous time than (one-shot) discrete time prisoner's dilemma games. Thus, the combined experimental evidence suggests that idiosyncrasies of the game matter to determine whether a continuous or discrete time setting facilitates cooperative behavior. Although these findings support researchers in making an informed decision on the time framework in an experiment, the potential benefit of continuous time in terms of a laboratory experiment's external validity is naturally hard to assess.

3.2 Timing in experiments

Economic lab experiments are used to verify theoretical predictions or to assess the potential implications of economic market designs in the field. For this purpose, human participants face repeated decisions in a given experimental scenario. In market experiments, repetition is usually implemented as a (fixed or random) number of successive (and otherwise independent) periods. A period does not start before all subjects have made a decision in the previous period. This yields synchronous (predominantly simultaneous) decision making by participants, which however does not correspond to most strategic interactions in reality such as competition between firms in a market. In reality, firms can make decisions about their products and prices at any given time and respond to their rivals' actions accordingly, i.e., decisions are asynchronous. In other words, in case an experiment is run with a discrete time framework in an effort to resemble the repeated nature of the real-world environment, this design is used to model a situation in which decisions are actually made in continuous time.

Since the computerization of economic lab experiments researchers implement different timing schemes. However, there is little evidence on how decision making in the lab differs between experimental setups in discrete and continuous time, although the body of literature is currently growing (Berninghaus et al., 2007; Friedman and Oprea, 2012; Oprea et al., 2014; Kephart and Friedman, 2015a; Kephart and Rose, 2015). In lack of a consensual definition across the literature, it is unclear which aspects constitute a discrete time framework and consequently, non-discrete time frameworks, in economic lab experiments. Before reviewing the extant literature, therefore, a classification of discrete and non-discrete time experiments is proposed.

3.2.1 Classification of discrete and non-discrete time experiments

In order to set out the scope of non-discrete time frameworks, a definition of what is commonly referred to as *discrete time* in experiments is necessary: Discrete time is a synchronous-move repeated games framework with an unlimited period length. A period, i.e., a discrete time step, ends only after all subjects have confirmed their decisions. All experimental modes that deviate from this set-up are thus experiments in non-discrete time and are reviewed in the next subsection. However, among the non-discrete time experiments again exist a variety of modes that require distinction.

The classification of non-discrete time experiments is motivated by Freeman and Ambady (2010), who show that the human reaction time for very simple computerized tasks as measured by the time needed to process information presented on the screen and to perform a mouse click lies above 0.5 seconds. Thus, in the most conservative way, *continuous time* in experiments is defined as a time framework with rapidly repeated periods of a fixed time length which does not exceed the threshold of human reaction time, i.e., period lengths of 0.5 seconds or below. Technically speaking, as computers perform operations in discrete steps, a computerized experiment is said to run in continuous time if the transaction time (period length) between the experimental server and its clients is smaller than the human participants' reaction time. In continuous time experiments an action instantaneously impacts profits and can be observed by other subjects accordingly. The (potential) consequences of an action cannot be tested prior to making the decision but have to be assessed meanwhile the decision itself is in effect. Moreover, with time running continuously it is virtually impossible that two participants in an experiment make decisions simultaneously. Consequently, in continuous time experiments the order or time of decision making is not exogenously given and thus, inter-period asynchronous interaction emerges naturally. Subjects can act and react upon each others' moves at a self-specified time. Profits and other outcome variables become flow values. The key aspect of a continuous time framework is thus that it endogenizes the timing of decisions and thereby captures asynchronicity in decision making as in many real-world strategic interactions, i.e., decision making which is neither simultaneous nor sequential.

Continuous time experiments need to be distinguished from those running in *near-continuous time*, which is a synchronous-move repeated games framework with constant, finite period lengths above the human reaction time, during which subjects have to decide on their action in the subsequent period. As in continuous time experiments, individual decisions are transferred from one period to the next, and hence, doing nothing results in choosing the same action as before. Without communication between subjects, decisions by rivals do not become public and profit-relevant before the end of a period. Therefore, as the reaction time is above the human decision threshold, interaction is potentially synchronized and decision making is simultaneous. Hence, as under discrete time, inter-period asynchronous interaction or even sequential moving may occur behaviorally, but not naturally. The advantages of near-continuous time in comparison to discrete time experiments are a high control over the length of the session and the possibility to collect a large amount of data in relatively short time. Thereby, patterns of repeated decisions may occur that would not have been observable in a discrete time experiment (with fewer periods). However, this time framework also bears two potential problems. First, different cognitive and physical abilities of human participants may have a greater influence on experimental results than in an experiment run in discrete time, i.e., some subjects may not be able to change actions fast enough and hence, data on intended decisions would be lost. Second, the repetition of short periods with a fixed length may induce an aspiration to use the limited amount of time in a period and adapt one's decision every period. Both caveats generally apply to continuous time experiments as well. However, since profit is a flow value in continuous time experiments, a small difference in participants' reaction times has only a relatively small impact on profits as subjects can react promptly to another subject's decision. For example, in a duopoly the additional profit gained by defecting from a cooperative state is linear in the rival's reaction time under a continuous time framework but step-wise constant under a near-continuous time framework. Consequently, for the same rival's reaction time below the near-continuous period length, (myopic) profits from defection are higher in near-continuous time than in continuous time. A potential problem of the continuous time framework—which is discussed later—is that the theoretical prediction of the repeated game may change due to its dynamic nature.

Finally, the continuous time framework has to be differentiated from a *clock or deadline mechanism*, which is a synchronous-move repeated games time framework with constant, finite period lengths under which subjects' current actions are common knowledge and may be changed (freely), but do not become binding, until a clock runs out or a deadline is reached. The currently chosen action at the time of the deadline becomes binding and constitutes the subject's profit-relevant decision for this period. Consequently, the current action of a subject may be interpreted as an intention for the final decision in the period but is profit-irrelevant, and thus, cheap talk. During a period subjects can react to each others' actions, which shall be referred to as intra-period asynchronous interaction. As Roth (1995, p. 324) points out, this experiment design gives some indication of how "last-minute agreements" in negotiations evolve.[1] With respect to experimental design, the clock or deadline mechanism is a hybrid of the continuous time framework and the near-continuous time framework. Whereas intra-period interaction between subjects (i.e., cheap talk before the deadline) is asynchronous, inter-period interaction between subjects (i.e., decision making at the deadline) is synchronized. See Roth (1995) for an overview on the effects of the clock or deadline mechanism and proposed models to explain these effects.

3.2.2 Review of non-discrete time experiments

Table 3.1 lists non-discrete time experiments in the extant literature and classifies them according to the definitions derived in the previous subsection. Apart from the type

[1] There are two further strands of experiments that implement a variant of this clock or deadline mechanism. The first strand (Dorsey, 1992; Goren et al., 2003; Ishii and Kurzban, 2008) introduces restrictions on how actions may be adjusted during the period, e.g., individual contributions in a public good game may only be increased but not decreased over time. Kurzban et al. (2001) compares public good experiments in a clock framework with and without revocable contributions. In the second strand (Levati and Neugebauer, 2004; Murphy et al., 2006), prior to a clock running out, the period may end by other means such as a player dropping out of an auction or exiting a market. Both strands may be viewed as extensions to the basic clock/deadline mechanism.

of game run in the laboratory, the length of a period, the mode of asynchronous interaction (i.e., between or within a period) and the ensuing classification to one of the non-discrete time frameworks are reported for each experimental study. For this purpose, a period is defined in the context of repeated games as the amount of time a subject has to decide on a binding action. Note that this is identical to the minimum amount of time that a binding decision by a subject holds. Consequently, a supergame is referred to as a complete sequence of a fixed or random number of periods.

Feeley et al. (1997), Berninghaus and Ehrhart (2003), and Berninghaus et al. (1999, 2006, 2007) are among the first to conduct continuous time experiments with period lengths below the human reaction time and a fixed supergame length of several minutes up to half an hour.[2] More recently, Cheung and Friedman (2009), Friedman and Oprea (2012), Oprea et al. (2014), Bigoni et al. (2015a), Kephart and Friedman (2015a), and Kephart and Rose (2015) run experiments in continuous time with supergame lengths from 20 seconds to four minutes. Berninghaus and Ehrhart (1998), Deck and Wilson (2002, 2003, 2008), Davis (2009a), Davis and Korenok (2009), Davis et al. (2009, 2010), and Friedman et al. (2015) conduct near-continuous time experiments with a high number of rapidly repeated periods. The clock or deadline framework is employed by Roth et al. (1988), Güth et al. (2002), Goren et al. (2004), and Deck and Nikiforakis (2012).

Of the continuous time experiments, only Berninghaus et al. (2007), Friedman and Oprea (2012), Oprea et al. (2014), Kephart and Friedman (2015a), and Kephart and Rose (2015) compare outcomes under both discrete and continuous time. Berninghaus et al. (2007) study network formation and network effects in social and economic networks in which connections to other players are beneficial but costly. They find that the formation of a certain star structure, which is the unique Nash equilibrium, prevails under both time frameworks. However, subjects are found to alternate the coveted position of the center player in the star network in continuous time but not in discrete time. Berninghaus et al. suggest that their results may be explained by inequity aversion. As the

[2]Note that Millner et al. (1990) already follows a continuous time approach with output variables given as flow values. However, technical constraints of the PLATO software used for the computerization of the experiment resulted in a transaction time between clients and server of about 5 seconds, which lies one order of magnitude above the threshold of 0.5 seconds.

TABLE 3.1: *Economic laboratory experiments in non-discrete time.*

Study	Type of game	Period length[†]	Async. interaction
	Continuous time		
Feeley et al. (1997)	Prisoner's dilemma	n/a[††]	Inter-period
Berninghaus et al. (1999)	Population	1/10 seconds	Inter-period
Berninghaus and Ehrhart (2003)	Evolutionary	1/10 seconds	Inter-period
Berninghaus et al. (2006)	Network formation	1/10 seconds	Inter-period
Berninghaus et al. (2007)	Network formation	1/5 seconds	Inter-period
Cheung and Friedman (2009)	Coordination	1/2 seconds	Inter-period
Knigge and Buskens (2010)	Network formation	n/a[††]	Inter-period
Friedman and Oprea (2012)	Prisoner's dilemma	1/20 seconds	Inter-period
Oprea et al. (2014)	Public good	1/10 seconds	Inter-period
Bigoni et al. (2015a)	Prisoner's dilemma	16/100 seconds	Inter-period
Kephart and Friedman (2015a)	Hotelling	1/20 seconds	Inter-period
Kephart and Rose (2015)	Hotelling	1/20 seconds	Inter-period
Chapter 7	Wholesale competition	1/2 seconds	Inter-period
	Near-continuous time		
Millner et al. (1990)[†††]	Posted offer	5 seconds	Inter-period
Berninghaus and Ehrhart (1998)	Public good	10–90 seconds	Inter-period
Deck and Wilson (2002)	Posted offer	3 seconds	Inter-*k*-periods-block
Deck and Wilson (2003)	Posted offer	3 seconds	Inter-20-periods-block
Deck and Wilson (2008)	Posted offer	1.7 seconds	Inter-period
Davis (2009a)	Posted offer	7 seconds	Inter-period
Davis and Korenok (2009)	Posted offer	7–70 seconds	Inter-period
Davis et al. (2009)	Posted offer	12 seconds	Inter-period
Davis et al. (2010)	Posted offer	12–18 seconds	Inter-period
Friedman et al. (2015)	Cournot competition	4 seconds	Inter-period
	Clock/deadline mechanism		
Roth et al. (1988)	Bargaining	9–12 minutes	Intra-period
Dorsey (1992)	Public good	180 seconds	Intra-period
Kurzban et al. (2001)	Public good	90 seconds	Intra-period
Güth et al. (2002)	Public good	3 minutes	Intra-period
Goren et al. (2003)	Public good	60–90 seconds	Intra-period
Goren et al. (2004)	Public good	60–90 seconds	Intra-period
Levati and Neugebauer (2004)	Public good	$\leq$ 50 seconds	Intra-period
Murphy et al. (2006)	Trust dilemma	$\leq$ 45 seconds	Intra-period
Ishii and Kurzban (2008)	Public good	90 seconds	Intra-period
Deck and Nikiforakis (2012)	Minimum-effort	60 seconds	Intra-period

[†] Period length is defined as the minimum time that a binding decision by a subject holds.
[††] The transaction time of the software is not stated, but assumed to be below 0.5 seconds.
[†††] The experiment uses the PLATO software. Period length is determined as its estimated latency.

discrete treatment is composed of only 15 periods whereas the continuous treatment runs for 30 minutes, subjects may find it easier to equalize payoffs among themselves in the latter.

Oprea et al. (2014) compare contributions to a public good between discrete time and continuous time over 10 minutes. In the continuous time treatments, contributions can be changed in real time. In the discrete time treatments, incstead, subjects decide on their contributions once a minute, i.e., they play 10 periods with a fixed length of one minute. In this setup with few discrete periods of a fixed period length, the authors find no differences in contributions between the two time frameworks.

In a yet simpler environment, Friedman and Oprea (2012) compare cooperative behavior in the prisoner's dilemma in discrete and continuous time. The authors find that the continuous time framework fosters cooperation among the players relative to discrete time. More precisely, they compare continuous and discrete variants of the prisoner's dilemma in supergames with a constant length of 60 seconds. In continuous time, they find a median mutual cooperation rate of 90 percent over the supergames' duration. With the duration of each supergame being fixed, the number of periods is decreased to eight in 60 seconds and finally to one in 60 seconds, i.e., a one-shot game.[3] The main finding of the study is that cooperation decreases as the number of periods decreases so that the median rate of mutual cooperation is zero in the one-shot treatments. In other words, cooperation is higher in a continuously repeated prisoner's dilemma than in a one-shot (discrete) prisoner's dilemma. Friedman and Oprea also analyze the subjects' individual behavior in the continuous time treatments and identify alternative strategies. A model of ϵ-equilibria (Radner, 1986; Bergin and MacLeod, 1993) predicts their findings very well. A key aspect of their experimental design is "that period lengths and potential payoffs are kept constant across [...] treatments" (Friedman and Oprea, 2012, p. 343). However, to achieve this, Friedman and Oprea are forced to implicitly change two treatment variables simultaneously in the transition from continuous time

[3]Comparably, Berninghaus and Ehrhart (1998) vary the number of periods (10, 30, and 90) in a public good game of fixed total session length of 15 minutes and find that cooperation increases with the number of repetitions.

to the one-shot (discrete) treatment. The first treatment variable is obviously the time framework of a repeated game, i.e., continuous or discrete, and the second treatment variable is the repetition of the game itself, i.e., repeated game or one-shot game.

Both Kephart and Friedman (2015a) as well as Kephart and Rose (2015) compare a discrete time and two continuous time variants of the Hotelling (1929) spatial competition model with and without vertical differentiation. Kephart and Friedman (2015a) find that under continuous time location choices resemble the static Nash equilibrium more closely than under discrete time. With vertical differentiation and an additional choice on price, Kephart and Rose (2015) find some support for the notion that continuous time increases cooperation. Whereas subjects may decide instantaneously in one of the continuous time treatments, they can change their decision only gradually at a specified speed in the other continuous treatment. Under discrete time, subjects have to decide on location (and price in case of Kephart and Rose (2015)) during a three second time interval. Note that with respect to the classification of timing in experiments derived above, the discrete time treatments clearly fall under the near-continuous time framework.

3.3 Experiment

The following experiment is aimed at studying and comparing the impact of discrete time and continuous time on experimental oligopoly competition. As a means of robustness, not only a single mode of competition but symmetric differentiated Bertrand as well as Cournot competition is run in duopolies and triopolies each. Thereby, the experiment captures three dichotomous treatment variables, namely discrete vs. continuous time, Bertrand vs. Cournot competition, and duopolies vs. triopolies in a full-factorial design, resulting in a total of eight treatments. The labels used to refer to the treatments are stated in Table 3.2 by appending abbreviations from left to right, e.g., RB3 refers to the continuous (real-time) Bertrand triopoly treatment.

TABLE 3.2: *Treatment variables and their values.*

Time framework	Competition model	Number of firms
Discrete time (D)	Bertrand (B)	Duopoly (2)
Continuous time (real time) (R)	Cournot (C)	Triopoly (3)

3.3.1 Oligopoly competition

Price competition à la Bertrand and quantity competition à la Cournot are the two workhorse models of IO. When comparing different designs in experiments on firm behavior, they serve as good proxies for a large share of models on oligopoly competition. As the Bertrand paradox of homogeneous price competition is often deemed unrealistic and as it yields a discontinuous demand function, the model by Singh and Vives (1984)—which generalizes the Hotelling (1929) model to exploit the duality between price and quantity competition in differentiated goods—is utilized for the experiment; more precisely the model's generalization to more than two firms is employed such as, e.g., in Häckner (2000) and Suetens and Potters (2007). See Appendix B.1 for a thorough analysis of the model with asymmetric firms and three different theoretical predictions, namely Nash equilibrium, Walrasian (competitive) equilibrium and collusive equilibrium.

Consider a market with $n \in \mathbb{N}$ firms. Each firm $i \in \{1,...,n\}$ produces a single good. The firms' goods are differentiated horizontally but homogeneous in vertical quality and have identical demand elasticity. Thus, firms are assumed to be symmetric. Note that asymmetric (inverse) demand may result in additional behavioral effects in the experiment which are not in focus here. See Chapter 4 for an asymmetric experimental application of the model. For the Cournot treatments, the inverse demand for firm i is given by

$$p_i = \omega - \lambda \left(q_i + \theta \sum_{j \neq i} q_j \right)$$

with $\omega, \lambda > 0$ and the degree of substitutability $\theta \in [-1,1]$. If $\theta < 0$ goods are complements, if $\theta = 0$ goods are independent of one another, and if $\theta = 1$ they are perfect substitutes. For non-perfect substitutes ($\theta < 1$), the corresponding demand function for firm i in the Bertrand treatments is given by

$$q_i = \Omega - \Lambda p_i + \Theta \frac{\sum_{j \neq i} p_j}{n-1}$$

with

$$\begin{aligned}
\Omega &= \frac{\omega}{\lambda(1+\theta(n-1))}, \\
\Lambda &= \frac{1+\theta(n-2)}{\lambda(1-\theta)(1+\theta(n-1))}, \\
\Theta &= \frac{\theta(n-1)}{\lambda(1-\theta)(1+\theta(n-1))},
\end{aligned}$$

and n as the number of firms with non-negative demand, i.e., firms that have not exited the market due to a too high price. If $q_i < 0$ firm i exits the market, its quantity is set to zero, and n is decreased by one. Normalizing costs to zero, firm i's profit is $\Pi_i = p_i q_i$.

For the empirical assessment of tacit collusion as a measure of competition intensity consider three equilibrium benchmarks for Bertrand and Cournot competition, respectively. Note that, although goods are differentiated horizontally, equilibrium prices, quantities, and profits are the same for all firms as firms are not differentiated vertically. First, under the Walrasian (competitive) equilibrium all firms are assumed to be price-takers so that they maximize their profit irrespective of their rivals' decisions. Second, the Nash equilibrium assumes that firms choose a price (quantity) such as to maximize their own profit given their rivals' prices (quantities). Third, under the collusive equilibrium firms are assumed to cooperate and hence, maximize their joint profits, i.e., engage in JPM. See Appendix B.1 for the derivation of these theoretical predictions.

It is straightforward that $\Pi^{JPM} \geq \Pi^{Nash}_{Bertrand}, \Pi^{Nash}_{Cournot} \geq \Pi^{Walras}$ for all valid parameter combinations. If goods are substitutes ($\theta > 0$), Nash prices and profits are higher under Cournot competition than under Bertrand competition. In contrast, consumer surplus and total welfare are higher under Bertrand competition than under Cournot competition as they are monotonically decreasing in prices. If goods are complements ($\theta < 0$), depending on the number of competitors, Nash prices and profits may be higher under Bertrand competition than under Cournot competition (Häckner, 2000).

3.3.2 Measuring competitiveness

As Nash prices, quantities, and profits do not coincide under Bertrand and Cournot and are additionally dependent on the number of competitors, these firm input or market output variables are not adequate to compare cooperative intentions, i.e., tacit collusion, across treatments. Therefore, combined indices of the *degree of tacit collusion* proposed by Suetens and Potters (2007) and Engel (2007) are used to compare tacit collusion between treatments irrespective of different theoretical predictions. The degree of tacit collusion is measured as the relative deviation of a price, quantity, or profit from the theoretical prediction towards the JPM price, quantity, or profit. With respect to Bertrand (Cournot) competition, a price (quantity) set by a firm can thereby be unambiguously converted to a degree of tacit collusion. Hence, for means of comparison between treatments, firms may be assumed to decide on a certain degree of tacit collusion instead of a price or quantity. In a similar fashion, a firm's profit as well as average profit of firms in a market may be expressed in a degree of tacit collusion. Therefore, consider a degree of tacit collusion based on model input, i.e., price in Bertrand and quantity in Cournot, as well as a degree of tacit collusion based on model output, i.e., profit. Formally, the degree of tacit collusion is

$$\varphi_x^E = \frac{\overline{x} - x^E}{x^{JPM} - x^E}$$

with $x \in \{p/q, \Pi\}$ and $E \in \{Nash, Walras\}$, resulting in four different measures depending on the theoretical benchmark (Nash or Walrasian equilibrium) and input or output. If $\varphi_x^E = 0$, the value of $\overline{x}$ corresponds to the theoretical prediction by the equilibrium concept E. If $\varphi_x^E = 1$, the market is completely collusive and competitors behave as a single monopolist. Note that $\varphi_{p/q}^E$ may exceed one as joint profit is not monotonic in price or quantity, but $\varphi_\Pi^E \leq 1$.

3.3.3 Repeated games in discrete and continuous time

Moving from the one-shot game introduced in the previous subsection to the repeated game implemented in the experiment several experimental design implications from the extant literature are inferred. First, in contrast to Friedman and Oprea (2012), the repeated version of the game is employed in both the discrete time treatments and the continuous time treatments. Second, the discrete time treatments are composed of 60 periods—much more than in Berninghaus et al. (2007) or Oprea et al. (2014)—to reduce differences to continuous time solely due to a longer time horizon of the experiment. Third and contrasting Kephart and Friedman (2015a) as well as Kephart and Rose (2015), the discrete time treatments refrain from limiting the time provided to subjects for their decision-making process in each period. Fourth, discrete time sessions are run first to set the duration of the continuous time sessions to equal the average duration of the discrete time sessions, which amounted to approximately 30 minutes. Hence, the total session length is similar across all treatments and one period in discrete time corresponds on average to 30 seconds in continuous time. The period length in the discrete time treatments is infinite and it is 0.2 seconds in the continuous time treatments, i.e., considerably below the conservative threshold of 0.5 seconds. With respect to the latter time framework, current profit represents a flow value of time. In an effort to maximize comparability between treatments, the profit displayed in the experimental software in the continuous time treatments is scaled to the profit that subjects would have earned if the current prices or quantities would be held constant for 30 seconds, ceteris paribus. Thereby, with same prices or quantities in one of the discrete time treatments and the corresponding continuous time treatment, the information presented to the subjects is not only qualitatively equal but also visually identical.

The model of differentiated Bertrand and Cournot competition considered in this experiment has a unique strict Nash equilibrium in the one-shot (stage) game. In discrete time, this also constitutes the unique subgame perfect equilibrium of the finitely repeated game. In continuous time, however, the theoretical prediction is not straightforward. Maskin and Tirole (1988a,b) consider two different continuous time frameworks with endogenous timing in duopolistic price and quantity competition and show that equilibrium behavior is similar to a sequential-move infinitely repeated duopoly. In particular, continuous time is modeled as a fine grid of periods in a sequential-move game with commitment to a price or quantity for a deterministic or stochastic length of time. Although the deterministic variant may rather apply to repeated games classified as near-continuous time, the stochastic variant does capture the asynchronous nature of continuous time quite well. Irrespective of the continuous time variant, a collusive equilibrium emerges for discount factors close to one. An assumption in their model is that the Markov property holds, i.e., that future states of the stochastic process only depend on the current state and not the sequence of states that preceded it. In a comparable fashion, Simon and Stinchcombe (1989, p. 1171) model continuous time as "a discrete time model, but with a grid that is infinitely fine" and thereby suggest a more general definition of games in continuous time. Friedman and Oprea (2012) point out that the model predicts mutual cooperation at all times in a prisoner's dilemma, which may be viewed as a highly abstracted variant of homogeneous Bertrand competition. In sum, theory predicts that, if anything, asynchronous-move continuous time is more prone to tacit collusion than simultaneous-move discrete time. Additionally, Bigoni et al. (2015a) find that a deterministic ending rule facilitates cooperation even more than a stochastic ending rule under continuous time, whereas other experimental evidence indicates that the opposite may hold under discrete time (Dal Bó, 2005). Theses findings add further support to the conjecture that the continuous time treatments in our experiment are expected to exhibit more tacit collusion than the discrete time experiments. In a nutshell, the theoretical and experimental evidence leads to the following hypothesis.

HYPOTHESIS 3.1. *Oligopoly competition in continuous time is, ceteris paribus, more prone to tacit collusion than in discrete time.*

TABLE 3.3: *Theoretical benchmarks of oligopoly competition for each treatment.*

	Bertrand	Cournot
Duopoly	$p^{Walras} = 0$	
	$q^{Walras} = 60.00$	
	$\Pi^{Walras} = 0$	
	$p^{Nash} = 25.00$	$p^{Nash} = 37.50$
	$q^{Nash} = 45.00$	$q^{Nash} = 37.50$
	$\Pi^{Nash} = 1125.00$	$\Pi^{Nash} = 1406.25$
	$p^{JPM} = 50.00$	
	$q^{JPM} = 40.00$	
	$\Pi^{JPM} = 1500.00$	
Triopoly	$p^{Walras} = 0$	
	$q^{Walras} = 42.86$	
	$\Pi^{Walras} = 0$	
	$p^{Nash} = 16.47$	$p^{Nash} = 30.00$
	$q^{Nash} = 35.71$	$q^{Nash} = 30.00$
	$\Pi^{Nash} = 595.24$	$\Pi^{Nash} = 900.00$
	$p^{JPM} = 50.00$	
	$q^{JPM} = 21.43$	
	$\Pi^{JPM} = 1071.43$	

3.3.4 Procedures

For the experiment, the parameters of the oligopoly competition model are $\omega = 100$, $\lambda = 1$, and $\theta = \frac{2}{3}$ so that goods are substitutes. Consequently, $\Omega = \frac{300}{2n+1}$, $\Lambda = \frac{6n-3}{2n+1}$, and $\Theta = \frac{6n-6}{2n+1}$. Table 3.3 shows the corresponding theoretical benchmarks of the one-shot game for each treatment.

In a further effort to maximize comparability between treatments and to prevent any source for behavioral effects other than the treatment, input and output variables are scaled in the following way. The action space of prices in Bertrand treatments and quantities in Cournot treatments is equally set to $[0, 100]$ with a minimum increment of 1 and the JPM action at a price or quantity of 50. This ensures that the collusive action is not more or less behaviorally attractive across treatments and that the search costs of finding the collusive action are the same in all treatments. With a similar intention profits are scaled so that they would be equal in Nash equilibrium. Thereby, a subject playing the Nash equilibrium of the one-shot game—given that its competitors play Nash as well—would make identical profits in all treatments.[4] Altogether, this precludes confounding effects of the experimental design and parametrization. Furthermore, perfect information is ensured in all treatments, i.e., subjects are provided with individual feedback about each competitor's price, quantity, and profit. Also, to prevent misinterpretation due to short-time treatment effects, the number of repetitions of the one-shot game, i.e., periods, is comparably high.

With respect to the technical requirements of continuous time and to ensure high control over the correct scaling of time in all treatments, the experiment is computerized with *Brownie*, a newly-developed *Java*-based experimental software (Müller and Normann, 2014).[5] All sessions were run at the Karlsruhe Institute of Technology in Karlsruhe, Germany between October and December 2014—first the discrete time treatments and then the continuous time treatments. Disregarding the first period, in which subjects familiarized themselves with the experimental software and decided on their initial price or quantity, the discrete time sessions took on average roughly 30 minutes. Continuous time sessions ran for the same amount of time (again without the phase of deciding on initial price or quantity). Therefore, on average, one period in discrete time corresponds to roughly 30 seconds in continuous time. Note that there are no practice periods, neither with nor without interaction between subjects, and thus, no unobservable learning confounds occur. The matching of subjects is constant throughout a session (fixed partner matching). In total, 240 students of economic fields with an average age of 22 years participated in the experiment. Subjects were recruited via the ORSEE platform (Greiner, 2015) and participated only in one of the treatments (between-subject design).

[4] Alternatively, profits may be standardized with respect to the collusive outcome. However, this would in turn lead to different Nash profits across treatments. Hence, firms would face diverse incentives to deviate from the theoretical Nash prediction. Moreover, the normalization based on Nash profits yields specific properties with regard to the degree of tacit collusion based on the Walrasian equilibrium, which will prove important for an experiment reported in Chapter 4 which also uses part of the data used here.

[5] Recently, further experimental software capturing continuous time is introduced, e.g., by Pettit et al. (2014) for experimenters with limited programming skills and by Hawkins (2014) for web-based experiments.

The protocol for each session follows five steps. First, upon entering the lab, subjects are randomly assigned to a chair, from which they can neither see nor speak to any other participant of the experiment. Second, after everyone has been seated, the experimental instructions are handed out to the participants in print and read aloud from a recording.[6] The recording ensures that any confounding effect of the reader's voice, accent, or intonation is identical across sessions from the same treatment and as similar as possible across treatments. Therefore, identical paragraphs across treatments are recorded once and the recording is used in all treatments. Third, prior to the beginning of the experiment, each participant has to complete a computerized test of questions regarding the comprehension of the instructions. It is only allowed to proceed to the next question after the correct answer to the current question is entered. Fourth, after all subjects have successfully completed the test, the experiment starts automatically. Over the course of experiment participants wear ear protectors so that they are not influenced by clicking noises of computer mouses or other disturbing noise. Fifth, following the end of the experiment, each participant is paid out the profits accumulated during the experiment privately and in cash. Following this protocol, the total length of a session from subjects' entering to leaving the lab was about one hour. The average payoff per subject is EUR 16.85.

3.4 Results

The experimental data amounts to 12 independent duopolies or triopolies in each treatment. Due to no-shows, two exceptions are the RB3 treatment for which there are only 11 triopolies and the RC3 treatment for which data on 13 triopolies exists since the number of no-shows necessitated an additional session. For each cohort there is data on market variables over 60 periods in a discrete time treatment and on 9,000 ticks (at an interval of 0.2 seconds each) in a continuous time treatment. Considering the comprehensiveness of the data, the statistical analysis initially deals with aggregate data on the

[6] As an example, the experimental instructions for the RB3 treatment together with a screenshot of the experimental software are provided in Appendix C.1.

TABLE 3.4: *Average degrees of tacit collusion across treatments.*

Treatment	N	$\varphi_{p/q}^{Nash}$	φ_{Π}^{Nash}	$\varphi_{p/q}^{Walras}$	φ_{Π}^{Walras}
DB2	12	0.860	0.861	0.930	0.965
		(0.285)	(0.326)	(0.142)	(0.081)
DB3	12	0.659	0.683	0.773	0.859
		(0.352)	(0.327)	(0.235)	(0.145)
DC2	12	0.674	0.532	0.918	0.971
		(0.574)	(0.994)	(0.143)	(0.062)
DC3	12	0.473	0.364	0.789	0.898
		(0.551)	(0.785)	(0.220)	(0.126)
RB2	12	0.769	0.736	0.885	0.934
		(0.371)	(0.468)	(0.185)	(0.117)
RB3	11	0.555	0.505	0.703	0.780
		(0.329)	(0.350)	(0.219)	(0.156)
RC2	12	0.842	0.760	0.960	0.985
		(0.279)	(0.374)	(0.070)	(0.023)
RC3	13	0.424	0.233	0.770	0.877
		(0.516)	(0.784)	(0.206)	(0.125)

Standard deviations in parentheses.

level of cohorts and is followed by further disaggregated analyses. Descriptive statistics on price respective quantity and profit, and thus first impressions on the treatment effects are provided by Table 3.4 in degrees of tacit collusion across treatments and averaged over cohorts.[7] As a first insight, comparing the average degree of tacit collusion based on Nash profits over the entire length of the experiment without controlling for the competition model or the number of competitors, discrete time is significantly more prone to tacit collusion than continuous time according to a one-tailed non-parametric Mann-Whitney U test ($z = 1.77, p = 0.038$). In the following, this preliminary finding is investigated thoroughly by means of panel analyses of cohort and individual behavior over the entire time horizon on market level and on firm level.

In order to allow for a comparison of panel data from discrete time treatments and continuous time treatments, the experimental data from the latter treatments is mapped to

[7]For Table 3.4, the first and last sixth of periods are dropped to reduce distortions by start- and end-game effects. For purposes of comparison, average degrees of tacit collusion over the entire time horizon across treatments are reported in Appendix D.1. All following non-descriptive statistical analyses are based on data from all periods.

the 60 periods of the discrete time treatments. In particular, for each discrete period, the degree of tacit collusion in the continuous time treatments is averaged over 30 seconds, i.e., 150 consecutive ticks of 0.2 seconds. Thereby, the first 30 seconds correspond to the first discrete period, the next 30 seconds correspond to the second discrete period, and so on. The mean is used as a single proxy for the behavior over 30 seconds as it has the advantage that a maximum of information about the distribution is preserved and that it is, loosely speaking, merely a reduction in data resolution rather than a reduction in data itself. In contrast to the median or other point statistics, changing the value of any single data point inevitably changes the mean as well. For a direct comparison of the two time frameworks using the mean is therefore arguably most conservative.

RESULT 3.1. *The degree of tacit collusion based on profits is significantly higher under discrete time than under continuous time.*

A firm's profit is determined not only by its own decisions but also by the decisions of its rivals. One firm's profit in a period is hence not independent from its rivals' profits. Therefore, the degree of tacit collusion based on profits is a market level outcome variable, i.e., it is measured using the average of each firm's profit in a duopoly or triopoly. There are a total of 96 markets across all treatments with 60 discretized periods each. Testing for treatment effects in such clustered panel data requires to control for the dependence between observations from the same market as opposed to observations from different markets. Consequently, the following multilevel mixed-effects regression model is estimated, for which treatment DB2 serves as a baseline:

$$\begin{aligned}\varphi^{E}_{\Pi,k,t} = \beta_0 + \xi_k \\ &+ \beta_{Continuous} \cdot Continuous \\ &+ \beta_{Triopoly} \cdot Triopoly \\ &+ \beta_{Cournot} \cdot Cournot \\ &+ (\beta_{Period} + \beta_{Period,k}) \cdot t \\ &+ \epsilon_{k,t},\end{aligned}$$

where $\varphi^{E}_{\Pi,k,t}$ is the degree of tacit collusion based on average firm profit Π on market, i.e., duopoly or triopoly, k in period t. On the market level, ξ_k is the random intercept that controls for intra-cluster correlation in terms of different base levels of tacit collusion between markets and $\beta_{Period,k}$ is a random slope for the time trend in each market. Table 3.5 reports estimates for the degree of tacit collusion based on Nash profits in Model (1) and on Walrasian profits in Model (2). Irrespective of the theoretical benchmark, continuous time has a significant negative effect on tacit collusion and reduces the degree of tacit collusion between 4 percentage points (pp) and 20 pp, ceteris paribus. This is in stark contrast to Hypothesis 3.1 and also contradicts previous experimental findings (Friedman and Oprea, 2012; Oprea et al., 2014). Yet, both control treatment dummies for the competition model and the number of firms have the expected effects. First, in line with the meta-study on oligopoly experiments presented in Chapter 4, triopolies exhibit (10 pp to 22 pp) less tacit collusion than duopolies. Second, price competition facilitates tacit collusion more than quantity competition if measured based on Nash profit. In this case, the degree of tacit collusion is almost 26 pp lower under quantity competition compared to price competition. However, this is reversed if tacit collusion is measured based on Walrasian profit. Then, the degree of tacit collusion under quantity competition is almost 5 pp higher than under price competition. This is also in line with the expectation as the Walrasian equilibrium is independent of the competition model so that the Walrasian-based degree of tacit collusion does not control for differing Nash predictions of price and quantity competition. Furthermore, there are no significant interaction effects between the treatment variables in either regression model. In conclusion, the effect of continuous time compared to discrete time is not only statistically significant but has similar magnitudes as the number of competitors and mode of competition—which is supported for the Nash-based degree of tacit collusion by insignificant Wald tests of pairwise coefficient comparisons in Model (1).

In the following, apart from these findings with respect to profit, i.e., an output variable, a similar yet complementary analysis of prices and quantities, i.e., input variables is conducted. Thereby, instead of aggregate market behavior the individual firm choices of prices and quantities are compared across treatments.

TABLE 3.5: *Multilevel mixed-effects linear regressions of tacit collusion on treatment variables.*

Covariate	(1) φ_{Π}^{Nash}	(2) φ_{Π}^{Walras}	(3) $\varphi_{p/q}^{Nash}$	(4) $\varphi_{p/q}^{Walras}$	(5) $\varphi_{p/q}^{Nash} \leq 1$	(6) $\varphi_{p/q}^{Walras} \leq 1$
Continuous	-0.196^{*}	-0.037^{*}	-0.109^{**}	-0.092^{**}	-0.106^{**}	-0.094^{**}
	(0.110)	(0.022)	(0.052)	(0.037)	(0.053)	(0.038)
Triopoly	-0.219^{**}	-0.097^{***}	-0.179^{***}	-0.138^{***}	-0.190^{***}	-0.145^{***}
	(0.110)	(0.022)	(0.054)	(0.038)	(0.054)	(0.039)
Cournot	-0.264^{**}	0.048^{**}	-0.276^{***}	-0.053	-0.235^{***}	-0.003
	(0.110)	(0.022)	(0.052)	(0.037)	(0.053)	(0.038)
Period	< 0.001	> -0.001	> -0.001	-0.001	< 0.001	< 0.001
	(0.002)	(< 0.001)	(0.001)	(< 0.001)	(0.001)	(< 0.001)
Constant	0.851^{***}	0.941^{***}	0.914^{***}	0.883^{***}	0.853^{***}	0.823^{***}
	(0.110)	(0.022)	(0.055)	(0.039)	(0.056)	(0.040)
Cohorts/Firms	96	96	240	240	240	240
Observations	5,760	5,760	14,400	14,400	13,876	13,876

Standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

RESULT 3.2. *The degree of tacit collusion based on prices and quantities is significantly higher under discrete time than under continuous time.*

Each decision by a firm on a price or quantity can be unambiguously transferred into a choice for a certain degree of tacit collusion, which makes decisions on prices and quantities comparable across treatments. Applying the same approach as above, firms' behavior as measured by the degree of tacit collusion is estimated over time whilst controlling for firm-specific random effects using the following multilevel mixed-effects regression model—again, all effects are estimated with respect to treatment DB2:

$$\begin{aligned}\varphi_{p/q,i,t}^{E} = \beta_0 + \xi_i \\ + \beta_{Continuous} \cdot Continuous \\ + \beta_{Triopoly} \cdot Triopoly \\ + \beta_{Cournot} \cdot Cournot \\ + (\beta_{Period} + \beta_{Period,i}) \cdot t \\ + \epsilon_{i,t},\end{aligned}$$

with $\varphi^E_{p/q,i,t}$ as the degree of tacit collusion based on firm i's price p or quantity q played in period t. Estimation results for Models (3) and (4), reported in Table 3.5, confirm that the degree of tacit collusion of firms' price and quantity choices is 9 pp to 11 pp significantly higher under discrete time than under continuous time, both with respect to Nash equilibrium as well as Walrasian equilibrium. Similarly, prices and quantities in triopolies are 14 pp to 18 pp less collusive than in duopolies. With respect to the mode of competition, however, price competition elicits more collusive behavior than quantity competition irrespective of the underlying theoretical benchmark. Although the difference is significant and economically relevant with almost 28 pp in the Nash-based degree of tacit collusion, quantity competition is—contrary to expectations—not more prone to tacit collusion than price competition based on Walrasian equilibrium. Again, there are no significant treatment interaction effects.

A possible criticism of the previous analysis is that $\varphi^E_{p/q}$ is not monotonic in *collusiveness* as the measure can exceed a value of one, which is, however, not related to more but less successful JPM than in case of $\varphi^E_{p/q} = 1$. In fact, 3.6% of firms' price and quantity choices are related to a degree of tacit collusion above one. However, any value of $\left|\varphi^E_{p/q}\right| < 1$ is a deviation from the collusive equilibrium. This is not captured in Models (3) and (4) in Table 3.5. Excluding all observations with $\varphi^E_{p/q} > 1$ yields Models (5) and (6), which show that the treatment effects are robust to degrees of tacit collusion exceeding one. Other alternatives dealing with these outliers such as folding down all observations with a degree of tacit collusion above one, i.e., rendering $1 - \left|1 - \varphi^E_{p/q}\right|$ as the dependent variable, lead to similar results.

3.5 Discussion

This study provides empirical evidence that tacit collusion is higher in discrete time experimental oligopolies than in continuous time experimental oligopolies. Thereby, discrete time is based on synchronized and simultaneous decision making, whereas continuous time is based on asynchronous and an endogenized sequence of decision making. For purposes of robustness, a full-factorial treatment design is considered, with (i) the two work-horse models of IO, namely Bertrand and Cournot competition, (ii) in duopolies and triopolies, (iii) under discrete time and continuous time. The key insights from the laboratory experiment can be summarized as follows. First, the replication of two well-known findings from the IO literature shows that participants in the experiment behaved in line with previous experimental endeavors of oligopoly competition: Duopolies are found to be more collusive than triopolies and Bertrand competition in prices is found to be more prone to tacit collusion than Cournot competition in quantities. Second, controlling for the competition model as well as the number of firms the main result of the study is derived: There is significantly more tacit collusion under discrete time than under continuous time, which is in stark contrast to the theory (Maskin and Tirole, 1988a,b; Simon and Stinchcombe, 1989) as well as previous experimental studies on continuous and discrete time repeated games (Friedman and Oprea, 2012; Oprea et al., 2014).

The implications for further research on IO are two-fold. First, researchers designing oligopoly experiments should consider that the time framework employed to capture the repeated nature of the underlying game affects their results. In particular, experimental investigations of tacit collusion—which are until now solely run in discrete or, more recently, in near-continuous time—may have potentially overestimated the supra-competitive effect. Furthermore, it cannot be ruled out that the mode of timing interacts with other properties of oligopoly competition such as market demand, cost structure or strategy space. Second and more general, the effect of continuous time on repeated non-cooperative games is ambiguous. In contrast to this study, experiments on simpler games such as contribution to a public good (Oprea et al., 2014) or the prisoner's dilemma (Friedman and Oprea, 2012) find no differences between time frameworks let alone higher propensities to collude under continuous time than under discrete time. The experiment described in this chapter differs from these two studies in several ways, especially with regard to a greater action space and a higher number of periods in the discrete time treatments. Thus, it may prove worthwhile to systematically vary the number of periods in future research on discrete time versus continuous time and extend the comparison to other games (with a different number of possible actions).

For a deeper understanding of why oligopolistic firms find it easier to tacitly collude under discrete time than under continuous time a more profound analysis of firms' behavior is warranted. Firms may apply different strategies or learn from past behavior in many different ways: For example, behavior by firms in repeated oligopoly competition may be characterized by a static strategy (see, e.g., Fudenberg et al. (2012) for strategies in a prisoner's dilemma), by a dynamic strategy such as the imitation of a competitor's behavior (Huck et al., 1999), or by learning from own and competitors' decisions in the past (Huck et al., 2004a). In particular, reinforcement learning, for which agent-based simulations show it to converge to collusion in a homogeneous Cournot oligopoly (Waltman and Kaymak, 2008), may be a fruitful approach in explicitly capturing the different dynamics of simultaneous-move discrete time and asynchronous-move continuous time. Furthermore, as continuous time makes simultaneous decision making virtually impossible, experiments comparing sequential-move and simultaneous-move games may be connected to the findings presented here. In fact, experiments on quantity (Huck et al., 2001) and price (Kübler and Müller, 2002) competition suggest that sequential-move interaction is less prone to tacit collusion than simultaneous-move competition. However, this finding holds only if the sequence of decision making is exogenous. Instead, if timing of sequential decisions is endogenous, behavior is equal to simultaneous-move oligopolies (Fonseca et al., 2005; Müller, 2006).

A key feature of the experimental design employed here is that it contrasts two extremes of time frameworks to each other: Discrete time with no limit on period lengths and continuous time with a fixed period length below the human reaction time. Obviously, this design inherently cedes control over the duration of sessions in discrete time and one may argue that both extremes lack parallelism to decision making in reality. Therefore, an investigation of period lengths in the transition from discrete time to continuous time is likely to provide valuable insights to disentangle the effect of the time framework, i.e., whether its effect is driven by the period length, the number of repetitions, or the (a)synchronicity of decision making. In particular, this calls for an experimental examination of near-continuous time with varying period lengths and different numbers of periods—an issue that is left to future work.

Part II

Market Structures and Tacit Collusion

Chapter 4

Number of Competitors and Tacit Collusion

THE most apparent characterizing feature of oligopolies is their limited number of firms in competition. In their overview on experimental oligopoly experiments, Potters and Suetens (2013, p. 17) summarize that "the scope of collusion is strongly affected by the number of competitors". Indeed, the consensus of the economic literature is that the ability to implicitly coordinate among competitors, i.e., to tacitly collude, is effectively reduced as the number of firms in a market increases. Thereby, a monotonic relationship is assumed where tacit collusion is "frequently observed with two sellers, rarely in markets with three sellers, and almost never in markets with four or more sellers" (Potters and Suetens, 2013, p. 17). However, to date there exists neither an empirical comparison of experimental studies varying the number of competitors nor a coherent experimental investigation of the effects of the number of competitors across different modes of competition.

Thus, the supposed fact that there is a negative monotonic relationship between the number of competitors in a market and the extent of tacit collusion is scrutinized with the following investigation that is organized in three complementary studies. First, over and beyond the meta-studies by Huck et al. (2004b), Suetens and Potters (2007), and Engel (2007), the relationship between the number of firms and tacit collusion, i.e., the deviation of prices or profits relative to a competitive benchmark outcome, is examined in an integrated review and a consistent empirical comparison of oligopoly experiments. Second, findings from an experiment are provided that is explicitly designed to systematically test the effect of the number of competitors in a market on tacit collusion under price and quantity competition. Third, as the majority of oligopoly experiments deal with situations of symmetric firms, whereas the majority of real-world oligopolies consist of firms which are asymmetric in their market power, a further experiment investigates whether previous findings with regard to the number of firms also hold in markets with asymmetric firms.

The remainder of this chapter is organized as follows. Section 4.1 highlights the policy implications of an evaluation of number effects in oligopolies for both antitrust and regulatory authorities. Regarding the relationship between the number of firms and tactic collusion, Sections 4.2, 4.3, and 4.4, report the design and results of the meta-analysis, the first experiment with symmetric firms, and the second experiment with asymmetric firms, respectively. Finally, Section 4.5 concludes and discusses the findings pooled over all three studies in the context of current policy issues.

4.1 Motivation

The question whether three firms are just as good as four firms in order to ensure effective competition is by far not only an academic one. In practice the question *how many competitors are enough* is frequently asked in the context of competition policy and ex ante regulation of markets. Naturally, merger control is inherently concerned with the effects of a reduced number of competitors in a respective market. For example, recent merger control proceedings in the European Union[1] as well as in the US[2] deal with cases that would reduce the remaining number of competitors from four to three major mobile telecommunications operators in the respective relevant market. Consequently, the question whether competition is just as fierce with three as with four

[1] Hutchinson 3G Austria / Orange Austria (European Commission, 2012), Telefónica Deutschland / E-Plus (European Commission, 2014b), Hutchinson 3G UK / Telefónica Ireland (European Commission, 2014c).

[2] AT&T / T-Mobile US (Federal Communications Commission, 2011), Sprint Corp / T-Mobile US (Federal Communications Commission, 2014).

competitors, everything else being equal, is paramount. Similarly, sector-specific regulatory agencies implicitly or explicitly examine the sufficient number of competitors when assessing the need for ex ante regulation of access obligations.[3]

While ex ante merger simulations based on empirical market data are apt to estimate case-specific consequences of an anticipated change in market conditions, laboratory experiments are well suited to identify systemic effects and to isolate distinct sources for a deviation in market prices through controlled variation of exogenous variables. Thus, experiments are a valuable means of comparing the effect of the number of competitors in an environment of high control allowing the researcher to randomize or hold constant any potential confounding variable. By contrast, empirical field studies are naturally framed in a specific market context and are thus neither generalizable per se nor immediately applicable to other market scenarios as causal relationships are inherently difficult to prove. Particularly with regard to the issue of tacit collusion, which is notoriously hard to detect in field studies, laboratory experiments can provide general insights by analyzing in- and out-of-equilibrium strategies and respective market outcomes relative to benchmark equilibria predicted by economic theory. See also Chapter 2 for an in-depth discussion on the advantageous and disadvantageous of the experimental method with regard to issues of regulatory policy.

The main findings of all three studies reported in this chapter can be summarized as follows. While they unisono provide further support for the notion that "two are few and four are many" (Huck et al., 2004b, p. 435) with regard to the difference in *competitiveness* between markets with a different number of firms, the competitive effect between three and four firms relative to two firms is found to be similar according to the tacit collusion measures introduced by Suetens and Potters (2007) and advocated by Engel (2007), which are also utilized in Chapter 3.

[3]See, e.g., the geographically segmented deregulation of the wholesale broadband access market in the UK, which is subject to the number of active competitors in a region (Ofcom, 2014).

4.2 Meta-analysis

The focus of the meta-analysis is on economic laboratory experiments on oligopoly competition varying the number of firms in the market. As highlighted before, with respect to the effect of the number of competitors on tacit collusion, the extant literature (e.g., Potters and Suetens, 2013) clearly suggests the following hypothesis.

HYPOTHESIS 4.1. *Tacit collusion in oligopolistic markets with two, three, and four competitors decreases monotonically with the number of competing firms in the market.*

4.2.1 Experimental designs

Most experimental studies that vary the number of competing firms in a market implement one of the two workhorse models in IO: price competition à la Bertrand (Fouraker and Siegel, 1963; Dolbear et al., 1968; Dufwenberg and Gneezy, 2000; Orzen, 2008; Davis, 2009b; Fonseca and Normann, 2012) or quantity competition à la Cournot (Fouraker and Siegel, 1963; Bosch-Domènech and Vriend, 2003; Huck et al., 2004b; Waichman et al., 2014).[4] A third strand of literature observes tacit collusion in posted-offer markets, i.e., simultaneous competition in prices and quantities (Ketcham et al., 1984; Alger, 1987; Brandts and Guillén, 2007; Ewing and Kruse, 2010). As the latter experiments use very diverse models and are hence hardly comparable between one another, the focus is on price or quantity competition here. Table 4.1 lists ten oligopoly experiments which are surveyed in this meta-analysis and that all vary the number of competitors in a market, n, in one way or another.[5]

[4]Note that merger experiments induce asymmetry exogenously (see Götte and Schmutzler (2009) for a comprehensive review) or endogenize merger formation which yields asymmetric markets post-merger (Lindqvist and Stennek, 2005). In order to prevent path dependencies from merger formation, only data from those experimental studies that vary the number of competing firms exogenously across treatments is used for this meta-analysis.

[5]To the extent of the author's knowledge, the list in Table 4.1 is complete with the exception of Abbink and Brandts (2005, 2008) for which no experimental data is attainable.

TABLE 4.1: *Economic laboratory experiments that vary the number of competing firms.*

Study	Competition	Information: Complete	Information: Perfect	Matching	n
		Bertrand (price) competition			
Fouraker and Siegel (1963)	Homogeneous	✓	✓/ ✗	Partner	{2,3}
Dolbear et al. (1968)	Differentiated	✓/ ✗	✓	Partner	{2,4,16}
Dufwenberg and Gneezy (2000)	Homogeneous	✓	✓	Stranger	{2,3,4}
Orzen (2008)	Differentiated	✓	✓	Partner, Stranger	{2,4}
Davis (2009b)	Homogeneous	✓/ ✗	✓	Partner	{2,3,4}
Fonseca and Normann (2012)	Homogeneous	✓	✓	Partner	{2,4,6,8}
		Cournot (quantity) competition			
Fouraker and Siegel (1963)	Homogeneous	✓	✓/ ✗	Partner	{2,3}
Bosch-Domènech and Vriend (2003)	Homogeneous	✓/ ✗	✓	Partner	{2,3}
Huck et al. (2004b)	Homogeneous	✓	✓/ ✗	Partner	{2,3,4,5}
Waichman et al. (2014)	Homogeneous	✓	✓/ ✗	Partner	{2,3}

✓: applicable | ✗: not applicable | ✓/ ✗: both (as treatment variable)

Six experiments employ price competition. Four of those investigate homogeneous Bertrand competition, i.e., firms' products are perfect substitutes. Hence, the firm with the lowest price supplies the entire market demand. The remaining two experiments use differentiated price competition, i.e., competitors' products are differentiated with regard to quality or consumers have heterogeneous preferences: Dolbear et al. (1968) consider a model in which the cross-price elasticity is half the own-price elasticity; in Orzen (2008) a fraction of consumers are price-insensitive "convenience shoppers" (Orzen, 2008, p. 392). All of the four quantity competition experiments included in this meta-analysis employ a homogeneous Cournot model.

Experiments differ further in the amount of information provided to participants. In a situation of complete information, each firm, represented by an individual participant, knows about (or can retrieve) the cost and demand function of all firms in the market. Moreover, a firm with perfect information can observe all decisions made by its competitors, and hence, has knowledge over the full history of the game. Lastly, all but one study employ a fixed matching of firms over the entire time horizon. Instead, Dufwenberg and Gneezy (2000) match firms randomly in each period. Orzen (2008) additionally compares partner and stranger matching in a between-subject manner.

4.2.2 Measuring competitiveness

In order to compare the effect of different numbers of firms using data from various experimental designs a measure of competitiveness or likewise, tacit collusion, in oligopolies is warranted. As absolute price or quantity levels are inconclusive across experiments, different metrics are proposed in the literature to measure competitiveness in experimental oligopoly outcomes. For a review of Cournot experiments, Huck et al. (2004b) report the ratio between a market's average total quantity $\overline{Q}$ and the total Nash quantity Q^{Nash}, $r = \overline{Q}/Q^{Nash}$. However, as Engel (2007, p. 494) points out, r is "sensitive to arbitrary changes in the level of $Q^{N[ash]}$". In addition, the measure is not well suited to quantify and compare non-equilibrium outcomes between treatments and experimental designs, because it does not incorporate the JPM quantity as a second benchmark.

Therefore, the measure introduced in Subsection 3.3.2, which combines the indices proposed by Engel (2007) and Suetens and Potters (2007) is utilized here as well. It measures tacit collusion as the relative deviation of average price or profit from the theoretical equilibrium $E \in \{Nash, Walras\}$ towards the JPM price (p^{JPM}) or profit (Π^{JPM}). Formally,

$$\varphi_x^E = \frac{\overline{x} - x^E}{x^{JPM} - x^E}$$

with $x \in \{p, \Pi\}$. In this vein, φ_x^E represents the degree of tacit collusion based on prices or profits as compared to either the Nash equilibrium or the Walrasian (competitive) equilibrium as the theoretical prediction. The concept of Walrasian equilibrium assumes all competitors to be price-takers and thus, under homogeneous Bertrand competition, the Nash prediction and the Walrasian prediction coincide. Moreover, in any oligopoly competition model, Walrasian profits cannot exceed Nash profits, i.e., $\Pi^{Walras} \leq \Pi^{Nash}$. If $\varphi_x^E = 0$, the average market outcome $\overline{x}$ corresponds to the theoretical prediction by the equilibrium concept E. If $\varphi_x^E = 1$, the market is completely collusive and competitors behave like in the case of a single monopolist. Note that φ_p^E may exceed one if joint profit is not monotonic in prices, but $\varphi_\Pi^E \leq 1$. The measures' lower limits, however, depend on the experimental design. To account for differences due to treatment designs and for means of robustness, tacit collusion is reported and compared in this meta-analysis by all four different metrics resulting from a full-factorial combination of variable x and equilibrium concept E.[6] Which of the metrics is considered to constitute the relevant benchmark for competitiveness depends foremost on the primary concern of the regulatory authority. Whereas tacit collusion based on profits (instead of prices or quantities) is arguably most relevant to the competitors in a market, regulatory authorities may be primarily concerned about consumer surplus.[7] In oligopoly markets, tacit collusion based on prices serves as an adequate proxy for the competitiveness of a market as viewed by consumers.

In addition, Friedman (1971) suggests a theoretical benchmark to assess the likelihood "that tacit collusion can be sustained as an equilibrium in an infinitely repeated game context as part of a grim trigger strategy" (Suetens and Potters, 2007, p. 73), which is given by

$$Friedman = \frac{\Pi^{JPM} - \Pi^{Nash}}{\Pi^{Defect} - \Pi^{JPM}}$$

with Π^{Defect} as the maximum profit for a firm that unilaterally deviates from a collusive agreement. Hence, the Friedman (1971) index measures the incentive to collude implicitly by comparing the collusive markup on the Nash profit to the additional profit for defecting from cooperation. In repeated oligopoly experiments each firm has to trade off short-term profits from deviating to foregone profits in future periods. The higher the Friedman (1971) index, the less profitable a deviation from a collusive agreement.[8] Although the Friedman (1971) index assumes an infinitely repeated game, it may nonetheless be informative in the context of finitely repeated games in experiments with fixed lengths across treatments as it is well-known that tacit collusion is no phenomenon that is limited to experiments with random termination rules.

[6] Suetens and Potters (2007) exclude negative prices in Cournot experiments from their calculation of the degree of tacit collusion. In this meta-analysis, however, negative prices are considered as well, as they correctly reflect the high competitiveness of excess capacity in Cournot markets.

[7] For the discussion about the relevant welfare standard in merger control see, e.g., Farrell and Katz (2006).

[8] For Orzen (2008) the Friedman (1971) index has to be averaged over all three successive phases in each treatment in order to gain a single index value.

TABLE 4.2: *Degrees of tacit collusion in economic laboratory experiments that vary the number of competing firms.*

Study	Treatment	Periods[†]	n	N	φ_p^{Nash}	φ_Π^{Nash}	φ_p^{Walras}	φ_Π^{Walras}	*Friedman*
Bertrand (price) competition									
Fouraker and Siegel (1963)	Complete information	$[1,15] \in [1,15]$	2	17	0.412	0.441	0.412	0.441	0.766
			3	10	0.039	−0.318	0.039	−0.318	0.311
	Incomplete information	$[1,15] \in [1,15]$	2	17	0.149	0.141	0.149	0.141	0.766
			3	11	0.019	−0.252	0.019	−0.252	0.311
Dolbear et al. (1968)	Complete information	$[8,12] \in [1,15]$	2	18	0.300	0.300	0.500	0.611	1.250
			4	9	−0.040	−0.200	0.257	0.333	1.250
Dufwenberg and Gneezy (2000)	2/3/4	$[1,10] \in [1,10]$	2	12	0.260	0.260	0.260	0.260	1.000
			3	8	0.067	0.067	0.067	0.067	0.497
			4	6	0.077	0.077	0.077	0.077	0.331
Orzen (2008)	Fixed matching	$[1,90] \in [1,90]$	2	6	0.352	0.352	0.604	0.604	0.624
			4	6	−0.025	−0.025	0.381	0.381	0.206
	Random matching	$[1,90] \in [1,90]$	2	6	0.113	0.113	0.462	0.462	0.624
			3	6	−0.008	−0.008	0.391	0.391	0.206
Davis (2009b)	2np/3np/4np	$[1,220] \in [1,220]$	2	6	0.113	0.113	0.113	0.113	0.754
			3	6	0.006	0.006	0.006	0.006	0.376
			4	6	0.006	0.006	0.006	0.006	0.251
Fonseca and Normann (2012)	NoTalk	$[1,29] \in [1,29]$	2	6	0.504	0.504	0.504	0.504	1.020
			4	6	0.060	0.060	0.060	0.060	0.338
			6	6	0.025	0.025	0.025	0.025	0.202
			8	6	0.011	0.011	0.011	0.011	0.145
Cournot (quantity) competition									
Fouraker and Siegel (1963)	Complete information	$[1,22] \in [1,22]$	2	16	−0.244	−1.371	0.585	0.737	1.000
			3	11	−0.266	−0.869	0.367	0.533	0.750
	Incomplete information	$[1,22] \in [1,22]$	2	16	−0.114	−0.533	0.629	0.830	1.000
			3	11	−0.260	−0.773	0.370	0.557	0.750
Bosch-Domènech and Vriend (2003)	Easy	$[1,22] \in [1,22]$	2	9	0.296	0.475	0.765	0.942	0.889
			3	6	−0.176	−0.399	0.451	0.688	0.732
	Hard	$[1,22] \in [1,22]$	2	9	−0.159	−0.428	0.614	0.841	0.889
			3	6	−0.107	−0.284	0.484	0.714	0.732
	Hardest	$[1,22] \in [1,22]$	2	9	−0.164	−0.410	0.612	0.843	0.889
			3	6	−0.491	−1.234	0.304	0.501	0.732
Huck et al. (2004b)	Unified frame	$[1,25] \in [1,25]$	2	6	0.403	0.600	0.801	0.956	0.889
			3	8	0.032	−0.002	0.516	0.750	0.750
			4	6	0.065	0.070	0.439	0.665	0.640
			5	6	−0.109	−0.270	0.260	0.436	0.556
Waichman et al. (2014)	DSNC/TSNC	$[1,17] \in [1,17]$	2	12	−0.154	−0.332	0.615	0.852	0.889
			3	13	−0.265	−0.600	0.367	0.600	0.750
	DMNC/TMNC	$[1,17] \in [1,17]$	2	10	−0.046	−0.094	0.651	0.878	0.889
			3	11	−0.062	−0.127	0.469	0.718	0.750

[†] Periods used to compute the average degree of tacit collusion. If possible, data from all periods is used to maximize comparability.

4.2.3 Results

Table 4.2 reports the number of independent observations N, the four collusion metrics, and the Friedman (1971) index for all experiments and treatments considered in this meta-analysis.[9] The following analysis is two-fold: In a first step, the analysis is limited to effects within single studies (intra-study). As a second step, tacit collusion in duopolies, triopolies, and quadropolies is compared across all studies (inter-study).

RESULT 4.1. *Within and across the surveyed oligopoly experiments, markets with two firms are significantly more prone to tacit collusion than markets with three as well as four firms, everything else being equal. However, markets with three firms do not facilitate tacit collusion significantly more than markets with four firms.*

Data on the level of independent observations within studies can be obtained for five experiments.[10] Table 4.3 provides p-values from one-tailed non-parametric Mann-Whitney U tests of intra-study number effects on tacit collusion in these experiments. Following Hypothesis 4.1, i.e., the general assumption of a monotonic relationship between tacit collusion and the number of competitors, the null hypothesis is that tacit collusion is always higher in a market with more firms. With the exception of the metrics based on Nash predictions for Fouraker and Siegel's Cournot treatments, all test results indicate that tacit collusion is higher in duopolies than in triopolies (2 vs. 3) or quadropolies (2 vs. 4) at the 5% level of significance. However, triopolies are not found to be more prone to tacit collusion than quadropolies (3 vs. 4), neither under Bertrand competition nor under Cournot competition.

For inter-study comparisons at first the most comparable treatments between studies are selected in an effort to rule out any explanations for differences other than the number of competitors. Thus, only treatments with complete and perfect information,

[9] The original experimental data is either collected from tables in the respective study, downloaded from an online repository, or provided by the authors. One exception is Bosch-Domènech and Vriend (2003) for which the data is retrieved from figures.

[10] The author thanks Hans-Theo Normann and Henrik Orzen for providing the experimental data used in Huck et al. (2004b) and Orzen (2008), respectively.

TABLE 4.3: *Intra-study one-tailed Mann-Whitney U tests and associated p-values.*

Study	Treatment	n	φ_p^{Nash}	φ_Π^{Nash}	φ_p^{Walras}	φ_Π^{Walras}
		Bertrand (price) competition				
Fouraker and Siegel (1963)	Complete information	2 vs. 3	< 0.001	< 0.001	< 0.001	< 0.001
	Incomplete information	2 vs. 3	0.003	< 0.001	0.003	< 0.001
Orzen (2008)	Fixed matching	2 vs. 4	0.005	0.005	0.005	0.005
	Random matching	2 vs. 4	0.002	0.002	0.002	0.002
Davis (2009b)	2np/3np/4np	2 vs. 3	0.008	0.008	0.008	0.008
		2 vs. 4	0.008	0.008	0.008	0.008
		3 vs. 4	0.437	0.437	0.437	0.437
		Cournot (quantity) competition				
Fouraker and Siegel (1963)	Complete information	2 vs. 3	0.294	0.215	0.008	0.004
	Incomplete information	2 vs. 3	0.084	0.215	< 0.001	< 0.001
Huck et al. (2004b)	Unified frame	2 vs. 3	0.019	0.019	0.004	0.004
		2 vs. 4	0.019	0.019	0.002	0.002
		3 vs. 4	0.261	0.261	0.261	0.261

which are apparent in all studies, are considered for the following analysis.[11] Consequently, there are ten independent duopoly observations, seven independent triopoly observations, and six independent quadropoly observations. As there is only a single study for any $n > 4$ the statistical analysis is limited to markets with $n \in \{2,3,4\}$ firms. The Friedman (1971) index, which is suggested to assess the likelihood of tacit collusion, predicts poorly if correlated with φ_Π^{Nash} ($\rho = 0.005, p = 0.983$) but is positively and significantly correlated with φ_Π^{Walras} ($\rho = 0.594, p = 0.003$). In order to control for potential dependencies between treatments from the same study, i.e., different base levels of tacit collusion between experimental settings, the following three-level linear random-intercept model is estimated:

$$\begin{aligned}\varphi^E_{x,s,m,n} = {} & \beta_0 + \xi_s + \zeta_m \\ & + \beta_{Duopoly} \cdot Duopoly \\ & + \beta_{Quadropoly} \cdot Quadropoly \\ & + \beta_{Cournot} \cdot Cournot \\ & + \epsilon_{s,m,n},\end{aligned}$$

[11]The following treatments reported in Table 4.2 are *not* considered in this step of the inter-study analysis: Incomplete information (Fouraker and Siegel, 1963), Random matching (Orzen, 2008), Hard and Hardest (Bosch-Domènech and Vriend, 2003), and DMNC/TMNC in which participants are managers instead of students (Waichman et al., 2014).

where $\varphi^E_{x,s,m,n}$ is the average degree of tacit collusion φ^E_x of markets with n competitors under model $m \in \{Bertrand, Cournot\}$ in study s, ζ_m is the error component shared between observations of the same model in study s (see Bertrand and Cournot treatments in Fouraker and Siegel, 1963), and ξ_s is the error component shared between observations from the same study. The results, as portrayed in Table 4.4, confirm the insight of the above intra-study findings that there is significantly more tacit collusion in duopolies compared to triopolies and quadropolies. Furthermore, there is no significant difference in tacit collusion between triopolies and quadropolies. In particular, the degree of tacit collusion is on average 26 pp to 35 pp higher in duopolies than triopolies across the different metrics. However, the same does not hold for the comparison between markets with three and four firms as triopolies are found to have on average an insignificant 2 pp lower to 5 pp higher degree of tacit collusion than quadropolies. Also notice that the regression analysis replicates the finding by Suetens and Potters (2007) that *Bertrand colludes more than Cournot*—however, only if tacit collusion is based on Nash predictions. In contrast, when compared to Walrasian equilibrium, this effect is significant in the opposite direction. Thus, if a competitive market outcome where price equals marginal cost represents the benchmark for the degree of tacit collusion, *Cournot may collude more than Bertrand*. All these results hold if tacit collusion metrics are averaged over all treatments from each study with the same competition model and two, three, or four firms, respectively. See Appendix D.2 for results of the respective multilevel mixed-effects linear regressions.

The use of multilevel regression models in meta-analyses has a shortcoming: The implicit weights associated to each observation, i.e., each treatment in a study, are of equal magnitude. However, each of these values stems from an experiment designed to predict a true effect. In other words, the averages of the degree of tacit collusion in each treatment of a study (i.e., the sample means) used in the analysis here are estimators of the true degree of tacit collusion (i.e., the population mean) in duopolies, triopolies, and quadropolies, respectively. Consequently, one might argue that the standard error of each sample mean should be considered as an indication of a sample mean's reliability. Meta-regression, a method vastly used in medical research (see, e.g., Higgins

TABLE 4.4: *Multilevel mixed-effects linear regressions of tacit collusion on number of competitors and competition model on the basis of most comparable treatments.*

Covariate	(1) φ_p^{Nash}	(2) φ_Π^{Nash}	(3) φ_p^{Walras}	(4) φ_Π^{Walras}
Duopoly	0.259***	0.352***	0.261***	0.314***
	(0.048)	(0.119)	(0.031)	(0.056)
Quadropoly	−0.020	0.018	−0.003	0.053
	(0.060)	(0.147)	(0.039)	(0.069)
Cournot	−0.227**	−0.461**	0.263***	0.533***
	(0.088)	(0.221)	(0.050)	(0.074)
Constant	0.056	−0.022	0.156***	0.088
	(0.067)	(0.168)	(0.049)	(0.067)
Studies	9	9	9	9
Models	10	10	10	10
Observations	23	23	23	23

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

and Thompson, 2002), does exactly this by using the within-treatment standard errors as the standard deviations of the normal error terms in the model. More specifically, a random-effects meta-regression model is estimated which allows for between-study variance not explained by the covariates, i.e., the dummies for the number of firms.[12] This yields a weighted regression in which the inverse of the sum of the estimated between-study variance and the estimates' within-treatment variances are the individual weights associated to each treatment.

Table 4.5 depicts the estimates of meta-regression models with the same dependent and independent variables as in the multilevel mixed-effects regressions. With one exception, the results are strikingly similar. The singular exception is the insignificant effect of the competition model in the degree of tacit collusion based on Nash profits. With an effect of considerable magnitude, its estimated standard error is yet conspicuously high. It cannot be ruled out that this is due to the different number of observations in the meta-regressions as the standard errors of treatment averages cannot be gath-

[12]The estimates reported in Table 4.5 are derived with the *metareg* command of the statistical software package *Stata* in its version 12. See Harbord and Higgins (2008) for further information on the command.

TABLE 4.5: *Meta-regression of tacit collusion on number of competitors and competition model on the basis of most comparable treatments.*

Covariate	(1) φ_p^{Nash}	(2) φ_Π^{Nash}	(3) φ_p^{Walras}	(4) φ_Π^{Walras}
Duopoly	0.269***	0.472**	0.308***	0.369***
	(0.077)	(0.201)	(0.073)	(0.106)
Quadropoly	0.026	0.198	0.083	0.148
	(0.082)	(0.215)	(0.082)	(0.115)
Cournot	−0.182**	−0.240	0.320***	0.576***
	(0.076)	(0.207)	(0.067)	(0.098)
Constant	0.023	−0.136	0.064	−0.024
	(0.063)	(0.164)	(0.064)	(0.088)
Observations	21	17	21	17

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

ered from all studies:[13] For this reason, Bosch-Domènech and Vriend (2003) cannot be considered in the meta-regressions and Dolbear et al. (1968) as well as Waichman et al. (2014) cannot be included in the regressions on the profit-based degrees of tacit collusion.

Although the previous analyses control for different base levels of tacit collusion between experiments in the multilevel mixed-effects regression models as well as for the reliability of sample means in the meta-regressions, the data used in the previous regression models may be unbalanced with regard to the different number of treatments with different numbers of competitors by the studies. Consequently, number effects are next investigated inter-study also via matched samples. By this means, a comparison of n_1 and n_2 competitors includes all studies that have conducted treatments with n_1 *and* n_2 competitors. Note that, therefore, the number of included studies varies between pairwise comparisons, e.g., when comparing two with four and two with three competitors. Table 4.6 presents average degrees of tacit collusion and p-values based on

[13]The standard errors of the degree of tacit collusion estimates are derived with the following relationship: $SE\left(\varphi_x^E\right) = \frac{1}{\sqrt{N}}\sqrt{\frac{1}{N-1}\sum_{i=1}^{N}\left(\frac{x-x^E}{x^{JPM}-x^E} - \frac{\overline{x}-x^E}{x^{JPM}-x^E}\right)^2} = \frac{1}{\sqrt{N}}\sqrt{\frac{1}{N-1}\sum_{i=1}^{N}\left(\frac{x-\overline{x}}{x^{JPM}-x^E}\right)^2} = \frac{1}{x^{JPM}-x^E}\frac{1}{\sqrt{N}}\sqrt{\frac{1}{N-1}\sum_{i=1}^{N}\left(x-\overline{x}\right)^2} = \frac{1}{x^{JPM}-x^E}SE(x)$ with N as the number of independent observations for the corresponding treatment.

TABLE 4.6: *Inter-study average degrees of tacit collusion and one-tailed matched-samples Wilcoxon signed-rank tests on the basis of most comparable treatments.*

	Studies	φ_p^{Nash}	φ_Π^{Nash}	φ_p^{Walras}	φ_Π^{Walras}
2 vs. 3					
Duopoly	7	0.155	0.027	0.507	0.614
Triopoly	7	−0.081	−0.302	0.259	0.332
p-value	7	0.009	0.046	0.009	0.009
2 vs. 4					
Duopoly	6	0.322	0.355	0.464	0.508
Quadropoly	6	0.024	−0.002	0.203	0.254
p-value	6	0.014	0.014	0.014	0.014
3 vs. 4					
Triopoly	3	0.035	0.024	0.196	0.274
Quadropoly	3	0.049	0.051	0.174	0.249
p-value	3	0.946	0.946	0.500	0.500

one-tailed non-parametric Wilcoxon signed-rank tests. Again, the tested null hypothesis is that tacit collusion is higher in markets with more firms than in markets with less firms.

Test results show that tacit collusion is significantly higher in duopolies than in triopolies (2 vs. 3) and quadropolies (2 vs. 4), respectively. However, based on all experiments that run triopolies as well as quadropolies, the former is not more prone to tacit collusion than the latter (3 vs. 4). In fact and in stark contrast to Hypothesis 4.1, tacit collusion may even be slightly higher in markets with four firms ($\varphi_\Pi^{Nash} = 0.051$) than in markets with three firms ($\varphi_\Pi^{Nash} = 0.024$) and this difference is almost significant at the 5% level ($N = 3, p = 0.054$). Again, results are similar if tacit collusion metrics are averaged over all treatments from each study with the same competition model and two, three, or four firms, respectively. See Appendix D.2 for results of similar Wilcoxon signed-rank tests. All these results support the previous findings in the context of intra-study comparisons. In sum, there is no evidence, neither individual nor aggregate, in the surveyed oligopoly experiments that markets with four firms may be more competitive than markets with three firms.

4.3 Experiment with symmetric firms

Meta-analyses provide valuable insight across heterogeneous studies by verifying robustness and external validity of systematic effects. However, meta-studies in general and the above in particular has several limitations. First, due to a lack of control for all differences between studies considered in the same analysis, the internal validity of meta-results is per definitionem questionable. Second, in this specific meta-analysis, the number of independent observations of the pairwise comparisons is rather low. In particular, only three studies cover triopoly and quadropoly treatments. Last but foremost, none of the experiments in the meta-study employs treatments with all the relevant characteristics considered here, i.e., Bertrand and Cournot markets with two, three, and four firms. Thus, in an effort to reassess the findings of the meta-analysis, an experiment with price and quantity competition in duopolies, triopolies, and quadropolies is warranted. In the following, such an experiment is reported which is based on a model that exploits the duality between Bertrand and Cournot. By means of controlled variation, this allows to validate the results obtained in the meta-analysis.

4.3.1 Oligopoly competition

Price competition à la Bertrand and quantity competition à la Cournot serve as good proxies for a large share of models on oligopoly competition. As homogeneous price competition is often deemed unrealistic and yields a discontinuous demand function, a model by Singh and Vives (1984) is considered that generalizes the Hotelling (1929) model to exploit the duality between price and quantity competition in differentiated goods. More precisely, the model's generalization to more than two firms (see, e.g., Häckner, 2000) is used, which is described in detailed in Subsection 3.3.1. See Appendix B.1 for a thorough analysis of the model with asymmetric firms and three different theoretical predictions, namely Nash equilibrium, Walrasian (competitive) equilibrium and collusive equilibrium. With regard to the model, in the following it is assumed that firms differ horizontally, but not vertically, i.e., all firms' products are equal in quality.

4.3.2 Procedures

Treatments cover Bertrand and Cournot competition in duopolies, triopolies, and quadropolies in a full-factorial design, i.e., there is a total of six different treatments. In the following, these treatments are referred to with abbreviations such as B4 for the Bertrand quadropoly treatment. The model is parametrized with $\omega = 100$, $\lambda = 1$, and $\theta = \frac{2}{3}$ so that goods are substitutes. Consequently, $\Omega = \frac{300}{2n+1}$, $\Lambda = \frac{6n-3}{2n+1}$, and $\Theta = \frac{6n-6}{2n+1}$. Table 4.7 shows the corresponding theoretical benchmarks of the one-shot game for each treatment. As Nash prices, quantities, and profits do not coincide under Bertrand and Cournot competition and are additionally dependent on the number of competitors n, these values are not adequate to compare cooperative intentions, i.e., tacit collusion, across treatments. Thus, the same measure as for the meta-analysis is utilized, i.e., the degree of tacit collusion φ_x^E.

Note that the parametrization is identical to the experiment reported in Chapter 3. The same holds for the experimental software, instructions, and all other procedural details. Therefore, data from discrete time sessions of duopoly and triopoly treatments is used here again. The quadropoly sessions were run in April 2015, also at the Karlsruhe Institute of Technology in Karlsruhe, Germany. In total, 212 students of economic fields with an average age of 22 years participated in the sessions of the experiment run in a between-subject design. Their payoff, which they received privately and in cash, averaged at EUR 17.61.

4.3.3 Hypotheses

Due to an effort to normalize input and output variables of the model, the different measures of the degree of tacit collusion have two desirable characteristics in the experiment. First, the Nash prediction-based degrees serve as good predictors of relative differences in tacit collusion between treatments as Nash equilibria vary with the competition model as well as with the number of firms in the market. In other words,

TABLE 4.7: *Theoretical benchmarks of oligopoly competition for each treatment with symmetric firms.*

<table>
<tr><th></th><th>Bertrand</th><th>Cournot</th></tr>
<tr><td rowspan="9">Duopoly</td><td colspan="2">$p^{Walras} = 0$</td></tr>
<tr><td colspan="2">$q^{Walras} = 60.00$</td></tr>
<tr><td colspan="2">$\Pi^{Walras} = 0$</td></tr>
<tr><td>$p^{Nash} = 25.00$</td><td>$p^{Nash} = 37.50$</td></tr>
<tr><td>$q^{Nash} = 45.00$</td><td>$q^{Nash} = 37.50$</td></tr>
<tr><td>$\Pi^{Nash} = 1125.00$</td><td>$\Pi^{Nash} = 1406.25$</td></tr>
<tr><td colspan="2">$p^{JPM} = 50.00$</td></tr>
<tr><td colspan="2">$q^{JPM} = 40.00$</td></tr>
<tr><td colspan="2">$\Pi^{JPM} = 1500.00$</td></tr>
<tr><td rowspan="9">Triopoly</td><td colspan="2">$p^{Walras} = 0$</td></tr>
<tr><td colspan="2">$q^{Walras} = 42.86$</td></tr>
<tr><td colspan="2">$\Pi^{Walras} = 0$</td></tr>
<tr><td>$p^{Nash} = 16.47$</td><td>$p^{Nash} = 30.00$</td></tr>
<tr><td>$q^{Nash} = 35.71$</td><td>$q^{Nash} = 30.00$</td></tr>
<tr><td>$\Pi^{Nash} = 595.24$</td><td>$\Pi^{Nash} = 900.00$</td></tr>
<tr><td colspan="2">$p^{JPM} = 50.00$</td></tr>
<tr><td colspan="2">$q^{JPM} = 21.43$</td></tr>
<tr><td colspan="2">$\Pi^{JPM} = 1071.43$</td></tr>
<tr><td rowspan="9">Quadropoly</td><td colspan="2">$p^{Walras} = 0$</td></tr>
<tr><td colspan="2">$q^{Walras} = 33.33$</td></tr>
<tr><td colspan="2">$\Pi^{Walras} = 0$</td></tr>
<tr><td>$p^{Nash} = 12.50$</td><td>$p^{Nash} = 25.00$</td></tr>
<tr><td>$q^{Nash} = 29.17$</td><td>$q^{Nash} = 25.00$</td></tr>
<tr><td>$\Pi^{Nash} = 364.58$</td><td>$\Pi^{Nash} = 625.00$</td></tr>
<tr><td colspan="2">$p^{JPM} = 50.00$</td></tr>
<tr><td colspan="2">$q^{JPM} = 16.67$</td></tr>
<tr><td colspan="2">$\Pi^{JPM} = 833.33$</td></tr>
</table>

TABLE 4.8: *Nash predictions as measured by the Walrasian-based degrees of tacit collusion.*

	Bertrand	Cournot
Duopoly	$\varphi_{p/q}^{Walras}(p^{Nash}) = 0.50$ $\varphi_{\Pi}^{Walras}(\Pi^{Nash}) = 0.75$	$\varphi_{p/q}^{Walras}(p^{Nash}) = 0.75$ $\varphi_{\Pi}^{Walras}(\Pi^{Nash}) = 0.94$
Triopoly	$\varphi_{p/q}^{Walras}(p^{Nash}) = 0.33$ $\varphi_{\Pi}^{Walras}(\Pi^{Nash}) = 0.56$	$\varphi_{p/q}^{Walras}(p^{Nash}) = 0.60$ $\varphi_{\Pi}^{Walras}(\Pi^{Nash}) = 0.84$
Quadropoly	$\varphi_{p/q}^{Walras}(p^{Nash}) = 0.25$ $\varphi_{\Pi}^{Walras}(\Pi^{Nash}) = 0.44$	$\varphi_{p/q}^{Walras}(p^{Nash}) = 0.50$ $\varphi_{\Pi}^{Walras}(\Pi^{Nash}) = 0.75$

these measures effectively control for the different theoretical predictions of the Nash concept. A difference in the degree of tacit collusion between treatments would thus indicate a higher or lower competitiveness in contrast to standard theory. The findings from the previous meta-analysis suggest the following hypothesis.

HYPOTHESIS 4.2. *The degrees of tacit collusion based on Nash prices and profits are significantly higher in markets with two firms than in markets with three as well as four firms but not significantly higher in markets with three firms than in markets with four firms, everything else being equal.*

Second, whilst the Nash-based degrees measure relative differences in tacit collusion, the Walrasian-based degrees measure absolute differences to a universal baseline, as the experiment is specifically designed to have a constant Walrasian equilibrium and collusive equilibrium across treatments. Due to the normalizations of input variables, choosing a price or quantity of $p, q \in [0, 100]$ in the experiment directly translates to a Walrasian price-based degree of tacit collusion of $2p\%$ in the Bertrand or $2(100 - q)\%$ in the Cournot treatments, respectively. Furthermore, the degree of tacit collusion based on Walrasian profit is identical across treatments and centered symmetrically about its maximum at a value of 50. Consequently, the Walrasian-based measures do not control for Nash predictions.

In order to emphasize that the Walrasian-based measures do not take account of the differing Nash equilibria, Table 4.8 depicts the degree of tacit collusion based on Walrasian predictions associated with each treatment's Nash equilibrium. It is decreasing with the number of firms in the market. Therefore, if participants in the experiment do not have an inexplicable preference towards a certain integer within the interval $[0, 100]$ or even choose prices and quantities randomly, these tacit collusion measures should decrease with the number of firms. By this means, the degree of tacit collusion based on Walrasian price and profit has two purposes. First, the measures serve as a robustness check if subjects' behavior in the experiment is reasonable. Second, in the model consumer surplus as well as total welfare are monotonically decreasing in prices if goods are substitutes and hence, for regulatory authorities, Walrasian equilibrium may also serve as a relevant theoretical benchmark.

HYPOTHESIS 4.3. *The degrees of tacit collusion based on Walrasian price and profit are significantly higher in markets with two firms than in markets with three as well as four firms and significantly higher in markets with three firms than in markets with four firms, everything else being equal.*

4.3.4 Results

The experimental data amounts—with one exception—to 12 Bertrand and Cournot duopolies, triopolies, and quadropolies each. The exception is the C4 treatment, for which there are only 11 independent observations due to a no-show. Before analyzing the experimental data longitudinally, Table 4.9 provides an overview of experimental data based on the level of independent cohorts over all 60 periods each.[14] Similar to the previous meta-study, the Friedman (1971) index predicts the degree of tacit collusion poorly in terms of φ_{Π}^{Nash} ($\rho = -0.151, p = 0.775$) but is significantly correlated with φ_{Π}^{Walras} ($\rho = 0.846, p = 0.034$).

For an in-depth analysis of firms' longitudinal behavior, a mixed-effects model is employed to control for different base levels of tacit collusion in cohorts via a random

[14] Note that one duopoly in treatment C2 is exceptionally competitive. In particular, its average degree of tacit collusion based on Nash profits lies almost three standard deviations below the treatment mean. All results reported in the following hold if this outlier is dropped.

TABLE 4.9: *Average degrees of tacit collusion across treatments.*

Treatment	N	$\varphi_{p/q}^{Nash}$	φ_{Π}^{Nash}	$\varphi_{p/q}^{Walras}$	φ_{Π}^{Walras}	*Friedman*
B2	12	0.832 (0.249)	0.806 (0.302)	0.916 (0.124)	0.951 (0.075)	0.750
B3	12	0.605 (0.324)	0.611 (0.301)	0.737 (0.216)	0.827 (0.134)	0.556
B4	12	0.433 (0.286)	0.390 (0.263)	0.575 (0.215)	0.657 (0.148)	0.438
C2	12	0.627 (0.550)	0.437 (1.030)	0.907 (0.138)	0.965 (0.064)	0.936
C3	12	0.397 (0.484)	0.249 (0.702)	0.759 (0.193)	0.880 (0.112)	0.831
C4	11	0.280 (0.391)	0.202 (0.556)	0.640 (0.196)	0.800 (0.139)	0.750

Standard deviations in parentheses.

intercept as well as for different time dependencies due to learning via a random slope. Thus, the estimated model is

$$\begin{aligned}\varphi^{E}_{x,k,t} = \beta_0 + \xi_k \\ + \beta_{Duopoly} \cdot Duopoly \\ + \beta_{Quadropoly} \cdot Quadropoly \\ + \beta_{Cournot} \cdot Cournot \\ + (\beta_{Period} + \beta_{Period,k}) \cdot t \\ + \epsilon_{k,t}\end{aligned}$$

with $\varphi^{E}_{x,k,t}$ as the average degree of tacit collusion of all firms' prices, quantities or profits in cohort k in period t. Table 4.10 shows the estimated coefficients for each of the four different measures of the degree of tacit collusion.[15] All results reported in the following with respect to prices or quantities hold also if the degree of tacit collusion is measured by transaction prices, i.e., prices weighted by the quantities sold.

[15]Note that due to the dualism of the competition model used in the experiment, the degrees of tacit collusion measured by prices or quantities coincide.

RESULT 4.2. *In the experiment with symmetric firms, the degree of tacit collusion based on Nash prices and profits is significantly higher in markets with two firms than in markets with three as well as four firms, everything else being equal. However, markets with three firms do not facilitate tacit collusion significantly more than markets with four firms.*

In line with the meta-analysis, the duopolies show on average a statistically significant 20 pp to 26 pp higher degree of tacit collusion than triopolies based on Nash predictions. Furthermore, the Nash-based degree of tacit collusion is statistically indistinguishable in experimental markets with three and four firms. According to the same measures, Bertrand competition colludes more than Cournot competition. In sum, the experiment confirms Hypothesis 4.2 and replicates the findings in the meta-analysis if tacit collusion is normalized by Nash predictions. Moreover, there is a small but significant negative time trend in the degree of tacit collusion based on Nash prices, which is a well-known property of economic lab experiments with fixed termination rules.

RESULT 4.3. *In the experiment with symmetric firms, the degree of tacit collusion based on Walrasian prices and profits is significantly higher in markets with two firms than in markets with three as well as four firms and significantly higher in markets with three firms than in markets with four firms, everything else being equal.*

With respect to the Walrasian-based degree of tacit collusion measures the data shows an almost symmetric significant 10 pp to 14 pp increase (decrease) in duopolies (quadropolies) compared to triopolies. Hence, without controlling for different Nash equilibria there is a monotonically decreasing, approximately linear trend of the degree of tacit collusion as the number of firms in the market increases, which supports Hypothesis 4.3. These findings indicate that subjects do indeed react to differences in theoretical predictions. In other words, although tacit collusion is higher than suggested by theory throughout, the treatment differences approximately match the differences in Nash predictions (see Table 4.8). The same holds for differences between the two modes of competition. Measured in Walrasian-based degree of tacit collusion, Nash prices in Cournot competition have a small constant markup compared to Bertrand

TABLE 4.10: *Multilevel mixed-effects linear regressions of tacit collusion on number of competitors and competition model under competition between symmetric firms.*

Covariate	(1) $\varphi_{p/q}^{Nash}$	(2) φ_{Π}^{Nash}	(3) $\varphi_{p/q}^{Walras}$	(4) φ_{Π}^{Walras}
Duopoly	0.204**	0.262**	0.144***	0.101***
	(0.094)	(0.123)	(0.045)	(0.031)
Quadropoly	−0.140	−0.081	−0.131***	−0.101***
	(0.095)	(0.124)	(0.046)	(0.031)
Cournot	−0.226***	−0.220**	−0.006	0.064**
	(0.078)	(0.101)	(0.037)	(0.026)
Period	−0.002*	−0.003	−0.001**	−0.001**
	(< 0.001)	(0.002)	(< 0.001)	(< 0.001)
Constant	0.670***	0.596***	0.784***	0.837***
	(0.077)	(0.101)	(0.037)	(0.025)
Cohorts	71	71	71	71
Observations	4,260	4,260	4,260	4,260

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

competition and Nash profits have a markup in the same direction that is increasing with the number of firms in the market. In this experimental sample, the latter difference is apparent which is indicative of an interaction effect between the competition model and quadropoly treatment dummy. Adding this interaction to the regression model on φ_{Π}^{Walras} its effect ($\beta_{Cournot \times Quadropoly} = 0.091, SE = 0.054, p = 0.090$) is in fact significant on the 10% level and renders the pure treatment effect of Cournot competition ($\beta_{Cournot} = 0.034, SE = 0.031, p = 0.261$) insignificant. None of the other collusion degree measures has a similar significant interaction effect (see Table D.4 in Appendix D.2). Moreover, there is a small yet significant negative time trend in both Walrasian-based degrees of tacit collusion as well as the degree of tacit collusion based on Nash prices and quantities.

In summary, the results attained from the experiment with symmetric firms validate the pooled findings of the meta-analysis. In particular, the experimental data provides evidence that diverging Nash predictions have some explanatory power of the degree of tacit collusion and that tacit collusion in triopolies and quadropolies is statistically indistinguishable if controlled for differences in Nash equilibria. Moreover, all results from the meta-analysis hold if the data from the experiment is added in the same aggregated manner as for the other studies.

4.4 Experiment with asymmetric firms

In reality, firms are likely not only to differ in horizontal terms but also in a vertical sense. The assumption of symmetry of firms with regard to technologies and cost structures, i.e., the assumption of vertical homogeneity, is frequently challenged in models of oligopoly competition. Although for some markets, e.g., for electricity, competition may be perceived as vertically symmetric, most oligopolistic markets for tangible and intangible goods alike exhibit differences in the market power of firms. As tacit collusion has been attributed to be driven by symmetry of firms in the economic literature (Ivaldi et al., 2003), number effects in oligopolies may also interact with the symmetry of firms. In other words, the propensities of different numbers of competitors may depend on the assumption of symmetry and thus, change if firms are differentiated not only horizontally but also vertically. Therefore, two additional experimental treatments are considered with asymmetric, i.e., vertically differentiated, firms to test whether differences in the degree of tacit collusion emerge between triopolies and quadropolies, which clearly is the most decisive comparison both scientifically as well as for antitrust policy.

Many oligopolistic industries emerged from former monopolies. This holds especially for network industries such as telecommunications or energy. In case such industries have not (yet) reached a state of relatively symmetric market power between firms, they are often characterized by one single firm dominating several other firms. In the following, for means of simplicity and as a reference to network industries as a prominent example, it will be convenient to think of an incumbent and two or three entrants competing against each other. More specifically, such a market design resembles the regularities of a vast majority of European mobile telecommunications markets, which are comprised of one large dominating and two or three smaller network operators, totaling at three or four cellular networks in each national market. In the model, asymmetry is implemented by establishing a single firm with a higher quality good (incumbent) than the remaining—two or three—firms (entrants). As consumers value quality, the incumbent's market share is higher than that of the entrants with equal prices. Equivalently, for equal market shares the incumbent may charge a higher price for its good than the entrants.

4.4.1 Asymmetric oligopoly competition

For means of comparability to the previous experimental treatments, the model introduced in Subsection 3.3.1 is extended to allow for asymmetric firms. See Appendix B.1 for a thorough analysis of the model with horizontal as well as vertical differentiation. In the model, ω_i constitutes the reservation price of firm i's consumers and thus, may be interpreted as the quality of firm i's product. Consequently, if the quality of one firm's product is higher than that of the other firms, i.e., $\omega_i > \omega_{-i}$, the former has higher market power that results in higher equilibrium price, market share, and thus, profit. Following the asymmetric market scenario described above, let $\Delta = \omega_{Incumbent} - \omega_{Entrant}$ denote the markup quality of the incumbent's good compared to the entrants' goods. Thereby, the extent of asymmetry in product quality may be determined by a single parameter.

4.4.2 Procedures

Two additional asymmetric treatments—an asymmetric Bertrand triopoly (B3A) and an asymmetric Bertrand quadropoly (B4A)—are considered as a robustness check with respect to the previous finding that (symmetric) triopolies are not significantly more prone to tacit collusion than quadropolies. The parametrization is the same as for the symmetric treatments, i.e., $\omega_{Entrant} = 100$, $\lambda = 1$, and $\theta = \frac{2}{3}$, except for the single asymmetry parameter. The only difference now is that Δ is greater than zero. Motivated by common market shares in European telecommunications markets, Δ is chosen such that

TABLE 4.11: *Theoretical benchmarks of oligopoly competition for the asymmetric Bertrand treatments.*

	Incumbent	Entrant
Triopoly	$p^{Walras} = 0$	$p^{Walras} = 0$
	$q^{Walras} = 55.92$	$q^{Walras} = 37.63$
	$\Pi^{Walras} = 0$	$\Pi^{Walras} = 0$
	$p^{Nash} = 19.38$	$p^{Nash} = 15.82$
	$q^{Nash} = 41.52$	$q^{Nash} = 33.90$
	$\Pi^{Nash} = 804.46$	$\Pi^{Nash} = 536.31$
	$p^{JPM} = 53.05$	$p^{JPM} = 50.00$
	$q^{JPM} = 27.96$	$q^{JPM} = 18.82$
	$\Pi^{JPM} = 1483.16$	$\Pi^{JPM} = 940.82$
Quadropoly	$p^{Walras} = 0$	$p^{Walras} = 0$
	$q^{Walras} = 44.50$	$q^{Walras} = 30.14$
	$\Pi^{Walras} = 0$	$\Pi^{Walras} = 0$
	$p^{Nash} = 14.67$	$p^{Nash} = 11.98$
	$q^{Nash} = 34.23$	$q^{Nash} = 27.95$
	$\Pi^{Nash} = 502.04$	$\Pi^{Nash} = 334.69$
	$p^{JPM} = 52.39$	$p^{JPM} = 50.00$
	$q^{JPM} = 22.25$	$q^{JPM} = 15.07$
	$\Pi^{JPM} = 1165.70$	$\Pi^{JPM} = 753.58$

the incumbent's Nash equilibrium profit is 50% higher than an entrant's Nash equilibrium profit. Thus, the incumbent's market share with regard to its proportion of joint Nash equilibrium profits is $3/7 \approx 43\%$ in a triopoly and $1/3 \approx 33\%$ in a quadropoly. As market power is a relative rather than an absolute concept, matching the equilibrium profit markup for different numbers of firms has two important advantageous over alternative approaches such as holding the incumbent's market share constant. First, entrants are equivalent to firms in the symmetric treatments which increases comparability between symmetric and asymmetric market structures. Second, the additional relative market power of the incumbent compared to any single entrant is independent of the number of firms which increases comparability between asymmetric triopolies and quadropolies. For the two asymmetric Bertrand treatments a Nash equilibrium profit markup for the incumbent of 50% requires $\Delta = 6.10$ in triopolies and $\Delta = 4.79$ in quadropolies. Table 4.11 lists the corresponding theoretical predictions of the one-shot game for both asymmetric treatments.

The same procedures as for the symmetric treatments are applied to further increase comparability across treatments and minimize potential experimental confounds. First, the action space of the incumbent is rescaled such that the JPM prices of all firms coincide at a price of 50 in an action space of [0,100]. Second, profits are standardized such that an entrant would have the same Nash equilibrium gains as a firm in any of the symmetric treatments. Consequently, incentives to deviate from the theoretical Nash prediction are equal for entrants and symmetric firms. The same scaling factor is applied to the entrants' as well as to the incumbent's profits so that the asymmetry in market power is not affected.

Except for an additional paragraph in the experimental instructions explaining how one of the firms deviates from the others, the exact same experimental procedures are followed for the asymmetric treatments as previously for the symmetric counterparts.[16] Again, the experiment was run at the Karlsruhe Institute of Technology in Karlsruhe, Germany, and participants were recruited via the ORSEE platform for sessions between

[16]For illustrative purposes, experimental instructions for the asymmetric Bertrand quadropoly treatment are provided in Appendix C.2.

TABLE 4.12: *Nash predictions as measured by Walrasian-based degree of tacit collusion under asymmetric Bertrand competition controlling for scaling in each treatment.*

	Incumbent	Entrant
Triopoly	$\varphi_{p/q}^{Walras}(p^{Nash}) = 0.37$	$\varphi_{p/q}^{Walras}(p^{Nash}) = 0.32$
	$\varphi_{\Pi}^{Walras}(\Pi^{Nash}) = 0.54$	$\varphi_{\Pi}^{Walras}(\Pi^{Nash}) = 0.57$
Quadropoly	$\varphi_{p/q}^{Walras}(p^{Nash}) = 0.28$	$\varphi_{p/q}^{Walras}(p^{Nash}) = 0.24$
	$\varphi_{\Pi}^{Walras}(\Pi^{Nash}) = 0.43$	$\varphi_{\Pi}^{Walras}(\Pi^{Nash}) = 0.44$

June and August 2015. None of the 84 students of economic fields participating in one of the two asymmetric treatments had previously participated in one of the symmetric treatments. The participants' payoff averaged at EUR 19.90.

4.4.3 Hypotheses

Evidence from both the meta-analysis and the experiment with symmetric firms suggests that triopolies do not facilitate tacit collusion more than quadropolies if all other market structure parameters are equal. Similar to the symmetric treatments, the rescaling of prices and profits described above yields an important property of the Walrasian-based degrees of tacit collusion: They measure tacit collusion in an absolute fashion since both the Walrasian equilibrium and the collusive equilibrium are constant across treatments. Thus, Nash predictions vary across firms and treatments if measured according to the Walrasian-based degrees of tacit collusion (see Table 4.12). Taken together, the previous findings suggest the following pooled hypothesis.

HYPOTHESIS 4.4. *The degrees of tacit collusion based on Walrasian (Nash) prices and profits are (not) significantly higher in markets with three asymmetric firms than in markets with four asymmetric firms, everything else being equal.*

Even if evidence suggests that asymmetric triopolies and quadropolies do not facilitate collusion to different extents, introducing asymmetry in an oligopoly whilst keeping the number of firms constant may alter the strategic interaction of firms and thus the propensities of firms to collude tacitly. Most of the economic literature suggests that symmetry is a driver of the ability to collude (see, e.g., Ivaldi et al., 2003). Fonseca and Normann (2008) test experimentally for the impact of asymmetry in capacity allocations on tacit collusion in Bertrand-Edgeworth duopolies and triopolies. Controlling for the number of firms they find that symmetric allocations facilitate tacit collusion compared to asymmetry. Following the theoretical and experimental evidence, the following hypothesis is derived.

HYPOTHESIS 4.5. *The degrees of tacit collusion based on Nash as well as Walrasian prices and profits are significantly lower in markets with asymmetric firms than in markets with symmetric firms, everything else being equal.*

4.4.4 Results

Same as for the symmetric treatments, there are 12 independent asymmetric Bertrand triopolies and quadropolies each. Summary statistics for both new treatments are provided in Table 4.13. Means over cohorts are computed by averaging over all firms, i.e., the incumbent and each entrant are weighted equally. However, all results reported in the following hold also if only the incumbent's degree of tacit collusion in prices and profits is considered as well as if only the entrants' degrees of tacit collusion are used. With regard to the former, the Friedman (1971) index is significantly correlated with φ_{Π}^{Nash} ($\rho = 0.424, p = 0.039$) as well as φ_{Π}^{Walras} ($\rho = 0.639, p = 0.001$). Regarding the entrants, the Friedman (1971) index—in line with the symmetric treatments—predicts φ_{Π}^{Nash} poorly ($\rho = 0.115, p = 0.592$) but has some explanatory power for φ_{Π}^{Walras} ($\rho = 0.393, p = 0.058$). Furthermore, the Friedman (1971) index predicts that the incumbent faces a higher incentive to tacitly collude—or, vice versa, that it has a lower incentive to deviate from a collusive agreement. Thus, according to the Friedman (1971) index, the entrants are supposed to be the drivers of competition. In the experimental sample, however, prices and profits in terms of the degree of tacit collusion vary neither largely nor significantly between firms based on Nash prices and profits as well as Wal-

TABLE 4.13: *Average degrees of tacit collusion across asymmetric treatments.*

Treatment	N	$\varphi^{Nash}_{p/q}$	φ^{Nash}_{Π}	$\varphi^{Walras}_{p/q}$	φ^{Walras}_{Π}	*Friedman* Incumbent	*Friedman* Entrant
B3A	12	0.332	0.316	0.554	0.700	0.792	0.444
		(0.296)	(0.276)	(0.197)	(0.121)		
B4A	12	0.244	0.214	0.432	0.560	0.619	0.379
		(0.163)	(0.246)	(0.122)	(0.137)		

Standard deviations in parentheses.

rasian profits. The single exception is the degree of tacit collusion based on Walrasian prices, according to which the incumbent chooses about 5 pp more collusive prices than the entrants over both treatments. This difference coincides with the disparity between the Nash predictions (see Table 4.12) and is significant according to a matched-samples Wilcoxon signed-rank test ($z = 3.29, p = 0.001$).

For an analysis of firms' behavior in the asymmetric treatments, a similar mixed-effects model as for the symmetric treatments is employed to control for different base levels of tacit collusion in cohorts via a random intercept as well as for different time dependencies due to learning via a random slope. Thus, the estimated model is

$$\begin{aligned}\varphi^{E}_{x,k,t} = \beta_0 + \xi_k & \\ &+ \beta_{Quadropoly} \cdot Quadropoly \\ &+ (\beta_{Period} + \beta_{Period,k}) \cdot t \\ &+ \epsilon_{k,t}\end{aligned}$$

with $\varphi^{E}_{x,k,t}$ as the average degree of tacit collusion of the incumbent's and the entrants' prices, quantities or profits in cohort k in period t. Table 4.14 provides estimated coefficients for each of the four different measures of the degree of tacit collusion.

RESULT 4.4. *In the experiment with asymmetric firms, the degree of tacit collusion based on Walrasian (Nash) prices and profits is (not) significantly higher in markets with three firms than in markets with four firms, everything else being equal.*

TABLE 4.14: *Multilevel mixed-effects linear regressions of tacit collusion on number of competitors under Bertrand competition between asymmetric firms.*

Covariate	(1) $\varphi_{p/q}^{Nash}$	(2) φ_{Π}^{Nash}	(3) $\varphi_{p/q}^{Walras}$	(4) φ_{Π}^{Walras}
Quadropoly	-0.174	-0.099	-0.173^{*}	-0.138^{***}
	(0.139)	(0.109)	(0.094)	(0.051)
Period	-0.004^{***}	-0.001	-0.003^{***}	-0.001
	(0.001)	(0.001)	(0.001)	(< 0.001)
Constant	0.498^{***}	0.346^{***}	0.666^{***}	0.720^{***}
	(0.098)	(0.077)	(0.066)	(0.036)
Cohorts	24	24	24	24
Observations	1,440	1,440	1,440	1,440

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The Nash-based degrees of tacit collusion are, on average, 10 pp to 17 pp lower in quadropolies than in triopolies. However, in line with the previous findings from the meta-analysis as well as the symmetric treatments, this difference is statistically insignificant. Also consistent with the results under symmetry, the Walrasian-based degrees of tacit collusion are significantly lower in markets with four firms than markets with three firms with a magnitude of 14 pp to 17 pp. Furthermore, the common negative time trend of prices due to an end-game effect can be found. All these results hold accordingly if incumbent and entrants are analyzed separately and thus, support Hypothesis 4.4 and replicate the findings under symmetry. Across symmetric and asymmetric market structures, however, the degree of tacit collusion may differ, as a comparison of Tables 4.9 and 4.13 indicates.

Although there is only a single parametrization difference between each asymmetric treatment and its symmetric counterpart, the necessary adjustment of Δ according to the number of firms in the market impedes a simultaneous analysis of asymmetry and the number of firms. In other words, the treatment dummy between asymmetric triopolies and quadropolies is not the same as the one between symmetric triopolies and quadropolies. Therefore, in order to assess the effect of the specific type of asymmetry implemented here, i.e., providing a single firm with a 50% higher Nash profit than its competitors, requires separate analyses of triopolies and quadropolies. In the same

TABLE 4.15: *Multilevel mixed-effects linear regressions of tacit collusion on (a)symmetry of firms in triopolies.*

Covariate	(1) $\varphi^{Nash}_{p/q}$	(2) φ^{Nash}_{Π}	(3) $\varphi^{Walras}_{p/q}$	(4) φ^{Walras}_{Π}
Asymmetry	-0.185	-0.291^{**}	-0.123	-0.125^{**}
	(0.149)	(0.119)	(0.099)	(0.052)
Period	-0.004^{***}	-0.001	-0.003^{***}	-0.001
	(0.001)	(0.001)	(0.001)	(< 0.001)
Constant	0.685^{***}	0.651^{***}	0.790^{***}	0.845^{***}
	(0.106)	(0.084)	(0.070)	(0.037)
Cohorts	24	24	24	24
Observations	1,440	1,440	1,440	1,440

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

vein as the previous regression analyses, mixed-effect regression models are estimated controlling for heteroscedasticity via a random intercept for the cohort and a random slope for the time trend.

RESULT 4.5. *Across both oligopoly experiments, the degree of tacit collusion based on Nash or Walrasian prices and profits is significantly higher in markets with symmetric than in markets with asymmetric firms, everything else being equal.*

Tables 4.15 and 4.16 depict estimates of mixed-effects linear regression models of tacit collusion on symmetry and asymmetry of firms, i.e.,

$$\begin{aligned}\varphi^{E}_{x,k,t} = \beta_0 + \xi_k \\ + \beta_{Asymmetry} \cdot Asymmetry \\ + (\beta_{Period} + \beta_{Period,k}) \cdot t \\ + \epsilon_{k,t},\end{aligned}$$

in triopolies and quadropolies, respectively. For maximum comparability, only the Bertrand treatments are included in the analysis for which there is data with both symmetric and asymmetric firms. In terms of profits, asymmetry is a significant driver of competition, with the degree of tacit collusion being 12 pp to 29 pp lower in asymmet-

TABLE 4.16: *Multilevel mixed-effects linear regressions of tacit collusion on (a)symmetry of firms in quadropolies.*

Covariate	(1) $\varphi_{p/q}^{Nash}$	(2) φ_{Π}^{Nash}	(3) $\varphi_{p/q}^{Walras}$	(4) φ_{Π}^{Walras}
Asymmetry	-0.205^{**}	-0.223^{**}	-0.155^{**}	-0.123^{**}
	(0.088)	(0.097)	(0.066)	(0.054)
Period	-0.003^{***}	-0.002^{**}	-0.002^{**}	-0.001^{**}
	(0.001)	(0.001)	(0.001)	(< 0.001)
Constant	0.527^{***}	0.480^{***}	0.645^{***}	0.708^{***}
	(0.062)	(0.068)	(0.047)	(0.038)
Cohorts	24	24	24	24
Observations	1,440	1,440	1,440	1,440

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

ric than symmetric triopolies and 12 pp to 22 pp lower in asymmetric than symmetric quadropolies. With regard to prices, the degree of tacit collusion is not significantly lower with asymmetry in triopolies, but it is 15 pp to 20 pp lower in quadropolies with asymmetric compared to symmetric firms.

Prominent theories of fairness and equity in the behavioral sciences (e.g., Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000) suggest that cooperation is harder to sustain in asymmetric than in symmetric games, which is in line with the finding here. In an effort to assess whether social preferences in fact account for the effect of asymmetry on tacit collusion, subjects' social value orientation is measured using the Murphy et al. (2011) questionnaire, which is filled out by every participant directly after the oligopoly experiment at the end of a session. Remember that incumbent firms are provided with higher market power than entrants in the asymmetric treatments. A comparison of the continuous social value orientation index reveals no differences between incumbents and entrants or subjects in triopolies and quadropolies. Among the four idealized social orientations of altruistic, prosocial, individualistic, and competitive behavior, the average participant is on the verge of prosocial and individualistic behavior. This finding is further corroborated in a categorical analysis which matches subjects to a single category. According to the classification, 43% of subjects are prosocials, 46% are individualists, and none are altruists or of competitive type—the remaining 11% cannot be assigned due to incomplete questionnaires. Again social orientations are not significantly different between subjects acting as firms of different types or participating in different treatments. Furthermore, social value orientations are neither correlated with the degree of tacit collusion connected to price decisions nor with subjects' total profit in the experiment. In sum, these findings suggest that social orientations cannot explain why asymmetric firms collude less than symmetric firms.

The combined experimental evidence suggests that asymmetry fosters competition between firms in a market considerably and significantly, which is in line with Hypothesis 4.5. Put into context, the effect size of implementing asymmetry in a triopoly or quadropoly by increasing the market power of a single firm is comparable to the number effect on tacit collusion between markets with two and markets with three firms.

4.5 Discussion

This chapter encompasses three different studies: Firstly a meta-analysis of extant experiments on competition in oligopolies; secondly an economic lab experiment specifically designed to validate the findings of the meta-analysis regarding the relationship between the number of firms and the level of competitiveness in a market; and thirdly a further economic lab experiment that extends the previous experiment from horizontally to also vertically differentiated, i.e., asymmetric firms. The individual as well as combined findings, implications, and limitations from these studies are detailed in the following.

The first study is a meta-analysis that provides an overview of the extant literature on number effects in experimental oligopolies and consolidates the results of heterogeneous study designs by measuring tacit collusion based on a set of metrics proposed in the literature. The consideration of these metrics provides a more consistent picture since these measures are independent of the specific experimental design. Their use is thus encouraged for future experimental studies on tacit collusion. Although the meta-analysis supports the view that markets tend to become more competitive as the number of competitors increases, it does not provide support for the prominent belief of a strictly monotonic relationship. While duopolies are more prone to tacit collusion than triopolies and quadropolies, respectively, triopolies are not found to be more collusive than quadropolies, everything else being equal. Hence, no positive effect on competitiveness of four relative to three firms in a market could be found with regard to tacit collusion. Moreover, the meta-analysis demonstrates that even the judgment of the competitiveness of a certain mode of competition (price vs. quantity competition) depends on the point of reference (Nash vs. Walrasian equilibrium). In particular, Suetens and Potters (2007) suggest that Bertrand colludes more than Cournot. However, as the empirical analysis reveals, this holds only with respect to Nash equilibria. Instead, if tacit collusion is measured with regard to Walrasian equilibrium, the opposite holds. This finding is in line with the stronger competition predicted by Nash equilibrium under Bertrand competition than under Cournot competition.

The limitations of this meta-analysis are two-fold. First, the number of experimental studies that investigate number effects, also for triopolies and quadropolies, is rather low and thus the results are based on a small number of observations. Second, the data collected from experimental studies is heterogeneous in quality, i.e., it is either obtained from the authors directly, from tables in the article, or even retrieved from figures. As a consequence, the granularity of the data varies across studies. Data on the level of independent observations from sessions is only provided for half of the studies considered here and hence, intra-study treatment differences are not replicable nor testable for the remaining studies. Since meta-analyses represent a valuable instrument to gain additional insights across heterogeneous studies and to check for the robustness of experimental findings, the author strongly supports that the availability of experimental data is further improved to allow for replicability of study results.

For the second study and in an effort to deal with the short-comings of the meta-analysis, an economic lab experiment is specifically designed to validate the findings of the meta-analysis regarding the relationship between the number of firms and the level of competitiveness in a market. In particular, in the experiment the number of firms is systematically varied between two and four, while additionally considering the two most prominent models of oligopoly competition, Bertrand and Cournot, with horizontally differentiated yet vertically symmetric firms. The results of the experiment largely correspond to the findings in the meta-analysis. In particular, the experiment provides further support for the notion that triopolies are not more prone to tacit collusion than quadropolies. However, this finding largely depends on what is viewed as the relevant theoretical prediction in markets with a different number of firms. As the experiment is especially designed to normalize prices, quantities, and profits between treatments, it is found to be crucial whether Nash equilibrium or Walrasian (competitive) equilibrium is viewed as the appropriate benchmark for the competitiveness of a market, e.g., by a competition authority. In case of the former, tacit collusion between markets with three and markets with four firms is indistinguishable. However, in case of the latter, tacit collusion decreases significantly as the number of firms in the market moves from two to three to four. Note that this finding is a consequence of the specific properties of the model and experimental design in the validation study, which is further illustrated by the opposing finding in the meta-study with respect to the Walrasian-based degrees of tacit collusion.

The central limitation of the experiment is that although it allows for horizontal differentiation among firms, it is assumed that firms' goods are all of equal quality so that they are not vertically differentiated. In comparison to real-world markets, a situation of (almost) symmetric firms is hardly probable. Instead, firms in oligopolistic markets often face a single dominant rival. An apparent example of such a market structure is the mobile telecommunications industry.

Therefore, the third study considers the same model of horizontally differentiated oligopoly competition as the second study, but additionally implements asymmetry among the firms by providing one firm with a higher market power than its rivals through an increased product quality. Since the previous findings clearly suggest that the comparison between three and four firms is crucial to the question of how many competitors are required for effective competition in a market, asymmetry is considered in triopolies and quadropolies only. The key finding is that the previous result holds: There is no significant difference in any of the considered tacit collusion metrics between markets with three or four asymmetric firms. Furthermore, a comparison between markets of the same number of competitors but with either symmetric or asymmetric firms reveals that asymmetry hampers tacit coordination and thus reduces tacit collusion based on profits and (in most cases) prices significantly.

Due to various potential forms of parametrization that result in asymmetry between firms, an obvious limitation of the third study is that there are not generalizable to any type of asymmetry in oligopolistic markets. A similar line of argument applies to how the parametrization strategy depends on the number of firms. More specific, the asymmetry between competitors may be measured in various ways, e.g., by differences in Nash profits or by absolute differences in product quality, and hence it cannot be ruled out that this also affects the assessment of competitive intensity in markets with asymmetric three or four firms.

Altogether, these results bear important implications for merger control and ex ante regulation. Everything else being equal, experimental evidence indicates that a market with three firms may ensure competition just as good as a market with four firms if Nash equilibrium is viewed as the relevant benchmark of effective competition. Instead, if regulatory authorities perceive Walrasian equilibrium as the appropriate theoretical prediction, the evidence is more nuanced. Whereas the meta-analysis reveals similar results as with regard to Nash equilibrium, the oligopoly experiments in the validation studies suggest that markets with four firms may indeed hinder tacit collusion compared to markets with three firms.

However, further research is warranted to assess how this latter finding depends on the specifics of the experimental design. Furthermore, future experimental work on number effects in oligopolies should assess these directly in conjunction with asymmetric market power of firms. In particular, a decrease in the number of competitors in a market, e.g., through a merger, is also likely to affect the horizontal and vertical differentiation of firms' products. For instance, a merger may introduce asymmetry in the market power of the remaining firms and thus not only relax competition due to the decrease in the number of firms but also foster competition.

Furthermore, to a risk-averse regulator not only averages of market outcomes may be of interest. Instead, authorities may also take into account the effect the number of firms and regulatory institutions in general have on the variance of the expected competitive intensity in order to minimize the possibility of tacit collusion. However, with the given samples in the meta-analysis as well as in the validation studies, there are no significant differences in this regard.

A limitation common to all parts of this chapter is that the nonexistence of an effect can hardly be inferred from the insignificance of a covariate's estimate. This implies that the insignificant difference in tacit collusion between markets with three firms and markets with four firms can only be judged with respect to the specific samples used in the meta-analysis and validation studies. Note that also retrospective power analysis, which is frequently advocated in need for an interpretation of insignificant effects, cannot solve this problem since "for any test the observed power is a 1:1 function of the p value" (Hoenig and Heisey, 2001, p. 20). As a further limitation to all studies, competition in experimental Bertrand and Cournot oligopolies is merely considered with exogenously symmetric or asymmetric firms but no endogenous merger formation is analyzed. Furthermore, neither the experiments considered in the meta-analysis nor the experiments in the validation studies allow for investments in order to increase market size, which arguably play an important role in most industries that are characterized by an oligopolistic market structure . Depending on these characteristics, the conclusion of what represents a *necessary number* of competitors may differ significantly. But even if "case-by-case analysis implies that there is no 'magic number'" (Walle and Wambach, 2014, p. 10), the findings reported here point to systematic effects with regard to tacit collusion that should be given careful consideration by competition and regulatory authorities when assessing the question of how to achieve and safeguard effective competition in a market.

Chapter 5

Price Discrimination and Tacit Collusion

WHEN firms sell their products in more than one (geographic) market, they may either charge the same price across markets, i.e., employ uniform pricing, or they may charge differentiated prices according to the specific market conditions, i.e., engage in price discrimination. According to conventional wisdom, firms should price discriminate whenever possible, due to asymmetric costs or differences in demand elasticity across markets. Although some exceptions to this conventional wisdom are identified (Dobson and Waterson, 2008), the existing literature agrees that price discrimination and uniform pricing generally yield different market outcomes when there are differences in the market conditions. On the contrary, there is currently no theory that predicts differences in market outcomes due to the two pricing regimes when there are *no* differences across markets.

This study suggests a new explanation for the observed differences that relates to behavioral aspects rather than demand- or supply-side effects. The remainder of this chapter is organized as follows. Section 5.1 introduces the issue of price discrimination vs. uniform pricing and summarizes the key results of the experimental analysis. Its design and procedures are reported in Section 5.2. In Section 5.3, the experimental results are derived and contrasted to theoretical predictions. Finally, Section 5.4 discusses the results and their policy implications while also acknowledging the study's limitations.

5.1 Motivation

This note demonstrates in a laboratory experiment that price discrimination leads to systematically higher average prices than uniform pricing even when firms and markets are symmetric in. More specific, in a symmetric homogeneous Bertrand duopoly, a new explanation is suggested for differences in economic outcomes between the two pricing regimes that relates to their impacts on tacit collusion, rather than cost or demand asymmetries. Thereby, this study is the first experimental investigation of tacit collusion in an industry scenario that specifically considers the possibility to price discriminate as a treatment variable.

In this context, the findings also relate to the literature on mutual forbearance (Edwards, 1955), which discusses the collusive effects of multimarket contact. Whereas under price discrimination the underlying markets remain, in principle, independent, uniform pricing creates a bond between the markets that effectively makes them one market. Porter (1980) argues that firms meeting in several markets (price discrimination) may find it easier to tacitly collude than firms meeting only in one market (uniform pricing). This is because every colluding firm anticipates that a price deviation in any *one* market will be punished by price cuts in *all* markets by the other firms. However, Bernheim and Whinston (1990) criticize this view and argue that a rational price deviation should never occur only in one, but in all markets simultaneously, thus rendering the multimarket retaliation as no more effective than the retaliation in a single market environment. Moreover, the authors formally establish an irrelevance result, which states that multimarket contact cannot facilitate tacit collusion between symmetric firms meeting in symmetric markets.[1] Hence, the findings of this experimental analysis can also not be explained by the mutual forbearance theory. Instead, Chapter 6 offers a new theoretical explanation in the context of multimarket contact that also relates to price discrimination.

[1] In their model, Bernheim and Whinston consider an infinite time horizon, whereas a finite time horizon is considered here. However, note that collusion is harder to sustain with a finite time horizon (Harrington, 1987) and thus, the irrelevance result remains to hold in the present context.

5.2 Experiment

The laboratory experiment explicitly addresses the issue of price discrimination and uniform pricing across (geographic) markets. It thereby considers symmetric markets and firms in an effort to depict an industry scenario for which existing theories predict no differences with respect to tacit collusion in prices.

5.2.1 Design

Consider an industry with two distinct markets, M_1 and M_2, in which two symmetric, price competing firms, $i \in \{A, B\}$, offer a homogeneous product for T periods, respectively. The supply of one unit of the product to either market implies the same marginal cost to each firm. Denote i's price for market $X \in \{M_1, M_2\}$ by p_i^X. Then, according to Bertrand competition, the demand of firm i in market X in each period is either the full market size if firm i offers at a lower price than its competitor, half the market size if firms offer the same price (below the consumers' homogeneous willingness to pay), or zero if firm i offers at a higher price than its rival. Consequently, i's total profit in each period is given by the sum of profits in both markets. Then, the only difference between price discrimination and uniform pricing is that prices have to be equal across markets in the latter case.

It is well known that the unique strict Nash equilibrium of the above Bertrand stage game is marginal cost pricing in both markets irrespective of whether firms can discriminate prices or not. Further, under reasonable assumptions about the equilibrium concept of the finitely repeated Bertrand game, the above unique equilibrium of the Bertrand stage game is also the unique price equilibrium of the repeated Bertrand game. For example, Farrell and Maskin (1989a) show that the price equilibrium of the Bertrand stage game is the unique weakly renegotiation proof price equilibrium of the repeated Bertrand game. It is also the unique subgame perfect equilibrium. In conclusion, the theoretical prediction of both pricing scenarios is equivalent in terms of equilibrium prices and hence, in terms of profits and consumer surplus.

The experiment is comprised of $T = 10$ repeated interactions (periods) of the Bertrand stage game. Profits are accumulated over the periods. For a more direct relation between reward signals and participants' decisions, the model is parametrized using EUR instead of an experimental currency unit. Marginal costs are set to EUR 0.30. Each market has 10 consumers with a willingness to pay of EUR 0.50 each. The minimum price increment is EUR 0.01. Treatments differ only with respect to whether participants can engage in price discrimination (PD) or are restricted to uniform pricing (UP) between the two markets. As noted above, the unique strict Nash equilibrium entails that both firms choose prices EUR 0.31 for both markets (treatment PD) or EUR 0.31 as the uniform price (treatment UP) during all periods.

5.2.2 Procedures

For each treatment condition, there are twelve cohorts with four subjects each, i.e., 96 participants in total. The experiment is designed between subject, i.e., participants are exclusively assigned to one treatment condition. In total, each subject participates in three *rounds*. Each round consists of ten consecutive repetitions of the Bertrand stage game, which are commonly referred to as periods. Within each round, participants are matched according to a fixed partner matching. However, after each round, participants are matched with a new partner that they did not previously encounter. Thus, each subject plays with all other participants of the same cohort for exactly one round (i.e., for ten periods). Since firms are designed to be symmetric, subjects are not labeled in any order. Instead, a firm's current partner is referred to as *the other firm*.

Every effort is made to ensure salience in the experiment. Participants are equipped with a calculator and the experimental software provides a forecast tool for demand and profit in the next round, given a subject's expectation of both firms' prices. Moreover, a history of previous prices within the same round and the same group is pro-

TABLE 5.1: *Summary statistics on subject and group level.*

Treatment	Variable	N	Mean	Std. dev.	Min.	Max.
UP	Average price	96	40.45	4.75	31.80	50.00
	Average market price	48	38.74	4.92	31.10	49.70
	Subject's profit	96	873.96	532.04	10.00	2,140.00
PD	Average price	96	44.50	5.06	34.45	50.00
	Average market price	48	42.89	5.84	33.10	49.60
	Subject's profit	96	1,289.17	625.27	255.00	2,240.00

vided. However, there is no exchange of information or interaction between subjects in different groups, i.e., no population feedback (Bruttel, 2009). To avoid budget effects, the earnings of only one round are paid out. Participants throw a dice to determine which of the last two rounds is paid out to them. The first round, which is declared a practice round, is not relevant for the final payoff and thus, it is not considered in the subsequent statistical analysis. The experimental instructions provided to the subjects cover all stated design features of the experiment, including the number of periods and rounds as well as how the profits and their final payment would be determined.[2]

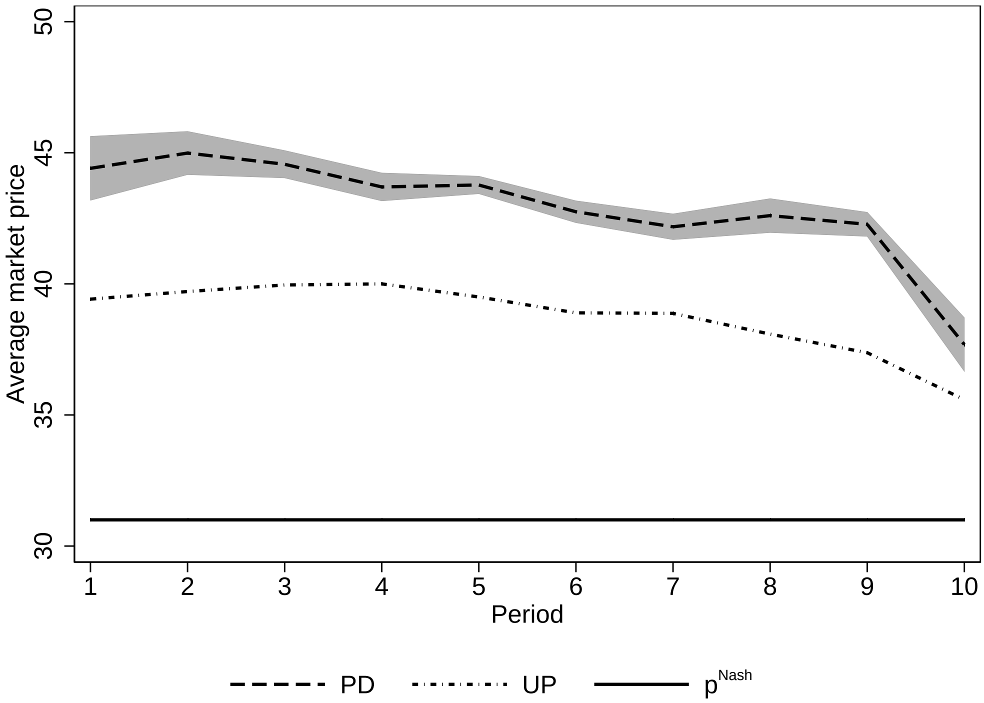
The experiment is computerized using *z-Tree* (Fischbacher, 2007). All sessions were run at the Karlsruhe Institute of Technology in Karlsruhe, Germany, in May and June 2012, and April 2013. Participants were recruited via the *ORSEE* platform (Greiner, 2015). Subjects were exclusively students of economic fields. None of the sessions lasted more than one hour. No initial budget was given to the participants. Subject's average monetary earning was EUR 10.86.

5.3 Results

The experimental data is aggregated by computing the average market price over all ten periods of a round. Note that under price discrimination the average is taken also across markets. At the group level an observation is uniquely identified by treatment (UP or PD), cohort (1 to 12), group (1 to 2), and round (1 or 2). Thus, there are 48 ob-

[2]Experimental instructions for the PD treatment are provided in Appendix C.3.

FIGURE 5.1: *Average market price over time across treatments. The boundaries of the gray corridor depict the average of minimum and maximum market prices across markets for the PD treatment.*



servations for each of the treatments. However, note that due to the matching scheme, observations on the group level from a single cohort are not statistically independent. This is controlled for by means of a hierarchical mixed-effects regression model and by considering only the cohort-averaged market prices, respectively. Prices in each group and round over time for both treatments are provided in Appendix D.3.

Table 5.1 reports descriptive statistics with respect to a subject's average price and profit and a group's average market price as a measure for tacit collusion. Moreover, Figure 5.1 shows the average market price for both treatments over the ten periods and allows to contrast them to the equilibrium price. Both table and figure indicate two notable deviations from the theoretical prediction. First, prices have a positive offset from marginal costs, i.e., from the theoretical equilibrium. This is in line with previous experimental results on Bertrand competition (e.g., Engel, 2007). Second, there seem to

TABLE 5.2: *Multilevel mixed-effects regression of the average market price.*

Covariate	Coef.	Std. err.	z	p	95% conf. interval	
PD	4.152	1.612	2.58	0.010	0.992	7.312
Round	1.344	0.834	1.61	0.107	-0.290	2.977
Constant	38.068	1.214	31.36	< 0.001	35.688	40.447

96 observations clustered in 24 cohorts.

be differences in market prices and hence in tacit collusion between the treatments. On average, the market price is EUR 0.42 (10.71%) higher for the PD treatment.

In order to test for differences in the average market price between treatments, consider the following two-level linear random-intercept model, which controls for the potential dependence of observations within one cohort:

$$\overline{p}_{k,g,Round} = \beta_0 + \zeta_k + \beta_{PD} \cdot PD + \beta_{Round} \cdot Round + \epsilon_{k,g,Round},$$

where $\overline{p}_{k,g,Round}$ is the average of market prices over a round's ten periods in group g in cohort k, PD is the treatment dummy, $Round$ is a dummy for first or second payout relevant round, and ζ_k is the error component shared between observations of the same cohort. Note that this regression model does not constitute a panel analysis in the classic sense as it uses aggregate data from ten repeated interactions in each group and round before rematching.

Table 5.2 reports the results, which show that the average market prices are significantly higher for the PD treatment, whilst the round has no significant impact, i.e., there is no learning effect. Also by a one-tailed non-parametric Mann-Whitney-U test on cohort averages, the market price is significantly higher under price discrimination ($z = 1.67, p = 0.047$). These findings suggest that the possibility to differentiate prices between geographic markets facilitates tacit price collusion more than uniform pricing. Hence, consumers' surplus decreases in the transition from uniform pricing to price discrimination.

5.4 Discussion

Contrary to existing theory, the laboratory experiment finds that tacit collusion is significantly higher under price discrimination than under uniform pricing. This result offers the insight that even under symmetric market conditions the mere possibility to be able to engage in differential pricing may facilitate collusion and thus, lead to higher prices than price uniformity.

This result bears important policy implications. For example, competition policy may investigate more closely the impact on competition when firms switch from uniform pricing to discriminatory pricing. Furthermore, whether price discrimination should be allowed for different geographic markets is currently under consideration by many national regulatory authorities in the telecommunications domain. Currently, in many countries telecommunications operators are bound by a universal service obligation, which usually includes a uniform pricing constraint. In order to stimulate investments in so-called next generation networks, regulators are considering to move towards a geographically segmented regulation, which would imply the possibility for price discrimination. For example, recently the German legislator has explicitly enacted that a differentiation of retail prices in next generation networks is not abusive per se. As the experimental results show, such a relaxation of the pricing constraints may also have unexpected consequences on consumers' surplus and should therefore be closely scrutinized by regulators.

Of course, these results are subject to several limitations. Although the competition model is believed to be fairly robust to alternative theoretical explanations (e.g., other-regarding preferences, heterogeneous products), only price competition is considered. Thus, it might be worthwhile to investigate whether the empirical results would also hold in the context of quantity (Cournot) competition. Future work may also address the role of elastic demand, i.e., heterogeneous willingness to pay among consumers, which may alter the collusive strategy. Likewise, it would be interesting to see whether the results carry over to settings in which there are more than two firms or markets.

Chapter 6

Multimarket Contact and Tacit Collusion

CONGLOMERATE firms operate on multiple geographic or product markets and are thus likely to meet a rival conglomerate firm in more than one market. Such multimarket contact of conglomerate firms is long-since suspected to facilitate tacit collusion. Sixty years ago, Edwards (1955) formulated the *mutual forbearance hypothesis* which suggests that multimarket contact reduces the competitive intensity, because firms meeting in multiple markets fear to trigger a price war across all markets if they undercut their rivals in any one market. In this chapter, a new theoretical explanation based on behavioral research is suggested for why multimarket contact of firms, or likewise, their organizational centralization, may facilitate tacit collusion between conglomerate firms.

The theory is motivated by the experimental analysis in Chapter 5, which shows that price discrimination across two identical geographic markets in a duopoly of conglomerate firms facilitates tacit collusion more than if firms are subject to a uniform pricing constraint obliging each of them to commit to a single price in all markets. Obviously, in this situation of symmetric markets, price discrimination and multimarket contact or uniform pricing and single market contact are effectively identical, respectively. In this chapter, based on this finding, a theory of price signaling is formulated that explains why multimarket contact allows for easier coordination between conglomerate rivals. Subsequently, this theory is successfully validated in a further experimental environment which is specifically designed so that existing theories of mutual forbearance fail to explain differences in cooperation tendencies between multimarket contact and single market contact.

The remainder of this chapter is structured as follows. Section 6.1 highlights the importance of multimarket research, briefly reviews the extant empirical and theoretical literature, and sketches the price signaling theory as well as its managerial and regulatory implications. Section 6.2 is dedicated to previous experimental work on multimarket contact. The theoretical model of price signaling under single and multimarket contact is formalized in Section 6.3 and the economic laboratory experiment designed to validate the assumptions of the model is reported in Section 6.4. In Section 6.5, experimental results are assessed with regard to the price signaling theory in order to investigate the theory's predictive power for the experimental price setting behavior. Finally, Section 6.6 discusses the results, derives implications for management and regulatory policy, and lists the study's limitations.

6.1 Motivation

Until today, the mutual forbearance hypothesis stimulates considerable empirical research on multimarket contact in several industry contexts, including manufacturing (Scott, 1982), airlines (Evans and Kessides, 1994; Gimeno and Woo, 1996; Baum and Korn, 1999; Ciliberto and Williams, 2014), cement (Jans and Rosenbaum, 1997; Ghemawat and Thomas, 2008), telecommunications (Parker and Röller, 1997), hotels (Fernandez and Marin, 1998), banking (Haveman and Nonnemaker, 2000; Koçak and Özcan, 2013), software (Young et al., 2000), footwear (Audia et al., 2001), media (Waldfogel and Wulf, 2006), insurances (Greve, 2008), and personal computers (Kang et al., 2010). A recent and comprehensive overview is provided by Yu and Cannella (2013). By and large, this research confirms a relationship between multimarket contact and tacit collusion. In regard to innovation activities by conglomerate firms, Alcácer and Zhao (2012) find that internal linkages across geographical locations may spur knowledge appropriation and that conglomerate firms reinforce such interdependency if they encounter competitors at a location, but not in the presence of a firm that merely engages in the same technological field. With respect to market entry (Ghemawat and Thomas, 2008) and market exit (Boeker et al., 1997), empirical findings show that firms seek multimarket contact to their rivals. Thus, firms are more likely to enter (geographic or product) markets in which their rivals are present and are in turn less likely to leave those markets, which suggests a favorable competitive environment under multimarket contact. Also with regard to market entry and exit, Koçak and Özcan (2013) study the banking industry and find support for an inverted-U-shaped relationship between geographic market entry and multimarket contact suggesting that firms choose to enter markets with some competitors but refrain from doing so in markets with many rivals. Kalnins (2004) find that firms in the franchised fast-food industry try to avoid divisional multimarket contact among their franchisees in case they want to induce intra-firm competition between divisions. Finally, regarding antitrust policy, Scott (2008) reviews conglomerate and horizontal mergers over different geographic areas and makes a strong case for considering multimarket contact as a potential anti-competitive harm.

Despite the empirical evidence, the theoretical underpinning *why* multimarket contact facilitates tacit collusion was for a long time under question. Bernheim and Whinston (1990, p. 3) highlight that Edwards' (1955) seminal reasoning is logically flawed, because "once a firm knows that it will be punished in every market, if it decides to cheat, it will do so in every market." In this vein, Bernheim and Whinston (1990) formally establish an *irrelevance result*, which states that multimarket contact may not explain mutual forbearance in situations where identical firms experiencing identical and constant returns to scale meet in identical markets. Therefore, to date "most researchers assume that mutual forbearance requires asymmetric markets, rivals, and competitive positions" (Yu and Cannella, 2013, p. 77). This view is challenged by Spagnolo (1999) as he shows that the irrelevance result depends on the assumption that firms' static objective functions are non-concave, i.e., that firms purely maximize expected profits. This is likely not to be the case whenever managerial objectives diverge from pure profit maximization. Moreover, also Matsushima (2001) builds on Bernheim and Whinston's seminal theoretical work and argues that the irrelevance result breaks down if firms cannot perfectly monitor each others' actions. Similar results are derived by Anand and Mendelson (1997), who suggest that in a world of imperfect information conglomerate firms are faced with a trade-off between maximizing coordination by centralization and maximizing exploitation of local knowledge by decentralization. In the extremes, the decision authority lies with the (conglomerate firm's) headquarter alone in the former case, whereas local branches would behave as independent (single product or market) firms in the latter case. On these grounds, Chang and Harrington (2000, 2003) develop an agent-based simulation in which conglomerate multimarket firms choose between centralization and decentralization strategically. Their simulation suggests that decentralization leads to higher profits than centralization under uncertainty and with asymmetric markets. However, if markets are identical and in the presence of perfect information, both organizational structures perform equally well—an observation akin to the irrelevance result.

In this study, a new theoretical explanation is offered for why multimarket contact, or likewise, organizational centralization, may facilitate tacit collusion between conglomerate firms. The explanation rests on two assumptions, which are scrutinized later. First, it is assumed that firms are able to signal their intention for collusive play solely through their price setting behavior by sharply raising their price in a market. Clearly, in the short run such price signaling will yield opportunity costs, as the signaling firm experiences a sharp decline in demand. However, if the other firm recognizes the collusive price signal and reacts to it accordingly, i.e., by raising its price as well, then tacit collusion can emerge in this vein without the need for explicit communication. Second, the main conjecture is that such price signaling can be conducted more efficiently in an environment where the same competitors meet in several markets rather than in one single market. In this context, the distinct strategic feature of multimarket contact is that firms are able to discriminate prices across the markets they operate in and thus they can send more nuanced price signals. To exemplify this point, consider a multimarket contact situation in which two conglomerate firms meet in two identical (e.g., geographically distinct) small markets with U consumers each, as opposed to a single market contact situation in which (non-conglomerate) firms meet in one large market with $2U$ consumers, everything else being equal. Hence, under multimarket contact a firm can send a price signal in only one small market which yields comparably less opportunity cost than if the signal is sent in both small markets, or equivalently in one large market as under single market contact. Because the same competitors meet in both markets under multimarket contact, a price signal that is sent in only one market may be just as effective in raising prices in *all* markets as a signal which is actually sent in all markets. In particular, the herein suggested price signaling theory shows that, in consequence, multimarket contact expedites the tacit process through which a collusive state is reached—provided that the probability that a firm's signal in any one market will evoke a reaction by the rival firm in the other market is sufficiently high. In other words, given the assumptions, then at any point in time, a collusive state is more likely to be reached under multimarket contact than under single market contact. Moreover, in the present example a firm incurs less opportunity costs to reach a collusive state under multimarket contact. Therefore, in conclusion, it is suggested that multimarket contact enables firms to signal more efficiently than under single market contact.

Note that this argument does not depend on the firms' asymmetry (Bernheim and Whinston, 1990), concave objective functions (Spagnolo, 1999) or imperfect monitoring (Anand and Mendelson, 1997; Matsushima, 2001; Chang and Harrington, 2003) as in previous literature. It only depends on the two assumptions, which are assessed and validated by means of an economic laboratory experiment. Hence, the results bear not only important insights for strategic management by demonstrating the strategic effects of decentralization of decision authority, but also for competition policy by highlighting that limiting firms' possibilities to engage in price signaling can effectively mitigate the emergence of tacit collusion. This is particularly relevant if tacit collusion is suspected among firms that meet in several geographically distinct, but otherwise relatively homogeneous markets (such as telecommunications or airline markets), where, for example, a uniform pricing constraint could therefore be an effective tool to render multimarket price signaling ineffective, and hence, may undermine the process that establishes tacit collusion.

6.2 Related experimental literature

As tacit collusion lacks explicit agreements, it is difficult to obtain robust empirical evidence for its existence in field data. Economic laboratory experiments, however, allow to specifically prohibit communication between firms so that tacit collusion can be measured. In the context of multimarket contact, early experimental studies are mainly motivated by the fact that empirical studies can hardly delineate the boundaries of individual geographic or product markets when testing the mutual forbearance hypothesis (see Kang et al., 2010, for a discussion of approaches to measure multimarket contact in empirical data). Naturally, such identification problems do not exist in economic laboratory experiments, where all focus variables are known and controlled by the researcher.

The first experimental investigation of the mutual forbearance hypothesis is owed to Feinberg and Sherman (1988) who find some support for the hypothesis in a within-subject design. Phillips and Mason (1992, 1996) confirm Bernheim and Whinston's theoretical prediction for asymmetric markets in the laboratory. In a large-scale laboratory experiment, Güth et al. (2015) consider horizontally differentiated quantity competition on two asymmetric markets and control for conglomeration and multimarket contact independently. Their results do not ascertain previous experimental findings. Instead, both conglomeration and multimarket contact are found to foster competition compared to single market contact with non-conglomerate firms. Comparably, in a recent experiment with a random termination rule, Yang et al. (2015) find that cooperation rates are lower when subjects play two asymmetric prisoner's dilemma games simultaneously instead of playing each game separately. Therefore, taken together, experimental findings on multimarket contact are mixed.

This work combines two hitherto separate strands of literature: The literature on multimarket contact and the literature on price signaling. Unfortunately, the term *signaling* is used ambiguously in the economic literature. Here it refers to *(price) signaling* exclusively as implicit communication through price setting behavior. This is in line with the extant experimental literature (Hoggatt et al., 1976; Durham et al., 2004; Davis et al., 2010). However, note that in a similar context signaling is also used to refer to explicit communication in the form of cheap talk about future prices (Grether and Plott, 1984; Holt and Davis, 1990; Cason, 1995; Cason and Davis, 1995), which is not considered in this study. More generally, signaling is associated with signaling games (Spence, 1974) in which players use signals to reveal their type (see Srinivasan, 1991, for an application in the context of multimarket contact). The pivotal conjecture of this study is that price signaling in terms of the first definition is facilitated by multimarket contact because firms meeting in several geographic or product markets can set different prices (i.e., distinct price signals) on each market. This allows conglomerate multimarket firms to signal prices more efficiently in contrast to non-conglomerate single market firms, which only have a single price (i.e., one price signal) at their disposal. Although novel in the context of multimarket contact, the conjecture on price signaling and tacit collusion is not new per se. However, previous research could not find clear evidence on the effectiveness of price signaling on the emergence of tacit collusion (Potters and Suetens, 2013).

Initial observations of signals in repeated price competition experiments are provided by Hoggatt et al. (1976) and Friedman and Hoggatt (1980). Plott (1982) discusses these early attempts to model the effect of signals and conjectures that price signaling occurs, but "it happens so infrequently [...] that the implications cannot be ascertained" (Plott, 1982, p. 1517).[1] Hoggatt et al. (1976) reports on oligopoly experiments with repeated price decisions. They differentiate between pulses ("sequence of two or three successive price changes which sum to zero", Hoggatt et al. (1976, p. 263)) and steps ("price change of unusually large magnitude", Hoggatt et al. (1976, p. 263)). Only the latter are found to have an effect on the price development and to be more probable in a positive direction if a firm's price is low, but in turn more probable in a negative direction if a firm's price is high, thus also indicating downward signaling. However, all these effects are found to be temporary and do not evolve in an overall effect on tacit collusion.

[1]Surprisingly, price signaling is addressed again not until 20 years later.

Comparably, in an auction experiment, where information about losing bids is a treatment variable, Dufwenberg and Gneezy (2002) find prices to be supra-competitive if bidders are informed about the losing bids in previous periods. They hypothesize that this is due to signaling behavior during repeated interaction but do not formally test their conjecture. Durham et al. (2004) observe signaling behavior in pricing decisions in an extensively repeated posted offer market experiment. According to Durham et al. (2004, p. 155), "a price signal is defined as any price submitted by any firm that is greater than or equal to the lowest posted price that failed to attract buyers in the previous period". Based on this definition, frequent signaling activity is detected in all experimental treatments, especially in those with sunk fixed cost. Price signals are found to elicit higher prices in the subsequent period, yet a test for an overall effect of price signaling on tacit collusion is not reported. Furthermore, the presence of fixed cost has a significantly positive effect on prices. As the presence of fixed cost assures firms a loss if they play the Nash equilibrium, subjects clearly face a particular incentive to collude. It cannot be ruled out that this accounts for the effect of price signals on prices in the immediately following period. Davis et al. (2010) explicitly address the open issue of a direct effect of price signaling on tacit collusion. To this end, past price choices (baseline treatment) are combined with non-binding price announcements (forecast treatment). The latter are based on cheap talk which, as noted above, is also sometimes referred to as price signals in the literature.[2] Recall that this study refers to price signals based on past price choices that do *not* require any means of explicit communication. Davis et al. consider a market with Bertrand-Edgeworth competition among three firms. The experiment comprises two successive sequences. Firms first play the baseline treatment and, after regrouping, they play the forecast treatment in which firms are additionally provided with the other firms' expectations on the maximum price in the next period. Thereby, a price signal is defined as a firm's price that is higher than its forecast on the rivals' price choices. Hence, the baseline treatment serves as a benchmark and price signaling is limited to the forecast treatment. In this market structure, market prices are supra-competitive throughout but not different between the two treatments. More-

[2]Cason and Davis (1995) study non-binding price communication in a multimarket environment. However, they do not compare their findings to a single market context.

over, there is frequent signaling activity in the forecast treatment raising prices in the immediately following period but no overall effect on tacit collusion.

Finally, in a duopoly experiment with symmetric firms and markets, Horstmann and Krämer (2013) find that firms compete more fiercely under a uniform pricing constraint across markets (i.e., under single market contact) than if firms are allowed to differentiate prices (i.e., under multimarket contact) as reported in Chapter 5. However, the study does not consider price signaling, and hence, cannot provide evidence *why* multimarket contact facilitates tacit collusion.

6.3 Price signaling under multimarket contact

The model of price signaling, which is formally developed in the following, provides an explanation why and under which conditions multimarket contact can facilitate tacit collusion. The model rests on two fundamental assumptions: First that, without the need for explicit communication, firms can induce the rival firm to raise its price by sending a price signal. As reviewed above, there exists evidence in the experimental literature that price signaling has a positive effect on prices at least in the short run. Second, it is assumed that under multimarket contact price signals that are sent in any one market may also raise the price in the other market(s).

For expositional clarity, the theoretical model is described in the most simplistic market setting under which alternative explanations why multimarket contact facilitates tacit collusion (cf. Bernheim and Whinston, 1990; Spagnolo, 1999; Matsushima, 2001) can be ruled out. Consider a setting in which two identical firms $i \in \{A, B\}$ each offer a homogeneous product with marginal cost of c. Under single market contact, the firms meet in only one product market $X \in \{M_1\}$ with $2U$ consumers. By contrast, under multimarket contact the firms meet in two identical (e.g., geographic) markets $X \in \{M_1, M_2\}$ with U consumers each. Under the assumption of repeated Bertrand competition and simultaneous decision-making, firm i sets a price $p_{i,t}^X$ in period $t = 1, ..., T$ in market X

and receives the full market demand if and only if it offers the lowest price in market X.[3] Otherwise, if both firms offer the same price, the market demand is split equally. As marginal cost pricing constitutes the unique strict Nash equilibrium of the Bertrand stage game (neglecting price increments), this is also the unique subgame perfect equilibrium (Selten, 1975) and the unique weakly renegotiation-proof equilibrium (Farrell and Maskin, 1989b) of the finitely repeated Bertrand game. To convey the working of the theory, consider the following example. Assume that in $t-1$ firm A sets the current market price (lowest price) in market X, i.e., $p^X_{t-1} \equiv \min\{p^X_{A,t-1}, p^X_{B,t-1}\} = p^X_{A,t-1}$ and that this price is above marginal cost. Therefore, according to the logic of Bertrand competition, firm A receives full market demand. Evidently, in this situation, assuming that firm A maintains this price in period t, the myopic best response by firm B in period t is to undercut the rival's price slightly or, if this would incur a loss, to match the rival's price. In any case, from a game-theoretical point of view firm B is not expected to raise its price in period t. However, from a long-term strategic point of view, such price setting behavior may occur nevertheless, and if it does, it is considered to be a price signal by which firm B wishes to implicitly communicate to firm A that it wants to coordinate on a higher market price, rather than to engage in a price war. More formally, firm i is said to send a price signal in period $t > 1$ if its price $p^X_{i,t}$ is greater than the rival's price $p^X_{-i,t-1}$ in the previous period.

On the grounds of this price signal definition, a simple iterative price setting strategy is suggested which demonstrates how multimarket contact can facilitate tacit collusion. For simplicity, consider the following additional assumptions about each firm's price setting behavior:

(i) When firm i seeks to communicate its collusive intention in period t through a price signal in market X, it will do so by setting the JPM price, i.e., $p^X_{i,t} = p^{JPM}$.

[3] For simplicity, price competition with homogeneous goods is assumed. However, the price signaling theory is not limited to this setting and can be readily adapted to quantity competition as well as differentiated goods.

(ii) When firm i reacts *collusively* in period t to a price signal sent by the rival firm $-i$ in period $t-1$, then it will do so by matching firm $-i$'s price signal, i.e., $p_{i,t}^{X} = p^{JPM}$.

(iii) On each market, the probability that a firm sends a collusive price signal in period t is $\sigma \in (0,1)$. Likewise, on each market the probability that a firm reacts collusively to a signal in the same market X is $\psi^{X} \in (0,1)$ and in the other market $-X$ is $\psi^{-X} \in (0,1)$.

(iv) Whenever firm i in period t neither sends nor collusively reacts to a signal, it will set the myopic best response price, i.e., $p_{i,t}^{X} = BR(p_{-i,t-1}^{X})$.

(v) Whenever both firms have reached a collusive state in period $t-1$ in market X, i.e., $p_{i,t-1}^{X} = p_{-i,t-1}^{X} = p^{JPM}$, then they will maintain that state in the subsequent period, i.e., $p_{i,t}^{X} = p_{-i,t}^{X} = p^{JPM}$.

To fix ideas, suppose that each firm i sets a random price $p_{i,1}^{X} < p^{JPM}$ in each market in the first period $t = 1$. The *price setting strategy* for $t > 1$ suggested above can then be written in pseudocode as follows:[4]

// Set best response price by default

for each market X **do** set $p_{i,t}^{X} = BR(p_{-i,t-1}^{X}) = \begin{cases} p_{-i,t-1}^{X} - \varepsilon & \text{if } p_{-i,t-1}^{X} > c, \\ c & \text{otherwise.} \end{cases}$

end for

// If signal is received, instead of best response, possibly react collusively

if other firm $-i$ has sent signal $p_{-i,t-1}^{X} = p^{JPM}$ in period $t-1$ in at least one market X **then**

  if other firm $-i$ has sent signal $p_{-i,t-1}^{X} = p^{JPM}$ in period $t-1$ in all markets **then**

    for each market X **do** with probability ψ_{X}, set $p_{i,t}^{X} = p^{JPM}$.

    end for

  else

    // Only possible for conglomerate firms

    in market X in which signal was received, with probability ψ^{X}, set $p_{i,t}^{X} = p^{JPM}$.

    in market $-X$ in which signal was *not* received, with probability ψ^{-X}, set $p_{i,t}^{-X} = p^{JPM}$.

  end if

else

[4] Let $\varepsilon > 0$ denote the smallest possible price increment.

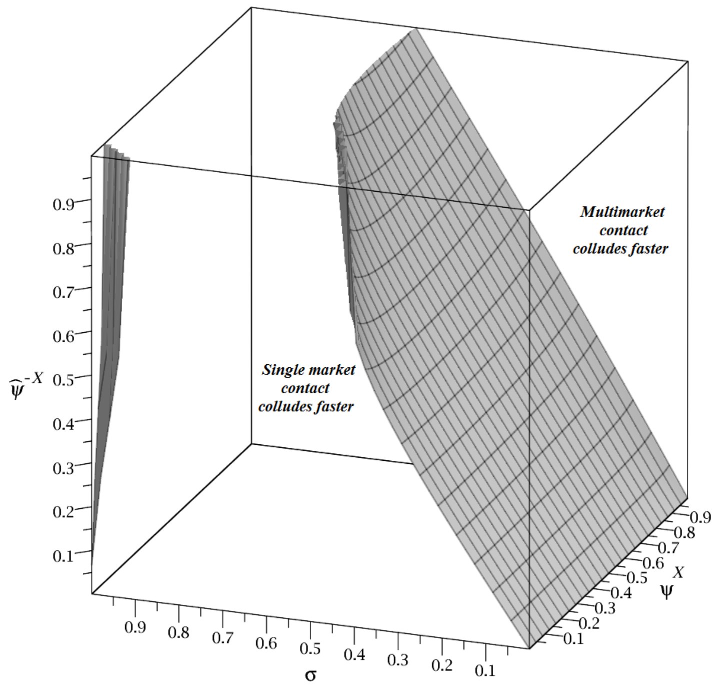
15: *// If no signal was received, instead of best response, possibly send price signal*
16: **for** each market X **do** with probability σ, set $p_{i,t}^{X} = p^{JPM}$.
17: **end for**
18: **end if**
19: *// Maintain collusive state*
20: **for** each market X **do**
21: **if** $p_{i,t-1}^{X} = p_{-i,t-1}^{X} = p^{JPM}$ in period $t-1$ in market X **then** set $p_{i,t}^{X} = p^{JPM}$.
22: **end if**
23: **end for**

Note that the price setting strategy does not distinguish between a single market and a multimarket environment except for the case handled in lines 10–12, which applies only to conglomerate firms that operate on both markets. In particular, line 12 captures the assumption that multimarket contact allows firms to infer a collusive intention for all markets from a price signal that was sent in only one market. The extent of this assumption is captured by ψ^{-X}. In the extreme, if $\psi^{-X} = 0$, a signal sent in market X will never evoke a price increase in market $-X$ and thus, multimarket contact is behaviorally not different from single market contact. In fact, such an industry scenario would not be distinguishable from an industry of conglomerate firms competing against each other on several markets but without ever meeting the same rival on more than one market.

The price setting strategy described above triggers a Markov price process, as a firm's price in t depends only on prices in $t-1$ and the time-independent probabilities $\sigma, \psi^{X}, \psi^{-X} \in (0,1)$. Moreover, it is easy to see that the collusive state $p_{i,t}^{X} = p_{-i,t}^{X} = p^{JPM}$ is an absorbing state, which can be reached from any other state with positive probability. Thus, independent of the market structure, the price process will eventually reach a collusive steady state, provided T approaches infinity. It is therefore interesting to analyze under which conditions multimarket contact will converge to the collusive outcome faster than single market contact.

PROPOSITION 6.1. *Under the specified price setting strategy the collusive outcome will be reached in fewer periods under multimarket contact if and only if the probability to re-*

FIGURE 6.1: *Critical cross-market signal reaction probability above which tacit collusion is reached faster under multimarket contact than under single market contact.*



act to a price signal on the other market is large enough, i.e., iff $\psi^{-X} > \widehat{\psi}^{-X}(\sigma,\psi^{X})$ *with* $\sigma \notin \left[\underline{\sigma}(\psi^{X}),\overline{\sigma}(\psi^{X})\right]$.

While the detailed proof is relegated to Appendix B.2, an intuition for this result is provided here. Note that ψ^{-X} only applies under multimarket contact, and thus, given the respective absorbing Markov processes under single market contact and multimarket contact, a critical value $\widehat{\psi}^{-X}(\sigma,\psi^{X})$ may be calculated for which the expected numbers of periods needed to reach the collusive outcome (on all markets) under each market scenario coincide. The surface depicted in Figure 6.1 is a graphical representation of this critical value.

When the probability of sending a signal is not too high, i.e., $\sigma < \underline{\sigma}(\psi^{X})$, then $\widehat{\psi}^{-X}(\sigma,\psi^{X}) < 1$ so that for any $\psi^{-X} > \widehat{\psi}^{-X}(\sigma,\psi^{X})$ the process of reaching the collusive outcome is expedited under multimarket contact. This is because a single price signal under multimarket contact elicits an increase in market price not only on the market that the signal was sent but also on the other market, which eventually leads to a faster conversion to the collusive outcome than under single market contact. However, as σ increases, the probability increases that there are not only one but rather two price signals sent by the firms. Under single market contact, such a situation directly leads to JPM whilst under multimarket contact this may result in JPM at most on one market. In this vein, if the probability of signaling is higher, i.e., $\sigma \geq \underline{\sigma}(\psi^X)$, then $\widehat{\psi}^{-X}(\sigma, \psi^X) \geq 1$, which is outside the valid parameter space, would be required to compensate for the faster conversion to the collusive outcome under single market contact. Consequently, if $\sigma \in \left[\underline{\sigma}(\psi^X), \overline{\sigma}(\psi^X)\right]$, firms find it easier to tacitly collude on one market than on two markets. Yet, for very high levels of the probability of sending a price signal, i.e., $\sigma > \overline{\sigma}(\psi^X)$, very frequent signaling occurs and multimarket contact may again facilitate tacit collusion more than single market contact. In this case, under both industry settings the collusive outcome is reached very fast due to parallel signaling by firms. To reach the collusive outcome by parallel signaling without the requirement of signal reaction, two parallel signals (one by each firm) are needed under single market contact, whereas four parallel signals (two by each firm) are required under multimarket contact. As the probabilities for different signals are independent of each other the probability for two parallel signals is given by σ^2, whereas four parallel signals occur with probability σ^4. For very high levels of σ close to one, the difference between these two probabilities decreases and the collusive state is reached through parallel signaling with almost the same probability. If, in this case, ψ^X is very small, a sufficiently high ψ^{-X} results in (slightly) faster convergence to the collusive outcome under multimarket contact than under single market contact.

Besides the faster process towards tacit coordination, the price setting strategy features another notable property. In case Proposition 6.1 holds, the opportunity costs for signaling under multimarket contact in two small markets with U consumers each are always lower than under single market contact, i.e., in one large market with $2U$ consumers. To see this, let Γ be the opportunity costs that a firm bears in any market when it signals (e.g., due to a loss in demand). Because the small market has half the size of the large market, with equal market prices in each market a signal in the small market incurs only half of the opportunity costs of a signal in the large market. Under single market contact, a firm sends a price signal with probability σ and thus in each period until the collusive state is reached, it has expected signaling costs of $\sigma\Gamma$.[5] Under multimarket contact, a firm signals on only one of the two markets with probability $2\sigma(1-\sigma)$ and on both markets simultaneously with probability σ^2. Thus, until a collusive state is reached, it also has expected signaling costs of $2\sigma(1-\sigma)\Gamma/2 + \sigma^2\Gamma = \sigma\Gamma$.[6] Consequently, whenever the condition in Proposition 6.1 holds, firms can expect to bear less opportunity costs for signaling in order to reach a collusive state under multimarket contact.

6.4 Experiment

The price setting strategy in the previous section and thus also Proposition 6.1 rest on the assumptions that firms send price signals with some probability (σ) and that the rival firm reacts to such price signals in the *same* market with some probability (ψ^X), but in the case of multimarket contact also in the *other* market with some probability (ψ^{-X}). In the following, these assumptions are tested by means of an economic laboratory experiment, and subsequently parameters σ, ψ^X and ψ^{-X} are estimated from experimental data. The experiment is specifically designed under the same single market and multimarket setting as the example for the price setting strategy, i.e., for which the existing theories (e.g., Bernheim and Whinston, 1990; Spagnolo, 1999; Anand and Mendelson, 1997; Matsushima, 2001; Chang and Harrington, 2003) do not predict any differences with respect to tacit collusion. Of course, in the experiment no assumptions or restrictions on the subjects' price setting behavior are imposed at all, which allows to study whether the observed behavior is in line with the theoretical model.

[5] For the sake of the simple argument to be made here, the cases where both firms signal simultaneously on a market or when firms react to signals are neglected. While taking this into account would considerably complicate the analysis, it will not change the offered insight.

[6] Again the same simplification as under single market contact is made here. In addition the probability that one of the two markets has already reached a collusive state is neglected. Evidently this will lead to less signals and thus reduce the expected opportunity costs of signaling under multimarket contact.

6.4.1 Design

The main experimental setup considers two identical markets with 10,000 consumers each. In each of the two markets the same two rival firms offer a homogeneous good and compete in prices, i.e., the markets follow the rules of Bertrand competition.[7] Each consumer has a valuation of monetary units (MU) 50 for the homogeneous good of both firms. The firms each have marginal cost of production of MU 10 in each market. The two firms interact for a total of T periods, where T is uniformly distributed on $[45,50]$. Hence, participants know that the experiment lasts at least 45 periods but no more than 50 periods. This termination rule is the result of a trade-off between the length of an experimental session and the effort to mitigate end-game effects.[8] The main advantage of this stopping rule compared to an unbounded random ending-rule is that the underlying repeated game is finite and thus, a folk theorem of tacit collusion does not apply. Selten et al. (1997) argue against unbounded random termination rules in laboratory experiments, noting that subjects know that the duration of the experiment will not in fact be unbounded, and thus, in such a case subjects build expectations about the total length of the experiment. As those expectations are unknown to the experimenter, they form a potential confound to experimental behavior and a potential loss of control is the consequence. Furthermore, Normann and Wallace (2012) find no difference in cooperation rates in prisoner's dilemma experiments with fixed or random ending rules. Note that in the experiment reported here the probability for a termination in period 45 is the same for all experimental sessions. Thus, only periods 1 to 45 are used for the statistical analysis.

Within this setup, the two investigated market structures are captured by the multimarket contact (MMC) treatment and the single market contact (SMC) treatment. In MMC firms may set prices independently in each market. By contrast, in SMC firms have to

[7]Note that the markets are independent of one another and there is no cross-market demand link (cf. Garcıa-Gallego and Georgantzıs, 2001).

[8]Such a termination rule is used for opening, intraday, and closing auctions in financial stock markets to limit auction price volatility towards the end of auctions, as is indicated exemplarily by the market model of the Frankfurt Stock Exchange (Deutsche Börse, 2015). Other experimental designs employ similar ending rules (see, e.g., Vossler et al., 2013).

choose the same price in both markets, i.e., a uniform pricing constraint is imposed, everything else being equal. Evidently, the uniform pricing constraint bonds the two (otherwise independent) markets and effectively renders them a single market. This setup is chosen for the experiment in order to keep the differences between the MMC and SMC treatment at a minimum with regard to the experimental design, procedures, and display of the experimental software.

In addition, as a robustness check for the theoretical model, a market structure with multimarket competition but without multimarket contact is considered. More precisely, in the additional experimental treatment a conglomerate firm competes against different non-conglomerate firms in each of the two duopoly markets. This treatment is therefore denoted as partial multimarket contact (PMC). Based on the theoretical model, the conjecture is that multimarket competition without multimarket contact, i.e., PMC, will lead to similar levels of tacit collusion as in the SMC treatment on both markets. Although the conglomerate firm may set different prices on both markets and hence, can send a price signal on one market only, this price signal should not be recognized as an intention to collude on both markets to the same extent as in MMC, because the conglomerate firm meets different firms in each market. The same reasoning applies even more so to the two non-conglomerate single market firms. A price signal by one of these firms does not imply an intention to collude by the other single market firm. Therefore, if the price signaling theory is valid under multimarket contact, an industry with only one conglomerate firm, i.e., multimarket competition without multimarket contact, should therefore not facilitate tacit collusion in the same way as multimarket contact. Consequently, the behavior in the PMC treatment should be similar to that in the SMC treatment and significantly different to that in the MMC treatment.[9] In terms of the proposed theory, the PMC treatment allows to investigate the interconnectedness

[9]Phillips and Mason (2001) compare multimarket competition without multimarket contact on two asymmetric markets to single market contact and find that connecting the asymmetric markets horizontally does not facilitate tacit collusion in both markets but rather leads to less (more) tacit collusion in one (the other) market. This result replicates previous findings by the authors (Phillips and Mason, 1992) under multimarket contact. They conclude that with asymmetric markets the impact of multimarket competition is similar to that of multimarket contact.

of markets with respect to the cross-market signal reaction probabilities in the firms' decision making process.

Note that standard economic theory does not expect any differences in price setting behavior between the three treatments, and also not between the two markets in the MMC and PMC treatment, as identical firms meet in identical markets with the same constant returns to scale technology, linear objective functions, and perfect information. Consequently, in the unique strict Nash equilibrium of the finitely repeated Bertrand stage game, all firms choose a price of MU 11 in all markets and periods in the SMC, MMC, and PMC treatment, respectively.

6.4.2 Procedures

The experiment is computerized with *z-Tree* (Fischbacher, 2007) and was conducted at the Karlsruhe Institute of Technology in Karlsruhe, Germany. The sessions for the SMC and MMC treatments were held between December 2012 and March 2013 and the sessions for the PMC treatment sessions were run in February and March 2014. In total, 185 students of economic fields were recruited as participants via the *ORSEE* platform (Greiner, 2015). The experiment was conducted between-subject, i.e., each participant was assigned to one of the treatments exclusively. Subjects were randomly assigned to cohorts of two (in the SMC and MMC treatment) or three (in the PMC treatment) and played with the same firm(s) for the whole time horizon of the game, i.e., a fixed partner matching. Thus, the experimental data is independent at the industry level and there are 78 independent observations.

The protocol for each of the sessions reads as follows: Upon entering the lab, participants are randomly assigned to a seat. Neither do they see each other, nor are they allowed to talk to other participants. To ensure common knowledge, instructions are handed out and read aloud by the person in charge of the experiment.[10] Subsequently,

[10]Experimental instructions for the MMC treatment together with a screenshot of the experimental software are provided in Appendix C.4.

a computerized questionnaire controls that participants know about the consequences of their actions during the experiment. Wrong answers are identified by the software and highlighted accordingly. Each participant repeats the questionnaire until all questions are answered correctly. After completion of the questionnaire by all participants the experiment starts automatically.

During the experiment perfect information is ensured, i.e., subjects are at all times informed about the past prices of all firms in their industry. The experimental software allows to display past prices both in a table and a price graph that visualizes the development of the cohort's prices over time. Moreover, subjects are equipped with a profit calculator in order to provide a maximum level of price transparency and salience. All sessions lasted between 30 minutes and one hour. In the experiment MU 1 million corresponded to EUR 1. None of the subjects experienced a loss. Their total monetary earnings averaged at EUR 11.88.

6.4.3 Results

There are 20 cohorts and thus, independent observations, for the SMC treatment, 29 for the MMC treatment, and 29 for the PMC treatment. The imbalance in the number of observations is a consequence of no-shows that happened to be more frequent in the SMC treatment. Prices by cohorts over time for all three treatments are provided in Appendix D.4.

The level of tacit collusion is assessed through market prices because marginal cost are equal in all treatments and markets, and thus no conversion into a collusion metric, e.g., the Lerner index or a degree of tacit collusion (cf. Engel, 2007), is necessary. For all subsequent analyses, if not stated otherwise, values of both markets are averaged in the MMC and PMC treatment in order to compare them to the SMC treatment.

Table 6.1 provides summary statistics for cohort level data from all three treatments. The average market price is computed over the first 45 periods of all independent ob-

TABLE 6.1: *Summary statistics on cohort level.*

Treatment	Variable	N	Mean	Std. dev.	Min.	Max.
SMC	Average market price	20	34.04	10.83	15.04	49.60
	Time to collude	9	19.00	9.08	5.00	34.00
MMC	Average market price	29	36.78	10.64	15.46	50.00
	Time to collude	13	6.62	6.78	1.00	24.00
PMC	Average market price	29	33.68	10.01	16.74	49.58
	Time to collude	15	15.87	10.45	1.00	33.00

servations. In the PMC treatment, prices set by the different types of firms average at MU 36.33 for conglomerate multimarket firms and MU 36.17 for non-conglomerate single market firms, which is not significantly different according to a matched-samples Wilcoxon signed-rank test ($z = 0.64, p = 0.524$). Statistics for the time to collude in the table are given for the collusive observations—two concepts that are introduced in the following.

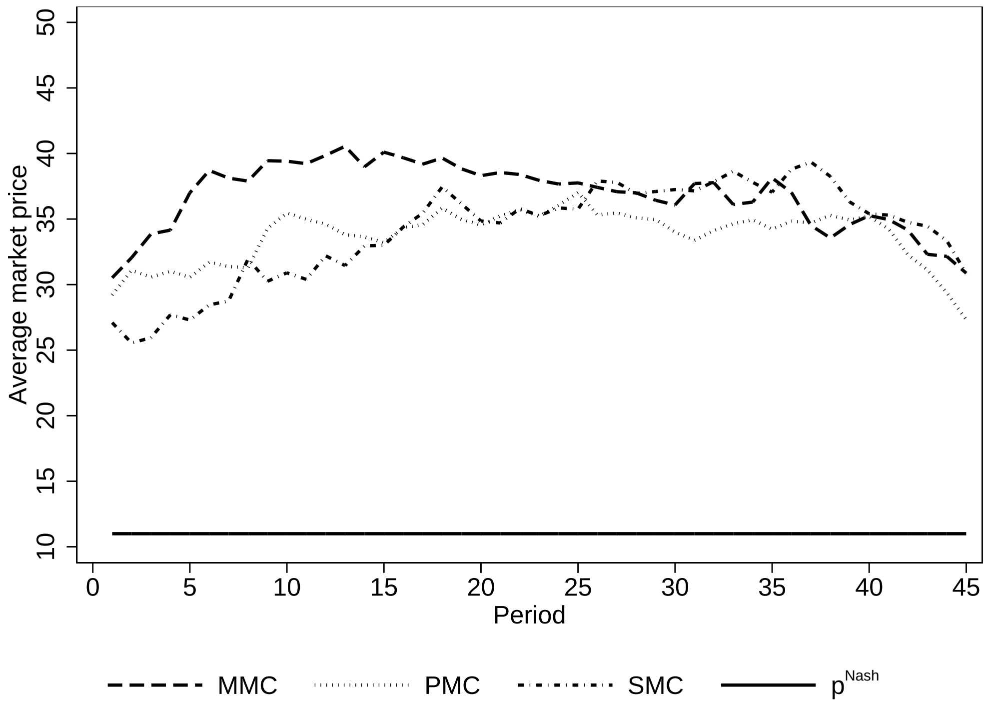
RESULT 6.1. *Market prices are significantly higher under multimarket contact than under single market contact or partial multimarket contact, but not significantly different between single market contact and partial multimarket contact.*

In line with previous experimental evidence (e.g., Engel, 2007), market prices are supra-competitive, i.e., above the strict Nash equilibrium, throughout the time time horizon of the experiment. Despite the effort to soften the end-game effect by experimental design, it can be clearly noticed in Figure 6.2. The illustration indicates further that market prices are initially higher under MMC compared to SMC and PMC. Controlling for dependencies between observations from the same cohort and thus, heteroscedasticity of errors across cohorts, consider the following two-level mixed-effects regression model:

$$\overline{p}_{k,t} = \beta_0 + \xi_k + \beta_{MMC} \cdot MMC + \beta_{PMC} \cdot PMC + (\beta_{Period} + \beta_{Period,k}) \cdot t + \epsilon_{k,t},$$

with $\overline{p}_{k,t}$ as the market price averaged over both markets in cohort k in period t, ξ_k as a random intercept controlling for the error component shared between repeated

FIGURE 6.2: *Average market price over time across treatments.*



observations from the same cohort k and $\beta_{Period,k}$ as a random slope for the time trend in market prices in each cohort. Estimates, as reported in Table 6.2, show that market prices are significantly higher under MMC than SMC but not different between SMC and PMC. Furthermore, a Wald test of the equality of treatment dummy coefficients shows that market prices are also significantly higher under MMC than PMC ($\chi^2(1) = 4.16, p = 0.041$). Figure 6.2 indicates that these differences are predominantly driven by the first half of the repeated game as market prices approach each other eventually. In fact, the average market price is almost 20% (15%) higher under MMC than SMC (PMC) during the first 25 periods. However, differences in average market prices fall to less than 5% during the 20 final periods. Overall, the average market price is about 10% higher under MMC than under the other two market structures, which are statistically indistinguishable throughout.

RESULT 6.2. *Multimarket contact does not significantly affect the likelihood for eventually achieving a collusive state.*

TABLE 6.2: *Multilevel mixed-effects linear regression of the average market price.*

Covariate	Coef.	Std. err.	z	p	95% conf. interval	
MMC	8.891	2.850	3.12	0.002	3.305	14.478
PMC	3.640	2.850	1.28	0.202	−1.947	9.226
Period	0.038	0.039	0.98	0.329	−0.038	0.115
Constant	29.391	2.193	13.40	< 0.001	25.093	33.690

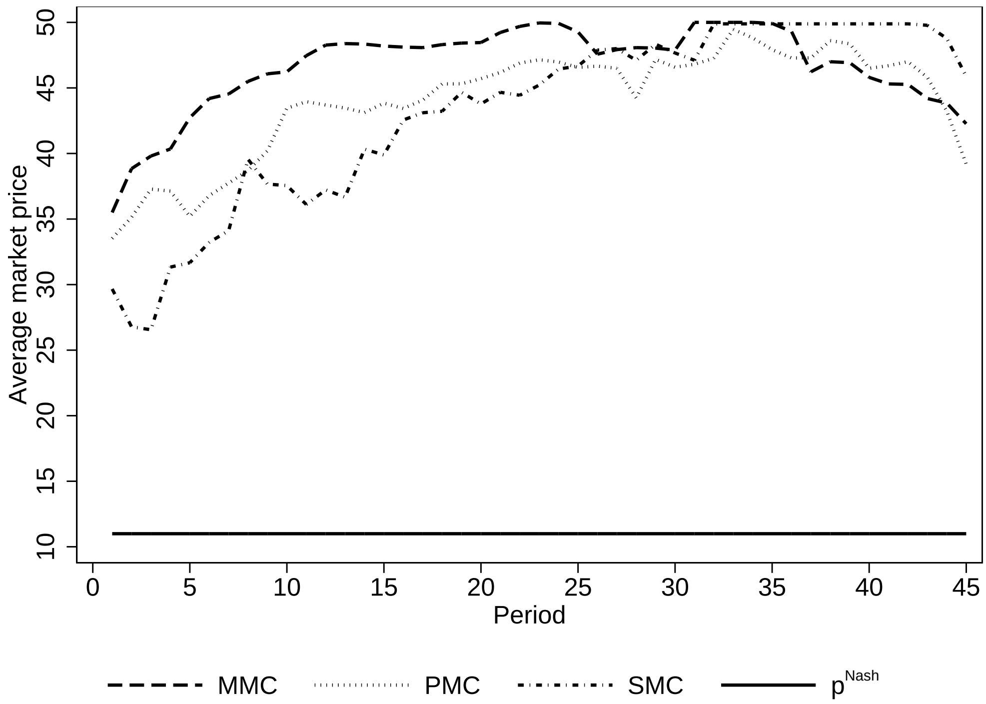
3,510 observations clustered in 78 cohorts.

A market is defined to be in a collusive state if both firms' prices are above the strict Nash equilibrium and do not change for at least five successive periods.[11] Note that, although arbitrary to some extent, the identification of collusive markets is robust against changes of the number of periods without price changes. In the MMC treatment the industry is conservatively defined to be in a collusive state if and only if the firms have reached a collusive state in both markets.[12] As the two markets in the PMC treatment are not strategically connected as in the MMC treatment, an industry is deemed to be in a collusive state also if that state is reached only on one of the markets. Generally, firms hold their prices once they have reached a collusive state until the onset of the end-game effect. There is only one exception to this rule: a duopoly in the MMC treatment in which a collusive state is reached again a few periods later. Throughout, the collusive state is achieved at the JPM price, i.e., consumers' willingness to pay, or one marginal price unit below it. Using the above definition, 9 out of 20 (45%) cohorts in the SMC treatment, 13 out of 29 (45%) cohorts in the MMC treatment, and 15 (four of those only on one market) out of 29 (52%) cohorts in the PMC treatment reach a collusive state at all. These relative frequencies are not significantly different according to Fisher's exact test ($p = 0.880$). In the following, these duopolies are denoted as *collusive*, whereas the remaining duopolies that do not reach a collusive state but may nevertheless show above-equilibrium prices are referred to as *non-collusive*. Figure 6.3 depicts the average

[11]Obviously, this definition covers the case of stable JPM pricing assumed in the price setting strategy. Yet, the definition adopted here is broader and serves as a robustness check whether there are stable collusive price configurations below the JPM price. In fact, there is no stable collusive price configuration that lies more than one marginal price unit below the JPM price. More specific, one observation in each of the three treatments is deemed to be in a collusive state at a price level of one marginal price unit below the JPM price.

[12]There is no duopoly in which firms reach a collusive state on one market alone. Neglecting time lags in reaching a collusive state between markets, collusive states are always either reached on both or none of the markets.

FIGURE 6.3: *Average market price of collusive duopolies over time across treatments.*



market prices in each period only for the collusive duopolies and highlights that the treatment differences prevail.

RESULT 6.3. *If a collusive state is achieved, it is reached significantly faster under multimarket contact.*

As the difference in market prices vanishes for later periods, it is now analyzed whether a collusive state is reached faster under MMC among the collusive duopolies. To this end, the number of periods needed to reach a collusive state is determined by the first period of the onset of a collusive state as defined above. With regard to the PMC treatment, the time until a collusive state is reached is determined in the most conservative way with respect to the hypothesis, namely as the mean rather than the maximum number of periods required on both markets in case a collusive state is reached on both markets. Then, the number of periods required to reach a collusive state is significantly lower in the MMC treatment than in the SMC treatment as well as the PMC treatment according to a non-parametric Kruskal-Wallis H test ($\chi^2(2) = 10.26, p = 0.006$) and multiple-comparison tests with Bonferroni-adjustment.[13] Furthermore, the time to collude is not significantly different between collusive cohorts from the PMC treatment and the SMC treatment. In particular, on average a collusive state is reached in the 19$^{\text{th}}$ period, i.e., after 42% of the minimum game length, in the SMC treatment, whereas it is already reached in the 7$^{\text{th}}$ period, i.e., after 15% of the minimum game length, in the MMC treatment. In the PMC treatment, a collusive state is reached on average in the 16$^{\text{th}}$ period, i.e., after 36% of the minimum game length.

6.5 Price setting behavior

While the previous results indicate that multimarket contact may indeed facilitate tacit collusion, an investigation into whether the suggested price setting strategy offers a reasonable microfoundation for the experimental results is still warranted. According to the price setting strategy, a firm's behavior is determined through the signaling probability σ as well as the signal reaction probabilities ψ^X and ψ^{-X}. Hence, in the following these parameters are estimated for each firm from the experimental data. Furthermore, the estimations are conducted separately for all collusive and for all non-collusive cohorts in an effort to explain why collusion occurred in some, but not in all cases. As firm parameters may influence each other and change over time, the estimation procedure is designed in the most conservative way possible in an effort to keep parameter estimations independent. First, in the multimarket treatments, reaching a collusive state on one market may strongly affect future behavior on the remaining market due to the fact that markets are strategically connected. Therefore, estimations of all parameters are based only on the periods and signals until a collusive state is reached on the first market. Second, in contrast to the other two treatments, in the PMC treatment not all firms operate on all markets. Consequently, markets have to be considered separately in the PMC treatment. However, this would inevitably reduce comparabil-

[13] While the more conservative non-parametric test is reported here, a parametric one-way analysis of variance (ANOVA) model with multiple-comparison tests yields the same result.

ity between parameter estimates between treatments. Therefore, in both the PMC and the MMC treatment, signal reaction parameters are only estimated for the market that reached a collusive state first. In a similar fashion, the probability of sending a price signal is estimated separately for each conglomerate firm but combined for the two non-conglomerate firms (in PMC). In other words, in order to harmonize the estimation process across treatments this procedure allows that the two non-conglomerate firms in the PMC treatment behave as one conglomerate firm. Clearly, this procedure is conservative with respect to the hypothesis that MMC facilitates tacit collusion compared to PMC. In line with the price setting strategy, a price signal in period t is identified as choosing the JPM price in period t when the other firm's price in that market in $t-1$ was lower than the JPM price.

The probability of sending a price signal σ for firms in the SMC treatment is readily given by the number of price signals by a firm divided by the number of periods until a collusive state is reached or by the number of total periods in case a collusive state is never reached. Under MMC, firms can send a price signal on none, one or both markets. According to a Bernoulli process, the probability that a conglomerate firm sends at least one price signal in a period is given by $\sigma^2 + 2\sigma(1-\sigma)$. This is equal to the number of periods in which a firm sends at least one price signal divided by the number of periods until a collusive state is reached on the first market or by the number of total periods in case a collusive state is never reached. In the PMC treatment, the same rationale as for the MMC treatment applies to the conglomerate firm individually and to the two non-conglomerate firms combined. With regard to the probability of recognizing and reacting to a price signal on market X, in all treatments it is equally determined for each firm how often it raises its own price on the same market (ψ^X) or the other market (ψ^{-X}) to the price level at which the price signal is sent, i.e., the JPM price, following a price signal by the other firm in the previous period.

The left part of Table 6.3 reports average point estimates for all three parameters according to the cohorts of firms specified above, i.e., for each treatment as well as collusive or non-collusive cohorts separately. Note that about half of all firms in each of the

TABLE 6.3: *Average probabilities for sending and reacting to a price signal and corresponding numbers of periods until the collusive state is reached in the experiment and according to the price setting strategy.*

		Parameters			Time to collude	
Collusiveness	Treatment	σ	ψ^X	ψ^{-X}	Experiment	Price setting strategy
Collusive	MMC	0.166	0.854	0.864	6.62	6.09
	PMC	0.116	0.686	0.467	15.87	12.66
	SMC	0.137	0.725		19.00	19.23
Non-collusive	MMC	0.078	0.150	0.138		116.28
	PMC	0.037	0.190	0.085		360.26
	SMC	0.041	0.047			588.08

treatments sent at least one price signal. The probabilities for reacting to a price signal can only be calculated for firms whose competitor(s) sent at least one price signal. As the probabilities for sending and reacting to price signals are not independent between firms from the same cohort, industry averages are reported and used in the following to test the foundations of the price signaling theory.

RESULT 6.4. *The probabilities for sending a price signal and reacting to it on the same market are significantly higher for collusive than for non-collusive cohorts within a treatment, but do not differ across treatments. However, the probability for reacting to a price signal on the other market is significantly higher under multimarket contact than under partial multimarket contact.*

In line with the conjecture, estimates of all three parameters are on average significantly higher for firms in a collusive cohort than in a non-collusive cohort, but are not different (with one notable exception, which is discussed below) between firms from collusive respective non-collusive cohorts across treatments. This is supported by corresponding ordinary least squares (OLS) regressions with treatment dummies, interaction effects, and the MMC treatment as the baseline. In particular, Tables 6.4, 6.5, and 6.6 report estimates of OLS regressions of the form

$$\begin{aligned} y_k = \beta_0 &+ \beta_{Collusive} \cdot Collusive + \beta_{PMC} \cdot PMC + \beta_{SMC} \cdot SMC \\ &+ \beta_{PMC \text{ x } Collusive} \cdot PMC \cdot Collusive + \beta_{SMC \text{ x } Collusive} \cdot SMC \cdot Collusive + \epsilon_k \end{aligned}$$

TABLE 6.4: *OLS regression of the probability for sending a price signal σ.*

Covariate	Coef.	Std. err.	t	p	95% conf. interval	
Collusive	0.088	0.047	1.87	0.065	−0.006	0.182
PMC	−0.041	0.046	−0.89	0.375	−0.133	0.051
SMC	−0.037	0.049	−0.75	0.458	−0.135	0.062
PMC x Collusive	−0.010	0.066	−0.14	0.885	−0.142	0.123
SMC x Collusive	0.008	0.074	0.11	0.916	−0.139	0.155
Constant	0.078	0.031	2.48	0.015	0.015	0.141

78 observations, $R^2 = 0.131$.

TABLE 6.5: *OLS regression of the probability for reacting to a price signal on the same market ψ^X.*

Covariate	Coef.	Std. err.	t	p	95% conf. interval	
Collusive	0.704	0.108	6.52	< 0.001	0.487	0.921
PMC	0.039	0.113	0.35	0.729	−0.187	0.265
SMC	−0.103	0.108	−0.96	0.342	−0.320	0.113
PMC x Collusive	−0.207	0.159	−1.31	0.197	−0.526	0.111
SMC x Collusive	−0.025	0.161	−0.16	0.876	−0.350	0.299
Constant	0.150	0.067	2.26	0.028	0.017	0.284

55 observations, $R^2 = 0.659$.

with $y \in \{\sigma, \psi^X, \psi^{-X}\}$. Obviously, as ψ^{-X} does not apply in the SMC treatment, the corresponding covariates are omitted for this regression. All three OLS regressions show that the effect of collusive cohorts is positive and significant throughout, while treatments as well as interactions between treatments and collusion are insignificant. The only exception—in accordance with the theoretical prediction—is the cross-market reaction to a price signal. Here, the average estimated probability ψ^{-X} is significantly lower for collusive PMC cohorts than for collusive MMC cohorts. In other words, under multimarket competition without multimarket contact price signals have a smaller cross-market impact than under multimarket contact. As the conglomerate firm in the PMC treatment meets different (non-conglomerate) rivals on both markets and hence, there is no multimarket contact as in the MMC treatment, this is in line with the price signaling theory. These parameter differences have implications for the time to collude that is estimated by the price setting strategy, as is proven analytically in the following. Thereby, it is assessed whether the price setting strategy successfully explains significant differences in the time needed to reach a collusive state using the parameters estimated from the experimental data.

TABLE 6.6: *OLS regression of the probability for reacting to a price signal on the other market* ψ^{-X}.

Covariate	Coef.	Std. err.	t	p	95% conf. interval	
Collusive	0.726	0.133	5.46	< 0.001	0.455	0.998
PMC	−0.053	0.126	−0.42	0.679	−0.311	0.205
PMC x Collusive	−0.344	0.193	−1.79	0.084	−0.737	0.049
Constant	0.138	0.075	1.84	0.075	−0.015	0.290

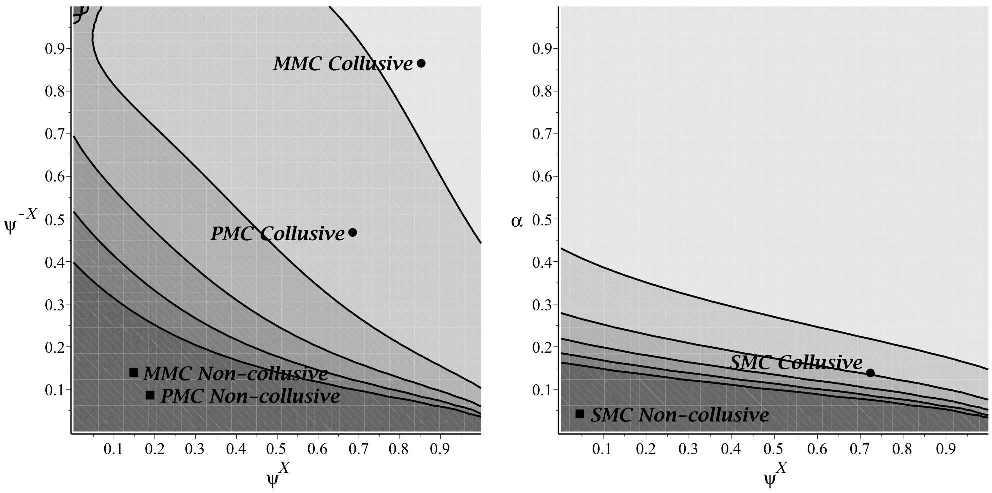
34 observations, $R^2 = 0.559$.

RESULT 6.5. *With the empirically observed signal sending and reaction probabilities in collusive cohorts, the price setting strategy accurately predicts the empirically observed time needed to reach a collusive state for each market scenario.*

The expected number of periods until JPM is reached is readily given by the expected time to absorption of the corresponding Markov chain. Given the transition matrix of a Markov chain with a unique absorbing state, the expected number of steps until that absorbing state is reached can be computed from its fundamental matrix (see Appendix B.2). Using the parameter estimations specified in the left part of Table 6.3, the expected number of periods until JPM is reached is calculated analytically as 6.09 for the collusive MMC cohorts and 19.23 for the collusive SMC cohorts. The right part of Table 6.3 allows to compare these values and shows that they match well to the empirical findings in the experiment, which are on average 6.62 periods in the MMC treatment and 19.00 periods in the SMC treatment.

In an effort to ensure comparability across treatments, signaling parameters in the PMC treatment and the MMC treatment are estimated with identical procedures. Consequently, in order not to impose further assumptions onto the price setting in the PMC treatment, the same analytical Markov chain is applied for MMC and PMC. This allows for cross-market reaction to price signals in PMC. As shown above, the probability to react to a price signal on the other market is significantly lower for collusive PMC firms than collusive MMC firms. This carries over to the time needed to reach a collusive state, which is estimated at 12.66 periods by the price setting strategy—slightly lower than the 15.87 periods in the experiment. For the observed signal probabilities for the

FIGURE 6.4: *Expected number of periods until a collusive state is reached.*



(A) *Multimarket contact.* (B) *Single market contact.*

Note. The lines in the contour plots delimit areas with an expected number of periods until a collusive state is reached in steps of tens from below 10 periods (light gray) to above 50 periods (dark gray). For each treatment, the circles and boxes depict the parameter estimates for collusive and non-collusive firms, respectively. Panel 6.4a is compiled using $\sigma = 0.1$.

non-collusive duopolies, the price setting strategy predicts that an industry does not converge to JPM until more than 100 periods in any treatment, i.e., more than double the 45 to 50 periods in the experiment.

These observations are supported visually by Figure 6.4 which depicts the expected number of periods until the collusive state is reached according to the proposed price setting strategy under MMC (Figure 6.4a) and under SMC (Figure 6.4b), respectively. For both contour plots, the time to collude is lowest in the top right corner and highest in the bottom left corner. Note that the vertical axes of the two panels in Figure 6.4 differ. In particular, for the sake of a two-dimensional presentation, Figure 6.4a is compiled using $\sigma = 0.1$ (i.e., the average over all parameter estimates from treatments MMC and PMC), whereas in Figure 6.4b $\psi^{-X} = 0$ by definition. Additionally, point estimates of parameters are depicted for the non-collusive cohorts of treatments as boxes and for the collusive cohorts of treatments as circles. Both panels in Figure 6.4 emphasize the differences between collusive and non-collusive parameter estimates from the same treatment as well as the differences in the corresponding expected steps until a collusive state is reached. In particular, non-collusive parameter estimates across MMC and PMC lie closely together. In relative comparison, the collusive parameter estimates for the probability of cross-market reaction to a price signal vary greatly across the two industry settings, thus portraying the distinction of multimarket contact and multimarket competition without multimarket contact.

The conclusion of these findings is twofold. First, both the probability for sending a price signal as well as the probability for recognizing and reacting to a price signal (on the same market or cross-market) are significantly higher for firms in collusive cohorts than for firms in non-collusive cohorts. Therefore, different values for the parameters may indicate different types of firms, i.e., those with and without intentions to tacitly collude. Second, the price setting strategy successfully predicts systematic differences in the behavior of firms and the extent of tacit collusion in the experiment. This evidence suggests that the strategy captures relevant aspects of oligopolistic competition under single market and multimarket contact.

6.6 Discussion

Multimarket contact is known to relax price competition in a number of circumstances. However, the extant literature relates this finding either to asymmetry of firms (Bernheim and Whinston, 1990), concave objective functions (Spagnolo, 1999) or imperfect monitoring of actions (Anand and Mendelson, 1997; Matsushima, 2001; Chang and Harrington, 2003). We explicitly consider an industry setting in which none of these theories apply and instead provide a new, theoretical explanation based on behavioral research for why multimarket contact may facilitate the emergence of tacit collusion even in—but not limited to—this case.

A price setting strategy is suggested that explains the process through which firms can tacitly reach a collusive outcome faster under multimarket contact than under single market contact, everything else being equal. This finding rests on two assumptions.

First, it is assumed that firms can implicitly communicate their intention for collusive play solely through their price setting behavior without communicating explicitly. Second, it is conjectured that such price signaling is more efficiently in a multimarket environment, because a price signal that is sent in only one market may also have a collusive cross-market impact on the other market. Firms meeting in several distinct (geographic or product) markets can discriminate prices across these markets, which offers conglomerate multimarket firms a means to signal their pricing intentions with less opportunity cost. In other words, a price signal that is sent on only one market (thus, constituting opportunity cost also only on that one market) may evoke a market price increase in all markets. Conversely, under single market contact of non-conglomerate firms, or equivalently, if conglomerate firms are subject to a uniform pricing constraint, or if authority over business decisions is decentralized, the price signaling possibilities are curtailed which effectively limits the efficiency of a price signal.

By means of an economic laboratory experiment, in which firms are not allowed to communicate explicitly, these two assumptions are validated and it is shown that the price setting strategy replicates behavior in the experiment accurately. Furthermore, the price setting strategy can also explain why some, but not all industries reach a collusive state if one controls for firm-specific parameters of the price setting strategy. In particular, multimarket contact expedites the speed at which a collusive state is reached, however it does not affect the likelihood of reaching a collusive state per se. Thus, one can argue that multimarket contact facilitates the emergence of tacit collusion. This also explains why prices are initially higher under multimarket contact—as firms collude earlier here—but why price differences eventually vanish in later periods—as multimarket contact does not affect the likelihood to achieve a collusive state after all.

The signal sending and reaction probabilities obtained from the experimental data are significantly higher for firms that reach a collusive outcome than for those firms that do not reach a collusive state, but are similar across treatments for collusive firms respective non-collusive firms. This clearly supports the rationale of the price setting strategy. With these parameters the process described by the price setting strategy converges to the collusive outcome, i.e., JPM, in a similar number of periods as in the experiment. Thus, the price setting strategy captures the emergence of tacit collusion accurately, both overall as well as the differences between multimarket and single market contact. Finally, it is worth noting that these results are robust in the sense that they are driven by multimarket contact and not multimarket competition per se, which is highlighted by similar market outcomes under single market and partial multimarket contact.

Evidently, these results bear important managerial as well as policy implications. First, conglomerate firms find it easier to collude tacitly with their conglomerate competitors if they are meeting them in more than one market. Second, in such an industry setting, organizational centralization may facilitate tacit coordination by mere observation of rivals' prices. Third, multimarket contact between oligopolistic firms, which is common in many industry contexts, should even more so be subject to scrutiny by antitrust authorities. Even if symmetric firms with linear objective functions and perfect information on their competitors meet in symmetric markets, which is so far not considered to be a concern, multimarket contact may facilitate the emergence of tacit collusion. Moreover, while this setting excludes alternative explanations, the behavioral explanation presented here is, of course, not limited to this case. Rather, it is likely that price signaling under multimarket contact also facilitates tacit collusion in industries with asymmetric firms and markets. Therefore, this study sheds new light on theoretical as well as empirical research on multimarket contact. Fourth, the results indicate that the implementation of uniform pricing constraints in regulated industries such as telecommunications markets may effectively mitigate tacit collusion. Hence, uniform pricing constraints could be considered by competition authorities in comparison to access regulation or price regulation. As the latter are often connected to issues of information asymmetries and regulatory commitment, uniform pricing may be an effective alternative to preserve competition in multimarket industries. However, side effects of such ex ante regulation should be scrutinized simultaneously.

This study stresses that underlying propensities of the communicative content of price choices between rivals should gain more attention in the context of multimarket contact. Yet, this finding is subject to several limitations which give rise to open questions for future research. In particular, the analysis is focused on a specific market setting. Although the suggested price setting strategy generalizes easily to other market settings, future research should delineate the boundaries and limiting conditions of the empirical findings in more detail. First, the price setting strategy should be formally adopted and empirically tested for asymmetric firms and markets. Second, future research should also address whether the findings carry over to other modes of price or quantity competition, e.g., with horizontal or vertical product differentiation. Third, it is conceivable that a higher number of markets and competing firms may alter the findings substantially, allowing firms to send even more sophisticated signals (as in the case of more markets), but also limiting the scope for strategic interaction (as in the case of more firms). Fourth and lastly, further insights on the dynamics of price adjustments may be gained by considering more nuanced definitions of price signals.

Chapter 7

Upstream and Downstream Competition, and Tacit Collusion

IN several industries effective retail competition downstream is only feasible if wholesale access is provided to an essential input resource upstream. The regulation of wholesale access to an upstream bottleneck resource that represents an essential input for non-integrated firms to compete in the retail market downstream stimulates considerable economic research. This issue arises most prominently in network industries such as the telecommunications (Krämer and Vogelsang, 2014) and energy (Boots et al., 2004) industries, in which the bottleneck arises naturally through subadditivity of the cost structure. Yet, access regulation is also of concern in other contexts such as the licensing of intellectual property (Dewatripont and Legros, 2013), where the bottleneck is constituted artificially. This chapter is concerned with these antitrust and regulatory issues—most prominently the emergence of tacit collusion—in a vertical industry structure of an upstream market and a downstream market along the same value chain. Thereby, different market structures at the upstream market and different regulations on the wholesale level are investigated by means of an economic laboratory experiment and assessed according to their effect on market outcomes with a particular attention on consumers.

The remainder of this chapter is organized as follows. Section 7.1 motivates this research and describes the industry scenarios under investigation: a monopoly and two duopoly variants with respect to the upstream market structure and in regard to regulation at the wholesale level a margin squeeze rule, which prohibits firms to set upstream prices above their downstream prices, or no regulation at all. In Section 7.2 the related literature on wholesale competition and margin squeeze regulation is reviewed. In Section 7.3, the experimental design is described and hypotheses derived based on theoretical predictions for four timing variants of the basic model. Section 7.4 presents the experimental results. In Section 7.5 results are examined with respect to the hypotheses and incentives for tacit collusion in a repeated game context are discussed. Finally, Section 7.6 discusses possible limitations and extensions.

7.1 Motivation

The anti-competitive effects that possibly arise in a scenario of an upstream bottleneck resource as well as accompanying regulatory remedies are widely studied in the literature for the case of a single access provider. In particular, the questions of how to set an optimal wholesale charge under access regulation (Armstrong et al., 1996; Armstrong and Vickers, 1998) and the incentive to raise rivals' costs (Salop and Scheffman, 1983, 1987) through price discrimination (DeGraba, 1990; Inderst and Valletti, 2009; Vickers, 1995) and non-price discrimination (Economides, 1998; Mandy, 2000; Mandy and Sappington, 2007; Weisman and Kang, 2001) are examined in detail. Moreover, firms' strategic incentives to vertically integrate across retail and wholesale markets and the effect of such conduct on competition (see Lafontaine and Slade, 2007, for an overview) are thoroughly investigated, in particular with respect to the softening of retail competition (Chen, 2001; Gans, 2007) and foreclosure of non-integrated resellers (Choi and Yi, 2000; Hart and Tirole, 1990; Ordover et al., 1990; Rey and Tirole, 2007) or upstream rivals (Chen and Riordan, 2007). Based on this theoretical literature, vertical foreclosure is also examined by means of economic laboratory experiments (Martin et al., 2001; Normann, 2011).

However, a set of new issues arises when there is more than one vertically integrated access provider such that competition at the wholesale level may emerge in addition to retail competition. Especially in the likely case of a highly concentrated wholesale market, i.e., a duopoly, the question arises whether access regulation is (still) warranted. Evidently, the answer to this question has direct ramifications on how regulators and competition authorities should deal with this kind of market structure, but also on whether authorities should promote the entry of a second integrated access provider in markets in which the essential input is currently supplied monopolistically. Although not confined to this context, the relevance of this scenario is exemplified in the telecommunications industry, in which technological progress and consolidation create an environment with few vertically integrated firms as well as several non-integrated resellers that rely on access, both in the fixed and in the mobile markets. On the one hand, with respect to fixed networks, technological progress leads to the roll out of new fiber-optic networks as well as the evolution of broadband cable networks, which both create new vertically integrated firms that compete most notably in densely populated urban areas with the traditional telecommunications incumbent. On the other hand, mobile telecommunications markets experience a recent wave of mergers and acquisitions that reduce the number of independent operators maintaining a distinct cellular infrastructure, thus increasing market concentration at the wholesale level.

Despite its practical relevance, the explicit analysis of simultaneous wholesale *and* retail competition in the presence of *both* vertically integrated and non-integrated firms receives less attention in the economic literature. The extant theoretical analyses, which are reviewed in detail below, suggest that wholesale competition is likely to improve and not deteriorate market outcomes for resellers and consumers, i.e., wholesale and downstream prices are lower compared to the case of a wholesale monopoly, although monopoly-like equilibria may exist. Thereby, the theoretical models generally rest on the assumption of effective competition at the wholesale level, in particular by assuming homogeneous Bertrand competition between duopolistic access providers (see, e.g., Bourreau et al., 2011). However, empirical results from both laboratory (see Engel (2007), Potters and Suetens (2013), and Chapter 4) and field studies (see, e.g., Parker and Röller, 1997, in the context of telecommunications markets) suggest that duopoly markets are prone to high levels of tacit collusion, which may give rise to market outcomes that differ from those identified in the theoretical literature.

This study scrutinizes the effect of wholesale competition on market outcomes by explicitly taking into account the emergence of tacit collusion that may arise from this scenario. Based on the Bourreau et al. (2011) framework of two integrated firms and one non-integrated reseller, an economic laboratory experiment is designed that allows to empirically observe market outcomes under various modes of wholesale competition while keeping all other factors fixed. Furthermore, a continuous time framework is employed that allows to endogenize the timing of price settings and therefore incorporates settings in which wholesale and retail prices are set simultaneously, which is also assumed in the context of upstream collusion (Nocke and White, 2007). First, the case where only one of the integrated firms provides wholesale access (access monopoly) is examined. This constitutes the benchmark case, which is extensively studied in the literature. Second, homogeneous Bertrand competition between the two integrated firms at the wholesale level is considered. In this setting, firms can adjust their wholesale prices at any time and the firm that offers the lower price serves the entire wholesale market. Third, as Bertrand competition is known to be susceptible to tacit collusion (Potters and Suetens, 2013), a variant of Bertrand competition at the wholesale level is implemented in which integrated firms are obliged to maintain their wholesale price for a fixed period of time (i.e., a price commitment), everything else being equal to the second case. Due to this price commitment, the firm that decides on the lower price is granted a wholesale monopoly position for some time. Thus, this latter treatment induces an element of *competition for the market* (Geroski, 2003), which is conjectured to hinder tacit collusion and intensify competition at the wholesale level. All three modes of wholesale competition are examined both under a no regulation regime, where firms are free to set wholesale and retail prices, and under a margin squeeze regulation regime, in which an integrated firm's wholesale price may not exceed its retail price. Margin squeeze regulation recently gained attention in the debate on *open access* policies and is perceived as a viable alternative to price regulation, e.g., by the European Commission (2013a), particularly when there is more than one wholesale access provider.

The results of this study indicate that, over and beyond the findings of the theoretical literature, wholesale competition may in fact lead to a worse market outcome for consumers than a wholesale monopoly. For the case of homogeneous Bertrand competition at the wholesale level both wholesale as well as retail market prices are above the level that is observed when there is only a single access provider. Drawing on the literature on upstream collusion (Nocke and White, 2007; Normann, 2009) it is shown that incentives for tacit collusion are actually higher under wholesale competition if an infinitely repeated game context is considered. Thus, even in the presence of wholesale competition regulators should closely monitor the outcomes of such vertically related markets. However, the results also demonstrate that wholesale competition may be intensified by simple procedural regulation, namely a price commitment, which in turn restores the theoretical prediction to the extant that access prices are lower than under a wholesale monopoly. However, even in this case, wholesale access prices remain well above the predicted Nash equilibrium, i.e., marginal costs. Furthermore, in the context of the open access debate, there is no evidence that a margin squeeze regulation reduces retail market prices compared to a no regulation regime. Although margin squeeze regulation may benefit the reseller, it tends to increase retail prices and thus reduce consumers' surplus.

7.2 Related literature

Two strands of literature are related to this study. First, the literature on upstream market structures considers and compares effects that arise in case the wholesale market is governed by a monopoly or by competition, i.e., in general a duopoly. Second, the literature on margin squeeze regulation investigates whether a margin squeeze is anti-competitive behavior at all and what effects on the market are connected to the implementation of such a regulatory remedy. Both strands of literature are reviewed in the following.

7.2.1 Wholesale monopoly and competition

Before reviewing the literature on wholesale competition, it is worth noting some of the effects that arise already in the presence of a monopolistic access provider. Even in the absence of regulation a vertically integrated firm may be willing to supply the wholesale market on a voluntary basis if the additional revenues generated at the wholesale level exceed the business stealing effect of the reseller in the retail market (Farrell and Weiser, 2003; Höffler and Schmidt, 2008). More generally, if downstream organizations exhibit efficiency advantages or if retail goods are sufficiently quality-differentiated (e.g., due to brand reputation or additional sales channels as illustrated by Banerjee and Dippon, 2009), the provision of wholesale services allows the integrated firm to generate additional revenues. In this case, the access provider benefits from a demand expansion effect relative to a situation where the integrated firm is the single seller of its goods in the retail market (Boudreau, 2010).

In the presence of wholesale competition the incentives to provide access on a voluntary basis are likely to be increased compared to a wholesale monopoly, because the integrated firms may now find themselves in a prisoner's dilemma with respect to the provision of the wholesale good (Brito and Pereira, 2010). Studies that investigate these incentives can be classified according to the assumed competition model: The majority considers price competition with horizontally differentiated retail products (Atiyas et al., 2015; Brito and Pereira, 2010; Bourreau et al., 2011; Höffler and Schmidt, 2008; Ordover and Shaffer, 2007) where competition is either spatial (Hotelling, 1929; Salop, 1979) or non-spatial (Shubik and Levitan, 1980). The remainder assumes quantity competition in the retail market (Dewenter and Haucap, 2006; Kalmus and Wiethaus, 2010). In particular, these studies are interested in the conditions under which resellers are supplied in equilibrium and whether resale actually increases downstream market efficiency. Although the precise nature of the supply and non-supply equilibria as well as the retail equilibria that emerge under wholesale competition depend on the specific modeling assumptions, all theoretical investigations agree that wholesale competition neither leads to more foreclosure of the reseller nor increases wholesale or retail market prices in comparison to a wholesale monopoly.

More specifically, under wholesale competition Ordover and Shaffer (2007) as well as Brito and Pereira (2010) find that integrated firms provide the reseller with the retail good at marginal cost if products are sufficiently differentiated although they would be individually better of without entry as retail prices and profits decrease. On the other hand, resellers are generally not supplied if retail products are close substitutes and none of the integrated firms has an incentive to make a profitable wholesale offer in the first place. Furthermore, Ordover and Shaffer (2007) show that the supply equilibrium disappears if inputs are differentiated as well, or if the reseller chooses its quality endogenously and cannot commit ex ante to its product positioning.

Moreover, the analyses by Brito and Pereira (2010) and Höffler and Schmidt (2008) reveal that if competition is spatial and the degree of quality differentiation is intermediate, one integrated firm may provide access while the other integrated firm makes an unprofitable offer. This finding of a *partial foreclosure equilibrium* is further generalized—including the case of non-spatial competition—by Bourreau et al. (2011) based on the characterization of the *softening effect*: A vertically integrated wholesale provider chooses its retail price with regard to its opportunity costs in the wholesale market (DeGraba, 2003) and thus is less aggressive in the retail market than its vertically integrated rival who does not provide wholesale access. In other words, the consideration of opportunity costs weakens competition in the retail market and may at the same time make it less attractive to compete for wholesale revenues. In consequence, the monopoly outcome may be restored, because the integrated rival of the access provider benefits from higher retail profits and thus prefers to exit the upstream market. Note, however, that the equilibrium hinges on the assumptions that retail goods are close substitutes *and* that at least one firm supplies the retail firm, e.g., due to a reseller's efficiency advantage (Bourreau et al., 2011) or due to regulatory coercion (Bourreau et al., 2015). Otherwise, marginal cost pricing in the upstream market constitutes the unique equilibrium under wholesale competition in the non-spatial model if goods are sufficiently differentiated (Höffler and Schmidt, 2008). Moreover, Atiyas et al. (2015) show that unobservable, more complex wholesale contracts may stimulate voluntary access and wholesale competition, thus making foreclosure of the reseller less likely.

Höffler and Schmidt (2008) investigate the effects of resale on consumer welfare, which may be increased either by a decline in retail prices and/or an increase in variety. Under the assumption that the reseller will be supplied by one of the integrated firms, i.e., there is no foreclosure, it is shown that resale may actually increase the market price if quality differentiation is sufficiently high. In the case of non-spatial competition the price increase is always compensated by an increase in variety with respect to consumer welfare. In the spatial model however, consumers may be worse off as the price effect dominates. Then again, if wholesale competition for resellers is considered in the non-spatial model, wholesale prices are found to equal marginal cost and, in consequence, retail prices are lower than compared to a situation without resale.

Whereas the reported analyses of wholesale competition focus exclusively on the one-shot interaction between firms, the literature on upstream collusion examines incentives for coordinated firm behavior in an infinitely repeated game setting. Nocke and White (2007) compare critical discount factors that are necessary to sustain collusion by the means of grim trigger strategies and find that vertical integration facilitates tacit collusion among upstream firms relative to a vertically separated industry structure. Normann (2009) replicates the finding that vertical integration facilitates upstream collusion for the case of linear input charges and a sequential setting of wholesale and retail prices, whereas Nocke and White (2007) model wholesale contracts as two-part tariffs and assume simultaneous price setting. Although sequential price setting is assumed by most of the presented studies, the work by Nocke and White (2007) exemplifies that the timing of firms' actions and the interaction may in fact be more diverse and nuanced.

This study contributes to the literature on wholesale competition by showing empirically that wholesale prices may be above the monopoly level even if theory predicts wholesale supply at marginal costs as the unique equilibrium. It is further shown that tacit collusion at the wholesale level may effectively be reduced by a price commitment rule that fosters the integrated firms' competition for the market. Moreover, the experimental framework allows for a systematic comparison of retail market prices and consumer surplus under the different modes of wholesale competition.

7.2.2 Margin squeeze regulation

In the presence of a duplicate infrastructure the traditional economic rationale for ex ante price regulation is no longer applicable as the bottleneck does not represent a single essential facility anymore (Renda, 2010). In consequence, regulators and competition agencies may be concerned with identifying suitable alternatives and regulatory rules that still ensure *open access* for downstream competitors, but give integrated firms more freedom in setting their wholesale prices (Krämer and Vogelsang, 2014). The *margin squeeze rule* represents a potential surrogate for price regulation that is already applied in various forms and different contexts. Next to its application in (European) competition law, the basic mechanism, which is designed to ensure a viable wholesale-retail margin for a downstream reseller, is also implemented by *retail minus X regulation* (Gonçalves, 2007) and the *efficient component-pricing rule* (Baumol et al., 1997). Ever since the landmark decision *Deutsche Telekom*[1] in 2003, the application of the margin squeeze rule as an antitrust instrument is controversially debated within the economic and the legal literature (Briglauer et al., 2011; Carlton, 2008; Geradin and O'Donoghue, 2005). While the European Commission has repeatedly convicted firms based on a margin squeeze accusation[2] and has been confirmed by European courts[3], the US Supreme Court has dismissed allegations based on the margin squeeze rationale in comparable cases (*Trinko* and *linkLine*).

[1]Commission Decision 2003/707/EC.

[2]See the Commission Decision of 4 July 2007 (Case COMP/38.784 – *Wanadoo España vs. Telefónica*).

[3]See the cases *Deutsche Telekom* (T-271/03, C-280/08), *Telefónica* (T-336/07, T-398/07 C-295/12), and *Telia-Sonera* (C-52/09).

The rationale for margin squeeze regulation is that protecting competitors in the context of monopolistic bottlenecks or concentrated input markets will ultimately benefit consumers. Particularly in competition policy the latter goal is emphasized and held in high regard, e.g., the European Commission (p.7 2009) clarifies that "what really matters is protecting an effective competitive process and not simply protecting competitors". Sector-specific regulation may widen the scope of application, as is illustrated by the debate about the relevant efficiency standard for the margin squeeze conduct (Geradin and O'Donoghue, 2005), but fundamentally still aims at the protection of consumers, where competition itself is a means to an end (Vogelsang, 2013).

In this vein, Jullien et al. (2014) provide an overview of the economic theories of harm that may qualify a margin squeeze as an abuse of market power and could provide the basis for a stand-alone antitrust doctrine. Petulowa and Saavedra (2014) qualify the circumstances under which a margin squeeze can occur in the case of differentiated goods and state that a margin squeeze is rather the result of competition and not of an exploitative abuse. Jullien et al. (2014) conclude that the effects of a margin squeeze rule are ambiguous as wholesale prices may decrease, but retail prices may also rise, due to a *price umbrella effect*. With regard to retail minus X regulation, Höffler and Schmidt (2008) criticize that its application may lead to consumer welfare losses and higher prices.

In the past, the margin squeeze rule has mostly been investigated in the case of a single access provider, as indispensability has initially constituted a central criterion in its application as an antitrust instrument. More recently, as illustrated by the *ex ante economic replicability test* in the European Commission's 2013a Recommendation on consistent non-discrimination, the margin squeeze test may also be applied to an environment with competing infrastructures (Jaunaux and Lebourges, 2015). While this rule is already applied in practice, little research is conducted with regard to actual consequences and the particular application context.

The experimental analysis in this study suggests that the margin squeeze rule is likely to be ineffective in lowering retail prices irrespective of the mode of wholesale competition. Although margin squeeze regulation may benefit the reseller in some circumstances, it tends to increase retail prices and thus reduces consumer surplus.

7.3 Experiment

The underlying experimental framework explicitly addresses the presented issues of wholesale competition and open access by incorporating an industry structure that allows for competition at the wholesale and retail level along the same value chain.

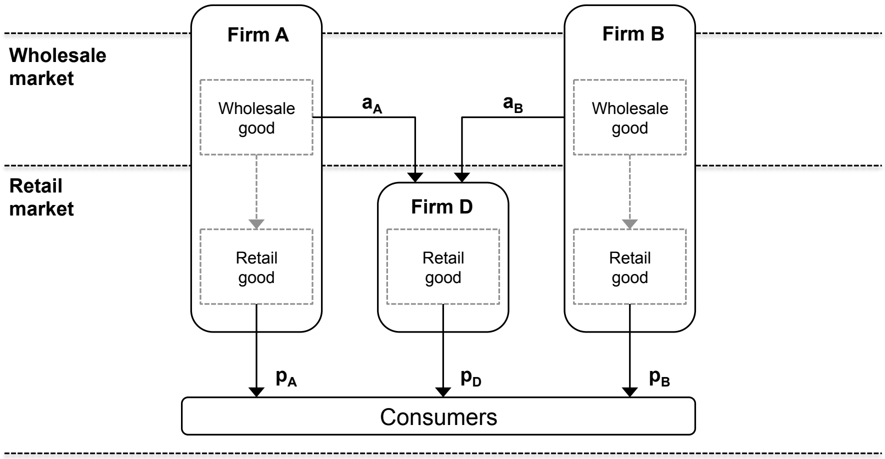
7.3.1 Theoretical model

The general experimental design is based on the model of upstream competition developed by Bourreau et al. (2011)—illustrated in Figure 7.1— in which two integrated firms (Firm A & Firm B) are able to supply the wholesale good, while a third firm (Firm D) operates only in the downstream market. In order to supply the retail good, the downstream reseller is required to purchase the wholesale good at the upstream market from one of the two integrated firms. The wholesale prices of Firm A and Firm B are denoted a_A and a_B, respectively. In the retail market, all firms choose their respective retail prices p_i, $i \in \{A, B, D\}$.

It is assumed that Firm D chooses the wholesale product with the lowest price and does not split its demand.[4] Thus, the integrated firms compete à la Bertrand with homogeneous goods. For each quantity that the downstream reseller supplies to consumers in the retail market it must buy an identical quantity of the wholesale good. In the downstream market, firms compete likewise in prices, but goods are differentiated. In line with previous theoretical studies on wholesale competition, competi-

[4]Note that, as the stage game is played repeatedly in the experiment, the following tie breaking rule is used: If Firm A and Firm B offer the same wholesale price, Firm D chooses to purchase access from the firm that has previously offered the lower price. If both integrated firms offer an identical wholesale price in the first period, the access provider is chosen randomly.

FIGURE 7.1: *Conceptual model of wholesale competition with two integrated firms and a non-integrated reseller (based on Bourreau et al., 2011, p. 683).*



tion in horizontally differentiated goods based on Shubik and Levitan (1980)[5] is assumed, where the retail demand of each firm i in the case of $n = 3$ active firms is given by $q_i = \frac{1}{3}(1 - p_i - \gamma(p_i - \frac{\sum_{j=1}^{3} p_j}{3}))$ and the differentiation parameter γ defines the degree of substitution between firms' retail goods. Across all treatments, $\gamma = 30$, which corresponds to a diversion ratio of $^{10}/_{21}$ for each pair-wise relationship between firms (Shapiro, 1996).

Throughout all experimental treatments, Firm D is modeled to mimic the behavior of a competitive fringe in the retail market that reacts to the price setting by the integrated firms, Firm A and Firm B. It is therefore assumed that Firm D always chooses the best-response retail price, i.e., the price that maximizes its profit given the wholesale and retail prices set by the integrated firms.

In the experiment, prices are scaled as follows: Values obtained by the Shubik and Levitan (1980) model are multiplied by $\phi = {}^{100}/_{0.15}$ and firms can decide for their prices on any integer in the range of zero to one hundred. In terms of the original Shubik and Levitan (1980) values, this corresponds to the price interval [0;0.15]. As a consequence, the joint profit among integrated firms' is maximized when integrated firms choose maximum prices in both the wholesale market ($a^{max} = 100$) and the retail market

[5]The experimental design follows Höffler (2008) with regard to the derivation of the demand structure and the active number of firms in the market. Therefore, consumers explicitly value variety.

($p^{max} = 100$). Therefore, the JPM outcome is identical across treatments. Moreover, the scaling allows for a more granular representation of the relevant price interval between the theoretically predicted competitive and collusive prices as well as the monopoly price in the case of a single access provider.

In contrast to the theoretical literature on wholesale competition, which usually prescribes a specific temporal sequence of actions, timing of price decisions is endogenized in the experiment by means of a continuous time framework. Endogenous timing of price setting has two aspects: First, no assumption is made on the sequence of upstream and downstream decisions. While it is frequently assumed that wholesale prices are set prior to retail prices (Bourreau et al., 2011), prices may also be chosen simultaneously (Nocke and White, 2007). Second, price setting of the integrated firms at a specific market level is equally unconstrained, i.e., these firms decide not only about the magnitude of a price, but also about timing when to change it. Therefore, the experimental design includes various time settings that are captured by the theoretical literature, but at the same time allows for a more general approach as it also incorporates additional settings that may arise endogenously. In consequence of the endogenous timing induced by the continuous time framework, multiple theoretical predictions may apply, depending on the specific temporal sequence of firms' actions. In order to provide a robust theoretical prediction, consider the following four alternative *timing models*, which are variants of either a sequential-move or a simultaneous-move game proposed in the theoretical literature:

(1) Two-stage game as suggested by Bourreau et al. (2011)[6]: First, integrated firms set their wholesale prices and the downstream reseller chooses its access provider. Second, all firms decide on their retail prices. Within each stage, firms set prices simultaneously.

[6]Note that, in contrast to Bourreau et al. (2011), in the experiment no assumption is made on that the downstream reseller will always be supplied by at least one integrated firm. Consequently, integrated firms may choose to set wholesale prices in excess of their own retail prices and consequently foreclose the reseller from the downstream market, which implies $q_D = 0$.

TABLE 7.1: *Predicted wholesale and retail price differences between industry structures.*

	No regulation		Margin squeeze regulation
Wholesale monopoly	*Monopoly outcome* or *Foreclosure*	$\geq$ $\leq$	*Constrained monopoly outcome*
	$\geq$		$>$
Wholesale competition	*Competitive outcome* or *Foreclosure*	$\geq$	*Competitive outcome*

(2) Three-stage Stackelberg game: Same as Timing Model (1) with the exception that the downstream reseller chooses its retail price in a third stage, i.e., after the integrated firms have chosen their respective retail prices.

(3) Simultaneous-move game as assumed by Nocke and White (2007): All firms set their prices, both wholesale and retail, simultaneously.

(4) Two-stage Stackelberg game: Same as Timing Model (3) with the exception that the downstream reseller chooses its retail price in a second stage, i.e., after the integrated firms have chosen their prices.

Table 7.1 depicts ordinal differences in theoretical equilibrium predictions of wholesale and retail prices for all four timing models. Note that the hypotheses regarding the direction of a price difference hold equally for wholesale and retail prices of each individual firm. Although the timing models vary with regard to the specific numerical predictions for equilibrium prices in the investigated scenarios, the direction of price effects between scenarios align—with one exception that is discussed below.[7]

In order to allow for a benchmark for the evaluation of wholesale competition, consider first the market outcome under a wholesale monopoly. In this scenario only Firm A offers a wholesale price and may provide the wholesale good to the reseller Firm D. By contrast, Firm B relies on its vertically integrated structure to produce its own wholesale good, but does not offer access to its wholesale resource. In the absence

[7]See Appendix B.3 for the complete analysis and a comprehensive comparison of all models.

of regulation, given $\gamma = 30$, Firm A is expected to set the wholesale price either at the wholesale monopoly level or such that the reseller is foreclosed from the downstream market. As shown in Appendix B.3, the latter outcome arises if the reseller's price reaction is not explicitly anticipated by the monopolistic wholesale provider, i.e., in Timing Models (1) and (3). The introduction of margin squeeze regulation changes the equilibrium outcome only if equilibrium prices of Firm A under no regulation violate the margin squeeze condition $a_A \leq p_A$. Whereas margin squeeze regulation is expected to decrease prices compared to the case of foreclosure under no regulation, it instead increases wholesale and retail prices for all firms according to the theoretical prediction in Timing Model (2). In sum, theoretical predictions on whether the implementation of margin squeeze regulation decreases prices in a wholesale monopoly are ambiguous.

In the case of wholesale competition, it is straightforward that symmetric marginal cost pricing, i.e., $a_A = a_B = 0$, is a Nash equilibrium, as is shown by Bourreau et al. (2011). The corresponding equilibrium retail prices are thus symmetric for all three firms. In Timing Models (2) and (4) this equilibrium is unique, because integrated firms anticipate that Firm D as a follower can only act as a price taker and therefore find it always profitable to make a viable wholesale offer.[8] In contrast, in Timing Models (1) and (3), there exists a second foreclosure equilibrium in which both integrated firms decide not to offer a viable wholesale price to the reseller, i.e., the reseller does not supply any retail consumers (Atiyas et al., 2015). Introducing margin squeeze regulation in the case of wholesale competition renders foreclosure impossible, thus, the competitive equilibrium remains as the unique predicted outcome in all presented timing models. In conclusion and in line with previous theoretical analyses, prices under wholesale competition are likely to be below prices in a wholesale monopoly and never exceed them across all model variants.

[8]Hence, modelling the reseller as a follower in a Stackelberg retail setting may be viewed as an alternative implementation of the a priori assumption made by Bourreau et al. (2011) which guarantees that the integrated firms have no incentive to foreclose the reseller.

7.3.2 Design

The experimental design is based on a continuous time framework in which participants can observe competitors' price changes immediately and market variables are updated in real time. Similar designs have recently been used in experimental economics, e.g., in the context of a prisoner's dilemma game (Bigoni et al., 2015a; Friedman and Oprea, 2012) as well as in a Hotelling setting (Kephart and Friedman, 2015b). Next to its property to endogenize the timing of the game and thereby to reconcile different timings proposed in the theoretical literature, the continuous time framework is chosen for the following reasons. First, continuous time is conjectured to promote the emergence of a theoretical prediction in complex industry settings (Kephart and Friedman, 2015b; Kephart and Rose, 2015). Second, under both Cournot as well as Bertrand competition, the extent of tacit collusion that emerges under continuous time and discrete time is systematically compared in Chapter 3 and lower levels of tacit collusion are found in continuous time for both competition models. Therefore, the continuous time framework offers a more conservative experimental test of the robustness of theory than a discrete time framework in the present context. Third, through the continuous feedback loop subjects can directly assess the interdependency between prices in the wholesale and retail market, which aids them in evaluating the impact of their decisions on their individual performance and on aggregate market outcomes.

The experiment is computerized using *Brownie*, a newly-developed experimental software (Müller and Normann, 2014) based on the *Java* programming language. The course of the experiment is separated in two phases: the trial phase and the game phase. During the trial phase subjects are able to test various price configurations for all firms in the industry and to observe the resulting payoffs, while these actions do not impact the subjects' earnings and are not visible to other participants, i.e., the subjects do *not* interact with each other during the trial phase. The game phase, which starts after all subjects confirm their initial prices in the trial phase, lasts for exactly 30 minutes. All decisions in the game phase directly impact the monetary payoff of the subjects. Earnings are the cumulative profits over the time horizon of the experiment. Current profits and cumulative earnings are displayed to subjects over the entire game phase.

As motivated above, the integrated firms, Firm A and Firm B, are represented by human subjects while the downstream reseller, Firm D, is represented by an automated software agent. The agent is programmed to constantly choose its profit-maximizing price given the wholesale and retail prices set by the integrated firms. Thereby, the software agent reacts immediately to any price change made by one of the other firms. In this setup, the experiment covers the following three modes of wholesale competition and two regulatory open access regimes in a full-factorial manner, thus ensuing six treatments (see Table 7.2):

Wholesale monopoly (WM): Only Firm A sets a wholesale price and can change it at any time. Firm B does not participate in the wholesale market.

Wholesale competition (WC): Firm A and Firm B set and can change wholesale prices at any time.

Wholesale competition with price commitment (WCPC): Firm A and Firm B set wholesale prices, however, each firm's wholesale price is fixed for an embargo period of 30 seconds after it is changed, everything else being equal to WC.

No regulation (NR): Firms set wholesale and retail prices freely.

Margin squeeze regulation (MSR): Firm A and Firm B may set neither their wholesale price above their own retail price nor their retail price below their own wholesale price. If firms set wholesale (retail) prices that violate these conditions, the experimental software displays a warning and sets the price to the allowed maximum (minimum), which is the current own retail (wholesale) price.

TABLE 7.2: *Full-factorial treatment design.*

	No regulation	Margin squeeze regulation
Wholesale monopoly	WM-NR	WM-MSR
Wholesale competition	WC-NR	WC-MSR
Wholesale competition with price commitment	WCPC-NR	WCPC-MSR

7.3.3 Procedures

The experimental sessions were conducted with students of economics fields at the Karlsruhe Institute of Technology in Karlsruhe, Germany. Student subjects were recruited via the *ORSEE* platform (Greiner, 2015). Overall, 128 subjects participated in the study and each participant played only one of the treatments (between-subject design). The average experimental session lasted 70 minutes. On average, subjects earned a performance-based payment of EUR 16.80 in addition to a base fee of EUR 5.00. Participants were randomly assigned to cohorts of two and interacted with the same firm for the entire time horizon of the experiment (fixed partner matching). Consequently, there are 64 independent observations at the cohort level, i.e., at the level of independent cohorts of subjects, as denoted by Table 7.2. The current market data is recorded every 500 ms, thus, there are 3,600 data tuples per market that include wholesale and retail prices as well as the corresponding quantities and profits.

While the main analyses and results focus on the student sample, there is also a complementary validation study for the WCPC-NR treatment with 16 industry professionals in an effort to address external validity concerns. The professionals were recruited from the regulatory department of a major German telecommunications operator, where they deal with issues of access regulation on a daily basis. The study was executed under identical conditions as in student experiments with three exceptions. First, the duration of the game phase is only ten minutes. Second, the payment scheme a lottery system, where participants can win one of three vouchers with a monetary value of EUR 30 each. The number of lottery tickets that participants receive depends on their payoff in the experiment. By this means, monotonicity is ensured with regard to the relationship between individual performance and payoffs. Third, each participant plays a second WCPC-NR treatment with a more differentiated retail market ($\gamma = 50$). The sequence of the two treatments is randomized across three experimental sessions.

All experimental sessions with students as well as industry professionals were conducted with the same experimental software and hardware in order to ensure consistency, particularly with regard to the graphical user interface. Each session was run according to the following protocol. Upon entering the laboratory, subjects are randomly assigned to a seat, from which they can neither see nor speak to any other participant of the experiment. Subsequently, the experimental instructions are handed to the participants in print and read aloud from a recording.[9] Paragraphs that are identical across treatments are recorded once and the recording is used in all treatments. Prior to the beginning of the experiment, each subject has to complete a computerized comprehension test that includes a set of questions regarding the experimental instructions and the experimental procedure. Participants are allowed to proceed to the next question only after entering the correct answer to the current one. After all subjects successfully complete the test, the experiment starts automatically. In addition to this procedure, student participants wore ear protectors from the beginning of the questionnaire until the end of the game phase in order to avoid any influence from clicking noises of computer mouses.

7.4 Results

In the following, market prices, firms' profits, and consumer welfare are evaluated across treatments for the main study with students and for the validation study with industry professionals. The wholesale market price a_m is given by the wholesale price that the entrant faces, i.e., the minimum of both wholesale offers. The retail market price τ_m is defined as the transaction price, which is the demand-weighted average of retail prices, i.e., $\tau_m = \sum_i \frac{q_i}{Q} \cdot p_i$ where Q is the aggregate market demand. Profits are given by the amount of money that participants earn during the game phase, i.e., the final payoff

[9]See Appendix C.5 for exemplary instructions for the WCPC-MSR treatment.

excluding the fixed base fee. The average profit of both integrated firms is denoted by π_{AB} and the profit of the downstream reseller by π_D. Consumer surplus is computed as the utility of a representative consumer given the supplied quantities of all three firms subtracted by the transaction price, i.e., $CS = \sum_i q_i - \frac{3}{2(1+\gamma)}(\sum_i q_i^2 + \frac{\gamma}{3}(\sum_i q_i)^2) - \sum_i q_i p_i$ (see Bouckaert and Kort, 2014, for a detailed derivation). For ease of interpretation consumer surplus is standardized as $\widetilde{CS} = \frac{CS - CS^{min}}{CS^{max} - CS^{min}}$ on the interval of eligible prices, i.e., $p_i \in [0,100]$. Thus, $\widetilde{CS} = 0$ denotes the minimum consumer surplus at $p_i = 100$, while $\widetilde{CS} = 1$ represents the maximum consumer surplus at $p_i = 0$. For a focus on market outcomes in a stable market environment and due to the complexity of the experiment start- and endgame effects are neglected by considering only the market data from recorded ticks 601 to 3,000 with 1 tick corresponding to 500 ms, i.e., the first five and last five minutes are dropped for the subsequent analysis. For the same reasons, the analysis is based on medians as this mitigates the impact of outliers in comparison to averages and should therefore provide a more conservative analysis (see, e.g., Friedman and Oprea (2012) for an identical approach in a continuous time experiment). Arguably, regulators and policy makers should be more interested in the *median* outcome that can be expected from a single scenario than the *average* effect across multiple co-existing scenarios.[10]

7.4.1 Main study

Table 7.3 presents the treatment medians of median values at the cohort level for market prices, firms' profits, and consumer surplus together with the number of independent markets and the number of partially dependent observed time ticks for the main study with students. In addition, Figure 7.2 depicts the period medians of wholesale and retail market prices across individual markets for each of the six treatment combinations. For purposes of illustration, every point in the graphs of Figure 7.2 is a median over 50 subsequent ticks.

[10]Nevertheless, a similar analysis based on means rather than on medians leads to comparable results as reported in Tables D.5 and D.6 as well as Figure D.9 in Appendix D.5.

FIGURE 7.2: *Median wholesale (dashed) and retail (solid) market prices over time across treatments.*

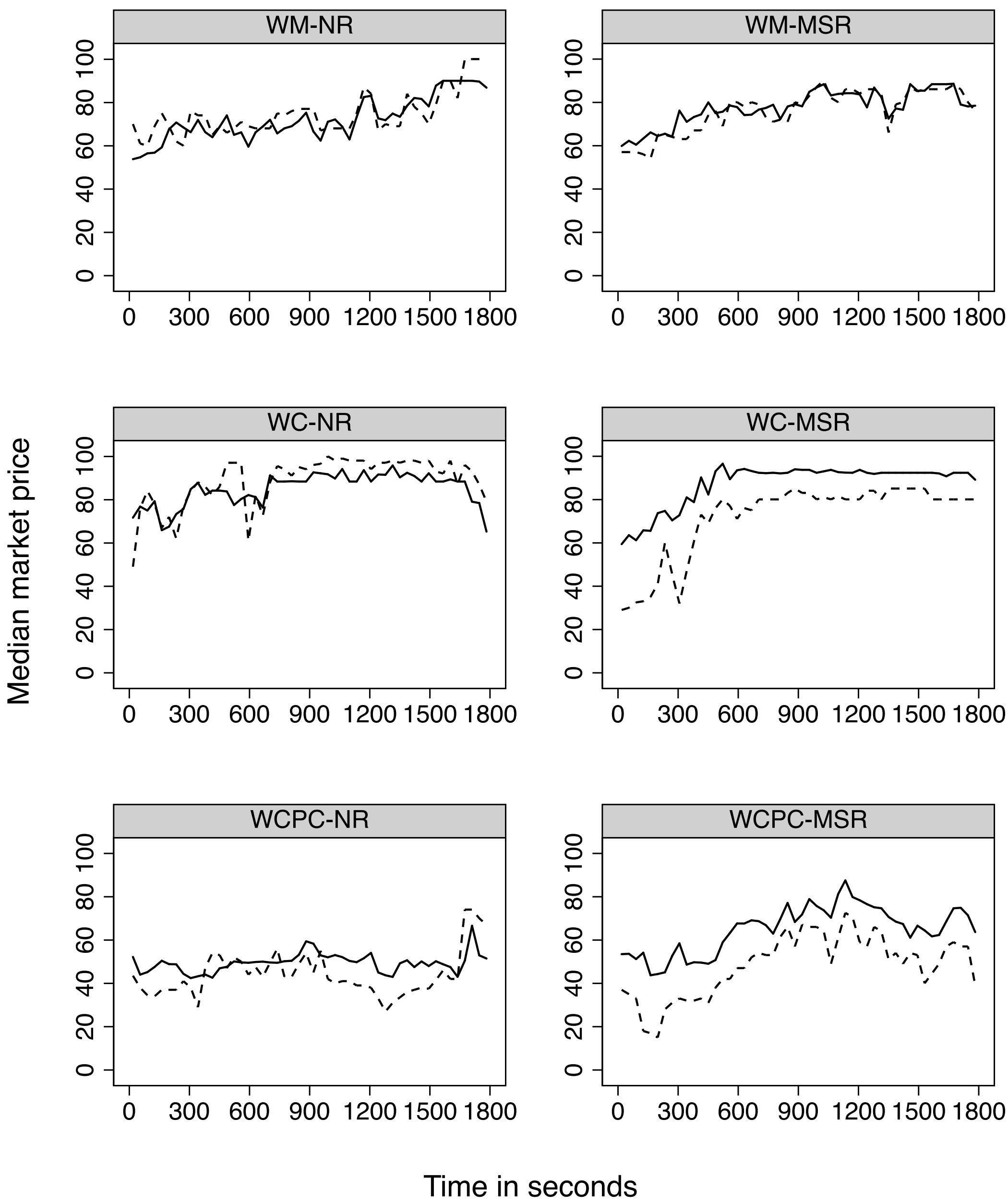


TABLE 7.3: *Median market prices, profits, and consumer surplus across treatments.*

Treatment	N	Tuples	a_m	τ_m	π_{AB}	π_D	$\widetilde{CS}$
WM-NR	12	28,800	73.573	65.499	16.383	0.899	0.292
WM-MSR	11	26,400	72.572	83.124	20.750	1.028	0.153
WC-NR	9	21,600	86.085	88.407	22.434	0.258	0.097
WC-MSR	10	24,000	83.082	92.281	22.802	2.339	0.062
WCPC-NR	10	24,000	40.540	49.560	12.243	2.491	0.461
WCPC-MSR	12	28,800	49.049	67.415	15.866	2.322	0.298
Total	64	153,600	72.071	76.159	18.093	1.672	0.213

Medians are based on minutes [5,25] of the game phase.

In order to evaluate treatment effects statistically, consider the following quantile regression (Koenker and Hallock, 2001):

$$\begin{aligned} Y_{k,t} = \beta_0 \\ &+ \beta_{WC} \cdot WC \\ &+ \beta_{WCPC} \cdot WCPC \\ &+ \beta_{MSR} \cdot MSR \\ &+ \beta_{WC \text{ x } MSR} \cdot WC \cdot MSR \\ &+ \beta_{WCPC \text{ x } MSR} \cdot WCPC \cdot MSR \\ &+ \beta_{Period} \cdot t \\ &+ \epsilon_{k,t}, \end{aligned}$$

where $Y_{k,t}$ denotes the respective market variable $Y \in \{a_m, \tau_m, \pi_{AB}, \pi_D, \widetilde{CS}\}$ in cohort k and period t. Treatment WM-NR is adopted as the baseline.[11] WC, WCPC, and MSR are dummy variables indicating the respective mode of wholesale market structure and open access regulation. Interactions WC x MSR and WCPC x MSR delineate the effects of margin squeeze regulation under a specific mode of wholesale competition. Standard errors are clustered on cohort level to control for intra-cluster correlation over repeated observations from periods in the same cohort (Parente and Santos Silva, 2015). The estimates of the respective models for market variables of interest, i.e., wholesale market price a_m, retail market price τ_m, integrated firms' average profit π_{AB}, reseller's

[11]Pairwise comparisons between treatments by means of quantile regressions are reported in Tables D.7, D.8, D.9, D.10, and D.11 in Appendix D.5.

TABLE 7.4: *Quantile regressions of market variables on industry structures.*

Covariate	(1) a_m	(2) τ_m	(3) π_{AB}	(4) π_D	(5) $\widetilde{CS}$
WC	23.879**	18.376*	4.839***	−0.813	−0.162**
	(10.073)	(9.738)	(1.830)	(0.815)	(0.080)
WCPC	−28.154***	−19.763**	−5.459***	1.130	0.212***
	(8.851)	(9.130)	(1.729)	(0.861)	(0.077)
MSR	9.401	9.721	2.358	0.173	−0.081
	(10.812)	(9.927)	(1.827)	(0.798)	(0.078)
WC x MSR	−24.309*	−7.156	−2.562	1.924**	0.060
	(13.731)	(12.170)	(2.403)	(0.926)	(0.100)
WCPC x MSR	−1.928	7.250	1.759	0.019	−0.088
	(17.188)	(12.692)	(2.774)	(0.930)	(0.116)
Period	0.004	0.004**	0.001*	> -0.001	> -0.001**
	(0.003)	(0.002)	(< 0.001)	(< 0.001)	(< 0.001)
Constant	62.893***	62.972***	16.251***	1.324*	0.319***
	(9.764)	(9.148)	(1.931)	(0.769)	(0.080)
Observations	153,600	153,600	153,600	153,600	153,600

Clustered standard errors (by cohort) in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

profit π_D, and consumer surplus $\widetilde{CS}$, are reported in Table 7.4 and interpreted in the following.

In the benchmark case WM-NR the wholesale monopolist sets a positive wholesale price as is indicated by the estimated constant in Model (1), which is similar in magnitude to the retail market price as reported in Model (2). This is in line with the observation that margin squeezes occur frequently, such that the non-integrated firm is *effectively foreclosed*, i.e., the wholesale market price is set above the retail prices of both integrated firms.[12] The median rate of foreclosure at the individual cohort level amounts to 49.52% in this scenario. Still, the profit of Firm D is found to be significantly different from zero, as indicated by the positive constant in Model (4). In other words, even in the case of an unregulated wholesale monopoly, the downstream reseller can profitably participate in the retail market. Due to its access monopoly, Firm A achieves a significantly ($p < 0.01$) higher median profit ($\pi_A = 19.031$) than its integrated com-

[12]Note that the non-integrated firm may still be marginally active in the retail market, since goods are differentiated.

petitor ($\pi_B = 14.294$).[13] Note that all of these results are in line with the theoretical prediction.

RESULT 7.1. *The introduction of wholesale competition reduces neither wholesale prices nor retail prices. In fact, under homogeneous Bertrand competition at the wholesale level consumers as well as the downstream reseller are worse off compared to the case of an unregulated wholesale monopoly.*

Surprisingly, relative to an unregulated wholesale monopoly, the introduction of homogeneous Bertrand competition at the wholesale level increases both wholesale and retail market prices significantly. While under WC-NR the wholesale market price rises by 24 percentage points (pp), consumers face an 18 pp higher retail market price in comparison to WM-NR. Although it is well-known that Bertrand competition yields supra-competitive prices, it is notable that under WC-NR prices are set even significantly above price levels of WM-NR. In consequence, the ability to tacitly collude in the wholesale market allows the integrated firms to extract higher profits than in the monopoly treatment as indicated in Model (3). While the effect on the reseller's profit is negative but insignificant, the median rate of foreclosure is 62.46%, and thus higher than under WM-NR.

RESULT 7.2. *Competition in the wholesale market can be stimulated by introducing competition for the market through a price commitment. Then, wholesale and retail prices are lower than under a wholesale monopoly, but remain above the theoretical prediction.*

Remarkably, the collusive effect of wholesale competition is set off by a simple wholesale price commitment for integrated firms. In particular, under WCPC-NR the wholesale market price decreases significantly by 28 pp (52 pp) relative to WM-NR (WC-NR), while the transaction price in the retail market is lowered significantly by almost 20 pp (38 pp). As a result, consumers' surplus increases significantly by 21 pp compared to

[13]See Table D.12 in Appendix D.5 for estimates of the quantile regression model $\pi_{k,i,t} = \beta_0 + \beta_{Firm\,A} \cdot Firm\,A + \beta_{Period} \cdot t + \epsilon_{k,i,t}$ which investigates the effect of the access provider role on Firm A's profit compared to Firm B's profit.

WM-NR as indicated by Model (5). In line with declining market prices, the integrated firms' profits decrease significantly as well. Despite lower wholesale prices, the margin between wholesale and retail prices remains relatively slim due to the increasing price competition at the retail level as is depicted by the lower left panel in Figure 7.2. In consequence, the median rate of foreclosure amounts to 29.10%. The effect on the reseller's profit is found to be insignificant, although positive in absolute terms. Evidently, the estimated wholesale access price of $62.893 - 28.154 = 34.739$ under WCPC-NR remains well above the theoretical prediction of $a_m = 0$.

RESULT 7.3. *There is no evidence that margin squeeze regulation reduces retail prices, and thus consumers do not benefit from such a regulation. However, the introduction of a margin squeeze regulation may reduce wholesale prices, and thus the downstream reseller may be better off.*

As reported above, margin squeezes are frequently observed under all market structures at the wholesale level. Since the primary justification for margin squeeze regulation is the prevention of exclusionary and exploitative abuses (Jullien et al., 2014), its impact on prices and market outcomes is examined in the following. The regression analyses reported in Table 7.4 reveal that margin squeeze regulation generally does not have a significant impact on market outcomes, but rather tends to increase wholesale and retail prices. In fact, the only reduction in wholesale prices evoked by margin squeeze regulation is found in the case of a particularly collusive wholesale market as under unregulated wholesale competition. More specific, the wholesale price under WC-MSR is significantly lower than under WC-NR, which is indicated visually by the middle panels in Figure 7.2 and supported empirically by the significant interaction effect WC x MSR. Although this effect is paralleled by an increase in the reseller's profit, margin squeeze regulation translates neither into significantly lower retail prices nor into significantly higher consumer surplus. Taken together, the empirical results do not provide any evidence that consumers or the reseller generally benefit from a margin squeeze regulation.

TABLE 7.5: *Quantile regressions of market variables on margin squeeze regulation under wholesale monopoly.*

Covariate	(1) a_m	(2) τ_m	(3) π_{AB}	(4) π_D	(5) $\widetilde{CS}$
MSR	8.286	9.928	2.661	0.165	-0.083
	(15.569)	(8.354)	(1.786)	(0.784)	(0.083)
Period	0.006	0.006***	0.001***	> -0.001	> -0.001*
	(0.007)	(0.002)	(0.001)	(< 0.001)	(< 0.001)
Constant	60.290***	59.911***	15.112***	1.287*	0.338***
	(11.192)	(7.423)	(1.991)	(0.703)	(0.096)
Observations	55,200	55,200	55,200	55,200	55,200

Clustered standard errors (by cohort) in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

In an effort to further investigate the impact of the margin squeeze regulation and to delineate effects on stakeholders under different wholesale competition models, consider additionally the following (reduced) quantile regression model for each of the wholesale market structures separately to allow for a pairwise comparison:

$$Y_{k,t} = \beta_0 + \beta_{MSR} \cdot MSR + \beta_{Period} \cdot t + \epsilon_{k,t}.$$

This analysis is of particular interest whenever policymakers are able to prescribe rules that govern the competition at the wholesale level but may find themselves unable to change the market structure completely. In these cases the margin squeeze condition may be considered as an ex ante regulatory remedy or as an ex post competition policy instrument. The effect of margin squeeze regulation is therefore examined under all three considered wholesale market structures. First, in the case of a wholesale monopoly (Table 7.5), margin squeeze regulation has a positive yet insignificant effect for all price and profit variables, while the corresponding coefficient for consumer surplus is negative and insignificant. In line with the theoretical prediction, margin squeeze regulation therefore does not seem to represent a suitable safeguard for effective competition nor a beneficiary instrument for consumers in the case of a wholesale monopoly when an integrated competitor is present.

TABLE 7.6: *Quantile regressions of market variables on margin squeeze regulation under wholesale competition without price commitment.*

Covariate	(1) a_m	(2) τ_m	(3) π_{AB}	(4) π_D	(5) $\widetilde{CS}$
MSR	-14.632^{*}	3.909	-0.149	2.051^{***}	-0.036
	(8.254)	(3.528)	(1.034)	(0.520)	(0.030)
Period	0.005	< 0.001	< 0.001	> -0.001	> -0.001
	(0.005)	(0.002)	(< 0.001)	(< 0.001)	(< 0.001)
Constant	85.979^{***}	88.125^{***}	22.068^{***}	0.650	0.105^{**}
	(15.948)	(5.347)	(1.638)	(0.869)	(0.045)
Observations	45,600	45,600	45,600	45,600	45,600

Clustered standard errors (by cohort) in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE 7.7: *Quantile regressions of market variables on margin squeeze regulation under wholesale competition with price commitment.*

Covariate	(1) a_m	(2) τ_m	(3) π_{AB}	(4) π_D	(5) $\widetilde{CS}$
MSR	7.983	17.081^{**}	4.157^{*}	0.188	-0.165^{**}
	(10.883)	(8.071)	(2.251)	(0.474)	(0.080)
Period	0.003	0.006^{*}	0.001	> -0.001	$> -0.001^{*}$
	(0.004)	(0.003)	(0.001)	(< 0.001)	(< 0.001)
Constant	37.475^{***}	39.162^{***}	10.374^{***}	2.335^{***}	0.572^{***}
	(8.131)	(5.664)	(1.921)	(0.609)	(0.058)
Observations	52,800	52,800	52,800	52,800	52,800

Clustered standard errors (by cohort) in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Second, in the case of wholesale competition (Table 7.6), margin squeeze regulation instead significantly reduces the wholesale market price, which resembles the net effect of the general margin squeeze impact, MSR, and the interaction effect WC x MSR reported in Table 7.4. The pairwise comparison likewise confirms the positive and significant impact on the reseller's profit compared to the unregulated regime. Again, there is no significant negative impact on the retail market price. Accordingly, the effect on consumer welfare is also insignificant. Therefore, it is concluded that the decline of the wholesale market price that results from margin squeeze regulation allows the reseller to increase its profitability, but retail prices do not decrease proportionately and hence, consumers are not better off.

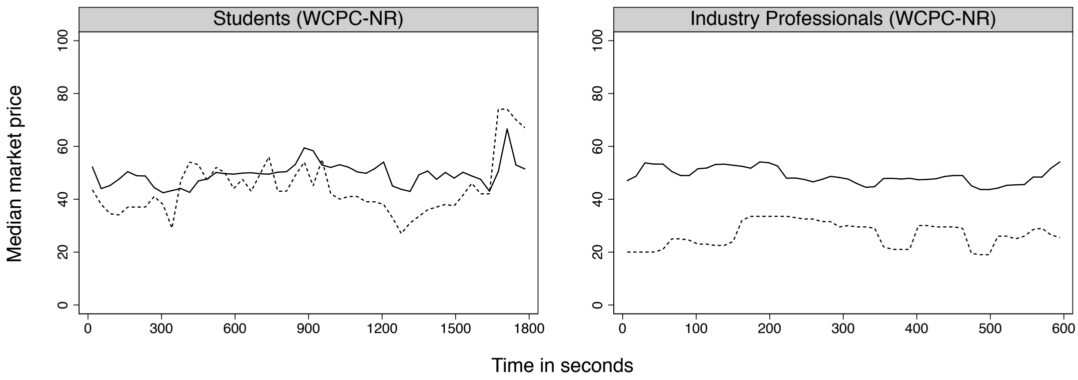
Third, in the case of wholesale competition with price commitment, a positive and significant effect on the retail market price (Table 7.7) advises further skepticism with regard to margin squeeze regulation and its impact on consumers. The magnitude of the relative price increase is estimated as 17 pp. The price increase benefits the integrated firms by means of significantly higher profits, while the effect on the downstream reseller's profit is insignificant. In sum, a margin squeeze regulation is clearly detrimental to consumers' interest in this scenario as consumer surplus decreases significantly by almost 17 pp and may therefore even offset the gains from wholesale competition. For completeness, a summary of all other pairwise comparisons between the treatments by means of quantile regressions is arranged in Appendix D.5.

7.4.2 Validation study

Figure 7.3 illustrates the median wholesale and retail prices under WCPC-NR both for the students treatment (left-hand panel) and for the industry professionals treatment (right-hand panel). Again, medians over 50 subsequent ticks are plotted for purposes of illustration. While wholesale market prices of professionals are lower according to the median value over all periods ($a_m^{Students} = 43.043$, $a_m^{Professionals} = 29.029$), retail market prices are almost identical ($\tau_m^{Students} = 50.326$, $\tau_m^{Professionals} = 50.613$). Note that for both subject pools wholesale prices are bounded away from zero which is the theoretical prediction.

Consider first market outcomes between industry professionals and students over the entire time horizon. In particular, the null hypothesis is that the median market prices in the students sample and the median market prices in the professionals sample are from populations with the same distribution. According to two-tailed non-parametric Mann-Whitney U tests there is no significant difference in wholesale market prices ($z = 1.42$, $p = 0.155$) nor in retail market prices ($z = 0.71$, $p = 0.477$). Also with respect to overall medians, i.e., the median of market medians, Fisher's exact test does not reject the equality of median market prices at the wholesale level ($p = 0.637$) or the retail level

FIGURE 7.3: *Median wholesale (dashed) and retail (solid) market prices over time of students and industry professionals.*



($p = 1.0$). Finally, the same result is obtained by a quantile regression that investigates the differences of the subject pools while controlling for the time trend and intra-cluster correlation, i.e.,

$$Y_{k,t} = \beta_0 + \beta_{Professionals} \cdot Professionals + \beta_{Period} \cdot t + \epsilon_{k,t}.$$

In order to obtain a comparable data basis with an equivalent number of periods for the industry professionals and students treatments, the measures of the students treatments are averaged over three subsequent 500 ms intervals. As shown in Table 7.8, the effect of the industry professionals subject pool is insignificant for all market variables except the reseller's profit. The higher profit of the entrant can be attributed to a larger spread between wholesale and retail prices in a subset of individual markets in the professionals treatment, which is also indicated by the negative coefficient for the median wholesale market price.

Naturally, general and conclusive evidence cannot be derived based on findings of statistical insignificance. However, in addition to the finding of statistical indifference, descriptive measures as portrayed in Figure 7.3 show quantitatively similar and qualitatively equal behavior for both subject pools.

TABLE 7.8: *Quantile regressions of market variables on subject type under wholesale competition with price commitment.*

Covariate	(1) a_m	(2) τ_m	(3) π_{AB}	(4) π_D	(5) $\widetilde{CS}$
Professionals	-14.465	1.170	-0.613	1.839***	0.003
	(10.259)	(10.908)	(2.923)	(0.600)	(0.126)
Period	0.001	0.003	0.001	< 0.001	> -0.001
	(0.011)	(0.008)	(0.002)	(< 0.001)	(< 0.001)
Constant	42.297***	48.270***	11.974***	2.043***	0.477***
	(9.791)	(6.920)	(2.179)	(0.427)	(0.078)
Observations	21,618	21,618	21,618	21,618	21,618

Clustered standard errors (by cohort) in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

7.5 Infinitely repeated game

In an effort to relate the empirical findings of the experiment to the four timing models introduced in Section 7.3, their theoretical predictions are considered in a repeated game context. Thereby, a comparison to observed experimental results may reveal which of the timing models best captures the idiosyncrasies of endogenous timing under the continuous time framework. Considering the benchmark scenario of an unregulated wholesale monopoly, observed wholesale prices suggest that the wholesale provider does generally not foreclose the downstream reseller, but rather charges the monopolistic wholesale price. Moreover, there is no evidence that margin squeeze regulation reduces wholesale prices in the monopoly scenario. Both observations are in line with predictions by Timing Models (2) and (4) and contradict predictions by Timing Models (1) and (3). This may be considered as support for the experimental design since it is in line with the intention to model the non-integrated reseller as a competitive fringe, whose reaction is immediate, but subsequent and anticipated by the integrated firms. Furthermore, median prices for all wholesale competition treatments are significantly above the competitive outcome, which is an equilibrium in all timing models. More specific, the significant increase in wholesale and retail prices from wholesale monopoly to wholesale competition contradicts the consensus prediction. Whereas wholesale prices close to $a_{max} = 100$ may be interpreted as an indication for the foreclosure outcome, which is predicted by Timing Models (1) and (3), observed retail prices close to $p_{max} = 100$ are predicted for the JPM outcome and for the foreclosure outcome. This suggests the presence of substantial tacit collusion among integrated firms in the wholesale competition scenario.

Although experiments in continuous time are thus far primarily used to consider static one-shot games (Friedman and Oprea, 2012; Bigoni et al., 2015a; Kephart and Friedman, 2015b), continuous time may also be interpreted as an infinite repetition of a one-shot game as described by the timing models. In this context, the incentives to tacitly collude in the upstream market can be compared in the spirit of Nocke and White (2007) and Normann (2009) with respect to the critical discount factor that is required to sustain collusive outcomes. Assuming a grim trigger strategy, deviations from JPM prices a_{max} and p_{max} are punished by infinite play of the competitive Nash equilibrium (cf. Nocke and White, 2007). Individual discount factors that support collusive behavior are computed by $\delta_i = \frac{\pi_i^{Deviate} - \pi_i^{JPM}}{\pi^{Deviate} - \pi_i^{Punish}}$ for $i \in \{A, B\}$, where π_i^{JPM} is firm i's share of the JPM profit, $\pi_i^{Deviate}$ is the maximum deviation profit that firm i can achieve by unilateral deviation, and π_i^{Punish} is firm i's profit in periods after deviation (cf. Normann, 2009). The minimum critical discount factor is then given by $\delta = \max\{\delta_A, \delta_B\}$. Although experiments in continuous time are thus far primarily used to consider static one-shot games (Friedman and Oprea, 2012; Bigoni et al., 2015a; Kephart and Friedman, 2015b), continuous time may also be interpreted as an infinite repetition of a one-shot game as described by the timing models. In this context, the incentives to tacitly collude in the upstream market can be compared in the spirit of Nocke and White (2007) and Normann (2009) with respect to the critical discount factor that is required to sustain collusive outcomes. Assuming a grim trigger strategy, deviations from JPM prices a_{max} and p_{max} are punished by infinite play of the competitive Nash equilibrium (cf. Nocke and White, 2007). Individual discount factors that support collusive behavior are computed by $\delta_i = \frac{\pi_i^{Deviate} - \pi_i^{JPM}}{\pi^{Deviate} - \pi_i^{Punish}}$ for $i \in \{A, B\}$, where π_i^{JPM} is firm i's share of the JPM profit, $\pi_i^{Deviate}$ is the maximum deviation profit that firm i can achieve by unilateral deviation, and π_i^{Punish} is firm i's profit in periods after deviation (cf. Normann, 2009). The minimum critical discount factor is then given by $\delta = \max\{\delta_A, \delta_B\}$.

TABLE 7.9: *Critical discount factors under wholesale monopoly.*

	Firm A				Firm B			
Timing Model	π_A^{JPM}	$\pi_A^{Deviate}$	π_A^{Punish}	δ_A	π_B^{JPM}	$\pi_B^{Deviate}$	π_B^{Punish}	δ_B
(1)	34.00	35.02	15.04	0.05	17.00	25.40	15.04	0.81
(2)	34.00	35.02	15.61	0.05	17.00	25.40	14.05	0.74
(3)	34.00	35.02	15.04	0.05	17.00	25.40	15.04	0.81
(4)	34.00	35.02	15.07	0.05	17.00	25.40	11.86	0.62

TABLE 7.10: *Critical discount factors under wholesale competition.*

	Firm A				Firm B			
Timing Model	π_A^{JPM}	$\pi_A^{Deviate}$	π_A^{Punish}	δ_A	π_B^{JPM}	$\pi_B^{Deviate}$	π_B^{Punish}	δ_B
(1), (2), (3), (4)	25.50	30.21	5.79	0.19	25.50	30.21	5.79	0.19

Following this approach, the minimum critical discount factors can be computed for wholesale monopoly and competition. Table 7.9 denotes the respective profits and discount factors for both integrated firms in case of a wholesale monopoly for each timing model. Likewise, Table 7.10 states profits and discount factors under wholesale competition. Here, critical discount factors are identical across all timing models, because in each model punishment is exercised through the competitive equilibrium. Moreover, integrated firms' critical discount factors under wholesale competition are symmetric because collusive and deviation profits are calculated as expected values, i.e., firms expect to be the access provider with probability one half.[14]

Pairwise comparisons of minimum critical discount factors under wholesale monopoly and competition show that collusion is sustainable for a larger range of discount factors under wholesale competition, independent of the assumed timing of the one-shot game. More specifically, Firm B has a stronger incentive to deviate in the case of a wholesale monopoly, because foregone profits in the case of punishment are relatively low compared to its JPM profit share. In contrast, in the case of wholesale competition, expected JPM profits are higher, while profits in the case of punishment are lower, thus making a deviation less attractive. Therefore, tacit collusion is less likely in the

[14] Alternatively, one may assume that firms gain the entire wholesale profit if they deviate. Irrespective, the ensuing minimum critical discount factor $\delta = 0.43$ is still lower than the ones reported in Table 7.9.

wholesale monopoly setting than in the wholesale duopoly setting. This may provide a theoretical rationale for Result 7.1.

However, notice that this does not provide a rationale for Result 7.2, because the same theoretical analysis applies to the case of wholesale competition with price commitment. To see this, consider price commitment to induce sequential-move rather than simultaneous-move interaction between the integrated firms regarding the wholesale price.[15] Evidently, this does not apply to the wholesale monopoly scenario and it is easy to see that this would also not change the equilibrium in the wholesale competition scenario: As each of the integrated firms has an incentive to be the access provider, the first mover will anticipate to be undercut by the second mover and thus set the minimum feasible access price, just like when access prices are determined simultaneously. Consequently, the alternative timing would result in the same critical discount factors and therefore the same prediction with respect to the incentives for tacit collusion. From a more behavioral perspective, one could argue that the price commitment limits the extent to which one of the integrated firms can immediately retaliate the other (in the sense of the grim trigger strategy), which therefore makes the punishment less severe, and ultimately tacit collusion less likely. However, in an infinitely repeated game this lack of punishment in a short (finite) period does not matter.[16] But from a behavioral perspective it may. After all the price commitment is able to secure the second-mover a guaranteed wholesale profit for a (short) period of time and as such, it may stimulate a notion of *competition for the market* that—in line with Result 7.2—amplifies the competitive process.

[15]Note that this timing makes sense only in Timing Models (1) and (2), because it is the very nature of Timing Models (3) and (4) that integrated firms' decisions are made simultaneously in the upstream and downstream markets.

[16]Obviously, it would matter in a finitely repeated game. However, note that in this case the only subgame-perfect equilibrium would also be to play the (unique) equilibrium of the one-shot game in each period. That is, for the case of wholesale competition, and irrespective of a price commitment, the competitive outcome would be played. Consequently this model variant does not provide a theoretical rationale for Result 7.2 either.

7.6 Discussion

Although the regulation of access to an essential upstream resource is a perennial issue for policymakers and industry stakeholders, the competitive supply of the bottleneck resource by vertically integrated firms is investigated only recently in the theoretical economic literature. By means of an economic laboratory experiment this study scrutinizes these theoretical analyses, particularly with respect to the effectiveness of wholesale competition in the relevant case when there are only two access providers. The results indicate that wholesale duopoly markets may be severely affected by high levels of tacit collusion. In particular, this is found to be the case under homogeneous Bertrand competition at the wholesale level, which is frequently assumed in the theoretical investigations (e.g., Bourreau et al., 2011, 2015). In this vein and in the spirit of a more *behaviorally oriented regulation* (Normann and Ricciuti, 2009), this experimental analysis serves as a regulatory testbed, which points at possible behavioral issues that may arise in practice. After all, in light of the tremendous impact that regulatory decisions have on the respective industry and—especially in the case of network industries—also on other industries, policymakers should be particularly mindful when theoretical predictions are not confirmed in the laboratory.

However, the study has shown that regulatory experiments cannot only provide a means to test the robustness of theory, but are also able to identify possible behavioral regulatory rules that enable policymakers to improve market outcomes. In the present context, a simple price commitment rule significantly improves market outcomes, although the competitive intensity in the wholesale market remains below the theoretical prediction.

Furthermore, in reference to the theoretical analyses by Petulowa and Saavedra (2014) and Jullien et al. (2014), the experimental results give a clear indication regarding the theoretically ambiguous effect of margin squeeze regulation on retail prices in the presence of wholesale competition by vertically integrated firms. More specifically, the experimental evidence supports the rationale that the ban of a margin squeeze can impede the intensity of competition in the retail market. Moreover, the experiment points to a particular problem of applying the margin squeeze rule to an environment of multiple firms operating in the wholesale and retail market: When tacit collusion in the wholesale market is stable and leads to prices above the Nash equilibrium, retail pricing is constrained correspondingly. Especially the integrated firm, which naturally has an incentive to be more aggressive in the retail market because it is not affected by the softening effect, may be restricted in setting lower retail prices as long as it decides not to undercut prices in the wholesale market. Although the margin squeeze rule, as an implicit open access rule, ensures non-discrimination between competitors, the premise to treat all market participants equally is not aligned with the diverse incentives that occur in the case of simultaneous retail and wholesale competition, e.g., due to the consideration of opportunity costs by the access provider. Thus, non-discrimination of competitors may not always be in the best interest of the consumer.[17]

With respect to the limitations of the experimental study, note that firms' investment incentives under the various market scenarios are not considered and thus, experimental insights are constrained to short-term issues of static efficiency. However, in many industries, particularly in network industries such as telecommunications, dynamic efficiency is considered to be at least equally important by policymakers. Nevertheless, the findings may still be informative in this context, as there is generally an inherent trade-off between static and dynamic efficiency (Briglauer et al., 2015; Cambini and Jiang, 2009; Krämer and Vogelsang, 2014) with some notable exceptions (Klumpp and Su, 2010). That is, dynamic investment incentives are to a large extent influenced by the expectations about the future (static) benefits that arise from a given market structure (especially the market shares of competitors, see Klumpp and Su, 2015), the obtained results may inform further research regarding the effects that arise under infrastructure-based competition with multiple wholesale providers. In this context, the experimental results also cast doubt on the premise that infrastructure-based competition should be the undisputed regulatory goal (Cave and Vogelsang, 2003; Cave, 2006), particularly

[17] Note that an additional well-known negative effect of non-discrimination on competition is articulated by the theory of *restoring monopoly power* (Rey and Tirole, 2007), where non-discrimination allows the upstream firm to resolve its commitment problem.

when *open access* for independent resellers is required at the same time (e.g., as indicated by the European Commission's (2014a) digital agenda). This is in line with empirical findings by Höffler (2007) which suggest that infrastructure duplication costs may be higher than the gains from (supposedly) intensified competition. More general with respect to economic experimentation, a second concern arises with respect to the external validity of findings, although the validation study with industry professionals corroborates the robustness of the obtained student subject pool results. Naturally, experimental results do not directly carry over to actual markets, however, at the same time, one should also be cautious to believe that theoretical predictions will hold in practice when they already fail in a laboratory environment. Furthermore, note that the results are based on the relative differences between treatments and should thus not be affected by factors that are held constant across treatments. Nevertheless, an empirical field study of access in context of infrastructure competition would certainly represent a highly valuable contribution complementing theoretical and experimental work.

Finally, this study also inspires future experimental work. First, rules and remedies that may intensify competition at the wholesale level may be investigated in further depth. While this study considers two alternative modes of wholesale competition which yield market outcomes below and above the wholesale monopoly treatment, the investigation of the underlying competitive process and further investigation of instruments that may intensify competition at the wholesale level appear promising. Second, the presented analysis may be extended by a variation of the number of competitors and resellers as well as the introduction of asymmetry between the integrated firms. With regard to the competition across different access levels and quality layers—as in Internet and telecommunications markets—such an extension could provide valuable insight for decision makers and regulators within these fast-moving industries.

Chapter 8

Conclusion

THE aim of this thesis is to provide theoretical and empirical evidence to researchers, regulators, and practitioners regarding the propensities to tacit collusion of market structures in and experimental designs for oligopolies and regulated industries. To this end, each of the preceding chapters of this thesis addresses one of the research questions formulated in Chapter 1. More specific, by means of qualitative analysis, theoretical modeling, and experimental investigation, findings are derived that are summarized and put into context in the following. Beyond a mere synopsis of results, Section 8.1 relates the findings to implications for regulatory policy, management strategies, and the theory of economic experimentation. Section 8.2 completes this thesis with a critical appraisal of the research designs, procedures, and results of the reported studies and finally derives propositions for future research on anti-competitive behavior in oligopolies and regulated industries.

8.1 Summary and implications

The main research questions posed in Chapter 1 fall into two categories: Whereas Research Questions 1, 2, and 3 consider whether and how generic features of oligopolistic markets facilitate tacit collusion and are therefore primarily addressed at regulators, Research Questions 4 and 5 are aimed at advances in and promotion of the methodology of economic experimentation in the laboratory and are thus directed towards researchers and regulators. In the following, the summary and implications of obtained results are organized by the corresponding research question.

Underlying Research Question 1 regarding the necessary number of firms in an industry to safeguard competition is the general agreement on the fact that the competitiveness of an industry increases with the number of competitors. However, as reported in Chapter 4, a meta-analysis of ten oligopoly experiments and two experimental studies of oligopoly competition, in which the number of symmetric or asymmetric firms as well as the mode of competition are systematically varied, contradict this hypothesis. More specific, contrary to prominent belief, the competitiveness of an industry does not strictly increase with the number of competitors. In fact, triopolies are not found to be less competitive than quadropolies relative to Nash equilibrium. Further, results from all three studies indicate that whether Bertrand competition colludes more than Cournot competition depends on the competitive benchmark. Whereas price competition is more prone to tacit collusion than quantity competition if tacit collusion is measured according to a deviation from Nash equilibrium, the opposite holds relative to Walrasian equilibrium. Evidently, these results bear important implications for merger control and ex ante regulation of markets in which the number of potential competitors is limited. First, in contrast to the prominent notion that "two are few and four are many" (Huck et al., 2004b, p. 435) based on previous experimental investigation, the meta-analysis and the two specifically designed experiments indicate that instead it may hold that *two are few and three are many*. This may be viewed as support for the EC's recent consent to four-to-three mergers in mobile telecommunications markets. Second, however, regulators should scrutinize which theoretical benchmark to regard as effective competition. If instead of Nash equilibrium the Walrasian equilibrium is considered to be the appropriate benchmark, e.g., as consumer surplus is maximized in this case, the two specifically designed experiments, which have identical Walrasian but differing Nash predictions indicate that quadropolies collude less than triopolies. Third, in their competitiveness assessment of markets, authorities should also take into account the experimental finding that asymmetry between firms fosters competition as this may change the perspective on an absolute evaluation of tacit collusion.

The extant evidence regarding Research Question 2 suggests no difference in terms of tacit collusion between multimarket contact across geographic or product markets and single market contact in case of profit-maximizing symmetric firms meeting in identical markets. Albeit several empirical studies show that multimarket contact, or likewise, organizational centralization, facilitates tacit collusion, "most researchers assume that mutual forbearance requires asymmetric markets, rivals, and competitive positions" (Yu and Cannella, 2013, p. 77). Also theoretical models require either asymmetry of firms, markets or information, or non-linearity in objective functions to rationalize that multimarket contact or single market contact facilitate tacit collusion more than the other. In Chapter 6 a novel, behavioral explanation is offered based on the experimental finding in Chapter 5 that price discrimination across two identical geographic markets in a duopoly of conglomerate firms facilitates tacit collusion more than if firms are subject to a uniform pricing constraint which obliges each of them to commit to a single price in all markets. This theory holds even when previous explanations fail. In particular, a price signaling strategy is suggested that provides a microfoundation for the process leading to tacit collusion and explains under which circumstances multimarket contact facilitates tacit collusion more than single market contact. The theory rests on the assumptions that firms can communicate collusive intentions solely through their price setting behavior and that such *price signaling* can be conducted more efficiently under multimarket contact. These assumptions are verified by means of an economic laboratory experiment, which also shows that the price signaling strategy predicts market outcomes accurately. These results bear not only important insights for strategic management but also for competition policy. On the one hand, organizational centralization of conglomerate firms meeting the same rivals in multiple markets may effectively facilitate tacit collusion. On the other hand, this is why multimarket contact between conglomerate firms should be subject to scrutiny by competition authorities. Both theory and experiment indicate that limiting firms' possibilities to engage in price signaling, e.g., by a uniform pricing constraint across geographic markets, can effectively mitigate the emergence of tacit collusion.

Complementary to the investigations regarding the number of firms in a single market and competition across multiple horizontally connected markets, Research Question 3 is concerned with antitrust and regulatory issues in a vertical context of an upstream market and a downstream market along the same value chain. In fact, in several industries effective retail competition downstream is only feasible if wholesale access is provided to an essential input resource upstream. More specific, Chapter 7 considers the case where wholesale access for a non-integrated reseller is provided competitively by two vertically integrated firms, who also compete with the reseller in the retail market—a scenario that is akin to several industries with vertically related markets such as network industries with infrastructure duplication, e.g., European fixed telecommunications markets. In an economic laboratory experiment with two subject pools, students and telecommunications industry professionals, market outcomes are compared in a full-factorial design of different modes of competition at the wholesale level and regulation of upstream prices. In particular, above and beyond the theoretical literature, the behavioral analysis reveals that under homogeneous Bertrand competition at the wholesale level, access and retail prices are likely to be higher in the duopoly case in which both integrated firms offer the upstream good than in the monopoly case of a single access provider. However, a simple *price commitment* rule that obligates the integrated firms to commit to an upstream price for a specified amount of time can stimulate competition at the wholesale level, such that prices drop below the monopoly level. Irrespective of the mode of wholesale competition, there is no evidence that a margin squeeze regulation—which conditions each firm's upstream price to be lower or equal to its downstream price—reduces retail market prices. In fact, although margin squeeze regulation may benefit the reseller, it tends to increase retail prices and thus reduce consumer surplus. The regulatory implications of these findings are twofold. First, contrary to intuition, competition at an upstream market can be detrimental for both downstream resellers and consumers compared to an upstream monopoly. In other words and relating to the industry structure considered here, in a case of multiple integrated firms (e.g., an incumbent and a cable operator in a fixed telecommunications market) it may be beneficial for resellers and consumers that only one firm (e.g., the incumbent) provides access while the other (e.g., the cable operator) does not. Note, however, that in the experiment there is no price regulation of access. Second, in case of wholesale competition, instead of margin squeeze regulation, which has no effect on competitiveness, regulators should consider more behaviorally oriented regulation such as a price commitment rule that successfully hinders tacit collusion and drives prices down below monopolistic levels.

Research Question 4 is addressed in Chapter 2 by means of a comprehensive review of extant experiments investigating *regulation in the lab*. Thereby, a classification of experiments emerges according to the key target of experiments, i.e., whether an experiment aims for a maximum of external validity or internal validity of its findings. Whereas in the former category most experiments are concerned with regulation in a specific industry and therefore design a complex experimental environment that closely resembles the specifics and regularities of that industry, experiments in the latter category consider a specific regulatory instrument in a generic market context and thus create a simple experimental design which encompasses a minimum of parameters. A qualitative analysis of a total of around 80 economic laboratory experiments along the four variables (i) policy problem, (ii) experimental design, (iii) experimental results, and (iv) policy implications results in 10 observations which may serve as guidelines with respect to the design and procedures of a regulation in the lab experiment. The chapter closes with an analysis of the experimental methodology's potential in advising regulators and policy makers. The following contributions and implications ensue from this qualitative research. First, it provides researchers with a consistent and holistic overview of experimental research on economic regulatory institutions and at the same time is a work of reference that offers regulators a summary of experimental insights in this regard. Second, the observations made from extant experiments provide the basis for a theory of regulation in the lab that aids researchers in conducting experiments on regulatory policy and promotes the methodology. Third, the review as well as the subsequent analysis of potential highlight that experiments can corroborate or undermine theoretical predictions and thus add complementary insights to regulatory policy and also other fields of public policy that should be scrutinized by policy makers.

In response to Research Question 5 regarding a specific element of experimental designs, Chapter 3 first classifies on modes of timing in experiments—namely discrete time, near-continuous time, continuous time, and a clock/deadline mechanism—and second reports on the very first oligopoly competition experiment with differentiated goods in discrete and continuous time. Continuous time experiments allow for real-time, asynchronous strategic interaction and are therefore argued to be a more realistic mode of interaction, particularly in the context of (electronic) markets. The experiment considers duopolies and triopolies both under Bertrand as well as Cournot competition and consistently finds that, ceteris paribus, tacit collusion is higher under discrete time than under continuous time, which contrasts the theoretical prediction. Thus, the results bear important methodological implications for research on oligopoly competition. First, researchers designing an oligopoly competition experiment can draw on these findings and make an informed decision about the mode of timing used to resemble the underlying repeated game. Second, previous experimental findings regarding oligopoly competition—which are exclusively drawn from a discrete time environment—may overestimate the extent of tacit collusion compared to the real-world scenario, which is argued to be resembled more closely by continuous time.

8.2 Limitations and outlook

This thesis has several limitations in its scope and methodologies which point to possible avenues for future theoretical, experimental, and empirical work on ex ante and ex post regulation of imperfectly competitive markets such as oligopolies. More general, the findings reported in this thesis motivate further experimental research on issues of economic and social regulation in an effort to provide valuable insights for regulatory policy and help policy makers in implementing better policies. The specific content-related and methodological limitations and propositions for future work ensuing from this thesis are detailed in the following.

The foremost and general limitation of this thesis is its specific focus on competition and thus tacit collusion as a means to evaluate the functioning of oligopolistic markets. However, apart from this static view on efficiency regulators are also concerned with dynamic efficiency, i.e., how the market's structure itself develops over time. Reconsider for instance the goal of the EC's DSM strategy to "ensure that markets operate more competitively [...], while ensuring the right regulatory conditions for innovation, investment, fair competition and a level playing field" (European Commission, 2015b, p. 3). As highlighted with respect to the telecommunications industry, regulators aim in fact at both static efficiency, i.e., competition, and dynamic efficiency, i.e., innovation and investments, at the same time. Yet there may be a trade-off between these two goals so that regulation which tries to maximize efficiency, i.e., total surplus, has a multi-attribute objective and will not unilaterally foster competition. Since none of the theoretical models or experimental studies in this thesis allows for endogenous product innovation or infrastructure investment, the implications are limited to static efficiency. Note that with respect to the experimental methodology the implementation of an investment or innovation stage considerably complicates the experimental design and procedures. Transferring such a multistage game to the lab poses the questions of how the game may be repeated (e.g., as a whole or only certain periods) and consequently of what the appropriate theoretical benchmark is (e.g., whether subjects can be assumed to engage in backward induction or forward induction). Not least because of this fact it would be a challenging yet promising direction for future experimental research on oligopoly competition to investigate how anti-competitive behavior is affected by endogenous investments and innovation.

Therefore, as neither investment nor innovation are allowed in any of the theoretical models or experiments in this thesis, the reported results warrant critical appraisal in this regard as well as with respect to other potential limitations. In particular, the experiment on the fewness of firms in markets described in Chapter 4 considers mere price or quantity competition over a fixed number of repetitions. Allowing firms to invest in product quality or likewise in infrastructure to increase the market size could not only provide new insights into the extent of investments but also affect the findings with respect to tacit collusion. On the one hand, investments may lead to asymmetry which fosters competition as shown in the experiment with (exogenously) asymmetric firms (Section 4.4). On the other hand, one may conjecture that investments—despite being sunk cost—make rivals less aggressive in an effort to recur the expenditures (cf. Krämer and Schnurr, 2014). Moreover, controlling for investments and thus accounting for static and dynamic efficiency at the same time may decrease the number of firms that a regulator deems necessary for a sufficient efficiency level. In sum, an experimental analysis of the number effects in oligopolies in an environment that allows for endogenous investment into product quality or infrastructure seems worthwhile.

The same line of argument holds for the experiment reported in Chapter 3 on discrete time and continuous time as it utilizes the same model of oligopoly competition. Furthermore, regarding the modes of timing, the experiment is limited to the two extremes on a spectrum: Pure discrete time with unbounded period lengths and continuous time that resembles real time in the experiment with a period length below the human reaction time. As the classification of timing modes reveals, the near-continuous time framework ranges in between these two extremes with period lengths of a few seconds. As there is no microfoundation explaining the finding that discrete time colludes more than continuous time, an experiment with systematically varying period lengths may provide valuable insights (cf. Friedman and Oprea, 2012). Moreover, agent-based simulation appear to be a promising methodology for an exploratory approach towards a microfoundation as it allows to easily test and compare not only different agent strategies but also modes of timing. In particular, a simulation could investigate how the same set of agents behaves if the sequence of decision-making is specified by simultaneous-move, sequential-move, or a stochastic decision time.

The lack of investments is also a limitation of the experiment on upstream and downstream competition described in Chapter 7. In fact, as the experiment is specifically concerned with access to an essential infrastructure, allowing for infrastructure investments appears to be a natural direction for future research. However, note that already the current version of the experiment is based on a multistage game in which integrated firms first choose their upstream prices and second all firms decide on their downstream prices. To merge both stages into one, the continuous time framework is utilized so that prices in all markets can be changed at any time. Although this makes theoretical predictions more complicated, continuous time also allows for experimental environments that are closer to the real world. Therefore, the experiment highlights that the new mode of timing allows for more diverse theoretical models to be tested in the laboratory.

Firms may also not invest in infrastructure or product quality in the experiment on price discrimination and uniform pricing reported in Chapter 5. Therefore, the experimental analysis does not allow to investigate whether the possibility to discriminate prices affects investment incentives for firms. In the same line of argument, it is also left to future work to assess whether investments in turn have an impact on firms' propensities to engage in price discrimination. Further research should also investigate how the issues of price discrimination vs. uniform pricing and multimarket contact vs. single market contact overlap in industry scenarios with asymmetric firms and markets.

Also the price signaling theory introduced in Chapter 6 does not consider the effect of investments. It may be conjectured that investments serve as a further signaling device above and beyond price signaling. In this line of argument, tacit collusion would be even more pervasive if firms invest prior to price competition. Other limitations of the price signaling theory is its limitation to two firms and markets. Whereas an extension to more than two markets could easily be implemented without changing the foundations of the theory, it is far from obvious how a price signal would be sent and received when there are more than two firms competing against each other. Further worthwhile advancements for the behavioral price signaling theory are scenarios considered by previous theories on the mutual forbearance hypothesis, e.g., asymmetric firms and markets.

Lastly, two further limitations of the experimental methodology in general are that it allows only for a relative assessment of treatment effects and that the external validity of the obtained empirical evidence is hardly assessable. First, due to the relative nature of experimental findings, no absolute statement about the size of an effect can be inferred. However, this holds likewise for any theoretical analysis and is therefore no shortcoming that is exclusive for experiments. On the contrary, experimentation allows for a quantification of effects at least within the boundaries of the experimental environment. Second, laboratory experiments allow for a high level of internal validity, but at the cost of a potential lack in external validity—although the use of practitioner subject pools may partly make up for this (Chapter 7). Furthermore, as Angrist and Pischke (2010, p. 23) note, the impossibility to assess external validity is not limited to experiments since "empirical evidence on any given causal effect is always local, derived from a particular time, place, and research design." Finally, with respect to the assessment of regulatory and antitrust policy, even if empirical evidence is always local in nature, a combination of complementary methodologies will clearly improve the robustness of findings and is thus encouraged for future research.

Appendix A

Review of Laboratory Experiments on Regulatory Policy

TABLE A.1: *Economic laboratory experiments on regulation in the electricity industry.*

	Policy problem	Experimental design	Experimental results	Policy implications
Denton et al. (2001b,a)	Ensuring competitiveness in the England and Wales wholesale electricity market through an efficient auction mechanism.	Three (six) human sellers (experienced students) with six (three) cost-asymmetric power generators and four human wholesale buyers are located in a three-node radial network and each bid prices and quantities in a uniform price double auction for 12 to 30 periods. Offers are either sealed or may be improved in real-time during a bidding period. Demand is step-wise constant decreasing in price and the share of inelastic demand is stochastic.	Prices are competitive throughout. The sealed-bid auction variant is more efficient than the real-time auction variant. Three sellers only exert more market power than six sellers under the real-time auction variant.	A uniform price sealed-bid double auction may effectively prevent the exertion of seller market power, whereas a real-time variant, e.g., common in stock markets, may be less suited.
Weiss (2002)	Ensuring competitiveness of wholesale electricity markets despite seller concentration and transmission constraints through an efficient auction mechanism and the effect of demand-side bidding.	One, three, or six human sellers (industry professionals) have nine, three, or three cost-asymmetric power generators each, respectively, are located along four nodes together with a computerized competitive fringe and bid prices and quantities for 33 periods over 12 weeks in an email-based uniform price sealed-bid auction. Transmission on one line is constrained for the first 23 periods. Demand is step-wise decreasing in price and computerized, except for one treatment with two human buyers and three human sellers.	Prices decrease with an increasing number of sellers, but less (more) so at nodes (not) affected by the transmission constraint. With demand-side bidding, prices are lower and close to the competitive equilibrium. Removing the transmission constraint, thus mimicking capacity investments, decreases prices at nodes previously benefiting from the constraint.	Increasing the number of electricity producers may reduce prices, but not if some firms may exert local market power due to transmission constraints. Demand-side bidding in a uniform price sealed-bid double auction may be more effective.

TABLE A.1: *Continued.*

	Policy problem	Experimental design	Experimental results	Policy implications
Thomas et al. (2002); Mount et al. (2002); Vossler et al. (2009)	Effects of a soft price cap introduced in California's wholesale electricity market with capacity allocated by a hybrid sealed-bid auction mechanism with uniform pricing below the price cap and with discriminatory pricing otherwise.	Six human sellers (students and professionals) with five cost-asymmetric power generators each bid prices and quantities for 25–50 periods. Without the price cap, a uniform price one-sided auction is used to allocate supply. Demand is uncertain and either inelastic or step-wise constant decreasing in price, but complete information on the seller's side is provided.	Elasticity of demand reduces prices, even more so in the absence of a soft price cap. Decreasing cost decrease prices only if the soft price cap is not in place. Students behave similar to the industry professionals.	A soft price cap may be insensitive to changes in load and generation cost. Thus, cost reductions may not be passed on to consumers. A uniform price double auction without price caps may be more effective in reaching competitive wholesale electricity markets.
Rassenti et al. (2003a,b)	Prevention of anti-competitive effects associated to market power in wholesale electricity auctions.	Five human sellers with asymmetric generation cost choose quantity and price and may have market power through asymmetric capacities. During 14 periods of four trials with varying demand, buyers bid for electricity in a uniform price or discriminatory price sealed-bid double auction and are either computerized or controlled by four humans. Demand is uncertain and step-wise constant decreasing in price, but complete information on the seller's side is provided.	Market power increases prices, but this effect is neutralized by uniform price demand-side bidding. Although prices are less volatile in the discriminatory price auction, they are also higher—even in the absence of market power.	Demand-side bidding may reduce the anti-competitive effects of market power which suggests decentralization in electricity markets. However, auction mechanisms should be designed to result in a uniform transaction price.

TABLE A.1: *Continued.*

	Policy problem	Experimental design	Experimental results	Policy implications
Abbink et al. (2003)	Potential efficiency losses in wholesale electricity auctions due to asymmetric information of electricity producers on future demand.	Four human sellers, two low-cost and two high-cost, with two cost-asymmetric power generators each bid prices and quantities for 30 periods in a uniform price or discriminatory price one-sided sealed-bid auction. Demand is step-wise constant decreasing in price. The share of inelastic demand is stochastic and its forecast is certain for low-cost sellers. For high-cost sellers, the forecast is certain under complete information and noisy under asymmetric information (between-subject).	With complete information, there is no difference in performance criteria between the two auction mechanisms. With asymmetric information, the uniform price auction is more efficient in terms of total surplus than the discriminatory auction due to higher marginal (not average) prices under the latter auction mechanism.	Uniform price auctions may be more efficient than discriminatory price auctions in case electricity producers have asymmetric information on future demand—however, not if information on future demand is symmetric and certain.
Kench (2004)	Potential of financial or physical transmission rights to mitigate market power through congested transmission lines in electricity networks.	Eight human sellers, four low-cost at a northern node and four high-cost at a southern node, with two or three units of electricity each bid prices for 13 periods of fixed length in a continuous double-oral auction. Four human buyers, located at the southern node, have demand of up to four units and bid simultaneously. Transmission between the two nodes may be congested and northern sellers have and bid for financial or physical transmission rights (between-subject) every three periods in a continuous double-oral auction.	With transmission rights, prices increase at the northern, but not at the southern node. Electricity prices at the northern node are again higher if physical (exclusive utilization) instead of financial (share of congestion charges) transmission rights are auctioned, but this is reversed for sellers without the respective transmission right.	Transmission rights may mitigate market power. Physical transmission rights may additionally remove uncertainty about electricity transmission more than financial rights.

TABLE A.1: *Continued.*

	Policy problem	Experimental design	Experimental results	Policy implications
Adilov et al. (2005)	Potential of demand-side management at the retail level to mitigate market power of producers in unregulated wholesale electricity auctions. In particular, under a demand-side program consumers are incentivized to reduce consumption during high load phases and under real-time pricing consumers decide on consumption based on price forecasts.	Six human sellers (experienced students) with three cost-asymmetric power generators and one computerized seller each choose price and quantity in a uniform price double auction for 11 periods of day and night pairs. 13 to 17 human buyers with discrete day and night demand decide on how much electricity to purchase. Demand and supply are parametrized with field data and face random shocks. Treatment conditions were applied within-subject. Conducted using *PowerWeb*.	Compared to a situation of fixed prices, price spikes are reduced under both demand-side mechanisms, consumer surplus increases substantially, and producer surplus decreases. Under the demand-side program (real-time pricing), total surplus, i.e., efficiency, decreases (increases) slightly.	Consumer participation in electricity markets may reduce market power of producers, but at the same time market efficiency may sink and corresponding demand-side mechanisms are very complex.
Chapman et al. (2004)	Minimum number of producers required for wholesale electricity uniform price auctions to exhibit effective competition.	24 human sellers (experienced students) with five cost-asymmetric power generators each choose price and quantity in a uniform price auction in groups of six for 40 periods, then in groups of 12 for 25 periods, and then all together for 25 periods. Afterwards, all sellers compete again three times for 20 periods each with pre-play communication of first cheap talk with a Chinese wall, then unrestricted cheap talk, and finally binding agreements (within-subject). Demand is stochastic, but based on a forecast. Other parameters were chosen to closely resemble real wholesale electricity markets. Conducted using *PowerWeb*.	Prices are supra-competitive throughout, but decrease with the increasing number of sellers in case no communication is allowed. Each of the three forms of explicit communication raises prices substantially, with unrestricted cheap talk and binding agreements reaching the reservation price.	Even markets with six, 12, and 24 electricity producers may allow for the exercise of market power. Any communication among suppliers, even cheap talk with a prohibition to talk about prices, may facilitate collusive behavior.

TABLE A.1: *Continued.*

	Policy problem	Experimental design	Experimental results	Policy implications
Bernard et al. (2005)	Potential of alternatives to the uniform price last accepted offer sealed-bid auction in wholesale electricity markets to prevent exploitation of market power and minimum number of producers required to exhibit effective competition.	Two, four, and six human sellers with three cost-asymmetric power generators each choose price and quantity for 75 periods in a uniform price last accepted offer auction, a uniform price first rejected offer auction, or a multiunit Vickrey auction in which a winner is paid the kth rejected bid for its kth sold unit (between-subject). Demand is inelastic with a fixed reservation price and complete information is provided.	Prices are highest for markets with two sellers. In markets with four sellers, prices are higher than markets with six sellers only for early periods. Across the numbers of sellers, prices are lowest in the multiunit Vickrey auction. In the uniform price first rejected offer auction, prices are lower than in the last accepted offer variant only for early periods.	Instead of the uniform price last accepted offer auction implemented in many wholesale electricity markets, a multiunit Vickrey auction may help to prevent the exploitation of market power.
Kiesling and Wilson (2007)	Effects of an automated mitigation procedure with a fixed reference offer on prices in electricity markets and investments in generation capacity.	Five human sellers with three cost-asymmetric power generators each bid prices and quantities during 45 rounds of 48 near-continuous time periods in a uniform price auction. Demand is stepwise decreasing in price and varies during the course of a round. Under the automated mitigation procedure, bids may not be higher than a seller-specific fixed reference offer. Under strong market power, excess capacity is lower than under weak market power. Treatment conditions are applied between-subject. Network capacity may be increased through investments during rounds 21 to 30.	The automated mitigation procedure with fixed reference offers reduces prices pre-investment and does not inhibit investments, but decreases prices post-investment only slightly under strong market power and not at all under weak market power. Investment in generation capacity decreases prices most strongly.	Automated mitigation procedures may effectively reduce prices in wholesale electricity markets prior to investments in generation capacities and at the same time do not seem to hamper investments.

TABLE A.1: *Continued.*

	Policy problem	Experimental design	Experimental results	Policy implications
Mount and Maneevitjit (2008)	Effects of the forward capacity market intended to provide financial incentives to build capacity towards an economically feasible mix and proposed by regulators in New England, in which annual capacity is purchased by the independent system operator three years in advance.	Three human incumbent sellers with existing power generators offer future capacity in a descending-clock auction and bid in a spot market for 10 periods. Computerized entrants as well as incumbents may invest in one or two types of new capacity. New capacity may be priced higher in the auction than installed capacity. Demand is inelastic in each period and increases over time.	If there is only one type of new generating capacity, incumbents foreclose entrants by compensating losses from low offers in the capacity market with earnings from the spot market. If there is a baseload and a peaking capacity, incumbents will invest only in new baseload capacity if entrants may do so as well.	The forward capacity market may provide financial incentives to incumbents and entrants alike to invest in new capacity if other regulatory measures do not prevent entrants from investing in both baselod and peaking capacity. A variant of this market is in fact currently implemented in New England.
Brandts et al. (2008)	Potential efficiency gains from adding forward contracting to an electricity spot market.	Three or four human sellers compete in a spot market, face increasing marginal cost, and choose quantities or supply functions, i.e., multiple price-quantity combinations, for 25 periods. Demand is elastic and complete information is provided. If a forward market is preceded, sellers choose quantities and two human traders compete á la Bertrand for the total quantity, before selling in the spot market as well.	Under both modes of spot market competition, introducing a forward market increases the quantity supplied as well as efficiency, whereas adding a further electricity producer instead increases only quantity, but not efficiency. With three sellers, prices are lower for supply function than quantity competition.	Forward markets may reduce prices and increase efficiency in electricity markets, not accounting for demand uncertainty and corresponding risk hedging.

TABLE A.1: *Continued.*

	Policy problem	Experimental design	Experimental results	Policy implications
Shawhan et al. (2011)	Effects of the automated mitigation procedure implemented in New York's wholesale electricity market, i.e., a reference offer replacing suppliers' offers if they exceed a price cap based on a moving average of their past transaction prices, on prices and investments in generation capacity.	Six human sellers each bid prices and quantities during 50 periods in a uniform price auction with alternating (un)congested periods. Without congestion, all sellers have three cost-asymmetric power generators and compete in one market. Under congestion, sellers are equally separated into two markets with six power generators each. Market power varies between-subject due to excess capacity and presence of the automated mitigation procedure.	The automated mitigation procedure reduces prices in congested periods substantially, but is less effective if sellers have strong market power and has no effect on prices in uncongested periods.	The automated mitigation procedure in New York may effectively keep prices at competitive levels in the wholesale electricity market. However, it may be less effective in decreasing prices if electricity producers have strong market power.
Henze et al. (2012)	Potential of price cap regulation in a spot market auction, regulatory holiday under which the price cap is lifted for new installed capacity, and price cap regulation with long term financial transmission rights in stimulating investment in network infrastructure by a monopolistic network owner.	Four human firms with asymmetric demand compete for usage of a network owned by another human firm in a lowest accepted bid uniform price sealed-bid auction subject to a price cap during five independent blocks of six periods each. At the beginning of a block (after every three periods) the network users (owner) may partly increase unit valuations (install additional capacity) at a per-period cost. The premium of the market price above the price cap is transferred to the regulator. Either the price cap may not apply for additional capacity or long term financial transmission rights are auctioned at the very beginning of a block in a lowest accepted bid uniform price sealed-bid auction subject to a price cap.	Prices are lowest, capacity is highest, and total as well as allocative and dynamic efficiency are highest with price cap regulation. Profits of the network owner are highest with regulatory holiday. Neither regulatory holiday nor long term financial transmission rights are beneficial in terms of welfare.	Price cap regulation in spot markets for monopolistic network capacity may not be outperformed by regulatory holiday for new capacity or price cap regulation with long term financial transmission contracts in terms of efficiency, although none of the scenarios reaches socially optimal levels of network capacity.

TABLE A.1: *Continued.*

	Policy problem	Experimental design	Experimental results	Policy implications
Le Coq and Orzen (2012)	Robustness of competition in wholesale electricity markets to changing market properties, in particular a raise of a fixed price cap and increasing demand.	Four human sellers with nine single-unit power generators with linear marginal cost offer prices for each generator in a uniform price auction for 12 periods. Prices are subject to an either low or high price cap (between-subject). Demand is inelastic and varies over time, but capacity exceeds demand throughout and complete information is provided.	In all treatments, market prices are higher, but close to marginal cost of last dispatched unit. Due to unilateral attempts to raise prices, allocative inefficiencies arise. These are more severe with a higher price cap and higher demand.	Excess capacity alone may not suffice to induce efficiency in electricity markets, but rather in addition price caps and demand should be low enough.
Jullien et al. (2012)	Efficiency of cross-border congestion management mechanisms in allocating scarce cross-border transmission capacity in a European internal electricity market, in particular the implicit auction implemented in many European countries and the coordinated explicit auction proposed by the European Transmission System Operators.	Four human vertically integrated sellers (experienced students) have power generators distributed in a three-node network with transmission constraints and play for 14 to 30 periods. In the implicit auction, firms submit node-specific offers (price and quantity) to buy or sell electricity in a combined uniform price sealed-bid auction, whereas in the coordinated explicit auction first transmission capacities and second electricity are allocated in separate uniform price sealed-bid auctions for each line and node (between-subject). Demand (supply) at each node is elastic, stepwise constant decreasing (increasing) in price, and varies over time.	With the implicit auction, efficiency is higher, price volatility is lower, and transmission capacities are better allocated than with the coordinated explicit auction due to the latter's obligation to use bought transmission capacities.	Cross-border congestion management in Europe may be more efficient with a simultaneous allocation of transmission capacity and electricity as in the implicit auction than with a sequential allocation as in the coordinated explicit auction.

TABLE A.1: *Continued.*

	Policy problem	Experimental design	Experimental results	Policy implications
van Koten and Ortmann (2013)	Potential efficiency gains from adding forward contracting and from increasing the number of firms by divestiture in an electricity spot market.	Two to four human sellers compete in a spot market, face increasing marginal cost, and choose quantities for 24 periods. Demand is elastic and complete information is provided. If a forward market is preceded, sellers choose quantities and two computerized traders compete in prices for the total quantity, before selling in the spot market as well. Aggregate production assets are kept constant over varying numbers of sellers.	Introducing a forward market and increasing the number of sellers by divestiture and thereby keeping the aggregate production assets constant increases the quantity supplied.	Forward markets and increasing the number of competitors by divestiture may intensify competition in electricity markets.

TABLE A.2: *Economic laboratory experiments on regulation in the telecommunications industry.*

	Policy problem	Experimental design	Experimental results	Policy implications
Banks et al. (2003)	Efficiency of the simultaneous multiround ascending auction used by the US Federal Communications Commission to assign spectrum licenses for telecommunications services and comparison to a combinatorial multiround ascending auction as mandated by the US Congress.	Varying numbers of human bidders (experienced students) bid for 10 licenses with single-item, sealed bids in the simultaneous multiround auction and with single-item and/or packaged, sealed bids in the combinatorial multiround ascending auction. Bidders face activity obligations to stay in the auction with equal (equal or unequal) eligibility points across licenses and a flexible (flexible or nonflexible) amount of eligibility in the combinatorial (simultaneous) auction.	In case of license value synergies, assignment efficiency is higher and auctioneer's revenue is lower in the combinatorial than in the simultaneous auction and, under the latter, efficiency (revenue) is higher with flexible eligibility (flexible eligibility and equal eligibility points). All these efficiency gains increase auction length.	A combinatorial multiround ascending auction may increase efficiency, but also auction length compared to the simultaneous multiround ascending auction, in which strict eligibility rules may hinder efficiency further.
Plott and Salmon (2004)	Prediction of length and final prices in the 3G spectrum auction in the UK in 2000 from patterns in early bids.	Eight (12) human bidders with asymmetric preferences compete for six (eight) spectrum licenses of two different types in a continuous simultaneous multiround ascending auction in which bidders may only buy up to one license and bids are subject to a fixed increment requirement. The auction runs continuously and ends if there is no increasing bid for three minutes.	A behavioral model to predict final prices and auction length is suggested. In the experiment, final prices are close the competitive equilibrium and efficiency is near its maximum. The number of bids ranges from 50 to 136, which is lower than predicted, but the model predicts half of all bids correctly.	The proposed behavioral model proposed may help auctioneers in predicting the total length as well as final prices of a spectrum auction early on to make informed decisions about the further auction design.

TABLE A.2: *Continued.*

	Policy problem	Experimental design	Experimental results	Policy implications
Abbink et al. (2005)	Potential efficiency and revenue of two multiround and sealed-bid hybrid auction mechanisms considered by the British Radiocommunications Agency prior to the 3G spectrum auction in the UK in 2000. Study commissioned by a potential bidder before commenting on the suggested auction mechanisms.	Eight human bidders, four incumbents and four entrants, with asymmetric preferences compete for four identical spectrum licenses in either a pure simultaneous multiround ascending auction with an increment requirement or a discriminatory or (lowest winning bid) uniform price variant of a hybrid auction mechanism according to which first a simultaneous multiround ascending auction with an increment requirement is used until only five bidders remain and then a final sealed-bid auction determines the four winners.	Efficiency, winner's curse, and aggressive entrant bidding is highest in the discriminatory price hybrid auction. With experienced subjects, differences in efficiency and auctioneer revenue decrease or disappear. The number of successful entrants is highest in the pure simultaneous multiround ascending auction.	Suggested hybrid auction mechanisms seem to be as efficient or revenue generating as a pure simultaneous multiround ascending auction. A variant of the latter was eventually employed for the 3G spectrum auction in the UK.
Seifert and Ehrhart (2005)	Comparison of 3G spectrum auctions in Germany and the UK in 2000 in terms of auctioneer revenue and bidder surplus.	Seven human bidders, two large incumbents, two smaller incumbents, and three entrants, with asymmetric preferences compete for 12 identical (two large and three small) licenses in the German (UK) variant of a simultaneous multiround ascending auction. In the UK variant, bidders may only buy up to one license. In the German variant, bidders first bid for two or three licenses and then for up to one license among the remaining licenses.	Auctioneer revenue (bidder surplus) is higher (lower) in the German variant than in the UK variant. Bidder surplus is even slightly negative in the German variant as bidders face the risk of overbidding, i.e., paying more than their actual valuations.	The design of 3G spectrum auction in Germany may result in higher auctioneer revenue than the design in the UK. However, bidders may be deterred by the German auction design resulting in lower participation as was so in the real world. Furthermore, subjects were students unexperienced with auctions.

TABLE A.2: *Continued.*

	Policy problem	Experimental design	Experimental results	Policy implications
Brunner et al. (2010)	Efficiency assessment of a combinatorial multiround auction format developed by the US Federal Communications Commission, in which bidders may have only one winning bid at the same time, in comparison to other combinatorial and simultaneous formats for spectrum auctions.	Eight human bidders (experienced students), thereof six regional and two global, compete for 12 licenses covering distinct geographic regions. For each bidder, value synergies arise from geographic adjacency of licenses. Either a simultaneous multiround ascending auction or one of three combinatorial auction formats in which bids may be single-item or packaged is used (between-subject). Bidders may either be allowed to have multiple or only one winning bid at the same time and prices may either increase by submitted bids or automatically by a clock. Activity rules and an increment requirement apply to all auctions.	Efficiency is higher in the combinatorial multiround auction with multiple winning bids allowed than in the other auctions. If license value synergies are high (low), efficiency is also higher in the other combinatorial auctions (in the simultaneous multiround ascending auction). Auctioneer revenue (bidder surplus) is higher (lower) in the combinatorial clock auction than in the other auctions.	Efficiency in combinatorial multiround auctions may be higher than in the simultaneous multiround ascending auction if the licenses in neighboring regions are assumed to have high value synergies. However, the variant developed by the US Federal Communications Commission may be outperformed by other combinatorial auctions.
Kagel et al. (2010)	Comparison of combinatorial and simultaneous price-guided, i.e., clocked, spectrum auctions with automatically increasing prices if licenses have value synergies due to geographic adjacency and prediction of bidders behavior.	Three human bidders, thereof two regional and one global, with asymmetric preferences compete for four or six licenses covering distinct geographic regions. Regional bidders have a positive valuation only for disjoint halves of licenses. In the combinatorial multiround clock auction, bids may be single-item or packaged and bidders may have only one winning bid at the same time. In the simultaneous multiround ascending clock auction, bids are single-item and activity rules apply (between-subject). In both auctions, prices increase automatically over rounds.	Efficiency is higher (lower) in the combinatorial multiround clock auction than the simultaneous multiround ascending clock auction if bidder valuations are easy (very hard) in terms of high (low) efficiency achieved in simulations; a classification of valuations that prevails in the lab, indicating that bidders focus on the most profitable license (package).	Combinatorial multiround clock auctions may be more efficient than simultaneous multiround ascending clock auctions if the distribution of bidder valuations does not pose a hard coordination problem. Price-guided, i.e., clocked, auctions may fail to coordinate buyers on relevant license packages. Reporting winning bids may facilitate tacit collusion.

TABLE A.2: *Continued.*

	Policy problem	Experimental design	Experimental results	Policy implications
Goeree and Holt (2010)	Efficiency of a combinatorial multiround ascending auction with predefined packages, referred to as hierarchical package bidding and developed for spectrum auctions in the US, and comparison to a variant with custom packages approximating shadow prices and a simultaneous multiround ascending auction.	Seven human bidders, thereof six regional and one global, with asymmetric preferences compete for 18 licenses organized in a national and regional circle and have value synergies from the number of licenses. Either a simultaneous multiround ascending auction or a combinatorial multiround ascending auction with custom (predefined) single-item or packaged bids is used (between-subject). Activity rules and an increment requirement apply. Bidders can have multiple winning bids at the same time.	Efficiency, auctioneer revenue, and the number of sold licenses are higher and bidder surplus is not lower in the combinatorial auction with predefined packages than in the other two auction formats. Without value synergies, efficiency is near its maximum for all three auction formats.	The hierarchical package bidding auction used by the US Federal Communications Commission in a 2008 spectrum auction may be more efficient than a combinatorial auction in which bidders can bid on any custom package of licenses or a simultaneous multiround ascending auction.
Scheffel et al. (2012)	Efficiency of a combinatorial multiround ascending auction with predefined packages, referred to as hierarchical package bidding and developed for spectrum auctions in the US, and comparison to a combinatorial auction with custom packages approximating shadow prices and a combinatorial clock auction.	Seven (six) human bidders, thereof six (five) regional and one global, with asymmetric preferences compete for 18 licenses and have value synergies from the number (geographic adjacency) of licenses. Either a combinatorial clock auction or a combinatorial multiround ascending auction with custom (predefined) single-item or packaged bids is used (between-subject). Activity rules and an increment requirement apply. Bidders can have multiple winning bids at the same time.	With value synergies from the number (geographic adjacency) of licenses, efficiency (efficiency and auctioneer revenue) is higher in the combinatorial auction with predefined packages (combinatorial clock auction) than in the combinatorial auction with custom (predefined) packages.	Efficiency may be highest either in combinatorial clock auctions or combinatorial multiround ascending auctions with predefined license packages as bidders' preselection of a small number of packages appears to be the main source of allocative inefficiencies in combinatorial spectrum auctions.

TABLE A.2: *Continued.*

	Policy problem	Experimental design	Experimental results	Policy implications
Bichler et al. (2013)	Efficiency and auctioneer revenue of the core-selecting combinatorial clock auction adopted in many countries for recent spectrum auctions compared to the simultaneous multiround ascending auction.	Four human bidders with asymmetric preferences compete for 24 licenses either under the base value model with 14 licenses with value synergies in band A and 10 licenses in band B or under the multiband value model with six licenses with value synergies in four bands each (between-subject). Throughout, bidders have a higher expected valuation for band A than the other band(s). Eligibility rules apply in the simultaneous multiround ascending auction. In the core-selecting combinatorial clock auction bids may be single-item or packaged and bidders first bid until there is no excess demand in any band and then bid in a final sealed-bid round.	Efficiency (auctioneer revenue) in the multiband value model (both value models) is lower in the core-selecting combinatorial clock auction than in the simultaneous multiround ascending auction. This may be due to the few bundles that were bid for compared to the very high number of potential bundles in the former auction type.	The core-selecting combinatorial clock auction recently adopted for spectrum auctions may neither be more efficient nor auctioneer revenue increasing than the simultaneous multiround ascending auction. Increased complexity and number of potential license bundles in an auction may decrease its efficiency.
Kagel et al. (2014)	Comparison of combinatorial and simultaneous price-guided, i.e., clocked, spectrum auctions with automatically increasing prices if licenses have value synergies due to shared fixed costs and prediction of bidders behavior.	Three human bidders, thereof two regional and one global, with asymmetric preferences compete for four or six licenses and each have fixed lumpy shipping costs independent of the number of licenses purchased, thereby introducing value synergies of licenses. Bidder valuations used are such that the efficient allocation is either (1) all licenses to the global bidder or a split between the two regional bidders or (2) some other allocation. Other aspects are similar to Kagel et al. (2010).	Efficiency is higher (lower) in the combinatorial multiround clock auction than the simultaneous multiround ascending clock auction if bidder valuations are such that the efficient allocation is (not) all licenses to the global bidder or a split between the two regional bidders.	Taking into account bidders' roles in spectrum auctions improves predictions about bidding behavior substantially. The efficiency of combinatorial and simultaneous multiround clock auctions is largely unaffected if constant shipping cost for licenses exist.

TABLE A.2: *Continued.*

	Policy problem	Experimental design	Experimental results	Policy implications
Bichler et al. (2014)	Increasing the efficiency of the simultaneous multiround ascending auction or combinatorial ascending clock and sealed-bid auctions frequently used in spectrum auctions by simplifying the bid language and the payment rule.	Four human bidders with asymmetric preferences compete for 24 licenses in four bands á six licenses each and have value synergies from the number of licenses within a band. Combinatorial auctions are used in either an ascending clock or a sealed-bid variant. The bid language is either compact (bidders can have only one winning bid within a band, but multiple in different bands) or fully expressive (bidders can have only one winning bid overall) bid language. The payment rule is either pay-as-bid or core-selecting. Activity rules and an increment requirement apply to all auctions.	Auction formats with a compact bid language (pay-as-bid payment rule) are more efficient (yield higher auctioneer revenue) than those with a fully expressive bid language (core-selecting payment rule). In ascending clock auctions with a compact bid language, bidders focus on the most profitable license (package).	Both simplifying the bid language and the payment rule may be beneficial for efficiency and auctioneer revenue in spectrum auctions, although pre-defining license packages to simplify the bid language in combinatorial auctions may be a difficult task for regulators.

TABLE A.3: *Economic laboratory experiments on the regulation of transportation systems.*

	Policy problem	Experimental design	Experimental results	Policy implications
Cox et al. (2002)	Comparison of a competition on (revenue-generating bids on route/time-slot combinations) and for (fare-structure bids on regional monopolies) the market mechanism to allocate track capacity on state-owned railways considered by the Dutch Ministry of Transport in the privatization of passenger rail service.	Four human train operators (experienced students) bid for allocation rights and route scheduling over five time slots with varying demand in a six node railway grid for three periods. With competition on (for) the rails, first a combinatorial multiround ascending (simultaneous multiround descending) auction is used with a route/time-slot combination (route with a regulated minimum schedule) as a single auction item and license fees (passenger fares) as prices is used and second trains are scheduled two (six) times.	The number of scheduled trains is highest and public revenue is positive under competition on the market. The number of transported passengers is higher, ticket prices are lower, and efficiency is higher under competition for the market than competition on the market, unless the minimum schedule is relatively inefficient.	A competition for the rail mechanism may be more efficient and beneficial for consumers, however, it is crucial that the regulator is capable of determining a relatively efficient mandatory minimum schedule.
Isacsson and Nilsson (2003)	Efficiency assessment of auction mechanisms to allocate track capacity on state-owned railways by varying the auction's pricing and stopping rules.	Four to eight human train operators (experienced subjects) with asymmetric valuations for 10 timed connections between two stations bid in an auction for 15 to 16 periods. The simultaneous auction's pricing rule (first-price or second-price) is varied between-subject, whereas its stopping rule (sealed-bid or multiround ascending) and conflicts due to free-riding incentives (three types of increasing degree of conflict) are varied within-subject over time.	Efficiency is above 90% throughout, similar in the second-price sealed-bid auction as in the multiround ascending auctions, but lower in the first-price sealed-bid auction. Auctioneer revenue is similar under all auction mechanisms. The number of train operators has no effect.	Several auction mechanisms, in particular multiround ascending auctions as well as a second-price sealed-bid auction, may be used to allocate track capacity on state-owned railways without implications for allocative efficiency and public revenue.

TABLE A.4: *Economic laboratory experiments on the regulation of emission permit trading markets.*

	Policy problem	Experimental design	Experimental results	Policy implications
Mougeot et al. (2011)	Potential of non-compliance speculation bidders in mitigating collusion based on cheap-talk in auction formats discussed for the initial allocation of emission permits as part of the emission trading scheme in the EU.	Six human firms, thereof either all emitters with asymmetric marginal abatement cost or four emitters and two speculators, first chat with each other (cheap-talk) and then compete for a fixed number of emission permits in either a uniform price sealed-bid auction or a uniform price multiround ascending clock auction during each of 12 periods (between-subject). Acquired permits are then traded in a uniform price sealed-bid double auction. Emitters face non-compliance penalties.	Auctioneer revenue is higher with the uniform price sealed-bid auction than the uniform price multiround ascending clock auction and higher (lower) with speculators in the former (latter) auction format, whereas efficiency is lower with speculators.	Opening emission permit auctions to non-compliance speculators may deteriorate efficiency, but increase auctioneer revenue if the auction format for initial emission permit allocation does not facilitate cheap-talk collusion from the outset which holds for a uniform price sealed-bid auction.
Cason and Gangadharan (2011)	Efficiency of cross-country trading enabled through governmental institutions or direct firm to firm trading in emissions permit programs, e.g., in an effort to meet international emissions reduction goals.	16 human firms with asymmetric marginal values of acquired (buyer role) or marginal cost of sold (seller role) permits are distributed across two high cost and two low cost geographical markets and trade in a continuous double auction for 15 periods. Cross-market trading may either be allowed, prohibited, or enabled through four human intermediaries with one in each market who can trade among each other and are the only ones allowed to bank permits over time.	Efficiency (price dispersion) is highest (lowest) with direct cross-market trading, followed by intermediary cross-market trading, and lowest (highest) if cross-market trading is prohibited. Profit of buyers in high cost markets and sellers in low cost markets is increased through cross-market trading.	Linking emission permit trading across countries with differing marginal abatement cost may increase efficiency, even more so if international permit trade is not ceded to governmental institutions as the additional level of trading would increase transaction cost.

TABLE A.4: *Continued.*

	Policy problem	Experimental design	Experimental results	Policy implications
Stranlund et al. (2011)	Potential of emission report audits in ensuring permit compliance and truthful emission reporting in emission trading programs.	Eight human firms of four types with asymmetric initial emission permit allocations and marginal abatement costs trade permits in a continuous double auction and then report emissions, are audited with fixed probability, and, if audited, pay a penalty for permit violations during six periods in each of three independent rounds. Rounds differed in that audit probability is high, low, or compliance is forced. Banking is allowed and permit supply drops after the first three periods.	Permit compliance rate is high throughout and unaffected by audit probability. Emission report violations and emissions are higher and permit price is lower with a low than with a high audit probability.	In emission trading programs that cannot rely on continuous emissions monitoring enforcement and compliance may also be ensured with imperfect emissions monitoring and low penalties for false reporting and permit violations, although truthful emission reporting may increase in audit probability.
Camacho-Cuena et al. (2012)	Incentives to invest in emission abatement technologies under alternative emission permit auction formats with or without preceding initial allocation of permits, i.e., grandfathering, proportional to emitters' maximum emission levels.	18 human firms produce a good at five different emission levels. Over six independent periods first firms may make a fixed investment in an abatement technology reducing emissions and second emission permits are either allocated in a uniform price multiround ascending clock auction, distributed freely and re-allocated in a continuous single unit double auction, or distributed freely and re-allocated in a uniform price multiround ascending clock double auction (between-subject).	Dynamic investment efficiency and overall efficiency are similar across allocation mechanisms. Final allocative static efficiency of the permit market is higher in the continuous single unit double auction than in the multiround ascending clock double auction, but unaffected by the initial allocation mechanism.	Emission permit auctions may provide the right incentives to invest in emission abatement technologies and neither grandfathering or auctioning are clearly superior in terms of dynamic or static efficiency.

TABLE A.4: *Continued.*

	Policy problem	Experimental design	Experimental results	Policy implications
Grimm and Ilieva (2013)	Evaluation of key design features of the EU Emission Trading System, in particular initial distribution of emission permits by auction or free allocation, i.e., grandfathering, which are both elements of the real-world market.	16 human firms produce a good at uncertain emissions. Emissions are traded in four subsequent continuous double auctions during each of four compliance periods. Emission permits may be banked and are allocated either at the beginning of a compliance period—freely or in a uniform price sealed-bid auction—or before each trade stage in a similar auction (between-subject). Firms may costly abate emissions and face penalties for excess emissions over their final permit holdings.	Optimal efficiency is never reached. Final allocative efficiency is higher with a single permit auction than with multiple permit auctions or equal free permit allocation. Permit price, abatement level, and permit banking are only initially higher if permits are allocated by auction than freely by grandfathering.	Initial distribution of emission permits by auctioning may lead to higher final efficiency than free allocation according to expected needs, although the latter is connected to a high initial efficiency. Thus, emission trading at secondary markets may decrease efficiency of initial grandfathering allocations.
Stranlund et al. (2014)	Potential of price controls and permission of permit banking in containing permit price risk such as price volatility over time and price dispersion at a certain time in emission permit markets.	Eight human firms of four types with asymmetric cash endowments, initial emission permit allocations, and uncertain marginal abatement costs trade permits in a continuous double auction for 13 to 20 periods. Banking, i.e., carrying emission permits over to future periods, may be allowed and/or price controls, i.e., a price ceiling and a price floor, may be enacted (between-subject). Compliance is enforced.	Permit price volatility between and dispersion within periods are lower with banking or price controls, whilst emission volatility between periods is higher. Price volatility is again lower with combined banking and price controls. Initial permit price is higher (lower) with banking (banking and price controls).	Combined price controls and allowance of banking in emission permit markets may decrease price volatility without affecting price dispersion, but at the cost of higher emission volatility which may be detrimental in the control of flow pollutants with strictly convex damages.

TABLE A.4: *Continued.*

	Policy problem	Experimental design	Experimental results	Policy implications
Taschini et al. (2014)	Effect of irreversible investments in emission abatement technologies and the presence of traders that are not subject to environmental regulation on emission permit markets in a cap and trade program.	12 human regulated firms with an asymmetric initial number of emission permits each produce a good whose demand and thus, related emissions, are stochastic. In each of four independent rounds, firms trade permits in a uniform price sealed-bid auction during 20 periods, face penalties for excess emissions, and may make an irreversible abatement investment cutting their emissions. Additionally, zero, three, or six human non-regulated firms (between-subject) compete in the permit market.	Permit price is higher than the marginal cost of abatement. With non-regulated firms price and liquidity in the permit market are higher and excess emissions of regulated firms are lower.	In industries with irreversible investments abating emissions permit prices may not decrease after investments, thereby increasing the total cost of meeting compliance obligations. Permit traders that are not subject to environmental regulation, e.g., banks or eco-friendly firms, may increase prices and liquidity.
Shobe et al. (2014)	Evaluation of key design features of the cap and trade program implemented in the market for greenhouse gas emission permits in California, US.	12 human firms (experienced students) produce a fixed price good and are endowed with a decreasing number of emission permits during each of 12 periods. Half of the firms need one (two) permit(s) to produce one unit of their good. Again half of each group must consign all (none) of their endowed permits to trade in a highest rejected or lowest accepted bid uniform price sealed-bid auction, after which a spot market follows. Permit holding limits are either tight or loose, permits in a price containment reserve are released either in or post auction.	Efficiency and permit liquidity (price variability) are lower (is higher) with tight than loose holding limits. Market outcomes are unaffected by the release timing of the price containment reserve and the auction's pricing rule.	The holding limit in California's emission permit market, intended to prevent market manipulation, may instead decrease liquidity, which is suggested to facilitate the exploitation of market power by large greenhouse gas emitters. The sale of price containment reserves may serve as a buffer for low liquidity times.

TABLE A.4: *Continued.*

	Policy problem	Experimental design	Experimental results	Policy implications
Perkis et al. (2015)	Potential of price controls such as a hard (soft) price ceiling fixing the maximum price (supply) but not the maximum supply (price) of emission permits in keeping their prices in permit auctions close to a target price specified by the regulator.	Eight human firms with asymmetric initial emission permit allocations and marginal abatement costs trade permits in a continuous double auction subject to a price floor during each of 14 periods. In each period a second continuous double auction may follow after a permit reserve is allocated freely with either no price controls, a price ceiling fixing the maximum price, or a price floor fixing the maximum supply, both at the same target price (between-subject).	Price converges above equilibrium with a reserve auction with a price floor, to equilibrium without a reserve auction or with a reserve auction without price controls, and below equilibrium to the target price with a reserve auction with a price ceiling in both auctions.	Hard price ceilings may effectively limit prices in emission permit markets and thus the risk for emitting firms connected to permit trading, whereas a soft price ceiling specifying a minimum price in a permit reserve auction may lead to high prices but limits the total amount of emissions.

TABLE A.5: *Economic laboratory experiments on the regulation of conservation markets.*

	Policy problem	Experimental design	Experimental results	Policy implications
Banerjee et al. (2012)	Effectiveness of conservation programs based on an agglomeration bonus that rewards spatial coordination between landowners depending on the number of landowners participating in the conservation program.	Six or 12 human landowners arranged on a circle own two parcels with different conservation costs each and choose which of the two to conserve for 20 periods. Payoffs depend on own and direct neighbors' choices and are lowest (highest) if only one landowner chooses the cheap parcel and the others choose the expensive parcels (all landowners choose the cheap parcel). Landowners have information on direct neighbors' actions.	Landowners choose the parcel with lower conservation cost and thus benefit from agglomeration bonus less in a bigger network with 12 than six landowners as coordination is impeded. However, global coordination is obtained in half of all networks.	Conservation programs with an agglomeration bonus may be more likely to lead to the efficient ecosystem service of globally coordinated land management on landscapes with fewer landowners, so that conservation programs may be organized to result in a limited number of participants.
Banerjee et al. (2014)	Improving the effectiveness of conservation programs based on an agglomeration bonus that rewards spatial coordination between landowners through regulated dissemination of information on neighboring landowners' program participation.	12 human landowners arranged on a circle choose between conservation management or nature farming on their land for 30 periods. Payoffs depend on own and direct neighbors' choices and are lowest (highest) if only one landowner chooses nature farming and the others conservation management (all landowners choose nature farming). Landowners have information on direct neighbors' actions or additionally on their direct neighbor's direct neighbors (between-subject).	Landowners choose nature farming more often with more information, but the share of nature farming choices and thus coordination drops sharply over time.	Effectiveness of an agglomeration bonus in incentivizing landowners to coordinate their land use for ecosystem services may be improved by implementing information dissemination about program participation among landowners.

TABLE A.5: *Continued.*

	Policy problem	Experimental design	Experimental results	Policy implications
Banerjee et al. (2015)	Effect of disclosure of information on regulators' preferences for spatially adjacent conservation areas on the efficiency of conservation auctions without a subsidized agglomeration bonus.	Six human landowners arranged on a circle compete in an inverse discriminatory price multi-round auction for conservation activities during 13 periods with five to 10 auction rounds each. Environmental benefits from conservation are asymmetric across landowners and time-variant unknown (known) to them (the regulator). Due to agglomeration benefits the regulator has preferences for bids from adjacent landowners and this information may or may not be disclosed (between-subject).	Landowners' profits are higher with information on agglomeration benefits, but the environmental effectiveness in terms of allocative efficiency is unchanged.	Disclosing regulators' preferences for spatially adjacent conservation areas may not lead to a more efficient allocation conservation activities but rather allow landowners to coordinate on higher prices.
Parkhurst et al. (2015)	Improving the effectiveness of conservation programs with incentive mechanisms that aggregate conservation decisions of geographically connected landowners such as an agglomeration bonus to prevent habitat fragmentation.	Four human landowners own 25 asymmetrically valuable parcels of land each (in a 10×10 grid) and face a conservation set-aside requirement of five parcels each for 20 periods. No value may be extracted from conserved parcels. Set-aside requirements may be tradable in a continuous multiunit double auction and landowners may additionally gain an agglomeration bonus for each border shared between two own conserved parcels.	The number of shared borders between conserved parcels (aggregate profits) are higher (lower) if set-aside requirements are tradable, even more so with the agglomeration bonus. Aggregate production is higher if set-aside requirements are tradable.	Allowing landowners to trade set-aside requirements in conservation programs may lead to a more efficient allocation and thus mitigate habitat fragmentation, even more so if landowners receive a subsidized agglomeration bonus for connected conservation areas.

TABLE A.6: *Economic laboratory experiments on the regulation of water markets.*

	Policy problem	Experimental design	Experimental results	Policy implications
Tisdell (2011)	Efficiency of a uniform price sealed-bid auction compared to a continuous multiunit double auction in the water market of a specific catchment area in Australia.	10 human farmers arranged in a nodal network can each plant six different crops with asymmetric values and maximum water usage and have asymmetric initial water allocations and thus, supply and demand, resulting in market power for some farmers. Water may be traded either in a uniform price sealed-bid auction or a continuous multiunit double auction for 20 periods. All bids and offers are made public after trading.	Allocative efficiency, water price, and traded quantity are higher in the uniform price sealed-bid auction than in the continuous multiunit double auction. For the latter, price and quantity are even lower than the competitive equilibrium.	A continuous multiunit double auction may not be more efficient than the traditional uniform price sealed-bid auction in allocating water if asymmetries in market power, as in real water markets, are taken into account.
García-Gallego et al. (2012a,b)	Short- and long-run efficiency of different market structures, in particular a private monopoly, a private or coordinated duopoly, and a public utility acting as a social planner in allocating water of differing quality to households and farmers.	One (two) human producers with an initial stock of high and low (either high or low) quality water sell it at varying extraction cost over a total of 50 periods to farmers and households valuing quality differently. Each period, a fixed amount of water is added to producers' stocks and demand is computerized to maximize consumer surplus. Producers choose a price schedule for the first five units of each water type. The market is cleared at a uniform price for each water type.	Price (quality to price ratio) is highest (lowest) in the private monopoly, followed by the duopoly, and lowest (highest) in the public monopoly. A coordinated duopoly is similar to the private monopoly. Resources are overexploited in all market structures, but deplete fastest in the public monopoly.	Water provision from a monopolistic public utility may be beneficial for consumers at the cost of higher resource overexploitation; however, the latter arises not only from market design but also due to human decision errors.

TABLE A.6: *Continued.*

	Policy problem	Experimental design	Experimental results	Policy implications
Holt et al. (2012)	Efficiency of alternative mechanisms to allocate water among competing farmers if water is a scarce common-pool resource with unidirectional flow as in the case of an exogenously filled water canal.	Six human farmers organized along a water canal own four fields with asymmetric time-variant productivity each and decide sequentially on water appropriation from a total of 12 units in each of six periods. The productivity of the three downstream farmers' fields is higher than the upstream farmers'. Irrigation triples the value of a field. Either cheap-talk, bilateral bargaining, a uniform price auction of water rights, or an optimal irrigation fee is implemented (between-subject).	Allocation efficiency is highest with the optimal irrigation fee, followed by the multiunit uniform price auction, followed by bilateral bargaining and cheap-talk (indistinguishable), and lowest in the baseline scenario.	An irrigation fee based on a Pigouvian tax may lead to optimal efficiency in a water market characterized by a common-pool resource with unidirectional flow. If an optimal irrigation fee cannot be calculated due to uncertainty, a multiunit uniform price auction may lead to high efficiency.
Lefebvre et al. (2012)	Effect of two security level water rights, as implemented in water markets in the western part of the US and two Australian states, compared to one security level water rights on allocation of water and risk of not being served unter water scarcity.	Six human farmers of two types with asymmetric crop values and irrigation-sensitivity trade first water rights and second the distributed water allocations, the quantity of which varies between two random weather scenarios, in separate continuous double auctions in each of 9 to 12 periods. There may be high and low security water rights with the former traded first and served first in case of scarcity and there are transaction cost either for trading water rights or water allocations.	Efficiency and profit are higher with two (one) security level water rights if there are no transaction cost for trading water rights (water allocations). With two security level water rights, high and low security types are allocated such that the variability of profits is reduces for less risk-tolerant farmers.	Security-differentiated water rights may enable an allocation of the risk of not being served under water scarcity according to the risk-preferences of farmers. However, the effect of the number of security levels on efficiency may heavily depend on transaction cost for trading water rights and allocations.

TABLE A.6: *Continued.*

	Policy problem	Experimental design	Experimental results	Policy implications
Hansen et al. (2014)	Potential of water options to increase water allocation efficiency in arid regions with highly weather-dependent water supply.	Four human sellers have asymmetric time-variant initial water allocations which are either certain or uncertain (within-subject) and one or four human buyers (between-subject) with asymmetric time-variant water valuations trade in a continuous double auction during each of 22 periods. There may be another concurrent continuous double auction for options for water rights, which allow buyers to purchase a unit of water at a fixed price after the state of the nature is revealed.	Price and quantity is lower with a single dominant buyer than four competing buyers. Efficiency is higher when water options can be traded and gains from trade are not only higher but also more evenly distributed.	Water option markets may lead to more efficient water allocations and equalized gains from trade, especially across sellers facing a single dominant buyer, which is reminiscent of the California water market structure.

TABLE A.7: *Economic laboratory experiments on price controls.*

	Policy problem	Experimental design	Experimental results	Policy implications
Dufwenberg et al. (2007)	Effect of price floors on competition.	Two or four human firms with zero production cost compete in a homogeneous Bertrand with inelastic demand over 10 periods with random matching. Firms are either subject to a low or high price floor.	With two (four) firms, prices are lower (similar) under a high than (and) a low price floor.	Price floors above production cost ensuring firms a profit may intensify competition rather than relaxing it.
Engelmann and Normann (2009); Engelmann and Müller (2011)	Effect of price ceilings on competition.	Two (four) human firms with asymmetric (symmetric) and increasing marginal production cost compete in homogeneous goods and choose prices in a posted offer market over 60 periods. Firms are subject to a (low) price ceiling either in the first or second 30 periods (or first to a high and then a low price ceiling). Demand is decreasing in price and either known or unknown to firms.	Prices are lower with the price ceiling than with unconstrained pricing and rise if the price ceiling is lifted over time but do not differ between a low or high price ceiling. The degree of collusion is unaffected.	Price ceilings below the monopoly price may intensify competition rather than relaxing it.
Berninghaus et al. (2012)	Effect of a price regulation that allows firms to raise their prices only at specified times on competition.	Two human symmetric firms compete in a differentiated Bertrand and choose one out of four prices over at least 30 periods. Firms are allowed to raise their prices either in any period or only in certain known periods.	Prices are higher and less volatile if firms may raise their prices only in regulated periods than in any period.	Price volatility regulation specifying the times of permitted price increases may lead to higher prices and thus facilitate collusion.
Horstmann and Krämer (2013)	Effect of a uniform pricing constraint across geographic markets on competition.	Two human symmetric firms compete in two separate homogeneous Bertrand markets over 10 periods. Firms are either allowed to choose different prices for both markets or obliged to choose the same price for both markets.	Prices are higher if firms are allowed discriminate prices across markets than if they have to choose a uniform price.	Uniform pricing constraints across geographic markets may intensify competition.

TABLE A.8: *Economic laboratory experiments on leniency programs for cartel detection.*

	Policy problem	Experimental design	Experimental results	Policy implications
Apesteguia et al. (2007)	Potential of different leniency programs that grant self-reporting cartel members a reward or a reduced fine compared to antitrust policy based on fixed cartel fines in deterring cartel formation and maintaining competition.	Three human firms compete in a homogeneous Bertrand with inelastic demand for one period. Firms may be able to chat, but for a chat to occur all firms have to indicate that they want to form a cartel. Price agreements are not binding. After learning the market price, firms may report an existing cartel and either all firms pay a fine, or the reporting firm(s) share a fine reduction or the fines of the non-reporting firm(s).	Market prices are lower if reporting firms get a fine reduction than if they are rewarded the fines of the non-reporting firms or if all firms pay a fine. The number of (reported) cartels is lower (higher) if the reporting firms pay a lesser than if all pay the same fine.	Leniency programs implemented in many OECD countries that grant self-reporting cartel members amnesty from fines or reduced fines in case of simultaneous reporting from multiple cartel members may effectively deter cartel formation and intensify competition.
Hinloopen and Soetevent (2008)	Potential of leniency programs that encourage early self-reporting of cartels through amnesty only for the first reporting cartel member in deterring cartel formation and mitigating cartel duration.	Three human firms compete in a homogeneous Bertrand with inelastic demand for at least 20 periods. Firms may be able to communicate preferred price ranges via a structured protocol if all firms indicate that they want to form a cartel. Price agreements are not binding. A cartel is detected and fined either with 0% or 15% probability. Beforehand, firms may be able to costly report a cartel for amnesty (a fine reduction) for the first (second) reporting firm.	Prices, the number of cartels, and cartel duration are lower if cartels can be reported within a leniency program than if cartels can only be detected but not reported, if cartels cannot also not be detected, or even if cartels cannot be formed.	Leniency programs that grant amnesty for the first and a fine reduction for subsequent self-reporting cartel members irrespective of the cartel's duration may be more effective in deterring cartel formation and intensifying competition than relying solely on cartel detection.
Hamaguchi et al. (2009)	Potential of leniency programs based on rewards or fine reductions in dissolving existing cartels.	Two or seven human firms form a cartel for at least one period and are detected with 10% probability. Firms can report the cartel for either a reward of others' then maturing fines, a partial or a full reduction of the fine. This is granted to either the first or all reporting firms.	Cooperation rates are lower with more firms and if reporting is rewarded than if it leads to fine reduction, but not different whether this applies to all or only the first reporting firm.	Leniency programs that grant rewards to self-reporting firms may be more effective in dissolving existing cartels than programs based on fine reductions.

TABLE A.8: *Continued.*

	Policy problem	Experimental design	Experimental results	Policy implications
Bigoni et al. (2012, 2015b)	Potential of leniency programs based on rewards or fine reductions compared to fines in deterring cartel formation and maintaining competition.	Two human firms compete in a differentiated Bertrand with elastic demand for at least 20 periods with a random matching with 15% probability. Firms can communicate preferred prices via a structured protocol if both indicate that they want to form a cartel. Price agreements are not binding. Firms can costly report a cartel before and after price choices for either a reward of the other firm's fine, no or a full reduction of the fine for the first reporting firm. A cartel is detected and fined either with 0% or 10% probability.	Prices and the number of (reported) cartels are lower (higher) with a reward or fine reduction for the first reporting firm than if all firms pay the fine. This holds even more so if fines are higher although expected fines are the same. If all firms pay the fine prices are even higher than if cartels are permitted. Prohibited cartels are more stable and choose higher prices.	Leniency programs both based on amnesty or rewards may deter cartel formation and reduce prices more than traditional antitrust law enforcement based on fines, which may even increase prices compared to a laissez-faire policy. However, leniency programs may also increase prices in those cartels that do form.
Hinloopen and Onderstal (2014)	Effect of traditional antitrust policy and leniency programs on cartel formation in single round and multiround auctions.	Three human firms compete in either a first-price sealed-bid auction or a multiround ascending auction during 40 periods. Firms form a cartel if all vote to cooperate, which constitutes a non-binding agreement that one randomly selected firm will be the only bidder and compensates the other firms. A cartel is detected and fined either with 0% or 15% probability. Beforehand, firms may be able to costly report a cartel for a fine reduction whose value decreases in expectation with the number of reporting firms.	In the first-price sealed-bid auction, the number of cartels is lower if cartels can be detected but not reported or detected and reported than if they cannot, firms defect from an agreement mostly if cartels can be detected but not reported, and winning bids are lowest if cartels can be detected and reported.	Antitrust policy may have no effect in multiround ascending auctions. In first-price sealed-bid auctions, traditional antitrust policy and leniency programs may both deter cartel formation, while the former (latter) decreases (increases) cartel stability and increases (decreases) winning bids.

TABLE A.9: *Economic laboratory experiments on vertical mergers.*

	Policy problem	Experimental design	Experimental results	Policy implications
Martin et al. (2001)	Anti-competitive effect of clearing a vertical merger between a downstream firm and a monopolistic upstream firm or of the publication of upstream offers to downstream firms by a monopolistic upstream firm on foreclosure of downstream competitors.	One human monopolistic upstream firm makes either public or secret price-quantity take-it-or-leave-it offers to either two human downstream firms or is vertically integrated with one downstream firm and makes an offer only to the remaining downstream firm over 10 periods with either random or fixed matching. Downstream firms compete in a homogeneous Cournot, provided they accept their respective upstream firm's offer.	Upstream output is restricted to the monopoly level more frequently if upstream offers are public or if the upstream firm is integrated than if upstream offers are secret, but is not differently frequent between the former two. More downstream surplus is extracted if the upstream firm is integrated.	Vertical integration of or merger with a monopolistic upstream firm may facilitate foreclosure of downstream competitors in a broad sense, although the upstream monopolist may restrict output equally if its offers to downstream firms are public.
Normann (2011)	Anti-competitive effect of clearing a vertical merger between a downstream firm and an upstream firm in duopolistic markets on foreclosure of downstream competitors.	Two human upstream firms compete in a homogeneous Bertrand with demand from two computerized downstream firms decreasing in price over 15 or 25 periods with random or fixed matching. One upstream firm may be integrated with one of the downstream firms making its profit strictly increasing in price.	Upstream prices are higher in markets with a vertically integrated firm than with separated firms. The vertically integrated firm charges higher prices than the non-integrated upstream firm.	Vertical integration or mergers in duopolistic markets may increase downstream prices and facilitate collusion between upstream firms and thus facilitate foreclosure of downstream competitors in a broad, but not in a narrow sense.

TABLE A.10: *Economic laboratory experiments on product bundling.*

	Policy problem	Experimental design	Experimental results	Policy implications
Caliskan et al. (2007); Muris and Smith (2008)	Effect of allowing or prohibiting a dominant firm to bundle products across markets on competitors and consumers.	Five human firms, of them one dominant firm selling products A and B, one competitive fringe selling A, and three firms selling B have asymmetric fixed cost and capacities, market-specific marginal cost, and choose prices in a posted offer market over 210 five seconds long periods. The dominant firm is either allowed or not to bundle both products and the fringe either exists or not.	Consumer and total surplus are unaffected by bundling and it deters entry to the B market only if the parameters favor exclusion. The presence of the fringe reduces transaction prices, but does not interact with bundling.	Bundling may not have adverse welfare effects and exclusion effects only in special cases so that regulation should not ban bundling per se.
Hinloopen et al. (2014)	Effect of allowing or prohibiting a firm to bundle products from a monopolistic and a competitive market on competitors and consumers.	Two human firms, of them one multi-product firm selling products D and M and one firm selling only D choose quantities in either a homogeneous Cournot or Stackelberg—with the dominant firm as first mover—over 15 periods with random matching. The dominant firm either bundles always and does not offer products separately, is prohibited to bundle products, or may decide whether to bundle or not.	Total surplus and profit of the single-product firm (consumer surplus and profit of the multi-product firm) are lower (unaffected) if bundling occurs, but the mere possibility to bundle does not affect market performance as the multi-product firm decides rarely to bundle.	The possibility to bundle may not have adverse welfare effects whereas a bundle itself does, although it occurs rarely. This supports the current regulatory policy in the EU to investigate each instance of bundling separately for abuses of a dominant position.

TABLE A.11: *Economic laboratory experiments on transparency regulation and information dissemination.*

	Policy problem	Experimental design	Experimental results	Policy implications
Huck et al. (1999, 2000)	Effect of publishing either only aggregate industry or also firm-specific data on competition.	Four symmetric firms compete in either a homogeneous Cournot, a differentiated Cournot, or a differentiated Bertrand over 40 periods. Firms are informed about either only average competitors' quantity or also firm-specific prices or quantities and profits of competitors.	Quantities (prices) in Cournot (Bertrand) markets are higher (similar) with firm-specific than (as with) aggregate information.	More information in form of an obligation to publish firm-specific data on output and profits may intensify competition in industries.
Dufwenberg and Gneezy (2002)	Effect of announcing none, winning, or winning and losing bids across inverse first-price sealed-bid auctions on competition.	Two firms compete in an inverse first-price sealed-bid auction with known common value, i.e., a homogeneous Bertrand, over 10 periods with random matching over 12 firms. Firms are informed about either bids of all 12 firms, only the six winning bids, or no bids at all.	Prices are initially similar but later higher if bids of all 12 firms are known and not different between the other two information scenarios.	Auctioneers may inhibit tacit collusion in inverse first-price sealed-bid auctions if they announce winning but not losing bids across parallel auctions.
Offerman et al. (2002)	Effect of publishing either only aggregate industry or also firm-specific data on competition.	Three symmetric firms compete in a homogeneous Cournot with increasing marginal cost over 100 periods. Firms are informed about either only total quantity and price, also firm-specific quantities, or, above that, also firm-specific profits.	Total quantities are highest with most, followed by least, and lowest with medium information. Individual quantities are more frequently collusive with more information.	More information in form of an obligation to publish firm-specific data on output (and also profit) may relax (intensify) competition in industries.

TABLE A.11: *Continued.*

	Policy problem	Experimental design	Experimental results	Policy implications
Altavilla et al. (2006)	Effect of publishing either firm-specific data on competitors or aggregate data from related markets on competition.	Two symmetric firms compete in either a homogeneous Cournot, a differentiated Cournot, or a differentiated Bertrand over 20 periods. Firms are informed about either only own profit, also the average profit across all duopolies, or instead also the price/quantity and profit of the competitor. There is a random (fixed) matching in the first two (the last) cases.	Quantities (prices) in Cournot (Bertrand) markets are lowest (highest) with information on average profit across duopolies (only on own profit). Price and quantity dispersion is reduced with more information.	More information in form of firm-specific data on output and profits (aggregate data on profits from other markets) may intensify (relax) competition in industries.
Bruttel (2009)	Effect of announcing either only competitors' bids or bids across parallel inverse first-price sealed-bid auctions on competition.	Two firms compete in an inverse first-price sealed-bid auction with known common value, i.e., a homogeneous Bertrand, over 10 or 25 periods with random matching over 12 firms. Firms are informed about either bids of all 12 firms or only their competitor's bid.	Prices are initially similar but later higher if bids of all 12 firms are known and approach the theoretical prediction if only the competitor's bid is known.	Auctioneers may inhibit tacit collusion in inverse first-price sealed-bid auctions if they do not announce bids across parallel auctions.
Sluijs et al. (2011); Henze et al. (2015)	Effect of transparency regulation, i.e., an obligation to disclose information about product quality, on quality and prices in markets for experience goods, e.g., broadband internet access or health care plans.	Two human sellers choose product quality and, after observing each others', choose prices. Four buyers valuing quality in different degrees decide from which seller to buy having full information, no information, full information only for half the buyers, or an imperfect signal on qualities (between-subject). Buyers with full information are computerized, the others are played by humans. Buyers are subject to a random matching for 30 periods.	Product quality is ordered (highest to lowest): full information, full information only for half the buyers, imperfect signal, no information. Time trend is negative for the latter, but positive for the other information treatments. Prices do not differ across treatments.	Transparency regulation may be effective in protecting consumers from low product quality and increasing welfare as it increases quality of experience goods substantially whilst keeping prices at similar levels.

Appendix B

Theoretical Analyses

B.1 Oligopoly competition

Let the relevant industry consist of $n \in \mathbb{N}$ firms. Each firm produces one good and goods between firms are differentiated. Considering the representative consumer's utility function suggested by Singh and Vives (1984) and extending the generalization by Häckner (2000), inverse demand for firm $i \in \{1,...,n\}$ is given by

$$p_i = \omega_i - \lambda_i q_i - \gamma \sum_{j \neq i} q_j$$

with $\omega_i, \lambda_i > 0, \forall i \in \{1,...,n\}$ and the degree of substitutability γ. If $\gamma < 0$ goods are complementary, if $\gamma = 0$ goods are independent of one another, and if $\gamma > 0$ they are substitutes. ω_i may be interpreted as quality and thus, differences among firms as vertical differentiation. With substitute goods, ω_i is also firm i's reservation price. λ_i is the elasticity of inverse demand of firm i's good. For simplicity, assume that $\lambda_i = \lambda, \forall i \in \{1,...,n\}$ and let $\theta = \frac{\gamma}{\lambda}$. This bounds $\theta \leq 1$ with goods being perfect substitutes if $\theta = 1$. The inverse demand for firm i then transforms to

$$p_i = \omega_i - \lambda \left(q_i + \theta \sum_{j \neq i} q_j \right). \tag{B.1}$$

Note that firms are vertically differentiated, i.e., asymmetric, and that symmetry requires $\omega_i = \omega, \forall i \in \{1,...,n\}$. To calculate the demand for firm i, summarize Equation (B.1) over all n firms, which results in

$$\sum_{i=1}^{n} p_i = \sum_{i=1}^{n} \omega_i - \lambda \left(\sum_{i=1}^{n} q_i + \theta(n-1) \sum_{i=1}^{n} q_i \right)$$

using $\sum_{i=1}^{n} \sum_{j \neq i} q_j = (n-1) \sum_{i=1}^{n} q_i$. Solving this for $\sum_{i=1}^{n} q_i$ yields

$$\sum_{i=1}^{n} q_i = \frac{1}{\lambda(1+\theta(n-1))} \sum_{i=1}^{n} (\omega_i - p_i).$$

As a transformation of this equation, noting that

$$\begin{aligned} \sum_{i=1}^{n} q_i &= q_i + \sum_{j \neq i} q_j, \\ \sum_{i=1}^{n} \omega_i &= \omega_i + \sum_{j \neq i} \omega_j, \end{aligned} \tag{B.2}$$

and using Equation (B.1), firm i's demand for non-perfect substitutes ($\theta < 1$) is given by

$$q_i = \frac{(\omega_i - p_i)(1+\theta(n-2)) - \theta \sum_{j \neq i} (\omega_j - p_j)}{\lambda(1-\theta)(1+\theta(n-1))} \tag{B.3}$$

provided that the quantity is non-negative and with n as the number of firms with non-negative demand. Otherwise, if $q_i < 0$, firm i exits the market and its demand is zero.

With costs normalized to zero and $q_{-i} = \{q_1,...,q_n\} \setminus q_i$, firm i's profit is given by $\Pi_i = p_i q_i$ with price $p_i(q_i, q_{-i})$ as a function of quantities in Cournot competition and quantity $q_i(p_i, p_{-i})$ as a function of prices in Bertrand competition. In the following analysis of Walrasian, Nash, and collusive equilibrium prices, quantities, and profits, subscripts are used to differentiate between Bertrand and Cournot competition.

Walrasian equilibrium

In the Walrasian equilibrium, also referred to as competitive equilibrium, firms are assumed to have no market power and hence, are price-takers with all prices at marginal cost. Therefore, the Walrasian equilibrium is identical under Bertrand and Cournot competition. Setting Equation (B.1) to marginal cost, i.e., zero, it can be transformed to

$$q_i(q_{-i}) = \frac{\omega_i - \lambda\theta \sum_{j \neq i} q_j}{\lambda}.$$

Summing over all n firms gives

$$\sum_{i=1}^{n} q_i = \frac{\sum_{i=1}^{n} \omega_i - \lambda\theta(n-1)\sum_{i=1}^{n} q_i}{\lambda},$$

which, using the previous Equation together with Equation (B.2), yields the Walrasian equilibrium

$$\begin{aligned}
q_i^{Walras} &= \frac{\omega_i(1+\theta(n-2)) - \theta\sum_{j\neq i}\omega_j}{\lambda(1-\theta)(1+\theta(n-1))}, \\
p_i^{Walras} &= 0, \\
\Pi_i^{Walras} &= 0.
\end{aligned} \tag{B.4}$$

Nash equilibrium

In the Nash equilibrium under Cournot competition firm i maximizes Π_i with respect to its quantity q_i given the other firms' quantities q_{-i}. Firm i's best response is given by

$$q_i(q_{-i}) = \frac{\omega_i - \lambda\theta \sum_{j \neq i} q_j}{2\lambda}$$

and its sum over all n firms amounts to

$$\sum_{i=1}^{n} q_i = \frac{\sum_{i=1}^{n} \omega_i - \lambda\theta(n-1)\sum_{i=1}^{n} q_i}{2\lambda}.$$

Using the previous Equation together with Equation (B.2), the Cournot Nash equilibrium can be retrieved as

$$\text{(B.5)}\quad \begin{aligned} q_{Cournot,i}^{Nash} &= \frac{\omega_i(2+\theta(n-2)) - \theta\sum_{j\neq i}\omega_j}{\lambda(2-\theta)(2+\theta(n-1))}, \\ p_{Cournot,i}^{Nash} &= \frac{\omega_i(2+\theta(n-2)) - \theta\sum_{j\neq i}\omega_j}{(2-\theta)(2+\theta(n-1))}, \\ \Pi_{Cournot,i}^{Nash} &= \frac{(\omega_i(2+\theta(n-2)) - \theta\sum_{j\neq i}\omega_j)^2}{\lambda(2-\theta)^2(2+\theta(n-1))^2}. \end{aligned}$$

In the Nash equilibrium under Bertrand competition firm i maximizes Π_i with respect to its price p_i given the other firms' prices p_{-i}. Firm i's response function can be calculated as

$$p_i(p_{-i}) = \frac{\omega_i}{2} - \frac{\theta\sum_{j\neq i}(\omega_j - p_j)}{2(1+\theta(n-2))}.$$

Summing over all n firms yields

$$\sum_{i=1}^{n} p_i = \frac{\sum_{i=1}^{n}\omega_i}{2} - \frac{\theta(n-1)\sum_{i=1}^{n}(\omega_i - p_i)}{2(1+\theta(n-2))},$$

which can be transformed using the previous Equation together with Equation (B.2) to retrieve the Bertrand Nash equilibrium

(B.6)

$$\begin{aligned} q_{Bertrand,i}^{Nash} &= \frac{(1+\theta(n-2))(\omega_i(\theta^2(n^2-5n+5)+3\theta(n-2)+2) - \theta(1+\theta(n-2))\sum_{j\neq i}\omega_j)}{\lambda(1-\theta)(1+\theta(n-1))(2+\theta(n-3))(2+\theta(2n-3))}, \\ p_{Bertrand,i}^{Nash} &= \frac{\omega_i(\theta^2(n^2-5n+5)+3\theta(n-2)+2) - \theta(1+\theta(n-2))\sum_{j\neq i}\omega_j}{(1+\theta(n-1))(2+\theta(n-3))(2+\theta(2n-3))}, \\ \Pi_{Bertrand,i}^{Nash} &= \frac{(1+\theta(n-2))(\omega_i(\theta^2(n^2-5n+5)+3\theta(n-2)+2) - \theta(1+\theta(n-2))\sum_{j\neq i}\omega_j)^2}{\lambda(1-\theta)(1+\theta(n-1))^2(2+\theta(n-3))^2(2+\theta(2n-3))^2}. \end{aligned}$$

As Häckner (2000) shows, Nash prices are always higher under Cournot competition than under Bertrand competition for substitute goods ($\theta > 0$). Instead, if goods are complements ($\theta < 0$) and vertical differentiation between firms is high, Nash prices of low-quality firms may be higher under Bertrand competition than under Cournot competition. With respect to profits there are different nuances. For complementary goods, Nash profits are always higher under Bertrand competition than under Cournot competition. Instead, if goods are substitutes, the opposite holds unless vertical differentiation between firms is low, when Nash profits of high-quality firms may be higher under Bertrand competition than under Cournot competition.

Collusive equilibrium

In the collusive equilibrium firms employ JPM, i.e., firms behave like a single monopolist and maximize $\sum_{i=1}^{n} \Pi_i$. Therefore, the collusive equilibrium is identical under Bertrand and Cournot competition. Using Equation (B.1) and summing over the corresponding profit functions, joint profit of all n firms is given by

$$\sum_{i=1}^{n} \Pi_i = \sum_{i=1}^{n} (\omega_i q_i) - \lambda \sum_{i=1}^{n} q_i^2 - \lambda\theta \sum_{i=1}^{n} (q_i \sum_{j \neq i} q_j).$$

Noting that $\frac{\partial \sum_{i=1}^{n} (q_i \sum_{j \neq i} q_j)}{\partial q_i} = 2 \sum_{j \neq i} q_j$, the first-order condition of joint profit maximization can be calculated as

$$q_i(q_{-i}) = \frac{\omega_i - 2\lambda\theta \sum_{j \neq i} q_j}{2\lambda}.$$

Again summing over all n firms results in

$$\sum_{i=1}^{n} q_i = \frac{\sum_{i=1}^{n} \omega_i}{2\lambda} - \theta(n-1) \sum_{i=1}^{n} q_i,$$

which finally yields the collusive equilibrium using the previous Equation and Equation (B.2) as

$$\begin{aligned} q^{JPM} &= \frac{\omega_i(1+\theta(n-2)) - \theta \sum_{j \neq i} \omega_j}{2\lambda(1-\theta)(1+\theta(n-1))}, \\ p^{JPM} &= \frac{\omega_i}{2}, \\ \Pi^{JPM} &= \frac{\omega_i(\omega_i(1+\theta(n-2)) - \theta \sum_{j \neq i} \omega_j)}{4\lambda(1-\theta)(1+\theta(n-1))}. \end{aligned} \tag{B.7}$$

Note that joint profit maximizing prices are linearly connected to vertical differentiation as firm i's price in collusive equilibrium depends solely on its own quality.

Symmetric firms

In case of symmetric firms without vertical product differentiation, i.e., $\omega_i = \omega, \forall i \in \{1, ..., N\}$, i's demand function, i.e., Equation (B.3), simplifies to

$$\begin{aligned} q_i &= \frac{(\omega - p_i)(1+\theta(n-2)) - \theta \sum_{j \neq i}(\omega - p_j)}{\lambda(1-\theta)(1+\theta(n-1))} \\ &= \underbrace{\frac{\omega}{\lambda(1+\theta(n-1))}}_{\Omega} - \underbrace{\frac{1+\theta(n-2)}{\lambda(1-\theta)(1+\theta(n-1))}}_{\Lambda} p_i + \underbrace{\frac{\theta(n-1)}{\lambda(1-\theta)(1+\theta(n-1))}}_{\Theta} \frac{\sum_{j \neq i} q_j}{n-1} \\ &= \Omega - \Lambda p_i + \Theta \frac{\sum_{j \neq i} q_j}{n-1} \end{aligned}$$

with $\Omega, \Lambda, \Theta > 0$ for substitute goods ($\theta > 0$). Consequently, the Walrasian equilibrium given by Equation (B.4), which predicts marginal cost pricing, simplifies to

$$\begin{aligned} q^{Walras} &= \frac{\omega}{\lambda(1+\theta(n-1))}, \\ p^{Walras} &= 0, \\ \Pi^{Walras} &= 0. \end{aligned}$$

In the Nash equilibrium under Cournot competition firm i maximizes Π_i with respect to q_i. With symmetric firms, Equation (B.5) yields the Cournot Nash equilibrium

$$\begin{aligned} q^{Nash}_{Cournot} &= \frac{\omega}{\lambda(2+\theta(n-1))}, \\ p^{Nash}_{Cournot} &= \frac{\omega}{2+\theta(n-1)}, \\ \Pi^{Nash}_{Cournot} &= \frac{\omega^2}{\lambda(2+\theta(n-1))^2}. \end{aligned}$$

In the Nash equilibrium under Bertrand competition firm i maximizes Π_i with respect to p_i. With symmetric firms and Equation (B.6), the Bertrand Nash equilibrium is given by

$$\begin{aligned} q_{Bertrand}^{Nash} &= \frac{\omega(1+\theta(n-2))}{\lambda(2+\theta(n-3))(1+\theta(n-1))}, \\ p_{Bertrand}^{Nash} &= \frac{\omega(1-\theta)}{2+\theta(n-3)}, \\ \Pi_{Bertrand}^{Nash} &= \frac{\omega^2(1-\theta)(1+\theta(n-2))}{\lambda(2+\theta(n-3))^2(1+\theta(n-1))}. \end{aligned}$$

Finally, in the collusive equilibrium, with firms employing JPM and irrespective of Bertrand or Cournot competition, Equation (B.7) simplifies to

$$\begin{aligned} q^{JPM} &= \frac{\omega}{2\lambda(1+\theta(n-1))}, \\ p^{JPM} &= \frac{\omega}{2}, \\ \Pi^{JPM} &= \frac{\omega^2}{4\lambda(1+\theta(n-1))}. \end{aligned}$$

B.2 Multimarket contact

Proof of Proposition 6.1 The *price setting strategy* depends only on the current state of the industry and not on the sequence of states and choices that preceded it. The process described by the strategy is thus memoryless, i.e., the Markov property holds. Consequently, it may be modeled by a Markov chain. With parameters σ, ψ^X, and ψ^{-X} bounded away from zero, the process described by the price setting strategy eventually reaches JPM with probability one—and remains in this state. Therefore, JPM pricing is an absorbing state which may be reached from any initial price configuration, i.e., from any other transient state of the process. Consequently, the Markov chain describing the process is an absorbing chain. Furthermore, requiring that the random initial prices are bounded away from the JPM price leads to a unique starting state.

The square transition matrix $H = (h_{e,f})$ of such an absorbing Markov chain, indicating the probabilities of transitioning from one state e to any other state f, can be written in the following canonical form (Grinstead and Snell, 2012, p. 415–432):

$$H = \begin{bmatrix} V & W \\ \mathbf{0} & I_w \end{bmatrix}$$

Let v be the number of transient, i.e., non-absorbing, states and w the number of absorbing states. Then, V is the v-by-v transition matrix between transient states, W is the v-by-w matrix of probabilities for transitioning from any of the transient to any of the absorbing states, $\mathbf{0}$ is the w-by-v zero matrix, and I_w is the w-by-w identity matrix. A crucial property of an absorbing Markov chain is that the expected number of times the process is in state f when starting from state e—before absorption—is given by the entries of the v-by-v fundamental matrix F, which is calculated as

$$F = (I_v - V)^{-1}$$

with I_v as the v-by-v identity matrix. Given the fundamental matrix, the expected number of steps before absorption from a starting state e is the eth entry of the vector

$$\tilde{\boldsymbol{t}} = F\mathbf{1}$$

with $\mathbf{1}$ as the length-v transposed unit vector. Due to this specific property of the absorbing Markov chain and as the process described by the price signaling strategy has a unique absorbing state, $\tilde{\boldsymbol{t}}$ constitutes the vector of the expected number of steps until JPM is reached on all markets when starting in state e.

Note that, according to the price setting strategy, firms play best response at all times that they are not sending or reacting to a price signal. Any price configuration in which all prices lie below the JPM price may thus w.l.o.g. be viewed as playing best response. Consequently, each market can have three different states that will be denoted according to the following notations:

- BR denotes that both firms do not signal and thus, play best response,
- s_i denotes that firm $i \in \{A, B\}$ sends a price signal and the other firm does not, and
- JPM denotes that both firms choose the JPM price.

It is worth noting that if both firms send a price signal in the same market at the same time, JPM is already reached on that market.

Under single market contact of non-conglomerate firms, i.e., SMC, ψ^{-X} does not apply as firms operate only on one market. Instead, the process at which the price setting strategy converges to JPM depends solely on the probabilities of sending a price signal, i.e., σ, and reacting to it (on the same market), i.e., ψ^X. The state space for the single market X is given by

$$S_{SMC} = \{X^{BR}, X^{s_i}, X^{JPM}\}.$$

The transition matrix indicating the probabilities of transitioning between these three states is

$$H_{SMC} = \begin{bmatrix} (1-\sigma)^2 & 2\sigma(1-\sigma) & \sigma^2 \\ (1-\sigma)(1-\psi^X) & \sigma(1-\psi^X) + (1-\sigma)\psi^X & \sigma\psi^X \\ 0 & 0 & 1 \end{bmatrix}$$

and reads as follows: The probability of transitioning from state e to state f is given by the entry in row e and column f. It is easy to see that the Markov chain specified by this transition matrix (in canonical form) is an absorbing chain. Then, the expected number of steps until state JPM is reached when starting with both firms playing BR is

$$\tilde{t}_{SMC} = \frac{2\sigma^2 - 2\sigma\psi^X - \sigma + \psi^X - 1}{\sigma^2(\sigma - \psi^X - 1)},$$

which is strictly monotonically decreasing in σ and ψ^X.

Under multimarket contact (MMC) between conglomerate firms, both firms may send a price signal on none, one or both markets and may also react to any price signal on none, one or both markets. As a consequence, both markets M_1 and M_2 have to be considered separately to indicate industry states, i.e., a tuple of market states, in case the states of both markets are not symmetric. The state space has a cardinality of $|S_{MMC}| = 10$ states and is given by

$$\begin{aligned} S_{MMC} = \{ &(X^{BR}, -X^{BR}), (M_1^{BR}, M_2^{s_i}), (M_1^{s_i}, M_2^{BR}), (X^{s_i}, -X^{s_{-i}}), (X^{s_i}, -X^{s_i}), \\ &(M_1^{BR}, M_2^{JPM}), (M_1^{JPM}, M_2^{BR}), (M_1^{s_i}, M_2^{JPM}), (M_1^{JPM}, M_2^{s_i}), (X^{JPM}, -X^{JPM}) \}. \end{aligned}$$

Thereby, when the order of markets does not matter, they are referred to generically as X and $-X$. This applies to all industry states in which the states of both markets are identical or in which market states are interchangeable in such a way that does not change transition probabilities to any other industry state. The transition matrix between states is

$$H_{MMC} = \begin{bmatrix} h_{1,1} & h_{1,2} & \cdots & h_{1,10} \\ h_{2,1} & h_{2,2} & \cdots & h_{2,10} \\ \vdots & \vdots & \ddots & \vdots \\ h_{10,1} & h_{10,2} & \cdots & h_{10,10} \end{bmatrix}$$

with

$$\begin{aligned}
h_{1,1} &= (1-\sigma)^4, \\
h_{1,2} = h_{1,3} &= 2\sigma(1-\sigma)^3, \\
h_{1,4} = h_{1,5} &= 2\sigma^2(1-\sigma)^2, \\
h_{1,6} = h_{1,7} &= \sigma^2(1-\sigma)^2, \\
h_{1,8} = h_{1,9} &= 2\sigma^3(1-\sigma), \\
h_{1,10} &= \sigma^4, \\
h_{2,1} &= (1-\sigma)^2(1-\psi^X)(1-\psi^{-X}), \\
h_{2,2} &= \sigma(1-\sigma)(1-\psi^X)(1-\psi^{-X}) + (1-\sigma)^2\psi^X(1-\psi^{-X}), \\
h_{2,3} &= \sigma(1-\sigma)(1-\psi^X)(1-\psi^{-X}) + (1-\sigma)^2(1-\psi^X)\psi^{-X}, \\
h_{2,4} &= \sigma(1-\sigma)\psi^X(1-\psi^{-X}) + \sigma(1-\sigma)(1-\psi^X)\psi^{-X},
\end{aligned}$$

$$
\begin{aligned}
h_{2,5} &= \sigma^2(1-\psi^X)(1-\psi^{-X}) + (1-\sigma)^2\psi^X\psi^{-X}, \\
h_{2,6} &= \sigma(1-\sigma)\psi^X(1-\psi^{-X}), \\
h_{2,7} &= \sigma(1-\sigma)(1-\psi^X)\psi^{-X}, \\
h_{2,8} &= \sigma^2\psi^X(1-\psi^{-X}) + \sigma(1-\sigma)\psi^X\psi^{-X}, \\
h_{2,9} &= \sigma^2(1-\psi^X)\psi^{-X} + \sigma(1-\sigma)\psi^X\psi^{-X}, \\
h_{2,10} &= \sigma^2\psi^X\psi^{-X}, \\
h_{3,1} &= (1-\sigma)^2(1-\psi^X)(1-\psi^{-X}), \\
h_{3,2} &= \sigma(1-\sigma)(1-\psi^X)(1-\psi^{-X}) + (1-\sigma)^2(1-\psi^X)\psi^{-X}, \\
h_{3,3} &= \sigma(1-\sigma)(1-\psi^X)(1-\psi^{-X}) + (1-\sigma)^2\psi^X(1-\psi^{-X}), \\
h_{3,4} &= \sigma(1-\sigma)\psi^X(1-\psi^{-X}) + \sigma(1-\sigma)(1-\psi^X)\psi^{-X}, \\
h_{3,5} &= \sigma^2(1-\psi^X)(1-\psi^{-X}) + (1-\sigma)^2\psi^X\psi^{-X}, \\
h_{3,6} &= \sigma(1-\sigma)(1-\psi^X)\psi^{-X}, \\
h_{3,7} &= \sigma(1-\sigma)\psi^X(1-\psi^{-X}), \\
h_{3,8} &= \sigma^2(1-\psi^X)\psi^{-X} + \sigma(1-\sigma)\psi^X\psi^{-X}, \\
h_{3,9} &= \sigma^2\psi^X(1-\psi^{-X}) + \sigma(1-\sigma)\psi^X\psi^{-X}, \\
h_{3,10} &= \sigma^2\psi^X\psi^{-X}, \\
h_{4,1} &= (1-\psi^X)^2(1-\psi^{-X})^2, \\
h_{4,2} = h_{4,3} &= \psi^X(1-\psi^X)(1-\psi^{-X})^2 + (1-\psi^X)^2\psi^{-X}(1-\psi^{-X}), \\
h_{4,4} &= \psi^{X^2}(1-\psi^{-X})^2 + (1-\psi^X)^2\psi^{-X^2}, \\
h_{4,5} &= 2\psi^X\psi^{-X}(1-\psi^X)(1-\psi^{-X}), \\
h_{4,6} = h_{4,7} &= \psi^X\psi^{-X}(1-\psi^X)(1-\psi^{-X}), \\
h_{4,8} = h_{4,9} &= \psi^X\psi^{-X}\psi^X(1-\psi^{-X}) + \psi^X\psi^{-X}(1-\psi^X)\psi^{-X}, \\
h_{4,10} &= \psi^{X^2}\psi^{-X^2}, \\
h_{5,1} &= (1-\sigma)^2(1-\psi^X)^2, \\
h_{5,2} = h_{5,3} &= \sigma(1-\sigma)(1-\psi^X)^2 + (1-\sigma)^2\psi^X(1-\psi^X), \\
h_{5,4} &= 2\sigma(1-\sigma)\psi^X(1-\psi^X), \\
h_{5,5} &= \sigma^2(1-\psi^X)^2 + (1-\sigma)^2\psi^{X^2},
\end{aligned}
$$

$$h_{5,6} = h_{5,7} = \sigma(1-\sigma)\psi^X(1-\psi^X),$$

$$h_{5,8} = h_{5,9} = \sigma^2\psi^X(1-\psi^X) + \sigma(1-\sigma)\psi^{X^2},$$

$$h_{5,10} = \sigma^2\psi^{X^2},$$

$$h_{6,1} = h_{6,2} = h_{6,3} = h_{6,4} = h_{6,5} = 0,$$

$$h_{6,6} = (1-\psi^{-X})^2,$$

$$h_{6,7} = 0,$$

$$h_{6,8} = 2\psi^{-X}(1-\psi^{-X}),$$

$$h_{6,9} = 0,$$

$$h_{6,10} = \psi^{-X^2},$$

$$h_{7,1} = h_{7,2} = h_{7,3} = h_{7,4} = h_{7,5} = h_{7,6} = 0,$$

$$h_{7,7} = (1-\psi^{-X})^2,$$

$$h_{7,8} = 0,$$

$$h_{7,9} = 2\psi^{-X}(1-\psi^{-X}),$$

$$h_{7,10} = \psi^{-X^2},$$

$$h_{8,1} = h_{8,2} = h_{8,3} = h_{8,4} = h_{8,5} = 0,$$

$$h_{8,6} = (1-\psi^X)(1-\psi^{-X}),$$

$$h_{8,7} = 0,$$

$$h_{8,8} = \psi^X(1-\psi^{-X}) + (1-\psi^X)\psi^{-X},$$

$$h_{8,9} = 0,$$

$$h_{8,10} = \psi^X\psi^{-X},$$

$$h_{9,1} = h_{9,2} = h_{9,3} = h_{9,4} = h_{9,5} = h_{9,6} = 0,$$

$$h_{9,7} = (1-\psi^X)(1-\psi^{-X}),$$

$$h_{9,8} = 0,$$

$$h_{9,9} = \psi^X(1-\psi^{-X}) + (1-\psi^X)\psi^{-X},$$

$$h_{9,10} = \psi^X\psi^{-X},$$

$$h_{10,1} = h_{10,2} = h_{10,3} = h_{10,4} = h_{10,5} = h_{10,6} = h_{10,7} = h_{10,8} = h_{10,9} = 0,$$

$$h_{10,10} = 1.$$

Exploiting the properties of the absorbing Markov chain starting with both firms playing BR on both markets yields the expected number of steps until JPM is reached on both markets $\tilde{t}_{MMC}$, which is monotonically decreasing in σ, ψ^X, and ψ^{-X}. As the term is very lengthy and thus uninformative, it is avoided to print it here.

Provided with the expected numbers of periods until the collusive state is reached under single market contact as well as under multimarket contact, a critical $\widehat{\psi}^{-X}(\sigma, \psi^X)$ can be computed above which $\tilde{t}_{MMC} < \tilde{t}_{SMC}$. As $\tilde{t}_{MMC}$ is monotonically decreasing in ψ^{-X} the critical value is unique. Again, the analytical result is very lengthy and can thus not be interpreted intuitively. Therefore, w.l.o.g. Figure 6.1 provides a graphical representation of $\widehat{\psi}^{-X}$ for all feasible values of σ, ψ^X, and ψ^{-X}. The collusive state of JPM is reached faster under multimarket contact than under single market contact for all parameter combinations above the surface. In particular, the critical probability of cross-market signal reacting is increasing in the probability of sending a price signal, but is far less sensitive to changes in the probability of reacting to a price signal on the same market. Furthermore, single market contact converges faster to JPM than multimarket contact for all feasible values of ψ^{-X} if $\sigma \in \left(\underline{\sigma}(\psi^X), \overline{\sigma}(\psi^X)\right)$. In other words, as Figure 6.1 indicates, there is an σ-frontier above which $\widehat{\psi}^{-X} > 1$ except for cases of very high values of σ together with very low values of ψ^X. □

B.3 Upstream and downstream competition

Retail demand for firm $i \in \{A, B, D\}$ in case of $n = 3$ active firms according to Shubik and Levitan (1980) is given by

$$q_i^{Triopoly} = \frac{1}{3} \cdot (1 - p_i - \gamma \cdot (p_i - \frac{p_A + p_B + p_D}{3}).$$

In the case of *foreclosure*, i.e., if both integrated firms' wholesale prices exceed their respective retail prices such that Firm D is unable to set a retail price that would yield a positive profit, Firm D does not supply any retail consumers and per firm retail demand for the remaining two firms is given by

$$q_i^{Duopoly} = \frac{1+\gamma}{3} \cdot (1 - p_i - \frac{\gamma}{(3+2\gamma)} \cdot (2 - (p_A + p_B)).$$

See Höffler (2007) for derivations of the demand functions. In the following, let $\gamma = 30$. Assume w.l.o.g. that Firm A provides wholesale access to the reseller Firm D. This yields profits

$$\pi_A = p_A \cdot q_A(p_A, p_B, p_D) + a \cdot q_D(p_A, p_B, p_D),$$
$$\pi_B = p_B \cdot q_B(p_A, p_B, p_D),$$
$$\pi_D = (p_D - a) \cdot q_D(p_A, p_B, p_D).$$

In the following, Nash predictions are calculated for the industry scenarios (i) wholesale monopoly under no regulation, (ii) wholesale monopoly under margin squeeze regulation, (iii) wholesale competition under no regulation, and (iv) wholesale competition under margin squeeze regulation, each for all four timing models as described in Subsection 7.3.1. For the sequential-move Timing Models (1), (2), and (4) subgame-perfect Nash equilibria are determined through backward induction. In order to facilitate the comparison of theoretical predictions and experimental results, final prices and profits are scaled as in the experiment. Note that scaling affects only the output, but calculations are conducted based on the original Shubik and Levitan (1980) values. Price (profit) values are multiplied by the factor $\phi = 100/0.15$ ($\Phi = 400$) to obtain scaled values.

Timing Model (1): Under a wholesale monopoly, in stage II, two (integrated) or three firms may operate in the retail market depending on the wholesale price chosen in stage I. In stage II, in case of foreclosure, i.e., $q_D = 0$, integrated firms' profit-maximizing prices are given by $p_A^{Duopoly} = p_B^{Duopoly} = 55.5\overline{5}$ and profits amount to $\pi_A^{Duopoly} = \pi_B^{Duopoly} = 15.04$. Instead, in case there is a viable wholesale offer, firms choose retail prices $p_A^{Triopoly} = \phi(\frac{1}{22} + \frac{265}{572}a)$, $p_B^{Triopoly} = \phi(\frac{1}{22} + \frac{155}{572}a)$, and $p_D^{Triopoly} =$

$\phi(\frac{1}{22} + \frac{193}{286}a)$. In stage I, anticipating retail prices, Firm A chooses the monopolistic wholesale price $a^{Triopoly} = 66.36$. Ensuing retail prices are given by $p_A^{Triopoly} = 61.05$, $p_B^{Triopoly} = 48.29$, and $p_D^{Triopoly} = 75.08$ and profits by $\pi_A^{Triopoly} = 14.97$, $\pi_B^{Triopoly} = 14.69$, and $\pi_D^{Triopoly} = 0.48$. Comparing $\pi_A^{Duopoly}$ to $\pi_A^{Triopoly}$, Firm A prefers the foreclosure outcome and thus sets a wholesale price $a^{Duopoly} \in (84.66, 100]$, which forces the reseller to exit the retail market.

Taking into account margin squeeze regulation, foreclosure is ruled out as a valid market outcome and therefore does not constitute an equilibrium. However, as $a_A^{Triopoly} > p_A^{Triopoly}$, Firm A would violate the margin squeeze condition. Instead, Firm A is required to maximize its profit π_A subject to the condition $a_A \leq p_A$ in stage II, while the other firms maximize profits unconstrained. This yields $p_A^{MSR} = a$, $p_B^{MSR} = \phi(\frac{1}{32} + \frac{365}{832}a)$, and $p_D^{MSR} = \phi(\frac{1}{32} + \frac{701}{832}a)$. In stage I, Firm A sets the monopoly wholesale price to $a_A^{MSR} = 66.16$ and corresponding retail prices are given by $p_A^{MSR} = 66.16$, $p_B^{MSR} = 49.86$, and $p_D^{MSR} = 76.57$ with profits $\pi_A^{MSR} = 15.09$, $\pi_B^{MSR} = 15.66$, and $p_D^{MSR} = 0.68$.

Considering unregulated wholesale competition, two equilibria emerge, namely a competitive and a foreclosure type. Atiyas et al. (2015) show that for $\gamma > 26.77$ (and observable wholesale contracts) the foreclosure outcome constitutes an additional Nash equilibrium next to the competitive outcome. As shown for the case of a wholesale monopoly, an integrated firm does not find it profitable to deviate from the state of coordinated foreclosure in the wholesale market, because wholesale profits are outweighed by the reseller's business stealing effect in the retail market, even at the monopoly price. Moreover, given the nonviable wholesale offers, no firm i has an incentive to deviate from its foreclosure price $p_i^{Duopoly}$. In contrast, as shown by Bourreau et al. (2012), if a firm is required to make a viable wholesale offer, integrated firms always find it profitable to undercut their rival in the wholesale market for $\gamma < 40.97$. Once wholesale prices are driven to zero, i.e., $a_A^{Competitive} = a_B^{Competitive} = 0$, firms cannot unilaterally increase the wholesale price profitably. Thus the competitive outcome $a_A^{Competitive} = a_B^{Competitive} = 0$ with ensuing retail prices $p_i^{Competitive} = 30.30$ and profits $\pi_i^{Competitive} = 5.79$, $\forall i \in \{A, B, D\}$, constitutes a Nash equilibrium.

TABLE B.1: *Theoretical predictions for Timing Model (1).*

	No regulation		Margin squeeze regulation	
Wholesale monopoly	$a_A =$	100.00	$a_A =$	66.19
	$p_A =$	55.56	$p_A =$	66.19
	$p_B =$	55.56	$p_B =$	49.86
	$p_D =$	100.00	$p_D =$	76.57
Wholesale competition	$a_A = a_B =$	100.00	$a_A = a_B =$	0.00
	$p_A = p_B =$	55.56	$p_A = p_B = p_D =$	30.30
	$p_D =$	100.00		
	or:			
	$a_A = a_B =$	0.00		
	$p_A = p_B = p_D =$	30.30		

Whereas in the case of no regulation two types of equilibria coexist, the competitive equilibrium is unique in the case of wholesale competition under margin squeeze regulation. Integrated firms are now unable to foreclose the reseller, due to the margin squeeze condition, while the Bertrand logic applies as described above for the unregulated case. These results are summarized in Table B.1.

Timing Model (2): In stage III, the reseller's optimal price as a follower is given by its best response function, i.e., $p_D = BR(p_A, p_B) = \phi(\frac{1}{42} + \frac{5}{21}p_A + \frac{5}{21}p_B + \frac{1}{2}a)$ across all scenarios. Anticipating the reseller's reaction, integrated firms' simultaneous (unconstrained) profit-maximization in stage II yields $p_A = \phi(\frac{13}{261} + \frac{5320}{15109}a)$ and $p_B = \phi(\frac{13}{261} + \frac{7595}{30218}a)$.

In the monopoly case, Firm A maximizes its profit by setting its wholesale price to $a_A = 67.39$ in stage I. Ensuing retail prices are given by $p_A^{Triopoly} = 56.94$, $p_B^{Triopoly} = 50.14$, and $p_D^{Triopoly} = 75.07$ and firms make profits $\pi_A^{Triopoly} = 15.61$, $\pi_B^{Triopoly} = 14.05$, and $\pi_D^{Triopoly} = 0.37$. Note that under this timing model $\pi_A^{Triopoly}$ exceeds the foreclosure profit $\pi_A^{Duopoly}$ as Firm A internalizes the reseller's reaction to its own prices. Therefore, Firm A finds it profitable to make a viable wholesale offer to Firm D.

Although foreclosure does not constitute an equilibrium under no regulation, Nash prices of Firm A still violate the margin squeeze condition. Taking into account this condition, constrained maximization of Firm A's profit in stage II yields prices

TABLE B.2: *Theoretical predictions for Timing Model (2).*

	No regulation		Margin squeeze regulation	
Wholesale monopoly	$a_A =$	67.39	$a_A =$	70.14
	$p_A =$	56.94	$p_A =$	70.14
	$p_B =$	50.15	$p_B =$	54.90
	$p_D =$	75.07	$p_D =$	80.72
Wholesale competition	$a_A = a_B =$	0.00	$a_A = a_B =$	0.00
	$p_A = p_B = p_D =$	30.30	$p_A = p_B = p_D =$	30.30

$p_A^{MSR} = a_A$ and $p_B^{MSR} = \phi(\frac{13}{391} + \frac{365}{782}a)$. The optimal wholesale price in stage I is given by $a^{MSR} = 70.14$ and respective retail prices are $p_A^{MSR} = 70.14$, $p_B^{MSR} = 54.90$, $p_D^{MSR} = 80.72$. Firms' profits amount to $\pi_A^{MSR} = 16.24$, $\pi_B^{MSR} = 16.84$, and $\pi_D^{MSR} = 0.70$. Note that margin squeeze regulation leads to unambiguously higher prices and increased profits compared to the no regulation outcome, given this timing model.

In the case of wholesale competition, the Bertrand logic, as laid out in Timing Model (1), applies equally with regard to the integrated firms' behavior in stage I. Moreover, the competitive outcome is unique, because one of the integrated firms will always find it profitable to unilaterally deviate from coordinated foreclosure and supply the reseller.

Given the theoretical prediction, margin squeeze regulation does not affect the market outcome under wholesale competition in Timing Model (2), because equilibrium prices in the unregulated outcome do not violate the margin squeeze constraint. See Table B.2 for a summary of results.

Timing Model (3): In the case of an unregulated monopolistic wholesale provider, simultaneous setting of all prices (wholesale and retail) leads to foreclosure as the unique equilibrium. Consider, in contrast, a situation in which Firm D makes positive profit, i.e., $a_A < p_D$. Obviously, Firm A can then increase its profit by setting $a_A = p_D$. However, Firm D would in turn increase its retail price p_D as long as it is able to obtain a positive demand ($q_D > 0$). Consequently, this *reverse* Bertrand logic gives rise to foreclosure as the unique equilibrium.

TABLE B.3: *Theoretical predictions for Timing Model (3).*

	No regulation		Margin squeeze regulation	
Wholesale monopoly	$a_A =$	100.00	$a_A =$	67.61
	$p_A =$	55.56	$p_A =$	67.61
	$p_B =$	55.56	$p_B =$	50.49
	$p_D =$	100.00	$p_D =$	77.80
Wholesale competition	$a_A = a_B =$	100.00	$a_A = a_B =$	0.00
	$p_A = p_B =$	55.56	$p_A = p_B = p_D =$	30.30
	$p_D =$	100.00		
	or:			
	$a_A = a_B =$	0.00		
	$p_A = p_B = p_D =$	30.30		

In the case of margin squeeze regulation, Firm A solves $\frac{\partial \pi_A}{\partial p_A} = 0$ and $\frac{\partial \pi_A}{\partial a_A} = 0$ simultaneously subject to the constraint $a_A \leq p_A$, while Firm B and Firm D solve first order conditions $\frac{\partial \pi_B}{\partial p_B} = 0$ and $\frac{\partial \pi_D}{\partial p_D} = 0$. Optimal prices are given by $a_A^{MSR} = p_A^{MSR} = 67.61$, $p_B^{MSR} = 50.49$, and $p_D^{MSR} = 77.80$ leading to profits $\pi_A^{MSR} = 15.08$, $\pi_B^{MSR} = 16.06$, and $\pi_D^{MSR} = 0.65$.

If both integrated firms are active in the wholesale market, the same rationale as under Timing Model (1) applies, i.e., the competitive as well as the foreclosure outcome constitute an equilibrium. On the one hand, if both firms choose a wholesale price that forecloses Firm D, there is no unilateral deviation that increases an integrated firm's profit, because the business stealing effect outweighs the wholesale revenue effect. On the other hand, in the case of the competitive outcome, an integrated firm is unable to establish the foreclosure outcome unilaterally.

If wholesale competition is combined with margin squeeze regulation, the foreclosure equilibrium disappears—as under Timing Model (1)—and the competitive outcome constitutes the unique equilibrium. These results are summarized in Table B.3.

Timing Model (4): Like in Timing Model (2), the reseller as a follower maximizes its profit given the previously set wholesale price(s) and retail prices of the integrated firms, i.e., according to its best response function it chooses $p_D = BR(p_A, p_B) = \phi(\frac{1}{42} + \frac{5}{21}p_A + \frac{5}{21}p_B + \frac{1}{2}a)$. In stage I, integrated firms maximize profits simultaneously taking

TABLE B.4: *Theoretical predictions for Timing Model (4).*

	No regulation		Margin squeeze regulation	
Wholesale monopoly	$a_A =$	51.25	$a_A =$	51.25
	$p_A =$	51.25	$p_A =$	51.25
	$p_B =$	46.09	$p_B =$	46.09
	$p_D =$	64.68	$p_D =$	64.68
Wholesale competition	$a_A = a_B =$	0.00	$a_A = a_B =$	0.00
	$p_A = p_B = p_D =$	30.30	$p_A = p_B = p_D =$	30.30

into account the reaction by Firm D in stage II. Optimal prices are given by $a_A^{Triopoly} = p_A^{Triopoly} = 51.25$, $p_B^{Triopoly} = 46.09$, and consequently $p_D^{Triopoly} = 64.67$. Accordingly, firms obtain profits $\pi_A^{Triopoly} = 15.07$, $\pi_B^{Triopoly} = 11.86$, and $\pi_D^{Triopoly} = 1.14$. Again, considering the reseller as a follower allows the integrated firms to internalize Firm D's reaction to their own prices which makes foreclosure relatively less profitable.

The margin squeeze condition is non-binding, because Firm A's equilibrium prices in the case of an unregulated wholesale monopoly do not constitute a margin squeeze. Therefore, the theoretical prediction is the same as under an unregulated wholesale monopoly.

Having ruled out foreclosure as an equilibrium in the case of a wholesale monopoly, the same rationale holds under unregulated wholesale competition, because an integrated firm has always an incentive to deviate in the upstream market and charge the monopolistic wholesale price. Thus, as argued above, the competitive outcome remains as the unique equilibrium.

In consequence, under wholesale competition the margin squeeze regulation does not affect the theoretical prediction and the outcome is identical to the unregulated wholesale competition scenario, i.e., $a_A^{Competitive} = a_B^{Competitive} = 0$, $p_i^{Competitive} = 30.30$, and $\pi_i^{Competitive} = 5.79$, $\forall i \in \{A, B, D\}$. See Table B.4 for a summary of results.

Appendix C

Experimental Instructions

For each experiment, the instructions of only one treatment are reported in the following. The instructions for the other treatments are identical except with respect to the specifics of the treatments. Note that the experimental instructions are translated from German and are only translations for information; they are not intended to be used in the lab. The instructions in the original language are carefully polished in grammar, style, comprehensibility, and avoidance of strategic guidance.

C.1 Sequences of decision-making

The following experimental instructions are for the RB3 treatment.

Preliminary remarks

Welcome to the experiment and thank you very much for your participation. In this experiment you can earn an amount of money that depends on your decisions and the decisions of the other participants. Please address the person in charge of the experiment in case of questions. Please do not talk to the other participants during the entire experiment. Throughout the experiment we will use the currency euro and its subunit cent. The euro that you will have earned by the end of the experiment will be paid to you in cash.

Experimental structure

There are three firms competing with each other:

- Firm A
- Firm B
- Firm C

Each firm is represented by one participant of the experiment. Throughout the experiment the same firms compete with each other. Which firm you represent is randomly chosen at the beginning of the experiment. Each firm offers a good that is demanded by consumers. There are no cost for producing these goods. You choose the price at which you want to sell your good. The quantity demanded of your good is determined by your price. Thereby, the following holds:

- The higher your price, the lower the quantity demanded of your good. Thereby, the quantity demanded of your good can fall to zero.
- The higher a price of the other firms, the higher the quantity demanded of your good. Thereby, only prices of firms selling a positive quantity are relevant.

Your profit is calculated by multiplying your price with the quantity demanded of your good.

Experimental procedure

The experiment is composed of two stages. At stage one you choose your initial price. Before making the final decision, you can test how a price combination affects the quantities and profits of all firms. After all firms made their final decision by pressing the button "Finalize decision", the second stage of the experiment begins. The second stage lasts exactly 30 minutes. During this time all decisions are made in real-time and without any interruptions. Your price decision is valid until you change your price. Every decision of a firm is immediately visible for all other firms.

Software display

FIGURE C.1: *Display of the experimental software.*

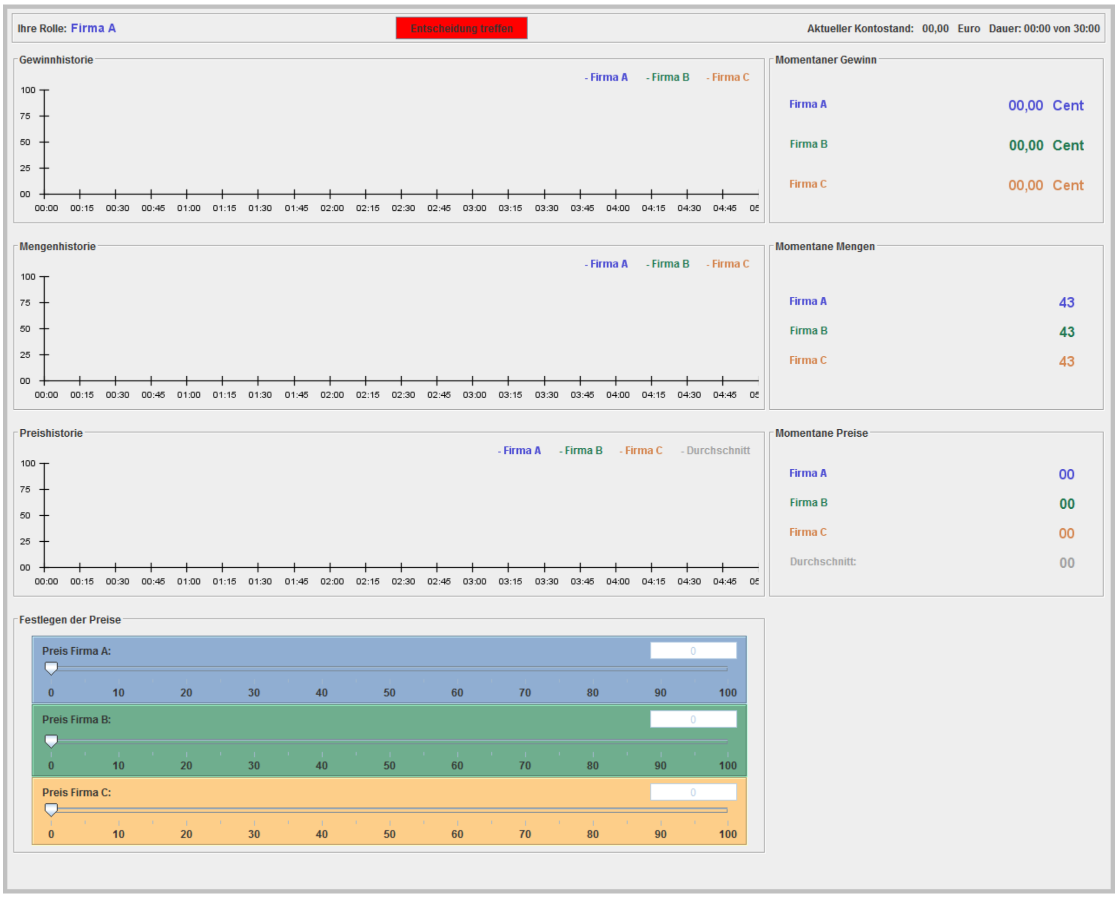


Figure C.1 depicts the display of the experimental software. To distinguish the firms, their information is colored as follows:

- Firm A: BLUE
- Firm B: GREEN
- Firm C: ORANGE

In the following, the individual parts of the display will be explained bottom up.

Decision and testing environment

On the left side you can set your price by using the slider of the firm. Please be aware that you can use all sliders during the first stage of the experiment and only the slider of your own firm during the second stage. During the second stage the sliders show the current prices of the other firms.

Prices

On the left side the history of all firms' prices as well as the average price is visualized. On the right side the current prices are displayed.

Quantities

On the left side the history of all firms' quantities is visualized. On the right side the current quantities are displayed.

Profits

On the left side the history of all firms' profits is visualized. On the right side the current profits are displayed. Please be aware that current profits are scaled to the profit you would earn if the current combination of all prices would be held for 30 seconds. As soon as one firm changes its price, the profits are recalculated. Your current profit is added to your account proportionally several times per second.

Status of the experiment

On the left side it is displayed which firm you represent. Figure C.1 shows this for firm A as an example. During the first stage there is a button "Finalize decision" in the middle of the display. Please press it when you are ready to finalize your decision. On the right side your current account balance and the remaining duration of the experiment is displayed. Your current account balance is the sum of all realized profits.

Concluding remarks

Before the experiment starts, you will be asked some comprehension questions on the screen with regard to the understanding of the rules and the course of the experiment. Please enter the respective answers into your computer. Afterwards, the experiment will start automatically and it will be displayed which firm you represent.

In case of any questions, please remain seated and give the person in charge of the experiment a hand signal. Please wait until the person in charge of the experiment has arrived at your seat. Talk as quietly as possible when asking your question. Please remain seated after the end of the experiment as well and wait for further instructions from the person in charge of the experiment.

C.2 Number of competitors

The following experimental instructions are for the B4A treatment.

Preliminary remarks

Welcome to the experiment and thank you very much for your participation. In this experiment you can earn an amount of money that depends on your decisions and the decisions of the other participants. Please address the person in charge of the experiment in case of questions. Please do not talk to the other participants during the entire experiment. Throughout the experiment we will use the currency euro and its subunit cent. The euro that you will have earned by the end of the experiment will be paid to you in cash.

Experimental structure

There are four firms competing with each other:

- Firm A
- Firm B
- Firm C
- Firm D

Each firm is represented by one participant of the experiment. Throughout the experiment the same firms compete with each other. Which firm you represent is randomly chosen at the beginning of the experiment. Each firm offers a good that is demanded by consumers. There are no cost for producing these goods. You choose the price at which you want to sell your good. The quantity demanded of your good is determined by your price. Thereby, the following holds:

- The higher your price, the lower the quantity demanded of your good. Thereby, the quantity demanded of your good can fall to zero.
- The higher a price of the other firms, the higher the quantity demanded of your good. Thereby, only prices of firms selling a positive quantity are relevant.

Please be aware that the quantity demanded of Firm A differs from the quantity demanded of any other firm, everything else being equal. If all firms choose the same price, the quantity demanded of Firm A is greater than the quantity demanded of any other single firm. Additionally, the following holds:

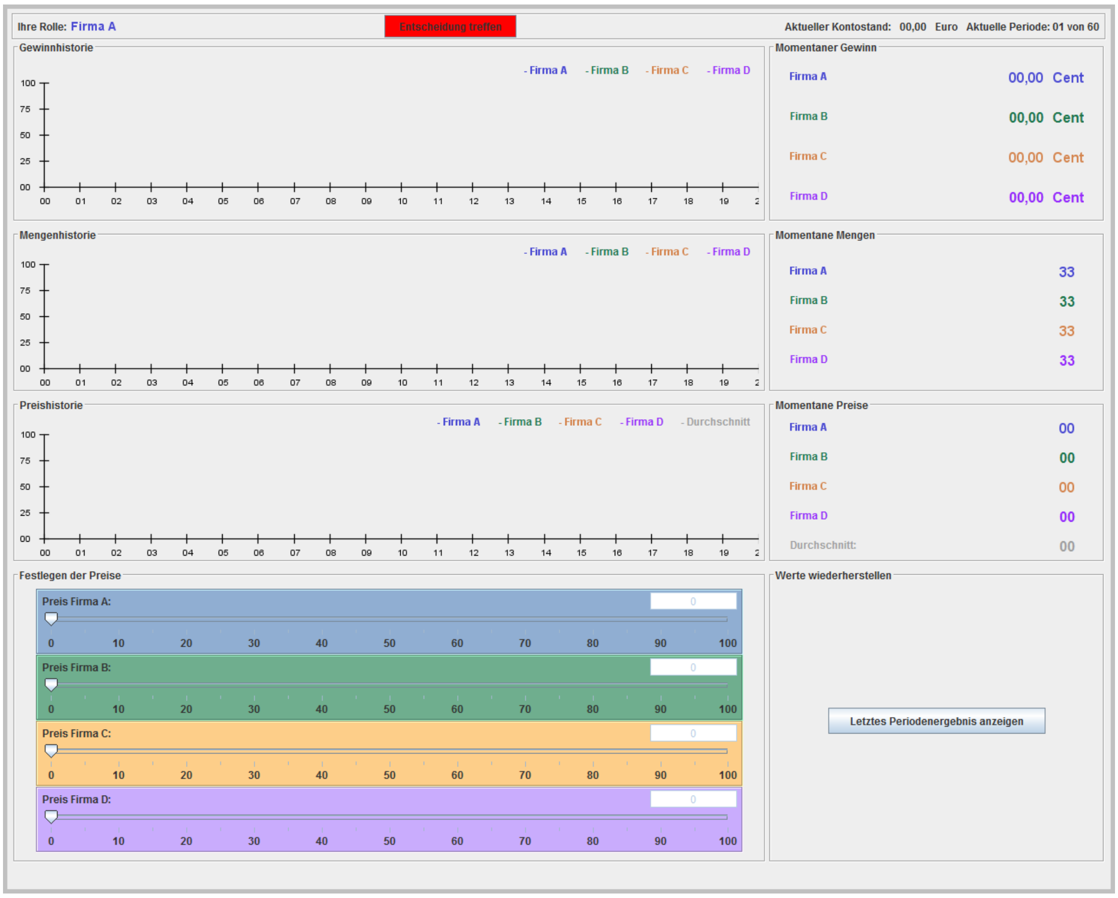
- If Firm A raises its price, the quantity demanded of Firm A decreases less than the quantity of any other firm if it raises its price.
- If another firm raises its price, the quantity demanded of Firm A increases more than the quantity of any other firm.

Your profit is calculated by multiplying your price with the quantity demanded of your good.

Experimental procedure

The experiment lasts 60 periods. In each period you chose your price. Before making the final decision, you can test how a price combination affects the quantities and profits of all firms. After all firms made their final decision by pressing the button "Finalize decision", quantities and profits are calculated and the next period begins.

FIGURE C.2: *Display of the experimental software.*



Software display

Figure C.2 depicts the display of the experimental software. To distinguish the firms, their information is colored as follows:

- Firm A: BLUE
- Firm B: GREEN
- Firm C: ORANGE
- Firm D: PURPLE

In the following, the individual parts of the display will be explained bottom up.

Decision and testing environment

On the left side you can set your price by using the slider of your firm. At the beginning of a period the sliders show the prices of the firms of the previous period. Before making the final decision, you can test the consequences of your price decision by adjusting the sliders of the other firms to your expectations. As soon as you release a slider, the quantities and profits that would result in the next period if the currently set prices in the testing environment get chosen are displayed on the right side of the screen above the sliders. On the right side you can reset the sliders to the prices of the last period by pressing the button “Show last period results”.

Prices

On the left side the history of all firms’ prices as well as the average price is visualized. On the right side the currently set prices in the testing environment are displayed.

Quantities

On the left side the history of all firms' quantities is visualized. On the right side the quantities that would result in the next period if the currently set prices in the testing environment get chosen are displayed.

Profits

On the left side the history of all firms' profits is visualized. On the right side the profits that would result in the next period if the currently set prices in the testing environment get chosen are displayed.

Status of the experiment

On the left side it is displayed which firm you represent. Figure C.2 shows this for firm A as an example. In the middle of the display is the button "Finalize decision". Please press it when you are ready to finalize your decision. On the right side your current account balance and the current period of the experiment is displayed. Your current account balance is the sum of all realised profits.

Concluding remarks

Before the experiment starts, you will be asked some comprehension questions on the screen with regard to the understanding of the rules and the course of the experiment. Please enter the respective answers into your computer. Afterwards, the experiment will start automatically and it will be displayed which firm you represent.

In case of any questions, please remain seated and give the person in charge of the experiment a hand signal. Please wait until the person in charge of the experiment has arrived at your seat. Talk as quietly as possible when asking your question. Please remain seated after the end of the experiment as well and wait for further instructions from the person in charge of the experiment.

C.3 Price discrimination

The following experimental instructions are for the PD treatment.

Preliminary remarks

Welcome to the experiment and thank you very much for your participation. If you read through these instructions carefully and consider them during the experiment, you can earn an amount of money that depends on your decisions and the decisions of the other participants. Please address the person in charge of the experiment in case of questions. Please do not talk to the other participants during the entire experiment.

Throughout the experiment we will use the currency euro and its subunit cent. The euro that you will have earned by the end of the experiment will be paid to you in cash.

The experiment is divided into several rounds. The decisions and the results of each round are not interdependent.

In each round, you will simulate the decision of a firm that sells a good to consumers. There is exactly one other firm apart from you (which will be called "the other firm" in the following). You will be competing against this other firm.

Setup of a round

In each round, you and the other firm offer a good in two markets. In each market there are 10 consumers.

The provision of the good will cost you 30 cent per consumer in market A and in market B. You and the other firm produce equal goods. Therefore, the consumers will buy the good from the firm that offers the good at a lower price. The consumers are willing to pay a maximum of 50 cent for the good.

One round consists of 10 retail periods. In each retail period you offer your good to all consumers. Thereby, you choose an individual price for market A and market B.

Demand

The demands in market A and market B will be determined separately and depend on your prices and the prices of the other firm. All consumers will always demand the good that is offered at the lower price. However, they are not willing to pay more than 50 cent per retail period.

Thus, if you offer your good in a market at a higher price than the other firm, there will be no demand for your good in this market. The same applies if one of the two firms offers the good at a price higher than 50 cent. If both firms offer the same price in a market, each firm will receive half of the demand in this market. If both prices of the same market are above 50 cent, there will be no demand for both firms in this market.

Profit

Your profit in a retail period depends on your prices and your demand. Therefore, the profit in one market is calculated as follows:

$$Profit = Demand \cdot (Price - Costs\ of\ Provision)$$

Your profit in a retail period is calculated by summing up your profit in market A and your profit in market B. The profits in both markets are being accumulated over all 10 retail periods and make your overall profit.

Example Firms 1 and 2 offer the following prices:

	Market A	Market B
Firm 1	35 cent	50 cent
Firm 2	40 cent	45 cent

The profits of firm 1 in both markets are calculated as follows:

- Profit in market A: $10 \cdot (35\text{ cent} - 30\text{ cent}) = \text{EUR } 0.50$
- Profit in market B: $0 \cdot (50\text{ cent} - 30\text{ cent}) = \text{EUR } 0.00$
- Profit of retail period: EUR 0.50 + EUR 0.00 = EUR 0.50

The profits of firm 2 in both markets are calculated as follows:

- Profit in market A: $0 \cdot (40\text{ cent} - 30\text{ cent}) = \text{EUR } 0.00$
- Profit in market B: $10 \cdot (45\text{ cent} - 30\text{ cent}) = \text{EUR } 1.50$
- Profit of retail period: EUR 0.00 + EUR 1.50 = EUR 1.50

Software display

FIGURE C.3: *Display of the experimental software.*

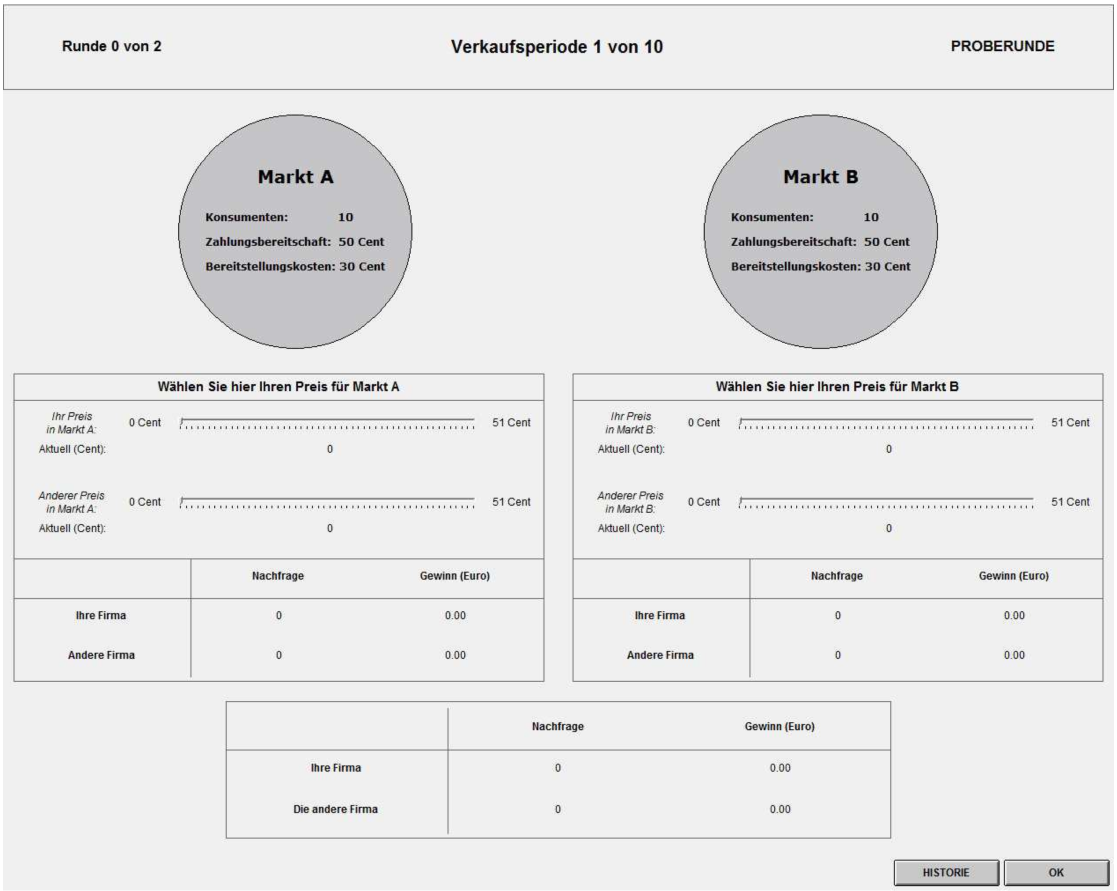


Figure C.3 shows the display of the experimental software during a retail period. The individual parts will be explained in detail below.

Markets The upper part of the display represents the two markets A and B exemplarily and repeats the key decision factors.

Decision and testing environment In the lower part of the display you can enter your prices for the two markets. In addition, you can test the consequences of your pricing decision. In order to do so, you have to indicate your prices as well as what you expect the prices of the other firm to be, by using the slider.

As soon as you release the slider, the values in the table beneath are being updated. The table shows the effects of the respective price combination on the demand and on the profit in one retail period.

For each market the demand and the profit of your firm and the other firm are being shown. The table beneath shows the demand and the profit accumulated over both markets for your firm and the other firm.

Please use the slider for your firm to set your prices. Note that the actual prices of the other firm are set by the corresponding firm and not by you. The slider for the other firm only serves as a means of decision support.

Please note: You cannot only use the testing environment in order to test the effects of your own pricing decision, but also to estimate the possible reactions to your current pricing decision of the other firm.

FIGURE C.4: *Retail history.*

	Periode 1	Periode 2	Periode 3	Periode 4	Periode 5	Periode 6	Periode 7	Periode 8	Periode 9	Periode 10
Ihr Preis für Markt A										
Anderer Preis für Markt A										
Ihr Preis für Markt B										
Anderer Preis für Markt B										
Ihr Gewinn aus beiden Märkten										
Anderer Gewinn aus beiden Märkten										

Retail history Clicking the button "History" opens the retail history. The main parameters of the past retail periods are shown in this section:

- Your price for market A
- The price of the other firm for market A

- Your price for market B
- The price of the other firm for market B
- Your profit in both markets
- The profit of the other firm in both markets

Another click on the button "History" closes the retail history again.

Course of the experiment

Overall, 3 rounds (0 to 2) are being played. Round 0 is a test run. Each round consists of 10 retail periods. The firm pairings are being randomly determined all over again in each round. However, it is excluded that you will ever play again with the same firm.

The information regarding the round and the course of the experiment are always being shown at the top level of the screen.

At the end of each round, your accumulated profits of all 10 retail periods that represent your payoff are being shown in euro. You do not have to memorize this value. At the end of the experiment, the payment of exactly one round will be paid oud to you. It will be chosen at random which of the rounds is being cashed out. To this end, you will have to roll a dice. The test run, round 0, is not included.

Concluding remarks

Do not hesitate to ask questions. As long as they refer to these instructions and not to possible strategies, we will answer your questions as far as possible. Please remember: The better you have understood these instructions, the more money you can make.

Before the experiment starts, you will be asked some questions on the screen with regard to the understanding of the rules and the course of the experiment. Please enter the respective answer into your computer. Afterwards, the experiment will start automatically.

In case of any questions, please remain seated and give the person in charge of the experiment a hand signal. Please wait until the person in charge of the experiment has arrived at your seat. Talk as quietly as possible when asking the question. Please remain seated after the end of the experiment as well and wait for further instructions from the person in charge of the experiment. You can make notes on the pad that is laid out for you on the table during the experiment. Please leave the experiment instructions, the calculator as well as the note pad at the table after the experiment.

C.4 Multimarket contact

The following experimental instructions are for the MMC treatment.

Preliminary remarks

Welcome to the experiment and thank you very much for your participation. If you read through these instructions carefully and consider them during the experiment, you can earn an amount of money that depends on your decisions and the decisions of the other participants. Please address the person in charge of the experiment in case of any questions. Please do not talk to the other participants during the entire experiment.

Throughout the experiment we will use the currencies monetary unit (short: MU) and euro (short: EUR) (1 million MU $\hat{=}$ EUR 1). The euro that you will have earned by the end of the experiment will be paid out to you in cash.

During the experiment, you will represent a firm that sells a good to consumers. There is exactly one other firm apart from you (which will be called "the other firm" in the following). You will be competing against this other firm.

The experiment consists of *at least 45 and at most 50 periods*. The exact number of periods will be determined at random and is identical for all participants of the experiment. Any number between 45 and 50 periods is *equally likely*.

During the entire experiment, you play against the same firm. The other firm is also represented by a participant of the experiment.

Setup of a period

In each period, you and the other firm offer a good in two markets. In each market there are 10000 consumers.

The provision of the good will cost you 10 MU per consumer in *market A* and in *market B*. You and the other firm produce equal goods. Therefore, the consumers will buy the good from the firm that offers the good at a lower price. The consumers are willing to pay a maximum of 50 MU for the good.

In each of the 45 to 50 periods, you offer your good to all consumers. Thereby, you choose an individual price for market A and market B.

Demand

The demands in market A and market B will be determined separately and depend on your prices and the prices of the other firm. All consumers will always demand the good that is offered at the lower price. However, they are not willing to pay more than 50 MU per period.

Thus, if you offer your good in a market at a higher price than the other firm, there will be no demand for your good in this market. The same applies if one of the two firms offers the good at a price higher than 50 MU. If both firms offer the same price in a market, each firm will receive half of the demand in this market. If both prices of the same market are above 50 MU, there will be no demand for both firms in this market.

Profit

Your profit in a retail period depends on your prices and your demand. Therefore, the profit in one market is calculated as follows:

$$Profit = Demand \cdot (Price - Cost\ of\ Provision)$$

Your profit in a period is calculated by summing up your profit in market A and your profit in market B. The profits in both markets are being accumulated over all periods and constitute your overall profit.

Example Firms 1 and 2 offer the following prices:

	Market A	Market B
Firm 1	35 MU	40 MU
Firm 2	45 MU	40 MU

The profits of firm 1 in both markets are calculated as follows:

- Profit in market A: $10000 \cdot (35\text{ MU} - 10\text{ MU}) = 250000\text{ MU} = \text{EUR } 0.25$
- Profit in market B: $\frac{1}{2} \cdot 10000 \cdot (40\text{ MU} - 10\text{ MU}) = 150000\text{ MU} = \text{EUR } 0.15$
- Profit of the period: $\text{EUR } 0.25 + \text{EUR } 0.15 = \text{EUR } 0.40$

The profits of firm 2 in both markets are calculated as follows:

- Profit in market A: $0 \cdot (45\text{ MU} - 10\text{ MU}) = 0\text{ MU} = \text{EUR } 0.00$
- Profit in market B: $\frac{1}{2} \cdot 10000 \cdot (40\text{ MU} - 10\text{ MU}) = 150000\text{ MU} = \text{EUR } 0.15$
- Profit of the period: EUR 0.00 + EUR 0.15 = EUR 0.15

Software display

FIGURE C.5: *Display of the experimental software.*

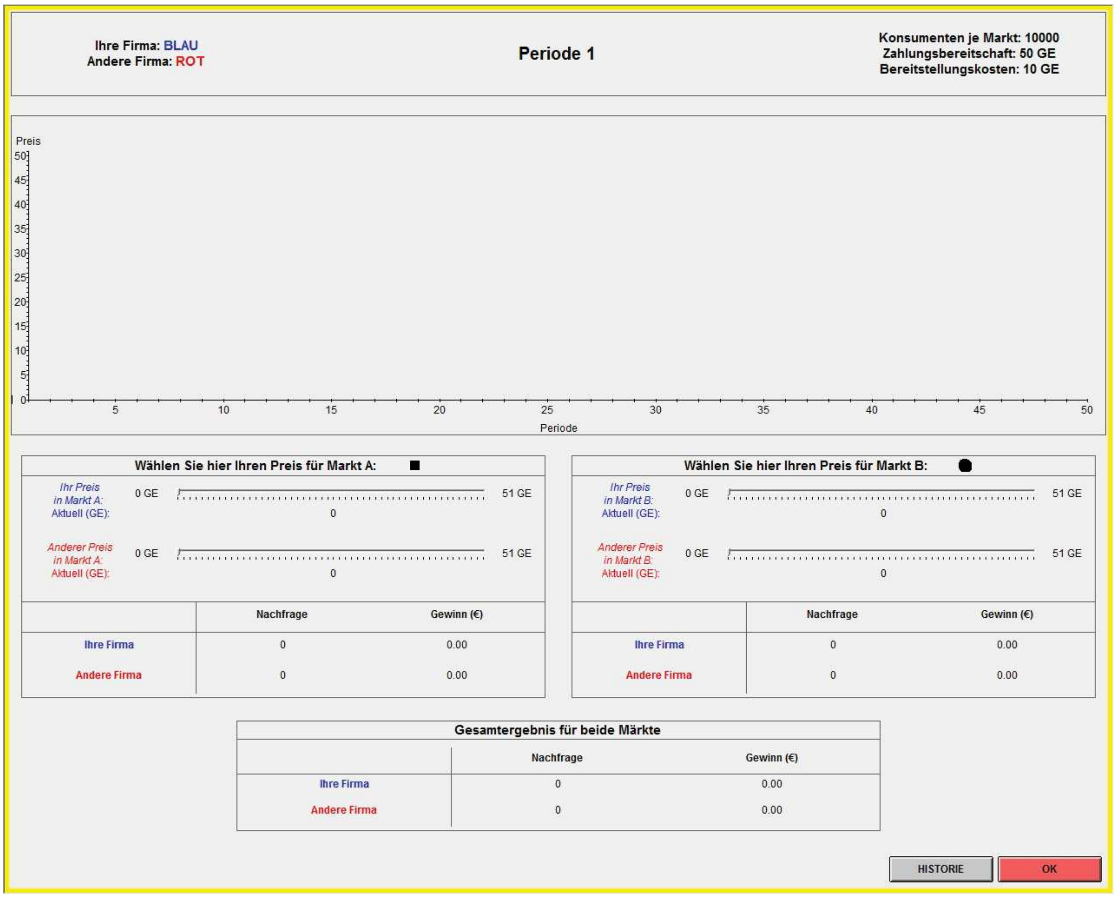


Figure C.5 shows the display of the experimental software during a period. To distinguish between both firms, the information for your firm is colored BLUE, whereas the information for the other firm is colored RED. The individual parts of the display will be explained in detail below.

Progress of the experiment The progress of the experiment is always shown at the top of the display. In the left part, the colors of your firm and the other firm are repeated. The middle part shows the current period of the experiment. The key decision factors are repeated in the right part.

Price trend The upper part of the display visualizes the trend of your prices and the trend of the prices of the other firm in both markets. The graphs that depict your prices are colored BLUE, whereas the graphs that depict the other firm's prices are colored RED. The graphs representing the prices in market A will be displayed as RECTANGLES and the graphs representing the prices in market B will be displayed as CIRCLES. During the experiment, the graphs will be drawn continuously from one to the next period.

Decision and testing environment In the lower part of the display you can enter your prices for the two markets. In addition, you can test the consequences of your pricing decision. In order to do so, you have to indicate your prices as well as your expectation of the prices of the other firm by using the slider.

As soon as you release the slider, the values in the table beneath are being updated. The table shows the effects of the respective price combination on the demand and on the profit in one period. For each market the demand and the profit of your firm and the other firm are being shown. The table beneath shows the demand and the profit accumulated over both markets for your firm and the other firm.

Please use the sliders for your firm to set your prices for the current period. Note that the actual prices of the other firm are set by the corresponding firm and not by you. Hence, the sliders for the other firm only serve as a means of decision support.

Please note: You cannot only use the testing environment in order to test the effects of your own pricing decision, but also to estimate the possible reactions to your current pricing decision of the other firm.

History By clicking the button "HISTORY" the following outcomes of the past periods are shown:

- Your price for market A in MU
- The price of the other firm for market A in MU
- Your price for market B in MU
- The price of the other firm for market B in MU
- Your profit in both markets in euro
- The profit of the other firm in both markets in euro

The history can be closed by clicking the button "HISTORY" again.

Concluding remarks

At the end of the experiment you will be shown your accumulated profits over all periods, which represent your payoff, in euro. After answering a short questionnaire you will be paid out your payoff by the person in charge of the experiment.

Do not hesitate to ask questions. As long as they refer to these instructions and not to possible strategies, we will answer your questions as good as possible. Please remember: The better you have understood these instructions, the more money you can earn.

Before the experiment starts, you will be asked some comprehension questions on the screen with regard to the understanding of the rules and the course of the experiment. Please enter the respective answers into your computer. Afterwards, the experiment will start automatically.

During the experiment you can take notes on the note pad that is laid out next to you on the table. In case of any questions, please remain seated and give the person in charge of the experiment a hand signal. Please wait until the person in charge of the experiment has arrived at your seat. Talk as quietly as possible when asking your question. Please remain seated after the end of the experiment as well and wait for further instructions by the person in charge of the experiment.

C.5 Upstream and downstream competition

The following experimental instructions are for the WCPC-MSR treatment.

Preliminary remarks

Welcome to the experiment and thank you very much for your participation.In this experiment you can earn an amount of money that depends on your decisions and the decisions of the other participants. Please address the person in charge of the experiment in case of questions. Please do not talk to the other participants during the entire experiment. Throughout the experiment we will use the currency euro and its subunits cent. At the beginning of the experiment your account balance is EUR 5.00. At the end of the experiment, the final account balance will be paid to you in cash.

During the experiment you represent a firm which is selling a good to consumers. Next to you, there are two other firms which are competing with you. All your decisions are made in real time, thus, they are immediately effective and visible to all other firms. Over the entire time horizon of the experiment, you play together with the same firms.

Experimental structure

There are three firms:

- Firm A
- Firm B
- Firm C

Firm A and Firm B are represented by participants of the experiment. Firm C acts computerized. Which firm you represent is randomly chosen at the beginning of the experiment and does not change over the entire experiment. Furthermore there are two markets:

- Wholesale market
- Retail market

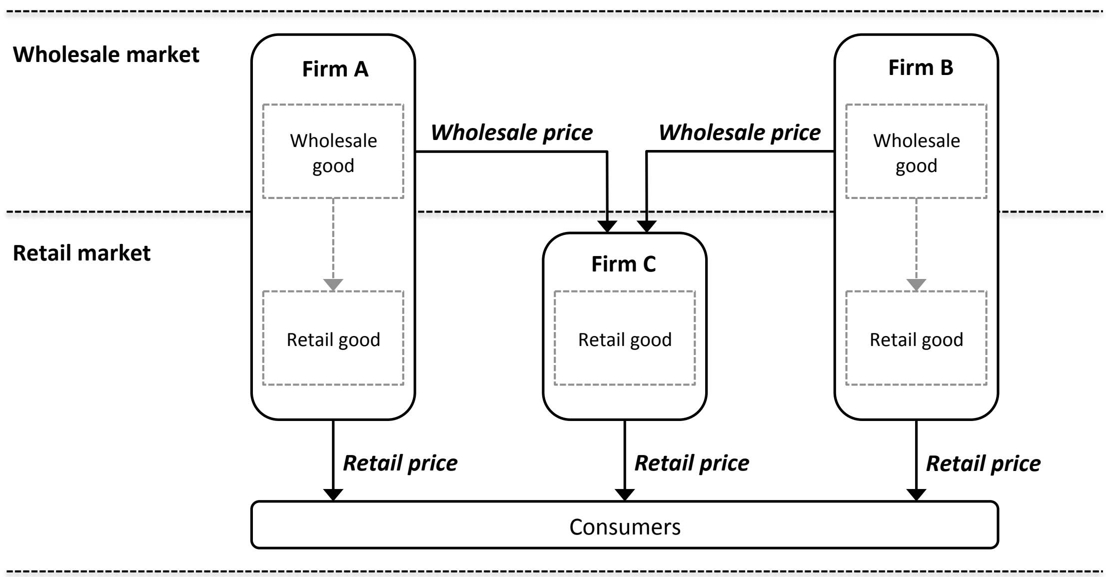
Figure C.6 visualizes the structure of the experiment. Each of the three firms offers a retail product on the retail market and chooses its retail price. In order to produce the retail product each firm needs a wholesale product. Only Firm A and Firm B offer the wholesale product in the wholesale market and choose their respective wholesale prices. Firm C has to buy the wholesale product from one of the two other firms in order to be able to offer its retail product.

Wholesale Market

The wholesale products of Firm A and Firm B are equal. Thereby, the following holds:

- Firm C chooses automatically the cheaper wholesale product to satisfy its demand.

FIGURE C.6: *Structure of the experiment.*



- If Firm A and Firm B offer the identical wholesale price, Firm C chooses the wholesale product from the firm which had previously offered the lower price.
- If Firm A and Firm B offer the identical wholesale price at the beginning, Firm C chooses randomly from which firm it purchases the wholesale product.

There are no handling costs for the wholesale product. The prices of the wholesale products range from 0 to 100.

Retail market

The retail products differ between firms. The demand of your retail product depends on your retail price and the retail prices of the other firms. Thereby, the following holds under the assumption that the other retail prices remain unchanged:

- If you increase your retail price, the demand of your retail product decreases.

- If one of the other firms increases its retail price, the demand of your retail products increases.
- If all firms increase their retail price, the total demand of all retail products decreases.

If your retail price is located below the average of all three retail prices, the demand of your retail product increases. If your retail price is located above the average of all three retail prices, the demand of your retail product decreases. The extent of the deviation of your retail price from the average of all three retail prices determines the magnitude of this effect. If your retail price is above the average of all three retail prices, the demand of your retail product may fall to zero. Firm C chooses its profit-maximizing retail price in reaction to the effective wholesale price and the retail prices chosen by Firm A and Firm B.

There are no handling costs for the retail product. The prices of the retail products range from 0 to 100.

Profits

The profits of the three firms depend on the retail and wholesale prices. The calculations for the profits of Firm A and Firm B depend on Firm C's decision which firm to choose as its wholesale provider.

If Firm C chooses to purchase its wholesale product from Firm A, the following holds for the profits of each firm:

$$Profit_A = Retail\ Price_A \cdot Demand_A + Wholesale\ Price_A \cdot Demand_C$$

$$Profit_B = Retail\ Price_B \cdot Demand_B$$

$$Profit_C = (Retail\ Price_C - Wholesale\ price_A) \cdot Demand_C$$

If Firm C chooses to purchase its wholesale product from Firm B, the following holds for the profits of each firm:

$$Profit_A = Retail\ Price_A \cdot Demand_A$$
$$Profit_B = Retail\ Price_B \cdot Demand_B + Wholesale\ Price_B \cdot Demand_C$$
$$Profit_C = (Retail\ Price_C - Wholesale\ Price_B) \cdot Demand_C$$

Experimental procedure

The experiment is composed of two stages. At the first stage, as Firm A or Firm B, you choose your initial retail price and your initial wholesale price. Before making your final decision, you can test how a price combination affects the profits of all three firms. This does not influence your account balance. After all firms have made their initial price decision and have confirmed their decisions with a click on "apply initial prices", the second stage of the experiment starts.

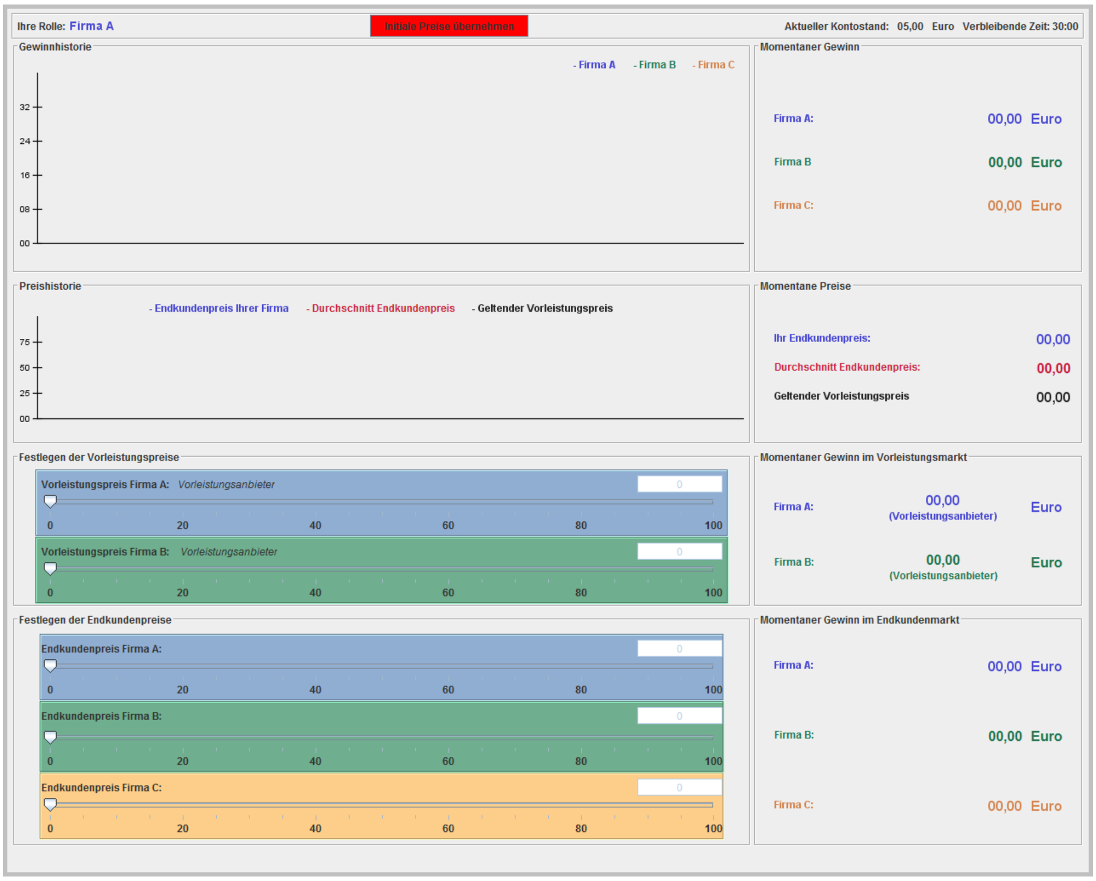
The second stage lasts exactly 30 minutes. During this period of time, all decisions are made in real time and without any interruptions. Your price decision remains effective until you change your price. Note that subsequent to a change of your wholesale price, the price cannot be changed again for the next 30 seconds. Furthermore, please be aware that your wholesale price can not be located above your retail price.

Software display

Figure C.7 depicts the display of the exeriment software. In order to distinguish the firms, their labels are colored as follows:

- Firm A: BLUE
- Firm B: GREEN

FIGURE C.7: *Display of the experimental software.*



- Firm C: ORANGE

In the following, the individual sections of the display will be explained from the bottom up:

Experimental progress

On the left-hand side, it is denoted whether you represent Firm A or Firm B. The figure illustrates this exemplarily for Firm A. On the right-hand side, your current account balance as well as the remaining duration of the experiment is displayed. Your current account balance consists of the initial balance of EUR 5.00 and the additionally earned profits during the experiment.

Current profits and profit history

On the right-hand side, the current profits of all firms are displayed. Note that current profits are scaled to the profit you would earn, if the current combination of all prices would be held over the entire 30 minutes of the experiment. As soon as one of the prices changes, the current profits are recalculated. On the left-hand side, the history of the current profits is displayed.

Current prices and price history

On the right-hand side, the current prices of all three firms are displayed. The effective wholesale price is always the lower wholesale price of both wholesale prices. On the left-hand side, the history of your retail price, the average retail price of all three firms and the effective wholesale price is displayed.

Wholesale prices and current profits in the wholesale market

On the left-hand side, Firm A and Firm B choose their wholesale prices. Be aware that Firm C offers no wholesale product and thus cannot choose a wholesale price. The wholesale price can be set with the corresponding slider by using the mouse or the arrow keys on the keyboard. Note that you can move all sliders at the first stage of the experiment and only the slider of your firm at the second stage of the experiment. The sliders of the other firms show their current wholesale prices. On the right-hand side the current profits in the wholesale market are displayed. Furthermore it is displayed which firms sells its wholesale product to Firm C. Note that subsequent to a change of your wholesale price, the price cannot be changed again for the next 30 seconds. Furthermore, please be aware that your wholesale price can not be located above your retail price.

Retail prices and current profits in the retail market

On the left-hand side, all of the three firms choose their retail price. The retail price can be set with the corresponding slider by using the mouse or the arrow keys on the keyboard. Note that you can move all sliders at the first stage of the experiment and only the slider of your firm at the second stage of the experiment. The sliders of the other firms show their current retail prices. On the right-hand side, the current profits in the retail market are displayed. Note that the displayed current profit of Firm C already includes the costs for the wholesale product.

Concluding remarks

Before the experiment starts, you will be asked a set of comprehension questions, displayed on the computer screen, that cover the rules and the procedure of the experiment. Please enter the respective answers. Thereupon, the experiment will start automatically and it is displayed which firm you represent.

In case of any questions during the experiment, please remain seated and inform the person in charge of the experiment by the means of a hand gesture. Please wait until the person in charge of the experiment has arrived at your seat. Talk as quietly as possible when asking your question. Please remain seated after the end of the experiment and wait for further instructions from the person in charge of the experiment.

Appendix D

Statistical Analyses

D.1 Sequences of decision-making

TABLE D.1: *Average degrees of tacit collusion over the entire time horizon across treatments.*

Treatment	Obs.	$\varphi_{p/q}^{Nash}$	φ_{Π}^{Nash}	$\varphi_{p/q}^{Walras}$	φ_{Π}^{Walras}
DB2	12	0.832	0.806	0.916	0.951
		(0.249)	(0.302)	(0.124)	(0.075)
DB3	12	0.605	0.611	0.737	0.827
		(0.324)	(0.301)	(0.216)	(0.134)
DC2	12	0.627	0.437	0.907	0.965
		(0.550)	(1.030)	(0.138)	(0.064)
DC3	12	0.397	0.249	0.759	0.880
		(0.484)	(0.702)	(0.193)	(0.112)
RB2	12	0.769	0.712	0.884	0.928
		(0.343)	(0.453)	(0.172)	(0.113)
RB3	11	0.539	0.491	0.693	0.774
		(0.306)	(0.324)	(0.204)	(0.144)
RC2	12	0.789	0.688	0.947	0.980
		(0.259)	(0.344)	(0.065)	(0.021)
RC3	13	0.386	0.186	0.754	0.870
		(0.473)	(0.745)	(0.189)	(0.119)

Standard deviations in parentheses.

D.2 Number of competitors

TABLE D.2: *Multilevel mixed-effects linear regressions of tacit collusion on number of competitors and competition model on the basis of all treatments.*

Covariate	(1) φ_p^{Nash}	(2) φ_Π^{Nash}	(3) φ_p^{Walras}	(4) φ_Π^{Walras}
Duopoly	0.208***	0.306***	0.233***	0.276***
	(0.037)	(0.075)	(0.030)	(0.044)
Quadropoly	−0.041	−0.007	−0.009	0.031
	(0.046)	(0.093)	(0.037)	(0.055)
Cournot	−0.249***	−0.473***	0.316***	0.587***
	(0.074)	(0.168)	(0.047)	(0.066)
Constant	0.077	0.003	0.138***	0.085
	(0.056)	(0.124)	(0.047)	(0.062)
Groups (s)	9	9	9	9
Groups (m)	10	10	10	10
Observations	23	23	23	23

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE D.3: *Inter-study average degrees of tacit collusion and one-tailed matched-samples Wilcoxon signed-rank tests on the basis of all treatments.*

	Studies	φ_p^{Nash}	φ_Π^{Nash}	φ_p^{Walras}	φ_Π^{Walras}
2 vs. 3					
Duopoly	7	0.110	−0.003	0.480	0.592
Triopoly	7	−0.079	−0.291	0.260	0.340
p-value	7	0.009	0.022	0.009	0.009
2 vs. 4					
Duopoly	6	0.302	0.335	0.452	0.496
Quadropoly	6	0.025	−0.001	0.204	0.254
p-value	6	0.014	0.014	0.014	0.014
3 vs. 4					
Triopoly	3	0.035	0.024	0.196	0.274
Quadropoly	3	0.049	0.051	0.174	0.249
p-value	3	0.946	0.946	0.500	0.500

TABLE D.4: *Mixed-effects linear regressions of tacit collusion on number of competitors and competition model with an interaction effect for validation experiment.*

Covariate	(1) $\varphi_{p/q}^{Nash}$	(2) φ_{Π}^{Nash}	(3) $\varphi_{p/q}^{Walras}$	(4) φ_{Π}^{Walras}
Duopoly	0.204**	0.262**	0.144***	0.101***
	(0.094)	(0.123)	(0.045)	(0.031)
Quadropoly	−0.148	−0.128	−0.145**	−0.145***
	(0.125)	(0.162)	(0.060)	(0.040)
Cournot	−0.230**	−0.251**	−0.016	0.034
	(0.094)	(0.123)	(0.045)	(0.031)
Quadropoly x Cournot	0.016	0.096	0.029	0.091*
	(0.166)	(0.216)	(0.080)	(0.054)
Period	−0.002*	−0.003	−0.001**	−0.001**
	(0.001)	(0.002)	(< 0.001)	(< 0.001)
Constant	0.673***	0.611***	0.788***	0.851***
	(0.082)	(0.106)	(0.039)	(0.026)
Groups	71	71	71	71
Observations	4,260	4,260	4,260	4,260

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

D.3 Price discrimination

FIGURE D.1: *Prices over time by groups and rounds for treatment UP.*

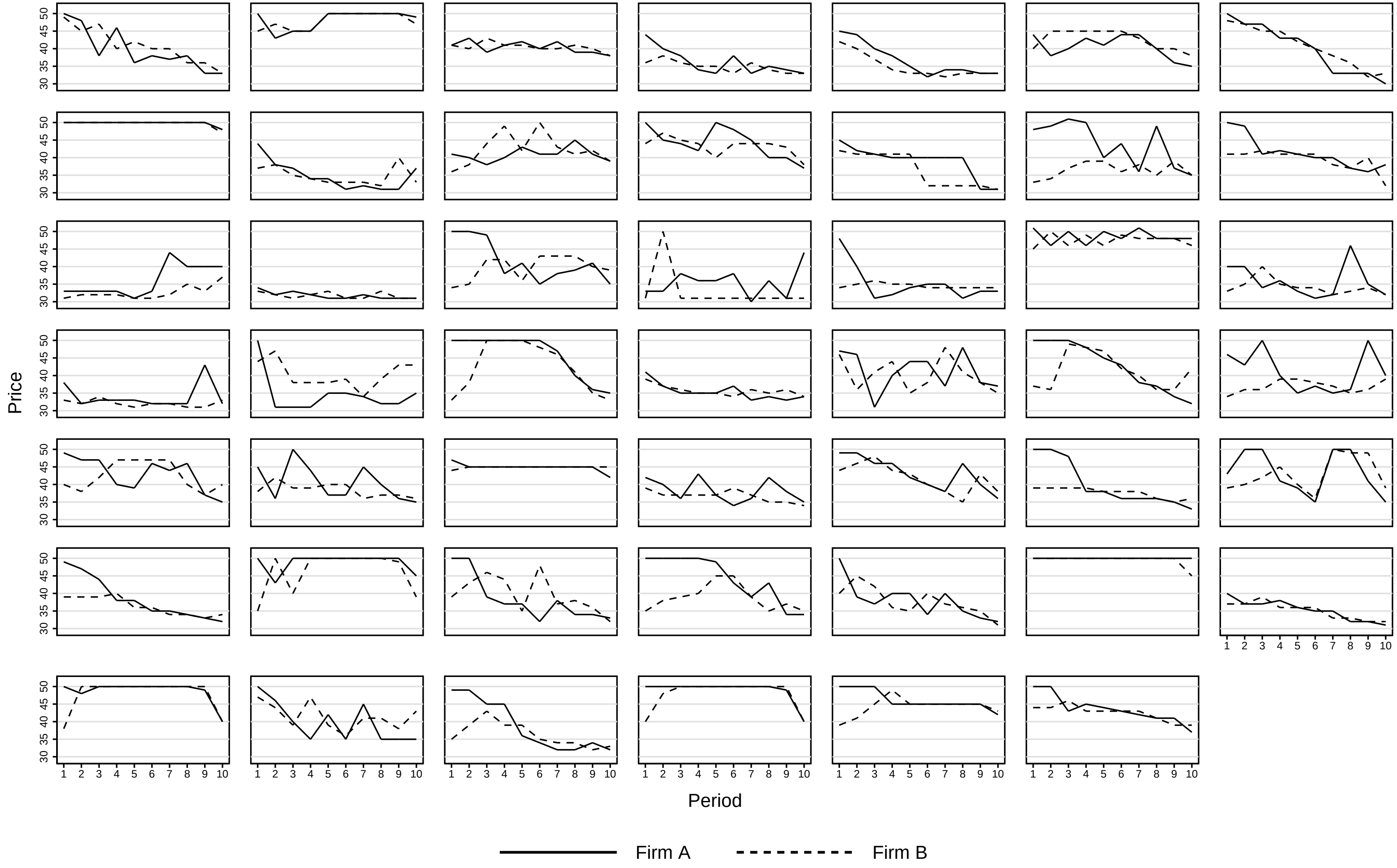


FIGURE D.2: *Prices over time in market M_1 by groups and rounds for treatment PD.*

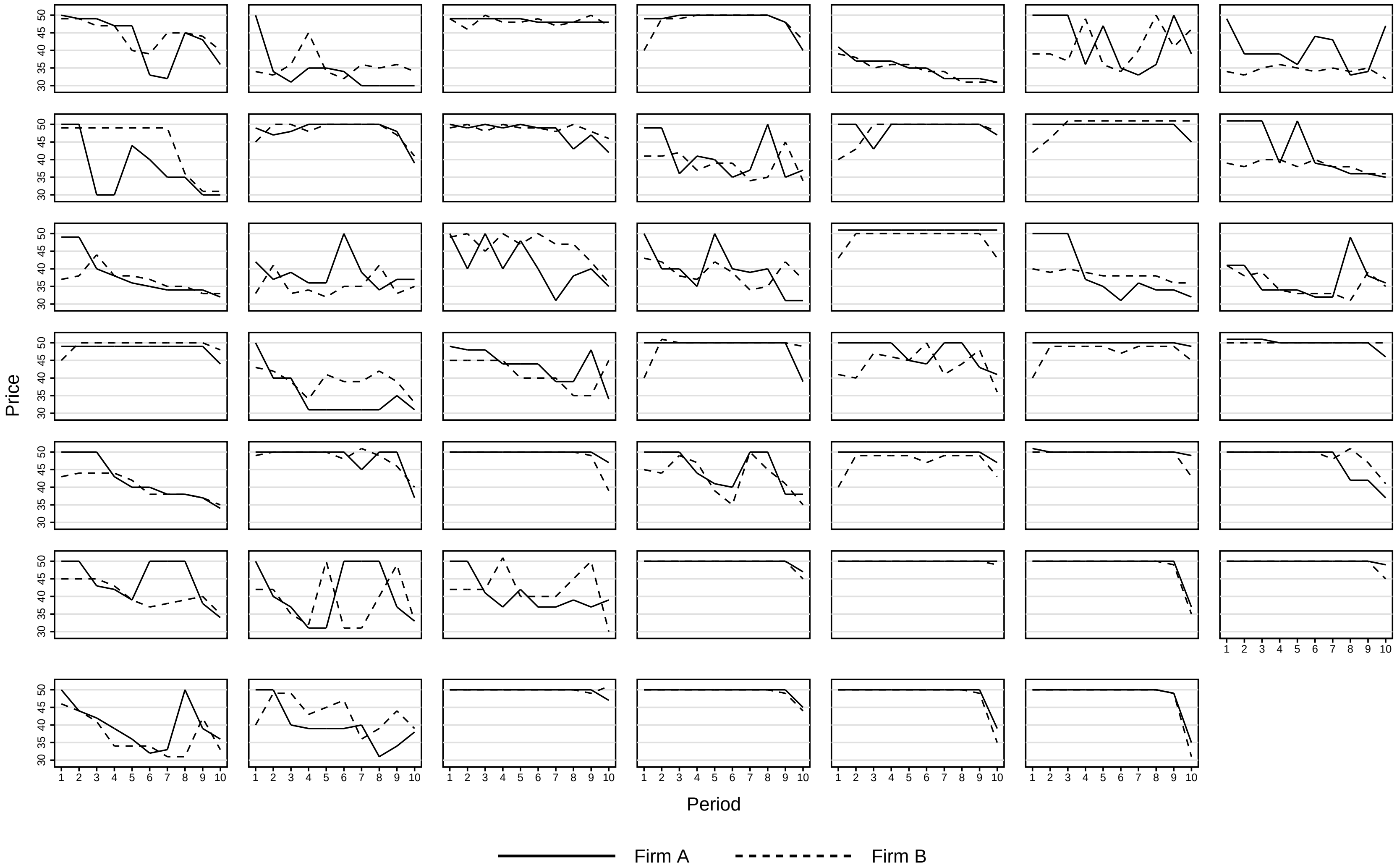
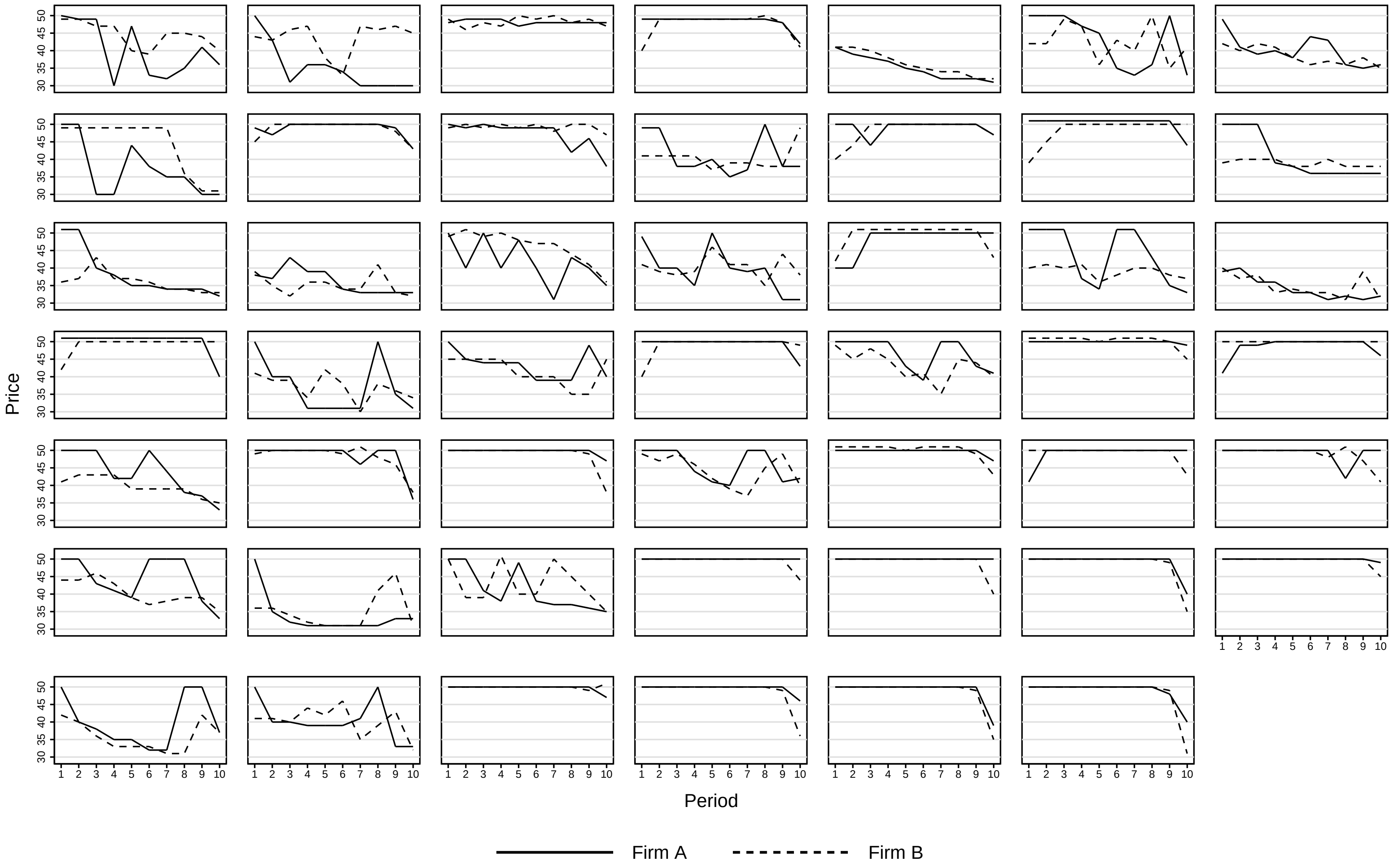


FIGURE D.3: *Prices over time in market M_2 by groups and rounds for treatment PD.*



D.4 Multimarket contact

FIGURE D.4: *Prices over time by cohorts for treatment SMC.*

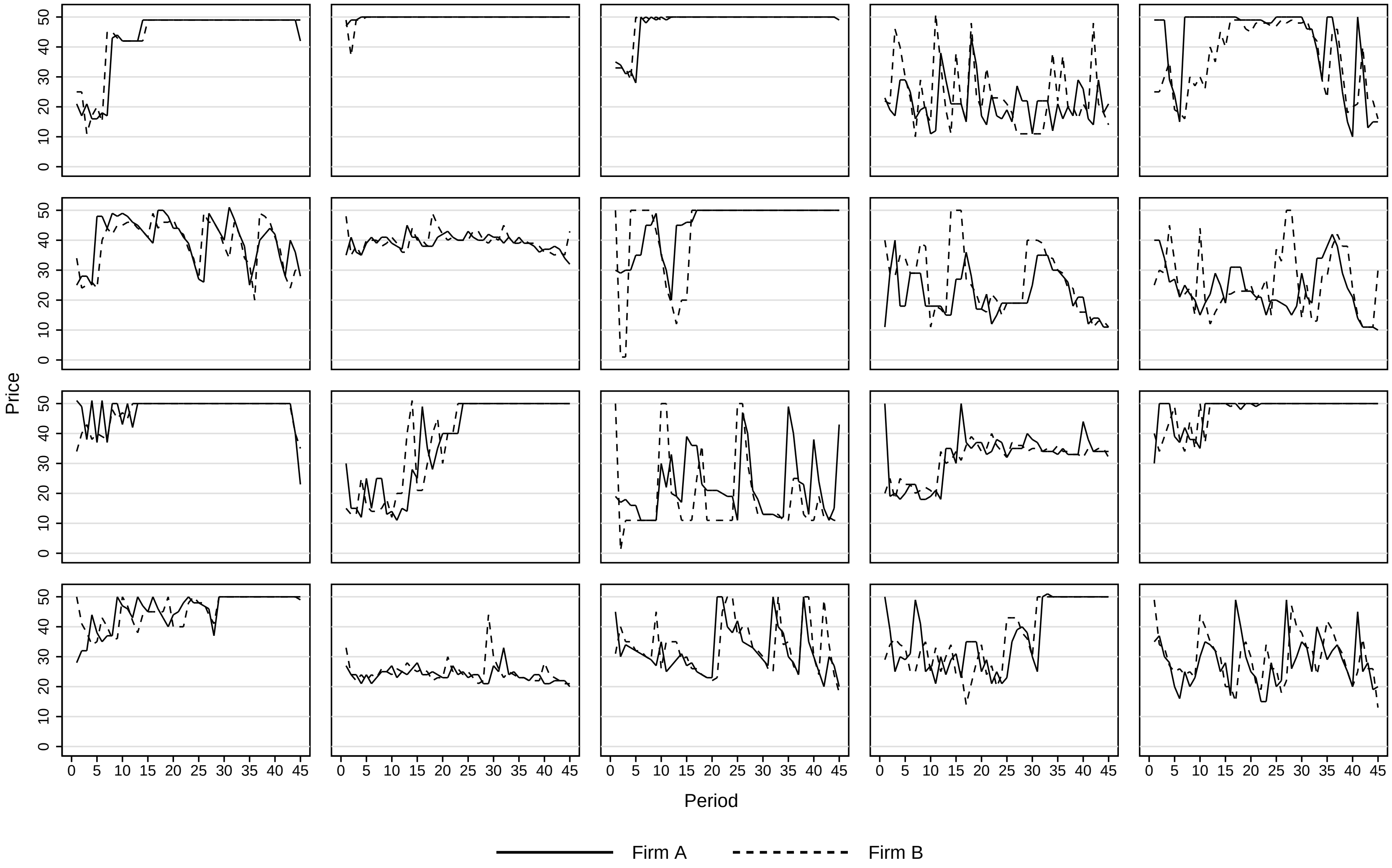


FIGURE D.5: *Prices over time in market M_1 by cohorts for treatment MMC.*

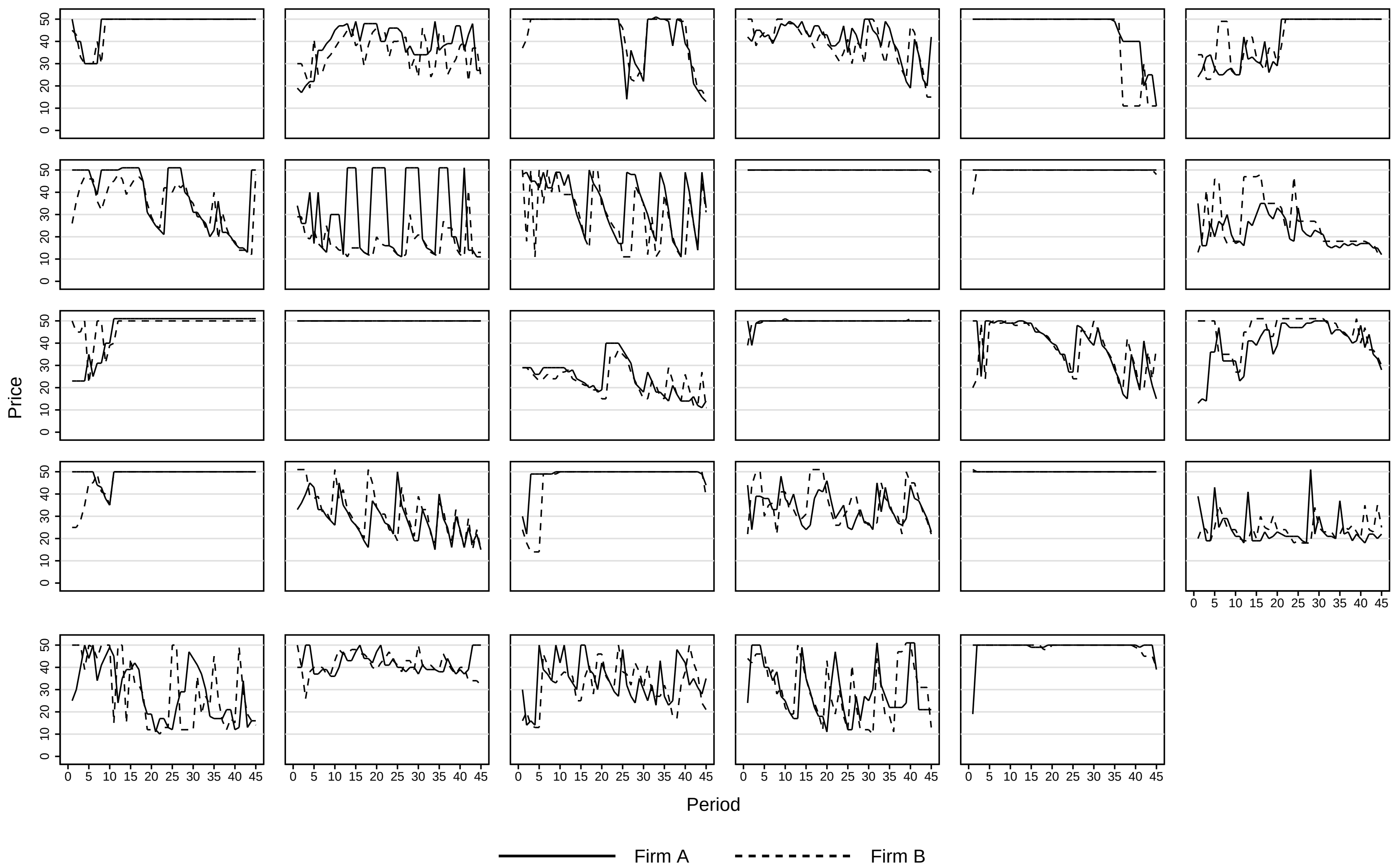


FIGURE D.6: *Prices over time in market M_2 by cohorts for treatment MMC.*

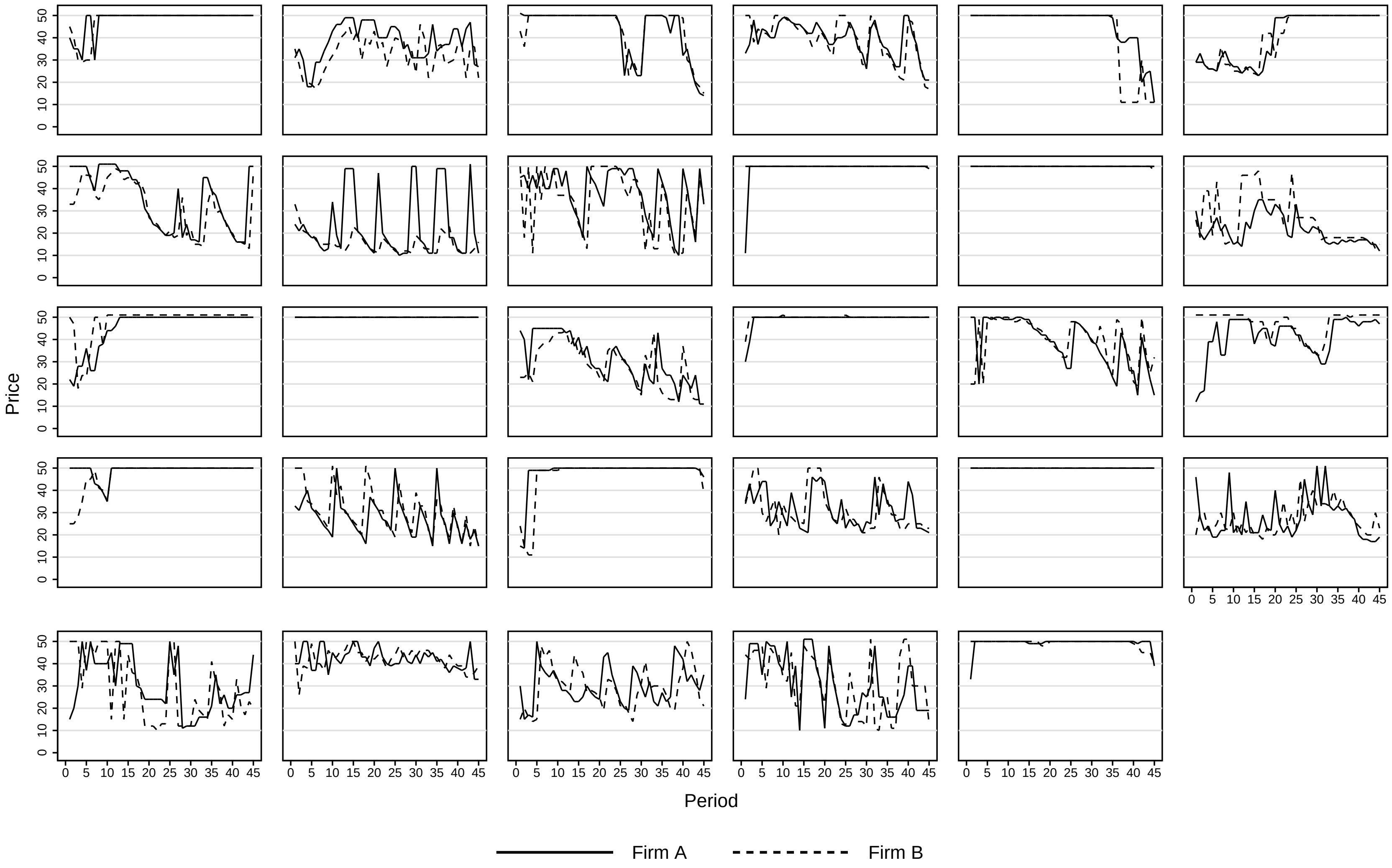


FIGURE D.7: *Prices over time in market M_1 by cohorts for treatment PMC.*

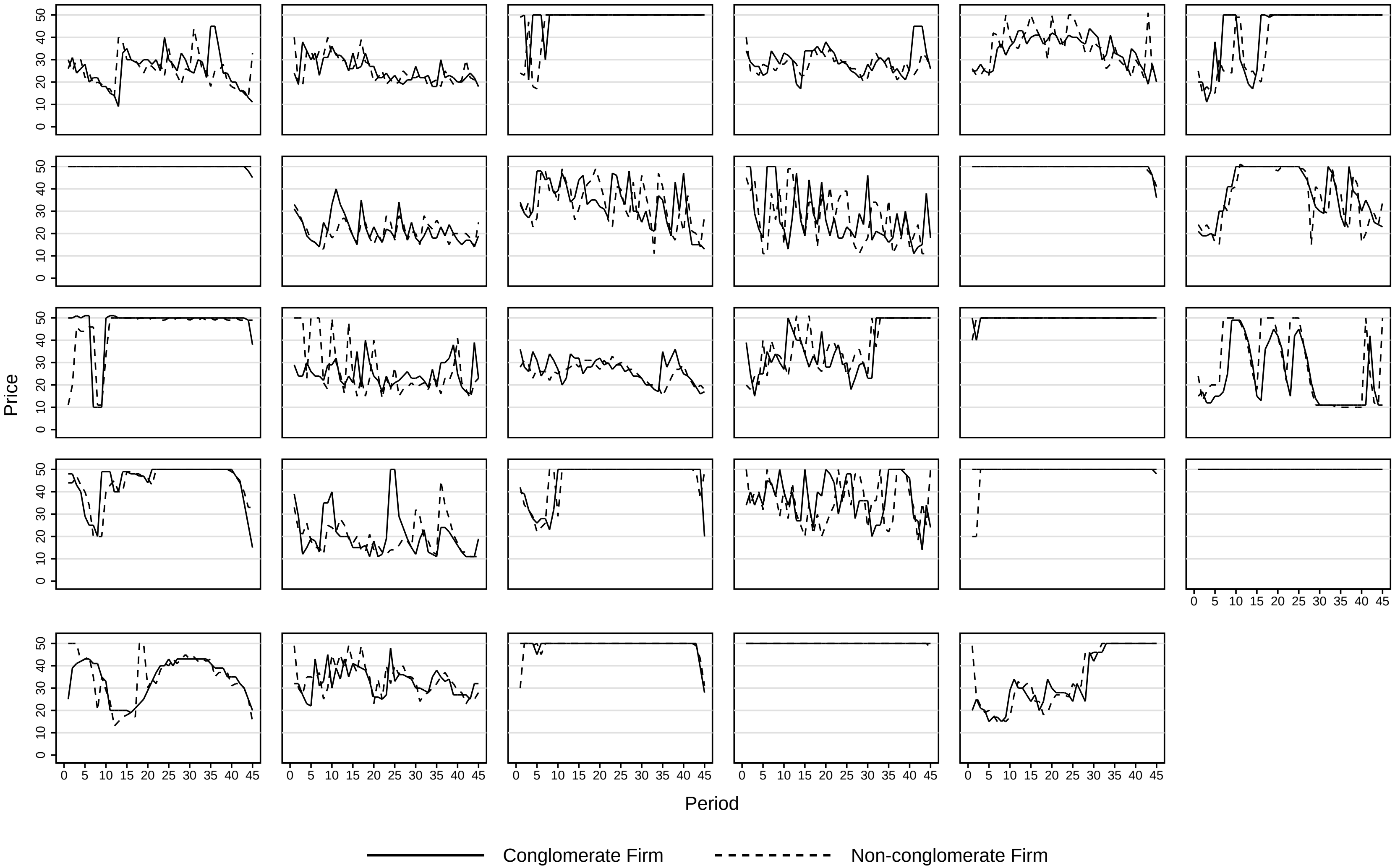
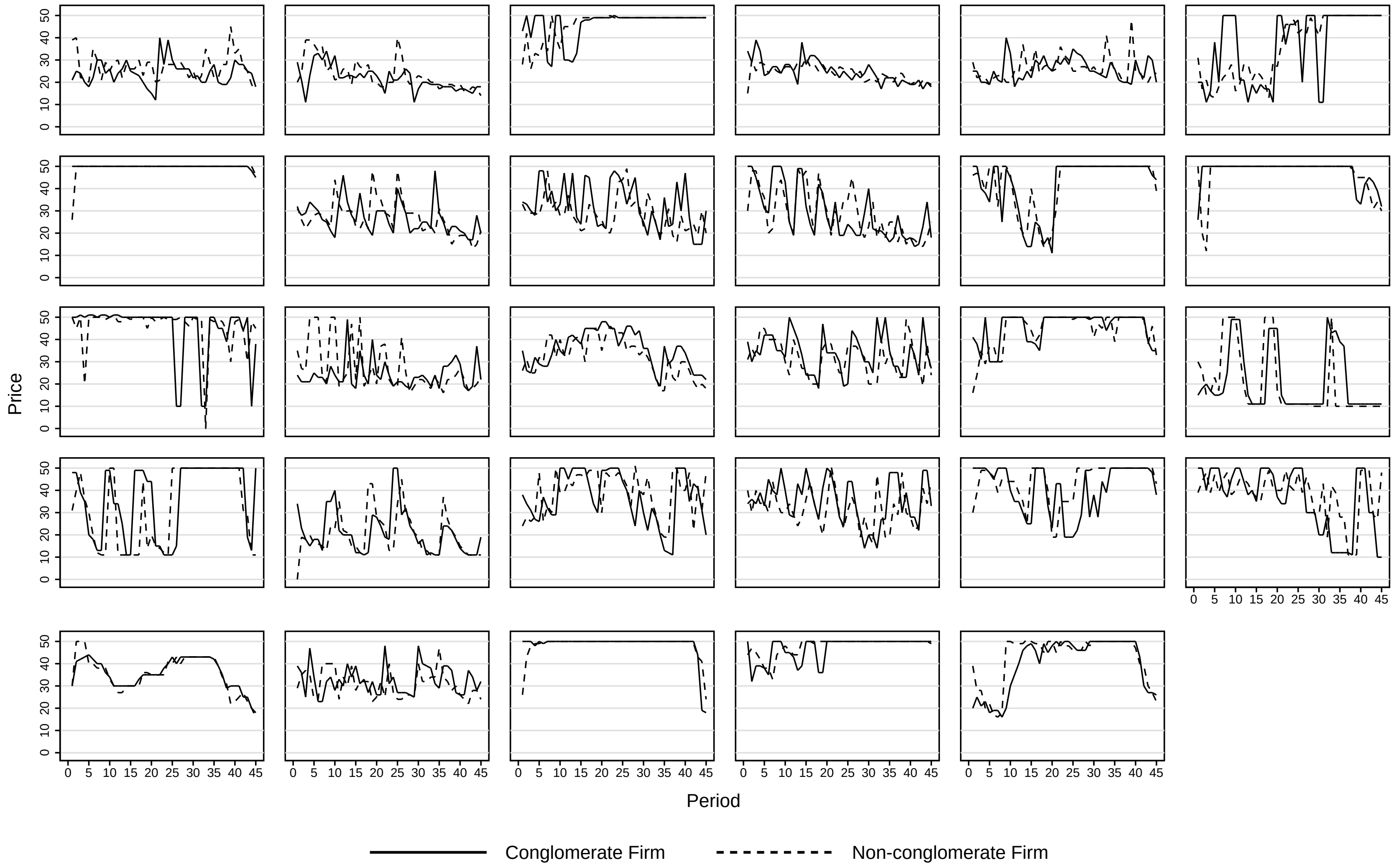


FIGURE D.8: *Prices over time in market M_2 by cohorts for treatment PMC.*



D.5 Upstream and downstream competition

TABLE D.5: *Average market prices, profits, and consumer surplus across treatments.*

Treatment	N	Tuples	a_m	ψ_m	π_{AB}	π_D	$\widetilde{CS}$
WM-NR	12	28,800	72.193	68.841	17.476	1.437	0.277
WM-MSR	11	26,400	72.368	74.253	18.808	1.366	0.230
WC-NR	9	21,600	76.960	78.092	19.611	1.441	0.197
WC-MSR	10	24,000	69.758	83.170	20.220	2.789	0.154
WCPC-NR	10	24,000	47.813	54.949	13.579	2.518	0.421
WCPC-MSR	12	28,800	52.429	67.187	16.277	3.165	0.306
Total	64	153,600	64.998	70.830	17.600	2.129	0.266

Averages are based on minutes [5,25] of the game phase.

TABLE D.6: *OLS regressions of market variables on industry structures.*

Covariate	(1) a_m	(2) ψ_m	(3) π_{AB}	(4) π_D	(5) $\widetilde{CS}$
WC	4.768	9.251	2.135	0.004	−0.080
	(9.504)	(7.990)	(2.075)	(0.587)	(0.074)
WCPC	−24.380***	−13.892*	−3.897*	1.082***	0.144*
	(9.130)	(7.645)	(1.973)	(0.406)	(0.073)
MSR	0.175	5.412	1.333	−0.071	−0.047
	(8.100)	(6.624)	(1.692)	(0.383)	(0.062)
WC x MSR	−7.377	−0.334	−0.724	1.419*	0.004
	(12.972)	(10.884)	(2.800)	(0.755)	(0.102)
WCPC x MSR	4.441	6.826	1.365	0.718	−0.067
	(12.501)	(10.339)	(2.668)	(0.573)	(0.099)
Period	0.004**	0.003***	0.001***	> -0.001	> -0.001***
	(0.001)	(0.001)	(< 0.001)	(< 0.001)	(< 0.001)
Constant	65.707***	62.550***	15.929***	1.539***	0.336***
	(5.886)	(4.647)	(1.175)	(0.344)	(0.043)
Observations	153,600	153,600	153,600	153,600	153,600

Clustered standard errors (by cohort) in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

FIGURE D.9: *Average wholesale (dashed) and retail (solid) market prices over time across treatments. For purposes of illustration, every point in the graphs is a median over 50 subsequent ticks.*

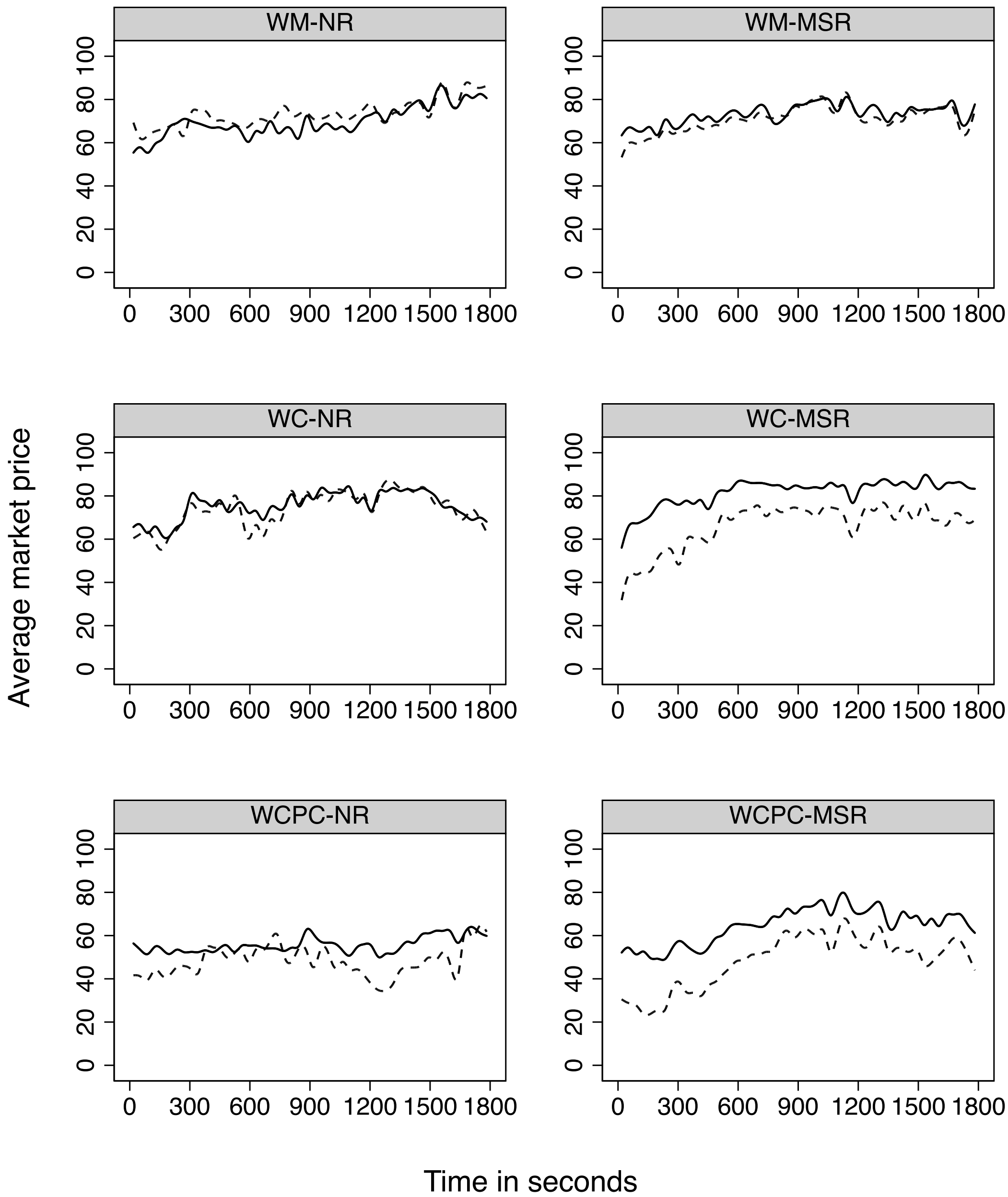


TABLE D.7: *Treatment comparisons of the wholesale market price a_m.*

	NR		MSR
WM	73.573	$\sim$	72.572
	$\wedge^{*}$		$\wr$
WC	86.085	$>^{*}$	83.082
	$\vee^{***}$		$\vee^{**}$
WCPC	40.540	$\sim$	49.049
	$\wedge^{***}$		$\wedge^{**}$
WM	73.573		72.572

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

TABLE D.8: *Treatment comparisons of the retail market price ψ_m.*

	NR		MSR
WM	65.499	$\sim$	83.124
	$\wedge^{**}$		$\wedge^{**}$
WC	88.407	$\sim$	92.281
	$\vee^{***}$		$\vee^{***}$
WCPC	49.560	$<^{*}$	67.415
	$\wedge^{**}$		$\wr$
WM	65.499		83.124

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

TABLE D.9: *Treatment comparisons of the integrated firms' average profit π_{AB}.*

	NR		MSR
WM	16.383	$\sim$	20.750
	$\wedge^{**}$		$\wr$
WC	22.434	$\sim$	22.802
	$\vee^{***}$		$\vee^{***}$
WCPC	12.243	$<^{*}$	15.866
	$\wedge^{***}$		$\wr$
WM	16.383		20.750

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

TABLE D.10: *Treatment comparisons of the reseller's profit π_D.*

	NR		MSR
WM	0.899	$\sim$	1.028
	$\wr$		$\wedge^{**}$
WC	0.258	$<^{***}$	2.339
	$\wedge^{***}$		$\wr$
WCPC	2.491	$\sim$	2.322
	$\wr$		$\vee^{***}$
WM	0.899		1.028

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

TABLE D.11: *Treatment comparisons of the consumer surplus $\widetilde{CS}$.*

	NR		MSR
WM	0.292	$\sim$	0.153
	$\vee^{*}$		$\vee^{**}$
WC	0.097	$\sim$	0.062
	$\wedge^{***}$		$\wedge^{***}$
WCPC	0.461	$>^{**}$	0.298
	$\vee^{***}$		$\vee^{*}$
WM	0.292		0.153

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

TABLE D.12: *Quantile regression of integrated firms' profit on access provider role under wholesale monopoly.*

Covariate	π_i
Firm A	5.416***
	(1.805)
Period	0.001
	(0.001)
Constant	18.604***
	(3.972)
Observations	57,600

Clustered standard errors (by cohort) in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

References

Abbink, K. and Brandts, J. (2005). Price competition under cost uncertainty: A laboratory analysis. *Economic Inquiry*, 43(3):636–648.

Abbink, K. and Brandts, J. (2008). 24. Pricing in Bertrand competition with increasing marginal costs. *Games and Economic Behavior*, 63(1):1–31.

Abbink, K., Brandts, J., and McDaniel, T. (2003). Asymmetric demand information in uniform and discriminatory call auctions: An experimental analysis motivated by electricity markets. *Journal of Regulatory Economics*, 23(2):125–144.

Abbink, K., Irlenbusch, B., Pezanis-Christou, P., Rockenbach, B., Sadrieh, A., and Selten, R. (2005). An experimental test of design alternatives for the british 3G/UMTS auction. *European Economic Review*, 49(2):505–530.

Adilov, N., Light, T., Schuler, R. E., Schulze, W. D., Toomey, D. E., and Zimmerman, R. D. (2005). Differences in capacity requirements, line flows and system operability under alternative deregulated market structures: Simulations derived from experimental trials. In *Power Engineering Society General Meeting*, volume 2, pages 2035–2041. IEEE.

Alcácer, J. and Zhao, M. (2012). Local r&d strategies and multilocation firms: The role of internal linkages. *Management Science*, 58(4):734–753.

Alexandersson, G. (2009). Rail privatization and competitive tendering in europe. *Built Environment*, 35(1):43–58.

Alger, D. (1987). Laboratory tests of equilibrium predictions with disequilibrium data. *The Review of Economic Studies*, 54(1):105–145.

Aloysius, J., Deck, C., and Farmer, A. (2012). A comparison of bundling and sequential pricing in competitive markets: Experimental evidence. *International Journal of the Economics of Business*, 19(1):25–51.

Altavilla, C., Luini, L., and Sbriglia, P. (2006). Social learning in market games. *Journal of Economic Behavior & Organization*, 61(4):632–652.

Anand, K. S. and Mendelson, H. (1997). Information and organization for horizontal multimarket coordination. *Management Science*, 43(12):1609–1627.

Andersson, O. and Wengström, E. (2007). Do antitrust laws facilitate collusion? experimental evidence on costly communication in duopolies. *The Scandinavian Journal of Economics*, 109(2):321–339.

Angrist, J. D. and Pischke, J.-S. (2010). The credibility revolution in empirical economics: How better research design is taking the con out of econometrics. *The Journal of Economic Perspectives*, 24(2):3–30.

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

Armstrong, M., Doyle, C., and Vickers, J. (1996). The access pricing problem: A synthesis. *The Journal of Industrial Economics*, 44(2):131–150.

Armstrong, M. and Vickers, J. (1998). The access pricing problem with deregulation: A note. *The Journal of Industrial Economics*, 46(1):115–121.

Atiyas, I., Doganoglu, T., and Inceoglu, F. (2015). Upstream competition and complexity of contracts. Mimeo. Available at `https://editorialexpress.com/cgi-bin/conference/download.cgi?db_name=EARIE42&paper_id=501`. Accessed January 29, 2016.

Audia, P. G., Sorenson, O., and Hage, J. (2001). Tradeoffs in the organization of production: Multiunit firms, geographic dispersion and organizational learning. In Baum, J. A. C. and Greve, H. R., editors, *Multiunit Organization and Multimarket Strategy*, volume 18 of *Advances in Strategic Management*, chapter 4, pages 75–105. Elsevier, Oxford.

Banerjee, A. and Dippon, C. M. (2009). Voluntary relationships among mobile network operators and mobile virtual network operators: An economic explanation. *Information Economics and Policy*, 21(1):72–84.

Banerjee, S., de Vries, F. P., Hanley, N., and van Soest, D. P. (2014). The impact of information provision on agglomeration bonus performance: An experimental study on local networks. *American Journal of Agricultural Economics*, 96(4):1009–1029.

Banerjee, S., Kwasnica, A. M., and Shortle, J. S. (2012). Agglomeration bonus in small and large local networks: A laboratory examination of spatial coordination. *Ecological Economics*, 84:142–152.

Banerjee, S., Kwasnica, A. M., and Shortle, J. S. (2015). Information and auction performance: A laboratory study of conservation auctions for spatially contiguous land management. *Environmental and Resource Economics*, pages 1–23.

Banks, J., Olson, M., Porter, D., Rassenti, S., and Smith, V. (2003). Theory, experiment and the federal communications commission spectrum auctions. *Journal of Economic Behavior & Organization*, 51(3):303–350.

Banks, J. S., Ledyard, J. O., and Porter, D. P. (1989). Allocating uncertain and unresponsive resources: An experimental approach. *RAND journal of economics*, 20(1):1–25.

Baum, J. A. and Korn, H. J. (1999). Dynamics of dyadic competitive interaction. *Strategic Management Journal*, 20(3):251–278.

Baumol, W. J. (1977). On the proper cost tests for natural monopoly in a multiproduct industry. *The American Economic Review*, 67(5):809–822.

Baumol, W. J., Ordover, J. A., and Willig, R. D. (1997). Parity pricing and its critics: A necessary condition for efficiency in the provision of bottleneck services to competitors. *Yale Journal on Regulation*, 14:145–163.

Behavioural Insights Team (2015). The Behavioural Insights Team Update report 2013-2015. Available at `http://38r8om2xjhhl25mw24492dir.wpengine.netdna-cdn.com/wp-content/uploads/2015/08/BIT_Update-Report-Final-2013-2015.pdf`. Accessed January 29, 2016.

Bergin, J. and MacLeod, W. B. (1993). Continuous time repeated games. *International Economic Review*, 34(1):21–37.

Bernard, J. C., Schulze, W. D., and Mount, T. D. (2005). Bidding behaviour in the multi-unit Vickrey and uniform price auctions. *Applied Economics Letters*, 12(10):589–595.

Bernheim, B. D. and Whinston, M. D. (1990). Multimarket contact and collusive behavior. *RAND Journal of Economics*, 21(1):1–26.

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

Berninghaus, S. K. and Ehrhart, K.-M. (1998). Time horizon and equilibrium selection in tacit coordination games: Experimental results. *Journal of Economic Behavior & Organization*, 37(2):231–248.

Berninghaus, S. K. and Ehrhart, K.-M. (2003). The power of ESS: An experimental study. *Journal of Evolutionary Economics*, 13(2):161–181.

Berninghaus, S. K., Ehrhart, K.-M., and Keser, C. (1999). Continuous-time strategy selection in linear population games. *Experimental Economics*, 2(1):41–57.

Berninghaus, S. K., Ehrhart, K.-M., and Ott, M. (2006). A network experiment in continuous time: The influence of link costs. *Experimental Economics*, 9(3):237–251.

Berninghaus, S. K., Ehrhart, K.-M., Ott, M., and Vogt, B. (2007). Evolution of networks—an experimental analysis. *Journal of Evolutionary Economics*, 17(3):317–347.

Bertrand, J. (1883). Théorie des rechesses: Revue des "Théorie mathématique de la richesse sociale" par Léon Walras et "Recherches sur les principes mathématiques de la théorie des richesses" par Augustin Cournot. *Journal des Savants*, 67:499–508.

Bichler, M., Goeree, J., Mayer, S., and Shabalin, P. (2014). Spectrum auction design: Simple auctions for complex sales. *Telecommunications Policy*, 38(7):613–622.

Bichler, M., Shabalin, P., and Wolf, J. (2013). Do core-selecting combinatorial clock auctions always lead to high efficiency? an experimental analysis of spectrum auction designs. *Experimental Economics*, 16(4):511–545.

Bigoni, M., Casari, M., Skrzypacz, A., and Spagnolo, G. (2015a). Time horizon and cooperation in continuous time. *Econometrica*, 83(2):587–616.

Bigoni, M., Fridolfsson, S.-O., Le Coq, C., and Spagnolo, G. (2012). Fines, leniency, and rewards in antitrust. *The RAND Journal of Economics*, 43(2):368–390.

Bigoni, M., Fridolfsson, S.-O., Le Coq, C., and Spagnolo, G. (2015b). Trust, leniency, and deterrence. *Journal of Law, Economics, and Organization*, 31(4):663–689.

Binmore, K. and Klemperer, P. (2002). The biggest auction ever: The sale of the British 3G telecom licences. *The Economic Journal*, 112(478):C74–C96.

Boardman, A. E., Laurin, C., Moore, M. A., and Vining, A. R. (2013). Efficiency, profitability and welfare gains from the canadian national railway privatization. *Research in Transportation Business & Management*, 6:19–30.

Boeker, W., Goodstein, J., Stephan, J., and Murmann, J. P. (1997). Competition in a multimarket environment: The case of market exit. *Organization Science*, 8(2):126–142.

Bolton, G. E. and Ockenfels, A. (2000). ERC: A theory of equity, reciprocity, and competition. *American Economic Review*, 90(1):166–193.

Boone, J., Müller, W., and Suetens, S. (2014). Naked exclusion in the lab: The case of sequential contracting. *The Journal of Industrial Economics*, 62(1):137–166.

Boots, M. G., Rijkers, F. A., and Hobbs, B. F. (2004). Trading in the downstream european gas market: A successive oligopoly approach. *The Energy Journal*, 25(3):73–102.

Bosch-Domènech, A. and Vriend, N. J. (2003). Imitation of successful behaviour in Cournot markets. *The Economic Journal*, 113(487):495–524.

Bouckaert, J. and Kort, P. M. (2014). Merger incentives and the failing firm defense. *The Journal of Industrial Economics*, 62(3):436–466.

Boudreau, K. (2010). Open platform strategies and innovation: Granting access vs. devolving control. *Management Science*, 56(10):1849–1872.

Bourreau, M., Cambini, C., and Hoernig, S. (2012). Geographic access rules and investment. Mimeo. Available at `http://ssrn.com/abstract=2153445`. Accessed January 29, 2016.

Bourreau, M., Cambini, C., and Hoernig, S. (2015). Geographic access markets and investments. *Information Economics and Policy*, 31:13–21.

Bourreau, M., Hombert, J., Pouyet, J., and Schutz, N. (2011). Upstream competition between vertically integrated firms. *The Journal of Industrial Economics*, 59(4):677–713.

Bower, J. and Bunn, D. (2001). Experimental analysis of the efficiency of uniform-price versus discriminatory auctions in the england and wales electricity market. *Journal* *of Economic Dynamics and Control*, 25(3–4):561–592.

Brandts, J. and Guillén, P. (2007). Collusion and fights in an experiment with price-setting firms and advance production. *The Journal of Industrial Economics*, 55(3):453–473.

Brandts, J., Pezanis-Christou, P., and Schram, A. (2008). Competition with forward contracts: a laboratory analysis motivated by electricity market design*. *Economic Journal*, 118(525):192–214.

Brewer, P. J. and Plott, C. R. (1996). A binary conflict ascending price (BICAP) mechanism for the decentralized allocation of the right to use railroad tracks. *International Journal of Industrial Organization*, 14(6):857–886.

Briglauer, W., Frübing, S., and Vogelsang, I. (2015). The impact of alternative public policies on the deployment of new communications infrastructure - A survey. *Review of Network Economics*.

Briglauer, W., Götz, G., and Schwarz, A. (2011). Margin squeeze in fixed-network telephony markets–competitive or anticompetitive? *Review of Network Economics*, 10(4):Art. 3.

Brito, D. and Pereira, P. (2010). Access to bottleneck inputs under oligopoly: A prisoners' dilemma? *Southern Economic Journal*, 76(3):660–677.

Broadbent, C. D., Brookshire, D. S., Coursey, D., and Tidwell, V. (2014). An experimental analysis of water leasing markets focusing on the agricultural sector. *Agricultural Water Management*, 142:88–98.

Brunner, C., Goeree, J. K., Holt, C. A., and Ledyard, J. O. (2010). An experimental test of flexible combinatorial spectrum auction formats. *American Economic Journal: Microeconomics*, 2(1):39–57.

Bruttel, L. V. (2009). Group dynamics in experimental studies—the Bertrand paradox revisited. *Journal of Economic Behavior & Organization*, 69(1):51–63.

Caliskan, A., Porter, D., Rassenti, S., Smith, V. L., and Wilson, B. J. (2007). Exclusionary bundling and the effects of a competitive fringe. *Journal of Institutional and Theoretical Economics*, 163(1):109–132.

Camacho-Cuena, E., Requate, T., and Waichman, I. (2012). Investment incentives under emission trading: An experimental study. *Environmental and Resource Economics*, 53(2):229–249.

Cambini, C. and Jiang, Y. (2009). Broadband investment and regulation: A literature review. *Telecommunications Policy*, 33(10):559–574.

Carlton, D. W. (2008). Should "price squeeze" be a recognized form of anticompetitive conduct? *Journal of Competition Law and Economics*, 4(2):271–278.

Carpenter, J. P., Harrison, G. W., and List, J. A., editors (2005). *Field Experiments in Economics*, volume 10 of *Research in Experimental Economics*. JAI Press, Greenwich, CT.

Cason, T. N. (1995). Cheap talk price signaling in laboratory markets. *Information Economics and Policy*, 7(2):183–204.

Cason, T. N. (2010). What can laboratory experiments teach us about emissions permit market design? *Agricultural and Resource Economics Review*, 39(2):151–161.

Cason, T. N. and Davis, D. D. (1995). Price communications in a multi-market context: An experimental investigation. *Review of Industrial Organization*, 10(6):769–787.

Cason, T. N. and Gangadharan, L. (2011). Price discovery and intermediation in linked emissions trading markets: A laboratory study. *Ecological Economics*, 70(7):1424–1433.

Cave, M. (2006). Encouraging infrastructure competition via the ladder of investment. *Telecommunications Policy*, 30(3):223–237.

Cave, M. and Vogelsang, I. (2003). How access pricing and entry interact. *Telecommunications Policy*, 27(10):717–727.

Chamberlin, E. H. (1948). An experimental imperfect market. *Journal of Political Economy*, 56(2):95–108.

Chang, M.-H. and Harrington, Jr., J. E. (2000). Centralization vs. decentralization in a multi-unit organization: A computational model of a retail chain as a multi-agent adaptive system. *Management Science*, 46(11):1427–1440.

Chang, M.-H. and Harrington, Jr., J. E. (2003). Multimarket competition, consumer search, and the organizational structure of multiunit firms. *Management Science*, 49(4):541–552.

Chapman, L. D., Vossler, C. A., Mount, T. D., Barboni, V., Thomas, R. J., and Zimmerman, R. D. (2004). Market efficiency, competition, and communication in electric power markets: experimental results. *Ecological Economics*, 48(3):317–327.

Chen, Y. (2001). On vertical mergers and their competitive effects. *The RAND Journal of Economics*, 32(4):667–685.

Chen, Y. and Riordan, M. H. (2007). Vertical integration, exclusive dealing, and expost cartelization. *The RAND Journal of Economics*, 38(1):1–21.

Cherry, T. L. and McEvoy, D. M. (2013). Enforcing compliance with environmental agreements in the absence of strong institutions: An experimental analysis. *Environmental and Resource Economics*, 54(1):63–77.

Cheung, Y.-W. and Friedman, D. (2009). Speculative attacks: A laboratory study in continuous time. *Journal of International Money and Finance*, 28(6):1064–1082.

Choi, J. and Yi, S. (2000). Vertical foreclosure with the choice of input specifications. *The RAND Journal of Economics*, 31(4):717–743.

Ciliberto, F. and Williams, J. W. (2014). Does multimarket contact facilitate tacit collusion? inference on conduct parameters in the airline industry. *The RAND Journal of Economics*, 45(4):764–791.

Coate, M. B. and Simons, J. J. (2012). In defense of market definition. *The Antitrust Bulletin*, 57(4):667–717.

Cook, J. and Rabotyagov, S. S. (2014). Assessing irrigators' preferences for water market lease attributes with a stated preferences approach. *Water Resources and Economics*, 7:19–38.

Cooper, J. C. and Kovacic, W. E. (2012). Behavioral economics: implications for regulatory behavior. *Journal of Regulatory Economics*, 41(1):41–58.

Cournot, A. (1838). *Recherches sur les principes mathématiques de la théorie des richesses*. Hachette, Paris, France.

Cox, J. C., Offerman, T., Olson, M. A., and Schram, A. J. (2002). Competition for versus on the rails: A laboratory experiment. *International Economic Review*, 43(3):709–736.

Dal Bó, P. (2005). Cooperation under the shadow of the future: Experimental evidence from infinitely repeated games. *American Economic Review*, 95(5):1591–1604.

Davis, D. (2009a). Pure numbers effects, market power, and tacit collusion in posted offer markets. *Journal of Economic Behavior & Organization*, 72(1):475–488.

Davis, D. (2009b). Pure numbers effects, market power, and tacit collusion in posted offer markets. *Journal of Economic Behavior & Organization*, 72(1):475–488.

Davis, D., Korenok, O., and Reilly, R. (2009). Re-matching, information and sequencing effects in posted offer markets. *Experimental economics*, 12(1):65–86.

Davis, D. D. (2002). Strategic interactions, market information and predicting the effects of mergers in differentiated product markets. *International Journal of Industrial Organization*, 20(9):1277–1312.

Davis, D. D. and Holt, C. A. (1993). *Experimental Economics*. Princeton University Press, Princeton, NJ.

Davis, D. D. and Korenok, O. (2009). Posted offer markets in near-continuous time: An experimental investigation. *Economic Inquiry*, 47(3):449–466.

Davis, D. D., Korenok, O., and Reilly, R. (2010). Cooperation without coordination: signaling, types and tacit collusion in laboratory oligopolies. *Experimental Economics*, 13(1):45–65.

Davis, D. D. and Wilson, B. J. (2002). Experimental methods and antitrust policy. In Isaac, R. M. and Holt, C. A., editors, *Experiments Investigating Market Power*, volume 9 of *Research in Experimental Economics*, pages 61–94. JAI Press, Greenwich, CT.

Davis, D. D. and Wilson, B. J. (2005). Differentiated product competition and the antitrust logit model: an experimental analysis. *Journal of Economic Behavior & Organization*, 57(1):89–113.

Davis, D. D. and Wilson, B. J. (2006). Equilibrium price dispersion, mergers and synergies: an experimental investigation of differentiated product competition. *International Journal of the Economics of Business*, 13(2):169–194.

de Jong, G. (2012). Application of experimental economics in transport and logistics. *European Transport\Trasporti Europei*, 50(3):1–23.

Deck, C. and Nikiforakis, N. (2012). Perfect and imperfect real-time monitoring in a minimum-effort game. *Experimental Economics*, 15(1):71–88.

Deck, C. and Smith, V. (2013). Using laboratory experiments in logistics and supply chain research. *Journal of Business Logistics*, 34(1):6–14.

Deck, C. A. and Wilson, B. J. (2002). The effectiveness of low price matching in mitigating the competitive pressure of low friction electronic markets. *Electronic Commerce Research*, 2(4):385–398.

Deck, C. A. and Wilson, B. J. (2003). Automated pricing rules in electronic posted offer markets. *Economic Inquiry*, 41(2):208–223.

Deck, C. A. and Wilson, B. J. (2008). Experimental gasoline markets. *Journal of Economic Behavior & Organization*, 67(1):134–149.

DeGraba, P. (1990). Input market price discrimination and the choice of technology. *American Economic Review*, 80(5):1246–1253.

DeGraba, P. (2003). A bottleneck input supplier's opportunity cost of competing downstream. *Journal of Regulatory Economics*, 23(3):287–297.

Denton, M. J., Rassenti, S. J., and Smith, V. L. (2001a). Spot market mechanism design and competitivity issues in electric power. *Journal of Economic Behavior & Organization*, 44(4):435–453.

Denton, M. J., Rassenti, S. J., Smith, V. L., and Backerman, S. R. (2001b). Market power in a deregulated electrical industry. *Decision Support Systems*, 30(3):357–381.

Deutsche Börse (2015). Xetra release 16.0: Market model equities. `http://www.deutsche-boerse-cash-market.com/blob/1193332/fc4798a519e3e9c91eed90222840cf52/data/xetra-deutsche-boerse-cash-market-market-model-equities.pdf`. Accessed January 29, 2016.

Dewatripont, M. and Legros, P. (2013). Essential patents, frand royalties and technological standards. *The Journal of Industrial Economics*, 61(4):913–937.

Dewenter, R. and Haucap, J. (2006). Incentives to licence virtual mobile network operators (MVNOs). In Dewenter, R. and Haucap, J., editors, *Access Pricing: Theory and Practice*. Elsevier, Amsterdam.

Dixit, V. V., Ortmann, A., Rutstrom, E., and Ukkusuri, S. (2015). Understanding transportation systems through the lenses of experimental economics: A review. Mimeo. Available at `http://ssrn.com/abstract=2546881`. Accessed January 29, 2016.

Dobson, P. W. and Waterson, M. (2008). Chain-store competition: Customized vs. uniform pricing. Mimeo. Available at `http://wrap.warwick.ac.uk/1375/1/WRAP_Dobson_twerp_840.pdf`. Accessed January 29, 2016.

Dolan, P. and Galizzi, M. M. (2014). Getting policy-makers to listen to field experiments. *Oxford Review of Economic Policy*, 30(4):725–752.

Dolbear, F. T., Lave, L. B., Bowman, G., Lieberman, A., Prescott, E., Rueter, F., and Sherman, R. (1968). Collusion in oligopoly: An experiment on the effect of numbers and information. *The Quarterly Journal of Economics*, 82(2):240–259.

Dormady, N. C. (2014). Carbon auctions, energy markets & market power: An experimental analysis. *Energy Economics*, 44:468–482.

Dorsey, R. E. (1992). The voluntary contributions mechanism with real time revisions. *Public Choice*, 73(3):261–282.

Dufwenberg, M. and Gneezy, U. (2000). Price competition and market concentration: an experimental study. *International Journal of Industrial Organization*, 18(1):7–22.

Dufwenberg, M. and Gneezy, U. (2002). Information disclosure in auctions: an experiment. *Journal of Economic Behavior & Organization*, 48(4):431–444.

Dufwenberg, M., Gneezy, U., Goeree, J. K., and Nagel, R. (2007). Price floors and competition. *Economic Theory*, 33(1):211–224.

Durham, Y., McCabe, K., Olson, M. A., Rassenti, S., and Smith, V. (2004). Oligopoly competition in fixed cost environments. *International Journal of Industrial Organization*, 22(2):147–162.

Economides, N. (1998). The incentive for non-price discrimination by an input monopolist. *International Journal of Industrial Organization*, 16(3):271–284.

Edelman, B. (2015). Does google leverage market power through tying and bundling? *Journal of Competition Law and Economics*, 11(2):365–400.

Edwards, C. D. (1955). Conglomerate bigness as a source of power. In *Business Concentration and Price Policy*, pages 331–359. Princeton University Press.

Ellerman, A. D. and Buchner, B. K. (2007). The European Union emissions trading scheme: Origins, allocation, and early results. *Review of environmental economics and policy*, 1(1):66–87.

Elliott, S. R., Godby, R., and Kruse, J. B. (2003). An experimental examination of vertical control and cost predation. *International Journal of Industrial Organization*, 21(2):253–281.

Emery, T. J., Tisdell, J. G., Green, B. S., Hartmann, K., Gardner, C., and León, R. (2015a). An experimental analysis of assignment problems and economic rent dissipation in quota managed fisheries. *Ocean & Coastal Management*, 106:10–28.

Emery, T. J., Tisdell, J. G., Green, B. S., Hartmann, K., Gardner, C., and León, R. (2015b). Experimental analysis of the use of fishery closures and cooperatives to reduce economic rent dissipation caused by assignment problems. *ICES Journal of Marine Science: Journal du Conseil*, pages 1–13.

Engel, C. (2007). How much collusion? a meta-analysis of oligopoly experiments. *Journal of Competition Law and Economics*, 3(4):491–549.

Engel, C. (2015). Tacit collusion: The neglected experimental evidence. *Journal of Empirical Legal Studies*, 12(3):537–577.

Engelmann, D. and Müller, W. (2011). Collusion through price ceilings? in search of a focal-point effect. *Journal of Economic Behavior & Organization*, 79(3):291–302.

Engelmann, D. and Normann, H.-T. (2009). Price ceilings as focal points? an experimental test. In Hinloopen, J. and Normann, H.-T., editors, *Experiments and Competition Policy*, chapter 4, pages 61–80. Cambridge University Press, Cambridge, UK.

European Commission (2009). Communication from the Commission – Guidance on the Commission's enforcement priorities in applying Article 82 of the EC Treaty to abusive exclusionary conduct by dominant undertakings (2009/C 45/02). *Official Journal of the European Union*, C 45:7–20.

European Commission (2012). Mergers: Commission clears acquisition of Austrian mobile phone operator Orange by H3G, subject to conditions. Available at `http://europa.eu/rapid/press-release_IP-12-1361_en.htm`. Accessed January 29, 2016.

European Commission (2013a). Commission recommendation on consistent non-discrimination obligations and costing methodologies to promote competition and enhance the broadband investment environment (2013/466/EU). *Official Journal of the European Union*, L 251:13–32.

European Commission (2013b). Proposal for a Regulation of the European Parliament and of the Council laying down measures concerning the European single market for electronic communications and to achieve a Connected Continent (COM(2013) 627)). Available at `http://eur-lex.europa.eu/legal-content/EN/TXT/?uri=COM:2013:0627:FIN`. Accessed January 29, 2016.

European Commission (2014a). Commission Staff Working Document. Implementation of the EU regulatory framework for electronic communications - 2014. SWD(2014) 249. Available at `http://ec.europa.eu/digital-agenda/en/download-scoreboard-reports`. Accessed January 29, 2016.

European Commission (2014b). Mergers: Commission clears acquisition of E-Plus by Telefónica Deutschland, subject to conditions. Available at `http://europa.eu/rapid/press-release_IP-14-771_en.htm`. Accessed January 29, 2016.

European Commission (2014c). Mergers: Commission clears acquisition of Telefónica Ireland by Hutchison 3G, subject to conditions. Available at http://europa.eu/rapid/press-release_IP-14-607_en.htm. Accessed January 29, 2016.

European Commission (2015a). Broadband coverage in Europe 2014: Mapping progress towards the coverage objectives of the digital agenda. Available at http://ec.europa.eu/newsroom/dae/document.cfm?action=display&doc_id=11195. Accessed January 29, 2016.

European Commission (2015b). Commission Staff Working Document: Implementation of the EU regulatory framework for electronic communication – 2015 (SWD(2015) 126). Available at http://ec.europa.eu/newsroom/dae/document.cfm?doc_id=9990. Accessed January 29, 2016.

European Commission (2015c). Communication from the Commission to the European Parliament, the Council, the European Economic and Social Committee and the Comittee of the Regions: A Digital Single Market Strategy for Europe (COM(2015) 192). Available at http://eur-lex.europa.eu/legal-content/EN/TXT/?uri=COM:2015:0192:FIN. Accessed January 29, 2016.

European Policy Center (2010). The economic impact of a european digital single market. Available at http://www.epc.eu/dsm/2/Study_by_Copenhagen.pdf. Accessed January 29, 2016.

Evans, W. N. and Kessides, I. N. (1994). Living by the "golden rule": Multimarket contact in the us airline industry. *The Quarterly Journal of Economics*, 109(2):341–366.

Ewing, B. T. and Kruse, J. B. (2010). An experimental examination of market concentration and capacity effects on price competition. *Journal of Business Valuation and Economic Loss Analysis*, 5(1):Article 3.

Falk, A. and Heckman, J. J. (2009). Lab experiments are a major source of knowledge in the social sciences. *Science*, 326(5952):535–538.

Farrell, J. and Katz, M. L. (2006). The economics of welfare standards in antitrust. *Competition Policy International*, 2(2):3–28.

Farrell, J. and Maskin, E. (1989a). Renegotiation in repeated games. *Games and Economic Behavior*, 1(4):327–360.

Farrell, J. and Maskin, E. (1989b). Renegotiation in repeated games. *Games and Economic Behavior*, 1(4):327–360.

Farrell, J. and Weiser, P. J. (2003). Modularity, vertical integration, and open access policies: Towards a convergence of antitrust and regulation in the internt age. *Harvard Journal of Law & Technology*, 17(1):85–134.

Federal Communications Commission (2011). WT Docket No. 11-65 – Staff analysis and findings. Available at `https://apps.fcc.gov/edocs_public/attachmatch/DA-11-1955A2.pdf`. Accessed January 29, 2016.

Federal Communications Commission (2014). Statement from FCC chairman Tom Wheeler on competition in the mobile marketplace. Available at `http://www.fcc.gov/document/chairman-wheeler-statement-competition-mobile-marketplace`. Accessed January 29, 2016.

Feeley, T. H., Tutzauer, F., Rosenfeld, H. L., and Young, M. J. (1997). Cooperation in an infinite-choice continuous-time prisoner's dilemma. *Simulation & Gaming*, 28(4):442–459.

Fehr, E. and Schmidt, K. M. (1999). A theory of fairness, competition, and cooperation. *The Quarterly Journal of Economics*, 114(3):817–868.

Feinberg, R. M. and Sherman, R. (1988). Mutual forbearance under experimental conditions. *Southern Economic Journal*, 54(4):985–993.

Fernandez, N. and Marin, P. L. (1998). Market power and multimarket contact: Some evidence from the spanish hotel industry. *The Journal of Industrial Economics*, 46(3):301–315.

Feuerstein, S. (2005). Collusion in industrial economics—a survey. *Journal of Industry, Competition and Trade*, 5(3–4):163–198.

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

Fonseca, M. A., Huck, S., and Normann, H.-T. (2005). Playing cournot although they shouldn't. *Economic Theory*, 25(3):669–677.

Fonseca, M. A. and Normann, H.-T. (2008). Mergers, asymmetries and collusion: Experimental evidence. *The Economic Journal*, 118(527):387–400.

Fonseca, M. A. and Normann, H.-T. (2012). Explicit vs. tacit collusion—the impact of communication in oligopoly experiments. *European Economic Review*, 56(8):1759–1772.

Fonseca, M. A. and Normann, H.-T. (2014). Endogenous cartel formation: Experimental evidence. *Economics Letters*, 125(2):223–225.

Fouraker, L. E. and Siegel, S. (1963). *Bargaining Behavior*. McGraw-Hill, New York.

Freeman, J. B. and Ambady, N. (2010). Mousetracker: Software for studying real-time mental processing using a computer mouse-tracking method. *Behavior Research Methods*, 42(1):226–241.

Friedman, D. and Huberman, B. (2005). Internet congestion: A laboratory experiment. In Rapoport, A. and Zwick, R., editors, *Experimental Business Research: Economic and Managerial Perspectives*, volume 2, chapter 4, pages 83–102. Springer, New York, NY.

Friedman, D., Huck, S., Oprea, R., and Weidenholzer, S. (2015). From imitation to collusion: Long-run learning in a low-information environment. *Journal of Economic Theory*, 155:185–205.

Friedman, D. and Oprea, R. (2012). A continuous dilemma. *American Economic Review*, 102(1):337–363.

Friedman, J. W. (1971). A non-cooperative equilibrium for supergames. *The Review of Economic Studies*, 38(1):1–12.

Friedman, J. W. and Hoggatt, A. C. (1980). *An Experiment in Non-cooperative Oligopoly*. JAI Press, Greenwhich, CT.

Friesen, L. and Gangadharan, L. (2013). Environmental markets: What do we learn from the lab? *Journal of Economic Surveys*, 27(3):515–535.

Fudenberg, D., Rand, D. G., and Dreber, A. (2012). Slow to anger and fast to forgive: Cooperation in an uncertain world. *American Economic Review*, 102(2):720–749.

Gans, J. S. (2007). Vertical contracting when competition for orders precedes procurement. *The Journal of Industrial Economics*, 55(2):325–346.

Garcıa-Gallego, A. and Georgantzıs, N. (2001). Multiproduct activity in an experimental differentiated oligopoly. *International Journal of Industrial Organization*, 19(3):493–518.

García-Gallego, A., Georgantzís, N., Hernán-González, R., and Kujal, P. (2012a). How do markets manage water resources? an experiment. *Environmental and Resource Economics*, 53(1):1–23.

García-Gallego, A., Georgantzís, N., Hernán-González, R., and Kujal, P. (2012b). Managing a duopolistic water market with confirmed proposals. an experiment. *Revista Internacional de Sociología*, 70(Extra 1):189–213.

Geradin, D. and O'Donoghue, R. (2005). The concurrent application of competition law and regulation: the case of margin squeeze abuses in the telecommunications sector. *Journal of Competition Law and Economics*, 1(2):355–425.

Geroski, P. A. (2003). Competition in markets and competition for markets. *Journal of Industry, Competition and Trade*, 3(3):151–166.

Ghemawat, P. and Thomas, C. (2008). Strategic interaction across countries and multinational agglomeration: An application to the cement industry. *Management Science*, 54(12):1980–1996.

Gimeno, J. and Woo, C. Y. (1996). Hypercompetition in a multimarket environment: The role of strategic similarity and multimarket contact in competitive de-escalation. *Organization Science*, 7(3):322–341.

Goeree, J. K. and Holt, C. A. (2010). Hierarchical package bidding: A paper & pencil combinatorial auction. *Games and Economic Behavior*, 70(1):146–169.

Gomez, R., Goeree, J. K., and Holt, C. A. (2008). Predatory pricing: Rare like a unicorn? In Plott, C. R. and Smith, V. L., editors, *Handbook of Experimental Economics Results*, volume 1, chapter 22, pages 178–184. North-Holland, Amsterdam.

Gonçalves, R. (2007). Cost orientation and xDSL services: Retail-minus vs. LRAIC. *Telecommunications Policy*, 31(8):524–529.

Goren, H., Kurzban, R., and Rapoport, A. (2003). Social loafing vs. social enhancement: Public goods provisioning in real-time with irrevocable commitments. *Organizational Behavior and Human Decision Processes*, 90(2):277–290.

Goren, H., Rapoport, A., and Kurzban, R. (2004). Revocable commitments to public goods provision under the real-time protocol of play. *Journal of Behavioral Decision Making*, 17(1):17–37.

Götte, L. and Schmutzler, A. (2009). Merger policy: what can we learn from experiments? In Hinloopen, J. and Normann, H.-T., editors, *Experiments and Competition Policy*, chapter 8, pages 185–216. Cambridge University Press, Cambridge, UK.

Greenlee, P., Reitman, D., and Sibley, D. S. (2011). Comment on Muris and Smith, "antitrust and bundled discounts: An experimental analysis". *Antitrust Law Journal*, 77(2):669–681.

Greiner, B. (2015). Subject pool recruitment procedures: organizing experiments with ORSEE. *Journal of the Economic Science Association*, 1(1):114–125.

Grether, D. M., Isaac, R. M., and Plott, C. R. (1981). The allocation of landing rights by unanimity among competitors. *The American Economic Review*, 71(2):166–171.

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.

Greve, H. R. (2008). Multimarket contact and sales growth: Evidence from insurance. *Strategic Management Journal*, 29(3):229–249.

Griliches, Z. (1986). Economic data issues. In Griliches, Z. and Intriligator, M. D., editors, *Handbook of Econometrics*, volume 3, chapter 25, pages 1465–1514. North-Holland, Amsterdam.

Grimm, V. and Ilieva, L. (2013). An experiment on emissions trading: the effect of different allocation mechanisms. *Journal of Regulatory Economics*, 44(3):308–338.

Grinstead, C. M. and Snell, J. L. (2012). *Introduction to probability*. American Mathematical Society, Providence, Rhode Island, USA, 2nd edition.

Güth, W., Häger, K., Kirchkamp, O., and Schwalbach, J. (2015). Testing forbearance experimentally: Duopolistic competition of conglomerate firms. *International Journal of the Economics of Business*, Articles in Advance:1–24.

Güth, W., Levati, M. V., and Stiehler, A. (2002). Privately contributing to public goods over time: An experimental study. Mimeo. Available at `http://hdl.handle.net/10419/65343`. Accessed January 29, 2016.

Häckner, J. (2000). A note on price and quantity competition in differentiated oligopolies. *Journal of Economic Theory*, 93(2):233–239.

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.

Hansen, K., Kaplan, J., and Kroll, S. (2014). Valuing options in water markets: A laboratory investigation. *Environmental and Resource Economics*, 57(1):59–80.

Harbord, R. M. and Higgins, J. P. T. (2008). Meta-regression in stata. *The Stata Journal*, 8(4):493–519.

Harrington, Joseph E., J. (1987). Collusion in multiproduct oligopoly games under a finite horizon. *International Economic Review*, 28(1):1–14.

Harrison, G. W. and List, J. A. (2004). Field experiments. *Journal of Economic literature*, 42(4):1009–1055.

Hart, O. and Tirole, J. (1990). Vertical integration and market foreclosure. *Brookings Papers on Economic Activity: Microeconomics*, pages 205–286.

Haucap, J. and Müller, H. C. (2013). The effects of gasoline price regulations: Experimental evidence. Mimeo. Available at `http://ssrn.com/abstract=2482288`. Accessed January 29, 2016.

Hausman, D. M. and Welch, B. (2010). Debate: To nudge or not to nudge. *Journal of Political Philosophy*, 18(1):123–136.

Haveman, H. A. and Nonnemaker, L. (2000). Competition in multiple geographic markets: The impact on growth and market entry. *Administrative Science Quarterly*, 45(2):232–267.

Hawkins, R. X. D. (2014). Conducting real-time multiplayer experiments on the web. *Behavior Research Methods*, pages 1–11.

Henze, B., Noussair, C., and Willems, B. (2012). Regulation of network infrastructure investments: an experimental evaluation. *Journal of Regulatory Economics*, 42(1):1–38.

Henze, B., Schuett, F., and Sluijs, J. P. (2015). Transparency in markets for experience goods: Experimental evidence. *Economic Inquiry*, 53(1):640–659.

Hesch, M. (2012). The effects of ringleader discrimination on cartel stability and deterrence – experimental insights. *Journal of Advanced Research in Law and Economics*, 3(1):26–42.

Higgins, J. and Thompson, S. G. (2002). Quantifying heterogeneity in a meta-analysis. *Statistics in medicine*, 21(11):1539–1558.

Hinloopen, J., Müller, W., and Normann, H.-T. (2014). Output commitment through product bundling: Experimental evidence. *European Economic Review*, 65:164–180.

Hinloopen, J. and Onderstal, S. (2014). Going once, going twice, reported! cartel activity and the effectiveness of antitrust policies in experimental auctions. *European Economic Review*, 70:317–336.

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

Hoenig, J. M. and Heisey, D. M. (2001). The abuse of power: The pervasive fallacy of power calculations for data analysis. *The American Statistician*, 55(1):19–24.

Höffler, F. (2007). Cost and benefits from infrastructure competition. Estimating welfare effects from broadband access competition. *Telecommunications Policy*, 31(6):401–418.

Höffler, F. (2008). On the consistent use of linear demand systems if not all varieties are available. *Economics Bulletin*, 4:1–5.

Höffler, F. and Schmidt, K. M. (2008). Two tales on resale. *International Journal of Industrial Organization*, 26(6):1448–1460.

Hoggatt, A. C., Friedman, J. W., and Gill, S. (1976). Price Signaling in Experimental Oligopoly. *The American Economic Review*, 66(2):261–266.

Holguín-Veras, J., Xu, N., de Jong, G., and Maurer, H. (2011). An experimental economics investigation of shipper-carrier interactions in the choice of mode and shipment size in freight transport. *Networks and Spatial Economics*, 11(3):509–532.

Holt, C. A. (1995). Industrial organization: A survey of laboratory research. In Kagel, J. H. and Roth, A. E., editors, *The Handbook of Experimental Economics*, chapter 5, pages 349–443. Princeton University Press, Princeton, NJ.

Holt, C. A. and Davis, D. D. (1990). The effects of non-binding price announcements on posted-offer markets. *Economics Letters*, 34(4):307–310.

Holt, C. A., Johnson, C. A., Mallow, C. A., and Sullivan, S. P. (2012). Water externalities: Tragedy of the common canal. *Southern Economic Journal*, 78(4):1142–1162.

Hong, J. T. and Plott, C. R. (1982). Rate filing policies for inland water transportation: an experimental approach. *The Bell Journal of Economics*, 13(1):1–19.

Horstmann, N. and Krämer, J. (2013). Price discrimination or uniform pricing: Which colludes more? *Economics Letters*, 120(3):379–383.

Horstmann, N. and Krämer, J. (2016). Tacit collusion of conglomerate firms through price signaling under multimarket contact. Mimeo. Available at `http://ssrn.com/abstract=2469452`. Accessed January 29, 2016.

Horstmann, N., Krämer, J., and Schnurr, D. (2015a). Number effects in oligopolies: How many competitors are enough to ensure competition? Mimeo. Available at `http://ssrn.com/abstract=2535862`. Accessed January 29, 2016.

Horstmann, N., Krämer, J., and Schnurr, D. (2015b). Oligopoly competition in continuous time. Mimeo. Available at http://ssrn.com/abstract=2630664. Accessed January 29, 2016.

Horstmann, N., Krämer, J., and Schnurr, D. (2016). Wholesale competition and open access regimes: Experimental evidence. Mimeo. Available at http://ssrn.com/abstract=2630660. Accessed January 29, 2016.

Hotelling, H. (1929). Stability in competition. *The Economic Journal*, 39(153):41–57.

Huck, S., Konrad, K. A., Müller, W., and Normann, H.-T. (2007). The merger paradox and why aspiration levels let it fail in the laboratory. *The Economic Journal*, 117(522):1073–1095.

Huck, S., Müller, W., and Normann, H.-T. (2001). Stackelberg beats cournot: On collusion and efficiency in experimental markets. *Economic Journal*, pages 749–765.

Huck, S., Normann, H.-T., and Oechssler, J. (1999). Learning in Cournot oligopoly–an experiment. *Economic Journal*, 109(454):80–95.

Huck, S., Normann, H.-T., and Oechssler, J. (2000). Does information about competitors' actions increase or decrease competition in experimental oligopoly markets? *International Journal of Industrial Organization*, 18(1):39–57.

Huck, S., Normann, H.-T., and Oechssler, J. (2004a). Through trial and error to collusion. *International Economic Review*, 45(1):205–224.

Huck, S., Normann, H.-T., and Oechssler, J. (2004b). Two are few and four are many: number effects in experimental oligopolies. *Journal of Economic Behavior & Organization*, 53(4):435–446.

Iftekhar, M. S. and Tisdell, J. G. (2014). Wildlife corridor market design: An experimental analysis of the impact of project selection criteria and bidding flexibility. *Ecological Economics*, 104:50–60.

Iftekhar, M. S. and Tisdell, J. G. (2015). Bidding and performance in multiple unit combinatorial fishery quota auctions: Role of information feedbacks. *Marine Policy*, 62:233–243.

Inderst, R. and Valletti, T. (2009). Price discrimination in input markets. *The RAND Journal of Economics*, 40(1):1–19.

Isacsson, G. and Nilsson, J.-E. (2003). An experimental comparison of track allocation mechanisms in the railway industry. *Journal of Transport Economics and Policy*, 37(3):353–381.

Ishii, K. and Kurzban, R. (2008). Public goods games in Japan. *Human Nature*, 19(2):138–156.

Italianer, A. (2015). Market opening: Regulation vs competition? Speech at the 48th Innsbruck Symposium, Innsbruck, Austria. Available at `http://ec.europa.eu/competition/speeches/text/sp2015_01_en.pdf`. Accessed January 29, 2016.

Ivaldi, M., Jullien, B., Rey, P., Seabright, P., and Tirole, J. (2003). The economics of tacit collusion. Report for DG Competition, European Commission.

Jans, I. and Rosenbaum, D. I. (1997). Multimarket contact and pricing: Evidence from the US cement industry. *International Journal of Industrial Organization*, 15(3):391–412.

Janssen, M. A. (2015). A behavioral perspective on the governance of common resources. *Current Opinion in Environmental Sustainability*, 12:1–5.

Janssen, M. A., Holahan, R., Lee, A., and Ostrom, E. (2010). Lab experiments for the study of social-ecological systems. *Science*, 328(5978):613–617.

Janssen, M. A., Lee, A., Waring, T. M., and Galafassi, D. (2014). Experimental platforms for behavioral experiments on social-ecological systems. *Ecology and Society*, 19(4):20.

Jaunaux, L. and Lebourges, M. (2015). Economic replicability tests for next-generation access networks. *Telecommunications Policy*, 39(6):486–501.

Joskow, P. L. and Noll, R. G. (1994). Deregulation and regulatory reform during the 1980s. In Feldstein, M., editor, *American Economic Policy in the 1980s*, chapter 6, pages 367–452. University of Chicago Press.

Jullien, B., Rey, P., and Saavedra, C. (2014). The economics of margin squeeze. Mimeo. Available at `http://idei.fr/doc/by/jullien/Margin_Squeeze_Policy_Paper_revised_March_2014.pdf`. Accessed January 29, 2016.

Jullien, C., Pignon, V., Robin, S., and Staropoli, C. (2012). Coordinating cross-border congestion management through auctions: An experimental approach to european solutions. *Energy Economics*, 34(1):1–13.

Kagel, J. H., Lien, Y., and Milgrom, P. (2010). Ascending prices and package bidding: A theoretical and experimental analysis. *American Economic Journal: Microeconomics*, 2(3):160–185.

Kagel, J. H., Lien, Y., and Milgrom, P. (2014). Ascending prices and package bidding: Further experimental analysis. *Games and Economic Behavior*, 85:210–231.

Kagel, J. H. and Roth, A. E., editors (1995). *The Handbook of Experimental Economics*. Princeton University Press, Princeton, NJ.

Kalmus, P. and Wiethaus, L. (2010). On the competitive effects of mobile virtual network operators. *Telecommunications Policy*, 34(5):262–269.

Kalnins, A. (2004). Divisional multimarket contact within and between multiunit organizations. *Academy of Management Journal*, 47(1):117–128.

Kang, W., Bayus, B. L., and Balasubramanian, S. (2010). The strategic effects of multimarket contact: Mutual forbearance and competitive response in the personal computer industry. *Journal of Marketing Research*, 47(3):415–427.

Kaplow, L. (2010). Why (ever) define markets? *Harvard Law Review*, 124(2):437–517.

Kaskiris, C., Butun, Y., and Jain, R. (2006). An experimental analysis of a combinatorial market mechanism for bandwidth trading. In *Proceedings of the 1st IEEE International Workshop on Bandwidth on Demand*, pages 19–26. IEEE.

Kaskiris, C., Jain, R., Rajagopal, R., and Varaiya, P. (2007). Combinatorial auction bandwidth trading: An experimental study. In Oda, S. H., editor, *Developments on Experimental Economics: New Approaches to Solving Real-world Problems*, volume 590 of *Lecture Notes in Economics and Mathematical Systems*, pages 181–186. Springer, Berlin, Germany.

Kench, B. T. (2004). Let's get physical! or financial? a study of electricity transmission rights. *Journal of Regulatory Economics*, 25(2):187–214.

Kephart, C. and Friedman, D. (2015a). Hotelling revisits the lab: equilibration in continuous and discrete time. *Journal of the Economic Science Association*, pages 1–14.

Kephart, C. and Friedman, D. (2015b). Hotelling revisits the lab: equilibration in continuous and discrete time. *Journal of the Economic Science Association*, 1(2):132–145.

Kephart, C. and Rose, L. (2015). Stability in competition? Hotelling in continuous time. Mimeo. Available at `http://www.cazaar.com/home/research/KephartRose_Hotelling_PL.pdf`. Accessed January 29, 2016.

Ketcham, J., Smith, V. L., and Williams, A. W. (1984). A comparison of posted-offer and double-auction pricing institutions. *The Review of Economic Studies*, 51(4):595–614.

Kiesling, L. (2005). Using economic experiments to test electricity policy. *The Electricity Journal*, 18(9):43–50.

Kiesling, L. and Wilson, B. J. (2007). An experimental analysis of the effects of automated mitigation procedures on investment and prices in wholesale electricity markets. *Journal of Regulatory Economics*, 31(3):313–334.

Klumpp, T. and Su, X. (2010). Open access and dynamic efficiency. *American Economic Journal: Microeconomics*, 2(2):64–96.

Klumpp, T. and Su, X. (2015). Strategic investment under open access: Theory and evidence. *The Journal of Industrial Economics*, 63(3):495–521.

Knigge, A. and Buskens, V. (2010). Coordination and cooperation problems in network good production. *Games*, 1(4):357–380.

Koçak, Ö. and Özcan, S. (2013). How does rivals' presence affect firms' decision to enter new markets? economic and sociological explanations. *Management Science*, 59(11):2586–2603.

Koenker, R. and Hallock, K. (2001). Quantile regression: An introduction. *Journal of Economic Perspectives*, 15(4):43–56.

Kousha, K. and Thelwall, M. (2007). Google scholar citations and google web/url citations: A multi-discipline exploratory analysis. *Journal of the American Society for Information Science and Technology*, 58(7):1055–1065.

Krämer, J. and Schnurr, D. (2014). A unified framework for open access regulation of telecommunications infrastructure: Review of the economic literature and policy guidelines. *Telecommunications Policy*, 38(11):1160–1179.

Krämer, J. and Vogelsang, I. (2014). Co-investments and tacit collusion in regulated network industries: Experimental evidence. Mimeo. Available at `http://ssrn.com/abstract=2119927`. Accessed January 29, 2016.

Kübler, D. and Müller, W. (2002). Simultaneous and sequential price competition in heterogeneous duopoly markets: Experimental evidence. *International Journal of Industrial Organization*, 20(10):1437–1460.

Kühn, K.-U. and Vives, X. (1995). *Information exchanges among firms and their impact on competition*. Office for Official Publications of the European Communities, Luxembourg.

Kurzban, R., McCabe, K., Smith, V. L., and Wilson, B. J. (2001). Incremental commitment and reciprocity in a real-time public goods game. *Personality and Social Psychology Bulletin*, 27(12):1662–1673.

Lafontaine, F. and Slade, M. (2007). Vertical integration and firm boundaries: The evidence. *Journal of Economic Literature*, 45(3):629–685.

Landeo, C. M. and Spier, K. E. (2009). Naked exclusion: An experimental study of contracts with externalities. *The American Economic Review*, 99(5):1850–1877.

Landeo, C. M. and Spier, K. E. (2012). Exclusive dealing and market foreclosure: Further experimental results. *Journal of Institutional and Theoretical Economics*, 168(1):150–170.

Le Coq, C. and Orzen, H. (2012). Price caps and fluctuating demands in electricity markets: Experimental evidence of competitive bidding. In *Proceedings of the 9th International Conference on the European Energy Market*, pages 1–4. IEEE.

Ledyard, J., Porter, D., and Wessen, R. (2000). A market-based mechanism for allocating space shuttle secondary payload priority. *Experimental Economics*, 2(3):173–195.

Ledyard, J. O., Porter, D., and Rangel, A. (1994). Using computerized exchange systems to solve an allocation problem in project management. *Journal of Organizational Computing and Electronic Commerce*, 4(3):271–296.

Lefebvre, M., Gangadharan, L., and Thoyer, S. (2012). Do security-differentiated water rights improve the performance of water markets? *American Journal of Agricultural Economics*, 94(5):1113–1135.

Levati, M. V. and Neugebauer, T. (2004). An application of the English clock market mechanism to public goods games. *Experimental Economics*, 7(2):153–169.

Lindqvist, T. and Stennek, J. (2005). The insiders' dilemma: An experiment on merger formation. *Experimental Economics*, 8(3):267–284.

Loewenstein, G. (1999). Experimental economics from the vantage-point of behavioural economics. *The Economic Journal*, 109(453):25–34.

Lunander, A. and Nilsson, J.-E. (2004). Taking the lab to the field: Experimental tests of alternative mechanisms to procure multiple contracts. *Journal of Regulatory Economics*, 25(1):39–58.

Lunn, P. (2014). *Regulatory Policy and Behavioural Economics*. OECD Publishing, Paris, France.

Maart-Noelck, S. C., Musshoff, O., and Maack, M. (2013). The impact of price floors on farmland investments: a real options based experimental analysis. *Applied Economics*, 45(35):4872–4882.

Mandy, D. and Sappington, D. (2007). Incentives for sabotage in vertically related industries. *Journal of Regulatory Economics*, 31(3):235–260.

Mandy, D. M. (2000). Killing the goose that may have laid the golden egg: Only the data know whether sabotage pays. *Journal of Regulatory Economics*, 17(2):157–172.

Martin, S., Normann, H.-T., and Snyder, C. M. (2001). Vertical foreclosure in experimental markets. *RAND Journal of Economics*, 32(3):466–496.

Mas-Colell, A., Whinston, M. D., and Green, J. R. (1995). *Microeconomic Theory*. Oxford University Press, New York, NY.

Maskin, E. and Tirole, J. (1988a). A theory of dynamic oligopoly, I: Overview and quantity competition with large fixed costs. *Econometrica*, 56(3):549–569.

Maskin, E. and Tirole, J. (1988b). A theory of dynamic oligopoly, II: Price competition, kinked demand curves, and edgeworth cycles. *Econometrica*, 56(3):571–599.

Matsushima, H. (2001). Multimarket contact, imperfect monitoring, and implicit collusion. *Journal of Economic Theory*, 98(1):158–178.

McCabe, K. A., Rassenti, S. J., and Smith, V. L. (1989). Designing 'smart' computer-assisted markets: An experimental auction for gas networks. *European Journal of Political Economy*, 5(2):259–283.

McCabe, K. A., Rassenti, S. J., and Smith, V. L. (1990). Auction design for composite goods: The natural gas industry. *Journal of Economic Behavior & Organization*, 14(1):127–149.

McCabe, K. A., Rassenti, S. J., and Smith, V. L. (1991). Smart computer-assisted markets. *Science*, 254(5031):534–538.

McEvoy, D. M., Cherry, T. L., and Stranlund, J. K. (2015). Endogenous minimum participation in international environmental agreements: An experimental analysis. *Environmental and Resource Economics*, pages 1–16.

McSmith, A. (2010). First obama, now cameron embraces 'nudge theory'. *The Independent*. Available at `http://www.independent.co.uk/news/uk/politics/first-obama-now-cameron-embraces-nudge-theory-2050127.html`. Accessed January 29, 2016.

Meho, L. I. and Yang, K. (2007). Impact of data sources on citation counts and rankings of LIS faculty: Web of Science versus Scopus and Google Scholar. *Journal of the American Society for Information Science and Technology*, 58(13):2105–2125.

Millner, E. L., Pratt, M. D., and Reilly, R. J. (1990). Contestability in real-time experimental flow markets. *RAND Journal of Economics*, 21(4):584–599.

Moeller, S. and Brady, C. (2014). *Intelligent M & A: Navigating the mergers and acquisitions minefield*. John Wiley & Sons, Chichester, UK.

Mougeot, M., Naegelen, F., Pelloux, B., and Rullière, J.-L. (2011). Breaking collusion in auctions through speculation: An experiment on CO_2 emission permit markets. *Journal of Public Economic Theory*, 13(5):829–856.

Mount, T. D. and Maneevitjit, S. (2008). Testing the performance of a forward capacity market with barriers to entry. In *Proceedings of the 41st Hawaii International Conference on System Sciences*, pages 175–184, Big Island, Hawaii.

Mount, T. D., Thomas, R. J., Vossler, C. A., and Zimmerman, R. D. (2002). Experimental evidence about the persistence of high prices in a soft-cap auction for electricity. In *Proceedings of the 25th Annual IAEE International Conference*, pages 1–10, Aberdeen, UK.

Müller, W. (2006). Allowing for two production periods in the cournot duopoly: experimental evidence. *Journal of Economic Behavior & Organization*, 60(1):100–111.

Müller, W. and Normann, H.-T. (2014). Experimental economics in antitrust. In Blair, R. D. and Sokol, D. D., editors, *The Oxford Handbook of International Antitrust Economics*, volume 1, chapter 10, pages 229–253. Oxford University Press, New York, NY.

Muris, T. J., Smith, V. J., Caliskan, A., and Kobayashi, B. H. (2011). Antitrust and bundled discounts: An experimental analysis – a reply. *Antitrust Law Journal*, 77(2):683–690.

Muris, T. J. and Smith, V. L. (2008). Antitrust and bundled discounts: An experimental analysis. *Antitrust Law Journal*, 75(2):399–432.

Murphy, R. O., Ackermann, K. A., and Handgraaf, M. (2011). Measuring social value orientation. *Judgment and Decision Making*, 6(8):771–781.

Murphy, R. O., Rapoport, A., and Parco, J. E. (2006). The breakdown of cooperation in iterative real-time trust dilemmas. *Experimental Economics*, 9(2):147–166.

Myrdal, G. (1953). *The Political Element in the Development of Economic Theory*. Routledge & Kegan Paul, London.

Nicolaisen, J., Petrov, V., and Tesfatsion, L. (2001). Market power and efficiency in a computational electricity market with discriminatory double-auction pricing. *IEEE Transactions on Evolutionary Computation*, 5(5):504–523.

Nilsson, J.-E. (1999). Allocation of track capacity: Experimental evidence on the use of priority auctioning in the railway industry. *International Journal of Industrial Organization*, 17(8):1139–1162.

Nocke, V. and White, L. (2007). Do vertical mergers facilitate upstream collusion? *American Economic Review*, 97(4):1321–1339.

Normann, H.-T. (2008). Experimental economics in antitrust. In Collins, W. D., editor, *Issues in Competition Law and Policy*, volume 1, pages 773–801. American Bar Association.

Normann, H.-T. (2009). Vertical integration, raising rivals' costs and upstream collusion. *European Economic Review*, 53(4):461–480.

Normann, H.-T. (2011). Vertical mergers, foreclosure and raising rivals'costs – experimental evidence. *The Journal of Industrial Economics*, 59(3):506–527.

Normann, H.-T. and Ricciuti, R. (2009). Laboratory experiments for economic policy making. *Journal of Economic Surveys*, 23(3):407–432.

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*, 41(3):707–718.

Noussair, C. N. and van Soest, D. P. (2014). Economic experiments and environmental policy. *Annual Review of Resource Economics*, 6(1):319–337.

Ofcom (2014). Review of the wholesale broadband access markets. Available at `http://stakeholders.ofcom.org.uk/binaries/consultations/review-wba-markets/statement/WBA-Statement.pdf`. Accessed January 29, 2016.

Offerman, T. and Potters, J. (2006). Does auctioning of entry licences induce collusion? an experimental study. *The Review of Economic Studies*, 73(3):769–791.

Offerman, T., Potters, J., and Sonnemans, J. (2002). Imitation and belief learning in an oligopoly experiment. *The Review of Economic Studies*, 69(4):973–997.

Oprea, R., Charness, G., and Friedman, D. (2014). Continuous time and communication in a public-goods experiment. *Journal of Economic Behavior & Organization*, 108:212–223.

Ordover, J. and Shaffer, G. (2007). Wholesale access in multi-firm markets: When is it profitable to supply a competitor? *International Journal of Industrial Organization*, 25(5):1026–1045.

Ordover, J. A., Saloner, G., Salop, S. C., et al. (1990). Equilibrium vertical foreclosure. *American Economic Review*, 80(1):127–42.

Orzen, H. (2008). Counterintuitive number effects in experimental oligopolies. *Experimental Economics*, 11(4):390–401.

O'Sullivan, A. and Sheffrin, S. M. (2003). *Economics: Principles in action*. Prentice Hall, Upper Saddle River, NJ.

Parcu, P. L. and Silvestri, V. (2014). Electronic communications regulation in europe: An overview of past and future problems. *Utilities Policy*, 31:246–255.

Parente, P. M. and Santos Silva, J. M. (2015). Quantile regression with clustered data. *Journal of Econometric Methods*, 5(1):1–15.

Parker, P. M. and Röller, L.-H. (1997). Collusive conduct in duopolies: multimarket contact and cross-ownership in the mobile telephone industry. *RAND Journal of Economics*, 28(2):304–322.

Parkhurst, G. M., Shogren, J. F., and Crocker, T. (2015). Tradable set-aside requirements (TSARs): Conserving spatially dependent environmental amenities. *Environmental and Resource Economics*, pages 1–26.

Pelkmans, J. and Renda, A. (2011). Single ecomms market? no such thing.... *Communications & Strategies*, 82:21–42.

Perkis, D. F., Cason, T. N., and Tyner, W. E. (2015). An experimental investigation of hard and soft price ceilings in emissions permit markets. *Environmental and Resource Economics*, pages 1–16.

Pettit, J., Friedman, D., Kephart, C., and Oprea, R. (2014). Software for continuous game experiments. *Experimental Economics*, 17(4):631–648.

Petulowa, M. and Saavedra, C. (2014). Margin squeeze in a regulatory environment: an application to differentiated product markets. Mimeo. Available at `http://ssrn.com/abstract=2236258`. Accessed January 29, 2016.

Phillips, O. R. and Mason, C. F. (1992). Mutual forbearance in experimental conglomerate markets. *RAND Journal of Economics*, 23(3):395–414.

Phillips, O. R. and Mason, C. F. (1996). Market regulation and multimarket rivalry. *RAND Journal of Economics*, 26(3):596–617.

Phillips, O. R. and Mason, C. F. (2001). Collusion in horizontally connected markets: Multimarket producers as conduits for learning. In Baum, J. A. C. and Greve, H. R., editors, *Multiunit Organization and Multimarket Strategy*, volume 18 of *Advances in Strategic Management*, chapter 8, pages 205–227. Elsevier, Oxford.

Pittman, R. (2005). Structural separation to create competition? the case of freight railways. *Review of Network Economics*, 4(3):181–196.

Plickert, P. and Beck, H. (2014). Kanzlerin sucht Verhaltensforscher. *Frankfurter Allgemeine Zeitung*. Available at `http://www.faz.net/aktuell/wirtschaft/wirtschaftspolitik/kanzlerin-angela-merkel-sucht-verhaltensforscher-13118345.html`. Accessed January 29, 2016.

Plott, C. R. (1982). Industrial organization theory and experimental economics. *journal of Economic Literature*, 20(4):1485–1527.

Plott, C. R. (1987). Dimensions of parallelism: Some policy applications of experimental methods. In Roth, A. E., editor, *Laboratory experimentation in economics: Six points of view*, chapter 7, pages 193–219. Cambridge University Press, New York, NY.

Plott, C. R. (1989). An updated review of industrial organization: Applications of experimental methods. In Schmalensee, R. and Willig, R., editors, *Handbook of Industrial Organization*, volume 2 of *Handbooks in Economics*, chapter 19. Elsevier.

Plott, C. R. and Porter, D. P. (1996). Market architectures and institutional testbedding: An experiment with space station pricing policies. *Journal of Economic Behavior & Organization*, 31(2):237–272.

Plott, C. R. and Salmon, T. C. (2004). The simultaneous, ascending auction: dynamics of price adjustment in experiments and in the uk3g spectrum auction. *Journal of Economic Behavior & Organization*, 53(3):353–383.

Porter, M. E. (1980). *Strategic interaction: Some lessons from industry histories for theory and antitrust policy*. Division of Research, Graduate School of Business Administration, Harvard University.

Potters, J., Rockenbach, B., Sadrieh, A., and Van Damme, E. (2004). Collusion under yardstick competition: an experimental study. *International Journal of Industrial Orga-*

nization, 22(7):1017–1038.

Potters, J. and Suetens, S. (2013). Oligopoly experiments in the current millennium. *Journal of Economic Surveys*, 27(3):439–460.

Radner, R. (1986). Can bounded rationality resolve the prisoner's dilemma? In Mas-Colell, A. and Hildenbrand, W., editors, *Contributions to Mathematical Economics*, pages 387–399. North-Holland, Amsterdam.

Rassenti, S., Smith, V., and McCabe, K. (1994). Designing a real time computer assisted auction for natural gas networks. In Cooper, W. and Whinston, A., editors, *New Directions in Computational Economics*, volume 4 of *Advances in Computational Economics*, pages 41–54. Kluwer Academic Publishers, Dordrecht, Netherlands.

Rassenti, S. J. and Smith, V. L. (1998). Deregulating electric power: Market design issues and experiments. In Chao, H.-p. and Huntington, H. G., editors, *Designing Competitive Electricity Markets*, volume 13 of *International Series in Operations Research & Management Science*, pages 105–120. Kluwer Academic Publishing, New York, NY.

Rassenti, S. J., Smith, V. L., and Bulfin, R. L. (1982). A combinatorial auction mechanism for airport time slot allocation. *The Bell Journal of Economics*, 13(2):402–417.

Rassenti, S. J., Smith, V. L., and Wilson, B. J. (2003a). Controlling market power and price spikes in electricity networks: Demand-side bidding. *Proceedings of the National Academy of Sciences*, 100(5):2998–3003.

Rassenti, S. J., Smith, V. L., and Wilson, B. J. (2003b). Discriminatory price auctions in electricity markets: Low volatility at the expense of high price levels. *Journal of Regulatory Economics*, 23(2):109–123.

Renda, A. (2010). Competition–regulation interface in telecommunications: What's left of the essential facility doctrine. *Telecommunications Policy*, 34(1):23–35.

Rey, P. and Tirole, J. (2007). A primer on foreclosure. In Armstrong, M. and Porter, R. H., editors, *Handbook of Industrial Organization: Vol 3*, pages 2145–2220. North-Holland, Amsterdam.

Ricciuti, R. (2008). Bringing macroeconomics into the lab. *Journal of Macroeconomics*, 30(1):216–237.

Roth, A. E. (1995). Bargaining experiments. In Kagel, J. H. and Roth, A. E., editors, *The handbook of experimental economics*, pages 253–348. Princeton University Press, Princeton, New Jersey.

Roth, A. E., Murnighan, J. K., and Schoumaker, F. (1988). The deadline effect in bargaining: Some experimental evidence. *American Economic Review*, 78(4):806–823.

Salop, S. C. (1979). Monopolistic competition with outside goods. *The Bell Journal of Economics*, 10(1):141–156.

Salop, S. C. and Scheffman, D. T. (1983). Raising rivals' costs. *American Economic Review*, 73(2):267–271.

Salop, S. C. and Scheffman, D. T. (1987). Cost-raising strategies. *The Journal of Industrial Economics*, 36(1):19–34.

Šauer, P., Fiala, P., Dvořák, A., Kolínský, O., Prášek, J., Ferbar, P., and Rederer, L. (2015). Improving quality of surface waters with coalition projects and environmental subsidy negotiation. *Polish Journal of Environmental Studies*, 24(3):1299–1307.

Scheffel, T., Ziegler, G., and Bichler, M. (2012). On the impact of package selection in combinatorial auctions: an experimental study in the context of spectrum auction design. *Experimental Economics*, 15(4):667–692.

Schram, A. (2005). Artificiality: The tension between internal and external validity in economic experiments. *Journal of Economic Methodology*, 12(2):225–237.

Scott, J. T. (1982). Multimarket contact and economic performance. *The Review of Economics and Statistics*, 64(3):368–375.

Scott, J. T. (2008). Multimarket contacts. In Collins, W. D., editor, *Issues in Competition Law and Policy*, volume 2, chapter 63, pages 1553–1573. American Bar Association, Chicago.

Seifert, S. and Ehrhart, K.-M. (2005). Design of the 3g spectrum auctions in the UK and Germany: An experimental investigation. *German Economic Review*, 6(2):229–248.

Selten, R. (1975). Reexamination of the perfectness concept for equilibrium points in extensive games. *International Journal of Game Theory*, 4(1):25–55.

Selten, R., Mitzkewitz, M., and Uhlich, G. R. (1997). Duopoly strategies programmed by experienced players. *Econometrica*, 65(3):517–555.

Shankar, M. (2015). Using behavioral science insights to make government more effective, simpler, and more people-friendly. *The White House Blog*. Available at `https://www.whitehouse.gov/blog/2015/02/09/behavioral-science-insights-make-government-more-effective-simpler-and-more-user-fri`. Accessed January 29, 2016.

Shapiro, C. (1996). Mergers with differentiated products. *Antitrust*, 10(2):23–30.

Shawhan, D. L., Messer, K. D., Schulze, W. D., and Schuler, R. E. (2011). An experimental test of automatic mitigation of wholesale electricity prices. *International Journal of Industrial Organization*, 29(1):46–53.

Shleifer, A. (1985). A theory of yardstick competition. *The RAND Journal of Economics*, 16(3):319–327.

Shobe, W., Holt, C., and Huetteman, T. (2014). Elements of emission market design: An experimental analysis of California's market for greenhouse gas allowances. *Journal of Economic Behavior & Organization*, 107:402–420.

Shubik, M. and Levitan, R. (1980). *Market Structure and Behavior*. Harvard University Press.

Siakantaris, N. (2000). Experimental economics under the microscope. *Cambridge Journal of Economics*, 24(3):267–281.

Simon, L. K. and Stinchcombe, M. B. (1989). Extensive form games in continuous time: Pure strategies. *Econometrica*, 57(5):1171–1214.

Singh, N. and Vives, X. (1984). Price and quantity competition in a differentiated duopoly. *RAND Journal of Economics*, 15(4):546–554.

Sluijs, J. P., Schuett, F., and Henze, B. (2011). Transparency regulation in broadband markets: Lessons from experimental research. *Telecommunications Policy*, 35(7):592–602.

Smith, A. M. (2011). An experimental study of exclusive contracts. *International Journal of Industrial Organization*, 29(1):4–13.

Smith, V. L. (1982). Microeconomic systems as an experimental science. *American Economic Review*, 72(5):923–955.

Smith, V. L. (2002). Method in experiment: Rhetoric and reality. *Experimental economics*, 5(2):91–110.

Spagnolo, G. (1999). On interdependent supergames: multimarket contact, concavity, and collusion. *Journal of Economic Theory*, 89(1):127–139.

Spence, A. M. (1974). *Market Signaling: Informational Transfer in Hiring and Related Screening Processes*. Harvard University Press, Cambridge, MA.

Srinivasan, K. (1991). Multiple market entry, cost signalling and entry deterrence. *Management Science*, 37(12):1539–1555.

Staropoli, C. and Jullien, C. (2006). Using laboratory experiments to design efficient market institutions: The case of wholesale electricity markets. *Annals of Public and Cooperative Economics*, 77(4):555–577.

Stranlund, J. K., Murphy, J. J., and Spraggon, J. M. (2011). An experimental analysis of compliance in dynamic emissions markets. *Journal of Environmental Economics and Management*, 62(3):414–429.

Stranlund, J. K., Murphy, J. J., and Spraggon, J. M. (2014). Price controls and banking in emissions trading: An experimental evaluation. *Journal of Environmental Economics and Management*, 68(1):71–86.

Suetens, S. and Potters, J. (2007). Bertrand colludes more than Cournot. *Experimental Economics*, 10(1):71–77.

Surowiecki, J. (2010). The regulation crisis. *The New Yorker*. Available at `http://www.newyorker.com/magazine/2010/06/14/the-regulation-crisis`. Accessed January 29, 2016.

Tanaka, K., Higashida, K., and Managi, S. (2014). A laboratory assessment of the choice of vessel size under individual transferable quota regimes. *Australian Journal of Agricultural and Resource Economics*, 58(3):353–373.

Taschini, L., Chesney, M., and Wang, M. (2014). Experimental comparison between markets on dynamic permit trading and investment in irreversible abatement with and without non-regulated companies. *Journal of Regulatory Economics*, 46(1):23–50.

Thaler, R. H. and Sunstein, C. R. (2008). *Nudge: Improving Decisions About Health, Wealth, and Happiness*. Yale University Press, New Haven, CT.

Thomas, R. J., Mount, T. D., Zimmerman, R. D., Schulze, W. D., Schuler, R. E., and Chapman, L. D. (2002). Testing the effects of price responsive demand on uniform price and soft-cap electricity auctions. In *Proceedings of the 35th Hawaii International Conference on System Sciences*, pages 757–765, Big Island, Hawaii.

Tirole, J. (1988). *The Theory of Industrial Organization*. MIT Press, Cambridge, MA.

Tisdell, J. G. (2011). Water markets in australia: an experimental analysis of alternative market mechanisms. *Australian Journal of Agricultural and Resource Economics*, 55(4):500–517.

Tisdell, J. G. and Iftekhar, M. S. (2013). Fisheries quota allocation: Laboratory experiments on simultaneous and combinatorial auctions. *Marine Policy*, 38:228–234.

Tremblay, V. J. and Tremblay, C. H. (2012). *New Perspectives on Industrial Organization: With Contributions from Behavioral Economics and Game Theory*, volume 12 of *Springer Texts in Business and Economics*. Springer, New York, NY.

van Gorp, N., Canoy, M., Canton, E., Meindert, L., Volkerink, B., Lemstra, W., Nooren, P., and Stokking, H. (2011). Steps towards a truly internal market for e-communications. Report by Ecorys, TUDelft, and TNO for the European Commission. Available at `http://ec.europa.eu/newsroom/dae/document.cfm?doc_id=2648`. Accessed January 29, 2016.

van Koten, S. and Ortmann, A. (2013). Structural versus behavioral remedies in the deregulation of electricity markets: An experimental investigation motivated by policy concerns. *European Economic Review*, 64:256–265.

Vestager, M. (2015). Competition in telecom markets. Speech at the 42nd Annual Conference on International Antitrust Law and Policy, New York City, NY. Available at `https://ec.europa.eu/commission/2014-2019/vestager/announcements/competition-telecom-markets_en`. Accessed January 29, 2016.

Vickers, J. (1995). Competition and regulation in vertically related markets. *The Review of Economic Studies*, 62(1):1–17.

Vogelsang, I. (2013). The endgame of telecommunications policy? A survey. *Review of Economics*, 64(3):193–270.

Vogt, N. (2015). Environmental risk negatively impacts trust and reciprocity in conservation contracts: Evidence from a laboratory experiment. *Environmental and Resource Economics*, pages 1–15.

Vogt, N., Reeson, A. F., and Bizer, K. (2013). Communication, competition and social gift exchange in an auction for public good provision. *Ecological Economics*, 93:11–19.

von Stackelberg, H. (1934). *Marktform und Gleichgewicht*. Verlag von Julius Springer, Vienna, Austria.

Vossler, C. A., Mount, T. D., Thomas, R. J., and Zimmerman, R. D. (2009). An experimental investigation of soft price caps in uniform price auction markets for wholesale electricity. *Journal of Regulatory Economics*, 36(1):44–59.

Vossler, C. A., Suter, J. F., and Poe, G. L. (2013). Experimental evidence on dynamic pollution tax policies. *Journal of Economic Behavior & Organization*, 93:101–115.

Waichman, I., Requate, T., and Siang, C. K. (2014). Communication in cournot competition: An experimental study. *Journal of Economic Psychology*, 42:1–16.

Waldfogel, J. and Wulf, J. (2006). Measuring the effect of multimarket contact on competition: Evidence from mergers following radio broadcast ownership deregulation. *Contributions in Economic Analysis & Policy*, 5(1):1–23.

Walle, S. V. and Wambach, J. (2014). No magic number to dial – the commission's review of mobile telecom mergers. *Competition Merger Brief*, 1/2014:10–16.

Waltman, L. and Kaymak, U. (2008). Q-learning agents in a Cournot oligopoly model. *Journal of Economic Dynamics and Control*, 32(10):3275–3293.

Webster, J. and Watson, R. T. (2002). Analyzing the past to prepare for the future: Writing a literature review. *Management Information Systems Quarterly*, 26(2):xiii–xxiii.

Weisman, D. L. and Kang, J. (2001). Incentives for discrimination when upstream monopolists participate in downstream markets. *Journal of Regulatory Economics*, 20(2):125–139.

Weiss, J. (2002). Market power and power markets. *Interfaces*, 32(5):37–46.

Weißensteiner, N. (2015). Karmasin vertraut jetzt auf Verhaltensökonomie. *derStandard.at*. Available at `http://derstandard.at/2000021988684/Karmasin-vertraut-jetzt-auf-Verhaltensoekonomie`. Accessed January 29, 2016.

Whinston, M. D. (1990). Tying, foreclosure, and exclusion. *American Economic Review*, 80(4):837–859.

Wilson, B. J. (2007). Experimental economics and antitrust: What can we learn from laboratory markets. *Antitrust*, 21(2):54–58.

Wilson, B. J. (2011). Using experimental economics to understand competition. In Drexl, J., Kerber, W., and Podszun, R., editors, *Competition Policy And The Economic Approach: Foundations and Limitations*, chapter 12, pages 230–244. Edward Elgar Publishing, Cheltenham, UK.

Yang, J., Kawamura, T., and Ogawa, K. (2015). Experimental multimarket contact inhibits cooperation. *Metroeconomica*, 67(1):21–43.

Young, G., Smith, K. G., Grimm, C. M., and Simon, D. (2000). Multimarket contact and resource dissimilarity: A competitive dynamics perspective. *Journal of Management*, 26(6):1217–1236.

Yu, T. and Cannella, A. A. (2013). A comprehensive review of multimarket competition research. *Journal of Management*, 39(1):76–109.

Zetland, D. (2013). All-in-auctions for water. *Journal of environmental management*, 115:78–86.

Zimmerman, R. D. and Thomas, R. J. (2004). PowerWeb: A tool for evaluating economic and reliability impacts of electric power market designs. In *Power Systems Conference and Exposition*, pages 1562–1567. IEEE.

Zimmerman, R. D., Thomas, R. J., Gan, D., and Murillo-Sánchez, C. E. (1999). A web-based platform for experimental investigation of electric power auctions. *Decision Support Systems*, 24(3):193–205.

List of Abbreviations

ANOVA	analysis of variance	168
DSL	digital subscriber line	6
DSM	digital single market	4
EC	European Commission	4
EU	European Union	2
IO	industrial organization	16
JPM	joint profit maximization	2
MMC	multimarket contact	160
MSR	margin squeeze regulation	195
MU	monetary units	160
NR	no regulation	195
OLS	ordinary least squares	170
PD	price discrimination	140
PMC	partial multimarket contact	161
pp	percentage points	90

SMC	single market contact	160
UK	United Kingdom	15
UP	uniform pricing	140
US	United States	2
WC	wholesale competition	195
WCPC	wholesale competition with price commitment	195
WM	wholesale monopoly	195

List of Figures

List of Tables