Collusion and Competition in Oligopolistic Markets

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Chapter 1

General Introduction

The benefits of competition for consumers are large and diverse. Competition usually leads to low prices, a high level of service, high product quality, product variety, and innovation (Tirole, 1988). It puts firms in a position, where they have to excel in order to win consumers, since otherwise, consumers will satisfy their needs elsewhere. Therefore, to maximize profits, firms in a competitive market try to produce the best product they can, provide a variety of products and improve processes (European Commission, 2020; Federal Trade Commission, 2020). Oligopolistic markets offer favorable conditions for vivid competition due to the number of firms in the market (compare Motta, 2004, p.51).

Yet, firms usually earn higher profits in a less competitive environment, which incentivizes them to restrict competition. In markets with repeated interaction, firms can avoid competition by colluding on parameters such as sales prices, quantities, production capacities, or investments in research and development. Collusion can either be explicit, via some sort of communication, or tacit, meaning firms implicitly reach an understanding that competition lowers their profits and thus coordinate on a less competitive outcome. Previous research has identified many factors and market conditions that affect the intensity of competition and the likelihood of collusion, such as buyer power (Snyder, 1996), product homogeneity (Ross, 1992), symmetry (Compte et al., 2002) or multi-market contact (Bernheim & Whinston, 1990). Understanding which factors have an impact and how they work are essential to antitrust authorities, so that they can take effective action and protect consumers.

In this thesis, I want to gain knowledge on whether further factors are likely to influence competition and look at the mechanisms behind it. For this purpose, I use laboratory market experiments.

Market experiments can give insights into otherwise difficult identifiable effects of certain factors and therefore complement the theoretical and empirical literature on many topics. The main advantages of laboratory market experiments are internal validity and ceteris paribus comparisons as well as avoiding problems with missing or confounded field data, which is a common issue when investigating collusion. Market experiments have evolved over the last 70 years from scrutinizing the competitive equilibrium in pit-markets (Chamberlin, 1948) to examining specific competition issues (Hong & Plott, 1982; Davis & Holt, 1993; Martin et al., 2001) and becoming relevant for competition policy. This led to antitrust authorities referring to market experiments when deciding on the introduction of new policies (e.g., Hong & Plott, 1982) and merger cases (e.g., Huck et al., 2004; Fonseca & Normann, 2008).

In the following three chapters, I extend the aforementioned literature by looking at the impact of novel factors and market settings on competition and collusion.

In Chapter 2 titled "Pricing Behavior in Partial Cartels", I analyze in experiments the pricing behavior of firms, when only a subset of firms in the market can communicate. Therefore, I use a repeated, asymmetric capacity-constraint price game. The data reveal that partial cartels form in this situation, and that in line with theory, a partial cartel is sufficient to increase market prices for all firms. Moreover, I find that prices of cartel insiders and outsiders are not on the same level, which contradicts common theoretical predictions. This is because communication allows cartel members to overcome a potential coordination problem and enables an outcome in (joint) mixed strategies. The results therefore underline the difference between tacit and explicit collusion and the resulting market outcomes.

Chapter 3, "An Experiment on Partial Cross-Ownership in Oligopolistic Markets", is joint work with Volker Benndorf. We experimentally study both unilateral and coordinated anti-competitive effects of passive minority shareholdings between horizontally competing firms. Firms have symmetric, non-controlling shares of each other in a static and a dynamic Bertrand setting. We provide novel theoretical predictions about the impact of minority shareholdings on prices based on the concept of Quantal Response Equilibrium (McKelvey & Palfrey, 1995). Contrary to previous Nash predictions, we explain that passive partial cross-ownership reduces the incentives to compete and favors unilateral effects that result in higher average prices. Theory further predicts coordinated effects to arise as the discount factor decreases with the degree of cross-ownership. We test these hypotheses in a laboratory experiment. Our results show that a lower discount factor based on partial cross-ownership actually materializes in more tacit collusion. This only further softens competition but is not necessary for higher prices. Even without coordinated effects we see an increase in price levels with the degree of partial cross-ownership due to the existence of unilateral effects. We find negative repercussions of passive minority shareholdings for consumers by means of both effects. The results are highly relevant for the current policy discussion on supranational level regarding the regulation of passive minority shareholdings, e.g., in the European Union or the United States.

Chapter 4, "Cournot Competition with an Unknown Number of Players: Experimental Evidence", is joint work with Claudia Möllers and Hans-Theo Normann. We examine repeated Cournot oligopolies, when there is uncertainty about the number of players. We argue that this kind of uncertainty may lead to a novel strategy. A sophisticated player produces more than the static Nash equilibrium output, attempting to fool other players into believing there are more players than there actually are. In this case, total output increases but quantities are distributed asymmetrically. We use a laboratory experiment to investigate behavior in this setting. The data largely confirms the existence of such sophisticated play. Whereas a first supergame is predominantly characterized by Nash play, a second (and final) supergame shows distinct signs of increased and more asymmetric outputs. Using a k-means cluster analysis (MacQueen et al., 1967), we identify groups particularly characterized by sophisticated play and separate them from groups that play Nash or collude tacitly. The results suggest that uncertainty might lead to an outcome that resembles a more competitive market.

Lastly, in Chapter 5, I conclude and summarize the main results and insights of my thesis.

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Chapter 2

Pricing Behavior in Partial Cartels

2.1 Introduction

Cartels are a highly debated phenomenon in the economic literature but little is known about partial cartels. Research that considers situations in which only a subset of firms in a market forms a cartel asserts that every firm in the market charges an increased price – even those who do not have explicitly agreed to anti-competitive behavior. This price increase of non-cartel members is linked to "umbrella effects" and is considered inevitable (Blair & Maurer, 1982). More precisely, theory states that the price of cartel insiders and outsiders to be equal (Inderst et al., 2014; Holler & Schinkel, 2017).

The models concerned with partial cartels merely consider cartel members as independent firms that tacitly coordinate on some focal price (e.g. the joint profit-maximizing price). On the contrary, in real markets the most essential feature of hardcore cartels is arguably communication, which allows cartel members to group and coordinate.[1] This difference is important because communication can ultimately enlarge the set of equilibria (Athey & Bagwell, 2001; Balliet, 2010; Rahman, 2014; Awaya & Krishna, 2016) and might enable an equilibrium, where prices are not on the same level.

We want to examine whether the standard predictions hold when we look at cartels, in which cartel members can communicate with each other. More specifically, we want to examine the described umbrella effect and check whether prices of cartel insiders and outsiders are on the same level in an explicit partial cartel.

In order to get more insights on this setting, we explore the actual pricing behavior of cartel insiders and outsiders in a laboratory experiment. The experimental setup is most helpful to gather information about the incentives in partial cartels as cartels are illegal, and data on factors like communication is rare. An extensive empirical examination is therefore difficult to implement.

Our experimental design is based on a simplified version of the model by Bos & Harrington (2010) with price competition and heterogeneous capacity constraints in a dynamic setting, where a stable partial cartel exists. The capacities are chosen in a way that outsiders cannot compensate the intended effect of the cartel and the internal and external stability condition of partial cartels are fulfilled.[2]

[1] For example, in EU competition law the burden of proof is reversed once communication between firms is proven. In this case, the involved firms have to prove that their communication had no effect on the market result whatsoever or will be charged for infringements of article 101 TFEU (European Commission, 2015).

[2] Partial cartels usually base on restrictions such as switching costs, heterogeneous goods, hetero-

The standard theoretical prediction in this model is that the largest firms in the market form a cartel and choose the monopoly price for residual demand. This price serves as an umbrella for outsiders. The latter then free-ride by undercutting the cartel price by a minimal amount.

We argue that communication enables cartel members to deviate *jointly* from the monopoly price and to coordinate on different prices. The formation of the cartel is therefore very similar to a merger and virtually changes the capacity allocation. The ensuing equilibrium with regard to this argument is in mixed strategies. Equal prices occur here only by chance.

Supporting standard theory, we find that a partial cartel is sufficient to distort market prices. Average market prices are higher when partial cartels form compared to no cartel in the market. This confirms the expected umbrella effect. However, we do not find average prices of cartel members and outsiders at the same level. Prices of outsiders are significantly lower than cartel members' prices on average.

This is because most of the observed cartel members play indeed some kind of *joint* mixed strategies in order to undercut outsiders, making it impossible for outsiders to adjust their prices optimally. The observed joint deviation is not possible without communication, as an unilateral price change of a cartel member could be interpreted as deviation from the cartel agreement and thus result in a breakdown of the cartel. With communication, cartel members do not need to play pure strategies but can deviate jointly from the former cartel price and undercut only the outsiders to increase their profits.

We further find that even if cartel members play pure strategies and outsiders are potentially able to set their prices at the same level, outsiders only do so after some periods and approach their profit-maximizing price slowly, which is why average prices of outsiders are lower than cartel members' prices.

Importantly, we show that the price levels of cartel insiders and outsiders induced by umbrella effects can be very different. Cartel members can use more dynamic and sophisticated strategies by explicitly agreeing on different prices on short notice because they can communicate. Equal prices of cartel insiders and outsiders might therefore be more appropriate for tacit collusion, where firms cannot directly coordinate on a price other than a focal point and have to stick to it once they manage to collude. This strong effect of communication is not considered by many models concerned with partial cartels and should be examined in further research as well.

geneous marginal costs or heterogeneous capacities etc.

The remainder of this paper is structured as follows. We first summarize the related literature of this paper (Section 2.2). We then explain the model our experiment is based on and show the corresponding equilibrium (Section 2.3). In Section 2.3.3 we derive the equilibrium of a model that includes communication. Section 2.4 describes the experimental design and procedures. After listing our hypotheses in Section 2.5, we describe our results in great detail (Section 2.6) and discuss them with regard to economic theory. Section 2.7 offers a conclusion to our contribution.

2.2 Related Literature

In the following section, we summarize the literature regarding partial cartels and illustrate the importance of communication for coordination and equilibria. Further, we discuss findings from the experimental industrial organization literature on explicit collusion.

The theoretical literature regarding partial cartels only focuses on cartel formation and stability, where partial cartels are deemed as one possible outcome. The pricing behavior of firms is consistently considered as a consequence of the formation process. The actual prices of cartel members and non-cartel members depend on the respective economic model that the cartel is scrutinized in. Also, the explanation of the umbrella effect can be different depending on the model, but eventually, all models predict equal prices between insiders and outsiders. In models of quantity competition, it is commonly assumed that when collusive firms reduce output, outsiders cannot compensate the entire reduction of supplied goods by the cartel due to increasing marginal costs of production. Therefore, the total supply decreases and prices increase accordingly, creating the aforementioned umbrella effect (Inderst et al., 2014; Holler & Schinkel, 2017). This causal connection has been shown in examinations of firms in Cournot models (Selten, 1973; Escrihuela-Villar, 2004) as well as Stackelberg models in both static (Shaffer, 1995) and dynamic settings (Martin, 1990; Konishi & Lin, 1999; Nocke, 1999; de Roos, 2004; Escrihuela-Villar, 2009; Zu et al., 2012).

Research on partial cartels in price competition also shows that the prices of cartel outsiders and cartel members are identical. Based on a static price leadership model defined by Markham (1951), it was shown that cartel members anticipate outsiders' reaction to a price increase and charge the monopoly price for the residual demand, also assuming increasing marginal costs of production (d'Aspremont et al., 1983; Donsimoni, 1985; Donsimoni et al., 1986; d'Aspremont & Gabszewicz, 1986; Daskin, 1989). Outsiders that are too small to influence the price take this price as given and produce until their price equals marginal costs (Blair & Maurer, 1982).

We do not rely on marginal costs in our model but enable partial cartels to emerge by restricting capacities of firms. Bos & Harrington (2010) determined equilibrium prices in a model of price competition with capacity constraints in a dynamic setting. The authors show that in this case the cartel price serves as an umbrella for outsiders who free-ride by undercutting the cartel price by a minimal amount without bearing the consequences - that is, reducing output.

None of the aforementioned models considers communication or distinguishes between explicit and tacit collusion. However, the market outcome can be very different if communication is available. Rahman (2014) shows that communication allows for more equilibria that would not be stable without communication. Awaya & Krishna (2016) show in their model an equilibrium with communication, in which profits are strictly greater than in any equilibrium without communication. Models that explicitly investigate collusion and include communication show that it can help to coordinate and enables more stable equilibria (e.g. Athey & Bagwell (2001, 2008)). Harrington & Skrzypacz (2011) describe a model with communication, which equilibrium fits recent detected cartels.

Communication not only enlarges the set of equilibria but also facilitates coordination and cooperation as shown by Crawford & Sobel (1982), Isaac & Walker (1988) and Farrell & Rabin (1996). Balliet (2010) illustrates that communication enhances cooperation in social dilemmas. Experiments by Cooper et al. (1989, 1992) and Charness & Dufwenberg (2006) underline these findings. In this paper, we use chat messages in our experiment, because free-form language is supposed to be most effective (Brosig et al., 2003) to increase cooperation. It is also assumed that, which this method, subjects feel more secure about their decisions as participants can send messages to reassure each other and reduce uncertainty with regards to their decisions (Crawford, 1998).

The collusion facilitating effect of communication is well known and has been shown in industrial organization experiments many times (e.g., Cooper & Kühn, 2014; Andersson & Wengström, 2007). Fonseca & Normann (2012) focus on the different market outcomes between tacit and explicit collusion in an experiment when either all firms can communicate with each other or none. They find that the profit gains from communication are greatest for medium-sized markets. In these papers, communication only enhances collusion rates but does not enable a different equilibrium.

There is some experimental literature on partial cartels. This literature, however, focuses on the formation process of partial cartels and antitrust policies. Clemens & Rau (2013, 2019) examine a Cournot game, where firms can decide on forming a partial cartel, and outsiders automatically play their best-response, which corresponds to prices at the same level. Independent research by Gomez-Martinez (2016) examines the formation process of partial cartels and the coordinated effects of mergers within an extension of the Bos & Harrington (2010) model. In contrast to this work, we allow firms to communicate before every period at no costs and without agreeing to a cartel beforehand. We further restrict communication to firms that can form a stable partial cartel, thereby determining cartel members exogenously, since we focus on the pricing behavior in cartelized markets and not cartel formation.

2.3 The Model

The model is characterized by a market with heterogeneous firms that differ in capacities installed but produce a homogeneous good. Firms have constant marginal costs normalized to zero, $c = 0$, and make their price decisions simultaneously. All firms have complete and perfect information.

We consider a market with inelastic demand. Demand is given by M for all $p \leq \bar{p} = 100$ and 0 if $p > 100$. Individual demand depends on firm i's price p_i and the vector of prices of the other firms p_{-i}.

Consumers are homogeneous and have the same valuation of the product. They buy from the firm with the lowest price first and buy from the firm with the next higher price only when supply of the firm with the lowest price is exhausted. We restrict the plethora of equilibria by a substantial assumption: if two or more firms charge the same price and therefore capacity exceeds demand at this price, demand is allocated proportional to each firm's capacity.

A firm's capacity is denoted by k_i such that $\sum k_i$ denotes the industry capacity and $\sum_{j \neq i} k_j$ is the capacity of a firm's competitors. Capacities are such that $\sum_{j \neq i} k_j \geq M > k_i$. No firm has sufficient capacity to supply the entire demand and any subset of $n - 1$ firms can serve the entire market. Total capacity of all firms strictly exceeds total demand, $\sum k_i > M$. For our experiment, we consider the case where two symmetric firms with high capacities and two symmetric firms with low capacities exist.

2.3.1 Static Nash Equilibrium

Due to the capacity allocation, the smallest firm does not sell a positive amount if it is the only firm that charges the highest price, and on the other hand, the largest firm sells at capacity if it solely charges the lowest price. This implies that the one-shot static Nash equilibrium in our model is the same as in a classic one-shot Bertrand game with homogeneous products. Taking into account the decision making of the other firms, the best-response of each firm is to charge a price equal to marginal costs, that is, $p_i = 0$.

Proposition 2.1. *In the static Nash equilibrium, all firms set a price of* $p_i = 0$*. Profits for all firms are* $\Pi_i = 0$*.*

2.3.2 Repeated Game Equilibrium

In an infinitely repeated game, collusion is possible if firms are sufficiently patient.

In our setup, we assume firms to set prices in order to maximize joint profits and according to the well-known trigger strategy (Friedman, 1971). Deviating from the cartel agreement would revert all firms to static Nash pricing at marginal cost for the remainder of the game, yielding profits of zero (see Proposition 2.1).

For the further analysis, let Γ denote the set of cartel members and O denote the set of cartel outsiders. Furthermore, $K_\Gamma = \sum_{i\in\Gamma} k_i$ is the joint capacity of the cartel members and $K_O = \sum_{i\notin\Gamma} k_i$ is the aggregate capacity of cartel outsiders.

For collusion to be an equilibrium, we need

$$\frac{\bar{p}\cdot k_i}{1-\delta}\cdot\frac{k_i}{K_\Gamma} > \bar{p} - 2\epsilon\cdot k_i$$

or

$$\delta > 1 - \frac{\bar{p} * \frac{k_i}{K_\Gamma}}{\bar{p} - 2\epsilon},$$

for all $i \in \Gamma$. Since the focus of this paper is not on cartel formation, but on the ensuing prices in a partial cartel, we only consider the case, where the discount factor δ is sufficiently high and hence, collusion is sustainable.[3]

Moreover, partial cartels can only arise if they fulfill the internal and external stability conditions defined by d'Aspremont et al. (1983). Internal stability is given as all cartel members earn higher profits joining the cartel. External stability is given due to the demand allocation rule. It is not profit-maximizing for an outsider to join the cartel and set the same price, because it would sell less output at an only slightly higher price.

Insiders' Prices When no communication between cartel members is possible, an equilibrium in pure strategies exists, where cartel insiders set the collusive profit-maximizing price. Cartel members anticipate that outsiders will price below the cartel price. Therefore, they cannot sell at capacity when setting high collusive prices but will only serve residual demand. However, due to the insufficient capacities of the outsiders to satisfy the entire demand, they still face positive residual demand. Therefore, the actual behavior of outsiders is supposed to be not relevant for cartel members. In our model with inelastic demand, the monopoly price for residual demand simply is the reservation price,

$$p_\Gamma = \bar{p}.$$

A price higher than the reservation price, $p_\Gamma = \bar{p}$, would result in demand of zero, as no consumer is willing to pay more than the reservation price $\bar{p}$. A lower price would merely lower profits but would not attract any more customers. Due to the distribution rule of demand, each cartel member's individual profit in each period is

$$\Pi_{i\in\Gamma} = \bar{p} \cdot [M - K_O] \cdot \left(\frac{k_i}{K_\Gamma}\right),$$

which is higher than competitive profits for any positive demand.

Outsiders' Prices The best-response of outsiders is to undercut the cartel price by a minimal amount ϵ and sell at capacity to maximize their individual profit. Outsiders set a price of

$$p_{i\in O} = p_\Gamma - \epsilon.$$

[3]In our experiment, we induce $\delta = 0.89$ while the critical discount factors of the firms are never higher than this. Hence, collusion is sustainable in every constellation.

Setting a lower price, $p_{i\in O} < p_\Gamma - \epsilon$, generates less profit, as the same output will be sold at a lower price. A higher price would reduce demand. Specifically, a price on the cartel level, $p_{i\in O} = p_\Gamma$ forces the firm to reduce sales according to the proportional demand allocation rule. The marginal increase in price cannot compensate these losses in quantity. A price above the cartel price, $p_{i\in O} > p_\Gamma$, results in no demand since cartel members can satisfy the entire demand at this price. Hence, no profit can be generated with this pricing strategy. The inflated price of the cartel can be used as an umbrella by cartel outsiders, who can free-ride by selling at a higher price without reducing their production. The consequential profits of each outsider in every period then amount to

$$\Pi_{i\in O} = [p_\Gamma - \epsilon] \cdot k_i$$

Proposition 2.2. *In an infinitely repeated game without communication, a subgame perfect equilibrium in pure strategies exists where firms charge prices of $p_\Gamma = \bar{p}$. Profits of each cartel member are*

$$\Pi_{i\in\Gamma} = \bar{p} \cdot [M - K_O] \cdot \left(\frac{k_i}{K_\Gamma}\right)$$

Cartel outsiders set a price of $p_{i\in O} = p_\Gamma - \epsilon$ and make profits of

$$\Pi_{i\in O} = [p_\Gamma - \epsilon] \cdot k_i$$

2.3.3 Repeated Game Equilibrium with Communication

Standard theory does not cover the possibility of cartel members to deviate jointly from an agreed price. Yet, communication and coordination are essential features of cartels. Since communication in our experiment is available to firms in every period, we argue that firms do not have to necessarily choose the maximum price but can quickly coordinate on any price of the price range without costs. This joint deviation is not possible without communication, as an unilateral price change of a cartel member could be interpreted as deviation from the cartel agreement - which would most likely result in a breakdown of the cartel and consequently in Nash reversion. With communication, a price change can be announced or discussed, which is why cartel stability is not threatened. Therefore, we want to think of cartel members as one merged firm, which acts accordingly, and consequently, dominates the market due to its size. This feature virtually changes the capacity allocation and relation of demand to capacities to

$$K_\Gamma = \sum_{i\in\Gamma} k_i > M > \sum_{i\notin\Gamma} k_i.$$

The equilibrium is then in mixed strategies. Firms randomize prices between the reservation price, $\bar{p} = 100$, and a lower bound that depends on capacities.

Insiders' Prices Cartel members cannot sell at capacity since their combined capacities exceed demand in any case, but they can sell a larger amount than residual demand by undercutting and serving the outsiders' former market share. This trade-off pays off until a minimum price of $\underline{p}$ is reached, at which the cartel, when serving the entire demand, would earn the same profit as selling residual demand at the maximum price, $\bar{p}$. The minimum price of the cartel is therefore

$$\underline{p} = \bar{p} \cdot \frac{(M - K_O)}{min\{K_\Gamma, M\}}$$

Outsiders' Prices Outsiders can only sell their supply if they choose a lower or equal price than the cartel. Since a price below $\underline{p}$ is a non-credible threat by cartel members, the minimum price of a non-cartel firm is identical to the cartel's minimum price. The maximum price is also identical to the cartel's maximum price, $\bar{p}$. As outsiders in our model are symmetric, this holds for every outsider.

Proposition 2.3. *In an infinitely repeated game with communication, the unique, symmetric equilibrium is in mixed strategies with support $\underline{p}$ and $\bar{p}$. The probability that the cartel charges a price p is given by the distribution function*

$$F^\Gamma(p) = \begin{cases} 1 - \frac{(M-K_O)\cdot\bar{p}}{M\cdot p} & \textit{if } p \in [\underline{p}, \bar{p}) \\ 1 & \textit{if } p = \bar{p} \end{cases}$$

Equilibrium profits of a single cartel member are $\Pi_{i\in\Gamma} = \underline{p} \cdot M \cdot \frac{k_i}{K_\Gamma}$.
The probability that outsiders charge a price p is given by the distribution function

$$F^O(p) = F^{o1}(p) = F^{o2}(p) = \begin{cases} \frac{M}{K_O} - \frac{(M-K_O)\cdot\bar{p}}{K_O\cdot p} & \textit{if } p \in [\underline{p}, \bar{p}] \end{cases}$$

Outsiders individual profit is $\Pi_{i\notin\Gamma} = \underline{p} \cdot k_i$.

The proofs of the propositions can be found in Appendix 2.10.3. A more general version of the model with asymmetric firms in Bertrand-Edgeworth markets, where only one firm can supply the entire demand and the remaining firms are minor players, is analyzed in great detail by De Francesco & Salvadori (2008) and Hirata (2009).

2.4 Experimental Design and Procedures

Subjects play the game as described in Section 2.3 with a capacity allocation of $k_1 = k_2 = 200$ (big firms) and $k_3 = k_4 = 50$ (small firms). Demand consists of $M = 300$ computer-simulated consumers who demand one unit of the good at minimal expense.

In the treatment group, we allow for unmediated communication only between large firms of each group, thereby determining cartel members exogenously. Subjects can communicate in every period before they have to make their price decision for one minute in the first three periods of each supergame and for 30 seconds in the remaining periods of each supergame. This period of time appears to be appropriate for communication since previous experiments showed that most conversations lasted less than 40 seconds (see for example Fonseca & Normann (2012)). Small firms just have to wait until the game continues. Subjects remain anonymous during the chat and are given neutral names like Firm 1 or Firm 2, which do not change during the session. Subjects are free to send as many messages as they like and to talk about what they want but have to respect two restrictions: the subjects' identities must not be revealed and messages with potentially offensive content are prohibited. All subjects are aware that only the large firms can communicate with each other in each group, and that only these firms can see the conversation.

In the control group sessions, subjects play the exact same game as described before and hereafter but do not enter the communication-stage of the game. Participants in this group do not know about the possibility of communication in the other sessions.

All subjects receive written instructions, which inform them about all features of the experiment and the markets prior to the start of the experiment. A translated version for both groups can be found in Appendix 2.10.4. Once all subjects have read the instructions, they can privately ask questions. At the beginning of the experiment, all subjects are randomly assigned to either a large firm or small firm, and subjects represent this firm for the entire experiment. In each period, before entering their payoff relevant price, subjects can use a profit calculator provided on the screen to test the potential impact of various own and other firm's decisions. In the following stage of each period, subjects have to enter their price at a computer terminal. Once all subjects do this, the period ends, and a screen displays the prices chosen by each firm in the market, the quantities sold by each firm and the profit of each individual firm in that particular period. Furthermore, the screen displays the accumulated profits of the respective firm (but not of the other firms) up to that point. Thereafter, the next period starts. We use a between subjects design for the experiment.

Participants are either in a control group or in a treatment group. Both treatments of the experiment respectively consist of three supergames that have multiple periods each to control for learning effects. Before each supergame subjects are randomly matched with three other firms, so that there are two large firms and two small firms in each market. Throughout a supergame, all subjects are matched with the same three other subjects in every period. Before each supergame, all subjects are randomly assigned to a new group. The length of a supergame is determined by a random termination rule with a continuation probability of 8/9. This may be interpreted as an induced discount factor of $\delta = 0.89$, which is high enough for sustainable collusion according to the theory. All subjects are informed about the continuation probability. The actual numbers of periods per supergame are determined ex ante by a virtual die to ensure the same length of supergames over all markets, sessions and treatments. We have 144 participants in total - 72 participants per treatment divided into three sessions with 24 participants each. Hence, we are able to observe 18 markets per supergame per treatment or 54 markets per treatment respectively. Table 2.1 gives an overview of the design per treatment.

Subjects receive a payment consisting of a show-up fee of 5 Euro plus the sum of profits they earned during the experiment. The show-up fee is provided to moderate the expected asymmetric payoffs for subjects due to the differences in capacity of large and small firms. We use an "Experimental Currency Unit" (ECU) for payments, with 15,000 ECU being worth 1 Euro. The experiments were run in the Düsseldorf Institute for Competition Economics (DICE) Laboratory for Experimental Economics at the Heinrich-Heine-University Düsseldorf in July 2016. The experiment was programmed and conducted with the experiment software z-Tree (Fischbacher, 2007). Sessions lasted for approximately 60 minutes without com-

	Supergame 1	Supergame 2	Supergame 3	Total
Periods	9	14	6	29
Markets	18	18	18	54
Participants	72	72	72	72

Table 2.1: Experimental setup.

munication and 80 minutes with communication. Subjects earned between 6 Euro and 26.50 Euro, with the average payment being 13.88 Euro. The online recruiting system ORSEE (Greiner, 2015) was used for recruitment, ensuring that subjects have not participated in similar experiments before. Subjects were students and non-students from a variety of backgrounds.

2.5 Hypotheses

We begin with the collusion facilitating effect of communication. In the experiment, big firms were not forced to establish cartels but could freely choose if they wanted to collude or not. Several experiments showed that communication has a pro collusive effect (as we described in Section 2.2), although communication should have no impact on firms behavior according to the extant literature. Fonseca & Normann (2012) showed that all firms in a market collude more frequently, when all firms are able to communicate with each other. In our experimental setup, only two of the four firms could communicate with each other in each market. Nevertheless, we expect these two firms to collude when communication is allowed, as the mechanics are identical. Moreover, there is no obvious reason why communication should lose its impact, when it is restricted to a subset of market participants, especially since there were no costs of communication and firms do not have to fear to be detected forming a cartel. Therefore, we expect that firms to coordinate on a common price when communication is available.

Hypothesis 2.1. *When communication is allowed, firms coordinate on prices and form a cartel.*

Once we observe cartels, we expect cartel members to increase prices above the competitive level, as we have shown in our equilibrium analysis. Theory predicts that a partial cartel raising its price is sufficient to increase prices for the whole market independent of the cartels' respective pricing strategy. In our experiment with the underlying model, we expect small firms to recognize the price increase of cartel insiders and infer that a cartel has been formed, which consequently makes small firms increase their prices, too.

Hypothesis 2.2. *When cartels form, prices of cartel insiders will be higher than in competition. Cartel outsiders follow and choose higher prices than in competition, too.*

The focus of our experiment is on the relation of cartel outsiders' prices to cartel insiders' prices. In standard approaches to collusion, cartels charge a unique price, that is, they play a pure strategy. In our experimental setting, the profit-maximizing price for a cartel is then the reservation price of consumers, $p^{\Gamma} = \bar{p} = 100$. Outsiders, pricing optimally by free-riding under the cartel's umbrella, should set a price at $p^{O} = p^{\Gamma} - \epsilon$ in every period, independent of the actual price the cartel coordinates on. In our experiment, the smallest reduction, ϵ, is 1 unit. Hence, outsiders' price should be $p^{O} = p^{\Gamma} - 1$. However, as we argue in Section 2.3.3, communication allows cartel members to play (joint) mixed strategies with prices between $\underline{p}$ and $\bar{p}$. Outsiders' best response would be to play mixed strategies as well, randomizing their price between $\underline{p}$ and $\bar{p}$. In this case, prices of insiders and outsiders would only be on the same level by chance. Hence, we expect prices to be different.

Hypothesis 2.3. *Insiders' and outsiders' prices will not be on the same level.*

2.6 Results

In this section, we summarize our results and verify the hypotheses on the basis of the data obtained from the experiment. Our intention is to analyze the pricing behavior of firms in partially cartelized markets.

We focus on the differences between periods in which markets exhibit partial cartels (hereafter called Cartel) and periods in which firm could not communicate and markets are characterized by competition (hereafter called Competition) in order to examine the umbrella effects. Hence, the definition of a cartel is crucial for our analysis. We further analyze the pricing behavior of cartel insiders and outsiders *within* markets that exhibit cartels to learn about the relation of their respective prices. Robustness checks where we compare solely the different treatment groups can be found in Appendix 2.10.2. The results presented hereafter do not change qualitatively when we do so.

Cartel Formation

We assume a cartel to be established only if communication was used and chosen prices of communicating firms were equal in subsequent periods. Only these observations constitute our benchmark CARTEL for the remainder of this paper. This is because communication between firms is legally considered as an essential characteristic of (hardcore) cartels and distinguishes tacit collusion from explicit collusion. We use an even tighter definition of a cartel and require an actual impact of communication to see that this was not only cheap talk.[4]

Cartels were established in 93.51% of the markets and in 80.27% of possible periods, respectively. Thereby, we observe a learning effect over supergames with increasing rates of collusion from 72.22% to 87.04%. These periods with active cartels will be the basis of our analysis of the price setting behavior in partial cartels. A deeper analysis of the periods without active cartels shows that some participants refused to use the chat and consequently did not set high prices. Our results show the power of communication once again and underline why the EU considers communication as an important factor for collusion. Considering the data, our first qualitative result is in line with the previous literature and confirms Hypothesis 2.1.

Result 2.1. *When communication is allowed, firms coordinate on prices and form a cartel.*

2.6.1 Price Levels and Umbrella Effects

In order to gain more information about the umbrella effect, we will next compare the price setting behavior of participants in our benchmark group (COMPETITION) and in cartelized markets (CARTEL). For a simpler analysis, we calculate the mean prices charged by firms with large capacity and the mean prices charged by firms with low capacity in each market for every period. This is reasonable since we are interested in the behavior of the two categories, potential cartel members and cartel outsiders. Furthermore, we have to take into account that prices are not fully independent within the categories in our treatment group since we consider a cartel as established if both big firms choose equal prices. Therefore, the average price is simply the price chosen by the cartel. Henceforth, we will call these proxies

[4] Due to our approach, some periods in our treatment group are periods without established cartels, whereas there are no periods in our control group with active cartels by definition due to the lack of communication. This results in some periods not belonging to any of the two categories.

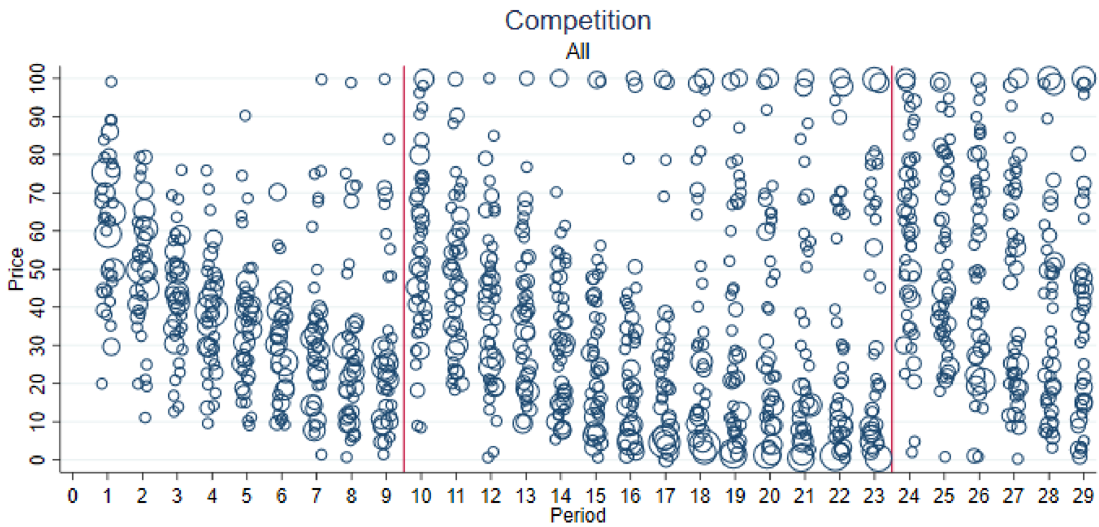

Figure 2.1: Chosen prices in Competition.

Diameter of circle indicates price frequency.

"BIGS" for the average price of both firms with high capacities and "SMALLS" for the average price of both firms with low capacities.

Benchmark: Pricing Behavior in Competition

We first evaluate our benchmark, that is prices chosen by participants in our control group without communication, Competition. Although we can observe fierce price competition, selling prices are not at marginal costs. Participants chose from the full range of available prices $\{0, ..., 100\}$ in their attempt to maximize profits. They ended up at an average selling price of 33.98 ECU. The average chosen price was 37.76 ECU.

In this context, BIGS have an average selling price of 34.35 ECU and SMALLS have an average selling price of 33.59 ECU over all periods and groups when firms cannot communicate. Counting each market average as one observation separated by BIGS and SMALLS, a Wilcoxon signed-rank test shows that the differences between chosen prices of BIGS and SMALLS are not statistically significant for each supergame ($p > 0.1$).[5] The average selling price level differs across groups and supergames between 7.86 ECU and 93 ECU. More precisely, BIGS average

[5]The tests were calculated with chosen prices to ensure a balanced sample. We do not have selling prices for every observation, since small firms cannot sell anything if they charge a higher price than both big firms due to the capacity allocation.

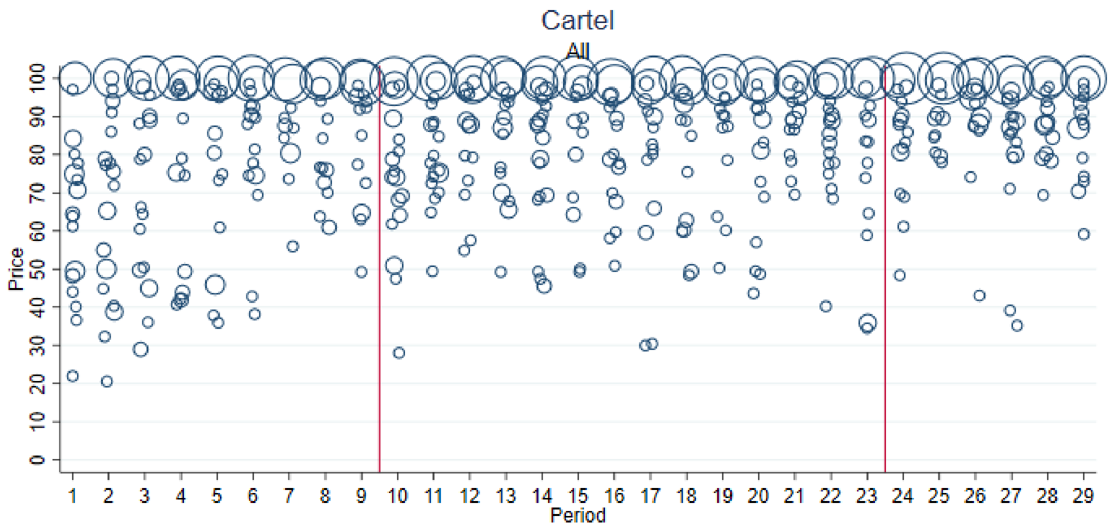

Figure 2.2: Chosen prices in Cartel.

Diameter of circle indicates price frequency.

selling price was between 8.44 ECU and 97.29 ECU across groups, whereas SMALLS average selling price was between 7.31 ECU and 93.42 ECU across groups.[6] Most groups show similar patterns of competition. Figure 2.1 illustrates the development and frequency of prices in competition. Figure 2.3 in Appendix 2.10.1 shows how close the average prices of BIGS and SMALLS are.

Pricing Behavior in Cartelized Markets

In the next paragraphs, we look at the selling prices of participants representing BIGS in periods when they decided to form a cartel (Cartel) and compare them to the selling prices in our benchmark Competition (see also Table 2.2).

Price Level of Cartel Members

Prices of BIGS were higher, when a cartel had been formed than prices of BIGS in Competition. A Mann-Whitney U test shows that prices of cartel members were statistically significantly different between Cartel and Competition in each supergame ($p < 0.01$). This should be no surprise and supports common theory on cartel pricing. Overall, cartel members had an average selling price of 96.21 ECU when they coordinated on prices. However, we observe significant group differences.

[6]The markets with high averages are two markets where tacit collusion occurred in earlier periods.

	avg. Price All	avg. Price BIGS	avg. Price SMALLS
Competition	33.98	34.35	33.59
Cartel	92.32	96.21	88.01

Table 2.2: Average selling prices COMPETITION/CARTEL.

The average selling price of cartels was between 73.5 ECU and 100 ECU.[7] This indicates that cartels exhibited heterogeneous price patterns across groups. We will discuss this later on in more detail. Nevertheless, our results are in line with common theory.

Result 2.2. *When partial cartels form, prices of cartel insiders are higher than in* COMPETITION.

Price Level of Cartel Outsiders

Prices of SMALLS were significantly higher in CARTEL than in COMPETITION over all groups per supergame as well (Mann-Whitney U test, $p < 0.01$). On average, selling prices of outsiders were at 88.01 ECU when a cartel was active (see also Table 2.2). The aforementioned group differences are also observable for small firms. SMALLS average selling price was between 37.5 ECU and 99 ECU across groups. The different price levels observed for cartel members seem to be reflected in outsiders' prices. Considering our results so far, it seems obvious that once a cartel forms, prices of cartelists rise and outsiders follow. Figure 2.2 illustrates the average prices over all markets and firms per period with cartels.

Result 2.3 (a). *When partial cartels form, prices of cartel outsiders are higher than in* COMPETITION.

Following from Result 2.2 and Result 2.3 (a) we can confirm our Hypothesis 2.2 and the common notion that when a cartel forms, the price level of the market rises overall, as we discussed in the related literature. Further support is given by average selling prices over all groups and all firms, which were at 92.32 ECU. A Mann-Whitney U test shows that average selling prices of all firms combined were higher when a cartel was active than in markets with competing firms for each supergame ($p < 0.01$). The group differences in price levels (average prices

[7] In one market, big firms chose the same price only once and therefore had an average price of 45 ECU.

between 68.75 to 99.5 ECU) can also be seen in these combined averages. Table 2.2 summarizes these results and leads to Result 2.3 (b).

Result 2.3 (b). *A partial cartel is sufficient to raise prices above the competitive level for the entire market.*

2.6.2 The Gap between Insiders' Prices and Outsiders' Prices

We now look at cartelized markets in more detail and compare our results with the equilibria of our model explained in Section 2.3 and Section 2.3.3.

We first notice, that cartel members did not always play pure strategies at the monopoly price but deviated from the monopoly price many times. The profit-maximizing price in pure strategies for a partial cartel is $p^{\Gamma} = \bar{p} = 100$. Over all markets, this price was chosen in 79.71% of possible times by cartel members. Considering descriptive statistics of the results in greater detail, we find that cartel behavior is quite heterogeneous. A deeper analysis of the price distribution reveals that the established cartels can roughly be categorized into two types. In 21 of the 49 cartelized markets cartel members played pure strategies and constantly chose the monopoly price of 100 ECU, i.e., in 100% of the possibilities. The average price of cartels in these markets is subsequently exactly 100 ECU. Cartels of this type (type 1) account for the majority of incidences where the monopoly price was chosen. Cartel firms of the other type (type 2) switched between many prices in the range between 35 ECU and 100 ECU. They played some kind of mixed strategies. The monopoly price of 100 ECU was chosen in 64.58% of the opportunities. Cartel members in these 28 markets account for all prices chosen by cartels which are below the monopoly price. The average price of cartels in these markets is 91.35 ECU.

Type-depending Prices

We want to know whether prices of outsiders and insiders were on the same level depending on the cartel type. In our model, "same level" is defined as an outsider price equal to the cartel price minus a minimal amount, $p^O = p^{\Gamma} - \epsilon$, which is $p^O = p^{\Gamma} - 1$ in our experiment.

Outsiders in markets with a cartel of type 1 charged the second highest price (99 ECU) on average only in 69.55% of the cases when 100 ECU was chosen by the cartel in every period. Their average price was therefore 90.4 ECU. Table 2.3 also shows that SMALLS did not always choose their best response.

	both outsiders		at least one outsider	
	$p^\Gamma \geq p^O \geq p^\Gamma - 1$	$p^\Gamma \geq p^O \geq p^\Gamma - 10$	$p^\Gamma \geq p^O \geq p^\Gamma - 1$	$p^\Gamma \geq p^O \geq p^\Gamma - 10$
cartel type 1				
$p^\Gamma = 100$	59.22%	72.63%	82.68%	88.27%
cartel type 2				
$p^\Gamma = 100$	19.35%	37.42%	45.81%	77.42%
$p^\Gamma \neq 100$	11.76%	18.82%	20.00%	41.18%
overall	16.67%	30.83%	36.67%	64.58%
pooled				
$p^\Gamma = 100$	40.72%	56.29%	65.57%	83.23%
$p^\Gamma \neq 100$	11.76%	18.82%	20.00%	41.18%
overall	34.84%	48.69%	56.32%	74.70%

Table 2.3: Prices of Insiders and Outsiders in CARTEL.

Outsiders in markets with a cartel of type 2 were only rarely in proximity of the cartel price. Outsiders' average prices in these markets is 83.51 ECU. A price of 99 ECU was only chosen 32.58% of the possible cases on average. A Wilcoxon signed-rank shows that the average chosen price of SMALLS and BIGS are significantly different for all type 1 and type 2 cartels separately ($p < 0.05$).

Over all markets, the respective predicted price for outsiders of each period, $p^O = p^\Gamma - 1$,[8] was chosen by both outsiders in only 34.84% of the cases. In 48.69% of the cases, both outsiders set a price in range of $p^O = p^\Gamma - 10$ to $p^O = p^\Gamma$. In 56.32% of the cases, at least one outsider charged the optimal price, $p^O = p^\Gamma - 1$ or the highest selling price $p^O = p^\Gamma$. In 74.70% of the cases, at least one outsider priced in close proximity to the cartel ($p^O = p^\Gamma - 10$) or set the same price as the cartel, $p^O = p^\Gamma$. A Wilcoxon signed-rank test stating that chosen prices of outsiders are on the optimal level ($p^O = p^\Gamma - 1$) can be rejected at the 1% level ($p < 0.01$) for each supergame.

Figure 2.5 and Figure 2.6 in Appendix 2.10.1 show the price patterns of the different types of cartels and the respective reaction by outsiders. In these plots we also see that cartels of type 2 do not randomize their prices according to the mixed equilibrium but rather try to undercut outsiders systematically. Observing that outsiders use the cartel price as umbrella, cartel members have an incentive to deviate *jointly* from the former cartel price and charge a price just below the outsiders' price, since they serve only the residual demand when pricing higher than outsiders (e.g. at the maximum price, $\bar{p}$). Once the cartel notices that outsiders undercut the minimum price, it charges the maximum price $\bar{p}^\Gamma$ again and earn the

[8]We also include here the highest selling price $p^O = p^\Gamma$.

maximum profit from residual demand, as we have shown in our equilibrium analysis for pure strategies. As soon as the outsiders adjust their prices and try to free-ride under the cartel's umbrella by setting $p^O = \bar{p}^\Gamma - \epsilon$, the cartel can undercut the outsiders again with $p^\Gamma = \bar{p} - 2\epsilon$ and the cycle starts again (compare Maskin & Tirole (1988)). In this case, prices can only be equal by coincidence. Hence, our Hypothesis 3, saying that outsider prices and cartel prices are not on the same level, can be verified.

Result 2.4. *Average prices of cartel outsiders are below cartel insiders' level.*

2.7 Conclusion

In economic theory it seems undisputed that if partial cartels form, cartel members charge a higher price and enable outsiders to raise their prices, too. More precisely, outsiders' prices and cartel prices are supposed to be on the same elevated level (compare Blair & Maurer, 1982; Inderst et al., 2014; Holler & Schinkel, 2017). However, the standard models treat cartel insiders as two independent firms, even though hardcore cartels are based on communication, which can enable a different equilibrium. We argue that communication allows for an equilibrium where prices of insiders and outsiders are not on the same level.

We examined explicitly the pricing behavior of cartel insiders and outsiders in partial cartels, when cartel members can communicate with each other. Due to the secret nature of cartels, we used a laboratory experiment for this purpose. More specifically, we conducted a repeated capacity-constrained price game with asymmetric firms on the basis of the model by Bos & Harrington (2010), where participants could form partial cartels by communication via a chat tool.

We confirm the common notion that market prices are distorted when a partial cartel exists compared to a market without a cartel. Although cartels were incomplete, we find that every firm in the market charged a higher price, confirming the umbrella effect. This underlines the threat even partial cartels pose on consumers.

We do not find prices of cartel insiders and outsiders on the same level. We identified two cases of pricing strategies or types of cartels. In the first case, participants representing cartel firms constantly chose the same price on monopoly level. Outsiders played their best-response and followed the cartel price as predicted by Bos & Harrington (2010). They could maximize their profit by free-riding under the umbrella. Nevertheless, outsiders were reluctant to adjust their prices. It took multiple periods of constant cartel prices until outsiders approximated the cartel price. Therefore, prices of outsiders were statistically significantly different than insiders' prices.

In the second case, cartels did not constantly choose a single price. Instead, they deviated jointly from the former cartel price as soon as outsiders adjusted their price in order to receive the entire demand. Consequently, outsiders could not charge prices on the level of cartel insiders but had to choose lower prices to avoid being exploited. This joint deviation is only possible due to communication, which is not covered by standard theory. Considering cartel firms as a single big firm, the subsequent equilibrium with one dominant firm is in mixed strategies, which applies well to our data.

The results therefore show that prices of insiders and outsiders are not on the same level if we consider explicit cartels, in which cartel members can communicate with each other.

It also shows the need for further research on explicit cartels. The market outcomes with explicit cartels might be different from market outcomes with tacit collusion also in other settings. Assuming that cartels do not face a coordination problem and behave like a merged firm, market characteristics that seemed to be crucial for collusion should be examined again. This however is beyond the scope of this study.

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2.10 Appendix

2.10.1 Figures

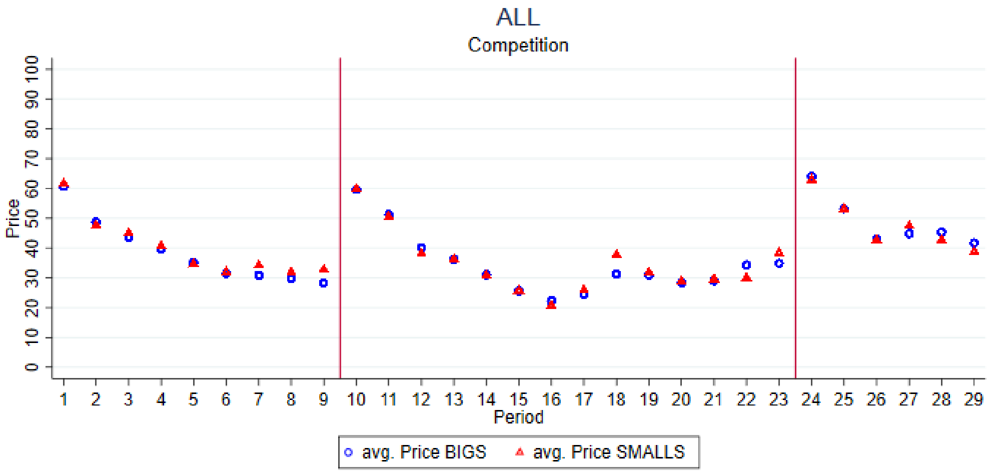

Figure 2.3: Average chosen prices in COMPETITION.

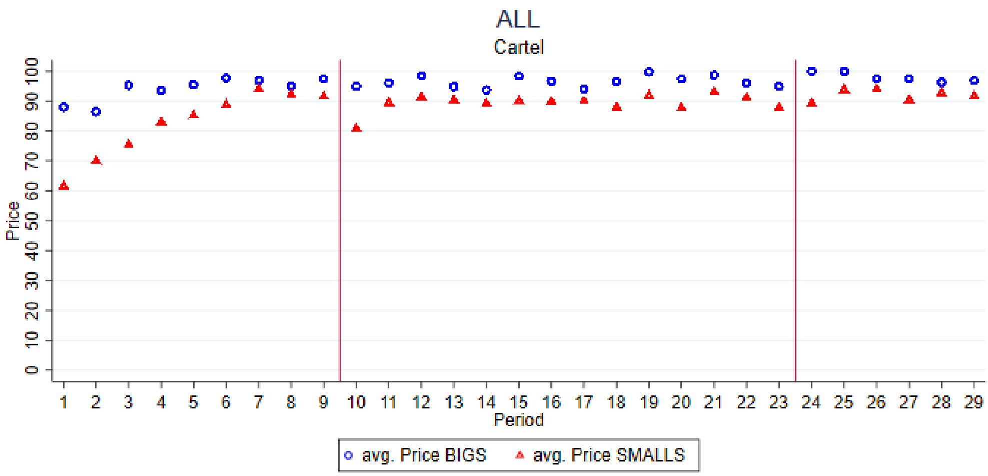

Figure 2.4: Average chosen prices in CARTEL.

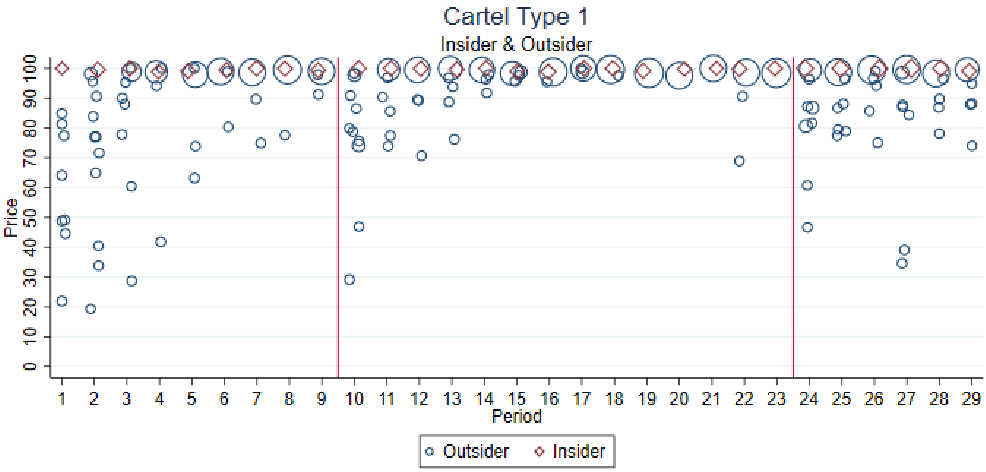

Figure 2.5: Average chosen prices in cartel type 1.

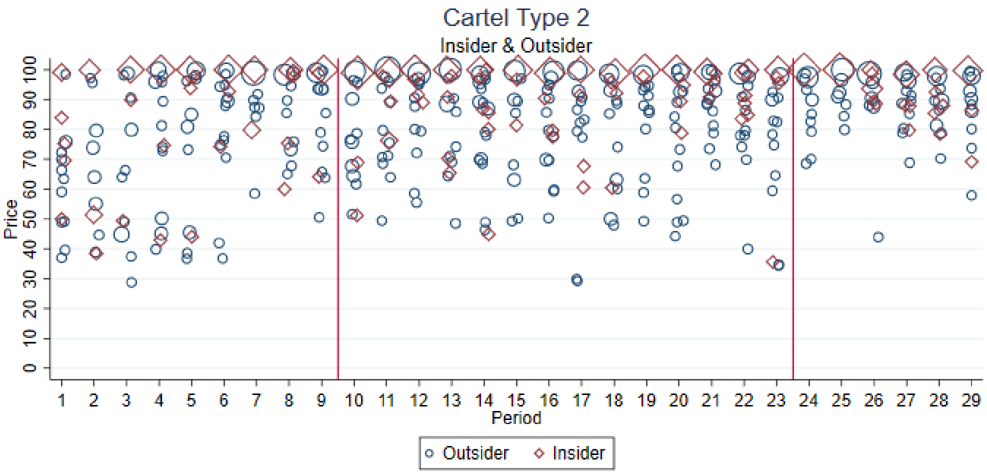

Figure 2.6: Average chosen prices in cartel type 2.

2.10.2 Robustness Checks

Regression Analysis

	Chosen Price	Selling Price
Cartel	53.47***	37.82***
	(7.766)	(1.440)
BIGS	1.394*	-0.00475
	(0.583)	(0.926)
Supergame	-5.748	-5.130
	(6.486)	(3.479)
Period	-1.570	-1.471***
	(0.932)	(0.277)
Period	0.693	0.664**
	(0.489)	(0.308)
SMALLS × Cartel	-6.641***	-8.151***
	(0.892)	(1.369)
Treatment		24.21***
		(1.272)
Constant	51.00***	42.49***
	(6.581)	(3.696)
Observations	4176	3483
Adjusted R^2	0.544	0.665

Table 2.4: Price Regressions.

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

Tables

Control Group vs. Treatment Group

chosen prices

chosen	avg. Price All (sd)	avg. Price BIGS (sd)	avg. Price SMALLS (sd)
Control	37.76054 (26.91135)	38.63027 (27.52155)	36.8908 (26.27138)
Treatment	86.34339 (20.38314)	89.53257 (19.56247)	83.15421 (20.69521)

Table 2.5: Average chosen prices in control group and treatment group.

selling prices

selling	avg. Price All (sd)	avg. Price BIGS (sd)	avg. Price SMALLS (sd)
Control	33.97987 (25.16332)	34.35199 (24.90563)	33.59924 (25.43444)
Treatment	86.84469 (20.32311)	90.25685 (19.18873)	83.14317 (20.87463)

Table 2.6: Average selling prices in control group and treatment group.

2.10.3 Proofs

Proof of Proposition 2.1. To show that $p = 0$ for all firms in the static Nash equilibrium, suppose a firm sets a price of $p \neq 0$.

Suppose a firm sets a price higher than costs, $p_i > 0$. This eliminates demand and yields no profit, as capacities are such that $\sum_{j \neq i} k_j \geq M > k_i$. The other firms in the market can supply the entire demand.

Otherwise, suppose a firm sets a price lower than costs, $p_i < 0$. Consequently, this leads to losses for the firm.

Hence, each firm sets a price at $p = 0$.

□

Proof of Proposition 2.2. We show that an equilibrium exists where cartel insiders permanently set the collusive profit-maximizing price and outsiders price below the cartel price.

Outsiders:

First we look at the prices of outsiders. Outsiders undercut the cartel price by a minimal amount ϵ, setting a price of

$$p_{i \in O} = p_\Gamma - \epsilon$$

and sell at capacity to maximize their individual profit. Suppose outsiders choose a different price. This reduces profits for all possible cases:

Case 1: Outsiders set a lower price, $p_{i \in O} < p_\Gamma - \epsilon$. Then the same output will be sold at a lower price and profits will be lower.

Case 2: Outsiders set a price on the cartel level, $p_{i \in O} = p_\Gamma$. Then the firm is forced to reduce sales according to the proportional demand allocation rule. The marginal increase in price cannot compensate these losses in quantity and profits will be lower.

Case 3: Outsiders set a price above the cartel price, $p_{i \in O} > p_\Gamma$. This results in no demand since cartel members can satisfy the entire demand at this price. Hence, no profit can be generated with this pricing strategy.

Hence, profits of each outsider in every period then amount to

$$\Pi_{i \in O} = [p_\Gamma - \epsilon]\, k_i.$$

Insiders:

Knowing that the outsiders will always undercut the cartel, it cannot sell at capacity when setting high collusive prices but will only serve residual demand. However, due to the insufficient capacities of the outsiders to satisfy the entire demand, the cartel still faces positive residual demand. The cartel price maximizes

$$p_\Gamma = p(K_\Gamma) = \text{argmax} \left(\frac{1}{1-\delta} \right) \cdot p \cdot \left[\frac{M - K_O}{K_\Gamma} \right].$$

With elastic demand the collusive price depends on the capacities under control of the cartel. In our model with inelastic demand, the monopoly price for residual demand simply is the reservation price,

$$p_\Gamma = \bar{p}.$$

Suppose the cartel chooses a different price:

Case 1: A price higher than the reservation price, $p_\Gamma > \bar{p}$, would result in demand of zero, as no consumer is willing to pay more than the reservation price $\bar{p}$. Hence, profits are zero.

Case 2: The cartel chooses a lower price, $p_\Gamma < \bar{p}$. This would merely lower profits but would not attract any more customers.

Due to the distribution rule of demand, each cartel member's individual profit in each period is

$$\Pi_{i \in \Gamma} = \bar{p} \cdot [M - K_O] \left(\frac{k_i}{K_\Gamma} \right).$$

□

Proof of Proposition 2.3. We solve for the unique, symmetric equilibrium in which two outsiders with symmetric capacities have symmetric distribution functions. The proof consists of several steps.

(i) - First, we characterize the equilibrium profits and support of the cartel. The cartel maximizes its worst-case payoff, in the situation where the outsiders play the strategies which cause the greatest harm to the cartel. Hence, the maxmin-payoff is the payoff that the cartel can always secure independent of the price setting of the outsiders. That is, serving the residual demand at the highest feasible price.

$$maxmin\ \Pi_\Gamma \geq (M - K_O) \cdot \bar{p}$$

It follows that the cartel does not set a price below $\underline{p}_\Gamma \geq \frac{M-K_O}{M} \cdot \bar{p}$.

(ii) - In the next steps, we characterize the equilibrium profits and support of the outsiders price strategy. Let k_{o1} and k_{o1} denote the capacity of outsider 1 and outsider 2, respectively. If the cartel's lowest price is $\underline{p}_\Gamma$, outsiders can always sell their capacity at this price. Consequently, their payoff is defined as

$$\underline{\Pi}_{o1} \geq \underline{p}_\Gamma \cdot k_{o1}$$
$$\underline{\Pi}_{o2} \geq \underline{p}_\Gamma \cdot k_{o2}$$

The lower bound for outsider prices are $\underline{p_{o1}} \geq \underline{p}_\Gamma$ and $\underline{p_{o2}} \geq \underline{p}_\Gamma$.

The lower and upper support of the price set is defined as follows:
Let $\underline{p} = max\{\underline{p}_i\}$, where $\underline{p}_i = \inf p_i$ and let $\bar{p} = max\{\bar{p}_i\}$, where $\bar{p}_i = \sup p_i$.

(iii) - Next, we want to show that outsiders do not set a price of 100 as they either sell nothing or sell less than capacity due to the proportional allocation rule. Hence, they are better off setting a price below 100.

Lemma 2.1. *Outsider 1 and outsider 2 set* $\bar{p}$ *with probability* 0*.*

We prove lemma 1 by contradiction for the three possible cases:

Case 1: only outsider 1 sets $\bar{p}$ with positive probability > 0. The cartel and outsider 2 set a price below $\bar{p} = 100$. This implies that outsider 1 makes a profit of 0. This is a contradiction to (ii). The same argument holds analogously for outsider 2.

Case 2: only outsider 1 and outsider 2 set $\bar{p}$ with positive probability > 0. The cartel sets a price below $\bar{p} = 100$. This implies that outsider 1 and outsider 2 each make a profit of 0. This is a contradiction to (ii).

Case 3: suppose the cartel sets $\bar{p}$ with positive probability > 0. If at least one other firms sets the same price, the proportional allocation rule applies. Due to the capacity allocation, this implies that outsiders are better off with $\bar{p} - \epsilon$.

To see this, let $F^{\Gamma}(100) - F^{\Gamma -}(100)$ be the probability that the cartel sets the highest price of 100 and $F^{o2}(100) - F^{o2-}(100)$ be the probability that outsider 2 sets the highest price of 100.

Then, a price of $\bar{p} - \epsilon$ is more profitable for outsider 1 if

$$\begin{aligned}
100 \cdot \left[F^{\Gamma}(100) - F^{\Gamma -}(100)\right] \cdot \left[F^{o2}(100) - F^{o2-}(100)\right] \cdot \frac{k_{o1}}{k_{\Gamma} + k_{o1} + k_{o2}} \cdot M \\
+100 \cdot \left[F^{\Gamma}(100) - F^{\Gamma -}(100)\right] \cdot \left[1 - (F^{o2}(100) - F^{o2-}(100))\right] \cdot \frac{k_{o1}}{k_{\Gamma} + k_{o1}} \cdot (M - k_{o2}) \\
+100 \cdot \left[1 - (F^{\Gamma}(100) - F^{\Gamma -}(100))\right] \cdot \left[F^{o2}(100) - F^{o2-}(100)\right] \cdot \frac{k_{o1}}{k_{o1} + k_{o2}} \cdot 0 \\
+100 \cdot \left[1 - (F^{\Gamma}(100) - F^{\Gamma -}(100))\right] \cdot \left[1 - (F^{o2}(100) - F^{o2-}(100))\right] \cdot \frac{k_{o1}}{k_{o1}} \cdot 0 \\
< (100 - \epsilon) \cdot \left[1 - F^{\Gamma -}(100 - \epsilon)\right] \cdot k_{o1}
\end{aligned}$$

When ϵ approaches 0, this is true:
$\lim_{\epsilon \to 0} (100 - \epsilon) \left[1 - F^{\Gamma -}(100 - \epsilon)\right] \cdot k_{o1} = 100 \left[1 - F^{\Gamma -}(100)\right] \cdot k_{o1}$

We next show that the cartel sets $\bar{p}$ with probability > 0.

Lemma 2.2. *The strategy of the cartel has a mass point at $\bar{p} = 100$.*

We prove this Lemma by contradiction. Suppose that the cartel does not set the maximal price $\bar{p}$ with positive probability. Then profits of outsider 1 (and analogously of outsider 2) go to 0 as $p_{o1} \to \bar{p}$, since $\lim_{p_{o1} \to \bar{p}} \left[1 - F^{\Gamma}(p_{o1})\right] = 0$. Because outsider 1 (and outsider 2) must earn its equilibrium profits $\hat{\Pi}_{o1} > 0$, there exists an interval $(\tilde{p}, \bar{p})$ with $\tilde{p} < \bar{p}$ in which outsider 1 sets prices with probability 0 (same holds for outsider 2).

To see this, take the largest such interval in which outsider 1 and outsider 2 do not set prices. Then the cartel also does not set a price in $(\tilde{p}, \bar{p})$ either, because it can serve the same demand while shifting probability mass in $(\tilde{p}, \bar{p})$ to $\bar{p}$. This is a contradiction, because the cartel does not have a mass point at $\bar{p}$ by assumption.

Alternatively, conclude that all firms price above $\tilde{p}$ with probability 0. This is also a contradiction because the cartels profit depends on its maximum price.

(iv) - We show that all firms have the same upper support of the distribution function, i.e., $\bar{p}_{o1} = \bar{p}_{o2} = \bar{p}_{\Gamma} = \bar{p} = \bar{p}_i$

Suppose otherwise.

Case 1: $\bar{p}_{o1} > \bar{p}_{\Gamma}$. In this case, there exists and interval $(\bar{p}_{\Gamma}, \bar{p}_{o1}]$ where outsiders have zero demand and consequently profits of $\Pi_{o1} < \underline{p}_{o1} \cdot k_{o1}$. A contradiction to (ii).

Case 2: $\bar{p}_{o1} < \bar{p}_{\Gamma}$. In this case, there exists and interval $(\bar{p}_{o1}, \bar{p}_{\Gamma})$ where outsider 1 and outsider 2 do not set a price. Hence, the cartel does not set a price in this interval and shifts probability mass to $\bar{p} - 2\epsilon$ in order to sell the same demand at a higher price.

This implies that no firm has a mass point at $\tilde{p}$. If no firm has a mass point at $\tilde{p}$, then firms are better off moving probability mass from $(\tilde{p} - \epsilon, \tilde{p})$ to $(\bar{p} - \epsilon 2, \bar{p})$. This contradicts that $(\tilde{p}, \bar{p}]$ is the maximal interval in which no firm prices. Hence, we conclude that (vi) holds.

So far we have shown that:
$\bar{p} = \bar{p}_{\Gamma} = \bar{p}_{o1} = \bar{p}_{o2}$

The cartel has a mass point at $\bar{p}$ — outsiders do not. This implies that $\bar{p} = 100$, i.e., the upper bound is the maximum price. Otherwise, the cartel could profitably raise its price.

$\underline{p}_{\Gamma} \leq \underline{p}_{o1} \;\wedge\; \underline{p}_{\Gamma} \leq \underline{p}_{o2}$

(v - a) We now show that the cartel cannot have a mass point at $\underline{p} = \underline{p}_{\Gamma}$

Suppose otherwise. Outsider 1 and outsider 2 do not want to set $\underline{p}$ with positive probability > 0 and they are better off moving probability mass from $(\underline{p}, \underline{p}+\epsilon)$ to $\underline{p}-\epsilon$ to avoid rationing. If outsider 1 or outsider 2 price below $\underline{p}+\epsilon$ with probability 0, the cartel can increase profits by moving probability mass from $\underline{p}$ to $\underline{p}+\epsilon/2$.

(v - b) Similarly, outsider 1 and outsider 2 cannot have a mass point at $\underline{p}$.

Suppose otherwise. If outsider 1 has a mass point at $\underline{p}_{o1}$, we have the interval $(\underline{p}_{o1}, \underset{\sim}{p})$ where no firm sets a price. Then the cartel moves its lowest price from $\underline{p}_{\Gamma}$ to $\underline{p}_{\Gamma}+\epsilon$. In this case, the outsiders move their lowest price from $\underline{p}_{o1}$ to $\underline{p}_{o1}+\epsilon$. This contradicts our starting definition of profits (ii), $\underline{\Pi}_{o1} \geq \underline{p}_{\Gamma} \cdot k_{o1}$.

We conclude $\underline{p}_{\Gamma} = \underline{p} = \underline{p}_{o1} = \underline{p}_{o2}$ and no firm has a mass point at $\underline{p}$.

This implies that $\hat{\Pi}_{\Gamma} = 100 \cdot (M - k_{o1} - k_{o2}) = \underline{p} \cdot M$ and $\underline{p} = 100 \cdot \frac{(M-k_{o1}-k_{o2})}{M}$

(vi) - We now define the distribution function of the cartel.

Let $j \neq \Gamma$ be a firm for which $\underline{p}_j = \underline{p}$. Then $\hat{\Pi}_j = \underline{p} \cdot k_j = 100 \cdot \frac{(M-k_{o1}-k_{o2})}{M} \cdot k_j$

By symmetry, outsiders have the same capacities, $k_{o1} = k_{o2}$, and outsider 1 and outsider 2 use the same strategy, $F^{o1}(p) = F^{o2}(p) = F^{O}(p)\ \forall\ p$. Consider the profit of minimally undercutting $\bar{p} = 100$.

$$\begin{aligned} \underline{p} \cdot k_j = &\Big[1 - F^{\Gamma-}(100-\epsilon)\Big] \cdot (100-\epsilon) \cdot k_j \\ &+ \Big[F^{\Gamma}(100-\epsilon) - F^{\Gamma-}(100-\epsilon)\Big] \cdot (100-\epsilon) \\ \cdot \left\{ \frac{k_j}{k_i+k_j} \cdot M \cdot \Big[1 - F^{O}(100-\epsilon)\Big] + \frac{k_j}{k_i+k_j} \cdot (M - k_{l\neq i,j}) \cdot F^{O}(100-\epsilon) \right\} \end{aligned}$$

The probability for the cartel to set the maximum price is then $\underline{p} \cdot k_j = q \cdot 100 \cdot k_j$, where q is the probability that the cartel sets $\bar{p}$.

$$\underline{p} \cdot k_j = \Big[1 - F^{\Gamma-}(p)\Big] \cdot p \cdot k_j + \Big[F^{\Gamma}(p) - F^{\Gamma-}(p)\Big]$$
$$\left[\frac{k_j}{k_i + k_j} \cdot M \cdot \Big[1 - F^{O}(p)\Big] + \frac{k_j}{k_i + k_j} \cdot (M - k_{l \neq i,j}) \cdot F^{O}(p)\right] \cdot p$$

If the cartel does not set the maximum price, the distribution function without the mass point is

$$\begin{aligned} \underline{p} \cdot k_j &= \Big[1 - F^{\Gamma-}(p)\Big] \cdot p \cdot k_j \\ \frac{\underline{p}}{p} &= 1 - F^{\Gamma-}(p) \\ F^{\Gamma-}(p) &= 1 - \frac{\underline{p}}{p} \end{aligned}$$

This sums up to 1 since $F^{\Gamma-}(p) = 1 - \frac{\underline{p}}{100} + q = 1$. Hence, the probability that the cartel charges a price p is given by the distribution function

$$F^{\Gamma}(p) = \begin{cases} 1 - \frac{(M-K_O)\cdot 100}{M \cdot p} & \text{if } p \in [\underline{p}, \bar{p}) \\ 1 & \text{if } p = \bar{p} \end{cases}$$

Equilibrium profits are $\Pi_{\Gamma} = \underline{p} \cdot M$.

(vii) - Finally, we derive the Distribution function of the outsiders.

We consider the case where $k_{o1} = k_{o2} = k_o$ and $F^2(\cdot) = F^3(\cdot) = F^O(\cdot)$. We have the following conditions for the outsiders

$$\begin{aligned} 100 \cdot (M - k_o - k_o) = (1 - F^O(p)) \cdot F^O(p) \cdot (M - k_o) \cdot \\ +(1 - F^O(p)) \cdot F^O(p) \cdot (M - k_o) \cdot \\ +(1 - F^O(p)) \cdot (1 - F^O(p)) \cdot M \cdot p \\ +F^O(p) \cdot F^O(p) \cdot (M - k_o - k_o) \cdot \end{aligned}$$

Hence, the probability that outsiders charge a price p is given by the distribution function

$$F^O(p) = F^{o1}(p) = F^{o2}(p) = \begin{cases} \frac{M}{K_O} - \frac{100\cdot(M-K_O)}{p\cdot K_O} & \text{if } p \in [\underline{p}, \bar{p}] \end{cases}$$

Note that $F^O(\underline{p}) = 0$ and $F^O(\bar{p}) = 1$. Outsiders individual profit is $\Pi_{i\notin\Gamma} = \underline{p} \cdot k_i$.

□

2.10.4 Experimental Instructions

Instructions for all groups — differences between treatments in italics — translated from German

Hello and welcome to our experiment!

Please read this instruction set very carefully to the end.In this experiment you will repeatedly make decisions to earn money. How much you earn depends on your decisions and on the decisions of three other randomly assigned participants. Please do not talk to your neighbors and be quiet during the entire experiment. If you have a question, please raise your hand. We will then come to your booth and answer your question personally. All participants receive (and are currently reading) the same instructions. You will remain completely anonymous to us and to the other participants. We do not save any data in connection with your name. At the end of the experiment, you will get your profit paid in cash.

Market

In this experiment you will have to make decisions for one of four firms in a market. All four firms sell the same product and there are no costs of producing this good. This market is made up of 300 identical consumers, each of whom wants to purchase one unit of the good at the lowest price. The consumers will pay as much as 100 Experimental Currency Units (ECU) for a unit of the good.

Firm 1 and firm 2 are able to produce 200 units of the product and can supply an according number of consumers each. Firm 3 and firm 4 are able to produce 50 units of the product and can supply 50 consumers each. Your earnings are calculated as the product of your sold units and your selected price.

Distribution of consumers

In each period, all firms have to set their price, at which they want to offer their units. The firm who set the lowest price will sell its capacity at the selected price. All consumers who haven't bought a unit yet will then buy from the firm with the second lowest price. When there are still consumers left who haven't bought a unit yet, consumers buy from the firm with the next lowest price. If more than one firm set the same price and if the number of consumers firms can supply is higher than the number of consumers who haven't bought the good, they will split the available consumers proportionally to the firms' capacity. An example is given later. At the end of each period, all the firms are informed of the chosen prices by all firms in their group, the number of consumers each firm served (= sold units), profits of each firm and their own cumulated profits over all periods. For simulations of your potential profits, we will provide you with a "Profit Calculator", where you can check possible combinations of prices chosen by firms and the associated profits, prior to your price selection.

Communication ***(only for treatment group)***
Prior to your price decision, firm 1 and firm 2 will be able to communicate with each other in the market. For that purpose, we will provide participants representing these firms with a chat box, which can be used to send messages to the other person representing firm 1 or firm 2. If you are firm 3 or firm 4 you will not be able to communicate or read messages and just have to wait. Only firm 1 and firm 2 in each market will be able to see the sent messages. In the first 3 periods of each game firms are allowed to communicate for 60 seconds, in each additional period they have 30 seconds for this purpose. They are allowed to post how many messages they like and talk about what they like. There are only two restrictions on messages: they may not post messages which identify themselves (e.g. age, gender, location etc.) and they may not use offensive language. After the assigned time expires, the chat box will close and all firms will have to choose their price.

Groups
You will be randomly assigned to one of the firms at the beginning of the experiment and remain assigned to this firm for the entire experiment. The experiment is divided into 3 games, that have multiple periods each. Throughout a game you will be matched with the same three other firms in every period. However, you will be assigned to a new group before each game.

Duration
After every period, the computer will draw a ball of a virtual urn with 9 balls which are numbered from 1 to 9, to determine whether the experiment continues. If a value of 9 is shown, the experiment is over. If any other value is shown, the experiment continues. The ball is then returned to the urn. The odds of playing another periods is therefore ˜89% in each period. At the end of the experiment, which is after 3 games, you will be told of the sum of profits made during the experiment, which will

be your payment. You will receive 1 Euro for every 15,000 ECU you earn during the experiment. You will also receive 5 Euro for participating.

Examples
For a better understanding, two illustrative examples follow:

Example 1:
Suppose that the firms choose the following prices: Firm 1 sets a price of 85 ECU, firm 2 chooses a price of 100 ECU, firm 3 chooses a price of 75 ECU and firm 4 chooses a price of 95 ECU. Firm 3 set the lowest price and therefore faces a demand of 300 consumers. It has only capacity to produce 50 units. Therefore it sells all its 50 units at a price of 75 ECU, making a profit of 50 * 75 ECU = 3,750 ECU. Firm 1 has the second lowest price and will face a demand only of 300-50=250 consumers. Firm 1 has a capacity of 200 and can supply 200 consumers at its price of 85 ECU, therefore making a profit of 200 * 85 ECU = 17,000 ECU. Firm 4 has the lowest remaining price and sells all its 50 units at a price of 95 ECU making a profit of 50 * 95 ECU = 4,750 ECU. There is no consumer in the market left who has not bought a unit of the good, therefore firm 2 sell no units at its price and has profits of zero, 0 * 100 ECU =0 ECU.

Example 2:
Suppose that the firms choose the following prices: Firm 1 sets its price at 38 ECU. Firm 2 and firm 3 both set their price at 65 ECU. Firm 4 sets its price at 99 ECU. Firm 1 sets the lowest price. All 300 consumer want to buy its units. Therefore it can sell all its units and has a profit of 200 * 38 ECU = 7,600 ECU. Given that firm 2 and firm 3 set the same price and also given that their combined capacity (200+50=250 units) is larger than the number of consumers (300-200=100), they will have to share the available consumer according to their capacities. Firm 2 has a capacity of 200, firm 3 has a capacity of 50. Hence, firm 2 will sell 200/(200+50)*100 = 80 units at a price of 65 ECU, therefore making a profit of 80 * 65 ECU = 5,200 ECU. Firm 3 will sell 50/(200+50)*100 = 20 units at a price of 65 ECU making a profit of 20 * 65 ECU = 1,300 ECU. All consumers are satisfied. Firm 4 sells no unit at its price of 99 ECU and thus makes no profit (0 * 99 ECU = 0 ECU).

Good luck!

Chapter 3

An Experiment on Partial Cross-Ownership in Oligopolistic Markets

Co-authored with Volker Benndorf

3.1 Introduction

Minority shareholdings have recently received much attention by scholars, competition authorities, and organizations. For example, Posner et al. (2017) argue that the U.S. should introduce a public enforcement policy of the Clayton Act to mitigate anti-competitive effects of minority shareholdings. At the same time, the authors also warn that regulation needs to minimize the resulting disruptions to equity markets. In Europe, the European Commission has released two papers exploring a stricter regulation of non-controlling shareholdings, and it has already expressed concerns about anti-competitive effects of such links in previous merger cases.[1] However, the responsible commissioner has also voiced reservations concerning novel legislation as it might hinder companies' business interests.[2]

Both cases suggest that it is not clear whether the anti-competitive effects of non-controlling investments are strong enough to warrant novel legislation. The point at issue is that novel regulation of minority shareholdings needs to consider both positive and negative effects of non-controlling investments. The upsides of minority shareholdings were subject of a recent report by the OECD (2017). They include efficiency gains, diversification of risks, reinforcement of business relationships, and access to new markets and technologies. The downside of non-controlling investments is that there may be anti-competitive effects. So far, such effects have been addressed in theoretical and empirical studies, and they are also the central topic of this paper.

The present article provides experimental evidence for the anti-competitive effects of minority shareholdings between direct competitors. We use a simple version of a model by Gilo et al. (2006) and vary the number of shares firms own of each other. All such degrees of *partial cross-ownership* (PCO) are considered in a static and a repeated-game framework which enables us to make ceteris paribus comparisons and clearly distinguish between coordinated and unilateral effects. To our best knowledge, we are the first to experimentally examine the impact of horizontal (passive) partial cross-ownership in oligopolistic markets.

We find strong evidence that partial cross-ownership may induce substantial

[1] See European Commission (2014, 2016) for the white papers and European Commission (2005, 2013) for the merger cases.

[2] During the 2016 ABA spring meeting, Commissioner Vestager recognized that such legislation would come with a considerable amount of red tape and would put a high administrative burden on businesses. See e.g., Knox (2016).

coordinated and unilateral anti-competitive effects. Average selling prices are positively correlated with the degree of partial cross-ownership in the static game and in the repeated game. In the repeated game, the price increases are primarily driven by collusive behavior. In the static game, the price increases are attenuated because collusive behavior does not play role. However, the price levels observed in the static game are higher the higher the degree of cross-ownership. This finding is inconsistent with the Nash predictions, but well predicted by Quantal Response Equilibrium (McKelvey & Palfrey, 1995).

We conclude that PCO reduces the incentives to compete and favors unilateral effects, which result in higher average prices. Tacit collusion, facilitated by partial cross-ownership, can further increase these unilateral effects but is not necessary for higher prices in this setup. Hence, the experimental data adds to the evidence for negative repercussions of passive minority shareholdings for consumers and its importance for competition policy.

The remainder of this paper is structured as follows. Section 3.2 discusses some practical aspects of minority shareholdings and gives more background information. Section 3.3 summarizes the related literature. In Section 3.4 we discuss the theoretical aspects of the game. We first present the model our experiment is based on, and then discuss the theoretical predictions. This includes standard approaches like Nash equilibrium and subgame-perfect equilibrium, but we also consider an alternative equilibrium concept for the static game. Section 3.5 gives an overview of our experimental design and procedures. In Section 3.6 we derive our hypotheses from theory and its implementation in our experiment. The results are shown in Section 3.7. Section 3.8 concludes.

3.2 Practical Relevance and Background Information

At present, the regulation of partial acquisitions and minority shareholdings is typically restricted to cases where the acquiring firm gains some form of control over the target. In Europe, most national competition authorities, plus the European Commission itself, do have a handle on partial acquisitions if such a change in control occurs. However, competition authorities in most jurisdictions cannot intervene when passive or non-controlling stakes are acquired.[3] The same is true for the United States. Here, minority shareholdings are regulated under Section 7 of the Clayton Act which explicitly excludes acquisitions that are solely for investment purposes. As a consequence, partial acquisitions have gone unchallenged many times even though they may come with anti-competitive effects.[4]

Non-controlling minority shareholdings are of interest in different economic settings. One important distinction is whether direct competitors acquire stakes in each other (cross ownership) or if a third party — for example, an institutional investor — acquires stakes in each of these competitors (common ownership). Both cases are highly relevant in real markets and can occur separately or simultaneously.[5] Common ownership has recently received much attention because of the success of index and other mutual funds (see e.g., Posner et al. (2017)). However, cross-ownership, which is the focus of the present paper, is also highly relevant because of its prevalence.

In modern economies, there are numerous examples where competing firms invest in mutual shareholdings. For example, the global automobile industry is characterized by a high degree of cross-ownership. In the "Alliance 2022" Renault owns 43.3% of Nissan while Nissan owns 15% of Renault and 34% of Mitsubishi. Daimler owns 3.1% of each Renault and Nissan who both have a 1.55% share in Daimler. Similarly, in 2009 Volkswagen bought 20% of Suzuki who in turn bought 2% of Volkswagen. Also, Alley (1997) reports that there was a sophisticated network of cross-shareholdings in the U.S. automobile industry. Gilo (2000) lists some further examples from the United States. They include Microsoft's acquisition of 7% of Apple's shares in 1997, Northwest Airlines' acquisition of 14% of shares from Continental Airlines in 2000, TCI purchase of 9% of Time Warner shares in 1996 and Gillette's 22.9% interest in Wilkinson Sword in 1990. Further examples include the insurance industry,[6] the airline industry,[7] Chinese travel-service providers,[8] the

[3] In some countries like Austria, Germany, the UK, Japan and Canada acquisition of non-controlling minority shareholdings is subject to merger control at a certain (high) threshold (compare European Commission, 2014).
[4] See, e.g., Gilo (2000).
[5] For example, Azar et al. (2016) observe both cases in the U.S. banking sector.
[6] Allianz and MunichRe owned reciprocal shareholdings of 25% in 2000.
[7] AirAsia and Malaysa Airlines swapped shares in 2011.
[8] There was a share swap between Qunar and Ctrip in 2015.

banking industry,[9] and the Scandinavian power market.[10]

Anti-competitive effects are usually distinguished between unilateral and coordinated effects. This separation originates from the theoretical literature on merger control. Unilateral effects arise when a firm gains additional market power and can raise prices without needing to coordinate with other competitors. In the setup we analyze, the Nash equilibrium of the static game does not change with the degree of cross-ownership. Hence, standard economic theory suggests that unilateral effects should not occur in this setup. Coordinated effects refer to the likelihood of tacit collusion, i.e., whether or not firms find it easier to coordinate their behavior in an anti-competitive way. Gilo et al. (2006) show that the critical discount factor for tacit collusion decreases with the degree of passive partial cross-ownership in a repeated Bertrand model with homogeneous goods. Hence, economic theory suggests that coordinated effects may arise in our experiment. However, note that this does not imply that firms will actually manage to coordinate on such outcomes. In fact, there are many other subgame-perfect Nash equilibria and the critical discount factor is already relatively low for the benchmark case without PCO.[11] It is therefore unclear whether minority shareholdings will actually lead to more collusive outcomes.

The experimental approach complements the theoretical and empirical literature on this topic. The main assets of lab experiments are control and internal validity such that the lab environment can, for example, help avoiding problems with missing or confounded field data. The effects revealed by our experiment are exclusively driven by the fact that competing firms internalize each others' profits. Confounds

[9]Ezrachi & Gilo (2006) report that Credit Agricole bought shares of Credit Lyonnais and Dietzenbacher et al. (2000) report multiple examples from the Dutch financial sector.

[10]See Amundsen & Bergman (2002).

[11]In the experiment, we induce a discount factor of 0.9 while the predicted critical discount factors for the different levels of partial cross-ownership range between 0.167 and 0.5. As a consequence, collusion on the monopoly price is always sustainable as a subgame-perfect equilibrium independent of the degree of partial cross-ownership.

like explicit collusion,[12] indirect shareholdings,[13] control rights,[14] or efficiency gains may be problematic in the real world, but they cannot affect the experimental data. The experiment also complements the theoretical literature on the anti-competitive effects of cross-ownership. The reason is that the standard predictions offer only limited guidance for the setup we analyze. The standard predictions for the static game, are counter-intuitive and conflict with the predictions of quantal response equilibrium. Our experimental data reveals that unilateral effects do play a role in the static game as prices are indeed higher the higher the degree of cross-ownership. As argued above, it is unclear whether partial cross-ownership will actually cause firms to become more collusive in the repeated game. In the experiment, we find that higher degrees of PCO actually do lead to more successful tacit collusion and that it does translate into even higher prices compared to the static game. In summary, the lab environment not only helps isolating the unilateral and coordinated effects of partial cross-ownership, but it also helps with selection issues arising from competing predictions.

3.3 Related Literature

Our research adds to the literature on non-controlling minority shares in horizontal markets. The distinction between controlling and non-controlling minority shareholdings are of importance both from an antitrust and an economic point of view. O'Brien & Salop (1999) argue that the anti-competitive effects of a partial acquisition could be larger when the acquiring firm obtains at least some control over the pricing decision of the target firm than without control. Assuming firms are able to obtain effective corporate control over all pricing decisions through voting stocks,

[12]Minority shareholdings may open new communication channels or give access to information, e.g., the European Directive on the exercise of certain rights of shareholders in listed companies (2007/36/CE) amended by the Directive 2017/828/EU says that shareholders shall have timely access to all information relevant to general meetings.

[13]In general, indirect shareholdings (for instance common ownership of institutional investors) and control rights also induce that competing firms internalize each others profits even if they do not directly participate in the other firm's profits. This makes it in practice very difficult to measure the ultimate degree of profit internalization between competing firms. The focus on passive cross-ownership in our experimental design allows us to directly control the ultimate degree of profit internalization between the firms.

[14]The minority shareholdings in our setup are only held by rivals and are truly passive in that the acquiring firms exert zero control over the target's pricing decisions and only have cash flow rights. In practice, minority shareholdings may often provide some control to the acquiring firm because it may gain board seats or the like.

Foros et al. (2011) show that partial ownership could result in even less competitive outcomes than a full merger because firms may increase their price above the price which would maximize joint profits and reduce competition even more. Brito et al. (2010) shed further light on the differences between voting shares and non-voting shares and conclude that it does not matter whether firms acquire a minority or majority share, as only the voting rights are crucial for the outcome. Stühmeier (2016) argues that the total effect on competition depends on both the financial stake and the level of corporate control of the acquiring firm in the target. We consider the case where firms have no control over their competitor even when they own more than 50% of the shares in order to consider a lower bound of effects.

We also add to the literature separating the anti-competitive effects of minority shareholdings between unilateral and coordinated effects. Coordinated anti-competitive effects of passive cross-ownership arise in Cournot markets as well as in Bertrand markets. Malueg (1992) analyzes tacit collusion between firms with passive partial ownership in a dynamic, symmetric Cournot model. Here, the demand function is of importance to enable sustainable collusion. We base our analysis on the model by Gilo et al. (2006) who show that the critical discount factor for tacit collusion decreases with the degree of passive partial cross-ownership in a repeated Bertrand model with homogeneous goods. However, since the Nash Equilibrium of the static Betrand game does not change with the degree of partial cross-ownership, standard economic theory suggests that unilateral effects should not occur in this setup—a prediction we look at in more detail.

Unilateral effects are of greater importance in Cournot markets compared to Bertrand markets. Reynolds & Snapp (1986), Farrell & Shapiro (1990), and Flath (1992) consider unilateral effects in static models with quantity competition. They find that as the degree of cross-ownership among competitors increases, competition is softer than in a perfectly competitive market and production levels fall toward the monopoly outcome because of firms' financial interests in their competitors. This is typically not the case in Bertrand markets. If firms compete in prices, it has been shown that unilateral effects only arise if products are heterogeneous. In this case, cross-ownership may induce firms with stakes in their competitors to set higher prices (Flath, 1991; Dietzenbacher et al., 2000) while there is no such effect in symmetric markets with homogeneous products. Shelegia & Spiegel (2012) find that price levels can be up to the monopoly outcome because firms internalize some of the competitive externality they exert on the other firms in the market with cost asymmetries. We consider a model with symmetric costs and homogeneous products. Hence, economic theory predicts that coordinated effects may arise in our experiment, whereas unilateral effects should not occur. We provide alternative predictions by using Quantal Response Equilibrium instead of Nash Equilibrium and show that unilateral effects can emerge (see Section 3.4.2).

The empirical research on partial cross-ownership in horizontal markets confirmed previous predictions from theory and showed anti-competitive effects in different industries. However, these papers do not always distinguish between the different types of effects (coordinated/unilateral) or shares (controlling/non-controlling). Brito et al. (2014) specifically estimate the unilateral effects of passive partial cross-ownership in the wet shaving industry and find higher prices. Other authors find an increased price-cost margin in the respective investigated industry, e.g., Dietzenbacher et al. (2000) for the Dutch financial sector or Nain & Wang (2016) in U.S. manufacturing industries due to partial cross-ownership.

Coordinated effects have been examined only in studies that do not distinguish between controlling and non-controlling shares. Alley (1997) finds anti-competitive effects of partial cross-ownership in the Japanese and U.S. automobile industries, which include active shares. They also cannot exclude possible information flows. Parker & Röller (1997) consider the U.S. cell phone industry and find higher prices for companies that are related through joint ventures, in which setup obviously communication and control is present. Trivieri (2007) confirms former results with data of the Italian banking industry and shows that firms with cross-ownership are less competitive than firms without these ties. They also do not separate active from passive shares. Heim et al. (2017) conduct an analysis of partial acquisition over all industries in 63 countries. They find that partial cross-ownership may function as a tool to stabilize collusive agreements but they do not differentiate between active and passive minority shareholdings either. Brito et al. (2018) calculate the discount factors for coordinated effects in the wet shaving industry.

Our research also adds to the literature on collusion in experimental markets. So far, extensive research exists on firm-, product-, and market-characteristics such as asymmetries of firms (Mason et al., 1992), differentiation of goods (Davis & Wilson, 2005) or market size (Dufwenberg & Gneezy, 2000; Huck et al., 2004). Other studies examine the effects of supply and demand functions (compare Engel, 2007), demand uncertainty (Feinberg & Snyder, 2002), the strategic variable (Suetens & Potters, 2007), timing of the decision (simultaneous/sequential) (Kübler & Müller, 2002;

Güth et al., 2006), the information environment (Mason & Phillips, 1997), learning dynamics and long-term behavior (Huck et al., 1999; Friedman et al., 2015), feedback (Huck et al., 2000) and communication (Fonseca & Normann, 2012; Harrington et al., 2016). Also, antitrust policies like leniency have been tested in the laboratory, revealing important insights (Hinloopen & Soetevent, 2008; Hinloopen & Onderstal, 2014). Partial cross-ownership has so far only been the focus of a study on vertical markets (Güth et al., 2007).

In the present paper, we look at truly passive partial cross-ownership and its coordinated and unilateral effects in a homogeneous symmetric Bertrand market. We focus on actual passive ownership, i.e., the investing firm has only a financial interest and cannot influence important parameters of competition (e.g. the pricing decision). Due to the homogeneity of goods and the non-controlling type of shares we expect the effects on competition to be at a lower bound. However, we still report evidence for substantial coordinated and unilateral effects in this setup. In the real world controlling shares are much more present and products are more heterogeneous. Consequently, the anti-competitive effects of partial cross-ownership may be even more severe in the real world.

3.4 The Model

In this section, we briefly present the simple version of the model by Gilo et al. (2006), which we use in the experiments. We further derive the discount factors for tacit collusion and present different solution concepts for the static game depending on the degree of cross-ownership.

3.4.1 Setup

The starting point is a standard Bertrand game with $n = 2$ symmetric firms. All firms produce the same homogeneous product at zero marginal costs. Each firm can supply the entire market. Firms set their prices $p_i, p_j \in \mathbb{R}_+$ simultaneously at the beginning of every period. Demand $Q(p)$ consists of 100 consumers who each buy one unit if the market price does not exceed 100. If the market price exceeds 100, all consumers will purchase zero units. The consumers buy the good at the lowest price only. If more than one firm sets the lowest price, the demand is split equally between these firms. The profits from the standard Bertrand game without any cross-ownership are referred to as “product-market profits” and denoted by $\hat{\pi}_i$.

$$\hat{\pi}_i(p_i, p_j) = \begin{cases} 100 \cdot p_i & \text{if } p_i < p_j \\ 50 \cdot p_i & \text{if } p_i = p_j \\ 0 & \text{if } p_i > p_j \end{cases} \tag{3.1}$$

Gilo et al. (2006) introduce the concept of “accounting profits” to adequately model partial cross-ownership.[15] The accounting profits consist of the product-market profits plus dividends from the firms they have shares of. In the general model, cross-ownership is specified using an ownership matrix. However, in our simplified version, we only consider symmetric duopolies such that a single parameter α is sufficient. In our version, α defines the share of firm i that belongs to firm j and vice versa. The resulting accounting profits are defined by a system of equations of the form $\tilde{\pi}_i = \hat{\pi}_i + \alpha \cdot \tilde{\pi}_j$ with the solution $\tilde{\pi}_i = (\hat{\pi}_i + \alpha \cdot \hat{\pi}_j)/(1 - \alpha^2)$, where $\tilde{\pi}_i$ only depends on product-market profits.

We use a further modification of the model to avoid confusion among the participants in the laboratory. The sum of the accounting profits as defined above will typically exceed the sum of the product-market profits. While this is unproblematic in the context of theoretical analyses, it may appear unintuitive and confusing to the participants of a laboratory experiment. Thus, we use a linear transformation of the accounting profits to align the aggregate final profits to the aggregate product-market profits. The transformation does not change the theoretical predictions in any way. It may be interpreted to result in the “profits of the real owners” of the firm. That means that the final profits only comprise the share of the accounting profits, which is not paid out as a dividend. Technically, we have $\pi_i = (1 - \alpha)\tilde{\pi}_i$. The profit functions used in the experiment are therefore given by

$$\pi_i = \frac{\hat{\pi}_i + \alpha \cdot \hat{\pi}_j}{1 + \alpha} \tag{3.2}$$

[15] The reason for introducing accounting profits is that relying solely on the product-market profits would not adequately capture partial cross-ownership. Consider, for example, a standard Bertrand outcome with $\hat{\pi}_i = 1000$ and $\hat{\pi}_j = 0$ and assume that either firm owns 20% of its rival. If the dividend payments are calculated using only the product-market profits, firm i would get final profits of 800 and firm j would get final profits of 200. Given the final profits, the dividend paid by firm i is too high ($0.2 \cdot 800 < 200$) and firm j pays zero dividends even though it realizes positive profits. The concept of the accounting profits ensures that the dividend payments are in line with the final profits.

3.4.2 Solution Concepts

In this subsection, we report the theoretical predictions for the games used in the experiment. The model for the repeated game follows the repeated-game logic and relies on trigger strategies with maximum punishment (Nash reversion). We first present the Nash equilibria of the static PCO game with continuous prices. A robustness check using a discrete price grid is discussed in Appendix 3.11.1. Changing the action set does not result in meaningful changes of the predictions. Finally, we suggest a behavioral model (Quantal Response Equilibrium), which predicts that prices may be positively correlated with the degree of cross-ownership.

Static Game Equilibrium

We can see from formula (3.2) that in a model with continuous prices, the static Bertrand equilibrium is unaffected by minority shareholdings. Since $0 \leq \alpha < 1$, the accounting profit is more sensitive to changes in the own product-market payoffs compared to the product-market payoffs of the other firm. Consequently, the best-reply schedule of the game with partial cross-ownership is identical to that of standard Bertrand competition. Hence, in a PCO game with continuous prices, the unique Nash Equilibrium is also identical to the usual Bertrand equilibrium where all firms choose prices equaling marginal costs ($p = 0$). This is true even if the shares approach 100% for any $\alpha < 1$. The firms are consequently making zero profits in equilibrium. However, this does not apply to games with discrete action sets. In this case, there may be further equilibria, but the difference between the equilibrium prices with continuous and discrete action sets are negligible for our study. A model using this action set is presented in Appendix 3.11.1.

This result is also true for a model where firms maximize the accounting profits and not just the profits of the *real* shareholders. The derivative with respect to the own product-market payoff is still steeper than the one with respect to the other product-market payoff.

Infinitely-Repeated Game Equilibrium

For the analysis of the repeated game, we assume that firms choose trigger strategies with maximum punishment (Nash reversion). Let π_i^C and π_i^D denote the final profits from collusion and from deviation, respectively. In general, collusion in a sense that all firms charge some price p^C exceeding marginal costs may be implemented as a subgame-perfect equilibrium of the infinitely repeated Bertrand game if the intertemporal discount factor δ is sufficiently high. Since firms make zero profits in the Nash Equilibrium of the stage game, the condition for collusion to be sustainable is

$$\frac{\pi_i^C}{1-\delta_i} \geq \pi_i^D \quad \text{or} \quad \delta_i \geq \hat{\delta} = 1 - \frac{\pi_i^C}{\pi_i^D} \quad \forall\, i \in \{i, j\} \tag{3.3}$$

If $\hat{\pi}^C$ denotes the monopoly payoff in the product market, we get the final profits $\pi_i^C = \hat{\pi}^C/2$ and $\pi_i^D = \hat{\pi}^C/(1+\alpha)$ for collusion and defection, respectively. Substituting these into the expression for the critical discount factor yields the following condition for Nash reversion:

$$\hat{\delta} = \frac{1-\alpha}{2} \tag{3.4}$$

As we can see from formula (3.4), the critical discount factor $\hat{\delta}$ gets lower the higher the degree of partial cross-ownership α is, i.e., collusion gets "easier" with a higher stake. Again, this result is independent of the question of whether firms maximize their accounting profits or the net payoffs of the *real* shareholders.

Quantal Response Equilibrium

We also make predictions about the impact of PCO on chosen prices using Quantal Response Equilibrium (QRE) by McKelvey & Palfrey (1995). QRE is an established concept to incorporate realistic limitations of participants in an equilibrium analysis which bases on rational choice modeling.[16] QRE is a statistical version of Nash Equilibrium where players may make errors in their decisions. The model has one free parameter λ which captures the amount of noise in players' behavior. For $\lambda = 0$, the chosen prices are purely random regardless of their expected payoff. In contrast, QRE will approach a Nash Equilibrium as $\lambda \to \infty$. The intuition behind QRE is that errors which reduce a player's expected profits by a large amount are less likely to be made compared to errors which are less costly. For the PCO game this implies that players may be more likely to choose high prices the higher the degree of cross-ownership. The reason is that higher degrees of PCO imply higher dividend payments if a firm is charging more than the rival. Hence, being undercut by the

[16] For example, Capra et al. (2002) use QRE to predict higher prices in a Bertrand game where the high-price firm's market share is larger than zero. Normann (2011) uses QRE to explain why integrated firms price less competitively than non-integrated firms.

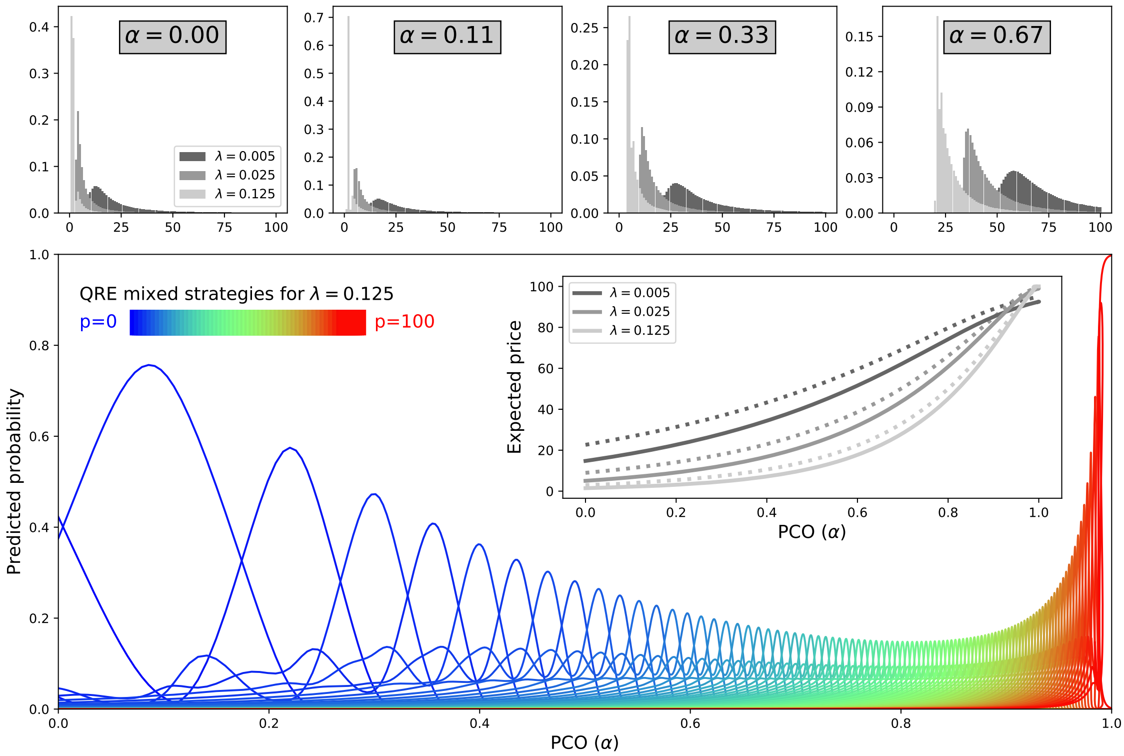

Figure 3.1: QRE predictions.

The upper panels show selected mixed strategies predicted by Quantal Response Equilibrium. The lower panel visualizes the evolution of the predicted mixed strategies and the expected prices over the PCO parameter α. Note that there are 101 colored lines in the figure—one for each action (integer prices in between 0 and 100).

rival is less costly the higher the degree of cross-ownership. At a given level of the QRE parameter λ players should therefore be more likely to choose higher prices as the degree of cross-ownership increases.

Partial cross-ownership changes the payoff structure in a way that does not affect the standard Nash prediction but has an impact on the QRE predictions. Figure 3.1 illustrates the predictions of the behavioral model for a number of values of the QRE parameter λ. The upper four panels display predicted mixed strategies for the values of α which we consider in the experiment. The lower panel shows how the predicted mixed strategies and the resulting expected prices evolve over the PCO parameter α.[17]

[17] We solve the system using the parameters from the experiment. This implies 101 pure strategies representing integer prices between 0 and 100 and four different degrees of partial cross-ownership. To solve the system, we follow the homotopy approach described in Turocy (2005) using a transformation from Turocy (2010) and a software tool provided by Eibelshäuser & Poensgen

The figures document that higher degrees of PCO imply more probability mass on higher prices when keeping λ constant. In the upper panels, it can be seen that the center of the distributions moves to the right as the value of λ increases. The lower panel shows the development of the predicted mixed strategies over the PCO parameter α for $\lambda = 0.05$. The predictions are as follows. For relatively low values of the PCO parameter, relatively low prices (bluish lines) are chosen with high probability. Intermediate prices around $p = 50$ (greenish lines) are chosen with high probability for $\alpha = 0.8$. High prices (reddish lines) are chosen more often when $\alpha \to 1$. This can be interpreted such that participants are expected to choose higher prices the higher the degree of partial cross-ownership. The intuition is that the higher the degree of PCO, the lower the losses of participants who are undercut by the other firm. Since QRE assumes that players best-respond in a noisy fashion, the probabilities for higher prices increase with the degree of PCO.

The inset of the figure visualizes the relation between prices and PCO. Here, we plot the expected prices given the predicted mixed strategies as a function of the PCO parameter α. Note that the solid lines represent selling prices whereas the dotted lines represent chosen prices. It can be seen that higher degrees of PCO practically always imply higher expected prices—independent of the precise value of λ.[18] Hence, the behavioral model extends the theoretical perspective in that it predicts a positive correlation between prices and the degree of PCO in the static game.

3.5 Experimental Design and Procedures

In the experiment, subjects play the game as described in Section 3.4 with choices limited to integer prices in the interval $[0; 100]$. Subjects are paid according to the payoff function $\pi_i = (\hat{\pi}_i + \alpha \cdot \hat{\pi}_j)/(1 + \alpha)$ where α denotes the degree of cross-ownership and $\hat{\pi}$ denotes the profits from a standard Bertrand duopoly with zero marginal costs and an inelastic demand function. We use the following wording to explain the payoff function to the participants. Subjects are told that their "profits" consist of "their income" times an "income factor" plus the "income of the other firm" times an "ownership factor." The income factor and ownership factor are defined as $1/(1 + \alpha)$ and $\alpha/(1 + \alpha)$, respectively. The numerical values are listed in the

(2019).

[18]The exceptions are $\lambda = 0$ and $\lambda \to \infty$ where the expected prices may be identical.

Degree of PCO	Number of periods Part 1	Part 2	Repeated game Subjects	Obs.	Static game Subjects	Obs.
$\alpha = 0$	8	11	36	6	24	2
$\alpha = 1/9$	10	7	36	6	24	2
$\alpha = 1/3$	9	12	36	6	24	2
$\alpha = 2/3$	13	10	36	6	24	2

Table 3.1: Treatments.

instructions and displayed on the screen. The experimental software also includes a payoff calculator where subjects could simulate outcomes by entering hypothetical prices.

In total, the experiment consists of eight treatments which are conducted between-subjects. We have a 4×2 design. The first dimension is the degree of partial cross-ownership. The second dimension is the matching routine. Stranger matching is used to analyze unilateral effects in the static game and partner matching tackles coordinated effects in the repeated game. In the repeated game, one session of 36 subjects results in six independent observations. In the static game, we use matching groups of 12 subjects to avoid spillovers across too many subjects. As a consequence, a session with 24 participants translates into two independent observations for the static game.

We use four different degrees of PCO, including a baseline case where the firms have no shares of their rival ($\alpha = 0$). The values $\alpha = 1/9$ and $\alpha = 1/3$ are chosen to cover a broad spectrum. The last case, where $\alpha = 2/3$ may at first appear to be of purely theoretical interest as in real industries shareholdings exceeding 50% will typically imply a change in control. Yet, even in the real world, exchanging non-voting stocks might result in dividend payments of similar magnitude. Furthermore, it is interesting to include a scenario with a high value of α. While this does not change the Nash Equilibrium, it has a strong influence on the QRE predictions. Therefore, we expect subjects in an experiment to adjust their behavior accordingly.

All sessions are divided into two parts. For example, in the sessions where $\alpha = 0$, the first part comprises the periods one to eight and the second part comprises the periods nine to nineteen.[19] In the repeated game, there is a structural break in that subjects are matched with a new opponent at the beginning of the second part. In the static game, there is no such break because subjects are matched with a random

[19]Participants actually played four parts with two different degrees of PCO. The analysis of parts three and four is the subject of another paper.

opponent in each period anyway. Yet, to ensure the comparability, we apply the same procedures to both matching routines.

The number of periods in the first and second part of a session is determined by a random walk with a continuation probability of 9/10. The reason is that partner matching is supposed to model an infinitely repeated game, and that the continuation probability may be interpreted as an induced discount factor of $\delta = 0.9$ in the repeated game. Again, the interpretation does not apply to the static game, but to minimize the differences between the treatments, we apply the same design to both matching routines. Note that that the randomization was only conducted once for each degree of PCO. This is why the number of periods is identical across different sessions of the same type.

The structure of a single period is as follows. Subjects are first presented with an information screen which reminds them about the degree of cross-ownership as well as the income and ownership factors as defined above. The information screen also highlights whether or not the participant is matched with the same subject as in the previous period. On the next screen, subjects have the possibility to simulate multiple outcomes using a payoff calculator. Afterwards, they have to decide on their final price for the current period. At the end of each period, participants are informed about the outcome of the respective period. The feedback includes information on their own price, the price of the other firm, their own profit, the profit of the other firm, the degree of PCO, and their own cumulative profit.

Payoffs consist of a show-up fee of 5 EUR plus the sum of profits the respective participant earned during the experiment. We use an Experimental Currency Unit (ECU) for payments, with 10,000 ECU being worth 1 EUR. Sessions are run in the DICE Laboratory for Experimental Economics using the standard combination of z-Tree (Fischbacher, 2007) and ORSEE (Greiner, 2015). Subjects are students and non-students from a variety of backgrounds. There were 144 participants in the repeated game and 96 participants in the static game. A translation of instructions can be found in the appendix. Final payments are between 9 EUR and 27 EUR with an average of 16.47 EUR. A session lasted approximately 75 minutes.

3.6 Hypotheses

In this section we present our hypotheses concerning behavior in the repeated game and in the static game. In short, we expect prices to be positively correlated with the degree of PCO in both cases. However, since collusion cannot occur in the static game, this correlation will be driven by unilateral effects whereas behavior in the repeated game may also be affected by coordinated effects.

Hypothesis 3.1. *There will be a positive correlation between average selling prices and the degree of partial cross-ownership in the static game and in the repeated game.*

In the static game, we expect to observe a positive correlation between average prices and the degree of PCO. The reason is that the higher the mutual shareholdings, the lower the gains firms realize by undercutting their rival and the lower the losses they make if they are undercut by their rival. As a consequence, subjects will choose higher prices more frequently and average prices will rise with the degree of partial cross-ownership. Note that the logic presented above is fully consistent with the predictions of Quantal Response Equilibrium as derived in Section 3.4.2.

We also expect prices to increase with the degree of PCO in the repeated game. The first reason is analogous to the argument we made for the static game. The higher the degree of cross-ownership, the lower the gains firms make when they undercut their rival and the lower the losses they suffer when their rival undercuts them. The second reason is that firms may be able to reach collusive outcomes. In this paper, we use the following definition for collusive outcomes: markets are collusive if both firms choose the monopoly price.[20] Since repeated-game arguments like trigger strategies do not apply to the static game, we do not expect subjects to reach collusive outcomes in the static game.

Hypothesis 3.2 (a). *In the static game, subjects will not be able to coordinate on the monopoly price for any degree of partial cross-ownership.*

Hypothesis 3.2 (b). *In the repeated game, subjects will more often coordinate on the monopoly price the higher the degree of partial cross-ownership.*

These expectations are largely consistent with the equilibrium analysis in Section 3.4 where we show that the critical discount factor for tacit collusion decreases

[20]There are several reasons for using this definition. First, the monopoly price ($p_i = p_j = 100$) is the only one where competitive motivations cannot play a role, second, the maximum price is a focal point (compare Scherer, 1980, p. 190), and third, subjects in our experiment rarely coordinate on a price which is not the monopoly price.

with α. While the theory does not necessarily predict more tacit collusion,[21] the decrease in the critical discount factor is driven by lower defection profits. Hence, deviation is less attractive for subjects, which may stabilize collusive agreements even though the discount factor we induce by using the random ending rule is high enough for collusion to be sustainable for all degrees of partial cross-ownership we consider in the experiment. Since collusion on the monopoly price implies a higher price level than under competition, we expect the average price to be higher with PCO than without PCO.

A consequence of Hypothesis 3.2 (a) and Hypothesis 3.2 (b) is that prices in the repeated game should be higher compared to the static game. In the repeated game, collusive behavior may play a role which is not the case in the static game. Hence, average prices in the repeated game should be higher.

Hypothesis 3.3. *Average prices will be higher in the repeated game compared to the static game.*

3.7 Results

3.7.1 Pricing Behavior

We first consider the selling prices chosen by participants. The selling price is defined as the lowest price in a market and is henceforth referred to as p_{min}. The left panel of Figure 3.2 summarizes the average selling prices (over all groups and periods) for all PCO levels in both treatments. The figure documents two important effects. First, prices tend to be higher the higher the degree of PCO, and second, prices are higher in the repeated game compared to the static game. We now consider these aspects one by one.

The degree of partial cross-ownership denoted by α affects prices positively in both treatments. Average selling prices are between 19.6 and 57.7 in the static game and between 37.8 and 81.9 in the repeated game. In both cases, they are lowest for $\alpha = 0$ and highest for $\alpha = 2/3$. The relation is almost monotonic, but the graph reveals one anomaly. The session with $\alpha = 1/3$ in the repeated game is different in that prices are lower than those for $\alpha = 1/9$. A potential explanation for this

[21] In the experiment, we induce a discount factor of $\delta = 0.9$. This implies that coordination on the maximum price may be an equilibrium outcome for all levels of the PCO parameter including the case with $\alpha = 0$.

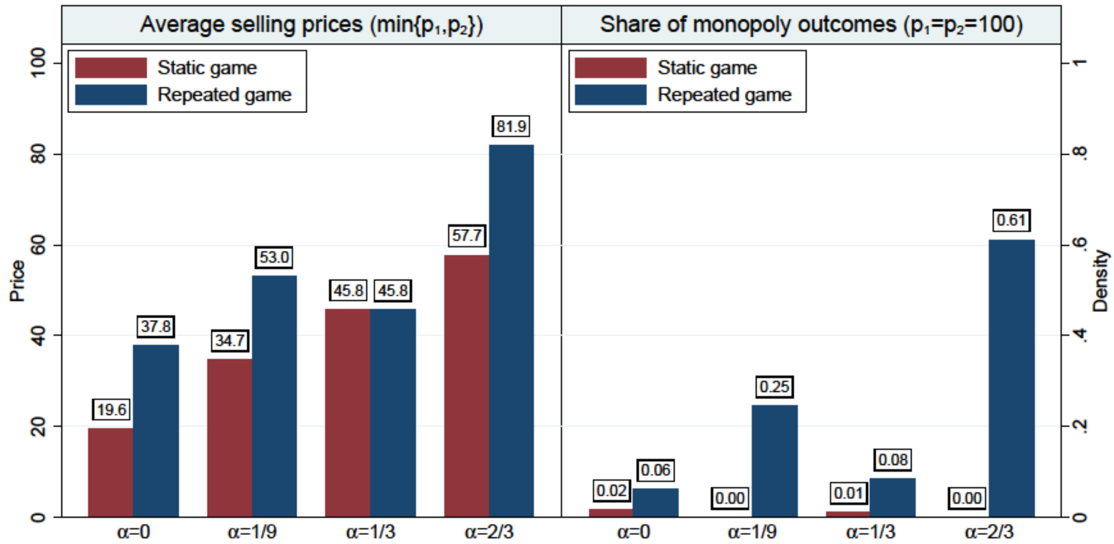

Figure 3.2: Average selling prices and share of collusive outcomes.

Comparison across PCO levels for the static and the repeated game.

will be discussed in a later subsection. Apart from this, the picture drawn by the figure is clear. The higher the degree of partial cross-ownership, the higher the average selling prices. The notion that prices increase with the degree of partial cross-ownership is also supported by non-parametric tests. Considering the static and the repeated game separately, Jonckheere-Terpstra tests for ordered ascending alternatives suggest a highly significant correlation between these measures in both cases ($p = 0.001$ for the static game and $p < 0.001$ for the repeated game).

Result 3.1. *Average selling prices increase with the degree of partial cross-ownership in the static game and in the repeated game. This is consistent with Hypothesis 3.1.*

Comparing the results from the repeated and from the static game we find that prices are higher in the repeated game. This result can not only be seen in the left panel of Figure 3.2 (the only exception is the case where $\alpha = 1/3$ where price levels in both games are very similar), it is also supported by a non-parametric test (one-sided Mann-Whitney ranksum test, $p = 0.047$).

Result 3.2. *Average selling prices are higher in the repeated game compared to the static game. This is in line with Hypothesis 3.3.*

The results 3.1 and 3.2 are both supported by the regression analysis presented in Table 3.2. In the regressions (1) to (3) the selling price is the dependent variable.

	(1) Min. Price	(2) Min. Price	(3) Min. Price	(4) Monopoly	(5) Monopoly
Repeated	15.17***	13.05*	9.463*	2.028***	
	(3.703)	(5.204)	(4.065)	(0.437)	
Alpha	58.39***		58.39***	2.190***	2.412***
	(7.277)		(7.297)	(0.575)	(0.601)
Second part	7.813***	7.813***	0.965	0.474***	0.451***
	(1.743)	(1.747)	(1.609)	(0.117)	(0.128)
Period	-0.219	-0.219	-0.219	0.0482***	0.0496**
	(0.217)	(0.216)	(0.220)	(0.0141)	(0.0156)
Static × Alpha		54.19***			
		(8.825)			
Repeated × Alpha		61.19***			
		(10.34)			
Repeated × Second part			11.41***		
			(2.595)		
Constant	20.29***	21.57***	23.72***	-3.952***	-2.003***
	(3.455)	(3.520)	(3.590)	(0.559)	(0.341)
Observations	2400	2400	2400	2400	1440

Table 3.2: OLS regressions of the average selling prices and probit regressions of subjects choosing a price of 100.

Standard errors in parentheses, clustered at the matching-group level, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Note that *Second part* is a dummy variable indicating whether or an observation originates from the second part of a session, and that *Period* refers to the period within a part and not to the period within the experimental session.

In all three regressions the coefficient for the PCO parameter α is positive and highly significant. The coefficient of the treatment dummy for the repeated game is also positive in all three cases. It is highly significant in regression (1) and weakly significant in the regressions (2) and (3). The reason is that regressions (2) and (3) contain interaction terms using the repeated dummy. We will discuss these regressions in a later subsection of the paper.

3.7.2 Collusive Behavior

Standard economic theory suggest that tacit collusion may affect subjects' behavior in the repeated game. The analysis in Section 3.4 shows that theory also suggests that collusive outcomes are more sustainable the higher the degree of cross-ownership. As a consequence, the aim of this subsection is to find out whether PCO had a bearing on collusive behavior.

To analyze collusive behavior, we define a dummy variable which equals one if both firms in a market charge the monopoly price (i.e., $p_i = p_j = 100$). The intuition for this is as follows. First, if this condition is met, the market will resemble a monopoly. Second, charging the maximum price is the only strategy where it is impossible to gain the entire market. This implies that competitive motives cannot play role here. Third, our data shows subjects rarely coordinate on a different price.

The right panel of Figure 3.2 summarizes the degree of collusive behavior for all levels of α in both treatments. As mentioned before, it presents the share of markets where both firms charge the monopoly price. The figure suggests that there is a positive relation between α and the amount of collusive behavior in the repeated game (the case with $\alpha = 1/3$ is also exceptional in this dimension). For $\alpha = 2/3$ more than 60% of the markets achieve fully collusive outcomes compared to only about 6% for $\alpha = 0$. For the repeated game, a Jonckheere-Terpstra test for ordered ascending alternatives supports the notion that collusive outcomes are reached more often the higher the degree of partial cross-ownership ($p = 0.004$). This is in contrast to the static game. Not surprisingly, collusive behavior does not play a role in the static game. While some subjects do try to achieve collusive outcomes in all variants, the collusion is hardly ever successful (less than 1% of all cases).

Result 3.3. *Subjects virtually never coordinate on the monopoly price in the static game. This is support for Hypothesis 3.2 (a).*

To shed further light on the effect of collusive behavior, we conduct the regressions (4) and (5). These are probit regressions where the dependent variable is the aforementioned dummy. Both regressions supports the notion that there is a positive correlation between the share of collusive markets and α in the repeated game. Since coordination hardly ever occurs in the static game, we conduct regression (5) as a robustness check. Here, we only include the data from the repeated game and find that the results are qualitatively the same as in regression (4).

Result 3.4. *In the repeated game, subjects successfully coordinate on the monopoly outcome in many cases and coordination is more often successful the higher the degree of partial cross-ownership. This is consistent with Hypothesis 3.2 (b).*

3.7.3 Unilateral and Coordinated Effects

In this subsection, we compare the data from the static game with the data from the repeated game and try to gain an assessment of the unilateral and coordinated anti-competitive effects. Our Result 2 already documents that average selling prices are higher in the repeated game compared to the static game. This difference is driven by a combination of the Results 3 and 4. In other words, subjects often achieve collusive outcomes in the repeated game but not in the static game. Since firms coordinate on the monopoly price, average prices are higher in the repeated game.

Regression (1) suggests that there is a premium of about 15 ECU in the repeated game. This surcharge is the result of coordinated effects which only affect behavior in the repeated game. We further explore the differences in price levels using the Regressions (2) and (3). Regression (2) confirms that the correlation between prices and α exists in both treatments. The estimate of the $Repeated \times Alpha$ coefficient is higher than the one for the static game, but these differences are insignificant.[22] This suggests that coordinated effects and unilateral effects are equally sensitive to the degree of cross-ownership. Regression (3) suggests that prices are higher in the second part of the repeated game but not in the second part of the static game. A possible explanation for this is that the start of the second part is a structural break in the repeated game but not in the static game.[23]

The difference between the coordinated effects and unilateral effects can also explain the lower price levels in the repeated game with $\alpha = 1/3$. The number of collusive outcomes is lower compared to the other treatments with positive degrees of partial cross-ownership. In this treatment, only 8% of the markets were collusive compared to about 25% in the treatment with $\alpha = 1/9$ and about 61% in the one with $\alpha = 2/3$. Apparently, coordinated effects did not materialize in the repeated game with $\alpha = 1/3$. The price levels in this treatment are, however, very similar to the ones from the static game. In both cases, average selling prices equal about 45.8 ECU. Hence, one might argue that in absence of coordinated effects, unilateral effects still have a considerable impact.

[22] Both 95% confidence intervals contain the point estimate of the other coefficient.

[23] Subjects are matched with a new partner only after the end of the first part in the repeated game but are rematched after each period in the static game.

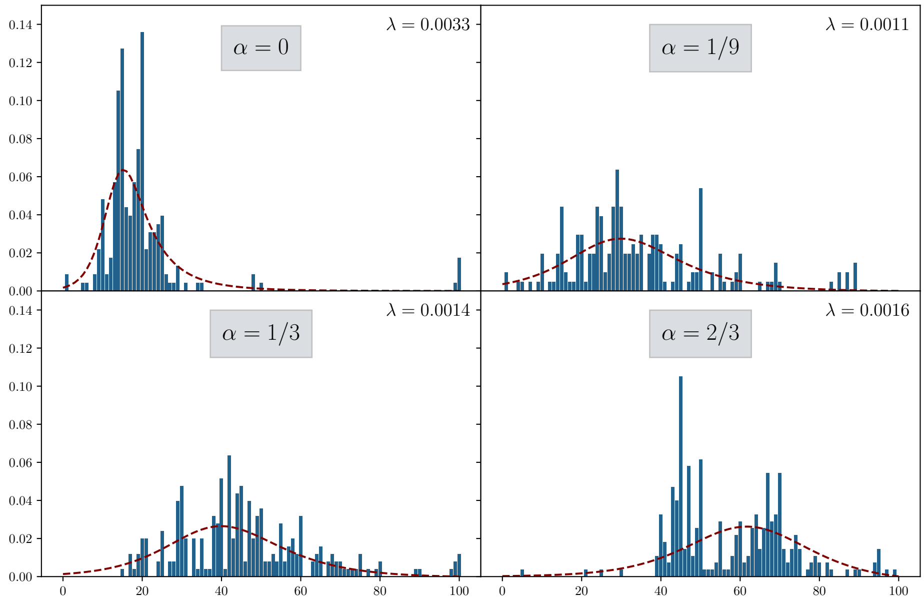

Figure 3.3: Predictions from fitted Quantal Response Model.

The blue bars show the distribution of the selling prices from the static game. The red line represents QRE predictions from a model fitted using maximum-likelihood. The optimal fits were derived separately for the different degrees of partial cross-ownership and the fitted values of the QRE parameter are indicated in the upper right corner of the corresponding panel.

3.7.4 Quantal Response Equilibrium

Figure 3.3 presents a fitted QRE model. The fits were derived using maximum likelihood and are based on the selling prices from both parts of the static game. The model allows the QRE parameter λ to differ across treatments. The optimal fits were derived separately for the different degrees of partial cross-ownership. Looking at the data, it can be seen that the fitted models perform reasonably well at explaining the data. For all values of the PCO parameter, the QRE model gives an accurate representation of the strategies which are chosen at negligible frequencies (see e.g., the domain $[50; 99]$ for $\alpha = 0$ or $[0; 38]$ for $\alpha = 2/3$). Also, the center of the distributions appear to be close to the ones of the empirical distributions. Only in the treatment with $\alpha = 2/3$ the modal choice of the participants does not coincide with the center of the predicted distributions. However, even in this case the QRE predictions are by and large accurate. This notion is also supported by likelihood-ratio tests. The QRE parameter is significantly different from zero in all cases (all $p < 0.001$).

Result 3.5. *A fitted QRE model performs well at explaining the data from the experiment. This is additional support for Hypotheses 3.1 and 3.2 (a).*

3.8 Conclusion

Competition authorities have lately expressed concerns about potential anti-competitive effects of minority shareholdings, but it is not clear if these effects are strong enough to warrant novel regulation. The present paper contributes to this discussion in a number of ways. First, we use a laboratory experiment to show the existence of unilateral and coordinated effects of partial cross-ownership and disentangle one from the other. The laboratory environment enables us to rule out possible confounding factors such as the interests of other shareholders, communication or control rights which appears rather challenging using field data. Further, we provide novel theoretical predictions about the impact of minority shareholdings on prices based on the concept of Quantal Response Equilibrium (QRE) as the Nash predictions are rather weak. We then test these predictions in the laboratory. To the best of our knowledge, this is the first experimental study examining non-controlling partial cross-ownership in horizontal markets.

Our experiment provides strong evidence for anti-competitive effects to occur when PCO is present. As for unilateral effects, the static game reveals that firms soften their competitive behavior with the degree of PCO and there is a monotonic increase of average prices which cannot be attributed to more collusive behavior. For the static game, standard theory predicts that firms' behavior should be unaffected by cross-ownership in that firms are expected to choose prices equaling marginal costs. Hence, according to Nash Equilibrium, unilateral effects should not occur and PCO should have no effect on pricing behavior. However, from a behavioral point of view, this prediction is not very likely to hold. While the existence of firms' incentive to undercut their rivals is unaffected by partial cross-ownership, their magnitude is not. In other words, the higher the degree of partial cross-ownership, the lower firms' incentive to lower their price. As a consequence firms should be more likely to charge higher prices the higher the degree of partial cross-ownership. This line of reasoning is consistent with Quantal Response Equilibrium which predicts firms setting higher prices on average with increasing PCO. The price patterns we observe are largely consistent with the ones predicted by QRE. Hence, our experiment shows that bounded rational behavior may explain the negative repercussions of partial cross-ownership.

We also find evidence for coordinated effects as we see high tacit collusion rates in the repeated game. More specifically, we find that such coordination is positively correlated with the degree of partial cross-ownership. Hence, lower discount factors induced by PCO often translate to more successful collusion and higher market prices.

The high tacit collusion rates seem to support this line of reasoning as an explanation. Participants in the experiment manage to coordinate on the monopoly price in up to 61% of all cases. As a direct result of this collusive behavior, we observe price increases of substantial magnitude in the repeated game.

Our paper highlights the importance of unilateral effects. Even though there is a massive increase of collusive behavior as the degree of cross-ownership increases, the differences between the average selling prices in the repeated and the static game are roughly constant. Hence, a large part of the price increases already takes place in absence of coordinated effects. This notion is also supported by another data point. In the repeated game with $\alpha = 1/3$, coordinate effects did not materialize. The comparison of price levels with the static game does, however, suggest that unilateral effects also affect the repeated game. As a consequence, average selling prices in that treatment are still more than twice as high as the benchmark case where cross-ownership and repeated interactions are absent.

We conclude that partial cross-ownership indeed allows for coordinated and unilateral effects to arise and thereby decreases competition. Moreover, it appears that tacit collusion only adds to unilateral anti-competitive effects but it is not necessary for market outcomes to be less competitive when partial cross-ownership exists. Even without tacit collusion, market prices are significantly higher in this setting. These findings demonstrate the potential harm for consumers of passive partial cross-ownership in single industries.

There are several reasons to assume that the anti-competitive effects of cross-ownership are even more problematic than our experiment suggests. First, our base model is homogeneous Bertrand competition among firms with the same cost structure. This model is exceptional in that the standard Nash predictions suggest that unilateral effects should not occur. This is not the case in other models like heterogeneous Bertrand competition or Cournot competition where prices will already be negatively affected in the base game. Consequently, a different base game is likely to yield stronger anti-competitive effects. Second, the lab experiment was designed to exclude a number of possible confounds like explicit collusion or control rights which may further reduce competition in the field.

3.9 Acknowledgments

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3.11 Appendix

3.11.1 Static Game with Discrete Prices

For the analysis of the discrete game, we assume that both firms have the same action set where possible prices are equidistant. Let d_p denote the minimum change in prices available to either firm. The actions of firms i and j are denoted with p_i and p_j.

We first consider symmetric equilibria. The case $p_i = p_j$ is a Nash Equilibrium if firm i does not have an incentive to undercut firm j. That means $0.5p_j \geq \frac{p_j - d_p}{1+\alpha}$ or $p_j \leq \frac{2d_p}{1-\alpha}$. In the experiments we used $d_p = 1$. As a consequence, there are three symmetric Nash equilibria in the treatments with $\alpha = 0$ and $\alpha = 1/9$. These equilibria have the form $p_i = p_j = p$ with $p \in \{0, 1, 2\}$. These three equilibria also exist in the other treatments, but in those treatments there are more symmetric equilibria. For $\alpha = 2/3$, there is a fourth equilibrium with $p = 3$, and for $\alpha = 2/3$, there are a fifth and sixth equilibrium with $p \in \{5, 6\}$.

There are no asymmetric Nash equilibria in all our treatments. Assume without any loss of generality that firm j chooses a higher price than firm i. If firm j sticks to her/his choice, she/he will earn $u_j(p_j) = \frac{\alpha p_i}{1+\alpha}$, and she/he will earn $u_j(p_i) = \frac{p_i}{2}$ when deviating to the price p_i. Now, firm j does not have an incentive to deviate if $p_i(\alpha - 1) \geq 0$. For any $\alpha < 1$, this inequality is only satisfied for $p_i = 0$. Hence, there cannot be an asymmetric equilibrium where the lower price is positive. Moreover, charging a price of zero also cannot be part of an asymmetric Nash Equilibrium. If a firm chooses zero, she/he will make zero profits whereas she/he will make positive profits when deviating to the (positive) price chosen by the rival firm. In total, there cannot be asymmetric Nash equilibria in the PCO game.

In summary, for the games we use in the experiment, the static Nash prediction is not as unaffected as the one we derived for a model with continuous prices. Having said that, we would like to point out that these additional Nash equilibria cannot explain the price increases we observe in our experiment. The maximum price consistent with Nash Equilibrium increases from two to six as α increases from zero to 2/3. In contrast, average selling prices in the experiments for the static game increase roughly 10 times as much (from about 20 to about 60) over the same range of α. Moreover, the prices consistent with Nash Equilibrium are rarely chosen in our dataset. In the static game, only about 0.2% of all choices lie in the corresponding price ranges.

3.11.2 Experimental Instructions

Instructions for all groups — differences between the one shot and repeated game in italics — translated from German

Hello and welcome to our experiment!

Please read these instructions carefully to the end. In this experiment you will repeatedly make decisions and can thereby earn money. How much you earn depends on your own decisions and on the decisions of a randomly assigned second participant. Please do not talk to your neighbors and be quiet during the entire experiment. If you have a question, please raise your hand—we will then come to your booth and will answer your question personally. All participants receive (and are currently reading) the same instructions. You will remain completely anonymous to us and to the other participants. We will not save any data in connection with your name. At the end of the experiment, you will be given your profit paid in cash.

Market

In this experiment you will have to make decisions for one of two firms in a market. Both firms are selling the same product and there are no costs of producing this good. This market is made up of 100 identical consumers, each of whom wants to purchase one unit of the good at the lowest price. The consumers will pay as much as 100 Experimental Currency Units (ECU) for a unit of the good. Both firms are able to produce 100 units of the product and can supply all consumers in the market.

Procedure

The experiment takes place as follows:

At the beginning of each period, both firms have to set their price at which they want to offer their units. The firm that sets the lowest price will sell all its 100 Units at the selected price. If both firms set the same price they will split the consumers equally – each firm sells 50 Units.

Earnings with partial ownership

The following calculations might seem a little confusing at the beginning – however, you do not need to calculate your earnings yourself. The computer can calculate the results for you. You can also test some price combinations in each round before you have to decide on your final price. The substance of the profit formula is: You might profit from the other firms earnings and the other firm might profit from your earnings. This is due to partial ownership that firms have of each other in a market and impact the profits. You have to distinguish between income and profit. Your income is calculated as the product of your sold units and your selected price.

Due to the partial ownership, your profits depend not only on your income but also on the income of the other firm. Each firm gets a certain share of the income of the respective other firm in its market. However, it also has to pay a certain share of its own income to the other firm. To calculate your profits you have to consider a Profitfactor = 1/(1+share) and a Sharefactor = Share/(1+share). Your actual profit of each period is then calculated according to the formula: Profit = Profitfactor * Your Income + Sharefactor * Income of the other firm.

At the end of each period you will be informed of the price you have chosen, the price chosen by the other firm in your market, how many consumers each firm served in the respective period, how much income each firm had and how much profit each firm made. Additionally, your own cumulated profits over all periods will be displayed. For simulations of your potential profits, we will provide you with a "Profit Calculator" in each period, where you can check possible combinations of prices chosen by firms and the associated profits, prior to your price selection. In the first two periods of each game you will be allowed to try this for 120 seconds. In each additional period of each period, you will only have 30 seconds for this.

Groups and Games

The experiment is divided into multiple games that have multiple periods each. After every period, another period will be played with a probability of 90% in the respective game. With a probability of 10% the game will end and a new game will start. The experiment ends once all games are completed. Before every period, you will be randomly assigned to one firm and a market with another firm. We will explicitly make you aware of this again during the experiment. *Throughout a game you will be matched with the same other firm in every period. However, you will be assigned to a new market with a different second firm in the market before each game.*

Payoffs

At the end of the experiment you will be told the sum of the profits made during the experiment, which will be your payment. You will receive 1 Euro for every 10,000

ECU you earn during the experiment. You will also receive 5 EUR for participating.

Examples

For a better understanding of the calculation of the profits, we provide two illustrative examples in what follows:

Example 1:

Suppose that each firm has a share of 25% of the respective other firm in the market. We therefore have a profitfactor of $1/(1+0.25) = 0.8$ and a sharefactor of $0.25/(1+0.25) = 0.2$.

Now, suppose the firms choose the following prices: Firm 1 sets a price of 21 ECU, firm 2 chooses a price of 64 ECU. Firm 1 sets the lowest price. Therefore, all consumers want to buy from firm 1. Firm 1 sells 100 Units at a price of 21 ECU and has an income of 100*21 ECU = 2,100 ECU. There is no consumer in the market left who has not bought a unit of the good yet. Firm 2 sells no Unit and has an income of 0*64 ECU = 0 ECU. Due to the Share firm 1 has of firm 2 and vice versa, the profits are Profit of firm 1 = 0,8 * 2,100 ECU + 0,2 * 0 ECU = 1,680 ECU. Profit of firm 2 = 0,8 * 0 ECU + 0,2 * 2,100 ECU = 420 ECU.

Example 2:

Suppose that each firm has a share of 17.65% of the respective other firm in the market. We therefore have a profitfactor of $1/(1+0.1765) = 0.85$ and a sharefactor of $0.1765/(1+0.1765) = 0.15$.

Now suppose the firms choose the following prices: firm 1 sets a price of 38 ECU, firm 2 chooses a price of 38 ECU. Firm 1 and firm 2 have both chosen the same and therefore the lowest price. Their combined capacities (100+100=200), however, are larger than the number of consumers in the market that want to buy a unit of that product (100). The consumers will be equally distributed between the firms. Firm 1 sells 100/2=50 units at a price of 38 ECU and has an income of 50*38 ECU = 1,900 ECU. Firm 2 also sells 100/2 = 50 units at a price of 38 ECU and has an income of 50*38 ECU = 1,900 ECU. Due to the Share firm 1 has of firm 2 and vice versa, the profits are Profit of firm 1 = 0,85 * 1,900 ECU + 0,15 * 1,900 ECU = 1,900 ECU. Profit of firm 2 = 0,85 * 1,900 ECU + 0,15 * 1,900 ECU = 1,900 ECU.

Good luck!

Chapter 4

Cournot Competition with an Unknown Number of Players: Experimental Evidence

Co-authored with Claudia Möllers and Hans-Theo Normann

4.1 Introduction

A recent and growing literature investigates games with an unknown number of players. While standard definitions of games begin with a commonly known set of players, this literature acknowledges that the number of co-players will in many circumstances be unknown. This was recognized, for example, for auctions, where Levin & Ozdenoren (2004) and Harstad et al. (2008) argue that the exact number of bidders is rarely known by the participating players. Various social dilemmas, including the public-goods game (Kim, 2018; Mill & Theelen, 2019) and the volunteer's dilemma (Hillenbrand & Winter, 2018; Hillenbrand et al., 2020), may involve large populations whose exact size will typically be unknown. A similar point can be made for contests (Lim & Matros, 2009; Boosey et al., 2017). Oligopoly interaction typically involves smaller sets of players, but Janssen & Rasmusen (2002) and Ritzberger (2009) (who analyze Bertrand competition) argue that, even if the true number of firms in the market is known, firms may be unaware which of their competitors are active.[1] Overall, games with an unknown number of players seems to be a highly relevant and fruitful area of research.

In this paper, we analyze repeated Cournot oligopolies in an environment, where the number of players is unknown. Cost and demand conditions are known, but the number of players is not—only their ex-ante distribution is common knowledge. We explore this scenario in laboratory experiments to investigate behavior in this setting.

The motivation for analyzing repeated Cournot oligopolies with an unknown number of players is that they may give rise to novel strategies. In Cournot models, actions are strategic substitutes. If the number of players is unknown, players may adopt—what we call—a sophisticated strategy: They may attempt to fool competitors into believing that there are more players in the market than is actually the case. They produce a larger than Nash output level expecting their rivals to adapt. This sophisticated strategy may backfire if the true number of players is larger than expected, but it will yield large gains when this is not the case, and thus, may be ex-ante payoff maximizing.

Contrary to this argument suggesting sophisticated play, there are good rea-

[1]Firms can be inactive in a geographical or time dimension, so that there is a positive probability that a given firm is not competing in a market. For example, supermarket discounters sometimes enter a certain market for a short period of time with special offers. In other industries, firms only offer their products within a certain radius of their (changing) branches.

sons to maintain that the lack of common knowledge of the number of players does *not* matter. Cournot oligopoly is a prime example where learning leads to Nash equilibrium. When players select strategies that are (at least rough) best replies against some measure or distribution of past output of their rivals, they should end up in equilibrium. Crucially, learning and convergence do not require knowledge of the number of players. Players respond to aggregate output (often referred to as adaptive play), and it is immaterial how many players produce that output. The best-response hypothesis first appears in Cournot (1838) and has been generalized later on. Milgrom & Roberts (1991) mention Cournot best-reply learning, fictitious play and Bayesian learning as adaptive processes that converge to Nash.[2] Accordingly, our second benchmark is that it does not matter whether the number of players is known, and that players will anyhow converge to static Nash.

In the light of these arguments, it is interesting to note that the conjectural variations literature (Bowley, 1924; Stackelberg, 1934; Hicks, 1935; Leontieff, 1936) long ago identified a potential pitfall in the logic of Cournot learning, which is related to our approach.[3] A player who recognizes that she is in a group of best-responding players can exploit this to her advantage. In a Cournot duopoly (the standard case in this literature), a player can gain by producing some output larger than Nash, provided her rival best responds. With such a strategy, total output increases, but the first player's payoff increases at the expense of the second. Play would not converge to Nash equilibrium.[4] Existing Cournot experiments find, however, little evidence in favor of this alleged pitfall, and Cournot oligopolies are usually

[2]See also Fudenberg & Levine (1993), Camerer & Hua Ho (1999), and the textbook treatment in Vega-Redondo (2003).

[3]Bowley (1924); Stackelberg (1934); Hicks (1935); Leontieff (1936) mark the beginning of the conjectural variations literature. A conjectural variation is a player's belief about how rival players will respond to changes in her output. The Cournot conjectural variation is the belief that rival players will not change their output at all. Then there are competitive, collusive, and other conjectural variations. Bresnahan (1981) suggested the notion of a consistent conjectural variation. Based on Cournot models, Daughety (1985) and Makowski (1987) pointed out that conjectural variations are a-rational concepts that cannot be rationalized. Dockner (1992); Sabourian (1992); Cabral (1995) analyze Cournot settings where the outcome of the repeated game equals the outcome of a conjectural-variations model. A more recent literature connects conjectural variations to evolutionary arguments (Dixon & Somma, 2003; Müller & Normann, 2005).

[4]Hicks (1935), p.15) refers to the committing player as the "active" player. Stackelberg (1934) is usually eponymous for the leader-follower model. The committing player would be the Stackelberg leader and the adaptive player the follower. The Stackelberg leader and follower roles emerge here in a simultaneous-move model. The Stackelberg leader knows the reaction function of the other player but not vice versa. Note that there are actually no sequential moves in Stackelberg (1934). See Giocoli (2005).

seen as reliably converging to Nash in experiments. When players do know the group size, presumably, only few players would be willing to repeatedly produce a disproportionally small fraction of the aggregate output. But with an unknown number of players, what we call sophisticated behavior may occur. Therefore, our paper can also be seen as a contribution to the conjectural variations literature in that we aim at identifying different conjectural variations in a novel setup—in games with unknown number of players.

Another hypothesis is that players neither play Nash nor sophisticated strategies but tacitly collude instead. At least for duopolies, it has been repeatedly documented that groups of two or three players cooperate to some extent, whereas larger groups reliably fail to do so (Huck et al., 2004; Horstmann et al., 2018). Nonetheless, tacit cooperation in duopolies is far from being the norm as only some duopolies manage to collude. It is also somewhat fragile as, for example, asymmetries can largely eliminate it (Fischer & Normann, 2019). Our expectation regarding tacit collusion is thus that uncertainty about the number of players reduces the degree of cooperation even when the true number of players is only small.

A key aspect of our experiment is that subjects play two supergames. This should facilitate learning, and such learning may in principle support all three outcomes we consider plausible (sophisticated play, Nash, collusion). We consider our repeated Cournot setting with an unknown number of players to be a relatively complex lab setting, and an unknown number of players will add to that complexity. We therefore hypothesize that the degree of sophisticated play will increase in the second supergame.

In our experiments, players play a simultaneous-move Cournot game in fixed groups over 25 periods. They know that a market has either two or four competitors, and that both cases are equally likely. Demand and cost in both markets are identical. After the end of period 25 and feedback about the true number of competitors, players are reshuffled to play a second (and final) supergame. In the second supergame, the number of players may or may not change. We elicit participants' beliefs about the number of players after period one and after the last period. As controls, we run experiments where the number of players is common knowledge.

Our results are as follows. In the first repeated game of the experiment, play is largely characterized by convergence to Nash equilibrium, although some groups of two collude tacitly. Players are able to gather meaningful information through the interaction with other players and, by and large, end up playing Nash or collude.

In the second supergame, we do have evidence of more sophisticated play. Outputs increase and become more asymmetric. We deepen this analysis by categorizing groups with a k-means cluster analysis (MacQueen et al., 1967). This analysis confirms that an unknown number of players leads to sophisticated play, especially in the second supergame.

The paper is structured as follows. In Section 4.2 we describe the basic model and the static Nash equilibrium. In Section 4.3 we describe our experimental design and the procedures. In Section 4.4 we phrase our hypotheses. In Section 4.5 we discuss the results. In Section 4.6 we elaborate on the cluster analysis. In Section 4.7 we conclude.

4.2 The Model

4.2.1 Basic Setup

We consider a Cournot model with n cost-symmetric players. Action sets for all players are non-negative quantities. The action of player $i \in \{1, \ldots, n\}$ is denoted by q_i, $q_i \geq 0$. The inverse demand function is given by

$$P(Q) = max\{a - Q, 0\}$$

where $Q = \sum_{i=1}^{n} q_i$ denotes total aggregate output of the n players. Production costs, $C(q_i)$, are linear in q_i with marginal production costs of c such that

$$C(q_i) = cq_i.$$

Player i's profit is given by

$$\pi_i = (P - c)q_i = \max\{a - c - Q, -c\} \cdot q_i.$$

4.2.2 Static Equilibrium and Joint-profit Maximum

Suppose first that the number of players is common knowledge. The derivation of the static Nash equilibrium is straightforward, and we can write equilibrium outputs as a function of the number of players, $q(n)$, where

$$q(n) = \frac{a-c}{n+1}.$$

Likewise, we write the corresponding Nash equilibrium profit as a function $\pi(n)$, where

$$\pi(n) = q(n)^2.$$

The symmetric joint-payoff maximizing outcome is when players divide the monopoly output, $q(1)$, evenly:

$$\frac{q(1)}{n} = \frac{a-c}{2n}.$$

The payoff in this case is $q(1)^2/n$ per player.

Now suppose the number of players is *not* known and, as in our experiments, assume that $n \in \{2, 4\}$, equally likely. Player i's expected payoffs when each of its competitors produces q_{-i} is

$$\frac{1}{2}\left(\max\{a - q_i - q_{-i}, 0\} - c\right) q_i + \frac{1}{2}\left(\max\{a - q_i - 3q_{-i}, 0\} - c\right) q_i$$

where the term on the left-hand side is the $n = 2$ contingency, and the term on the right-hand side is the payoff that results when $n = 4$. Because prices are restricted to be non-negative, this payoff function is not differentiable and best response functions are more intricate than in standard cases. The best response is defined piecewise, depending on whether $p > 0$ in either market, that is, whether the non-negativity operators, $p = \max\{a - Q, 0\}$, are binding. Generally, the best response reads

$$\arg\max_{q_i}\left(\frac{1}{2}\left(\max\{a - q_i - q_{-i}, 0\} - c\right) q_i + \frac{1}{2}\left(\max\{a - q_i - 3q_{-i}, 0\} - c\right) q_i\right)$$

The best reply is thus either $\frac{a-2q_{-i}-c}{2}$ when $p > 0|_{n=4}$ case, or it is $\frac{a-q_{-i}-2c}{2}$ if $p = 0|_{n=4}$, or it is zero when $p = 0|_{n=2}$.

We obtain potential equilibria by solving $q^* = (a - 2q^* - c)/2$ and get

$$q^* = \frac{a-c}{4}$$

This is the standard Nash solution for the expected number of players, $n = 3$.

Solving $q^* = (a - q^* - 2c)/2$, we obtain

$$q^* = \frac{a - 2c}{3}$$

for the $p = 0|_{n=4}$ case.

For the parameters of our experiments, $a = 101$ and $c = 1$, we obtain the following. Provided $p = 0|_{n=4}$, we get $q^* = \frac{a-2c}{3} = 33$ yielding a payoff of 544.5. Alternatively, player i can produce less such that $p > 0|_{n=4}$. In that case, $\frac{a-2\cdot 33-c}{2} = 17.0$ and the payoff is 289.0. Since this payoff is lower, $q^* = 33$ is a Nash equilibrium. If $p > 0|_{n=4}$, we obtain $q^* = \frac{a-c}{4} = 25$. The payoff would be 625 here. However, player i can produce more such that $p = 0|_{n=4}$ and the best response is $\frac{a-25-2c}{2} = 37.0$ with a payoff of 684.5. So $q^* = 25$ is not a Nash equilibrium for our parameters. In our experiment, the unique equilibrium reads $q^* = 33$, that is, it involves *overproduction* in the sense that players produce more than in the static Nash equilibrium with the expected number of players ($n = 3$).

4.2.3 Repeated Play

The Cournot base game is repeated a finite number of periods, T. A period is denoted by $t \in \{1, ..., T\}$. Future periods are not discounted. The number of players as well as demand and cost remain constant during the T periods. Formally, this is a dynamic game of incomplete information. Players hold a prior about the number of players and may update this belief about the number of players in the dynamic setting.

We argue that the incomplete information about the number of players can give rise to strategic play where players exploit the uncertainty by producing larger outputs, potentially deceiving rivals about the true number of players. If the number of players is unknown, sophisticated players may fool competitors into believing that there are more players in the market than is actually the case. They produce a larger than Nash output level expecting their rivals to best respond.

Consider a $n = 2$ group with one sophisticated player and one myopic player. The myopic player chooses q^* in the first period, i.e., the Bayesian-Nash equilibrium output of the static game, and decides non-strategically regarding future periods. From the perspective of the myopic player, the total output of her rival(s) in the first period, $t = 1$, should turn out to be either q^* or $3q^*$. Given the $t = 1$ feedback about total output, she plays $q(2)$ or $q(4)$ for the rest of the game. The sophisticated player can gain by producing $3q^*$ in $t = 1$, the output of three myopic players. She may successfully deceive her rival (who believes there are $n = 4$ players) and thus produce $3q(4)$ for the rest of the game. If it turns out there are actually $n = 4$ players in the group, the sophisticated player will make a loss, at least in the first period, $t = 1$, but in terms of ex-ante expected payoffs, she may gain.

This strategy may backfire, however, when there is more than one sophisticated player. In $n = 2$ groups, two sophisticated players would clash in the first period. They might then acknowledge this, retreat, and henceforth choose $n = 2$ Nash quantities, but they might also obstinately keep pursuing this strategy and end up in endured wars of attrition. In a group of $n = 4$ players, a sophisticated player may not be able to distinguish three myopic opponents (producing $3q^*$ in total) from a single opponent who persistently pursues the sophisticated strategy and also produces $3q^*$. Again, this may lead to above Nash output due to a sophisticated player who keeps believing the true number of players is $n = 2$ rather than $n = 4$.

All these contingencies suggest that an unknown number of players leads to higher than Nash aggregate output. Further, the range of outputs, meaning the difference between the highest and lowest chosen quantity, should also be higher than in the treatment with a known number of players. Sophisticated players choose high outputs, other players choose low quantities. We elaborate on this in Section 4.4 below.

4.3 Experimental Design and Procedures

Players compete in a Cournot market setup as described in Section 4.2. They face an inverse demand with intercept $a = 101$, so $p(Q) = max\{101 - Q, 0\}$ with total output Q. Quantity choices have to be chosen from the interval $[0, 400]$ with 0.01 being the grid size.[5] The marginal costs of production are $c = 1$ and there are no fixed cost, so $C(q_i) = q_i$. Players' payoffs are $\pi_i = (max\{101 - Q, 0\} - 1) \cdot q_i$. Subjects are told to represent a firm and have to decide on their output every period. Players play this Cournot game repeatedly. They play finitely repeated games with a length of $T = 25$ periods. Subjects play two supergames each of which lasts 25 periods, that is, they play 50 periods in total.

The matching procedures are as follows. Within a supergame, matching is fixed.

[5] We wanted players to be able to choose outputs higher than the market size, a, in order to allow for high-output sophisticated play. The limit of 400 is way above any plausible action.

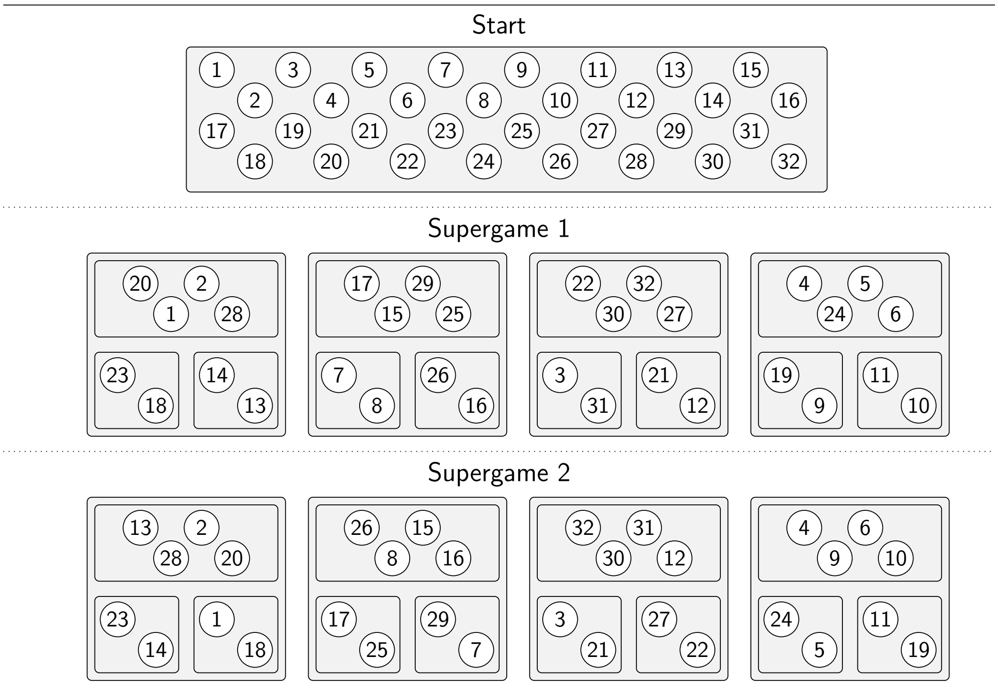


Figure 4.1: Experimental design.

All sessions have exactly 32 participants who were partitioned into four matching groups with eight participants. These matching groups consisted of two duopolies and one quadropoly. For supergame 2, new duopolies and quadropolies were randomly matched within these matching groups.

Between supergames, there is a random re-matching of markets within a matching group. In all experiments, subjects are allocated into matching groups with eight participants. In each matching group, there are exactly two duopolies and one quadropoly. See Figure 4.1. Subjects are allocated randomly within the matching groups, so the individual probability of being in a duopoly/quadropoly is 50% each. The random draw is independent for the two supergames. This means that some subjects play twice in a duopoly, some play twice in a market with four players, and some play each oligopoly size once.

Our treatment variable is whether or not subjects know how many players are in the market. The labels for the two treatments are Unknown and Known. In Unknown, subjects know the probability distribution over the two market sizes (50% chance of being in a duopoly or quadropoly, respectively), but they do not know the actual number of opponents. Participants learn the actual number of competitors only after the completion of each supergame. Treatment KNOWN is identical to UNKNOWN except that we tell subjects the number of competitors right after the random draw and before each supergame begins. Apart from the matching procedure (into groups with two duopolies and one quadropoly), KNOWN is like a standard repeated Cournot experiment with a known number of players.

In UNKNOWN, we elicit the beliefs about the number of players, twice in each supergame. Participants have to guess the number of players after the first period and after the last period of play. This is incentivized. For each correct guess, subjects receive 3,000 Experimental Currency Units (ECU). Subjects learn whether their guesses were correct at the end of the supergame. In KNOWN, there is, of course, no corresponding belief-elicitation stage.

For both treatments, we conducted three sessions. As mentioned above, all sessions involved 32 subjects who were subdivided into four independent matching groups. Table 4.1 summarizes our treatments in terms of the number of sessions, matching groups and participants.

Treatment	# sessions	# groups	# participants
KNOWN	3	12	96
UNKNOWN	3	12	96
Total	6	24	192

Table 4.1: Number of sessions, independent matching groups and number of participants.

The experiments were run in Spring 2019 at the Düsseldorf Institute for Competition Economics' laboratory for experimental economics (DICElab) at the Heinrich-Heine-University Düsseldorf. We recruited subjects from a pool of voluntary potential participants, using the online recruiting system ORSEE (Greiner, 2015) to ensure that each subject participated in the experiment only once. We had a total of 192 participants. Subjects were students and non-students from various backgrounds.

Once subjects arrived at the lab, they received written instructions explaining all features of the experiment. The translated instructions can be found in Appendix 4.10.2. After getting the opportunity to ask questions in private, all subjects had to answer control questions before the experiment started.

The experiment was programmed and conducted with the experiment software z-Tree (Fischbacher, 2007). In each period, subjects got the opportunity to use a profit calculator for 30 seconds. They could enter their own quantity and the cumulated quantity of all other players (regardless of whether they knew how many players were actually present). The calculator showed the market price and own payoff as feedback. The information was saved for comparison in a table on the screen in the current period. After all participants had entered their quantity decision, participants got feedback on their own output, the total output of all players, the market price, their own period's payoff as well as their own cumulated profit of the respective supergame. In each supergame, subjects were endowed with 9,000 ECU because of potential losses.

After the market experiment, risk attitudes were elicited in a non-incentivized manner. Subjects had to choose either a sure amount of money or a lottery, similar to the methods used by Holt & Laury (2002). Three lists were shown to subjects successively with an increasing probability of the loss.

At the end of the experiment, 3,000 ECU were exchanged for 1 Euro. Sessions lasted approximately 90 minutes and subjects earned, on average, 19.29 Euro.

4.4 Hypotheses

Following the analysis in Section 4.2, we expect three different outcomes. These possible outcomes serve as benchmarks for the interpretation of our data. First, we make assumptions about which outcome we consider likely for which treatment. We then translate these conjectures into testable hypotheses.

The first benchmark is the *static Nash equilibrium* with individual-level outputs $q(n)$, $n \in \{2, 4\}$. In theory, the unique prediction of a finitely repeated game with a Known number of competitors is symmetric Nash play. In fact, previous experiments (Huck et al., 2004) show that Cournot oligopolies are well rationalized by Nash when the number of players is known to be four.

The second benchmark is the *symmetric joint-profit maximizing output* with outputs of $q(1)/n$, $n \in \{2, 4\}$. Although standard theory would not predict (tacit) collusion in a finitely repeated game, we know that it is quite common in experimental duopolies but much less so when markets are bigger (Huck et al., 2004). Hence, we expect some degree of tacit collusion to occur when $n = 2$, especially in Known. We expect no collusion in $n = 4$ groups. Also, the uncertainty about the number of players should reduce collusion in Unknown. We expect the coordination on $q(1)/n$ to be more difficult if the number of players, n, is unknown.

The third benchmark is *sophisticated play*. As elaborated in Section 4.2, average outputs should be higher than in the static Nash equilibrium, and they are characterized by substantial differences between outputs within groups (in the static Nash benchmark, by contrast, outputs should be symmetric). Below, we measure these differences by the within-market range of quantity.[6]

We maintain the following hypotheses for our experiment. First, we expect more sophisticated play in UNKNOWN than in KNOWN, and this should hold for $n = 2$ and $n = 4$. Only in UNKNOWN can players plausibly attempt the sophisticated strategy. In KNOWN it would seem odd that some players produce a substantially larger share of average output, and it is unclear, why rival players would accept this. Further, there should be less collusive play in UNKNOWN than in KNOWN, also leading to larger outputs in UNKNOWN. We accordingly hypothesize:

Hypothesis 4.1. *(a) Average quantities in the* UNKNOWN *treatments are higher than in their* KNOWN *counterparts. (b) Within-market ranges of outputs in* UNKNOWN *are larger than in* KNOWN*.*

The second hypothesis is based on the argument that there will be more sophisticated play in supergame 2 than in supergame 1. The repeated Cournot setting is already a relatively complex lab setting, even more so with an unknown number of players. Subjects might need time to familiarize with this environment, but sophisticated play requires full comprehension of the game, making it more likely to occur in the second supergame. Further, subjects may experience that they have been fooled in the first supergame (after getting feedback on the true n) and then attempt sophisticated strategies themselves. We therefore hypothesize that sophisticated strategies will occur predominantly in the second supergame, implying the following testable hypotheses for outputs:

Hypothesis 4.2. *(a) In* UNKNOWN*, average quantities are higher in supergame 2 compared to supergame 1. In* KNOWN*, this is not the case. (b) In* UNKNOWN*, within-market ranges of outputs are larger in supergame 2 compared to supergame 1. In* KNOWN*, this is not the case.*

[6] The range of outputs, denoted by r, is the difference between the highest and lowest quantity in a market. Formally, $r := \max_i q_i - \min_i q_i$ and parameter $i \in \{1, 2\}$ for duopolies and $i \in \{1, ..., 4\}$ for markets with four players.

4.5 Results

We give an overview of the results in Figure 4.2 and Table 4.2. Figure 4.2 shows the timelines of average individual outputs for all treatments, separately for the two supergames. In Table 4.2, we report the results of ordinary least squares (OLS) regressions with individual output and within-market range as dependent variables and dummies for our four treatments as independent variables, suppressing the regression constant (top panel of Table 4.2). The regressions are clustered at the matching-group level. The coefficients are, accordingly, treatment averages accompanied by a standard error.

4.5.1 Overview

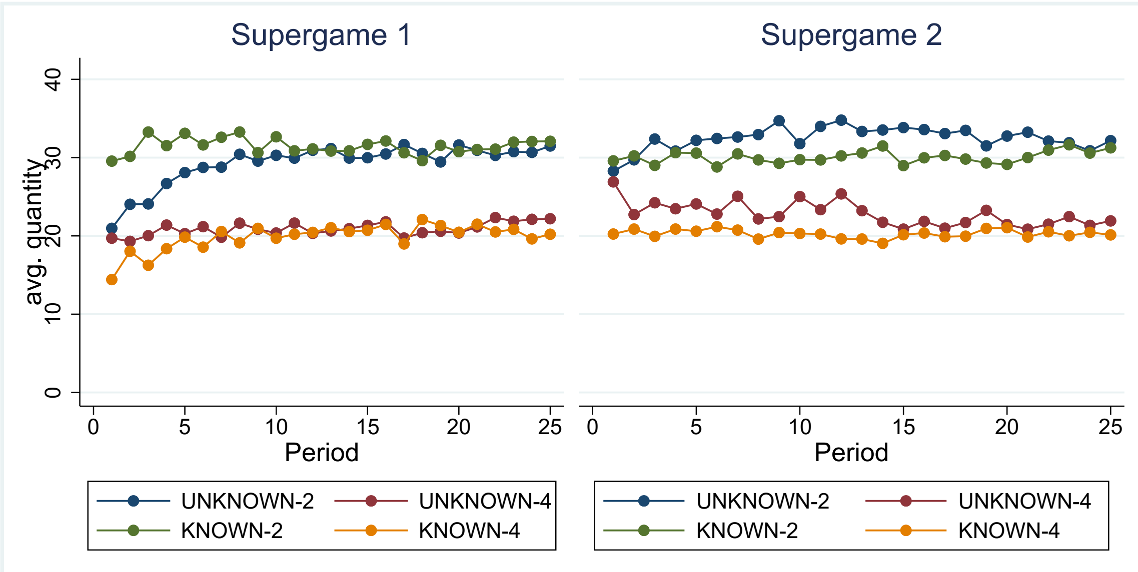


Figure 4.2: Average quantities over all groups per period, treatment and supergame.

In supergame 1, treatments UNKNOWN-2 and UNKNOWN-4 begin in $t = 1$ with virtually identical outputs at a level below KNOWN-2 and above KNOWN-4. After several periods of play, outputs in KNOWN-4 and UNKNOWN-4 then converge to a level of about 20 whereas KNOWN-2 and UNKNOWN-2 converge from about $t = 10$ to a level slightly above 30 (see Figure 4.2).

Averages over all periods are 29.26 and 31.47 in UNKNOWN-2 and KNOWN-2, respectively (see top panel of Table 4.2). Quantities are somewhat below the Nash benchmark of $q(2) = 33.33$, which suggests some degree of tacit collusion (our second benchmark). In markets with four players, we obtain averages of 20.88 in Unknown-4 and 19.83 in Known-4 (Table 4.2) which is rather close to the Nash benchmark of $q(4) = 20$. This is in line with previous Cournot experiments (see Huck et al. (2004)).

	Supergame 1		Supergame 2		Difference	
	q	r	q	r	q	r
U2	29.26***	12.09***	32.49***	16.01***	3.22**	3.93*
	(1.129)	(2.449)	(1.795)	(2.982)	(1.199)	(2.157)
K2	31.47***	6.07***	30.08***	4.46***	-1.39	-1.61
	(1.033)	(1.111)	(0.981)	(1.023)	(1.177)	(1.219)
U4	20.88***	27.19***	22.83***	30.13***	1.95**	2.94
	(0.430)	(1.621)	(0.698)	(4.295)	(0.846)	(4.764)
K4	19.83***	18.17***	20.27***	17.98***	0.43	-0.19
	(0.566)	(2.278)	(0.694)	(2.290)	(0.633)	(3.740)
Observations	4800	1800	4800	1800	9600	3600
Adjusted R^2	0.847	0.623	0.822	0.552	0.157	0.254
U2 − K2	-2.21	6.02**	2.41	11.55***		
	(1.531)	(2.689)	(2.046)	(3.152)		
U2 − U4	8.38***	-15.10***	9.65***	-14.12***		
	(1.149)	(2.526)	(2.017)	(4.765)		
U4 − K4	1.05	9.03***	2.57**	12.15**		
	(0.711)	(2.796)	(0.985)	(4.867)		
K4 − K2	-11.64***	12.10***	-9.81***	13.52***		
	(1.061)	(2.808)	(1.306)	(2.147)		

Table 4.2: OLS-regressions for quantities and ranges.

Clustered at the matching-group level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The coefficients shown in the top panel indicate treatment averages because we suppress the regression constant. The bottom panel reports treatment differences, with statistical significance based on post-hoc tests. Standard errors in parentheses. Un stands for treatment Unknown-n, $n \in \{2, 4\}$ and likewise Kn for Known-n.

In supergame 2, several distinct changes occur. First, period-one quantities in the two Unknown treatments are at a higher level. Outputs are roughly seven units larger, consistent with more sophisticated play. As before, they are still rather similar for Unknown-2 and Unknown-4. Second, convergence of the Known and Unknown treatments takes more time compared to supergame 1, and the Unknown variants have higher outputs than their Known counterparts throughout. In the second repeated game, convergence takes at least until period 15 ($n = 4$) or period 22 ($n = 2$). A further difference between the two supergames is that play in Known-4 starts at a substantially increased level, namely at an average of about 20 (the Nash benchmark) straight from $t = 1$ on, and then remains roughly constant over time.

We observe an average quantity of 32.49 in Unknown-2 compared to 30.08 in Known-2 in supergame 2. With four players, averages are 22.83 in Unknown-4 and 20.27 in Known-4. There are two possible reasons for higher quantities in Unknown compared to Known. Either there are more markets with sophisticated play, as expected from Hypothesis 4.1, or there are fewer markets with tacit collusion. Both potential treatment differences would lead to higher aggregate quantities. In order to disentangle them, we categorize individual markets in a cluster analysis in Section 4.6.

4.5.2 Treatment Differences in Outputs and Ranges

In the following, we discuss the treatment effects on average quantities (Hypothesis 4.1(a)), average quantity range within markets (Hypothesis 4.1(b)), the difference of quantities between supergames (Hypothesis 4.2(a)), and the difference of within-market ranges between supergames (Hypothesis 4.2(b)) in comprehensive regression analyses. For the treatment comparisons, we perform a series of post-hoc Wald tests to check for the statistical significance of the differences between coefficients (bottom panel of Table 4.2). For the supergame comparisons, see the columns captioned “Difference” in the top panel of Table 4.2.

We observe higher average quantities in Unknown compared to Known in supergame 2, as expected. In markets with four players, the difference between Unknown-4 and Known-4 is 2.57 in supergame 2 and this is statistically significant ($t = 2.61$, $p = 0.016$). Average quantities in Unknown-2 are 2.41 higher than in Known-2 in supergame 2 as expected, however, the difference is insignificant ($t = 1.18$, $p = 0.252$). We do not get support for Hypothesis 4.1(a) in supergame 1. With four players, the difference between Unknown-4 compared to Known-4 is 1.05 and thus in line with Hypothesis 4.1(a), but the difference is insignificant

($t = 1.48$, $p = 0.154$). Quantities are smaller (on average by 2.21) in Unknown-2 compared to Known-2 in the first supergame. The effect is insignificant ($t = -1.44$, $p = 0.163$).

Result 4.1 (i). *In line with Hypothesis 4.1(a), outputs are larger in* Unknown *than in* Known *in supergame 2. This effect is significant for the four-player treatments. Further in line with Hypothesis 4.1(a), outputs are insignificantly larger in* Unknown-4 *than in* Known-4 *in supergame 1.*

In line with Hypothesis 4.1(b), the average range of quantities is significantly larger in Unknown than in Known for both supergames. With a difference of 6.02, the range is roughly doubled in Unknown-2 compared to Known-2 in supergame 1 ($t = 2.24$, $p = 0.035$). In supergame 2, the range is roughly 3.5-fold with a treatment difference of 11.55 ($t = 3.66$, $p = 0.001$). Comparing Unknown-4 and Known-4, the average range is roughly 50% larger in the first supergame with an absolute difference of 9.03 ($t = 3.23$, $p = 0.004$). Differences even increase in supergame 2. The range in Unknown-4 is 67% or 12.15 higher than in Known-4 ($t = 2.50$, $p = 0.020$).

Result 4.1 (ii). *Consistent with Hypothesis 4.1(b), ranges in both supergames are significantly higher in* Unknown *compared to their* Known *counterparts.*

When comparing outputs between supergames (see "Difference" in Table 4.2), we find increased outputs in supergame 2 which is in line with Hypothesis 4.2(a). Quantities in supergame 2 are significantly larger than in supergame 1 in our Unknown treatments. We obtain an average difference between supergames of 3.22 in Unknown-2 and 1.95 in Unknown-4, both significant to the 5% level (Unknown-2: $t = 2.69$, $p = 0.013$, Unknown-4: $t = 2.31$, $p = 0.030$). When the number of competitors is known, quantities are not significantly different between supergames. Known-2 averages even decrease between supergames ($t = -1.18$, $p = 0.250$), while the difference of Known-4 is close to zero ($t = 0.69$, $p = 0.500$).

Result 4.2 (i). *Consistent with Hypothesis 4.2(a), average outputs of the* Unknown *treatments in supergame 2 are significantly higher than in supergame 1. Also consistent with Hypothesis 4.2(a), this is not the case for* Known *treatments.*

Regarding Hypothesis 4.2(b), the range of the Unknown treatments increases between supergames for both market sizes (see "Difference" in the top panel of

Table 4.2). This difference is only statistically significant in Unknown-2 ($t = 1.82$, $p = 0.082$) but not in Unknown-4 ($t = 0.62$, $p = 0.543$). This finding is in contrast to the Known treatments where the range for both $n = 2$ and $n = 4$ even decreases between supergames. It turns out the differences between the changes of the ranges are significantly different between treatments for the two-player markets ($t = 2.23$, $p = 0.036$).

Result 4.2 (ii). *Consistent with Hypothesis 4.2(b), within-market ranges of outputs of the* Unknown *treatments are larger in supergame 2 compared to supergame 1, albeit this effect is not significant. Also consistent with Hypothesis 4.2(b), this is not the case for* Known *treatments.*

4.5.3 Guesses

Table 4.3 summarizes the guesses about the number of players in the Unknown treatments. There are two guesses in each supergame, one after $t = 1$ and one after $t = 25$. We refrain from making statements about statistical significance in this section, mostly because of the lack of independence of observations within matching groups.

It is, however, quite telling that subjects have a reasonably good idea about the number of players only in the first supergame of Unknown-2 (0.917, 0.854) but not in the second supergame (0.771, 0.883). In the first supergame of Unknown-4, guesses are hardly better than randomization (0.604, 0.583). In the second supergame, guesses are about half way between a random and a perfect guess (0.771, 0.792).

	Supergame 1		Supergame 2	
	t=1	t=25	t=1	t=25
U2	0.917	0.854	0.771	0.833
U4	0.604	0.583	0.771	0.792

Table 4.3: Share of correct guesses about the number of players in the Unknown treatments.

U2 stands for treatment Unknown-2, and U4 for Unknown-4.

Table 4.3 shows several findings pertinent to our research questions. First, guesses in Unknown-2 become less accurate in the second supergame, while guesses in Unknown-4 become more accurate. The variation seems remarkable and is consistent with more sophisticated play in the second supergame. Feedback on aggregate output of the other players should be a key driving force behind the guesses. As seen in Section 4.5.2, average output increases in supergame 2. Hence, subjects can be expected to guess more often they are in an $n = 4$ group both in the duopolies and the quadropolies. Consequently, guesses are getting worse in $n = 2$ markets and better in $n = 4$ markets in the second supergame. Second, basic intuition about learning suggests that subjects' second guess ($t = 25$) should be better than the first ($t = 1$), but this is not the case in supergame 1. Guesses are worse after $t = 25$ by about -6.3 ($n = 2$) and -0.21 ($n = 4$) percentage points. In supergame 2, guesses do improve in $t = 25$ by 6.2 ($n = 2$) percentage points, but for $n = 4$ the improvement (0.21) is close to zero again. A third finding in Table 4.3 is that guesses are (weakly) better in Unknown-2 than in Unknown-4. This effect is particularly pronounced in supergame 1.

4.6 Cluster Analysis

In this section, we deepen the analysis by looking at individual markets and by clustering them into different classes. Individual-level quantities for all markets are shown in Appendix 4.10.1. They exhibit highly heterogeneous behaviors and market outcomes.

To categorize the heterogeneous markets into distinct groups, we conduct a k-means cluster analysis (MacQueen et al., 1967) which partitions the data into k clusters such that each observation belongs to the cluster with the nearest mean. The k-means analysis minimizes within-cluster variances (squared Euclidean distances). The number of clusters and the clustering criteria are chosen exogenously. Accordingly, one has to be careful with attributing causal inferences here. The quality measure of the k-means analysis, η^2, measures the proportional reduction of the within-cluster sum of squares compared to the total sum of squares.

As for the clustering criteria, we use the average output and the average range, both taken at the market level. These criteria are suitable to identify the clusters we expect to see according to our benchmarks and previous results.

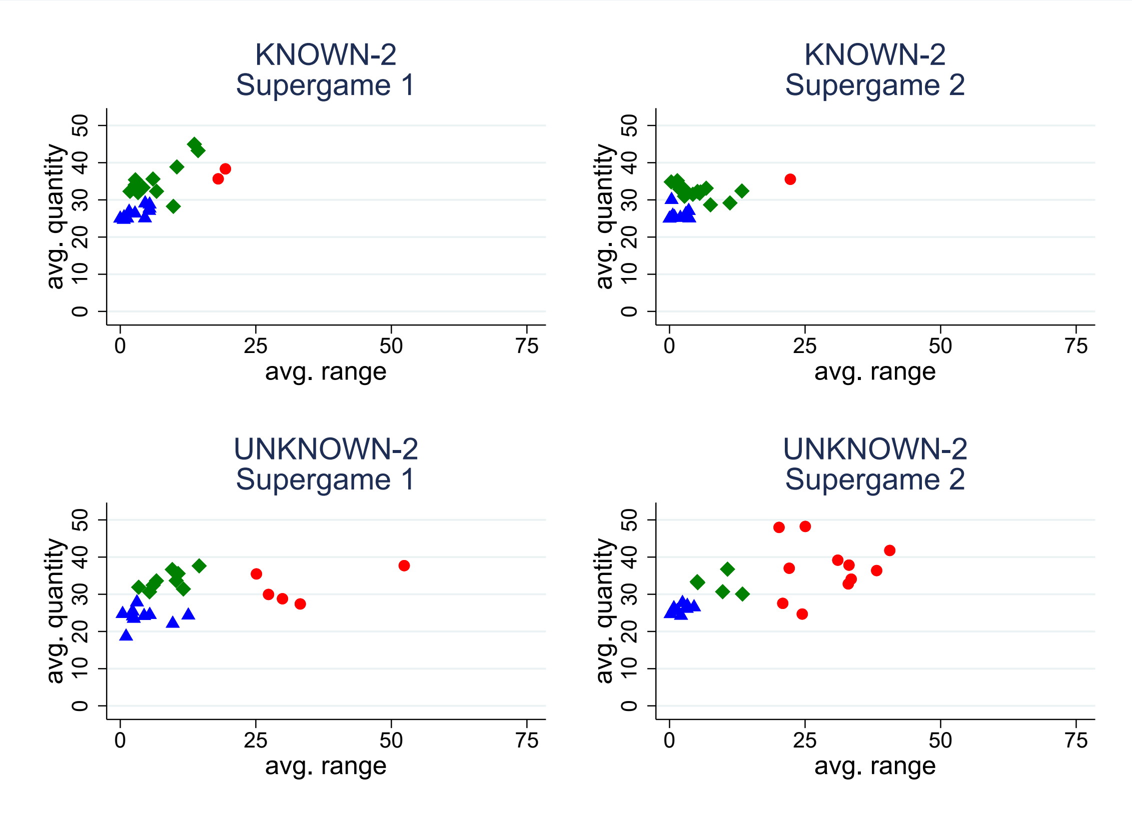


Figure 4.3: Cluster analysis of the duopolies.

Each dot shows one group, different color for different clusters - green squares: Nash, blue triangles: cooperative, red circles: sophisticated.

4.6.1 Two-Player Markets

In two-player markets, we expect to see our three possible benchmarks and consequently choose $k = 3$ clusters. These are:

- Nash play, characterized by symmetric outputs, and average output per player at Nash level,
- cooperative play, characterized by symmetric outputs, and average output per player at a below-Nash level,
- sophisticated play, characterized by asymmetric outputs, and average output per player at an above-Nash level.

The number of clusters appears to be appropriate. For $k = 3$, we have $\eta^2 = 0.70$, meaning three clusters reduce 70% of the variation of all observations. Using $k = 4$

clusters instead does not lead to an improvement since $\eta^2 = 0.70$, same as with $k = 3$.

Figure 4.3 shows a scatter plot of group-level average quantities and average ranges by supergame and treatment. Comparing the clustered data points to our benchmark, we can identify all predicted groups: Blue dots represent collusive clusters with low output levels and low range. Nash behavior (green) is characterized by higher outputs but also somewhat higher range. Sophisticated play (red dots) finally exhibit substantially higher ranges and higher outputs.

The k-means analysis further confirms our results from Section 4.5.2. In the Known treatments, we see predominantly Nash and collusive behavior. In the Unknown treatments, there is clearly sophisticated behavior identifiable, but also Nash markets and even some collusive groups are present (see Table 4.4 for the precise count of the three clusters). In the second supergame, however, we see many markets with sophisticated play and a decrease in Nash play.

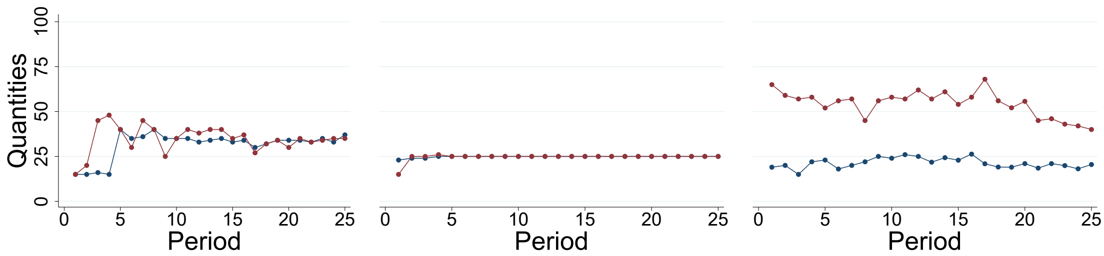
We show an example of the chosen quantities over time for each cluster in Figures 4.4a–4.4c. For a complete series of figures of all markets, see Appendix 4.10.1.

	Super-		Nash			cooperative			sophisticated	
	game	count	π low-q	π high-q	count	π low-q	π high-q	count	π low-q	π high-q
K2	1	11	923.32	989.55	11	1196.72	1241.65	2	780.94	1000.53
K2	2	14	1063.45	1129.94	9	1196.42	1257.85	1	697.52	1269.40
U2	1	9	945.19	1073.66	10	1114.47	1274.39	5	511.88	1519.01
U2	2	5	941.49	1177.89	8	1201.58	1233.54	11	493.38	1233.99

Table 4.4: Average payoff of low-output (π low-q) and high-output (π high-q) players by cluster for two-player markets.

We identify one high-output player per group as the player with the highest total output over all periods per supergame; likewise for the low-output player. U2 stands for treatment Unknown-2, and K2 for Known-2.

To illustrate the differences within the clusters of the k-means analysis, we show the average profits per cluster of the player with the lowest average output and the player with the highest average output of each market in Table 4.4. As expected, joint profits are highest in the cooperative cluster. Nash profits are lower but still rather symmetric. Sophisticated clusters have asymmetric payoffs and the lowest joint profits.



(a) Nash (b) cooperation (c) sophisticated

Figure 4.4: Examples of clusters in two-player markets.

4.6.2 Four-Player Markets

The heterogeneity between groups also exist for four-player markets (see Appendix 4.10.1 for group-level figures). We dismiss collusion here as an option and choose $k = 2$ clusters. The expected clusters are:

- Nash play, characterized by symmetric outputs, and average output per player at Nash level,
- sophisticated play, characterized by asymmetric outputs, and average output per player at an above-Nash level.

In terms of the quality of the k-means analysis, two clusters seem reasonable to categorize the data meaningfully. We obtain $\eta^2 = 0.49$ for $k = 2$, meaning two clusters reduce 49% of the variation of all observations. While we find an increased $\eta^2 = 0.71$ for $k = 3$, we stick to $k = 2$ because there are no-ex ante reasons to allow for $k = 3$, and we already cover 47 of the 48 markets with only two clusters.[7] To illustrate the predicted outcomes, we show an example of each cluster in Figures 4.6a–4.6b.

Figure 4.5 is a scatter plot of the clusters suggested by k-means. In four-player markets, only small differences in outputs between groups occur, but they differ within groups. We observe a large fraction of sophisticated clusters (see Table 4.5 for a precise count). Table 4.5 also shows the differences in payoffs between clusters. Even the Nash cluster has substantial payoff differences, suggesting that play has not perfectly converged to Nash at the individual level. As with the duopolies, sophisticated clusters have low joint profits and large payoff differences.

[7] A k-means analysis with three clusters would additionally identify only one outlier with extremely high average range as a third cluster. See the bottom right panel of Figure 4.5.

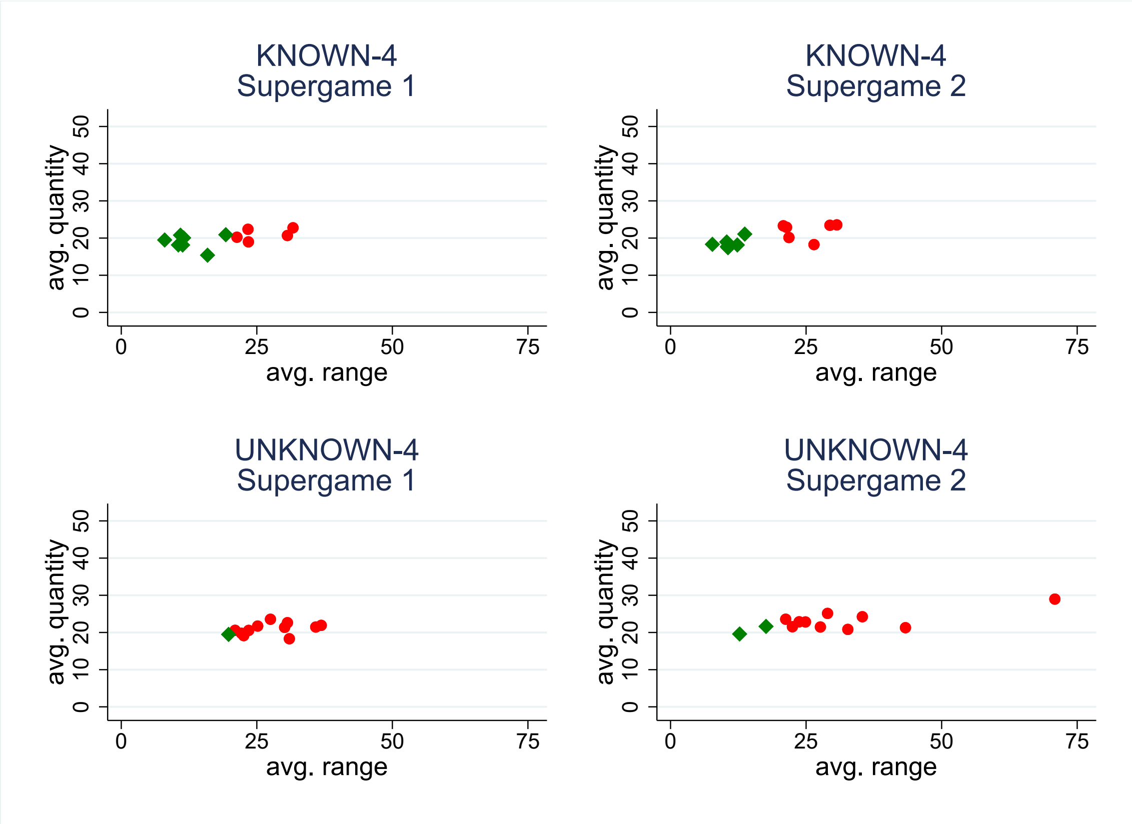


Figure 4.5: Cluster analysis of the quadropolies.

Each dot shows one group, different color for different cluster - green squares: Nash, red circles: sophisticated.

In Section 4.4, we argue that the second supergame should be characterized by more sophisticated play. Figure 4.5 shows that a shift to higher ranges and higher quantities between supergame 1 and supergame 2 also occurs in four-player markets. In supergame 2, we can identify many groups with sophisticated patterns, while supergame 1 exhibits much lower ranges. This result supports our hypothesis. Independent of the true number of players, players try to use the uncertainty to gain higher payoffs by producing more output.

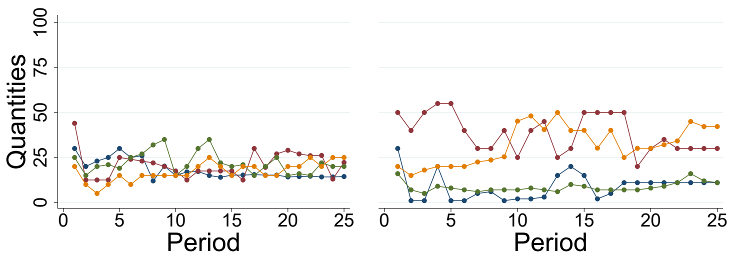
4.6.3 Does it pay to be sophisticated?

We analyze the profitability of the sophisticated strategy by looking at ex-post payoffs in the different k-means clusters realized in Unknown-2 and Unknown-4. We compare a high-output sophisticated player to an average Nash and, in Unknown-2, to an average cooperative player.

	Super-game	Nash			sophisticated		
		count	π low-q	π high-q	count	π low-q	π high-q
K4	1	7	328.84	510.25	5	184.07	482.03
K4	2	6	362.78	548.08	6	171.66	386.37
U4	1	1	251.24	535.16	11	167.01	504.66
U4	2	2	287.82	424.05	10	125.54	355.68

Table 4.5: Average payoff of low-output (π low-q) and high-output (π high-q) players by cluster for four-player markets.

We identify one high-output player per group as the player with the highest total output over all periods per supergame; likewise for the low-output player. U4 stands for treatment Unknown-4, and K4 for Known-4.



(a) Nash (b) sophisticated

Figure 4.6: Examples of clusters in four-player markets.

The Unknown-2 data in Table 4.4 indicate that a high-output sophisticated player realized a payoff of 1519.01 in supergame 1 and 1233.99 in supergame 2. The average player in the Nash cluster earns 1009.42 and 1059.69 in supergames 1 and 2, respectively. We note that the sophisticated player earns more than players in the Nash cluster. She even earns more than a player in the cooperative cluster, where the average payoffs are 1194.43 (supergame 1) and 1217.56 (supergame 2).

This additional payoff a sophisticated player earns in Unknown-2 has to be contrasted to potential losses this strategy faces in Unknown-4 (because, ex ante, the player does not know which group size she faces). From Table 4.5, a high-output player in the sophisticated cluster realizes a payoff of 504.66 in supergame 1 and 355.68 in supergame 2. The average player in the Nash cluster earns 409.47 and 342.06 in supergames 1 and 2, respectively. We conclude that even in the four-player case, the sophisticated strategy pays—although the difference to Nash-cluster payoffs is not big.

Overall, playing a high-output strategy appears to pay. A disclaimer to this result is that we are giving the sophisticated strategy the best chance, since we take the successful high-output player and compare it to the average player in the Nash cluster. Also, the sophisticated strategy is inefficient (from the players' perspective) because the increased output hurts the other players whose average payoff is substantially lower than in the Nash cluster (306.89 in supergame 1 and 225.46 in supergame 2).[8] A third disclaimer is that, with more supergames, too many players may adopt the strategy, so eventually, it may backfire.

4.7 Conclusion

In many situations, the actual number of players in a game will not be known with certainty. In auctions (Levin & Ozdenoren, 2004; Harstad et al., 2008), social dilemmas (Ashlagi et al., 2006; Hillenbrand & Winter, 2018; Kim, 2018; Mill & Theelen, 2019; Hillenbrand et al., 2020) or contests (Lim & Matros, 2009; Boosey et al., 2017), participants may only have a rough idea of how many players are in the game. Also in oligopolistic markets (Janssen & Rasmusen, 2002; Ritzberger, 2009), players may be unaware which of their competitors are actually active, suggesting the situation resembles a game with an unknown number of players.

We add to this literature the analysis of Cournot oligopolies, and we do so in a repeated-game context. A static (one-shot) analysis of Cournot with an unknown number of players is straightforward. In theory, players play the static Nash equilibrium, maximizing expected profits. The repeated-game analysis of this framework, however, gives rise to novel and intriguing strategies. Sophisticated players may try to exploit the uncertainty about the number of players by producing the output of multiple players, attempting to gain above Nash-profits.

We use a laboratory experiment to gain insights on the actual behavior of players in this setup. In our experiment, players could be in a two-player or four-player Cournot-market, both cases equally likely, to which they only received feedback about the total output of their rivals. We compare this treatment with an unknown number of players to standard oligopolies, where participants know the true number of players. Subjects play two finitely repeated supergames.

Our experimental results provide indication that sophisticated play actually occurs. In supergame 1, outputs converge to Nash output both in two- and four-player markets quickly—we do not see much sophisticated play. In supergame 2, players

[8] Efficiency from a consumer or welfare perspective increases, of course, since sophisticated play boosts aggregate output.

increasingly adopt sophisticated strategies. Outputs rise and asymmetries (measured by the range of within-market outputs) increase in supergame 2 compared to supergame 1, when the number of players is unknown. Our hypotheses is further supported by the percentage of correct guesses about how many players are in the market. Apparently, many players were not able to distinguish between high outputs produced by one sophisticated player or three players plainly producing their Nash output.

A potential downside of these sophisticated strategies is, if too many players adopt these strategies, they become a costly gamble. For our data with a twice repeated supergame, such worries seem unwarranted. Even though we do observe episodes of persisting wars of attrition, we find that, regardless of the number of players and in both supergames, players in the sophisticated cluster have higher payoffs than players in the Nash cluster. Overall, it appears that the sophisticated strategy pays.

Our findings also shed light on the conjectural variations literature.[9] This line of research is about how players believe their rivals will react if they vary their output. With symmetric players, it is difficult to see how any heterogeneity in conjectures will arise. Experimental tests of consistent conjectural variations (Holt, 1985) show convergence of play to Nash equilibrium or tacit cooperation, but find no evidence in favor of the consistent-conjectures outcomes. Our experiments with an unknown number of players can be interpreted as a novel approach to conjectural variations. Sophisticated players believe their rivals to myopically adapt to their increased output. Hicks (1935) and Stackelberg (1934) allowed for differences in conjectural variations, and there are parallels of our sophisticated players to Hicks' active player and the Stackelberg leader.

[9]See the original contributions by Bowley (1924); Stackelberg (1934); Hicks (1935) and Leontieff (1936). More recent references include Bresnahan (1981); Daughety (1985); Makowski (1987); Dockner (1992); Sabourian (1992); Cabral (1995); Dixon & Somma (2003); Müller & Normann (2005).

4.8 Acknowledgments

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4.10 Appendix

4.10.1 Individual Markets

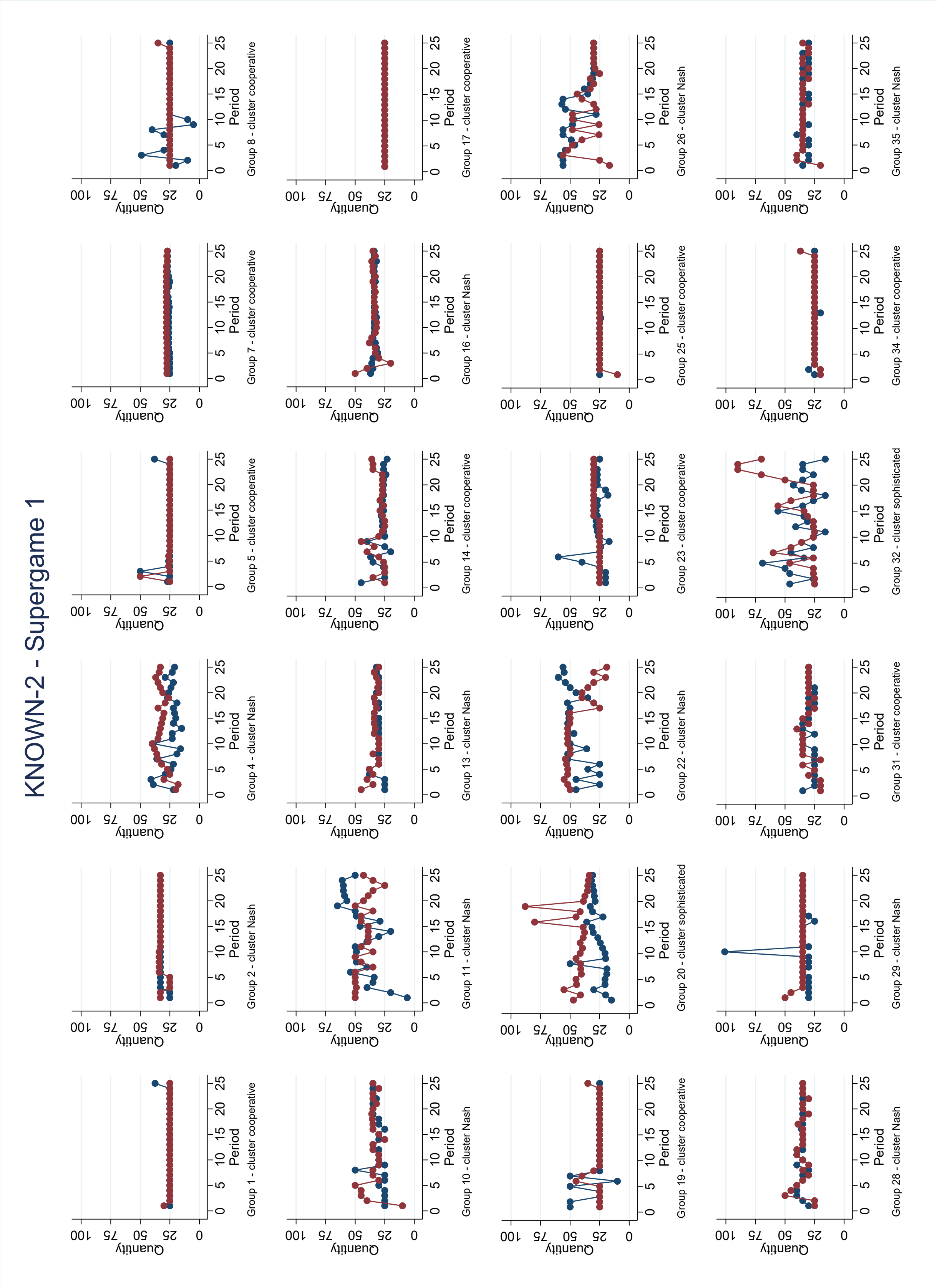

Figure 4.7: KNOWN-2, Supergame 1, Individual Markets.

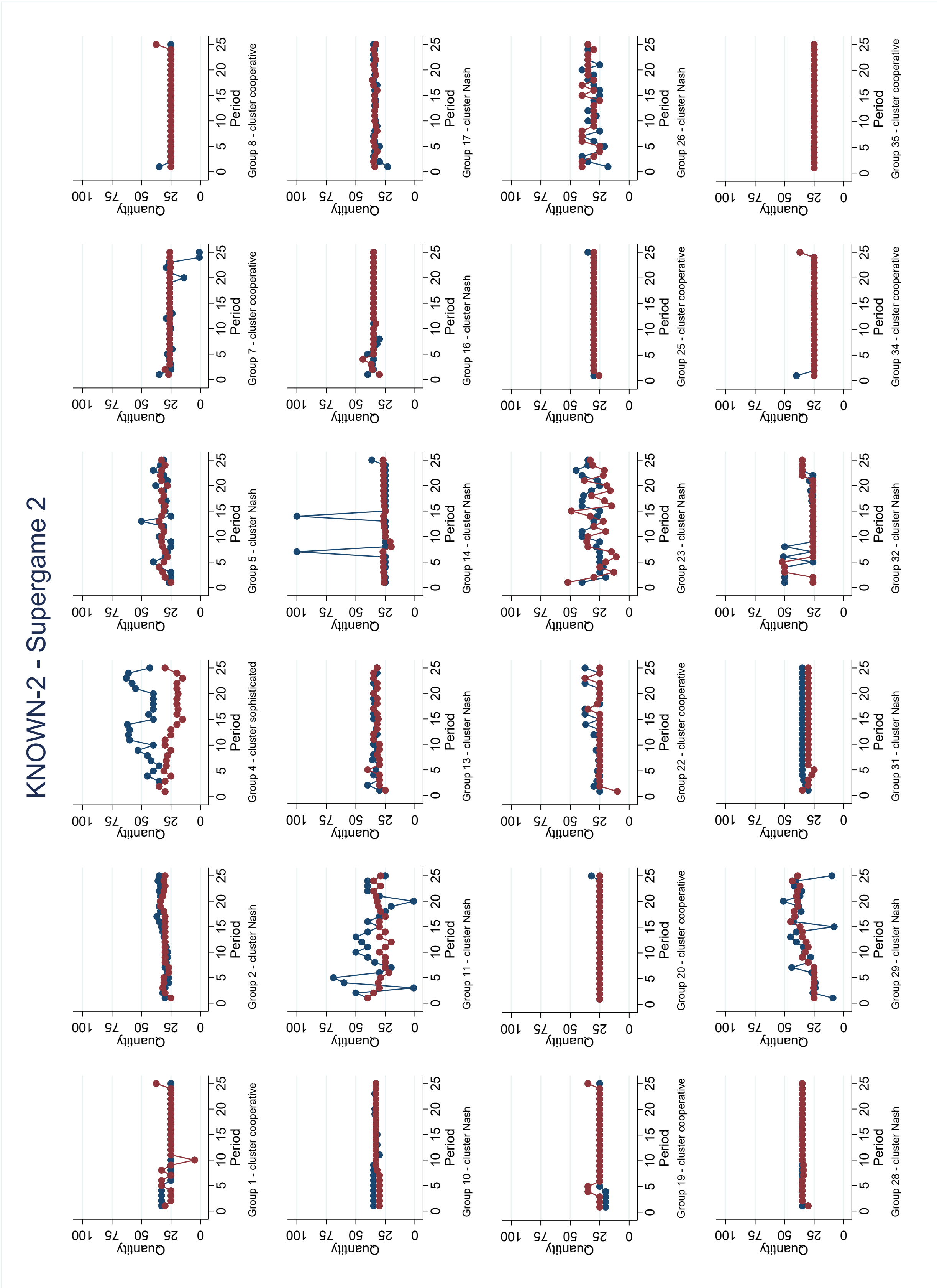

Figure 4.8: KNOWN-2, Supergame 2, Individual Markets.

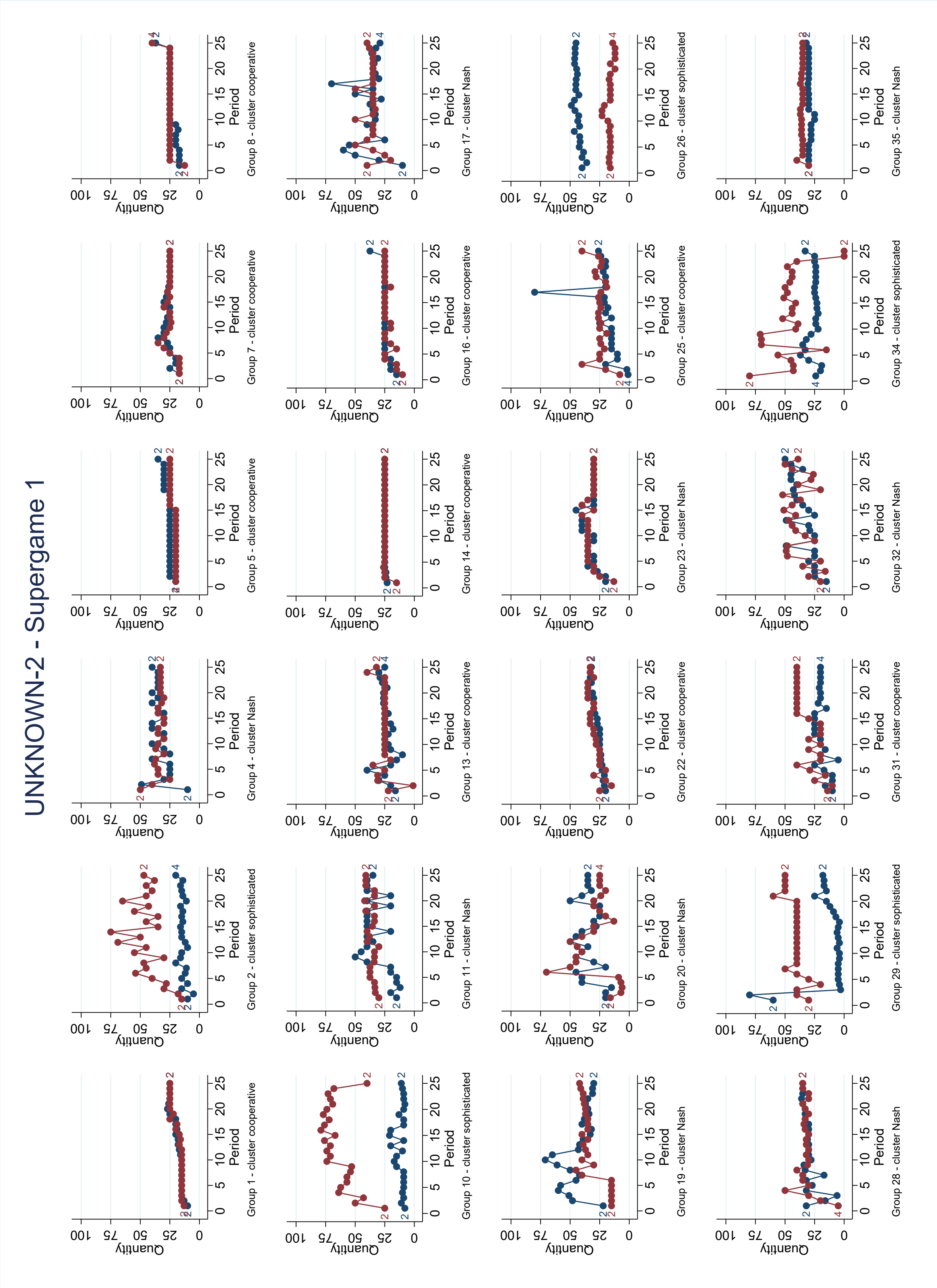

Figure 4.9: UNKNOWN-2, Supergame 1, Individual Markets.

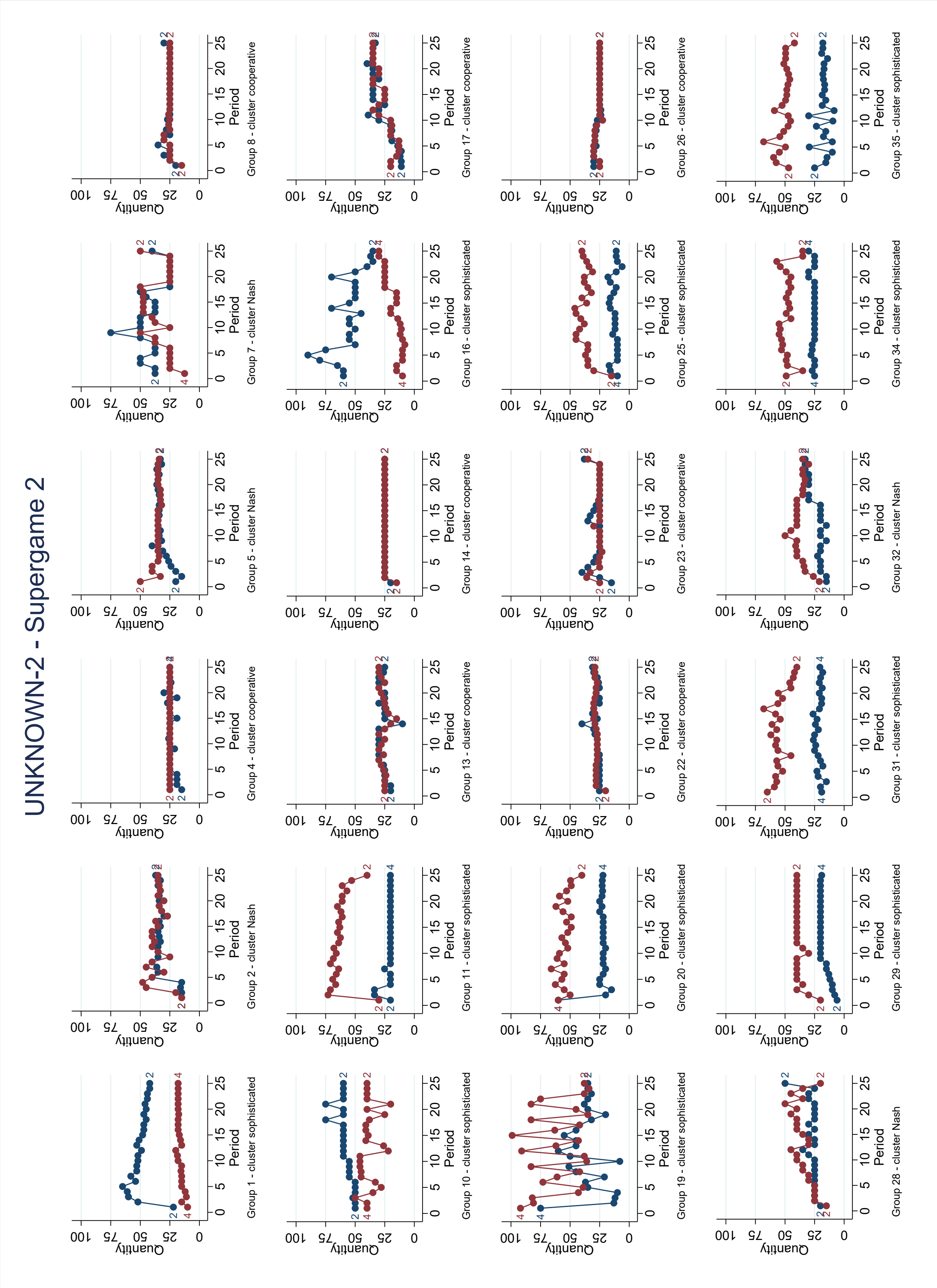

Figure 4.10: UNKNOWN-2, Supergame 2, Individual Markets.

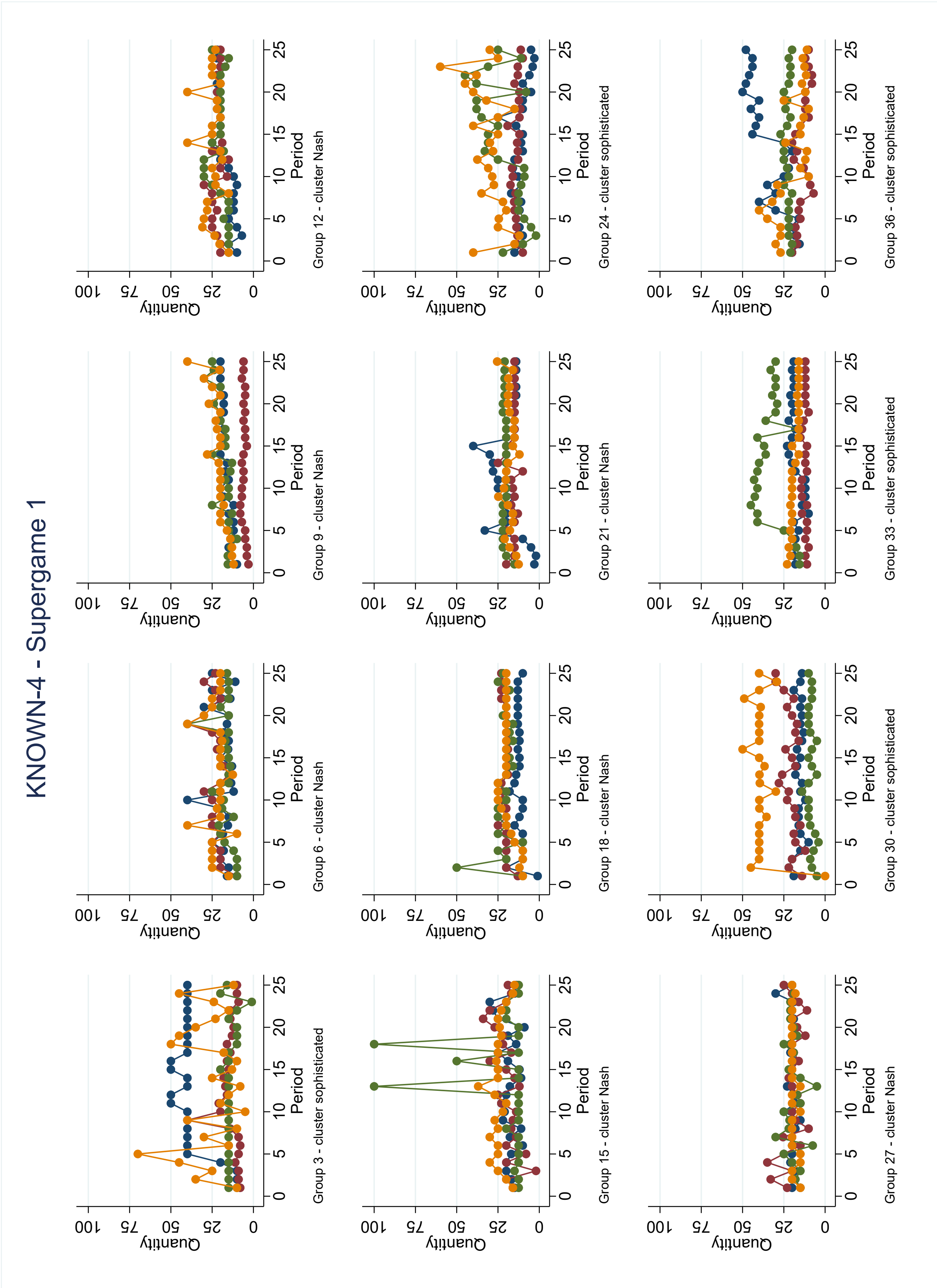

Figure 4.11: KNOWN-4, Supergame 1, Individual Markets.

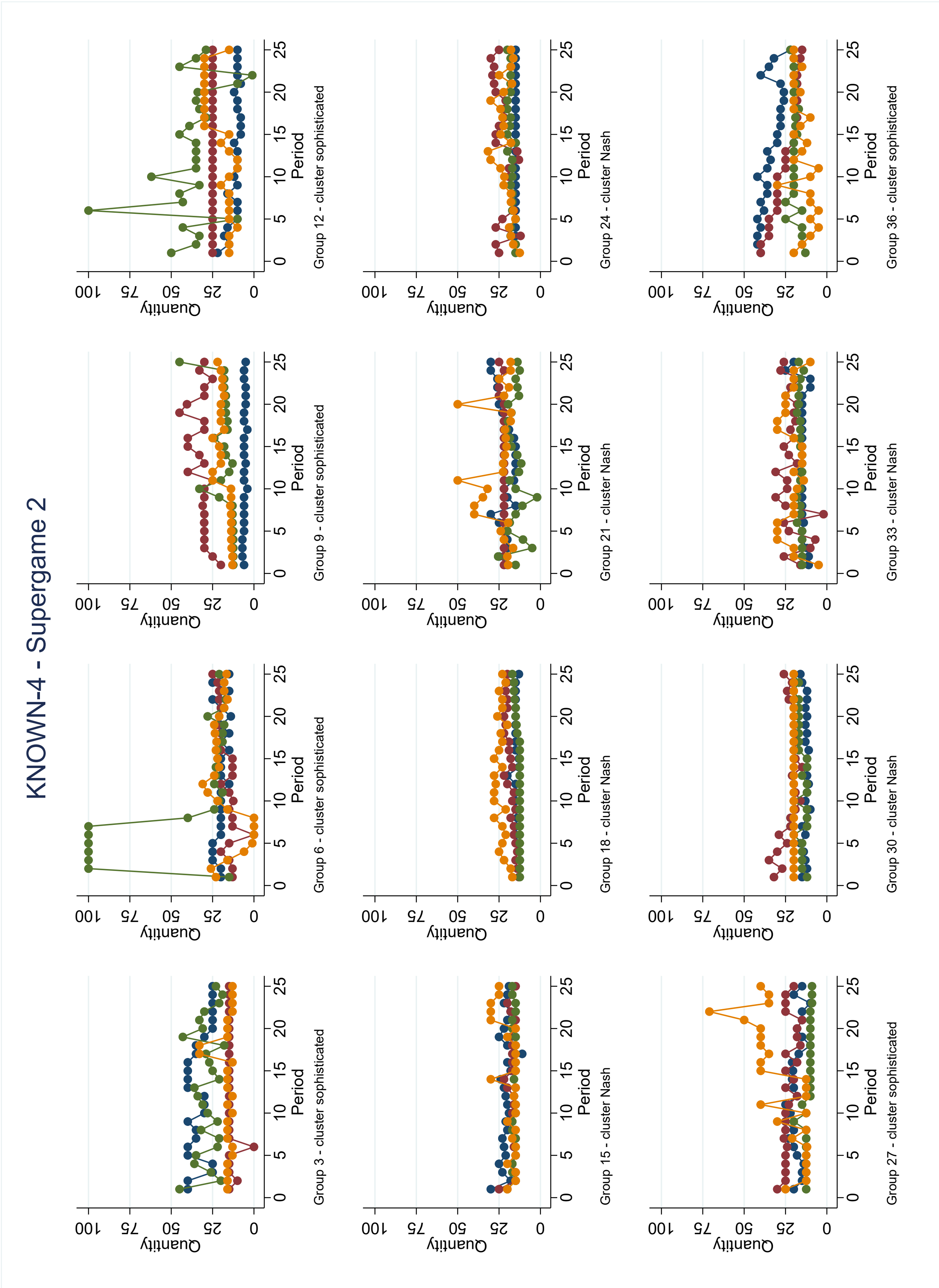

Figure 4.12: KNOWN-4, Supergame 2, Individual Markets.

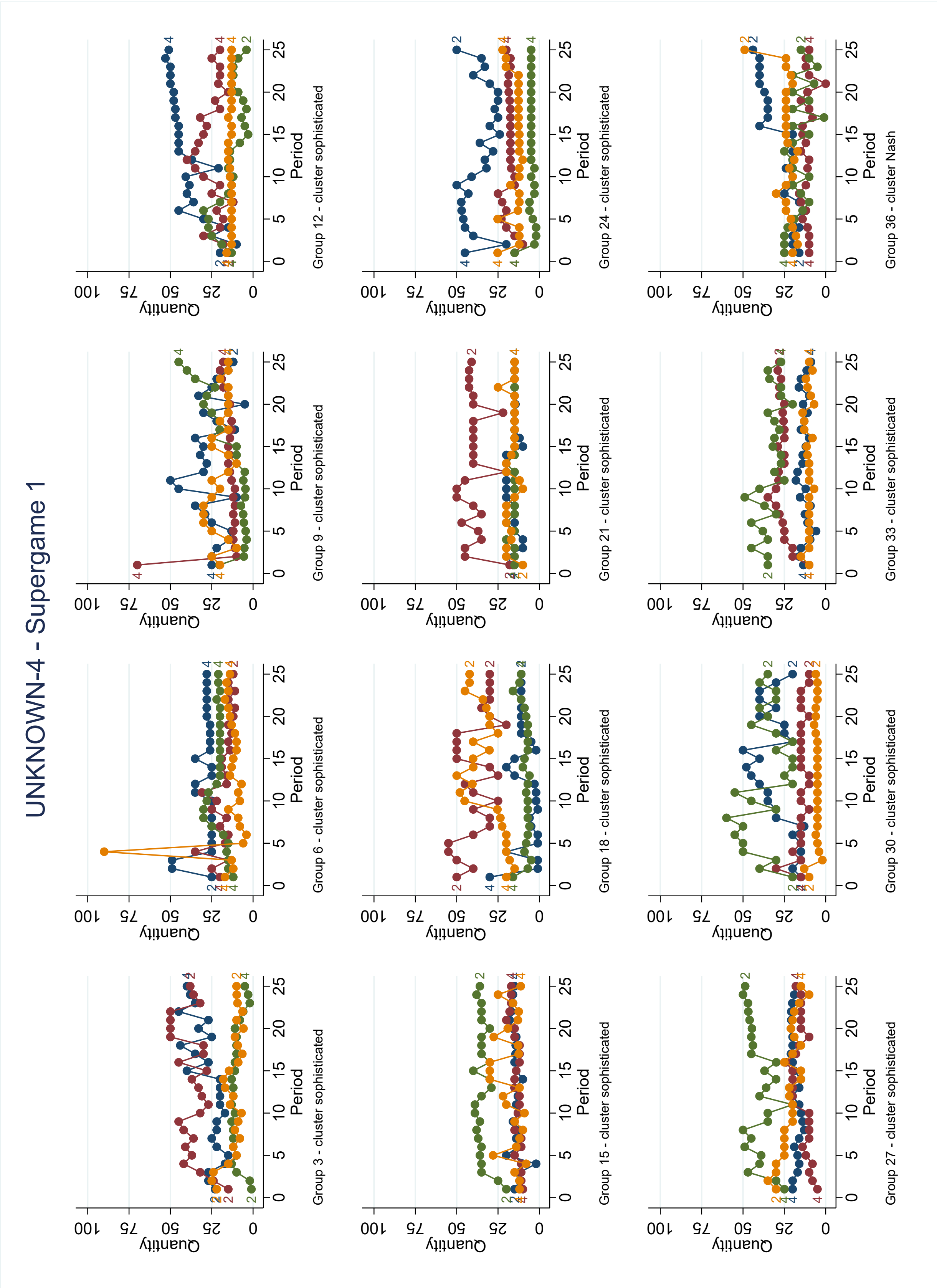

Figure 4.13: UNKNOWN-4, Supergame 1, Individual Markets.

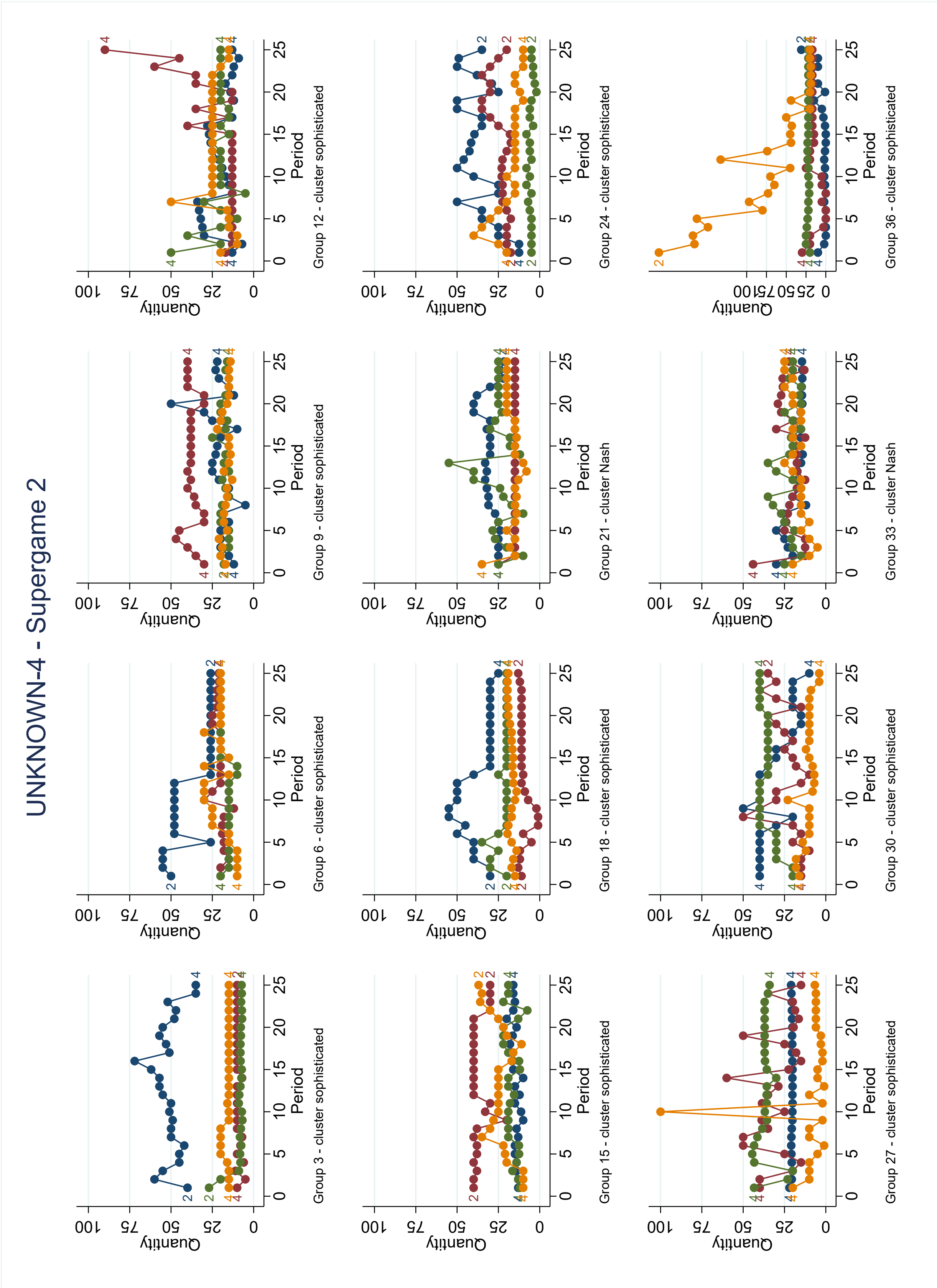

Figure 4.14: UNKNOWN-4, Supergame 2, Individual Markets.

4.10.2 Experimental Instructions

Instructions for all groups — differences between treatments in italics — translated from German

Hello and welcome to our experiment!

Please read these instructions carefully to the end.

In this experiment you will repeatedly make decisions and can thereby earn money. How much you earn depends on your own decisions and on the decisions of other randomly assigned participants. Please do not talk to your neighbors and be quiet during the entire experiment. If you have a question, please raise your hand - we will then come to your booth and will answer your question personally. All participants receive (and are currently reading) the same instructions. You will remain completely anonymous to us and to the other participants. We will not save any data in connection with your name. At the end of the experiment, you will be given your profit paid in cash.

Firms and Markets

In this experiment you will have to make decisions for a firm in a market. You are active either on a market with 2 firms or on a market with 4 firms (i.e. either you and 1 other firm or you and 3 other firms). All firms are producing and selling the same product.

Your task is to decide how much of this good you want to produce. The smallest unit you can produce is 0.01. The following rule applies: The larger the produced quantity of the good (i.e. total quantity), the smaller the price which you and the other firms get for the product. The price decreases with each additional unit by 1 ECU. "ECU" is our laboratory currency and stands for "Experimental Currency Unit". The price can be 101 ECU at the maximum and not smaller than 0 ECU – if the total quantity is 101 or larger, the market price will be set at 0.

Market price = 101 – Total Quantity of all Firms (including your quantity)

If total quantity is larger than 101: Market price = 0

Profit calculation

For each produced unit of the good you have costs of 1 ECU. You profit is calculated by the quantity of produced units multiplied with the market price less the costs per produced unit.

Profit = (Market price – production costs of 1 ECU) * your quantity

If the market price is smaller than 1 ECU you can incur losses, as you still have to pay the costs of production of 1 ECU per unit. The consumers on the market always buy all units of the good at the market price. Consumers are simulated by the computer, as they are not represented by participants of this experiment. To simulate potential profits, we provide you with a "profit calculator" which you can use prior to your quantity decision if you like to. In order to calculate a profit with the profit calculator, you have to enter you own quantity and the sum of quantities of all other players (that is the total quantity minus your own quantity).

Procedures

In each period each firm decides on a quantity which it wants to produce in this period. At the end of each period, you will be informed about the total quantity of all firms in your market, the resulting market price and your own profit. Additionally, it will be displayed how much ECU you already earned in the entire game.

Games

The experiment is divided into two games which consist of 25 periods each. Within one game, you play 25 periods always with the same firm or same firms in one market, respectively. After 25 periods, the first game is over and a new game will be started. After two games (i.e. after 2x25 periods) the experiment is over. You start each game with an initial endowment of 9,000 ECU. Matching At the beginning of a new game you will be randomly allocated to a market in which either 1 other firm (probability 50%) or 3 other firms (probability 50%) are active. You will be informed about the number of firms you play with in the market at the beginning of each game *You will only at the end of a game be informed about the actual number of firms on this market, i.e. if you have been with 1 or 3 more firms in a market. During a game you will not be informed about the matching. You have to guess two times how many firms you are playing with during a game. Once after the first* *period and once at the end of each game. In total, you have to take 4 guesses. 2 guesses for the first game and 2 guesses for the second game. Each time you guessed the number of firms on your market correctly, you will get 1 Euro to your account.*

Payoff

At the end of the experiment you will be informed about the sum of your profits over all periods and games. You receive 1 Euro per 3000 ECU you earned during the entire experiment. *Additionally, you receive the money for the correct guesses.*

Examples

In order to illustrate the calculation of the profits more understandable, some examples are shown in the following:

Example 1:

Suppose you decide to produce 16 units. The quantity of the other firm or the total quantity of all other firms in the market, respectively, is 78 units. The total quantity of all firms in the market is then 94 units.

The market price is then: 101 – 94 = 7 ECU.

You profit is then: (7-1) * 16 = 96 ECU.

Hence, your profit in this period is 96 ECU.

Example 2:

Suppose you decide to produce 56.3 units. The quantity of the other firm or the total quantity of all other firms in the market, respectively, is 89.7 units. The total quantity of all firms in the market is then 146 units.

The market price is then: 101 – 146 = -45 ECU.

You profit is then: (0-1) * 56.3 = -56.3 ECU.

Hence, you make a loss of -56.3 ECU in this period.

Good luck!

Chapter 5

Conclusion

Many factors and market conditions influence the degree of competition and determine the likelihood of collusion in a market. In this thesis, I present three essays about the effects of various factors on competition and collusion. To test theoretical predictions and to gain more knowledge on the mechanism behind, I use laboratory market experiments.

In Chapter 2, I look at the different prices of cartel insiders and outsiders in experimental markets, when only a subset of the firms can communicate. I use a repeated, capacity-constrained price game with asymmetric firms on the basis of the model by Bos & Harrington (2010) and test it in the laboratory. The data from the experiment show that communication between these firms enables partial cartels to form. Moreover, we see that a partial cartel is sufficient to increase market prices for all firms in the market. However, prices of insiders and outsiders are not on the same level, which contradicts common theoretical predictions. The difference in prices comes from considering explicit cartels, in which cartel members can communicate with each other. I explain that communication allows cartel members to overcome a potential coordination problem and enables an outcome in (joint) mixed strategies, which would not be feasible with tacit collusion. The results therefore underline the anti-competitive effect of communication, even if only a subset of firms can communicate, and also shed light on the difference between tacit and explicit collusion and the resulting market outcomes.

In Chapter 3, my co-author and I provide experimental evidence for the anti-competitive effects of minority shareholdings between direct competitors. We use a simple version of a model by Gilo et al. (2006) and vary the number of symmetric, non-controlling shares firms own of each other in a static and dynamic Bertrand setting with homogeneous goods. Previous theory states that only coordinated effects arise if the discount factor decreases with the degree of cross-ownership. We provide novel theoretical predictions about the impact of minority shareholdings on prices based on the concept of Quantal Response Equilibrium (McKelvey & Palfrey, 1995). Contrary to previous Nash predictions, we explain that passive partial cross-ownership reduces the incentives to compete and favors unilateral effects that result in higher average prices. We test these hypotheses in a laboratory experiment. Our results show an increase in price levels with the degree of partial cross-ownership due to the existence of unilateral effects. Competition is further softened by coordinated effects. A lower discount factor, based on partial cross-ownership, actually materializes in more tacit collusion. Hence, partial cross-ownership decreases competition in through both unilateral and coordinated effects.

In Chapter 4, my co-authors and I analyze uncertainty about the number of players in Cournot oligopolies. We use a repeated game setup, where cost and demand conditions are known, but the number of players is not. We argue that this kind of uncertainty may lead to a novel strategy. A sophisticated player produces more than the static Nash equilibrium output, attempting to fool other players into believing there are more players than is actually the case. We explore this scenario in laboratory experiments to investigate behavior in this setting. Our data confirm that an unknown number of players leads to sophisticated play. Total output increases, but quantities are distributed asymmetrically. Hence, uncertainty about the number of competitors can lead to a market outcome closer to perfect competition.

Overall, this thesis illustrates that market outcomes can be strongly affected by factors such as communication, uncertainty about the number of players or changes in the market structure through minority shareholdings.

Understanding these effects and mechanisms enable antitrust authorities to intervene and prevent negative repercussions as to protect consumer welfare.

This thesis therefore suggests that limiting the threshold of allowed minority shareholdings in competitors could potentially preserve competition in affected markets. Also, preventing communication between competitors might keep prices low - not only for communicating firms but also for uninvolved firms in the respective market. Lastly, concealing or obfuscating the number of competitors might lead to more output than under certainty, potentially increasing the welfare for consumers.

For the future, more research on identifying additional factors, and a constant dialogue between practitioners and academia is needed. This better enables lawmakers and antitrust authorities to make well-informed decisions, giving consumers the benefits of competition, including low prices, high quality and innovation.

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Eidesstattliche Versicherung

Ich, Herr Johannes Josef Odenkirchen, versichere an Eides statt, dass die vorliegende Dissertation von mir selbstständig und ohne unzulässige fremde Hilfe unter Beachtung der „Grundsätze zur Sicherung guter wissenschaftlicher Praxis an der Heinrich-Heine-Universität Düsseldorf“ erstellt worden ist.

Düsseldorf, der 12. Mai 2020

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Unterschrift