

Price Competition in Oligopolies with Demand Rationing: Experimental Studies

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vorgelegt von
Martin Jacobs, M.Sc.
aus Wolfsburg

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Dekan:	Prof. Dr. Achim Walter
Erstbegutachtung:	Prof. Dr. Till Requate
Zweitbegutachtung:	Prof. Horst Raff, PhD

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Acknowledgements

When I started to work as a research assistant at the Chair of Innovation, Competition Policy and New Institutional Economics at Kiel University in April 2012, there were many topics in microeconomics which I found interesting. One might therefore argue that I went well with this chair with the wide range of topics its research covers. However, the downside of my broad interest was that I had no idea which topic I should concern myself with in my PhD project. I owe the idea for my PhD project to my first supervisor Prof. Dr. Till Requate. Prof. Requate drew my attention to a topic in industrial organization he had worked on almost 20 years ago and for which sound experimental evidence was still lacking: Bertrand-Edgeworth price competition with increasing marginal costs. An inquiry of the relevant literature revealed that there were almost no experiments which considered such markets. Moreover, whereas the experimental literature on oligopoly conduct in related market settings was less scarce, gaps could be detected also in experimental investigations of Bertrand-Edgeworth markets with fixed capacities and of two-stage Kreps-Scheinkman markets. Thus it came about that the dissertation at hand has a clear focus and contains a series of experimental studies of oligopoly markets where firms compete in prices and demand rationing matters.

I express my gratitude to my first supervisor Prof. Dr. Till Requate for his guidance and advice. He accomplished to find the balance between letting me develop my own ideas and instructing me when instruction was needed. His comments and the discussions with him helped tremendously to improve my work. At his chair I was provided with a work environment which enabled me to efficiently pursue the research agenda that led to the dissertation at hand. I would also like to thank Prof. Horst Raff, PhD for the second supervision of this dissertation.

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I Introduction

1 General overview of the topic

From the outset of experimental economics in the mid-20th century, studies of experimental oligopoly markets have played an important role in this field. The earliest experimental oligopolies in the literature stem from the late 1950s (Hogatt, 1959; Sauermann and Selten, 1959). They are based on the Cournot (1838) model and thus investigate firms' quantity choices. Soon afterwards, Fouraker and Siegel (1963) set a milestone with the systematic examination of their oligopoly markets where either quantity or price was the choice variable. The underlying model of their pricing markets is due to Bertrand (1883). Fouraker and Siegel were the first to conduct different treatments of an oligopoly experiment. They varied, one at a time, the number of competitors in the market and the amount of information disclosed to the participants, holding constant all other probably relevant features of their laboratory markets, and were thus able to isolate the effects of those variations.

Nowadays, experimental methods are widely established in economics. Conduct in oligopolies is still a frequent object of investigation in economic experiments, and the classic models by Cournot and Bertrand are still the most frequent basis for experimental oligopoly markets.¹ However, experiments based on more complex oligopoly models have come to complement that work since the 1990s. In contrast to the ample experimental results that have been obtained in laboratory Cournot and Bertrand markets, the evidence regarding some more complex models is still somewhat scattered. The dissertation at hand aims at closing some of the gaps in the recent literature on experimental oligopolies.

The classical oligopoly models suffer from some shortcomings. The Cournot (1838) model predicts the intuitive result that the market price will be the lower the more firms operate in the market and will converge to the competitive price when the number of firms approaches

¹ Huck et al. (2004) provide a survey of Cournot experiments. An overview of recent oligopoly experiments in general can be found in the survey by Potters and Suetens (2013). Engel (2007) presents a meta-study on oligopoly experiments.

infinity. The model assumes a uniform market price being determined as the market-clearing price given aggregate supply on the market. Yet, it remains dubious how the effective market price emerges in reality if firms only decide on their production quantities. The then necessary side assumption of a Walrasian *tâtonnement* which via a trial-and-error process determines the equilibrium price has often been deemed unrealistic. Bertrand (1883) criticized Cournot's approach of quantity competition and assumed instead that firms compete in prices. While that assumption appears reasonable, the thus modified model yields the unrealistic implication that the market price in equilibrium will equal the perfectly competitive price as soon as there are at least two firms in the market.

The experimental investigations presented in this dissertation are based on two amendments proposed to overcome those deficiencies. The first suggested modification is due to Edgeworth (1925). He proposed that capacity constraints be added to the Bertrand model. Capacity constraints can be either exogenously given or endogenously brought about via the introduction of increasing marginal production costs. In the first case, for each firm a capacity limit is set beyond which further supply is impossible; in the second case, for a firm's given price choice marginal costs from some point exceed the sales price so that the firm is unwilling to supply beyond that point. In both versions of the so-called Bertrand-Edgeworth model, equilibrium analysis is very intricate. In fact, the Bertrand-Edgeworth model does not possess any Nash equilibria in pure strategies.² Two chapters of the dissertation at hand are concerned with oligopoly experiments based on the Bertrand-Edgeworth model. The experiment presented in chapter II considers the case of increasing marginal costs, the one in chapter IV deals with fixed exogenous capacity constraints.

The second proposed modification is the model by Kreps and Scheinkman (1983) that may be regarded as a synthesis of the Cournot and the Bertrand model. In the two-stage Kreps-Scheinkman setting, firms decide on both quantities and prices. At the first stage, quantities are chosen and production costs are incurred; at the second stage, firms compete in prices after having been informed about their competitors' production quantities. Apart from its more realistic setting compared to the Cournot and Bertrand model, the Kreps-Scheinkman model also yields intuitive predictions. If suitable side assumptions are imposed, its outcome will be identical to the Cournot outcome. The experiment presented in chapter III investigates the effects of several variations within the Kreps-Scheinkman framework.

² This holds if one neglects degenerate cases with very low or high capacities where the equilibrium again corresponds to the Cournot (market-clearing pricing) or, respectively, the Bertrand (marginal cost pricing) outcome.

A common feature of both discussed modified models, and hence all three experiments in this dissertation, is that demand rationing plays a crucial role. Demand rationing occurs when firms are unable or unwilling to satisfy all customers who want to make a purchase for the stated price and therefore turn some customers away. Which customers are turned away has an impact on the demanded quantities other firms charging higher prices face. The equilibrium predictions for the oligopoly models considered in this dissertation significantly hinge upon the assumed demand rationing rule. There are two prominent demand rationing rules in the literature: proportional and efficient demand rationing. Proportional rationing implies that customers are served in random order. Efficient rationing, by contrast, assumes that customers are served in the order of their willingness to pay with those with the highest willingness to pay being served first. In real markets, the effective demand rationing rule is an empirical matter which can hardly be influenced. Proportional demand rationing is a realistic assumption for those markets where customers drop by stores and make purchases rather incidentally. Efficient rationing can rather be found in markets where customers have more well-defined purchasing plans and take additional costs, e.g. queuing time, in order not to be rationed.³ In the artificial setting of laboratory experiments with computer-simulated demand, it is possible to freely manipulate the rationing rule and isolate the effect of such a manipulation. Demand rationing tremendously affects the Nash equilibrium predictions in the underlying models. In Bertrand-Edgeworth markets, mean price and profit in the mixed-strategy Nash equilibrium are considerably higher under proportional than under efficient rationing. Hence, proportional rationing is predicted to lead to a more collusive market conduct. In Kreps-Scheinkman markets, the Cournot outcome result depends on the assumption of efficient demand rationing. Under proportional rationing, there will in general be a mixed-strategy Nash equilibrium which is more competitive than the Cournot outcome (Davidson and Deneckere, 1986).

2 Overview of the experiments and their results

The arrangement of chapters in the dissertation at hand conforms to the order in which the experiments were conducted. The study contained in chapter II is entitled "Bertrand-

³ Note that this argument is in a strict sense somewhat inconsistent. If a customer really incurred additional costs up to her willingness to pay by queuing, she would be better off with making a purchase from a higher-price firm without queuing. One may argue that customers with a higher willingness to pay can make credible threats to incur additional costs up to the difference to the next highest price and thus deter customers with a willingness to pay in that price range, which leads to some ordering of customers according to their willingness to pay, albeit in most cases no perfect efficient rationing.

Edgeworth Markets with Increasing Marginal Costs and Voluntary Trading: Experimental Evidence". Inquiries into the Bertrand-Edgeworth model with increasing marginal costs by Allen and Hellwig (1986) and Vives (1986) point to the sensitivity of its outcomes to the employed rationing scheme. Requate (1994) considered the infinitely repeated supergame version of the model and found that simple pricing patterns where all firms set a uniform price in all periods as long as no one deviates can be sustained as subgame perfect Nash equilibria. For the one-shot version of the model, however, a characterization of Nash equilibrium is unavailable. Hence, we solve numerically for the Nash equilibria in a discretized version of the parameterized model with the aid of the Gambit game theory software tool (McKelvey et al., 2014). To the best of our knowledge, the only experiment on price competition with increasing marginal costs and voluntary trading conducted so far is an explorative study by Kruse (1993).

Our experimental design contains three treatment variables which can each take two different states so that there are eight treatments altogether. The treatment variables are the number of firms in a market (duopoly or triopoly), the demand rationing scheme (proportional or efficient), and the subject matching rule (fixed or random). We find that two of our treatment variables entail large and significant effects. Prices and profits are substantially higher in duopoly than in triopoly and with proportional compared to efficient rationing. By contrast, the effect of the subject matching rule is small. The two large treatment effects we find are qualitatively in accordance with the theory predictions. However, our data do not reflect the quantitative Nash predictions of mean and median prices and profits, much less the predicted distribution of prices. In general, prices and profits are lower than predicted, i.e. conduct is more competitive than in the Nash equilibrium. The distance to equilibrium is larger in treatments with proportional rationing. We find no incidence of pricing patterns that accord with the simple subgame perfect Nash equilibria described by Requate (1994). Moreover, whereas Nash equilibria in pure strategies exist only in the infinitely repeated game, in the fixed matching condition which resembles the infinitely repeated game conforming stable pricing patterns are significantly less frequent. Since the prices that can be observed when stable pricing patterns occur are rather low, we conjecture that the intention behind stable pricing is to play safe rather than to try to set up collusion. Consequently, stable pricing is observed more often in the more uncertain environment with random subject matching. As alternatives to the Nash predictions, the off-equilibrium theories of myopic best response pricing (giving rise to Edgeworth cycles) and imitation of competitors' past prices are investigated. At first sight, our data suggest that only imitation has predictive power when

the two alternative theories are considered jointly. Yet, a closer scrutiny reveals that the ability of the two theories to explain our data is interacted with the treatment conditions. While in the duopoly myopic best response pricing accounts for the main effect, the main explaining factor in triopoly is imitation of the lower of the two competitors' prices from the last round. It appears reasonable that firms use the rather sophisticated pattern of myopic best response pricing as a form of imperfect collusion in duopoly, but resort to imitation in triopoly where finding the best response price is more intricate.

Chapter III contains the study "Number of Firms, Rationing, Matching, and Knowledge: A Comprehensive Study of Variations in Experimental Kreps-Scheinkman Markets". The theoretical basis for this study is the model by Kreps and Scheinkman (1983) and the sensitivity of its outcome to demand rationing detected by Davidson and Deneckere (1986). A supergame version of the model was considered by Benoît and Krishna (1987). While the experimental evidence concerning Kreps-Scheinkman markets is somewhat richer than in the case of price competition with increasing marginal costs,⁴ also this evidence is still scattered.

The study presented in chapter III aims at closing some gaps in the experimental literature and at drawing a comprehensive picture of the effects of variations within the Kreps-Scheinkman setting. For this purpose, we conduct seven different treatments of Kreps-Scheinkman markets and investigate the effects of four treatment variables: number of firms in a market (duopoly or triopoly), demand rationing (proportional or efficient), subject matching (fixed or random), and subjects' understanding of the Kreps-Scheinkman setting, briefly referred to as "knowledge" (low or high). We find that conduct is more competitive than the Cournot outcome in all treatments. As to decisions at the first stage, market capacities are persistently higher in triopoly than in duopoly. Subjects with high knowledge about the market setting choose lower capacities in the initial periods, but the difference to subjects with low knowledge vanishes when the latter commence to reduce their capacities in the course of the experiment. Neither demand rationing nor subject matching affects capacity choices. At the second stage, also demand rationing entails a significant effect. Prices are higher with proportional than with efficient rationing. Moreover, higher prices are chosen in duopoly than in triopoly. Knowledge again has an effect only in the initial rounds where subjects with high knowledge choose higher prices. Subject matching does not entail a significant effect on price choices. All described effects at the second stage are retained if capacity choices from the first stage are controlled for. In most treatments, the sensitivity of

⁴ Former experiments which investigate the effect of variations within Kreps-Scheinkman markets comprise Brandts and Guillen (2007), Le Coq and Sturluson (2012), and Lepore and Shafran (2013).

pricing to installed capacity is low at first, but increases with the number of periods played. An exception is the treatment with high knowledge where the sensitivity is large from the beginning. Apparently, many subjects in the low-knowledge treatments who initially have little understanding of how the market functions have to learn about the link between their decisions at the first and at the second stage by experience. The Cournot benchmark does poorly as a predictor of outcomes in absolute terms. Nevertheless, exact Cournot capacity choices occur significantly more often under efficient rationing, where the Cournot outcome is the equilibrium, than under proportional rationing. An analogous result is obtained for price choices when only markets with sufficiently low capacities such that market clearing is the pure-strategy Nash equilibrium are considered. Exact market-clearing prices are chosen more frequently under efficient than under proportional rationing. Overproduction at the first stage and subsequent fierce price competition at the second yield profits substantially falling short of the equilibrium predictions. Overproduction persists even after many rounds when subjects have had ample opportunities to learn from their experience.

Chapter IV returns to Bertrand-Edgeworth markets and considers fixed exogenous capacity constraints. The study in chapter IV is entitled "Demand Rationing in Bertrand-Edgeworth Markets with Fixed Capacities: An Experiment". The choice situation is the same as at the second stage in the Kreps-Scheinkman setting, but, contrary to the Kreps-Scheinkman game, the firms have had no opportunity before to decide on their capacities. Our study is inspired by the seminal experiment by Kruse et al. (1994). New insights are obtained as we also investigate the effect of demand rationing and consider imitation as an alternative explaining factor of individual pricing patterns. For the case of duopoly with symmetric exogenous capacities with which we are concerned in that study, we can mostly revert to closed-form solutions for the mixed-strategy Nash equilibria having been obtained in the theoretical literature (Beckmann, 1967; Levitan and Shubik, 1972; Allen and Hellwig, 1993).

Our experiment consists of four treatments; we vary the amount of capacity (high or low) and the demand rationing scheme (proportional or efficient). Both treatment variables entail significant effects. Prices are higher with low than with high capacities and higher with proportional than with efficient rationing. Those effects are qualitatively in accordance with the Nash equilibrium predictions. However, prices are significantly higher than the Nash prediction in all treatments, even if the Nash equilibrium is in pure strategies. The distance to equilibrium is larger when capacities are high. Profits are significantly higher than the Nash prediction only with high capacities; with low capacities profits approach equilibrium levels in the long run. We examine myopic best response pricing and imitation of the competitor's

price as possible explanations of dynamic pricing patterns. Considered separately, each approach can explain our data partially, and the myopic best response theory does somewhat better. A simultaneous analysis of both factors is inhibited by the tremendous collinearity between them.

3 Structure of the dissertation and co-authorship

As described above, the dissertation at hand contains three experimental studies. The content of chapters II, III, and IV each corresponds to one study. For each study, a modified working paper version is available. Two of the studies are joint work with one co-author. Table 1 provides an overview of the titles of the studies, the corresponding chapter in this dissertation, the working paper versions, and the authorships.

Chapter	Title	Working Paper Version	Authors
II	Bertrand-Edgeworth Markets with Increasing Marginal Costs and Voluntary Trading: Experimental Evidence	Economics Working Paper 2016-01, University of Kiel	Martin Jacobs, Till Requate
III	Number of Firms, Rationing, Matching, and Knowledge: A Comprehensive Study of Variations in Experimental Kreps-Scheinkman Markets	Economics Working Paper 2016-02, University of Kiel	Martin Jacobs
IV	Demand Rationing in Bertrand-Edgeworth Markets with Fixed Capacities: An Experiment	Economics Working Paper 2016-03, University of Kiel	Martin Jacobs, Till Requate

Table 1: Studies contained in the dissertation.

The studies presented in chapters II and IV are joint work with my supervisor Prof. Dr. Till Requate. Prof. Requate detected the gap in the experimental literature on Bertrand-Edgeworth oligopoly and had already calculated the theory benchmarks for the model with increasing marginal costs when I started to work on my PhD project. While the author of this dissertation is responsible for the main work regarding the design of the experiments, the writing of the instructions, the conduct of the experimental sessions, the statistical analysis, and the writing of the corresponding papers, all those aspects profited considerably from Prof. Requate's collaboration and helpful suggestions.

The author of this dissertation noticed the missing of a comprehensive study of treatment effects in experimental Kreps-Scheinkman markets and proposed to enlarge his PhD project to the two-stage Kreps-Scheinkman oligopoly setting. He is the sole author of the study contained in chapter III. Nevertheless, also the work presented in that chapter benefitted from Prof. Requate's assistance. Prof. Requate provided crucial remarks on how to improve details of the experimental design and the instructions in order to make the Kreps-Scheinkman market setting as comprehensible as possible to the subjects. Moreover, it was Prof. Requate who suggested varying subjects' understanding of the market setting and conducting the "high-knowledge" sessions with subjects recruited from his Bachelor class on competition policy and industrial organization. Not least, the experiment would not have been possible without the financial means from Prof. Requate's Chair of Innovation, Competition Policy and New Institutional Economics at Kiel University.

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II Bertrand-Edgeworth Markets with Increasing Marginal Costs and Voluntary Trading: Experimental Evidence^{*}

Abstract: Price competition with increasing marginal costs, though relevant for many markets, appears as an under-researched field in the experimental oligopoly literature. We provide results from an experiment that varies the number of firms as well as the demand rationing and matching schemes in Bertrand-Edgeworth markets with increasing marginal costs and voluntary trading. We find that prices and profits are substantially higher in duopoly than in triopoly and with proportional compared to efficient demand rationing. The matching rule has little effect on prices and profits. Nash equilibrium predictions do not capture observed behavior. Neither the mixed-strategy Nash equilibria of the underlying one-shot game nor, for the fixed matching condition, the symmetric stationary outcome pure-strategy Nash equilibria of the infinitely repeated game are supported by the data. In contrast to results from related experiments, behavior is largely more competitive than predicted by Nash equilibrium theory. Individual pricing decisions can predominantly be explained by either myopic best responses (Edgeworth cycles) or simple imitative behavior, where the complexity of the decision situation plays a crucial role in which behavioral pattern applies.

JEL classification: C72, C90, D43, L13.

Key words: Bertrand-Edgeworth, demand rationing, increasing marginal costs, Edgeworth cycles, oligopoly, laboratory experiment.

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1 Introduction

In his "Papers Relating to Political Economy", Edgeworth (1925) advanced oligopoly theory by his critique of the Bertrand model. He suggested abandoning Bertrand's assumption that firms can produce any quantity at constant marginal costs and assuming instead capacity restrictions or increasing marginal costs. The Bertrand (1883) model predicts that price competition leads to marginal cost pricing and zero profits whenever there is more than one firm in the market. Edgeworth's modifications abandon that prediction and thus make the model more realistic. In this study, we concentrate our attention on Edgeworth's second alternative supposition, that of increasing marginal costs. Note that strictly convex production costs, or, put differently, decreasing returns to scale, are also a key ingredient of the neoclassical model of perfect competition. However, the modification also complicates analysis tremendously. The Bertrand-Edgeworth model has no pure-strategy Nash equilibrium. Edgeworth himself suggested that firms will react myopically to their competitors' price choices, thus giving rise to the price cycles that were later named after him. Advances in game theory later allowed for a more extensive analysis. Existence of Nash equilibrium in mixed strategies has been proven. It has been shown that equilibrium predictions can be heavily influenced by seemingly minor changes in the model design, notably by the rule according to which demand is rationed.

A considerable part of experimental economics has been concerned with conduct in oligopolies. The experimental investigation of the Bertrand-Edgeworth model, however, stands out as an under-researched topic. Compared to the wealth of experiments that study behavior in Bertrand and Cournot markets with diverse variations, experimental studies in the more realistic Bertrand-Edgeworth setting are relatively scarce. Additionally, most of the few existing studies deal with the case of fixed capacities instead of increasing marginal costs. We are aware of only three studies that consider price competition with increasing marginal costs in an experimental setting. Two of them – Abbink and Brandts (2008) and Argenton and Müller (2012) – assume, contrary to Edgeworth, that firms are obliged to cover the entire market demand at the price they choose. That assumption, while considerably simplifying the analysis, does not reflect reality in most cases where suppliers are free to limit the quantity they wish to bring to the market. The assumption pertains only to a minority of heavily state-regulated markets, notably for utilities. The third study by Kruse (1993) is, to the best of our knowledge, the only former experiment of price competition with increasing marginal costs

and voluntary trading. In this study, however, only demand rationing is varied and behavior on the individual level is hardly analyzed.

Further experiments based on the Bertrand-Edgeworth model appear fruitful to us for at least two reasons. Firstly, since the model's assumptions are more realistic than those made by Cournot and Bertrand, experimental results may more easily be transferred to real oligopolies. Secondly, as game-theoretic analysis has revealed that the model's outcome is highly sensitive to seemingly minor modifications in design, the question arises whether subjects in experiments and actual firms will in fact react to such inconspicuous changes.

In this study, we present an experiment of price oligopoly with linear demand and a quadratic cost function. We investigate both duopoly and triopoly markets. Furthermore, we vary the demand rationing rule and the matching scheme. We find substantially higher prices and profits in duopoly compared to triopoly and under proportional compared to efficient demand rationing. In contrast, it has only a small positive effect on prices and profits when subjects repeatedly interact within fixed groups instead of being re-matched after every round of the game. Analysis of individual price choices does not support the static Nash equilibrium prediction. In contrast to results from related experiments, behavior is largely more competitive than predicted by the Nash equilibrium. We observe both myopic best response and imitation behavior. While myopic best responses are the main driving factor in duopoly, subjects seem to resort to the simpler strategy of imitation in the more complex triopoly. Furthermore, subjects are more prone to stable pricing, i.e. set the same price in successive periods, under random matching. That stands in contrast to theory since a Nash equilibrium supporting such pricing patterns exists only in the infinitely repeated game. Our finding can be explained by the observation that stable pricing is hardly an indication of collusion in our data; rather, most subjects exhibiting stable behavior constantly set low prices following a risk averse maximin strategy.

The remainder of this study is structured as follows. Section 2 discusses Edgeworth's model of price competition and later studies in game theory that ensued from it. Section 3 reviews the related experimental literature. Sections 4 and 5 introduce the setup of the model that underlies our experiment and the experimental procedures. Section 6 presents the rather complex Nash equilibrium predictions of the calibrated model. We present and discuss our experimental results in section 7. Section 8 concludes.

2 Theory

The model of simultaneous price oligopoly by Bertrand (1883) assumes that firms can produce any quantity of a homogenous product at constant marginal costs and therefore suggests perfect competition leading to marginal cost pricing and zero profits whenever there is more than one firm in the market. To overcome this unrealistic result, Edgeworth (1925) proposed to either introduce production capacity constraints for firms or substitute strictly convex for linear production costs. In the latter case, self-imposed capacity constraints emerge implicitly since each firm is willing to produce only as long as its marginal production costs are no higher than the sales price it has set. Trading is voluntary, so firms are free to turn customers away when further supply becomes unprofitable. As in the Bertrand model, productions are made to order after demands have been revealed. Production costs are therefore incurred only for units that can be sold in the market.

The thus modified model turned out to be very intricate. It has no Nash equilibrium in pure strategies. The existence of equilibrium in mixed strategies was proven only many decades after Edgeworth's proposal (Dixon, 1984; Dasgupta and Maskin, 1986; Maskin, 1986). While equilibrium strategies have been characterized for special cases of the model with fixed capacity constraints (Beckmann, 1967; Levitan and Shubik, 1972; Osborne and Pitchik, 1986; Vives, 1986; Allen and Hellwig, 1993; Hirata, 2009; De Francesco and Salvadori, 2010; 2013), an equilibrium characterization for the model with strictly convex costs still is unavailable. Instead, further model modifications have been suggested in order to restore existence of pure-strategy Nash equilibrium. Those suggestions comprise weakening the voluntary trading constraint (Dixon, 1990; 1992; Dastidar, 1995) and allowing only discrete pricing (Dixon, 1993; Roy Chowdhury, 2008).

The complexity of Bertrand-Edgeworth models is in part due to the fact that their outcomes depend on the assumption made about demand rationing. Customers first try to buy the good from the firm offering at the lowest price. Due to the capacity constraints, that firm may well be unable to satisfy the entire market demand. Some residual demand will then be left for the other firms offering at higher prices. To determine residual demands, an assumption about how demand is rationed is necessary. Two rationing schemes are prominent in the literature. The one, proportional rationing, assumes that customers are served in random order. According to the other scheme, efficient rationing, customers are served in order of their willingness to pay for the product. Those customers with the highest willingness to pay are served first, thus leaving the least residual demand among all possible rationing schemes. The

effect of different rationing schemes on residual demand is illustrated in Figure 1 for a triopoly with linear total demand $D(p) = 1000 - p$. In the figure, the bold line represents the remaining demand for firm 3 with given capacities ($q_1 = 300, q_2 = 200$) and prices ($p_1 = 200, p_2 = 400$) of firms 1 and 2. While under proportional rationing residual demand is obtained by rotating the demand curve around the reservation price, under efficient rationing the demand curve is shifted leftwards in parallel.

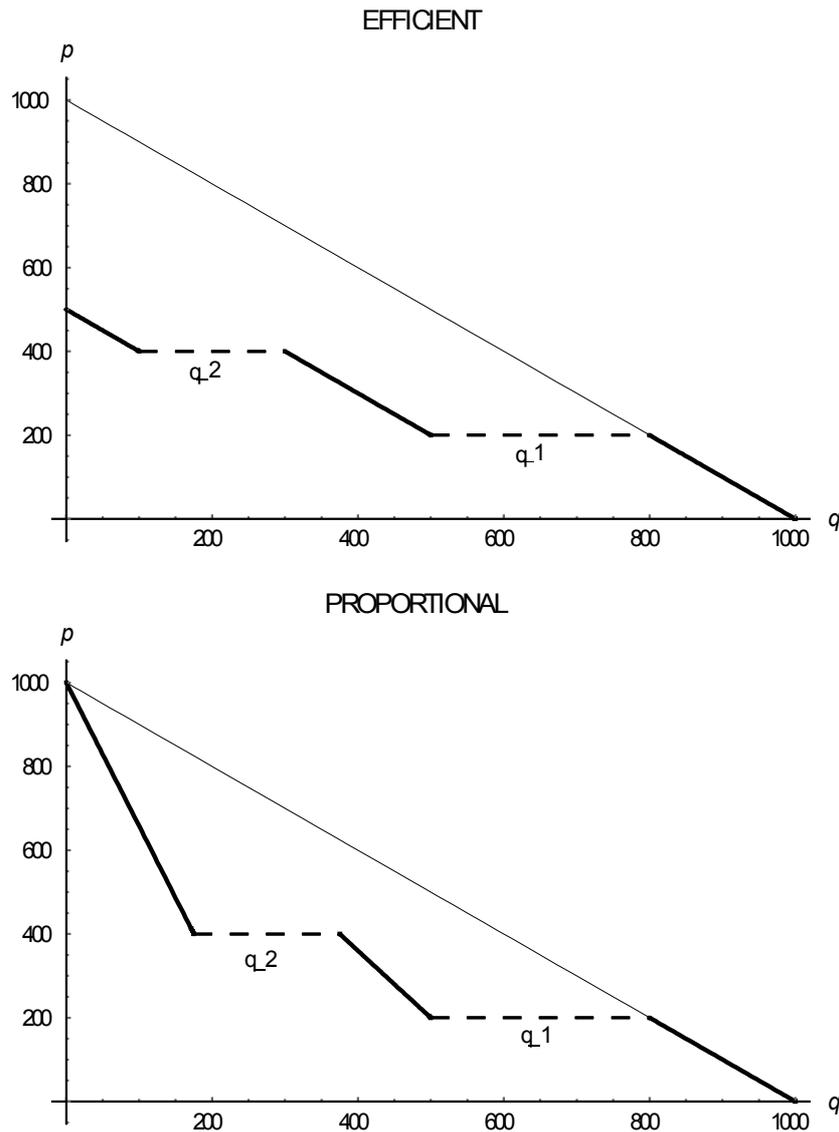


Figure 1: Residual demand under proportional and efficient rationing.

Whereas a full characterization of the equilibrium is unavailable, some results about its properties have been obtained. Allen and Hellwig (1986a; 1986b) show that, as the number of competitors increases, the probability mass of the equilibrium strategies moves towards the competitive price. However, under proportional rationing, there is no convergence in the

supports. A strictly positive, albeit declining, probability for a high price near the monopoly price always remains. An intuition for this result may be provided by the fact that if all firms but one set the competitive price, the last firm maximizes its profit by acting as a monopolist vis-à-vis its residual demand. Under proportional rationing, a random portion of customers is left for that firm and it pays off to set a high price. Under efficient rationing, in contrast, there is no point for the last firm in setting a high price since all customers with a high willingness to pay have already been served by others. In that case, also the supports of the equilibrium strategies converge (Vives, 1986).¹

Further studies consider repeated interaction of firms, thus allowing for supergame effects. If there is a sufficiently high probability for the firms to interact again, threats of future punishment can deter them from uncooperative conduct. In repeated price competition with exogenous capacities, Brock and Scheinkman (1985) find two opposing effects of an increasing number of firms in the market. On the one hand, as the number of firms grows, each firm in a cartel receives a declining share of the cartel profit, which makes defection more attractive. On the other hand, with more firms there is more total capacity in the market, which allows for a fiercer retaliation against a defector. As it turns out, the effect is non-monotonic; cartel power attains a maximum for some intermediate number of firms.

Moreover, as shown by Abreu (1988), infinitely repeated games possess subgame perfect Nash equilibria in pure strategies which follow a rather simple stick-and-carrot approach. Whenever a player deviates from collusion, a predefined phase of punishment is executed. In the Bertrand-Edgeworth framework, such equilibria are characterized by Lambson (1987) for the case with fixed capacities and by Requate (1994) for the case with increasing marginal costs.

3 Related experiments

Compared to the plenty of experiments investigating oligopolies in standard Cournot or Bertrand markets, the number of studies concerned with Bertrand-Edgeworth markets is relatively small. In a seminal study, Kruse et al. (1994) consider the effect of capacity and information conditions on pricing in a four-player Bertrand-Edgeworth game with exogenously given capacities and proportional demand rationing. They find that higher capacities go along with lower prices whereas the information condition entails no significant

¹ Börgers (1992) shows that this result can also be arrived at if one deploys iterated elimination of dominated strategies instead of the Nash equilibrium concept.

effect. Moreover, prices are higher than predicted by the static Nash equilibrium and there is evidence that subjects to some extent adjust their prices according to a myopic best response rationale as put forward by Edgeworth. A closely related experiment is conducted by Fonseca and Normann (2013) who vary capacities and the number of firms in the market.² They also observe falling prices with increasing capacities and dynamic pricing patterns which are more in accordance with Edgeworth cycles than with the static Nash equilibrium. The explanatory power of Edgeworth cycle theory decreases with increasing market capacity and is lower in triopoly than in duopoly. Fonseca and Normann (2008) investigate duopolies and triopolies with symmetric and asymmetric firms. They observe that symmetric firms and a market with fewer firms lead to higher prices. Heymann et al. (2014) find that their experimental data fit a simple heuristic model according to which a subject increments her price as long as her sales hit the capacity constraint and lowers her price by a certain amount if she is unable to sell up to her capacity.

All experiments mentioned in the above paragraph pertain to a setting with constant marginal costs up to a fixed exogenous cap. A Bertrand-Edgeworth setting with strictly convex costs is investigated by Abbink and Brandts (2008). However, they abandon the voluntary trading constraint. In their experiment, the firm offering at the lowest price is obliged to satisfy the entire demand at its price. Residual demand for the other firms thus equals zero. Varying the number of firms in the market between two, three and four, Abbink and Brandts observe that a market with more firms yields lower prices. Yet, prices remain substantially above the competitive level. Moreover, price choices concentrate on a focal price not predicted by any benchmark result, which the authors rationalize by employing an imitation model. In a similar framework, Argenton and Müller (2012) study cost asymmetry in duopolies and find that asymmetry can be conducive to collusion.

Few experiments have studied the effect of demand rationing. Kruse (1993) compares proportional and efficient rationing in posted-offer markets with two sellers and U-shaped average costs. She finds that prices are higher with proportional rationing. That effect as well as the observed price levels are in accordance with the Nash prediction for the underlying one-shot game, though in the experiment repeated play was executed. Jacobs and Requate (2016) obtain a similar effect of rationing in price competition with fixed exogenous capacities. In contrast to Kruse, they observe that prices largely are higher than predicted by the Nash equilibrium of the one-shot game. Lepore and Shafran (2013) and Jacobs (2016)

² Fonseca and Normann employ a model with box demand, so an assumption about demand rationing is superfluous.

vary demand rationing in a two-stage quantity setting and pricing experiment resembling the model by Kreps and Scheinkman (1983). Both studies find that while rationing has no significant effect on capacity choices at the first stage, prices at the second stage are higher with proportional rationing. Contrary to the aforementioned experiments, prices are considerably lower than the Nash prediction in that setting. Another study in which several different rationing schemes are employed is by Buchheit and Feltovich (2011). However, they aim at investigating a sunk cost effect and do not use demand rationing as a treatment variable; rather, in their experiment the effective rationing scheme is determined randomly.

4 The model

The underlying model of our experiment is a price-setting oligopoly market in which n identical firms offer a single homogenous good. Firms are confronted with a linear market demand function

$$D(p) = \frac{\alpha - p}{\beta}, \alpha, \beta > 0,$$

representing the total volume of the good that can be sold in the market at a certain price in each period. Each firm i has a quadratic cost function

$$C_i(q_i) = \frac{\gamma}{2} q_i^2, \gamma > 0, i = 1, \dots, n.$$

There are no fixed costs. As productions are made to order, q_i represents both the produced and the sold quantity of firm i . A firm will stop producing and selling when its marginal production cost reaches its marginal revenue which is equal to the price it has set. Each firm therefore has an implicit capacity constraint K_i depending on its individual price p_i :

$$K_i(p_i) = \frac{p_i}{\gamma}, i = 1, \dots, n.$$

If demand is not enough for all firms to exhaust their capacities, firms with lower prices will sell first and demand will be rationed according to the effective rationing scheme. We employ both proportional and efficient rationing. The quantity of firm i thus is the minimum of its implicit capacity and its residual demand $d_i(\mathbf{p})$ which depends on the price vector \mathbf{p} :

$$q_i(\mathbf{p}) = \min\{d_i(\mathbf{p}), K_i(p_i)\}, i = 1, \dots, n.$$

The residual demand of firm i varies with the rationing scheme and is given by

$$d_i(\mathbf{p}) = \begin{cases} \max \left\{ 0, \frac{D(p_i)}{m} \left(1 - \sum_{j|p_j < p_i} \frac{K_j(p_j)}{D(p_j)} \right) \right\} & \text{for proportional rationing} \\ \max \left\{ 0, \frac{1}{m} \left(D(p_i) - \sum_{j|p_j < p_i} K_j(p_j) \right) \right\} & \text{for efficient rationing} \end{cases},$$

$$i = 1, \dots, n,$$

where m is the number of firms in the market charging the same price as i (including i itself). We assume demand to be divided equally among firms in case of price ties.³ Lastly, profits are given by

$$\Pi_i(\mathbf{p}) = p_i q_i(\mathbf{p}) - C_i(q_i(\mathbf{p})), i = 1, \dots, n.$$

We calculate three benchmark outcomes. First consider the outcome in the competitive benchmark case which is characterized by marginal cost pricing. Since all firms have identical cost schedules, the marginal cost pricing rationale leads to identical price choices of all firms and to symmetric quantities. Quantities can therefore be expressed by $q_i(p) = D(p)/n$, $i = 1, \dots, n$. Equating price and marginal cost then yields firms' quantities $q_i^W = \alpha/(n\beta + \gamma)$, $i = 1, \dots, n$, and the competitive (Walrasian) benchmark price

$$p^W = \frac{\alpha\gamma}{n\beta + \gamma}.$$

The corresponding profit for each firm is

$$\Pi_i^W = \frac{\gamma}{2} \left(\frac{\alpha}{n\beta + \gamma} \right)^2, i = 1, \dots, n.$$

The second benchmark is the Cournot outcome which obtains when firms choose quantities rather than prices. Though there is no rationale for the Cournot outcome in our specific setting, it is the equilibrium prediction in the related Kreps-Scheinkman model of two-stage capacity setting and price competition under efficient demand rationing. Choosing simultaneously their quantities q_i , firms maximize $\Pi_i = q_i \left(\alpha - \beta(q_i + \sum_{j \neq i} q_j) \right) - \gamma q_i^2/2$, which yields $q_i^Q = \alpha/((n+1)\beta + \gamma)$, $i = 1, \dots, n$. The corresponding endogenous Cournot price is

$$p^Q = \frac{\alpha(\beta + \gamma)}{(n+1)\beta + \gamma}.$$

Profits are

³ As it is common in the literature on demand rationing, residual demand under proportional rationing is defined by the expectation of all possible customer reservation price orderings, and we do not actually model the order of such prices as a random variable. Zouhar (2015) notes that this difference as well as the exact tie-breaking rule (when several firms set the same price) affects quantities and profits in some cases when firms are to choose both prices and quantities simultaneously. Yet, his arguments do not apply to our setting in which, by construction, the produced quantity of each firm cannot be larger than its residual demand.

$$\Pi_i^Q = \frac{\alpha^2(2\beta + \gamma)}{2((n + 1)\beta + \gamma)^2}, i = 1, \dots, n.$$

The third benchmark is full collusion maximizing industry profit which is denoted by Π without subscript. As costs are strictly convex, maximization of joint profit demands symmetric quantities and thus symmetric pricing. The collusive price then is the argument that maximizes $\Pi = \sum_{i=1}^n \Pi_i = pD(p) - nC(D(p)/n)$, which is

$$p^C = \frac{\alpha(n\beta + \gamma)}{2n\beta + \gamma}.$$

Firms' profits under full collusion are given by

$$\Pi_i^C = \frac{\alpha^2}{4n\beta + 2\gamma}, i = 1, \dots, n.$$

Due to symmetric pricing, all benchmark outcomes are independent of the effective rationing scheme.

5 Experimental setup

In our experiment, we investigate the model described in the last section for the duopoly and triopoly ($n = 2, 3$) with parameterization $\alpha = 1000$, $\beta = 1$, $\gamma = 1$. Subjects could choose integers from the interval $[0, 1000]$ as prices. Price is the only choice variable as quantities are determined endogenously. Table 1 shows the benchmark prices and profits for the one-shot version of the game with our parameterization.⁴

		$n = 2$	$n = 3$
full competition	p^W	333	250
	Π_i^W	55,444.5	31,250
	Π^W	110,889	93,750
Cournot	p^Q	500	400
	Π_i^Q	93,750	60,000
	Π^Q	187,500	180,000
full collusion	p^C	600	571
	Π_i^C	100,000	71,248.5
	Π^C	200,000	214,285.5

Variables p , Π_i , and Π denote market price, firm profit, and market profit, respectively. Superscripts W , Q , and C denote the fully competitive, Cournot, and fully collusive benchmarks.

Table 1: Benchmark outcomes.

⁴ Where applicable, prices were rounded to the nearest integer. Integer prices were used to calculate profits.

Subjects chose prices repeatedly over several periods. Each experiment lasted at least 20 periods. From the 20th period on, the probability for the experiment to continue for another period was $5/6$. In order to enhance the comparability of our data, the total number of periods was equalized for all sessions as it was determined in advance by one random draw; it turned out that 24 periods were to be played.⁵

We conducted treatments with both fixed matching, where the same subjects interacted in one market throughout all periods, and random matching, where subjects were randomly assigned to markets anew in each period. While the fixed matching condition is clearly more in accordance with real oligopolies, random matching allows for a strict test of the static Nash equilibrium predictions of the game. On the whole, our experiment consists of eight treatments in a $2 \times 2 \times 2$ -design with the number of firms in a market ($n = 2, 3$), the rationing scheme (proportional, efficient), and the matching scheme (fixed, random) as treatment variables.

The experiment was conducted in the economics experimental labs at the Universities of Kiel and Heidelberg in June/July and November/December 2013. We usually conducted two sessions per treatment, one at each lab, yielding data of 9 to 11 markets for each treatment.⁶ Subjects from all fields of study were recruited and each subject participated only once.⁷ The sessions proceeded as follows. Upon their arrival, the subjects were randomly seated at computer terminals in the lab. They could not infer with whom of the other subjects they would interact in the experiment. Printed instructions provided complete information about the setup of the experiment, including cost and demand,⁸ and about how their payoff would be determined. The instructions did not state any of the benchmark outcomes calculated in the last section. When reading was finished, the experimenter gave a short presentation highlighting the experiment's main features. Then, the computerized experiment – using Fischbacher's (2007) z-tree software – started. First, subjects had to correctly answer a set of yes-no questions checking the understanding of the experimental procedures and setup before they could proceed. Three unpaid trial periods were played before the 24 paid periods started.⁹ Throughout all periods, subjects could use a profit calculator implemented on their screens. When a subject entered a vector of hypothetical prices – one for each firm –, the profit

⁵ A similar procedure is applied by Fonseca and Normann (2013).

⁶ In the duopoly treatment with efficient rationing and fixed matching, no-shows necessitated a third session which was conducted at Kiel University.

⁷ Subjects were recruited in lectures in Kiel. In Heidelberg the ORSEE recruitment software (Greiner, 2015) was used.

⁸ As students from all fields participated, cost and demand were described verbally rather than algebraically.

⁹ In the fixed matching treatments, subjects were re-matched after the trial periods. The new matching was then effective in all paid periods.

calculator returned the subject's sales quantity and profit resulting from the chosen price combination.¹⁰ After each period, subjects were informed about the price choices in their market and about their individual sales quantity and profit. While information on the quantities and profits of other players was not stated explicitly, due to the symmetry of players, subjects could easily calculate those numbers using the profit calculator. In the end, subjects were paid the sum of their earnings from all paid periods at a predefined exchange rate in private.¹¹ An average session lasted about 105 minutes; average earnings per subject were 15.0 Euro.

6 Equilibrium predictions

Figures 2 and 3 show the best response correspondences with our parameterization in the oligopoly with two and three firms, respectively, each for proportional and efficient rationing. The best response functions for the duopoly in Figure 2 consist of three sections. In the leftmost section, the other firm sets such a low price p that the best response $BR(p)$ is to set a high price and thus act as a monopolist vis-à-vis residual demand. In the middle section, underbidding the other price by one unit is the best response. In the rightmost section, the best response price is the monopoly price, which happens whenever the other firm sets a price higher than the monopoly price. Comparing the best response functions for the two rationing schemes, it can be seen that proportional rationing supports higher best response prices in the leftmost section since it leaves more residual demand to the firm with the higher price. Moreover, under proportional rationing the discrete jump in the best response price between the leftmost and the middle section occurs at a somewhat higher price of the other firm (at $p = 391$ as compared to $p = 367$ under efficient rationing) and is considerably larger (the best response price jumps from 576 to 390 under proportional and from 423 to 366 under efficient rationing).

Figure 3 illustrates the best response correspondences for the triopoly in the form of three-dimensional plots. The two horizontal axes correspond to the two prices p_1 and p_2 set by the two other firms. The vertical axis displays the best response price $BR(p_1, p_2)$. Regarding all possible combinations of p_1 and p_2 , best response prices range from 290 to 667 under proportional and from 268 to 667 under efficient rationing. A better understanding of what is

¹⁰ Instructions for an exemplary treatment, including a screenshot of the profit calculator, can be found in Appendix A.

¹¹ Throughout the experiment, money was accounted in experimental currency units (ECU). The exchange rates were 120,000 ECU per Euro in the duopoly treatments and 60,000 ECU per Euro in the triopoly treatments.

going on may be obtained if one looks at the density plots in Figure 4. This figure depicts best response *strategies*. The five regions of different brightness in the figure show the five strategies which can be distinguished, each of which takes the form of either underbidding or monopolizing some residual demand. First consider region (iii) in the lower left corner. In that region, the best response is characterized by setting the highest price and monopolizing residual demand since both other prices are low. When the prices of the other firms rise, underbidding becomes the best response at some point. Regions (i) and (ii) correspond to underbidding strategies. In region (i) the best response is to underbid the lower of the two other prices by one unit. If one of the other prices is sufficiently low, it can pay to only underbid the higher of the two other prices. This is the case in region (ii). The two strategies left again describe monopolistic behavior. In region (iv) sprawling from the upper left and lower right corners, one of the two other prices is so high and the other so low that the best response is to monopolize residual demand after the lowest-price firm has sold up to its implicit capacity, which leads to a best response price lower than the higher other price. The last strategy is monopolistic behavior vis-à-vis entire market demand; it obtains in region (v) when both other firms charge prices higher than the monopoly price.

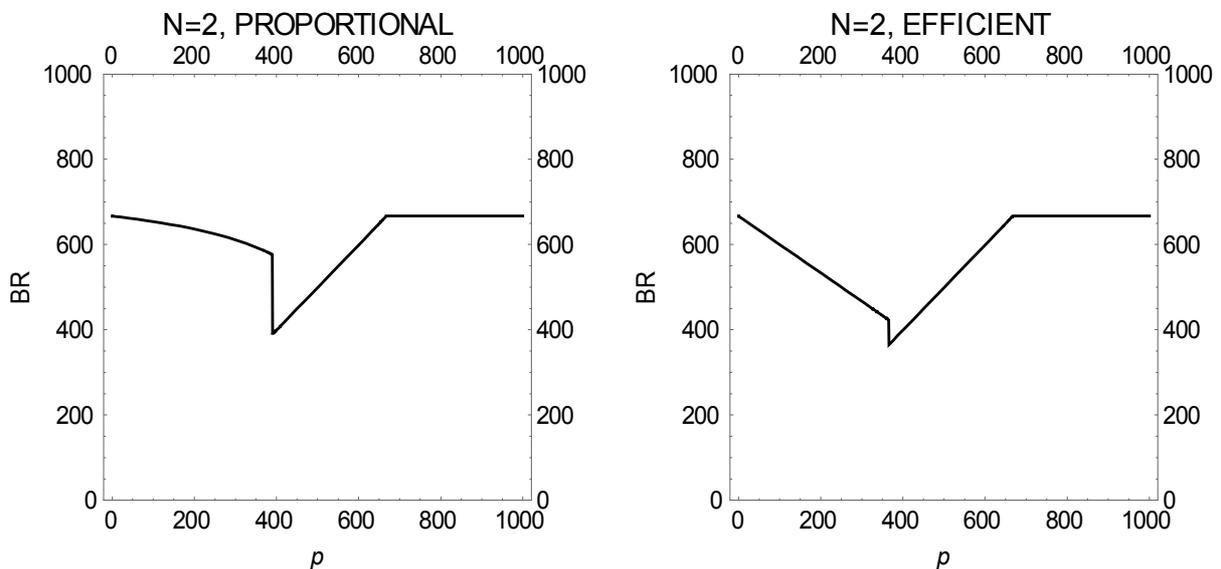


Figure 2: Best response functions in the two-player game.

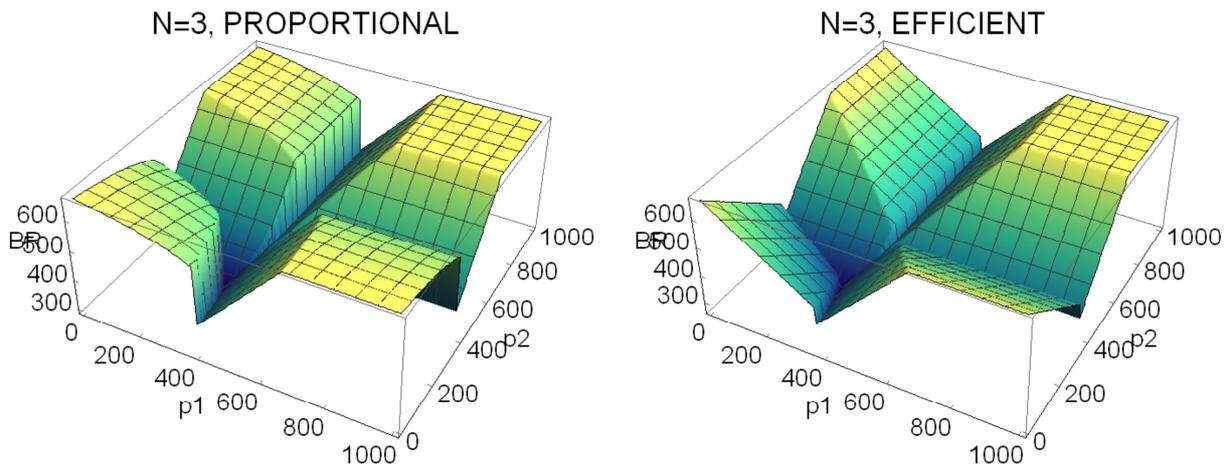


Figure 3: Best response prices in the three-player game.

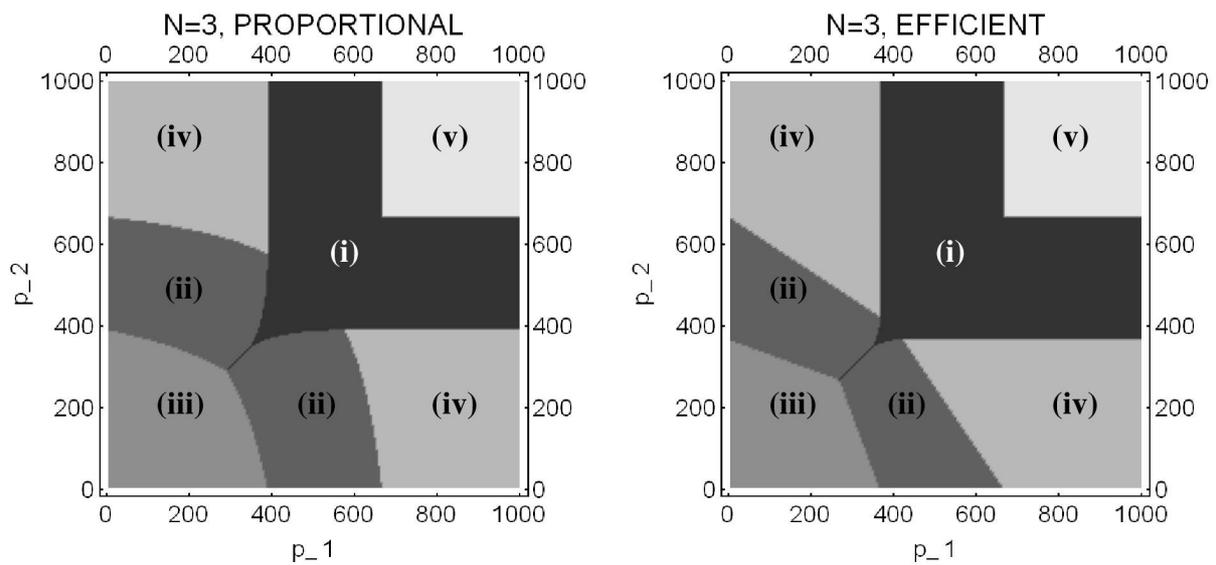


Figure 4: Best response strategies in the three-player game. The five regions, from darkest to brightest, indicate five different best response strategies: (i) underbid the lower other price, (ii) underbid the higher other price, (iii) set the highest price and monopolize residual demand, (iv) set the middle price and monopolize residual demand, and (v) set the lowest price and monopolize residual demand.

The discretization of the pricing model with strictly convex costs can in general give rise to Nash equilibria in pure strategies (Dixon, 1993). However, the price grid we employ is sufficiently dense that there is no such equilibrium. We calculated the mixed-strategy one-shot Nash equilibria under efficient and proportional rationing, each for duopoly and triopoly,

using the Gambit game theory software tool (McKelvey et al., 2014). Under proportional rationing, the set of rationalizable prices is very large, which makes the computation of the equilibria complex. Having been unable to have the equilibria calculated on a 2.1 GHz processor within ten days computation time, we slightly changed the input for the games with proportional rationing. Allowing only even integers as permissible prices proved successful. The densities of the calculated equilibria are displayed in Figure 5.¹² All equilibria have full support on the respective set of rationalizable prices. Under efficient rationing, those price intervals are relatively short: [366, 423] in duopoly and [268, 309] in triopoly. Under proportional rationing, the supports of the mixed-strategy equilibria are [390, 576] in duopoly and [290, 542] in triopoly. The equilibrium densities exhibit a U-shaped pattern which is much more pronounced under efficient rationing. Table 2 shows the expected mean individual prices and profits with their standard errors as well as the median prices and profits in Nash equilibrium. Note that uniqueness of the equilibria we calculated is not guaranteed.

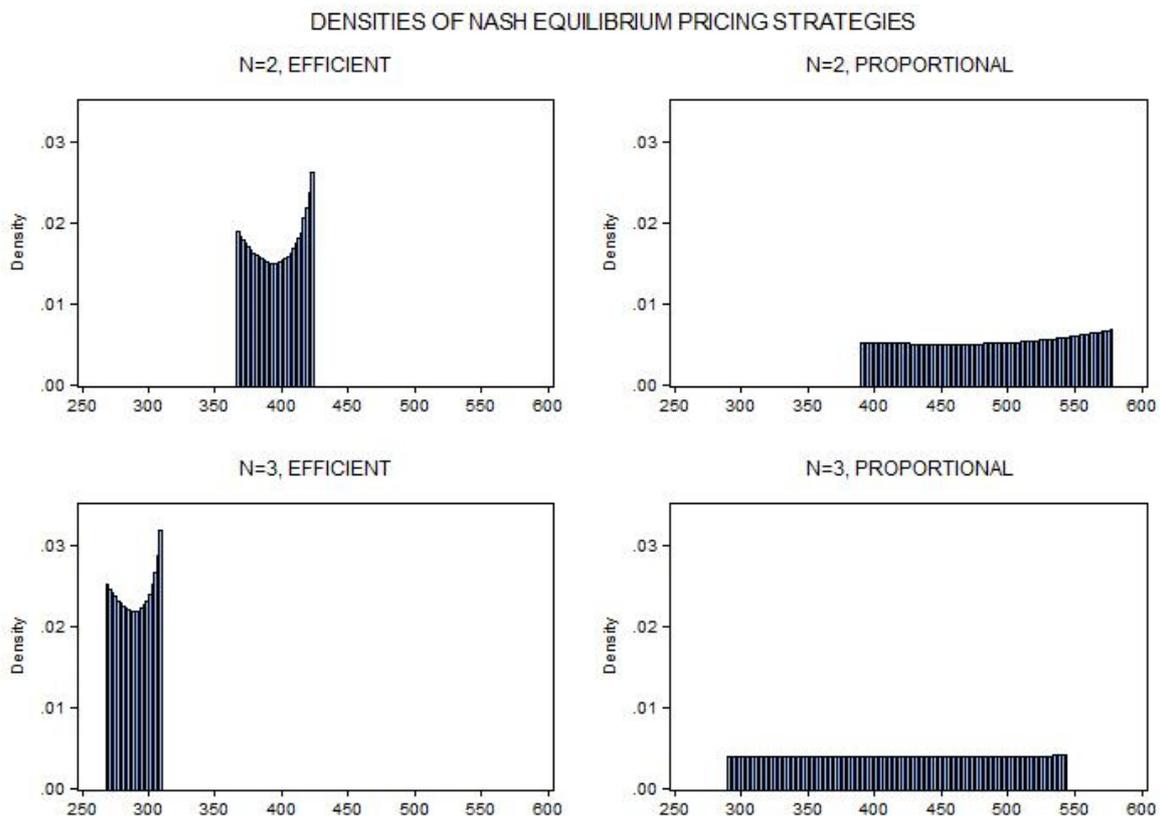


Figure 5: Densities of Nash equilibrium pricing strategies.

¹² The class width in all histograms in the figure is 2 ECU. So, for efficient rationing, the bars show averaged densities over two prices.

	Individual Price	Individual Profit
$n = 2$, efficient	395.745 (17.665) [396.000]	68,451.259 (7,527.133) [66,978.000]
$n = 2$, proportional	487.008 (55.471) [490.000]	71,010.396 (40,439.669) [76,050.000]
$n = 3$, efficient	289.007 (12.577) [289.000]	37,167.132 (5,322.551) [37,812.500]
$n = 3$, proportional	416.191 (73.463) [416.000]	44,498.162 (33,204.116) [52,448.000]

Standard errors in parentheses. Medians in square brackets.

Table 2: Nash equilibrium predictions of individual price and profit.

All analysis so far pertains to the one-shot game. Requate (1994) analyzes infinitely repeated price competition with increasing marginal costs. He finds that the repeated game possesses subgame perfect Nash equilibria of a simple form. In such a symmetric stationary outcome equilibrium, a unique price is set by all firms throughout the entire game as long as no one deviates from that path. In case of a deviation, the severest credible punishment is executed from the next period on forever. The set of prices which can be sustained in equilibrium depends on both the number of firms in the market and the discount factor. For our parameterization and for discount factors we consider reasonable, the sets of sustainable equilibrium prices are large,¹³ i.e. there is a large multitude of pure-strategy Nash equilibria. The issue then becomes coordination on one equilibrium. Since the collusive benchmark price that maximizes industry profit can be sustained, it appears as a prime candidate for coordination.¹⁴ For a fixed discount factor, interestingly, the set of sustainable prices in pure-strategy Nash equilibrium is smaller under proportional than under efficient rationing, which contrasts with the larger supports of the mixed-strategy equilibria under proportional rationing in the one-shot game.

¹³ Since subjects are paid off their earnings from all periods at one point in time at the end of the experiment, one may assume that earnings are not discounted at all. In that case, the intervals of sustainable equilibrium prices are [405, 795] in duopoly with proportional rationing, [370, 830] in duopoly with efficient rationing, [296, 846] in triopoly with proportional rationing, and [269, 873] in triopoly with efficient rationing. Taking instead 5/6, the continuation probability from the 20th period on, as discount factor, the respective intervals are [410, 718], [371, 762], [298, 720], and [269, 759].

¹⁴ The other way round, we can check how large the discount factors need to be in order to make the collusive benchmark outcome sustainable in equilibrium. Those critical discount factors are 0.716 in duopoly with proportional rationing, 0.645 in duopoly with efficient rationing, 0.735 in triopoly with proportional rationing, and 0.697 in triopoly with efficient rationing.

7 Results

We divide the presentation of our results into two parts. First, data on the market level are presented to compare the results to the benchmark outcomes and to elicit treatment effects. We then turn to the analysis of individual pricing decisions and inquire accordance of behavior with Nash predictions, its stability, and incidence of Edgeworth cycles or imitation. Within this section we also motivate and state our hypotheses and discuss our results.

7.1 Market performance

Summary statistics of the observed market data are provided in Table 3. Each treatment is denoted according to the scheme "number of firms per market, demand rationing scheme, matching rule"; for example, the duopoly treatment with efficient rationing and random matching is named " $n = 2$, efficient, random". The price measure we employ is the average price in a market, weighted by the quantities the firms were able to sell at their respective prices.¹⁵ Profit is measured as the sum of firms' profits in a market. We will refer to these measures as "price" and "profit" throughout this subsection. Table 3 for each treatment displays the average price and profit together with their standard deviations as well as the median price and profit for all (1-24) and for the last ten (15-24) periods.

The evolution of prices and profits over the periods of the experiment is depicted in Figures 6 and 7. The data points in the figures refer to averages over all markets in the respective period and treatment.¹⁶ In all treatments prices first decline and then mostly tend to stabilize in the later periods. The time trend is less clear for profits. Whereas profits rise and then tend to stabilize in the duopoly, there is no trend, or, if at all, a small decline, in the triopoly. Comparing the data to the benchmarks from Table 1, it can be seen that both prices and profits lie above the competitive and below the Cournot benchmark for all treatments and in all periods.

As to the effects of the three treatment variables, we set up the following hypotheses:

Hypothesis 1a: Prices and profits will be higher in duopoly than in triopoly.

Hypothesis 1b: Market conduct will be more collusive in duopoly than in triopoly.

We state Hypothesis 1 in two different versions since the benchmark prices and profits in Table 1 vary with the number of firms in the market. Therefore, one cannot simply infer from higher prices and profits in the duopoly that duopoly markets are more collusive. To

¹⁵ In this we follow Fonseca and Normann (2008; 2013).

¹⁶ Graphs for the evolution of prices in single markets with fixed matching can be found in Appendix B.

investigate collusiveness, we set up a measure that normalizes prices and profits using the benchmark outcomes. That measure is elaborated on at the presentation of the results below.

Hypothesis 2: Prices and profits will be higher with proportional than with efficient demand rationing.

Hypothesis 3: Prices and profits will be higher with fixed than with random subject matching.

For Hypotheses 2 and 3, a case discrimination is not needed as the benchmark prices and profits are not affected by the rationing or matching condition. Higher collusiveness thus is equivalent to higher prices and profits.

<i>periods</i>	Market Price		Market Profit	
	<i>1-24</i>	<i>15-24</i>	<i>1-24</i>	<i>15-24</i>
<i>n</i> = 2, efficient, random	392.697 (17.985) [389.148]	383.403 (9.379) [383.797]	133,917.068 (9,444.181) [135,713.250]	134,762.983 (2,721.021) [135,006.250]
<i>n</i> = 2, efficient, fixed	405.315 (30.514) [399.563]	393.777 (20.763) [390.331]	135,566.395 (10,044.922) [136,720.250]	137,297.132 (7,475.537) [136,723.250]
<i>n</i> = 2, proportional, random	432.096 (42.099) [424.051]	423.052 (35.679) [415.891]	142,633.735 (9,211.928) [143,414.098]	143,913.980 (7,961.372) [144,112.625]
<i>n</i> = 2, proportional, fixed	445.288 (33.845) [444.422]	437.411 (32.472) [434.477]	143,006.802 (9,984.003) [143,162.621]	144,168.121 (8,983.197) [143,998.267]
<i>n</i> = 3, efficient, random	289.756 (13.854) [287.577]	279.861 (4.902) [279.866]	110,913.016 (4,133.332) [111,193.250]	110,089.409 (3,237.614) [110,126.500]
<i>n</i> = 3, efficient, fixed	306.348 (39.612) [293.510]	294.207 (36.899) [283.134]	112,813.473 (8,786.764) [111,952.375]	111,194.639 (5,848.259) [110,979.250]
<i>n</i> = 3, proportional, random	317.398 (22.457) [313.313]	311.301 (22.585) [304.648]	118,115.571 (6,498.513) [116,081.110]	118,712.124 (6,175.035) [116,395.410]
<i>n</i> = 3, proportional, fixed	316.841 (26.550) [313.149]	314.193 (31.302) [305.775]	118,590.824 (7,392.573) [115,928.617]	118,267.086 (7,984.267) [115,570.282]

Standard errors in parentheses. Medians in square brackets.

Table 3: Average market price and profit.

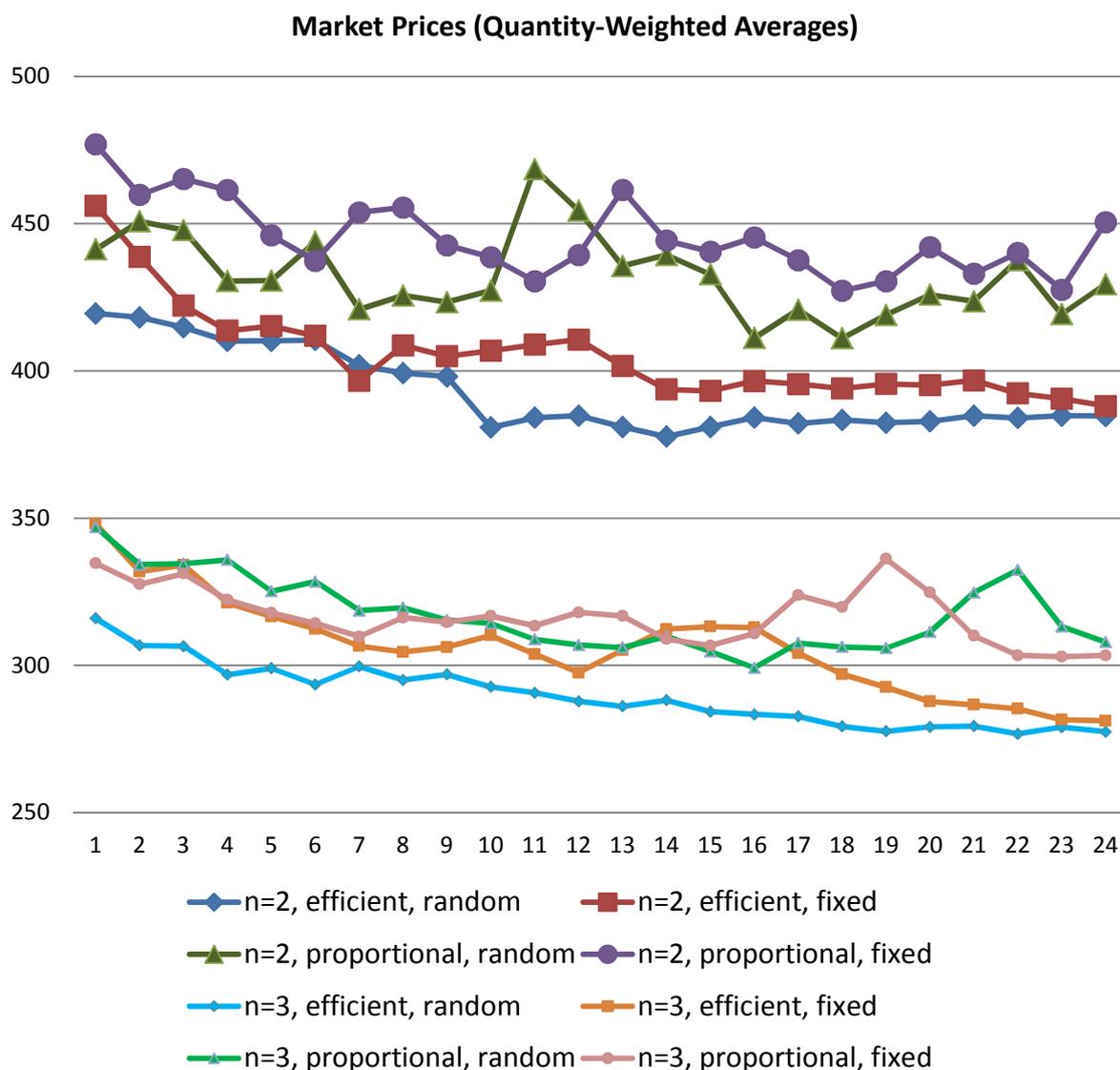


Figure 6: Series of market prices.

Hypotheses 1 and 2 accord with both theory predictions and evidence from earlier related experiments. As the analysis in section 6 has shown, the expected Nash equilibrium price is higher in duopoly than in triopoly and higher with proportional than with efficient rationing.¹⁷ The same relations hold true for the equilibrium predictions for profits. Furthermore, one can infer from the best response correspondences in section 6 that setting higher prices under proportional than under efficient rationing is in a sense dominant: For every given other price (in duopoly) or vector of other prices (in triopoly), the best response price under proportional rationing is at least as high as the best response price under efficient rationing. In the repeated

¹⁷ While the equilibrium price predictions presented in Table 2 pertain to individual and not market level prices, adding quantity weights leaves the qualitative relationships unaffected.

game, however, there is no unambiguous theory prediction for the rationing effect. One may argue that efficient rationing allows for punishing deviant behavior in an implicit cartel more effectively as less residual demand is left. Consequently, the threat of more severe punishment may sustain more collusion in the first place. Yet, for reasonable discount factors, full collusion can be sustained under either rationing rule. The experimental results by Kruse (1993) let us expect that proportional rationing goes along with higher prices also when fixed matching is employed. As to the procollusive effect of fewer firms, our hypothesis is in accordance with the results by Abbink and Brandts (2008) and by Fonseca and Normann (2008).

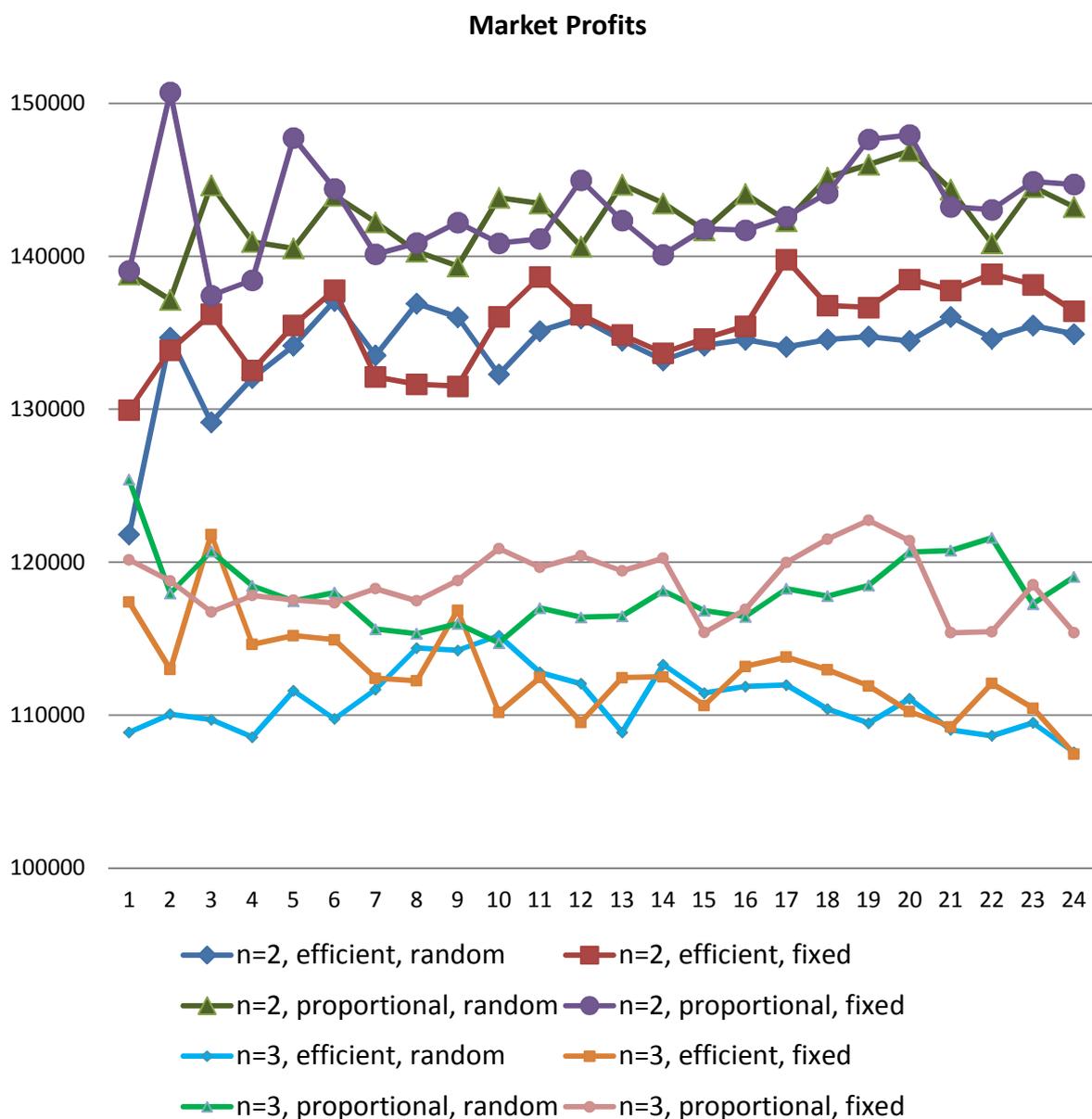


Figure 7: Series of market profits.

As regards Hypothesis 3, we cannot produce evidence from related experiments since those do not vary the matching scheme. From the theoretical viewpoint, however, we expect higher prices and profits with fixed matching from the fact that full collusion is supported by a Nash equilibrium only in the infinitely repeated game which the fixed matching condition resembles.

We start to investigate the effect of the treatment variables with a visual analysis of Figures 6 and 7. In every of the 24 periods we compare the averaged prices and profits pairwise between treatments. For such a pairwise comparison, we use two treatments which differ only with respect to the relevant treatment variable while the two other variables are held constant. Each of the two other variables can take two states. Therefore, $2 * 2 = 4$ pairwise comparisons apply for each treatment variable.¹⁸ Multiplying the number of pairwise comparisons with the number of periods yields $4 * 24 = 96$ instances of comparison. When we compare the duopoly data to those from the triopoly, we find that both prices and profits are higher in duopoly in every single of those 96 instances. An analogous comparison of treatments with different demand rationing schemes yields that prices (profits) are higher with proportional than with efficient rationing in 90 (94) of 96 instances. Fixed as compared to random matching also a predominantly positive effect on prices and profits, as well, although the picture is not as clear-cut as with the other two treatment variables. Prices (profits) are higher with fixed matching in 78 (66) of 96 cases.

We now turn to a formal analysis and perform nonparametric significance tests of subsamples within single periods. For the sake of conciseness, we restrict our attention to the last ten periods. Our results remain qualitatively unchanged if we consider all 24 periods. To formally test for treatment effects, we repeat the pairwise comparison of treatments, holding all but one variable constant, and perform Mann-Whitney tests. The significance test results are summarized in Table 4.¹⁹ The observed difference between duopoly and triopoly is highly significant for both prices and profits. In every single test, we find that prices and profits are significantly higher in the duopoly at the one percent level.²⁰ The results for the effect of demand rationing are somewhat less overwhelming, but still powerful. Both prices and profits are significantly higher under proportional rationing at least at the five percent level in 38 out

¹⁸ For example, when investigating the effect of the rationing scheme, the following four pairwise treatment comparisons apply: " $n = 2$, efficient, random" vs. " $n = 2$, proportional, random", " $n = 2$, efficient, fixed" vs. " $n = 2$, proportional, fixed", " $n = 3$, efficient, random" vs. " $n = 3$, proportional, random", and " $n = 3$, efficient, fixed" vs. " $n = 3$, proportional, fixed".

¹⁹ Since we consider only the last ten periods, now 40 instances of comparison (four pairwise treatment comparisons multiplied by ten periods) apply.

²⁰ All p -values refer to two-tailed tests.

Variable	significant			insignificant	significant with wrong sign
	$p < 0.01$	$0.01 \leq p < 0.05$	$0.05 \leq p < 0.1$	$p \geq 0.1$	$p < 0.1$
supported hypothesis					
Market Price					
$(n = 2) > (n = 3)$	40/40	0/40	0/40	0/40	0/40
proportional > efficient	28/40	10/40	0/40	2/40	0/40
fixed > random	0/40	1/40	5/40	33/40	1/40
Market Price: Collusiveness Index					
$(n = 2) > (n = 3)$	32/40	5/40	3/40	0/40	0/40
Market Profit					
$(n = 2) > (n = 3)$	40/40	0/40	0/40	0/40	0/40
proportional > efficient	30/40	8/40	1/40	1/40	0/40
fixed > random	0/40	0/40	2/40	36/40	2/40
Market Profit: Collusiveness Index					
$(n = 2) > (n = 3)$	39/40	0/40	1/40	0/40	0/40

Pairwise comparison of treatments in periods 15 to 24. Frequencies of results of two-tailed Mann-Whitney tests. Table entries in the first three columns indicate the number of cases in which the null hypothesis of equal medians is rejected at the displayed significance level in favor of the supported hypothesis. " $(n = 2) > (n = 3)$ " ("proportional > efficient", "fixed > random") means that the median of the variable is higher in the duopoly than in the triopoly (under proportional than under efficient rationing, under fixed than under random matching).

Table 4: Results of significance tests for treatment effects.

of 40 cases. By contrast, the visual impression of higher prices and profits under fixed than under random matching is not supported by significance test results. The difference between the two matching conditions is mostly insignificant. We find significance in the expected direction at the five percent level in only one case for prices and no such instances for profits – even less than what would be expected by coincidence, given that there is no difference between the two matching conditions.

In order to compare collusiveness between duopoly and triopoly, we define collusiveness indices that normalize the absolute data. The price collusiveness index is defined as

$$pcoll = \frac{p - p^W}{p^C - p^W},$$

where p is the observed price in a given market and period, and p^W and p^C are the benchmark prices as defined in section 4. A value of the collusiveness index of one (zero) thus corresponds to the fully collusive (competitive) outcome. An analogous index is defined for profits.²¹ Performing Mann-Whitney tests on the collusiveness indices, we find that pricing in the duopoly is significantly more collusive and that this conduct yields to profits significantly closer to the collusive benchmark. The collusiveness index is significantly higher in duopoly at least at the five percent level in 37 (39) of 40 cases for prices (profits).

Rather than to rely only on pairwise comparisons, we can make use of our full data set by multivariate regression analysis. Since there is heavy positive autocorrelation within markets in our data, we perform generalized least squares (GLS) regressions that correct the standard errors for effects of autocorrelation and heteroskedasticity on the market level. We investigate regressions with price as well as profit as dependent variable. Independent variables comprise three dummy variables, " $n = 3$ ", "proportional", and "fixed", related to the three dimensions of the treatment variable space. Each dummy equals one in those treatments for which the condition mentioned in the caption holds true and zero otherwise. Time trends are captured by the variable " $1/\text{period}$ ", the inverse of the period. Additional specifications furthermore include interaction terms of the treatment space dummies.

The regression results are presented in Table 5. All specifications indicate that the three treatment space dummy variables have the expected signs. The effects of the number of firms and of demand rationing are strongly significant.²² Contrary to the results of the pairwise significance tests, also the matching entails a statistically significant effect in three of the four

²¹ Engel (2007) uses a similar collusiveness measure in his meta-study of oligopoly experiments.

²² In order to be able to analyze Hypothesis 1b by means of multivariate regressions, we also ran regressions (1) to (4) with the above-defined price or profit collusiveness measures as dependent variable. The " $n = 3$ " dummy variable is negative and significant at the one percent level in all four regressions.

regressions. In the fourth, regression (2), the effect of matching approaches significance ($p = 0.1126$). Nevertheless, the economic significance of the matching effect remains doubtful. Consider for example regression (1). The estimated price increase with fixed matching by 7.4 ECU corresponds to an increase of no more than approximately two percentage points of the price collusiveness index. The effects of the other treatment variables are much more sizable: Prices are about 32 ECU higher with proportional than with efficient rationing and about 113 ECU higher in duopoly than in triopoly. The same pattern can be found in the other regressions.

Dependent Variable	Market Price		Market Profit	
	(1)	(2)	(3)	(4)
c	392.131*** (2.411)	389.716*** (3.364)	135,133.745*** (460.711)	134,276.370*** (616.179)
1/period	46.325*** (3.452)	46.325*** (3.452)	-1,381.658 (1,271.250)	-1,381.658 (1,212.250)
$n = 3$	-112.867*** (2.399)	-110.008*** (4.709)	-24,291.920*** (429.267)	-23,492.088*** (831.167)
proportional	31.710*** (2.399)	34.098*** (4.709)	7,291.657*** (429.267)	8,496.899*** (831.167)
fixed	7.432*** (2.399)	7.303 (4.601)	966.909** (429.267)	2,005.167** (812.057)
$(n = 3)$ *proportional		0.339 (6.751)		-468.684 (1,191.661)
$(n = 3)$ *fixed		3.736 (6.583)		-156.196 (1,162.013)
proportional*fixed		8.100 (6.583)		-1,245.670 (1,162.013)
$(n = 3)$ *proportional*fixed		-24.008** (9.430)		-1,555.620 (1,664.431)
Observations	1,920	1,920	1,920	1,920
Adjusted R ²	0.573	0.589	0.645	0.659

GLS regressions with clustered errors on the market level. Clustered standard errors in parentheses. ***, **, and * denote significance at the 1, 5, and 10 percent level. See accompanying text for variable definitions.

Table 5: Regression results for market level data.

The variable "1/period" is positive and significant in the price regressions whereas it is insignificant in the profit regressions, i.e. there is a significant downward time trend for prices, but no time trend for profits. The coefficient of the constant shows the level to which prices or, respectively, profits converge in the long run in treatment " $n = 2$, efficient, random" when "1/period" approaches and all dummy variables are zero. Adding the coefficients of the suitable treatment space dummies (and, in regressions (2) and (4), their

interactions) yields the convergence levels for the other treatments. The results confirm that both prices and profits converge to levels well above the fully competitive and well below the Cournot outcome.

Combining the evidence regarding the treatment effects, we conclude that Hypothesis 1 (in both its versions) and Hypothesis 2 are strongly supported. In contrast, we have found only limited support for Hypothesis 3, and the observed effect is rather small.

Result 1: Prices and profits are substantially and significantly higher in duopoly than in triopoly. Market conduct is significantly more collusive in duopoly than in triopoly.

Result 2: Prices and profits are substantially and significantly higher under proportional than under efficient demand rationing.

Result 3: Prices and profits are slightly higher under fixed than under random subject matching. The effect is mostly statistically significant in multivariate regressions, but not in pairwise treatment comparisons.

As has been mentioned above, most oligopoly experiments employ only fixed matching because that assumption is closer to real oligopoly markets. Having found that the matching condition does not entail a substantial effect on the collusiveness of our oligopoly markets, we can provide some justification for that approach and for testing also static predictions in a fixed matching setting with repeated interaction.

Aside from treatment effects, we are also interested in the general level of collusiveness in our experimental setting. We again employ the price collusiveness measure to quantify collusiveness. The data in Table 6 indicate that our markets are rather competitive. When zero (one) is defined as the fully competitive (collusive) price, the collusiveness of the mean price, averaged over all treatments, is 0.251. In 93.1 percent of all observations the collusiveness is smaller than 0.5, i.e. the price is closer to the fully competitive than to the fully collusive benchmark price. Only in 0.6 percent of the observations is a price collusiveness of at least 0.75 attained, and there is not a single instance in which the fully collusive benchmark is reached. Moreover, we test for every single treatment and each of the last ten periods whether the median price or profit equals any of the benchmarks in Table 1. We observe that in every of the 80 instances²³ both prices and profits are significantly higher than the fully competitive benchmark (all p 's < 0.01, Wilcoxon signed rank tests) and significantly lower than the Cournot (all p 's < 0.05 for prices, all p 's < 0.01 for profits) or the fully collusive (all p 's < 0.01) benchmark.

²³ The 80 instances of comparison to the benchmarks result from eight treatments multiplied by ten periods.

	$pcoll < 0$	$0 \leq pcoll < 0.25$	$0.25 \leq pcoll < 0.5$	$0.5 \leq pcoll < 0.75$	$0.75 \leq pcoll < 1$	mean $pcoll$
all treatments	0.002 (3/1920)	0.612 (1175/1920)	0.318 (610/1920)	0.063 (120/1920)	0.006 (12/1920)	0.251
$n = 2$, efficient, random	0.004 (1/240)	0.708 (170/240)	0.288 (69/240)	0.000 (0/240)	0.000 (0/240)	0.224
$n = 2$, efficient, fixed	0.008 (2/264)	0.492 (130/264)	0.439 (116/264)	0.061 (16/264)	0.000 (0/240)	0.271
$n = 2$, proportional, random	0.000 (0/240)	0.233 (56/240)	0.579 (139/240)	0.158 (38/240)	0.029 (7/240)	0.371
$n = 2$, proportional, fixed	0.000 (0/240)	0.079 (19/240)	0.675 (162/240)	0.238 (57/240)	0.008 (2/240)	0.421
$n = 3$, efficient, random	0.000 (0/240)	0.975 (234/240)	0.025 (6/240)	0.000 (0/240)	0.000 (0/240)	0.124
$n = 3$, efficient, fixed	0.000 (0/240)	0.829 (199/240)	0.133 (32/240)	0.025 (6/240)	0.013 (3/240)	0.176
$n = 3$, proportional, random	0.000 (0/216)	0.806 (174/216)	0.194 (42/216)	0.000 (0/216)	0.000 (0/216)	0.210
$n = 3$, proportional, fixed	0.000 (0/240)	0.804 (193/240)	0.183 (44/240)	0.013 (3/240)	0.000 (0/240)	0.208

Relative frequencies of market prices in the respective collusiveness ranges and mean collusiveness. $pcoll = 0$ ($pcoll = 1$) corresponds to the fully competitive (collusive) price.

Table 6: Collusiveness of market prices: Frequencies.

Comparing the competitiveness in our markets to that in related experiments, we find that behavior in our setting is more competitive than in the markets in Abbink and Brandts (2008) who observe pricing at or above the Cournot level when firms have to satisfy all demand at the price they choose. Pricing below the Cournot benchmark is in line with what has been found in experimental tests of the Kreps-Scheinkman model with inexperienced subjects (Davis, 1999; Muren, 2000; Goodwin and Mestelman, 2010; Jacobs, 2016). Furthermore, the overall competitiveness in our markets seems comparable to the results by Dufwenberg and Gneezy (2000) in an experiment of price competition with no costs.

The question of session-specific effects is salient in our procedures because we conducted sessions with participants from two different subject pools at different universities. Subject pool effects may thus have translated into session effects. To test whether subjects' price choices diverge in different sessions, we regress the price on a constant, the time trend variable, and a set of session dummies and then test for equality of those dummy coefficients that refer to the same treatment. We find no significant session effects (all p 's > 0.10 , F -tests).²⁴

7.2 Individual behavior

We now turn to the analysis of individual price choices. Figure 8 for each treatment displays the densities of observed individual prices.

7.2.1 Nash equilibrium predictions

We first compare the empirical individual price choices to the static Nash equilibrium predictions from section 6, testing our Hypothesis 4:

Hypothesis 4: The distribution of individual price choices in any treatment follows the probability distribution of prices in the respective static mixed-strategy Nash equilibrium.

There are four Nash equilibria to be compared to the data from eight different treatments. Each Nash equilibrium serves as a standard of comparison for two treatments with different matching conditions.

²⁴ However, in treatment " $n = 2$, proportional, fixed", the session effect approaches significance ($p = 0.1014$). The values of the coefficients indicate that prices are circa 11 ECU higher in the session conducted at Heidelberg University. All other session effects are smaller and clearly insignificant.

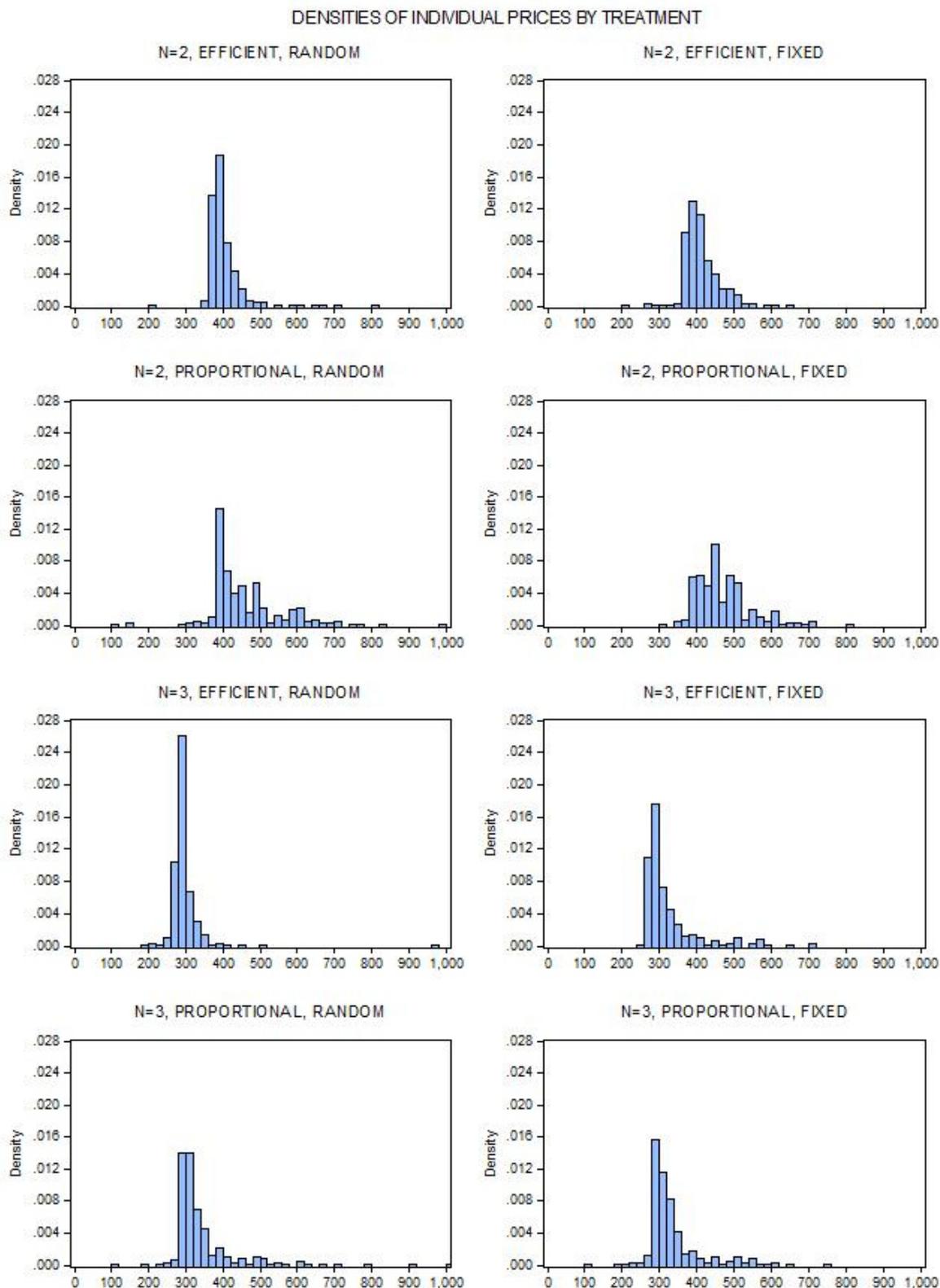


Figure 8: Densities of prices.

A visual comparison of Figures 5 and 8 already reveals that there is hardly any similarity between the predicted equilibrium densities and the according empirical data. Statistical analysis confirms the visual impression: In each case, a Kolmogorov-Smirnov test at the one percent significance level rejects the hypothesis that the equilibrium and the empirical price distribution are equal. This result remains unchanged if the early periods in which some subjects may still have to learn are disregarded and only the data from the last ten periods are used for the tests. These findings are consistent with those from experiments in the related setting of price competition with fixed exogenous capacities (Kruse et al., 1994; Fonseca and Normann, 2013; Jacobs and Requate, 2016).

Whereas the test results are powerful, it appears unsatisfactory that, due to the lacking uniqueness of equilibrium, there could in principle be other Nash equilibria which fit the empirical data. We tackle this problem with Bernheim's (1984) rationalizability concept. Iterative elimination of prices that are never a best response yields the sets of rationalizable prices. As noted in section 6, one then finds that those sets coincide with the supports of the equilibria we calculated. Since all prices chosen with a strictly positive probability in a mixed-strategy Nash equilibrium must be rationalizable, we know that the cumulative distribution function of any Nash equilibrium strategy must attain the value of zero for all prices lower than the lowest rationalizable price and the value of one for all prices equal to or higher than the highest rationalizable price. Using only this information, Kolmogorov-Smirnov tests reveal that the empirical price distributions (using data from all periods) are significantly different from any possible static Nash equilibrium distribution ($p < 0.01$ in seven out of eight treatments; $p < 0.05$ in the eighth treatment, " $n = 2$, proportional, fixed"). In fact, non-rationalizable price choices are quite common in the data. Pooling the data from all treatments, the share of non-rationalizable price choices is 21.8 percent. Among single treatments, the shares range from 12.9 to 35.4 percent, with no obvious correlation to any treatment variable. Moreover, if only price choices in the last ten periods are considered, the share of non-rationalizable prices is somewhat diminished to 16.9 percent, but is still sizable. Accordingly, we conclude:

Result 4: Empirical price choices do not follow any mixed-strategy static Nash equilibrium. Non-rationalizable prices are chosen frequently.

Although the Nash equilibrium price distributions do not provide a proper description of the empirical data, some properties of the equilibria we calculated might nevertheless be reflected in the data if we consider a higher aggregation level. As a weakened version of Hypothesis 4, we formulate Hypothesis 5:

Hypothesis 5: The average empirical price and profit levels in any treatment accord with the average price and profit levels predicted by the respective static mixed-strategy Nash equilibrium.

To inquire this weaker hypothesis, we compare the predicted Nash equilibrium means and medians for prices and profits from Table 2 with the empirical observations summarized in Table 7.²⁵ Figure 9 provides a graphical illustration of the comparison of mean prices and profits. Nash predictions are depicted as triangles, empirical data as circles. In order to avoid disturbances by outliers from early periods, only empirical data from the last ten periods are considered.

Panel A: Individual Price				
<i>periods</i>	<i>1-24</i>		<i>15-24</i>	
	<i>mean</i>	<i>comparison to Nash prediction</i>	<i>mean</i>	<i>comparison to Nash prediction</i>
<i>n</i> = 2, efficient, random	400.000 (42.032) [391.500]	=	385.800 (15.831) [380.000]	<***
<i>n</i> = 2, efficient, fixed	411.019 (41.956) [400.000]	>***	396.018 (25.546) [394.000]	=
<i>n</i> = 2, proportional, random	453.129 (89.636) [422.500]	<***	440.970 (74.413) [400.000]	<***
<i>n</i> = 2, proportional, fixed	463.646 (66.518) [450.000]	<***	452.405 (59.961) [440.000]	<***
<i>n</i> = 3, efficient, random	293.907 (35.222) [289.000]	=	281.190 (11.480) [280.000]	<***
<i>n</i> = 3, efficient, fixed	317.790 (66.201) [296.000]	>***	298.153 (46.154) [284.000]	<***
<i>n</i> = 3, proportional, random	338.704 (84.454) [312.000]	<***	332.611 (81.119) [299.000]	<***
<i>n</i> = 3, proportional, fixed	332.063 (67.082) [311.000]	<***	327.563 (67.829) [304.000]	<***

...

²⁵ The numbers in Table 7 differ from those in Table 3 because in Table 7 (i) the individual prices are not weighted by sales quantities and (ii) profits in the same market are not summed up.

Panel B: Individual Profit				
<i>periods</i>	<i>1-24</i>		<i>15-24</i>	
	<i>mean</i>	<i>comparison to Nash prediction</i>	<i>mean</i>	<i>comparison to Nash prediction</i>
<i>n</i> = 2, efficient, random	66,958.534 (12,317.095) [66,486.000]	=	67,381.471 (4,485.735) [66,486.000]	=
<i>n</i> = 2, efficient, fixed	67,783.197 (18,852.463) [67,147.500]	=	68,648.566 (10,214.421) [66,937.500]	=
<i>n</i> = 2, proportional, random	71,316.868 (26,563.938) [73,142.560]	<***	71,956.990 (19,304.860) [73,052.280]	<***
<i>n</i> = 2, proportional, fixed	71,503.401 (30,193.821) [75,136.000]	=	72,084.061 (25,072.270) [74,305.250]	=
<i>n</i> = 3, efficient, random	36,971.005 (8,974.914) [38,642.000]	>***	36,696.470 (3,339.204) [37,812.500]	=
<i>n</i> = 3, efficient, fixed	37,604.491 (16,556.877) [38,920.500]	>***	37,064.880 (13,144.703) [37,950.250]	=
<i>n</i> = 3, proportional, random	39,371.857 (17,207.199) [43,512.500]	<***	39,570.708 (14,126.615) [42,050.000]	<***
<i>n</i> = 3, proportional, fixed	39,530.275 (16,750.299) [42,340.500]	<***	39,422.362 (15,943.523) [42,050.000]	<***

Standard errors in parentheses. Medians in square brackets. The entries in the columns "comparison to Nash prediction" show the results of two-tailed Wilcoxon signed rank tests with the null hypothesis that the observed median is equal to the predicted Nash equilibrium median stated in Table 2. "<" (>") indicates that the observed median is significantly lower (higher) than the Nash prediction, where ***, **, and * denote the 1, 5, and 10 percent significance levels. "=" indicates that equality of the two medians cannot be rejected at the 10 percent significance level.

Table 7: Individual price (Panel A) and profit (Panel B) data with comparison to Nash prediction.

Inspection of the mean individual prices in Panel A of Table 7 and in the accompanying graphical illustrations in the upper two panels of Figure 9 shows that under proportional rationing the observed prices are considerably lower than their Nash predictions. Deviations from equilibrium predictions are larger in triopoly than in duopoly. In the treatments with efficient rationing, however, mean prices come rather close to the equilibrium levels. Considering only the last ten periods, the largest relative difference between the observed and the predicted mean price in an efficient rationing treatment corresponds to a 3.2 percent

deviation from the equilibrium price. That is, while the variation of the rationing scheme entails a sizable effect on prices, the predicted effect is even larger. In the upper two panels of Figure 9 this result is reflected by the larger distance between the orange and the blue triangles than between the orange and the blue circles.

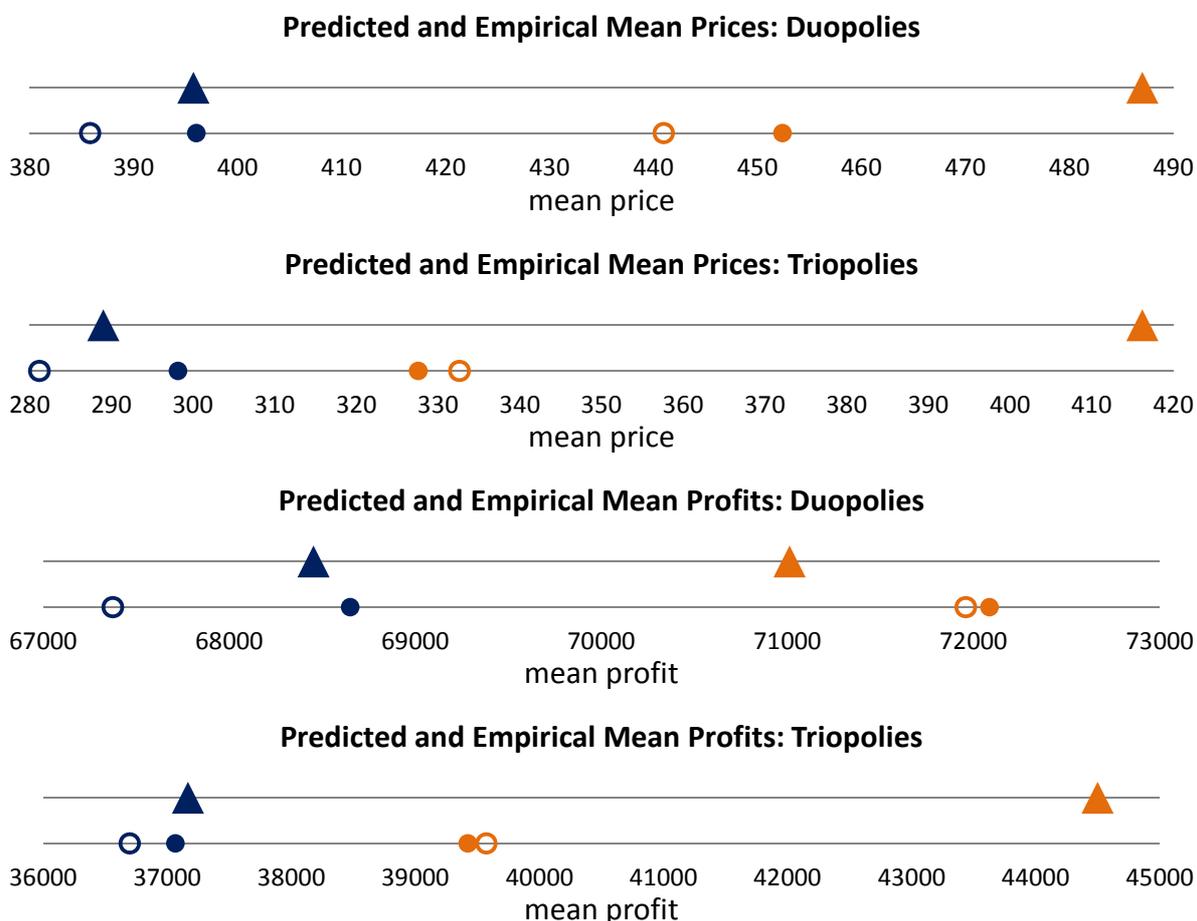


Figure 9: Comparison of Nash predictions (triangles) for mean individual prices and profits to empirical data (circles) from periods 15 to 24. Orange (blue) color corresponds to proportional (efficient) rationing. Filled (void) circles denote the empirical means in fixed (random) matching treatments.

A quite similar picture emerges when individual profits in Panel B of Table 7 and in the lower two panels of Figure 9 are considered. Mean profits under efficient rationing are close to equilibrium predictions, diverging from the mean equilibrium profit by no more than two percent in the last ten periods. However, this holds true also in the duopoly treatments with proportional rationing. Profits are clearly lower than predicted by the Nash equilibrium only in the triopoly with proportional rationing.

Since the Nash equilibrium prices and profits are not normally distributed, we resort to medians rather than means for significance tests and perform Wilcoxon signed rank tests.²⁶ The test results are indicated in the columns captioned "comparison to Nash prediction" in Table 7. Whereas the test results are somewhat ambiguous when data from all periods are pooled, confinement to the last ten periods brings about more clarity. In six (five) out of eight treatments, the median price (profit) is significantly lower than the Nash prediction.²⁷ In the remaining instances, equality to the Nash prediction cannot be rejected at the ten percent significance level. This is the case for both price and profit in the two treatments with efficient rationing and fixed matching. The median price or profit is never significantly higher than the Nash prediction. According to these findings, we summarize:

Result 5: The average empirical price and profit levels are mostly lower than the respective static Nash equilibrium predictions. The difference to Nash predictions is much more pronounced in treatments with proportional demand rationing. In the two treatments with efficient demand rationing and fixed subject matching, average prices and profits are close to their Nash predictions.

Consequently, behavior in our setting is quite competitive as measured not only by the average value of the collusiveness index on the market level. Individual pricing is also largely more competitive than predicted by the static mixed-strategy Nash equilibrium. The last result contrasts with evidence from related experiments. Kruse (1993) in the study most closely related to ours observes profits that are not significantly different from the Nash predictions. Kruse et al. (1994), Fonseca and Normann (2013), and Jacobs and Requate (2016) find that prices and profits are rather higher than the Nash prediction in price competition with fixed exogenous capacities.

²⁶ If we ignore the distributional assumption and perform Gauß's z-tests (using the standard errors predicted for the Nash equilibrium and data from the last ten periods), we receive the following results: Empirical mean prices are significantly lower than the Nash prediction at the one percent level in six treatments. In one treatment (" $n = 3$, efficient, fixed") the price is significantly higher at the one percent level, and in one treatment (" $n = 2$, efficient, fixed") there is no significant difference at the ten percent level. Mean profits are significantly lower than the Nash prediction at the one percent level in one treatment (" $n = 3$, proportional, fixed") and at the five percent level in two more (" $n = 2$, efficient, random" and " $n = 3$, proportional, random"). There is no significant difference at the ten percent level in the remaining five treatments.

²⁷ In treatment " $n = 3$, efficient, random" the Wilcoxon signed rank test for the last ten periods indicates a significant difference although the predicted and the empirical median profit are equal. That occurs since the empirical distribution is so asymmetric that a significant difference in the rank sums between the lower and the upper half of the observations ensues. The assumption of a symmetric distribution contained in the Wilcoxon signed rank test may be considered impermissible since the Nash equilibrium profit distributions are not symmetric. Acknowledging this problem, we nevertheless prefer the Wilcoxon signed rank test to the sign test because of the much greater statistical power of the former. Using instead the sign test leads to weaker, but qualitatively similar results: The empirical median profit (using data from the last ten periods) is significantly lower than the Nash prediction at the one percent level in three treatments (" $n = 2$, proportional, random", " $n = 3$, proportional, random", and " $n = 3$, proportional fixed"); in the remaining five treatments there is no significant difference at the ten percent level. For the price data the Wilcoxon signed rank test is unproblematic anyway since the predicted Nash equilibrium price distributions are close to perfect symmetry.

As to the treatments with fixed matching, another theory prediction is given by the subgame perfect stationary outcome pure-strategy Nash equilibria for the infinitely repeated game as described by Requate (1994). Those are the basis for our next hypothesis:

Hypothesis 6: Individual pricing patterns in the fixed matching treatments follow a stationary outcome pure-strategy Nash equilibrium.

In those equilibria, a uniform price is set by all firms in the market in all periods as long as no firm deviates. Also full collusion is sustainable at reasonable discount factors. However, as is already obvious from the empirical price distributions in Figure 8, subjects do, if at all, only seldom manage to cooperate at or near the fully collusive level. Nevertheless, since a large range of prices is sustainable in equilibrium, one may expect lasting cooperation also at lower price levels. Such cooperation, however, is, on the one hand, difficult to achieve because firms have to coordinate on a price without explicit communication. On the other hand, it is fragile since a single deviation triggers punishment forever after in equilibrium. Acknowledging these difficulties, we look in our data for uniform stationary pricing and count as successful equilibrium cooperation all cases in which all firms in a market choose a uniform price for two successive periods, the shortest time interval for which stationarity can be tested. We find that there is only one single instance of successful cooperation according to our definition. In a duopoly with proportional rationing, the two players manage to coordinate on the Cournot price of 500 in periods 19 and 20 after one player set this price, presumably as a signal, in periods 17 and 18. In period 21, one player deviates to a price of 490 and a phase of underbidding starts. If the assumption of a uniform price is relaxed and asymmetric, but individually stationary pricing is allowed for, there are all in all five instances of cooperation in the data, all in duopoly markets. The conclusion thus is clear-cut:

Result 6: There is no evidence of behavior in the fixed matching treatments that accords with the stationary outcome subgame perfect pure-strategy Nash equilibria of the infinitely repeated game.

7.2.2 *Stable pricing*

Despite the missing evidence for behavior according to the pure-strategy Nash equilibria, one may still presume to find evidence of the different Nash equilibrium predictions under random and fixed matching on the individual level. Whereas in the one-shot game there is only a mixed-strategy equilibrium in which firms have to randomize over prices, the infinitely repeated game also possesses Nash equilibria in pure strategies. Although we have found that players do not achieve to maintain the simple stationary pure-strategy equilibria, attempts to

reach them may be reflected by sequences of individual stable pricing as signals to collude. Under random matching, in contrast, it does not make much sense for players to signal to others with whom they will play again in the next period only with minor probability. We therefore hypothesize:

Hypothesis 7: Stable individual pricing is more frequent in fixed matching treatments.

	<i>n</i> = 2		<i>n</i> = 3		pooled
	efficient	proportional	efficient	proportional	
random	0.237 (109/460)	0.252 (116/460)	0.229 (158/690)	0.163 (101/621)	0.217 (484/2231)
fixed	0.069 (35/506)	0.133 (61/460)	0.159 (110/690)	0.091 (63/690)	0.115 (269/2346)

Relative frequencies of stable individual prices over two consecutive periods.

Table 8: Stable individual pricing: Frequencies.

Dependent Variable	stableprice
c	-0.639*** (0.083)
1/period	-0.676** (0.295)
<i>n</i> = 3	-0.037 (0.071)
proportional	-0.092 (0.070)
fixed	-0.418*** (0.068)
Observations	4577
McFadden R ²	0.025

Probit regression. Heteroskedasticity and autocorrelation consistent standard errors in parentheses. ***, **, and * denote significance at the 1, 5, and 10 percent level. "stableprice" equals 1 if a subject has not changed her price compared to the last period and 0 otherwise.

Table 9: Stable individual pricing: Regression results.

We start to investigate the hypothesis by counting the instances of stable individual pricing in each treatment. An instance of individual price stability occurs whenever a subject does not change her price compared to the last period.²⁸ Table 8 presents the relative frequencies of stable pricing. Contrary to our conjecture, the shares are lower in the fixed matching

²⁸ All results in this paragraph remain qualitatively unchanged if we demand price stability for three consecutive periods instead.

treatments. Overall, subjects choose the same price as in the last period in 21.7 percent of all cases under random matching, but only in 11.5 percent of all instances in the fixed matching condition. The hypothesis that the share of stable prices is independent of the matching condition is rejected for each pairwise treatment comparison and also for the pooled data (all p 's < 0.01 , chi-square tests). Additionally, we set up a probit regression with a stable pricing dummy as dependent variable. The regression results in Table 9 again show that stable pricing is significantly less probable under fixed matching. As indicated by the significant negative coefficient of "1/period", stable pricing is more probable in later periods.²⁹

We thus have a clear negative result:

Result 7: Stable individual pricing is *less* frequent in fixed matching treatments.

What causes this unexpected effect of matching on price stability? Whereas we cannot provide a definite answer, looking at the treatment-specific distributions of stable prices in Figure 10 leads us to a plausible presumption. We expected that instances of stable pricing would mostly arise as attempts to set up collusion. However, there are only few cases of stable pricing in the data that can be regarded as collusive. Instead, we observe clearly right-skewed distributions due to large frequencies of relatively low stable prices. This effect is absent only in the duopoly treatments with fixed matching; in triopoly, the effect is present in all four treatments, but stronger under random matching. Those patterns of constantly setting relatively low prices may be considered to follow the intention to play safe, i.e. to ensure oneself a certain minimum payoff, no matter what the other players will do. A similar phenomenon is observed by Abbink and Brandts (2008) where subjects often chose the lowest possible price assuring a non-negative payoff. An extreme case of such risk averse behavior would be to choose one's price according to the maximin criterion. In our setting, the maximin prices are 375 or 376 in duopoly and 267 in triopoly.³⁰ Those values match the modes of the distributions in Figure 10 fairly well. Hence, stable pricing does not signify an environment conducive to collusion where players try to cooperate. Contrariwise, it indicates a complex environment in which others' choices appear so unpredictable that many players resort to playing a low-risk near maximin strategy. Consequently, stable pricing occurs more often under random matching where players are more uncertain which prices they will be confronted with next period when they are matched with new competitors. Following this

²⁹ Interestingly, when we add demographic variables, we find further significant effects, according to which younger and male subjects are more prone to stable pricing.

³⁰ Due to the discretization of the price space, the maximin price can vary with the rationing scheme, although the effect of rationing is tiny. In our setting, 375 (376) is the maximin price in duopoly with efficient (proportional) rationing. In the triopoly, the maximin price is 267 under either rationing rule.

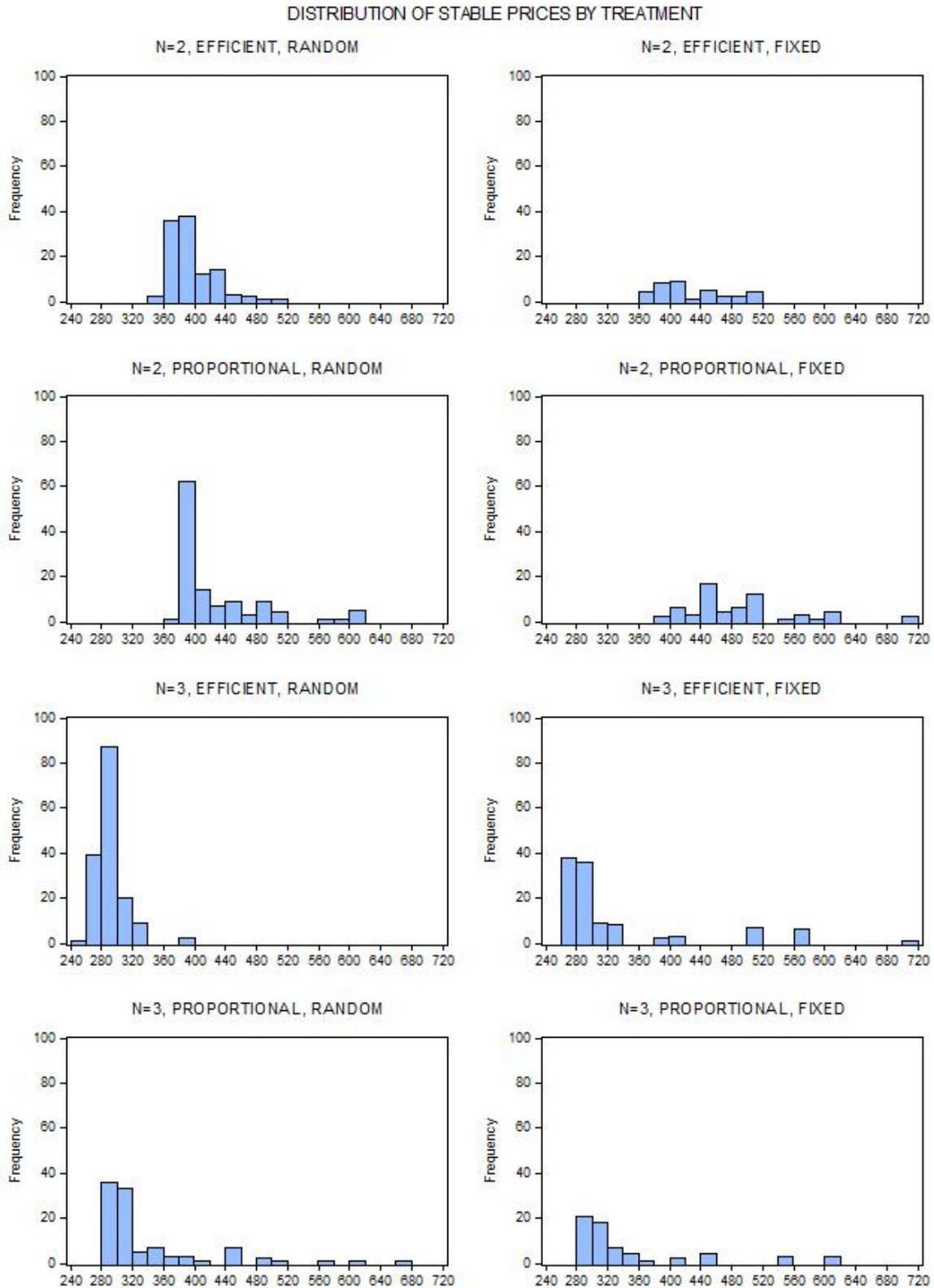


Figure 10: Distribution of stable prices.

interpretation, for some players the increase in the number of firms in the market from two to three complicates the pricing game that much and makes its outcome so unpredictable that they play a maximin strategy even under fixed matching.

7.2.3 *Myopic best response and imitation*

Since pricing does not accord with the Nash equilibrium predictions, we investigate two off-equilibrium behavioral strategies as alternative hypotheses.

Hypothesis 8a: Individual price choices are based on a myopic best response rationale as purported by Edgeworth's price cycling theory.

The figures in Appendix B showing price evolutions in single markets indicate price cycles in some markets. Furthermore, Kruse et al. (1994) and Fonseca and Normann (2013) have found supportive evidence for myopic best response behavior à la Edgeworth in related experiments.

The second alternative hypothesis is imitation:

Hypothesis 8b: Individual price choices are based on imitation of other firms' past prices.

The imitation hypothesis is supported by evidence from the experiment by Abbink and Brandts (2008) who observe price imitation as a driving force in price competition with increasing marginal costs and compulsory trading. Moreover, Heymann et al. (2014) find that pricing in a Bertrand-Edgeworth experiment with fixed exogenous capacities can be explained by simple heuristics. From that point of view, myopic best response behavior may already be too advanced a strategy.

We start with a replication of the regression equation in Kruse et al. (1994). The dependent variable is $p_{i,t} - p_{i,t-1}$, the change of subject i 's individual price choice in period t compared to the last period $t - 1$. $BR_{i,t-1}$ is i 's best response price to the price vector she was confronted with in the last period; it thus equals Edgeworth's theory prediction for i 's current price. Independent variables are the Edgeworth prediction for i 's current price minus i 's actual price in the last period, $BR_{i,t-1} - p_{i,t-1}$, and its lag, $BR_{i,t-2} - p_{i,t-2}$. The independent variables measure the shares of immediate and lagged adjustment to Edgeworth's myopic best response price. A strict interpretation of Edgeworth's theory, i.e. immediate and perfect adjustment to the best response price, translates into the prediction that the coefficient of $BR_{i,t-1} - p_{i,t-1}$ takes a value of one. However, what we expect from the results of related studies and what we indeed observe is only partial adjustment. The results of the regression, presented in column (1) of Table 10, are in line with those by Kruse et al. (1994) and Fonseca

and Normann (2013). We find a highly significant positive immediate adjustment effect; the coefficient indicates that an individual on average adjusts her price towards the Edgeworth price prediction by circa 22 percent. Running the regression for single treatments yields highly significant positive adjustment effects, as well (results not reported).

Dependent Variable	$p_{i,t} - p_{i,t-1}$				
	(1)	(2)	(3)	(4)	(5)
Sample Restriction	None	None	None	$n = 2$	$n = 3$
c	-4.728*** (1.003)	-1.932** (0.937)	-1.902** (0.952)	-0.817 (1.551)	3.272** (1.381)
$BR_{i,t-1} - p_{i,t-1}$	0.219*** (0.009)	0.001 (0.011)	0.697*** (0.167)	0.451*** (0.154)	0.046*** (0.012)
$BR_{i,t-2} - p_{i,t-2}$	-0.012 (0.009)	0.002 (0.011)	0.001 (0.011)	-0.112 (0.154)	0.013 (0.011)
$p_{-i,t-1} - p_{i,t-1}$		0.468*** (0.017)	-0.238 (0.162)	0.019 (0.148)	
$p_{-t,t-2} - p_{i,t-2}$		-0.037** (0.017)	-0.040** (0.017)	0.100 (0.149)	
$(BR_{i,t-1} - p_{i,t-1}) * (n = 3)$			-0.365** (0.146)		
$(BR_{i,t-1} - p_{i,t-1}) * \text{proportional}$			-0.354*** (0.095)		
$(BR_{i,t-1} - p_{i,t-1}) * \text{fixed}$			0.014 (0.025)		
$(p_{-i,t-1} - p_{i,t-1}) * (n = 3)$			0.452*** (0.143)		
$(p_{-i,t-1} - p_{i,t-1}) * \text{proportional}$			0.315*** (0.094)		
$(p_{-i,t-1} - p_{i,t-1}) * \text{fixed}$			0.012 (0.037)		
$p_{-i,t-1}^L - p_{i,t-1}$					0.530*** (0.024)
$p_{-i,t-2}^L - p_{i,t-2}$					-0.107*** (0.023)
$p_{-i,t-1}^H - p_{i,t-1}$					0.054*** (0.020)
$p_{-i,t-2}^H - p_{i,t-2}$					-0.011 (0.019)
Observations	4,378	4,378	4,378	1,804	2,574
Adjusted R ²	0.127	0.253	0.259	0.230	0.320

Ordinary least squares regressions. Standard errors in parentheses. ***, **, and * denote significance at the 1, 5, and 10 percent level. See accompanying text for variable definitions.

Table 10: Edgeworth price adjustment and imitation: Regression results.

However, the aim of regression (1) is just to check whether we can reproduce the results by Kruse et al. (1994) and by Fonseca and Normann (2013) in our setting. The regression is not suited to test Hypothesis 8a against Hypothesis 8b since imitation is not contained as an alternative explaining factor in the model. The Edgeworth adjustment effect we have found and that has been found in the earlier studies may obscure the simpler behavioral pattern of imitation. Since for a large set of price vectors the best response is to underbid the price of another firm by one unit, there is considerable collinearity between imitative and myopic best response behavior. What is essentially imitation may thus have strengthened the Edgeworth adjustment effect in regression (1).

In specification (2) we consider both myopic best response and imitation, adding to model (1) variables that measure the adjustment towards the other prices p_{-i} observed by i in the last period. The additional independent variables in regression (2) are $p_{-i,t-1} - p_{i,t-1}$ and its lag, $p_{-i,t-2} - p_{i,t-2}$. In duopoly, $p_{-i,t-1}$ is simply the price of the other firm i was matched with in period $t - 1$; in triopoly, we define $p_{-i,t-1}$ as the arithmetic mean of the two other prices. The results of regression (2) in Table 10 suggest that price changes are due to imitation rather than myopic best response behavior. Inclusion of the additional variables considerably increases the fit of the regression; the share of explained variance of the dependent variable roughly doubles. The coefficients measuring the immediate and lagged Edgeworth adjustment are clearly insignificant and virtually equal to zero. In contrast, the immediate imitation effect of circa 47 percent is highly significant. The significant negative coefficient of the lagged term indicates that the imitative adjustment towards other prices slows down if those other prices stay constant. At this stage we conclude:

Preliminary Result 8: Individual price choices are based on imitation of other firms' past prices and not on a myopic best response rationale.

Yet, things may be more complicated than that one of our two hypotheses is completely right and the other is completely wrong. One may suppose that the extent to which imitation or myopic best responses explain pricing varies across treatments. Specifically, we conjecture that imitation has more explaining power in triopoly where more sophisticated strategies such as finding the best response to past price vectors are more intricate. This conjecture is corroborated by our above finding that the simple pattern of stable maximin pricing occurs more frequently in triopoly and by the result by Fonseca and Normann (2013) that Edgeworth pricing cycles are more prominent in duopoly than in triopoly. Moreover, myopic best response behavior appears reasonable only within fixed groups of competitors. It does not make sense to adapt to the specific price choice of someone with whom the subject will not

interact again in the next period. In contrast, prices of other firms can provide subjects with an orientation of which price range is reasonable independent of the effective matching scheme. Consequently, we set up a third hypothesis:

Hypothesis 8c: The extent to which pricing is based on myopic best response or imitative behavior varies with the treatment conditions. Imitation has more explaining power in triopoly than in duopoly and with random than with fixed subject matching.

Estimations of specification (2) for single treatments are interfered, in one case even precluded, by collinearity. However, the (unreported) results broadly suggest that imitation is indeed more prominent in triopoly. In contrast, we cannot detect a clear effect of matching. As a more viable alternative, we augment specification (2) with interaction terms. We include interactions of both the immediate Edgeworth adjustment variable $BR_{i,t-1} - p_{i,t-1}$ and the immediate imitation variable $p_{-i,t-1} - p_{i,t-1}$ with the three treatment space dummies " $n = 3$ ", "proportional", and "fixed". With respect to the number of firms in the market, our conjecture is confirmed by the regression results presented in column (3) of Table 10. In the baseline treatment " $n = 2$, efficient, random" (where all treatment space dummies are zero), myopic best response behavior and not imitation explains pricing. While the results imply a highly significant immediate Edgeworth adjustment of circa 70 percent for this treatment, the immediate imitation effect is insignificant. In treatments with triopolies and/or proportional rationing, the immediate Edgeworth adjustment effect is significantly and substantially diminished whereas the immediate imitation effect gains importance. This is indicated by significant negative (positive) coefficients of the interaction terms with myopic best response pricing (imitation). Compared to the baseline, triopoly decreases the coefficient of the Edgeworth adjustment effect by 36 percentage points and increases the coefficient of the imitation effect by 45 percentage points. The effect of proportional rationing, while somewhat smaller, is also substantial: The Edgeworth adjustment coefficient is decreased by 35 percentage points and the imitation coefficient is increased by 32 percentage points. Though we acknowledge that proportional rationing complicates the underlying oligopoly model and thus the identification of the best response to a given price vector, we find the magnitude of the effect entailed by the rationing scheme quite surprising. The matching condition, on the contrary, surprisingly, does not entail a significant effect.³¹

³¹ Additionally, we run a regression of model (3) with further interaction terms pertaining to gender, age, and field of study. The only significant effect (at the five percent level) we find is that subjects who do not study economics or a related field adjust their price to the best response price by seven percentage points less. That appears plausible if one assumes that best response pricing is a rather advanced behavioral pattern that requires some grasp of economics.

Having found that the number of firms entails the largest effect on which behavioral pattern best explains pricing, we divide the sample according to that dimension and investigate the Edgeworth adjustment and imitation effects separately for duopoly and triopoly markets.³² Regression (4) in Table 10 is a replication of regression (2) on the restricted sample considering only duopoly treatments. The results show a highly significant Edgeworth adjustment effect and no significant imitation effect.³³ Regression (5) in Table 10 estimates the effects for the triopoly. Since only triopoly markets are included, we do not collect the two other prices an individual has observed in one term, but consider both of them with own variables in the regression. $p_{-i,t-1}^L$ denotes the lower of the two prices i has observed in period $t - 1$. Analogously, $p_{-i,t-1}^H$ denotes the higher of the two prices. Thus, $p_{-i,t-1}^L - p_{i,t-1}$ and $p_{-i,t-1}^H - p_{i,t-1}$ measure the adjustment towards the lower and, respectively, the higher other price observed in the last period. Again, we also include the first lags of these terms to allow for delayed adjustment. The regression results reveal that both an Edgeworth adjustment effect and imitation of the lower and higher other prices are present in the data. All three effects are highly statistically significant. Yet, imitation of the lower price entails by far the largest effect. The average individual in the triopoly adjusts her price by 53 percent to the lower of the two other prices observed in the last period; the higher price and the best response price, in contrast, each account only for an adjustment of about five percent.

Result 8: The extent to which pricing is based on myopic best response or imitative behavior varies with the number of firms in the market. Myopic best response (imitation) is the main explaining factor in duopolies (triopolies). The matching condition does not influence the rationale of individual price choices.

Thus, we have found partial support for both Edgeworth's hypothesis of myopic best response pricing and the hypothesis of simple imitation. In the duopoly individual pricing is well explained by Edgeworth's theory. In the triopoly, in contrast, imitation of past other

³² To recheck our hypothesis that pricing patterns are also influenced by the matching condition, we additionally run regression (2) separately for markets with fixed and random matching. The results of the two regressions on a partitioned sample are largely similar to those of the regression on the full sample. Under fixed as well as under random matching, the immediate imitation effect is highly significant and of magnitude 0.45 to 0.5 while the immediate Edgeworth adjustment effect is insignificant and close to zero. Consequently, we again find no evidence that the matching condition has a substantial influence on the rationale of individual price choices.

³³ While we are still able to receive clear results in regressions (3) and (4), the tremendous increase in the standard errors of the coefficients compared to regression (2) points to the issue of collinearity mentioned above. The variance inflation factors of the coefficients attain values of up to 72 in regression (4) and even up to 444 in regression (3). As collinearity can cause overfitting and make coefficient estimates highly dependent on particular samples, we test the validity of our results by a jackknife resampling approach. We run regressions (3) and (4) with varying subsamples of our data, leaving out observations from one particular period at a time. The coefficient estimates from those regressions with subsamples come close to the estimates with the full sample, which gives us confidence that the coefficient estimates reported for regressions (3) and (4) are not artifacts caused by overfitting to observations of a particular sample.

prices possesses more explaining power. While playing the mixed-strategy Nash equilibrium is too complicated anyway, subjects can achieve higher profits than under full competition by choosing myopic best response prices. However, subjects seem to resort to the simpler behavioral pattern of imitation when finding the best response price becomes more intricate. This interpretation partly coincides and partly contrasts with Fonseca's and Normann's (2013, p. 220) conjecture. We may agree with them that myopic best responses à la Edgeworth are more common in markets with fewer firms because this kind of behavior constitutes a form of imperfect tacit collusion. Yet, Fonseca and Normann furthermore argue that, as in markets with more firms and thus higher production capacity it is less attractive to be the high-price firm since this results in a relatively larger loss of potential profits, it may pay off for firms to behave unpredictably. We do not agree that behavior in markets with more firms follows such a rationale. After all, we observe that in triopoly subjects substitute myopic best responses for another behavioral pattern which is hardly unpredictable, namely simple imitation of others' prices.

8 Conclusion

This study investigates by means of a laboratory experiment firm conduct in a Bertrand-Edgeworth framework with increasing marginal costs and voluntary trading. While the Bertrand-Edgeworth setting is more realistic than the standard Cournot or Bertrand model, there have so far been relatively few experiments based on the Bertrand-Edgeworth model. Moreover, virtually all those earlier experiments either research the case with fixed capacity constraints instead of strictly convex costs or, if they do consider strictly convex costs, abandon the voluntary trading constraint, which restricts the applicability of those studies to a small minority of markets. Our study thus closes a relevant gap in the experimental economics literature.

The game-theoretic predictions of our underlying model are sophisticated. For the one-shot game there is only a mixed-strategy Nash equilibrium whose features are influenced tremendously by the rather inconspicuous change of the demand rationing rule. In the infinitely repeated game there is a large multiplicity of equilibria, but the stationary outcome pure-strategy equilibria that follow a simple stick-and-carrot approach and allow for full collusion to be sustained may appear focal. Further off-equilibrium predictions are Edgeworth's supposition that firms optimize myopically, taking into account the past choices

of their competitors, thus giving rise to price cycles, and the hypothesis that subjects simply imitate others' prices they have observed in the past.

Our experiment examines the effect of three treatment variables: number of firms in a market, demand rationing, and subject matching. We find that subjects behave considerably and significantly more collusively in duopoly than in triopoly and with proportional than with efficient demand rationing. Whether subjects are re-matched in every period or play in fixed groups repeatedly entails only a small effect. Whereas the former two effects reflect Nash equilibrium predictions qualitatively, the quantitative prognoses mostly do poorly. Conduct is largely more competitive than predicted by the static Nash equilibrium and also than might be expected from the results of related experimental studies. We observe some risk averse near maximin pricing, especially under random matching. The stationary outcome pure-strategy Nash equilibria of the infinitely repeated game we investigate do not possess any explaining power. In contrast, Edgeworth's price cycle theory rationalizes our data partially. Examining also imitation of other firms' past prices as an alternative behavioral pattern, we find that imitation is the main driving force in triopolies while in duopolies myopic best response pricing à la Edgeworth best explains behavior. We hypothesize that the complexity of the decision situation is crucial to what behavioral strategy subjects employ. Whereas play according to the mixed-strategy Nash equilibrium is computationally infeasible anyway, subjects can realize profits above the competitive level by mutually choosing their respective myopic best response price. When finding the myopic best response price becomes more intricate, subjects resort to imitation as a simpler strategy.

Further experiments of Bertrand-Edgeworth markets with increasing marginal costs and voluntary trading appear desirable for a validation of our results and a closer scrutiny of some phenomena we have found. As the conduct in our setting is quite competitive, it seems worthwhile to investigate whether changes in the setting such as the admission of overt communication or a variation in the amount of information provided to the subjects about the market or competitors' behavior and success will permit subjects to attain more collusion also in Bertrand-Edgeworth markets with increasing marginal costs.

Appendix A

English translation of the written instructions in treatment " $n = 2$, efficient, fixed".

Welcome to the Laboratory for Economic Experiments at Kiel University.

You are about to participate in an economic experiment where you will have to make some decisions. You can also earn some money. The amount of money will depend on both your own decisions and the decisions of the other participants in the experiment.

Please read these instructions carefully. If after reading there are any questions, please raise your hand. An experimenter will approach you and answer your questions in private. Please do not communicate with other participants during the experiment.

All participants receive the same instructions.

In this experiment you represent a **firm** that produces and sells a single good. On a market you **compete with one other firm** being represented by one of the other participants. Both firms produce and sell the same good.

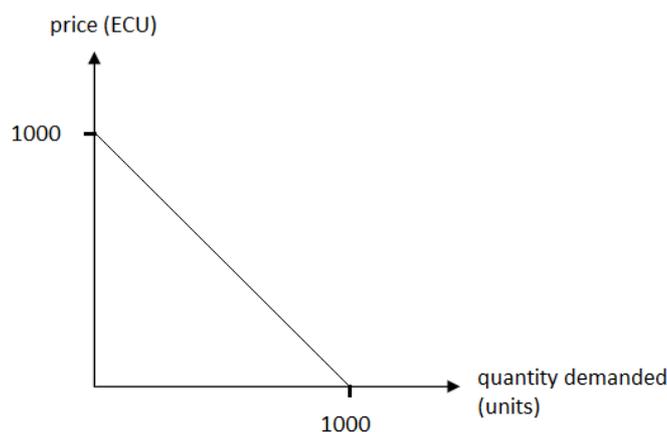
The experiment consists of **multiple periods**. There will be at least 20 periods. The exact number of periods will be determined by a random process. The experiment will end after the 20th period with probability 1/6. With probability 5/6, there will be another period. Provided that the 21st period will take place, after the 21st period there will be an identical random decision on whether the experiment will continue for another period. The same holds true after all following periods, if applicable. The random decisions made after each period will be independent of one another.

At the beginning of the experiment, the participants will be randomly matched into groups of two. The other member of your group will be your competitor. The grouping will be fixed throughout the experiment.

In each period you and the other firm must simultaneously and independently **decide on the price** at which to offer the good on the market. All **integers from 0 up to 1000 are admissible** prices.

Throughout the experiment, all money amounts will be accounted in **Experimental Currency Units (ECU)**. Your total profit will be paid off to you after the experiment at an **exchange rate of 120,000 ECU/Euro**, i.e. for each 120,000 ECU earned in the experiment you will receive 1 Euro.

The demand side of the market will be simulated by the computer and will be identical in all periods. There are many different potential customers with different willingnesses to pay for the good. A sale will come about if the offer price is no higher than a customer's maximum willingness to pay. At a price of 0 ECU there will be a total demand for 1000 units on the market. With a price increase by 1 ECU, demand is reduced by 1 unit. At a price of 1000 ECU no one will be ready to buy the good. Here is a graphical representation of this relationship:

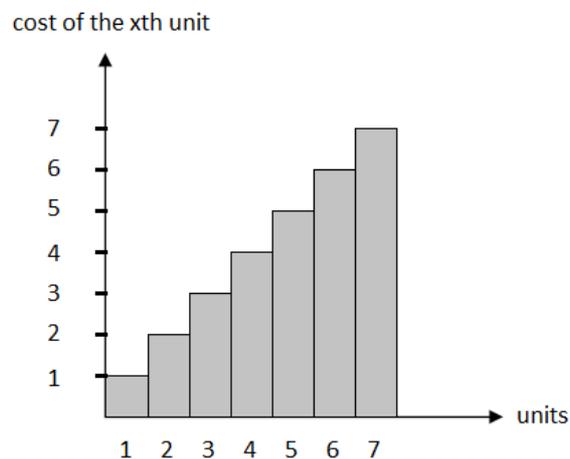


Customers will first try to purchase the good from the firm offering at a lower price. Those customers with the highest willingness to pay will be served first. In case one firm does not meet its whole demand, the remaining demand will be left for the other firm that offers the good at a higher price. If both firms choose the same price, demand will be divided equally among them.

Firms incur production costs that rise progressively in the quantity produced. The production costs are 1 ECU for the first unit produced, 2 ECU for the second unit, 3 ECU for the third unit, and so on. Your total production cost is the sum of the costs for the respective units. In the diagram below, total costs equal the grey area up to the respective quantity. Due to the progressive cost schedule, it can be unprofitable for a firm to satisfy its whole

demand at a low price. You only want to produce and sell an additional unit of the good as long as the revenue from its sale exceeds its production cost. Therefore, for any offer price chosen by you, there exists an upper production limit above which further units become unprofitable to produce.

Example: Assume you choose an offer price of 5 ECU, i.e. every unit sold yields a revenue of 5 ECU. In this case it will be unprofitable for you to produce more than 5 units. The production of a 6th unit would incur costs of 6 ECU, while its sale would yield only 5 ECU. Your profit (revenues minus cost) would decrease.



The arrangements in the experiment are such that quantities are computed automatically; you only have to decide on your offer price. The algorithm calculating the quantities ensures that you will produce only up to your implicit production limit explained above. Furthermore, it is ensured that each firm in each period only produces as many units as it can sell at the effective prices.

Your **profit** (in ECU) per period equals the offer **price chosen by you multiplied by your sales quantity minus the production cost** for this quantity. Your profit thus depends on the offer prices chosen in that period by you and by the other firm.

To get a general idea of your quantities and profits resulting from different price combinations, you may use the "**profit calculator**". Simply enter two hypothetical prices (one for each firm) and click on "Calculate". Then your resulting quantity and profit will be displayed. You may use the profit calculator throughout the whole experiment.

Moreover, after each period a screen with a "**history**" will inform you about the outcomes so far. For all past periods the "history" shows the prices chosen by the two firms, your resulting quantity, your resulting profit per period, and your total profit over all previous periods so far.

The following two screenshots illustrate the use of the profit calculator and the history:

Here you can enter two hypothetical prices and have the results calculated.

Here you can enter and submit your price choice for the current period.

Profit calculator

My price:
Other price:

Calculate

Decision

My price:

Submit price choice

My price	Other price	My quantity	My profit
300	400	300	45000
400	400	300	75000
400	300	300	75000
400	399	201	69200

The results of the profit calculator are displayed here.

Your total profit so far is displayed here.

The results of the last period are displayed here.

History of results

Your total profit so far: 160014

Your price: 500
Other price: 346
Your quantity: 154
Your profit: 65142

Period	Your price	Other price	Your quantity	Your profit
-2.0	0	0	0	0
-1.0	0	0	0	0
0.0	0	0	0	0
1.0	500	325	175	72188
2.0	213	432	213	22685
3.0	500	346	154	65142

This is the history showing the outcomes of all past periods. Zeros are displayed for trial periods.

To practice there will be **three trial periods** before the actual periods start. The procedures in the trial periods are the same as in the actual periods described above, but the outcomes of the trial periods will not add to your total profit or payoff. After the trial periods, the participants will be randomly re-matched into new groups. The new groups will stay fixed throughout all actual periods of the experiment. The profit calculator will be available to you during both trial and actual periods. The history, however, will not be displayed after trial periods and will only contain the data of the actual periods.

Your **total profit** is the **sum of your profits in the single periods** of the experiment (not including the trial periods).

Your total profit will be **paid to you** after the experiment at the above-mentioned exchange rate. None of the other participants will come to know your profit, and neither will you come to know the profit of any other participant. Furthermore, no participant will come to know with whom he or she interacted during the experiment.

If you click on the "Continue" button on your screen, some statements will appear to check whether all participants have understood the instructions. Please decide whether those statements are right or wrong. As soon as all participants will have evaluated the statements correctly, the first trial period will start.

If there are any questions concerning the experimental procedures, please raise your hand. Enjoy the experiment!

Appendix B

Evolution of the market price in single markets with fixed matching.

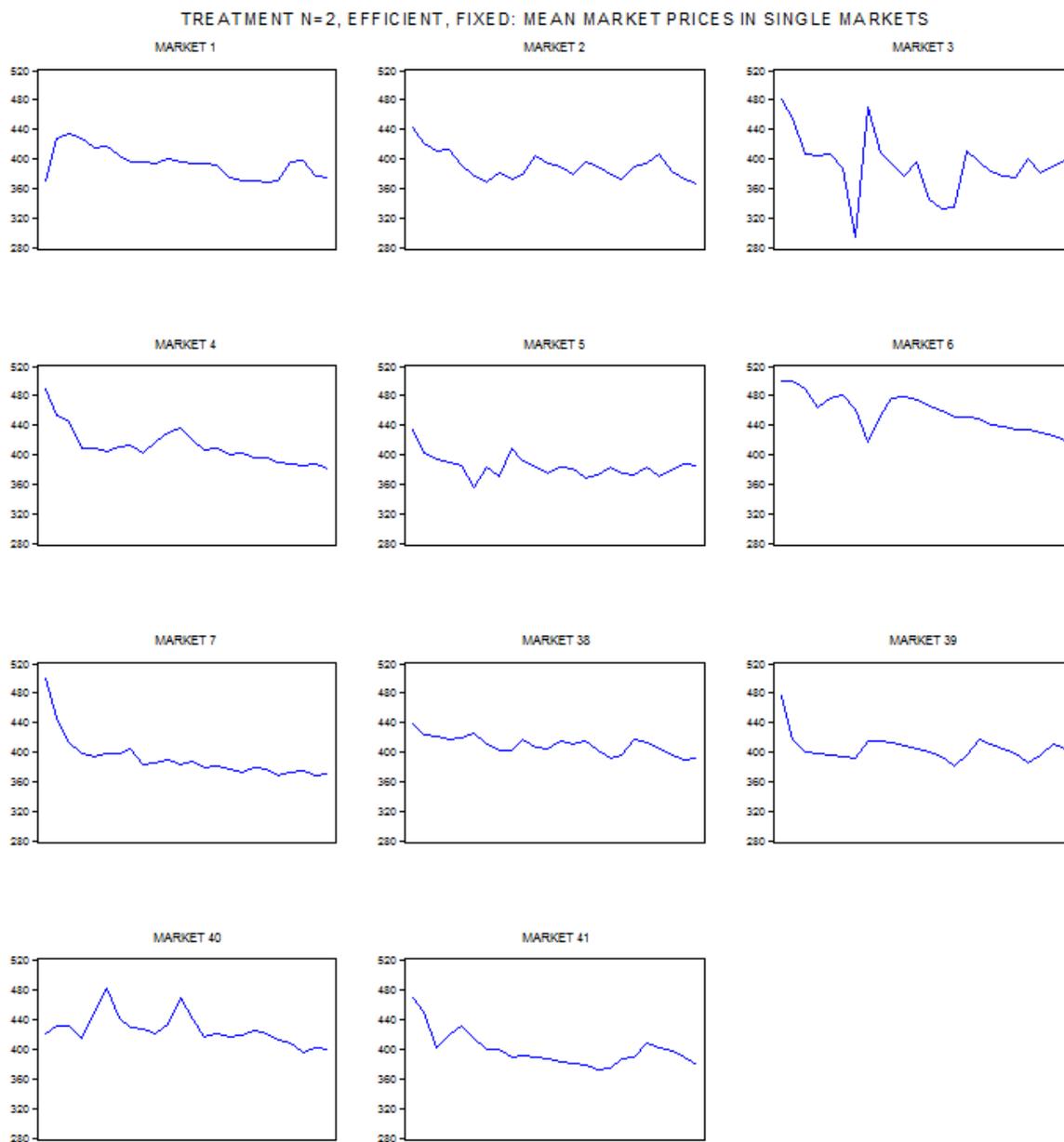


Figure B1: Mean market prices in single markets in treatment "n = 2, efficient, fixed".

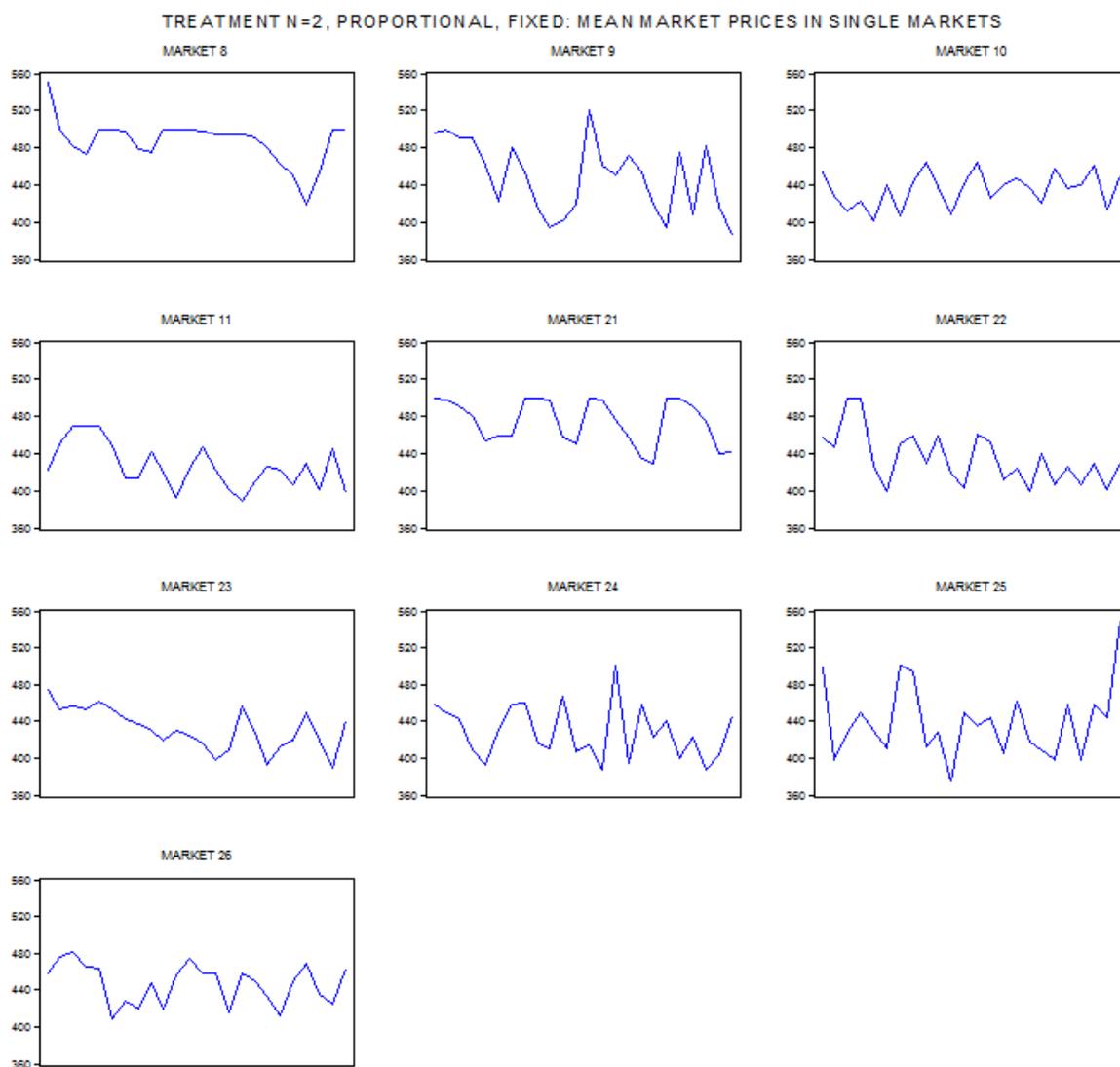


Figure B2: Mean market prices in single markets in treatment " $n = 2$, proportional, fixed".

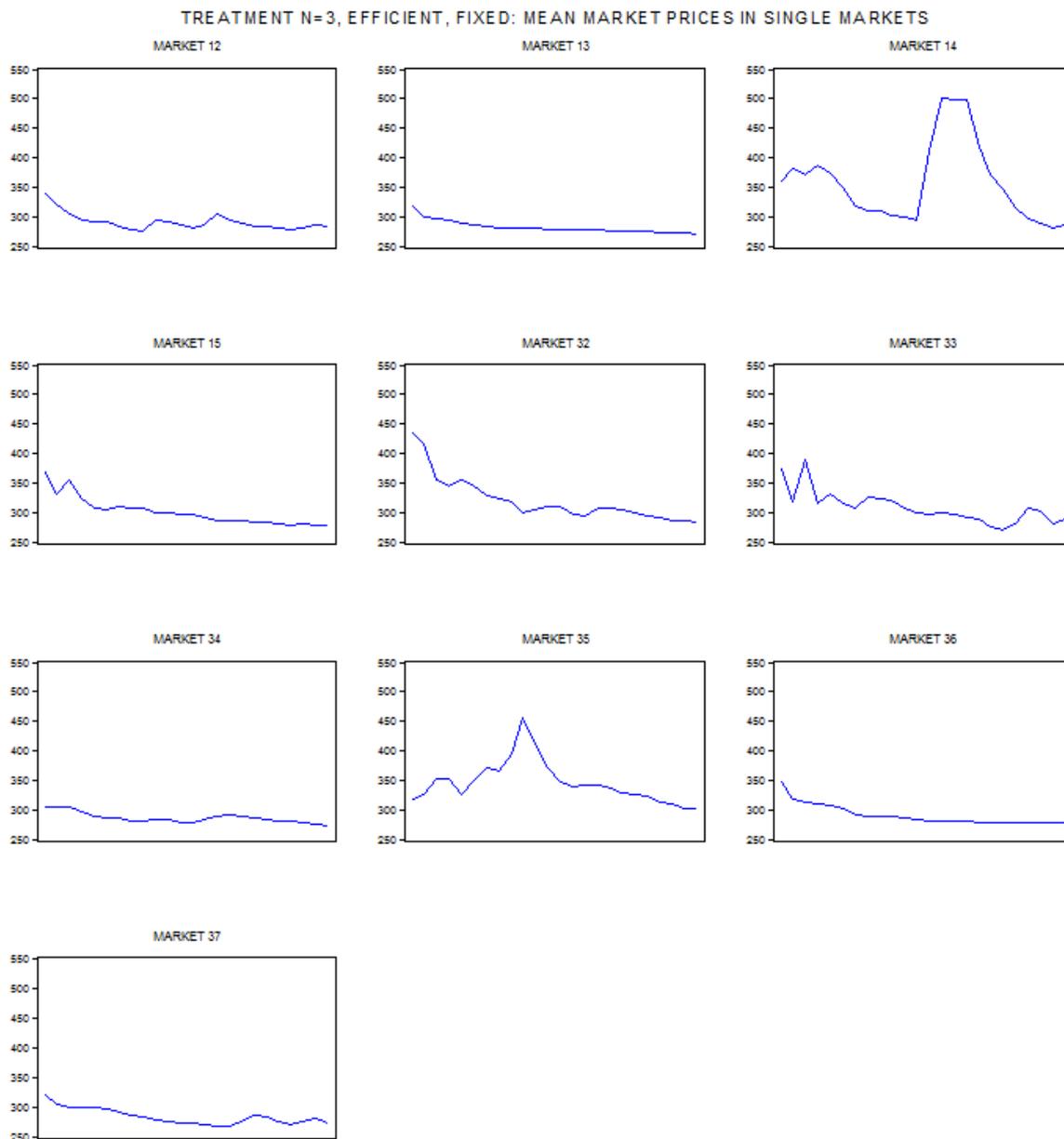


Figure B3: Mean market prices in single markets in treatment " $n = 3$, efficient, fixed".

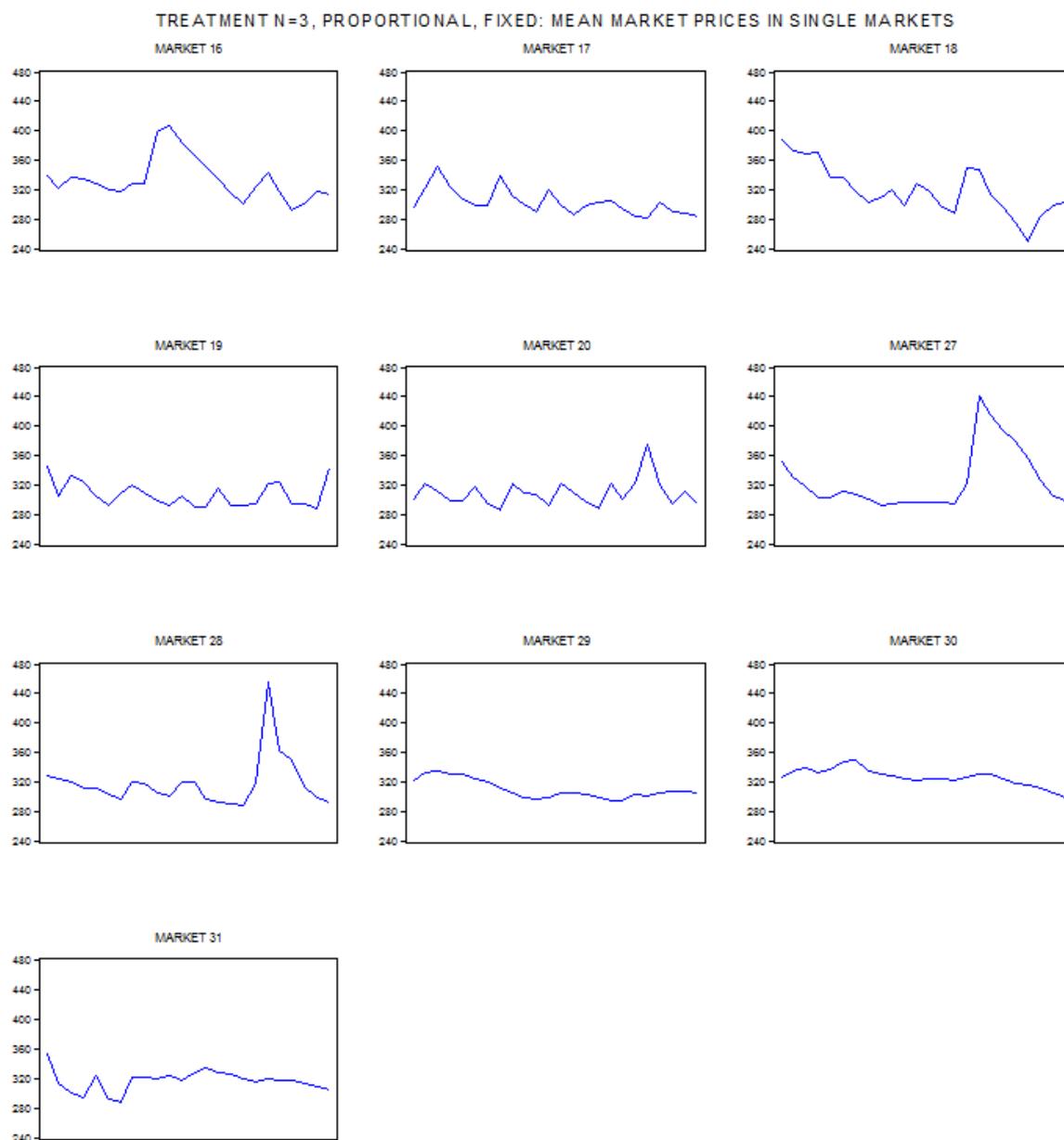


Figure B4: Mean market prices in single markets in treatment " $n = 3$, proportional, fixed".

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III Number of Firms, Rationing, Matching, and Knowledge: A Comprehensive Study of Variations in Experimental Kreps-Scheinkman Markets^{*}

Abstract. This study provides a comprehensive picture of experimental Kreps-Scheinkman markets with capacity choice at the first stage and subsequent price competition at the second. We conduct seven different treatments of Kreps-Scheinkman markets, varying the number of firms, the demand rationing scheme, the subject matching, and subjects' knowledge about the market. We find that only the number of firms entails a persistent effect on capacity choices. Price choices are affected by both the number of firms and the rationing scheme. Subjects in the high-knowledge condition behave in the same manner from the first periods as subjects with low knowledge do in later periods after having gained experience. In all treatments conduct is generally more competitive than the Cournot outcome, irrespective of whether the Cournot outcome is the Nash equilibrium or not. Nevertheless, the Cournot model entails some predictive power. Exact Cournot choices are more likely to occur for both capacities and prices under efficient demand rationing, where the Cournot outcome is the equilibrium, than under proportional rationing.

JEL classification: C90, D43, L11, L13.

Key words: Kreps-Scheinkman, Cournot, price competition, capacity choice, demand rationing, oligopoly, laboratory experiment.

^{*} Corresponding working paper: Martin Jacobs (2016), Number of Firms, Rationing, Matching, and Knowledge: A Comprehensive Study of Variations in Experimental Kreps-Scheinkman Markets, Economics Working Paper 2016-02, University of Kiel.

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1 Introduction

The model of sequential capacity and price choice by Kreps and Scheinkman (1983) (hereafter KS) provides a more realistic description of oligopolies than the classical models by Cournot (1838) and Bertrand (1883) with only one choice variable. The most remarkable feature of the KS model is that its outcome is, under suitable assumptions about demand rationing, equivalent to the Cournot outcome. This implies that prices decline as more firms operate in the market.

While the KS model has been put to experimental tests against other market institutions, and more recently also variations within the KS model have come into the focus of experimental research, the evidence about the latter is still scattered and limited. The aim of this study is to provide a comprehensive analysis on how different experimental conditions affect the results within KS markets. For this purpose, we set up seven treatments mimicking the KS market environment while controlling for (i) the number of firms in a market (duopoly vs. triopoly), (ii) demand rationing (efficient vs. proportional), (iii) subject matching (random vs. fixed), and (iv) subjects' understanding about the KS market mechanism, referred to as "knowledge" (low vs. high). To the best of our knowledge, this is the first study to experimentally investigate the effect of the latter two variables in KS markets. While the impacts of both number of firms and demand rationing have been investigated in one study already, our results present a comprehensive picture of what drives outcomes in experimental KS markets.

As to capacity choices, we find, contrary to existing literature, that the number of firms in the market significantly affects behavior. Notably, larger capacities are chosen in triopoly than in duopoly. Demand rationing, in line with earlier findings, does not significantly influence capacity choice. The matching procedure does not have an impact either. Subjects with good understanding of the KS mechanism ("high-knowledge" treatment) choose lower capacities in the early periods, but the difference vanishes in later periods when subjects in the "low-knowledge" treatments gain experience. While capacities tend to decline as the number of periods already played increases, they converge to levels significantly higher than the Cournot benchmark in all treatments. Exact Cournot capacity choices are rarely observed, but occur significantly more often under efficient demand rationing, where the Cournot outcome is the Nash equilibrium prediction, than under proportional rationing.

As to the second-stage outcomes, we observe considerably lower prices than in the Cournot outcome. Once more, the number of firms entails a significant effect: duopoly yields higher prices than triopoly. "High knowledge" again shows an effect only in the early periods, and

subject matching never influences pricing in a significant way. Contrary to the first stage, also the rationing scheme has a significant effect: prices are higher with proportional than with efficient rationing. All effects remain valid if we control for the capacities chosen at the first stage.

The sensitivity of pricing to installed capacity is low in the initial rounds and increases with the number of periods played. That is, subjects have to learn about the effect of their capacity choices on the pricing subgame at the second stage. Only in the high-knowledge condition do subjects exhibit a large sensitivity from the beginning.

If subjects choose capacities sufficiently low such that market-clearing pricing is the pure-strategy equilibrium at the second stage, markets do not always clear. Again there is a significant effect of demand rationing on behavior. The exact market-clearing price is chosen more often under efficient rationing.

As a result of the choices at the two stages, average profits substantially fall short of the Cournot benchmark. It remains a puzzle why subjects continue to install excess capacities even after many rounds. Explanatory approaches from the literature do not withstand a closer scrutiny of our data.

The remainder of this study is structured as follows. In sections 2 and 3 we briefly review the theoretical and experimental literature on KS markets. Section 4 explains the design and procedures of our experiment. Section 5 provides the equilibrium predictions. We present and discuss our results in section 6. Section 7 concludes.

2 Theory

The two most prominent oligopoly models by Cournot (1838) and Bertrand (1883) both suffer from major shortcomings. While the Cournot model of competition in production quantities yields the intuitive result that the market price is the lower the more firms compete in the market, the mechanism by which the market price is determined remains apocryphal. Bertrand makes the more realistic assumption that firms compete in prices. However, his model implies the unrealistic result that the price is equal to marginal cost and firms earn zero profits as soon as there is more than one firm in the market. KS suggest a resolution of this dilemma by introducing a two-stage duopoly setting. At the first stage, the two producers of a homogenous good simultaneously choose their production capacities and incur costs for building capacity. At the second stage, simultaneous price competition takes place, where each firm can sell only up to its capacity chosen before. Customers first access the firm which

offers the good at the lower price. Demand is rationed according to the efficient scheme, i.e. those customers with the highest willingness to pay for the good are served first. KS show that the unique Nash equilibrium in this setting is equivalent to the Cournot outcome.

Davidson and Deneckere (1986) detect that the KS result is sensitive to which rationing rule is assumed. They argue that under different rationing rules leaving more residual demand to the firm with the higher price generally only a mixed-strategy equilibrium will exist whose outcome is more competitive than the Cournot outcome. This matter is elaborated on and clarified by Lepore (2009). While the KS result does not necessarily hold under other than the efficient rationing rule, it still *can* hold. Investigating the alternative assumption of proportional or random demand rationing, Lepore provides conditions for the Cournot outcome to be an equilibrium and to be the unique equilibrium of the two-stage game. Loosely speaking, the KS result still holds under proportional rationing if the cost for building capacity is sufficiently high relative to customers' willingness to pay.^{1,2}

Whereas the KS result thus to some extent hinges upon the rationing assumption, other ingredients of the original model have been found to be unproblematic. Bocard and Wauthy (2000; 2004) generalize the KS result to the oligopoly with more than two firms. Benoît and Krishna (1987) deal with a dynamic setting in which firms play the KS game repeatedly. When firms have to choose their capacities once and for all at the beginning or can only adjust their capacity choice by a small amount in each period, the Cournot outcome is a subgame perfect Nash equilibrium. However, the equilibrium is no longer unique. All further subgame perfect equilibria of the game entail excess capacities which are necessary threats to sustain collusion. When capacities can be adjusted freely in each period, excess capacities are no longer needed and also full collusion with monopoly-level profits can be sustained in equilibrium, provided that discounting is not too severe.

3 Experimental literature

Early experimental tests of the KS model are mainly concerned with performance compared to theoretical benchmarks and to other market institutions. The main result is that subjects in KS markets choose higher capacities and lower prices than predicted. When subjects gain

¹ In two further studies Lepore (2008; 2012) is concerned with the conditions for the Cournot outcome to be an equilibrium of the KS game under either rationing rule when the competitor's production costs are uncertain or, respectively, market demand is uncertain when firms choose capacities.

² That strand of literature follows the assumption made by KS that the market demand function is concave. Madden (1998) and Wu et al. (2012) analyze the conditions for the KS result to hold under different specifications of demand.

experience, their behavior approaches, but mostly does not reach the Cournot-Nash equilibrium.³

Davis (1999) compares treatments of price competition with and without advance production in posted offer triopoly markets with efficient demand rationing, finding prices to settle somewhere between the competitive (Bertrand) and the Cournot benchmark. Profits increase throughout the experiment, but do not converge to the predicted Cournot level. The reported data about sold quantities indicate that capacities (not being reported) are chosen above equilibrium level.

Muren (2000) investigates KS triopolies with efficient rationing and compares her outcomes to both theoretical predictions and the experimental results on Cournot and Bertrand by Fouraker and Siegel (1963). She observes that inexperienced subjects set capacities considerably above the Cournot level. In early periods, capacities even exceed the predicted level with full competition. Capacities then fall, but are still closer to the competitive than to the Cournot benchmark by the last rounds. Price choices are not reported. As a result of massive overproduction, more than half of the inexperienced subjects do not realize overall profits throughout the experiment. Experienced subjects' capacity choices are still somewhat above, but come close to the Cournot prediction.

Goodwin and Mestelman (2010) compare KS duopolies with efficient rationing to Cournot markets and, similarly to Muren, vary subjects' experience. While inexperienced subjects in Cournot markets behave close to the theory prediction right from the beginning, in KS markets too high capacities and too low prices are set first, but in the last rounds behavior converges to the Cournot outcome. Experienced subjects behave similarly, namely somewhat more collusive than the Cournot-Nash prediction, in Cournot and KS markets.

Some more recent experiments are, like our study, mainly interested in the effects of variations *within* the KS framework. Considering price competition with advance production in markets with two and three firms, Brandts and Guillen (2007) find no significant difference between market capacities in duopolies and triopolies. However, in duopoly firms choose higher prices at the second stage.⁴

The experience effect is further scrutinized by Le Coq and Sturluson (2012) who not only let inexperienced and experienced subjects play against each other in homogeneous groups, but also pair inexperienced with experienced subjects, thus varying both the subject's own and

³ Another related strand of literature which is not considered here deals with sequential capacity and price choice of firms producing heterogeneous goods, see e.g. Anderhub et al. (2003).

⁴ Brandts and Guillen assume box demand which, strictly speaking, does not fit the KS model.

her competitor's experience, where subjects' experience levels are common knowledge. Moreover, in contrast to all aforementioned studies, subjects do not play in fixed groups, but are re-matched in every round of the experiment. Le Coq and Struluson find that capacities (prices) decrease (increase) with both the subject's own and her competitor's experience. Furthermore, experienced subjects are more responsive in their price choices to capacities installed at the first stage. The authors conjecture that the subjects' level of rationality increases with experience and that experienced players anticipate and respond to the lesser rationality of their inexperienced competitors.

Finally, Lepore and Shafran (2013) put differently parameterized versions of the KS model with diverging equilibrium predictions to the test. They vary the rule by which demand is rationed and the cost of installing capacity and thus conduct four different KS duopoly treatments. In their low-cost condition with proportional demand rationing, the Cournot outcome is not a Nash equilibrium. Subjects are randomly re-matched in every round in order to approximate a one-shot game more closely. Lepore and Shafran observe that Cournot capacities are chosen frequently in the high-cost condition while higher capacities are common in the low-cost treatments. While the demand rationing rule does not influence capacity choice, pricing is affected as in the low-cost treatments subjects set higher prices under proportional rationing, controlling for capacity. In the high-cost treatments, Cournot capacities are mostly followed by Cournot prices at the second stage.

4 Experimental design and procedures

As our experiment aims at drawing a comprehensive picture of the effects of different variations within the KS setting, our design contains four treatment variables: number of firms in the market, demand rationing, subject matching, and understanding of the KS market mechanism, briefly referred to as "knowledge".

All in all, we conducted seven treatments with different combinations of variable states. A matrix of the conducted treatments is provided in Figure 1 where the seven black numbered squares stand for the seven treatment conditions. Treatments which differ with respect to only one of the four treatment variables are connected with a line. The core treatment structure is made up of the treatment variables "number of firms" (duopoly and triopoly) and "demand rationing" (efficient and proportional) which are varied in a 2x2-design (treatments 1-4). In these four treatments, subjects interact within fixed groups throughout all rounds.

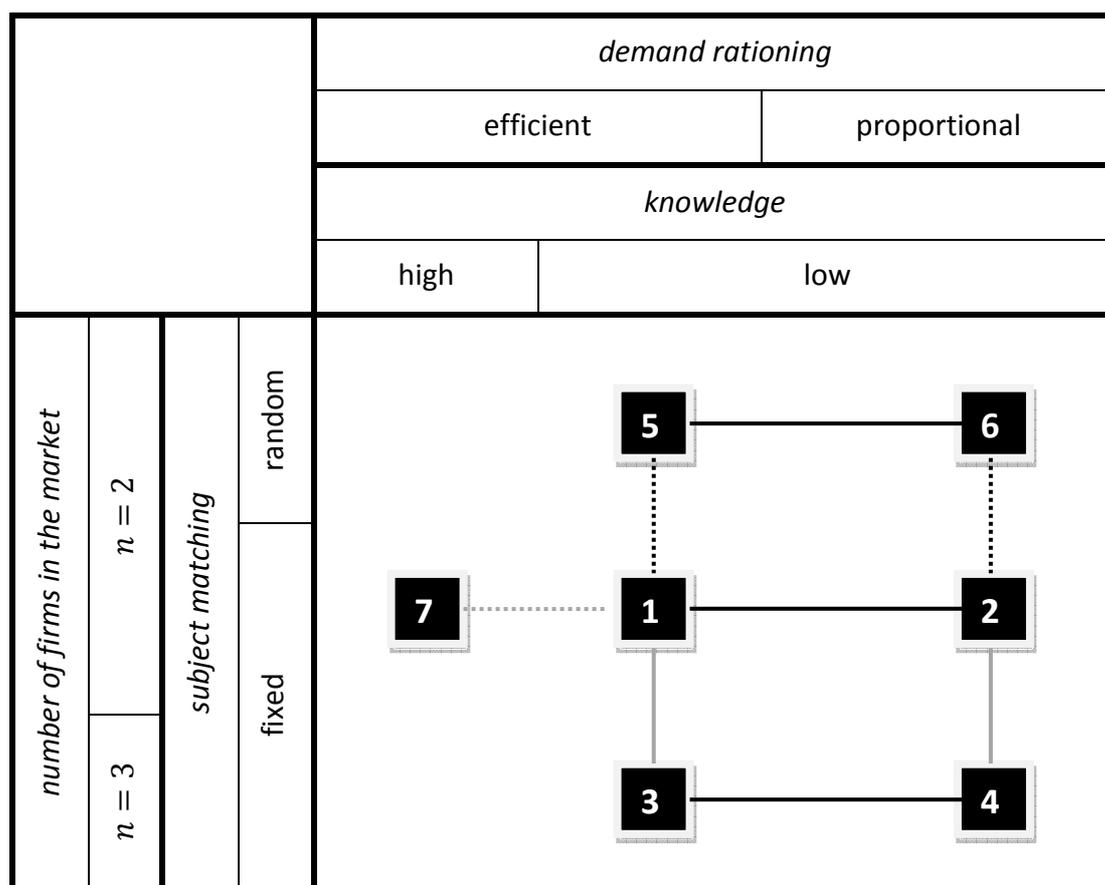


Figure 1: The treatment matrix.

Additionally, we also conducted two duopoly treatments with random subject matching and different rationing rules (treatments 5 and 6). While fixed matching is the prevailing condition in related experiments and certainly the most realistic assumption with respect to real oligopoly markets, the original KS model pertains to a one-shot game. Treatments with random matching resembling a one-shot game are therefore needed to rigorously test the predictive power of the KS Cournot outcome and its sensitivity to the employed rationing scheme. Due to financial constraints and because the results of a study by Kruse (1993) suggest that one-shot Nash equilibria can predict behavior also in fixed matching treatments, we abstained from conducting random matching treatments in the triopoly. The remaining treatment number 7 has the purpose to investigate the effect of subjects' understanding of the KS setting. For the so-called "high-knowledge" condition, subjects were recruited from an industrial organization class in the undergraduate economics program. They received special, more extensive instructions in order to allow for a distinguished understanding of the underlying model.⁵ As the number of subjects suitable for the high-knowledge condition was

⁵ The instructions are explained in greater detail below.

limited, we conducted only one high-knowledge treatment, namely duopolies with efficient rationing and fixed matching. Whereas the experience dimension in earlier studies considered the effect of learning-by-doing, our high-knowledge condition enhances theoretical understanding *ex ante*.

The linear total demand function in our duopoly and triopoly markets is $D(p) = 1000 - p$. At the first stage, firms choose their individual capacities k_i . Each firm can install 0 to 1000 integer units of capacity where each such unit costs 100 Experimental currency units (ECU). At the second stage, each firm decides about its price p_i , where integer values from 0 to 1000 ECU are admissible prices. When choosing prices, firms know their competitors' capacity choices from the first stage. Computer-simulated buyers first approach the firm that offers the good at the lowest price. If that firm cannot satisfy all the demand with its capacity, residual demand is left for the other firms. The quantity q_i firm i is able to sell in any period depends on the price vector \mathbf{p} , the capacity vector \mathbf{k} , and the effective rationing scheme,

$q_i(\mathbf{p}, \mathbf{k})$

$$= \begin{cases} \max \left\{ 0, \min \left\{ \frac{1}{m} \left(1000 - p_i - \sum_{j|p_j < p_i} k_j \right), k_i \right\} \right\} & \text{for efficient rationing} \\ \max \left\{ 0, \min \left\{ \frac{1000 - p_i}{m} \left(1 - \sum_{j|p_j < p_i} \frac{k_j}{1000 - p_j} \right), k_i \right\} \right\} & \text{for proportional rationing} \end{cases}$$

where m represents the number of firms charging the same price as firm i (including i itself). Firm i 's profit is given by $\Pi_i(\mathbf{p}, \mathbf{k}) = p_i q_i(\mathbf{p}, \mathbf{k}) - 100k_i$.⁶

In total 15 sessions were conducted in the economics experimental lab at Kiel University in three waves during January, July, and November 2014. 170 students from all fields of study participated as subjects in the low-knowledge treatments.⁷ Due to some no-shows, the number of markets per treatment varies between 11 and 14. For the high-knowledge treatment, 32 other students were recruited from an industrial organization class in the undergraduate economics program. All those students were familiar with the standard Cournot and Bertrand oligopoly models (but not with the KS model). Each subject participated only once in the experiment.

⁶ As it is common in the literature on demand rationing, residual demand under proportional rationing is defined by the expectation of all possible customer reservation price orderings, and we do not actually model the order of such prices as a random variable. Zouhar (2015) notes that this difference as well as the exact tie-breaking rule (when several firms set the same price) affects quantities and profits in some cases. While those effects also cause differences concerning the Nash equilibrium outcomes, the differences are tiny.

⁷ For part of the sessions, the hroot software package (Bock et al., 2014) was used for recruitment.

Subjects were randomly seated in the lab upon their arrival. They could not infer with whom of the other subjects they would interact during the experiment. Subjects were provided with printed instructions.⁸ In the low-knowledge treatments the settings were explained verbally and graphically, and examples illustrating demand rationing were given. The more extensive instructions for the high-knowledge treatment also contained an algebraic representation of the underlying model. Furthermore, candidates for the profit-maximizing prices were proposed given capacities and the competitor's price. The experiment was computerized using z-tree (Fischbacher, 2007). In all treatments subjects could simulate the results of their decisions by using a built-in profit calculator which returned a subject's resulting sales quantity and profit when hypothetical capacity and price choices for all firms were entered.

When subjects finished reading the instructions, the experimenter again highlighted the experiment's main features in a short presentation. Before subjects could proceed to the actual experiment in the computer program, they had to master a set of tasks. In the low-knowledge treatments, some rather simple yes-no questions about the experimental setup and procedures had to be answered correctly. In the high-knowledge treatment, subjects were required to calculate their sales and profit for three given sets of capacity and price vectors without the aid of the profit calculator. Moreover, in three more tasks, now with the profit calculator, they had to determine the profit-maximizing price given hypothetical capacities and a hypothetical price of the competitor. In one of those three tasks, we asked for the profit-maximizing price given (i) Cournot capacities were installed at the first stage and (ii) the market clearing Cournot price was set by the competitor at the second stage. The instructions and training in the high-knowledge treatment were tailored in a way to give the Nash equilibrium prediction its best shot without actually telling the subjects what the Nash equilibrium is and thereby implementing recommended play.

After the tasks had been finished, three unpaid trial rounds and 18 paid rounds of the KS game were played. Since reading the instructions and working through the tasks would take considerably more time in the high-knowledge treatment, we reduced the number of paid rounds to nine for that treatment. The number of rounds to be played was common knowledge. An average session took about 135 minutes in both knowledge conditions. After each round, subjects were informed about all choices in their market and about their sales quantity and profit resulting from those choices. Subjects were not informed about their

⁸ The instructions of the high-knowledge treatment, exemplary instructions of one low-knowledge treatment, and the control questions to be answered by the subjects can be found in the appendices.

competitors' profits explicitly, but could easily inquire them using the profit calculator if they wanted to.

Subjects received a show-up fee of 6 Euro plus the sum of their earnings in the paid rounds at a predefined exchange rate. Moreover, they had a starting balance to compensate for losses in early periods.⁹ Subjects had an unlimited credit during the experiment; if their balance became negative, they could nevertheless go on installing capacity and selling. In case a subject's balance was negative after the last round, only the show-up fee was paid. Payments were made in private. The average payment per subject was 19.7 Euro, including the show-up fee.

5 Equilibrium predictions

The setting originally investigated by KS is most closely resembled in treatment 5, the duopoly with efficient demand rationing and random subject matching. The Nash equilibrium prediction for this treatment is the Cournot outcome with individual capacities $k_i = 300$ installed at the first stage (implying a market capacity of $K = \sum_i k_i = 600$) and a market-clearing price of $p = 400$ set at the second stage. The resulting equilibrium profit per firm is $\Pi_i = 90000$. In order to investigate the effect predicted by Davidson and Deneckere (1986), demand rationing is varied in treatment 6. Although Davidson and Deneckere do not provide a closed-form equilibrium solution of their modified two-stage game, they show that the KS result does no longer hold and that in general there will be a unique mixed-strategy Nash equilibrium that, given the low production costs in our setting, is more competitive than the Cournot outcome. The last prediction stands in contrast to experimental evidence indicating that markets are less competitive with proportional than with efficient demand rationing (Kruse, 1993; Lepore and Shafran, 2013; Jacobs and Requate, 2016a; 2016b).

The five remaining treatments employ fixed subject matching. They thus deviate from the original KS model, but are closer to real oligopoly markets. With that matching, repeated game effects may possibly arise. In case of infinite repetition and a sufficiently high discount factor, all outcomes that dominate the one-shot Nash equilibrium can be sustained by a suitable trigger strategy (Friedman, 1971). Benoît and Krishna (1987) show that in the KS framework this can be achieved without excess capacities if capacities can be freely adjusted

⁹ The exchange rate was 40,000 ECU per Euro in the triopoly treatments and in the high-knowledge duopoly treatment, and 80,000 ECU per Euro in the low-knowledge duopoly treatments. The starting balance was 240,000 ECU in the triopoly treatments, 200,000 ECU in the high-knowledge duopoly treatment, and 320,000 ECU in the low-knowledge duopoly treatments.

in each round, as is the case in our setting. Nevertheless, we maintain the equilibrium of the one-shot game also as a prediction for the fixed matching treatments, i.e. we predict the Cournot outcome for treatments 1, 3, and 7, and a more competitive mixed-strategy equilibrium outcome for treatments 2 and 4. The altered subject matching is theoretically irrelevant since our subjects were informed in advance about the number of rounds to be played. The repeated game induced by our experiment can thus be solved via backward induction and the equilibrium predictions for the stage game remain valid. Moreover, the results by Kruse (1993) indicate that the one-shot Nash equilibrium predicts behavior in price competition experiments with fixed matching even if subjects do not know when the game will end. The variation of knowledge in treatment 7 does not influence the theory prediction either. While subjects in the high-knowledge treatment receive more extensive instructions and training, also the instructions given in the low-knowledge treatments allow for full information. Boccard and Wauthy (2000; 2004) have extended the KS result beyond duopoly; hence our predictions for the triopoly treatments 3 and 4. In triopoly, the Cournot outcome corresponds to a market capacity of $K = 675$ at the first stage and a market price of $p = 325$ at the second stage; the equilibrium profit per firm is $\Pi_i = 50625$. Figure 2 summarizes the equilibrium predictions.

				<i>demand rationing</i>	
				efficient	proportional
<i>number of firms in the market</i>	$n = 2$	<i>subject matching</i>	random	Treatment 5: Cournot outcome: $k_i = 300, K = 600,$ $p = 400, \Pi_i = 90000.$	Treatment 6: Unique mixed-strategy NE, more competitive than Cournot outcome.
			fixed	Treatments 1 and 7: Cournot outcome: $k_i = 300, K = 600,$ $p = 400, \Pi_i = 90000.$	Treatment 2: Unique mixed-strategy NE, more competitive than Cournot outcome.
	$n = 3$		fixed	Treatment 3: Cournot outcome: $k_i = 225, K = 675,$ $p = 325, \Pi_i = 50625.$	Treatment 4: Unique mixed-strategy NE, more competitive than Cournot outcome.

Figure 2: Nash equilibrium (NE) predictions for the treatments.

6 Results

After a short remark on the effect of bankruptcies, we will analyze choices at the two stages of the game separately. We first consider capacity choices and then proceed to the analysis of price choices. Finally we examine the profits that result from subjects' choices at the two stages.

6.1 Bankruptcies

Our bankruptcy rules exposed in section 4 are a potential source of contamination for our data. As subjects can go on playing, but cannot lose further money when their balance is negative, they may have an incentive to behave more adventurously in order to have a chance to return to a positive balance. When designing the experiment, we considered the starting balance sufficiently high to prevent bankruptcies and therefore chose a rule as simple as possible to deal with the hypothetical case of a bankruptcy. However, it turned out that in fact six of our 202 subjects went bankrupt in the course of the experiment. Four bankruptcies occurred in treatment 3, where there were two markets in each of which two of the three participating subjects went bankrupt. Two further bankruptcies occurred in treatments 1 and 4. Despite the possible effect of bankruptcies on incentives, we do not find outcomes in markets with bankruptcies to be substantially different from those without bankruptcies when we look into the data.¹⁰ Markets with bankruptcies are therefore included in our analyses to retain a larger data set. Our results would change only slightly and all conclusions we draw will remain the same if we exclude those markets where bankruptcies occurred.

6.2 Capacity choices

Figures 3 and 4 summarize capacity choices at the first stage. Figure 3 aggregates the data within each treatment and period and shows the development of the mean market capacities in each treatment across periods. Combining the data from several periods, Table 1 numerically presents the information visualized in Figure 3. In the histograms in Figure 4, in contrast, choices across markets in the same treatment are not aggregated, but the distributions of market capacities in each treatment over all periods are illustrated.

¹⁰ Muren (2000) reports on a similar incentive problem in her KS experiment. She also concludes that bankruptcies did not substantially affect her data.

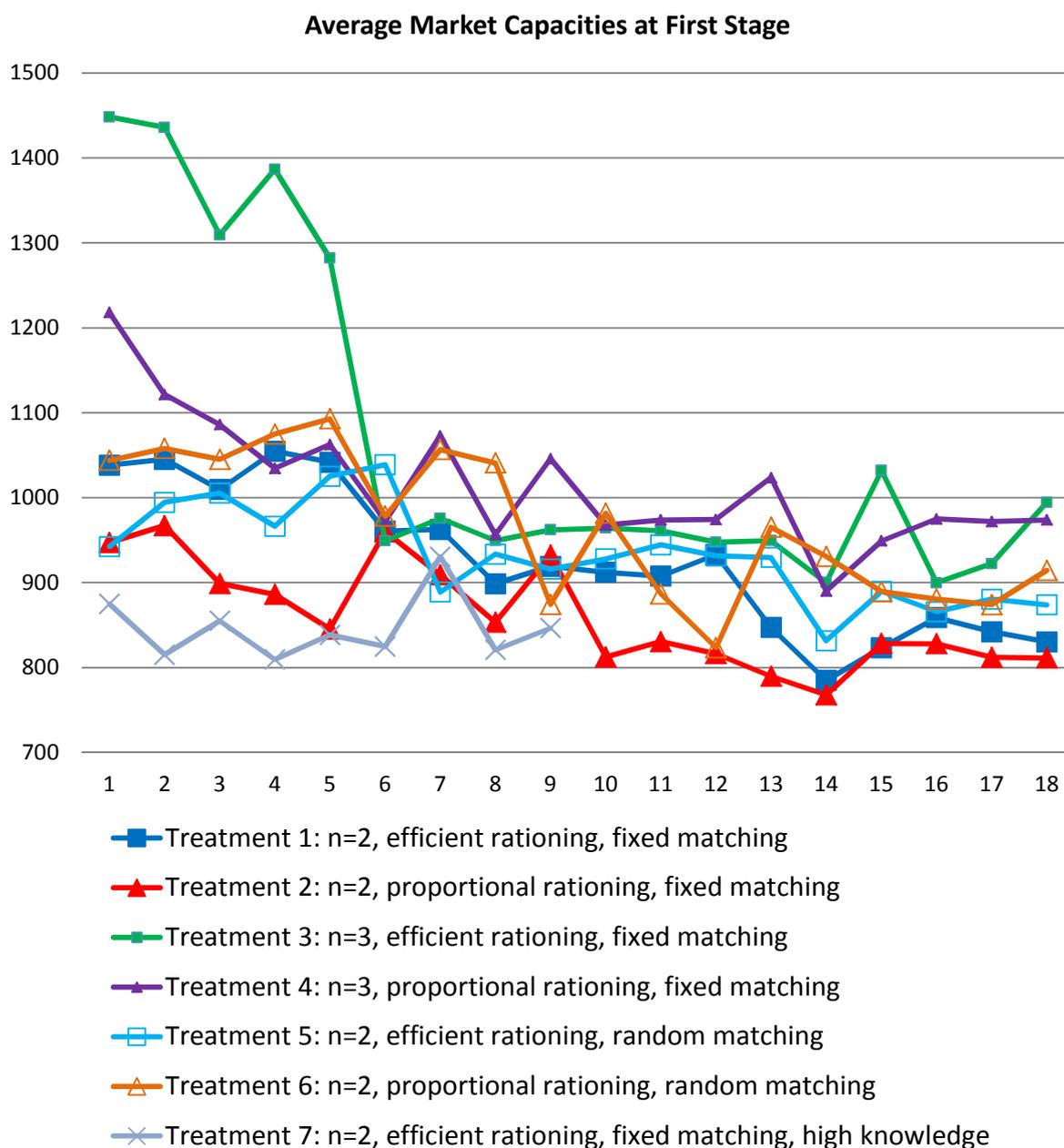


Figure 3: Series of market capacities.

The first obvious result from Figure 3 is that mean market capacities in all treatments are considerably higher than Cournot capacities. Comparing the mean capacities across all periods in Table 1 to the respective Cournot benchmark, we find that the observed mean capacities are largely 40 to 60 percent above Cournot levels. This holds true for all treatments, no matter whether the Cournot outcome is the theoretical prediction or not. Although the mean capacities in Table 1 are mostly somewhat higher than the corresponding medians, the latter are still substantially above Cournot levels. The results thus are not driven by a few outliers, which is also confirmed by the capacity distributions in Figure 4.

Market Capacities				
	<i>periods</i>	<i>1-18</i>	<i>1-9</i>	<i>10-18</i>
Treatment 1 <i>n</i> = 2, eff. rat., fixed match.		926.163 (296.191) [850.000]	992.325 (294.493) [1000.000]	860.000 (283.917) [800.000]
Treatment 2 <i>n</i> = 2, prop. rat., fixed match.		860.940 (189.428) [825.500]	911.102 (197.477) [900.000]	810.778 (167.406) [800.000]
Treatment 3 <i>n</i> = 3, eff. rat., fixed match.		1083.051 (387.863) [950.000]	1203.750 (455.162) [1200.000]	962.352 (256.321) [896.000]
Treatment 4 <i>n</i> = 3, prop. rat., fixed match.		1014.991 (256.595) [1000.000]	1063.361 (278.391) [1010.000]	966.620 (223.770) [948.000]
Treatment 5 <i>n</i> = 2, eff. rat., random match.		932.621 (202.022) [919.500]	967.949 (196.472) [950.000]	897.293 (202.284) [900.000]
Treatment 6 <i>n</i> = 2, prop. rat., random match.		967.324 (289.262) [950.000]	1029.463 (274.644) [1002.500]	905.185 (291.380) [900.000]
Treatment 7 <i>n</i> = 2, eff. rat., fixed match., high knowledge		n.a.	846.229 (191.890) [829.000]	n.a.
Standard errors in parentheses. Medians in square brackets.				

Table 1: Market capacities: Summary statistics.

Figure 3 indicates that mean capacities across treatments diverge in the first periods. The highest capacities are observed in the triopoly treatments (3 and 4),¹¹ the lowest in the high-knowledge treatment (7). Mean capacities then decline over periods. An exception is the high-knowledge treatment which features no visible time trend. By period nine, the last period for which observations from the high-knowledge treatment are available, the mean capacities in the six other treatments have roughly approached that in the high-knowledge treatment. Towards the last periods, mean capacities appear to reach constant levels. Those hypothesized convergence levels are close to each other for all treatments. Capacities in the triopolies still are the highest, but there is no big difference to the capacities in duopoly markets. In the last few periods, capacities in the triopoly treatments are largely between 900 and 1000 while those in the duopoly treatments are mostly between 800 and 900 (see also the rightmost column of Table 1).

¹¹ The very large mean capacities observed in the first periods in treatment 3 are partly due to markets in which bankruptcies occurred. However, even if one disregards those markets, treatment 3 still has the highest mean capacities among all treatments in the first periods.

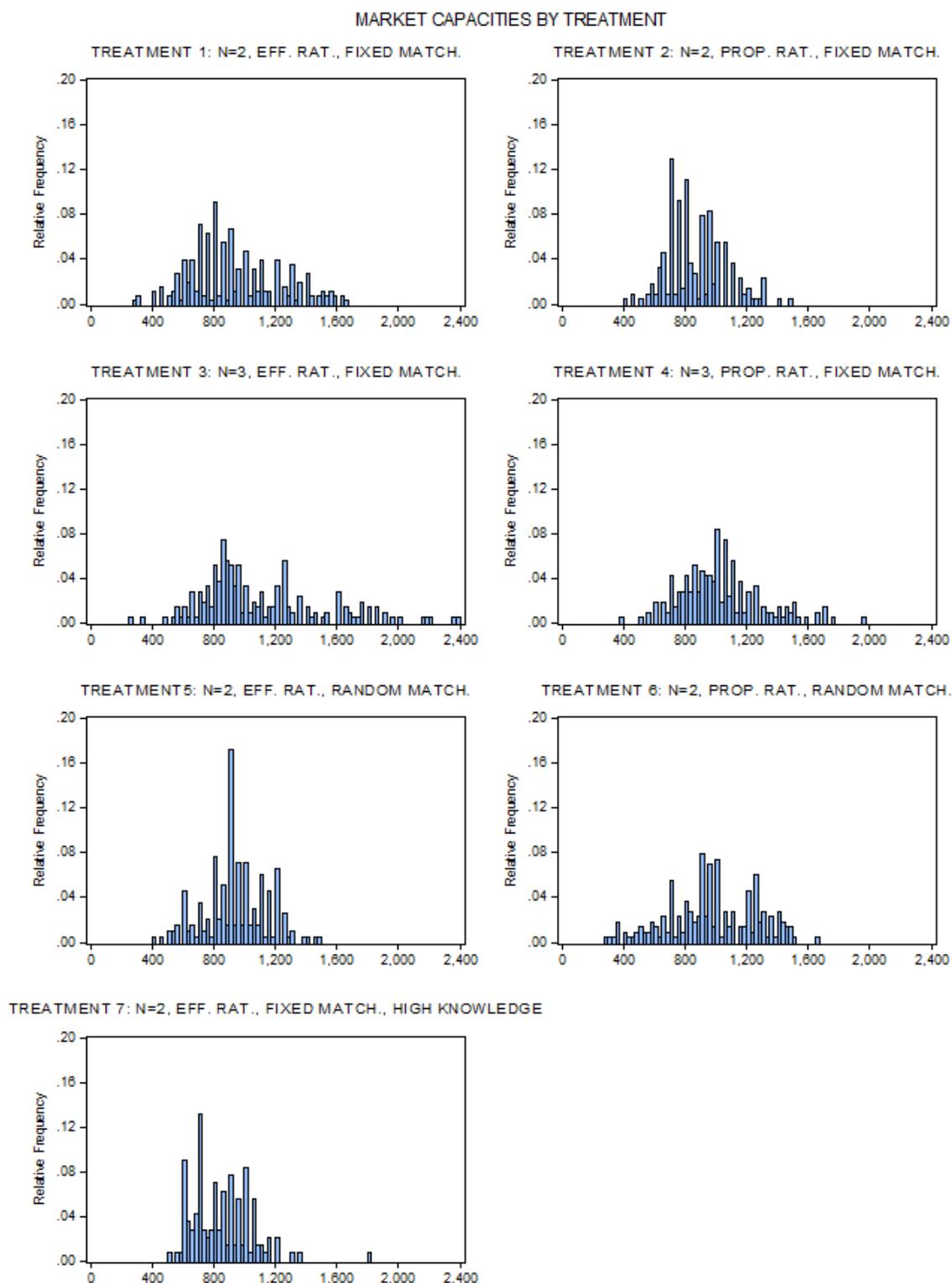


Figure 4: Distributions of market capacities.

To investigate those observations formally, we set up a regression that originates from Noussair et al. (1995) and has been applied to KS experiments by Davis (1999), Le Coq and Sturluson (2012), and Lepore and Shafran (2013). It has the following form:

$$K_{ht} = \sum_{j=1}^7 \left(\beta_j TD_j \frac{1}{t} + \gamma_j TD_j \frac{t-1}{t} \right) + \varepsilon_{ht}.$$

The dependent variable in the regression is the market capacity K in market h and period t . By TD_j , $j = 1, \dots, 7$, we denote a set of treatment dummy variables, where TD_j equals one in treatment j and zero else. Interacted with two different period weights, each treatment dummy appears twice on the right hand side of the equation. The first period weight, $1/t$, equals one in the first period and then declines whereas the second weight, $(t-1)/t$, equals zero in the first period and then increases, approaching one as t goes to infinity. The two weights sum up to the value of one in each period. Consequently, the β_j coefficients estimate market capacities in the first period (starting points) and the γ_j coefficients estimate the long-run market capacity convergence levels in treatment j . The regression thus allows for different convergence patterns in each treatment.¹²

We ran a generalized least squares (GLS) regression that corrected the standard errors for autocorrelation and heteroskedasticity within markets across periods. Regression results are presented in Panel A of Table 2. In all treatments market capacities converge to levels significantly larger than the respective Cournot capacity (all p 's < 0.01 , t -tests).¹³ Convergence levels are rather close to 900, which is the competitive quantity. Indeed, in five of the seven treatments the convergence level is not significantly different from 900 at the ten percent level. Only in the high-knowledge treatment 7 do market capacities converge to a level significantly lower than the competitive quantity ($p < 0.05$). The convergence level in treatment 3 is significantly larger than 900, but this result hinges upon the inclusion of markets with bankruptcies. The low R^2 of the regression indicates that there is a lot of variance in the capacity choice data which cannot be captured by treatment-specific convergence processes.¹⁴

In order to elicit treatment effects, we compare the coefficients of two treatments which differ only with respect to one treatment variable. Recall the treatment matrix in Figure 1 to see that there are three pairwise comparisons to investigate the effect of demand rationing (treatment 1 vs. treatment 2, 3 vs. 4, and 5 vs. 6), two for the number of firms in the market (1 vs. 3 and 2 vs. 4), two for the subject matching rule (1 vs. 5 and 2 vs. 6), and one for the knowledge condition (1 vs. 7). We consider effects in both the starting points and the

¹² Unlike other specifications in the literature, we do not allow for different starting levels for each session or market within the same treatment.

¹³ All p -values refer to two-tailed tests.

¹⁴ Alternative specifications we tried yielded identical implications and even lower R^2 's.

Panel A: Estimated Starting Points (β 's) and Convergence Levels (γ 's) of Market Capacities

	β_j	γ_j
Treatment 1 <i>n</i> = 2, eff. rat., fixed match.	1,063.578 (67.474)	866.707 (36.111)
Treatment 2 <i>n</i> = 2, prop. rat., fixed match.	984.21 (72.880)	841.501 (39.004)
Treatment 3 <i>n</i> = 3, eff. rat., fixed match.	1,536.977 (72.880)	976.892 (39.004)
Treatment 4 <i>n</i> = 3, prop. rat., fixed match.	1,208.245 (72.880)	935.012 (39.004)
Treatment 5 <i>n</i> = 2, eff. rat., random match.	981.549 (76.121)	911.532 (40.739)
Treatment 6 <i>n</i> = 2, prop. rat., random match.	1,073.534 (72.880)	929.163 (39.004)
Treatment 7 <i>n</i> = 2, eff. rat., fixed match., high knowledge	864.64 (61.661)	813.966 (40.883)
Observations	1,458	
Adjusted R ²	0.064	
GLS regression with clustered errors on the market level. Clustered standard errors in parentheses.		

Panel B: Treatment Effects

<i>demand rationing</i>	$\beta_1 = \beta_2$	$\gamma_1 = \gamma_2$
	$\beta_3 >^{***} \beta_4$	$\gamma_3 = \gamma_4$
	$\beta_5 = \beta_6$	$\gamma_5 = \gamma_6$
<i>number of firms in the market</i>	$\beta_1 <^{***} \beta_3$	$\gamma_1 <^{**} \gamma_3$
	$\beta_2 <^{**} \beta_4$	$\gamma_2 <^* \gamma_4$
<i>subject matching</i>	$\beta_1 = \beta_5$	$\gamma_1 = \gamma_5$
	$\beta_2 = \beta_6$	$\gamma_2 = \gamma_6$
<i>knowledge</i>	$\beta_1 >^{**} \beta_7$	$\gamma_1 = \gamma_7$
Results of two-tailed <i>t</i> -tests on equality of coefficients from Panel A. ">" ("<<") indicates that the first coefficient is significantly larger (smaller) than the second, where ***, **, and * denote the 1, 5, and 10 percent significance levels. "=" indicates that equality of the two coefficients cannot be rejected at the 10 percent significance level.		

Table 2: Market capacities: Regression results (Panel A) and treatment effects (Panel B).

convergence levels. The significance test results are presented in Panel B of Table 2. As to demand rationing and subject matching, the test results show no valid treatment effects. There is a significant effect of demand rationing on starting point capacities in only one of three pairwise comparisons, and no significant effect on capacity convergence levels. Subject matching never entails a significant effect. With respect to the knowledge condition, the visual impression from Figure 3 is confirmed by the test results. The high-knowledge

treatment yields significantly lower market capacities than the control treatment at the start, but the two convergence levels do not significantly differ from each other. The number of firms in the market is the only treatment variable in our setting which entails a uniform and persistent significant effect on capacities. Market capacities are significantly larger in triopoly than in duopoly in both pairwise comparisons and for both starting points and convergence levels.¹⁵

Our finding of capacities substantially above the Cournot prediction is in line with the observations in the majority of KS experiments. The results of earlier studies indicate that some features of our setting, namely inexperienced subjects (Muren, 2000; Goodwin and Mestelman, 2010; Le Coq and Sturluson, 2012) and low costs for installing capacity (Lepore and Shafran, 2013), are conducive to high capacity choices. Thus the high capacity levels observed in our experiment do not come out as a big surprise. As to the treatment effects, only two of our four treatment variables have already been varied in related studies. Our finding that demand rationing does not significantly affect capacity choices is in line with the result by Lepore and Shafran (2013). Yet, the significantly higher capacities observed in triopolies stand in contrast to the result by Brandts and Guillen (2007) who do not find such an effect.

Figure 4 already indicates that in all treatments capacities are higher than the Cournot benchmark in the vast majority of markets. Table 3 shows the number of instances in which the exact Cournot capacity was installed. Such instances are rare; their average frequency across all treatments is 1.8 percent.¹⁶ In the two triopoly treatments there is not a single instance in which the market capacity equals the Cournot benchmark. Apart from the fact that coordination on the Nash equilibrium becomes more difficult with more players, another important reason for this may be a prominent number effect. The Cournot market capacity in duopoly is 600 and requires each player to set a capacity of 300 if capacities are shared equally. In triopoly, by contrast, each player has to choose a capacity of 225, a less prominent

¹⁵ As an alternative approach to investigate treatment effects, we also applied nonparametric significance tests on market capacity data from different treatments. In the treatments with fixed subject matching, mean capacities in a market over all periods are taken as observations while in the treatments with random subject matching the data from single periods are pooled. Pairwise comparing market capacities in two treatments as described above, Mann-Whitney tests yield results largely identical to those in Panel B of Table 2. Demand rationing and subject matching never affect capacity choices significantly. Capacities are significantly larger in triopoly than in duopoly ($p < 0.05$ for treatment 2 vs. treatment 4 and $p < 0.1$ for treatment 1 vs. treatment 3). High knowledge does not significantly decrease mean market capacities over all periods (1-9), although there is a significant difference in four single periods (periods 2-5, all p 's < 0.1).

¹⁶ Lepore and Shafran (2013) also find that Cournot capacities are relatively rare in their low-cost duopoly treatments. They observe frequencies of *individual* Cournot capacities between 7 and 9 percent. However, those frequencies cannot readily be compared to ours. Lepore and Shafran count instances where individuals choose the individual Cournot capacity while we count instances where the sum of all individual capacity choices in a market equals the Cournot market capacity. Moreover, they employ a much coarser capacity grid than we do, which renders observations of the exact Cournot capacity more likely in their setting.

number than 300, to bring about the Cournot market capacity of 675. Indeed, if we consider individual capacity choices, we do not observe any choices of 225 in the triopoly treatments either. The modal individual capacity choice is 400 in all duopoly treatments with an average frequency of 17.0 percent and 300 in the two triopoly treatments with an average frequency of 13.6 percent. Hence, subjects do not only choose higher than Cournot capacities, but also concentrate their choices on prominent numbers.

Frequencies of Exact Cournot Market Capacities by Treatment	
Treatment 1	0.032
<i>n</i> = 2, eff. rat., fixed match.	(8/252)
Treatment 2	0.009
<i>n</i> = 2, prop. rat., fixed match.	(2/216)
Treatment 3	0.000
<i>n</i> = 3, eff. rat., fixed match.	(0/216)
Treatment 4	0.000
<i>n</i> = 3, prop. rat., fixed match.	(0/216)
Treatment 5	0.030
<i>n</i> = 2, eff. rat., random match.	(6/198)
Treatment 6	0.014
<i>n</i> = 2, prop. rat., random match.	(3/216)
Treatment 7	0.049
<i>n</i> = 2, eff. rat., fixed match., high knowledge	(7/144)
Treatments 1-7	0.018
	(26/1458)

Table 3: Frequencies of Cournot capacities.

While the overall level of Cournot capacity choices is very low, Cournot capacities occur significantly more frequently in those treatments where the Cournot outcome is the Nash equilibrium (treatments 1, 3, 5, and 7; those treatments with efficient demand rationing) than in those where it is not (treatments 2, 4, and 6; those treatments with proportional demand rationing). In the four treatments where the Cournot outcome is the theoretical prediction, the frequency of exact Cournot market capacities is 2.6 percent whereas in the other treatments it is 0.8 percent. The hypothesis that the frequency is independent of the equilibrium prediction is rejected by a chi-squared test at the one percent significance level.¹⁷

¹⁷ One may argue that the high knowledge treatment condition rather than demand rationing influences the frequency of Cournot capacities. The sample for the test is then biased because there is no counterpart treatment

6.3 Price choices

We now look at price choices at the second stage. As measure for the market price we take the average price in a given market and period weighted by sales quantities, i.e. total sales revenue divided by total sales quantity. Both Table 4 and Figure 5 show the development of market prices across periods within each treatment. As at the first stage, we observe a much more competitive conduct than predicted by the Cournot benchmark, i.e. prices are substantially lower than the Cournot price of 400 (in duopoly) or, respectively, 325 (in triopoly). Prices are lowest in the triopoly treatments (3 and 4) and highest in the duopoly treatments with proportional rationing (2 and 6) as well as in the high-knowledge treatment (7). Prices increase somewhat throughout the experiment in most treatments.

Market Prices				
	<i>periods</i>	<i>1-18</i>	<i>1-9</i>	<i>10-18</i>
Treatment 1		248.159	224.006	272.312
<i>n</i> = 2, eff. rat., fixed match.		(104.638)	(97.497)	(106.322)
		[242.857]	[205.044]	[276.188]
Treatment 2		310.254	297.457	323.052
<i>n</i> = 2, prop. rat., fixed match.		(90.772)	(89.703)	(90.436)
		[307.689]	[292.861]	[328.204]
Treatment 3		171.095	160.864	181.327
<i>n</i> = 3, eff. rat., fixed match.		(86.306)	(85.707)	(86.081)
		[148.443]	[131.391]	[160.987]
Treatment 4		214.815	201.467	228.162
<i>n</i> = 3, prop. rat., fixed match.		(82.141)	(78.063)	(84.284)
		[204.711]	[188.537]	[216.192]
Treatment 5		239.689	231.712	247.667
<i>n</i> = 2, eff. rat., random match.		(93.422)	(83.414)	(102.264)
		[223.333]	[218.200]	[229.772]
Treatment 6		296.431	263.900	328.963
<i>n</i> = 2, prop. rat., random match.		(146.042)	(127.253)	(156.608)
		[266.987]	[241.120]	[307.317]
Treatment 7		n.a.	277.258	n.a.
<i>n</i> = 2, eff. rat., fixed match., high knowledge			(80.407)	
			[270.272]	

Standard errors in parentheses. Medians in square brackets.

Table 4: Market prices: Summary statistics.

In a first step, we analyze the impact of the treatment conditions on market prices without taking into account the influence of chosen capacities on pricing decisions. We perform a

of treatment 7 with proportional rationing. However, when the data from treatment 7 are excluded from the sample, the test result is still significant at the five percent level.

regression analogous to that explained in section 6.2 (the only change being the replacement of the endogenous variable) and again inquire the effect of the treatment variables by comparing treatments pairwise. The results are presented in column (1) of Panel B of Table 5. Concentrating our attention on the effects on the estimated convergence levels (the γ coefficients), we obtain clear-cut results. The convergence price level is significantly lower under efficient than under proportional demand rationing, and lower in triopoly than in duopoly. Subject matching does not entail a significant effect on prices. The high-knowledge condition yields higher prices in the first periods, but fails to produce a significant effect on the convergence price level.

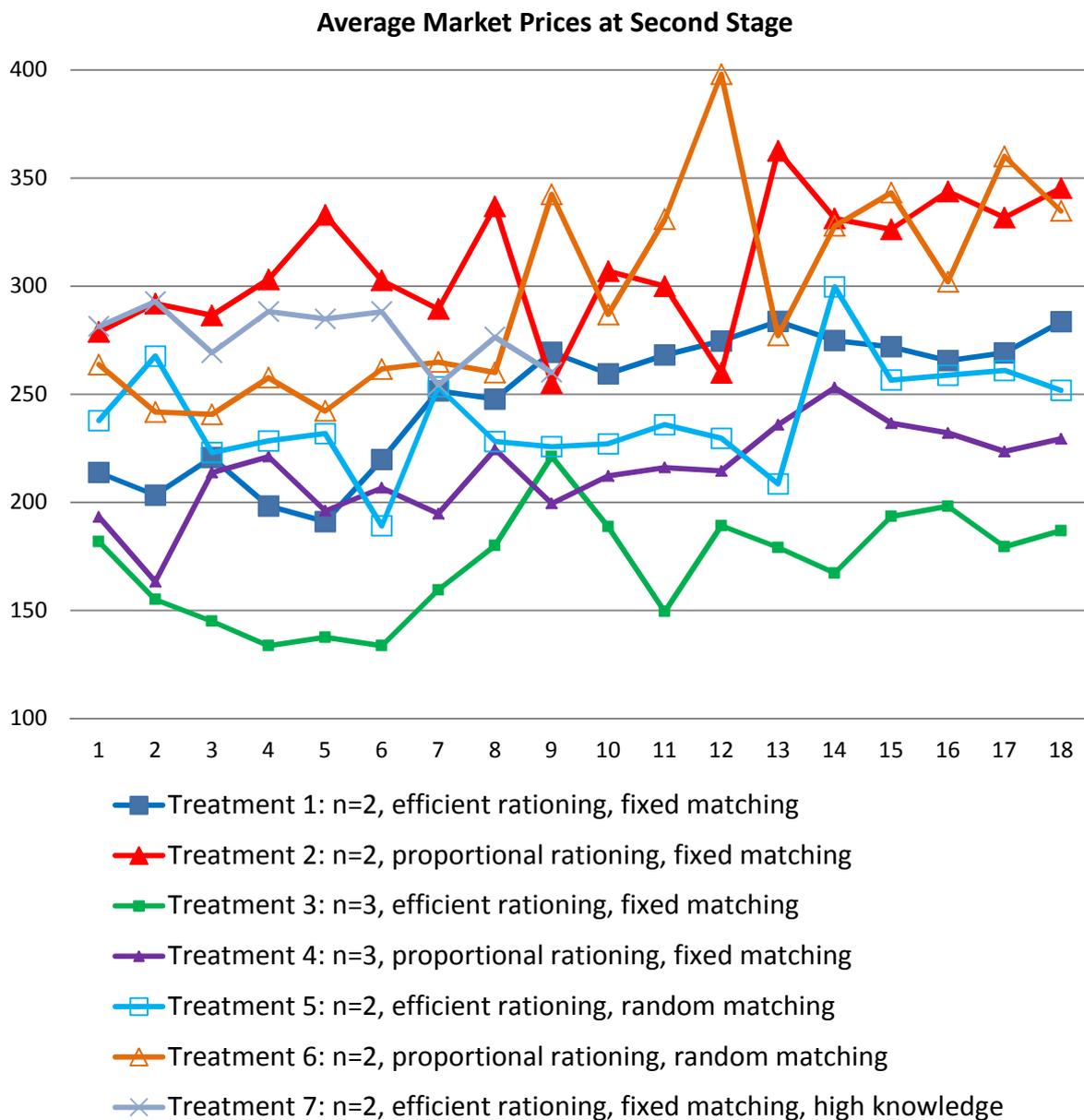


Figure 5: Series of market prices.

Panel A: Estimated Starting Points (β 's) and Convergence Levels (γ 's) of Market Prices

	(1)		(2)		(3)	
	β_j	γ_j	β_j	γ_j	β_j	γ_j
Treatment 1	203.122	247.597	389.191	394.011	663.495	670.772
<i>n</i> = 2, eff. rat., fixed match.	(19.844)	(12.353)	(17.486)	(8.159)	(21.904)	(15.502)
Treatment 2	281.190	321.223	449.882	445.892	712.719	723.044
<i>n</i> = 2, prop. rat., fixed match.	(21.434)	(13.343)	(18.654)	(8.743)	(22.689)	(15.924)
Treatment 3	177.785	157.158	402.973	323.999	695.264	600.183
<i>n</i> = 3, eff. rat., fixed match.	(21.434)	(13.343)	(19.430)	(8.846)	(22.012)	(15.869)
Treatment 4	192.367	219.178	392.318	386.299	673.652	663.031
<i>n</i> = 3, prop. rat., fixed match.	(21.434)	(13.343)	(18.732)	(8.953)	(22.650)	(16.550)
Treatment 5	237.739	234.718	411.669	385.860	686.126	664.511
<i>n</i> = 2, eff. rat., random match.	(22.388)	(13.936)	(19.283)	(8.953)	(23.228)	(16.534)
Treatment 6	250.775	298.103	435.323	456.654	719.704	731.143
<i>n</i> = 2, prop. rat., random match.	(21.434)	(13.343)	(18.651)	(8.448)	(22.782)	(15.868)
Treatment 7	279.638	277.809	411.885	400.823	691.094	677.005
<i>n</i> = 2, eff. rat., fixed match., high knowledge	(18.718)	(15.389)	(15.742)	(11.731)	(20.343)	(17.593)
cap(600,800]			-97.359			
			(6.722)			
cap(800,1000]			-154.071			
			(6.591)			
cap(1000,1200]			-212.236			
			(7.106)			
cap(1200,1400]			-236.052			
			(8.110)			
cap(1400,1600]			-265.027			
			(10.454)			
cap(1600,...]			-235.971			
			(13.266)			
cap					-0.653	
					(0.028)	
cap ²					1.95*10 ⁻⁴	
					(1.29*10 ⁻⁵)	
Observations	1,458		1,458		1,458	
Adjusted R ²	0.086		0.561		0.577	

GLS regressions with clustered errors on the market level. Clustered standard errors in parentheses.

...

Panel B: Treatment Effects

	(1)	(2)	(3)	
<i>demand rationing</i>	$\beta_1 <^{***} \beta_2$ $\beta_3 = \beta_4$ $\beta_5 = \beta_6$	$\gamma_1 <^{***} \gamma_2$ $\gamma_3 <^{***} \gamma_4$ $\gamma_5 <^{***} \gamma_6$	$\beta_1 <^{**} \beta_2$ $\beta_3 = \beta_4$ $\beta_5 = \beta_6$	$\gamma_1 <^{***} \gamma_2$ $\gamma_3 <^{***} \gamma_4$ $\gamma_5 <^{***} \gamma_6$
<i>number of firms in the market</i>	$\beta_1 = \beta_3$ $\beta_2 >^{***} \beta_4$	$\gamma_1 >^{***} \gamma_3$ $\gamma_2 >^{***} \gamma_4$	$\beta_1 = \beta_3$ $\beta_2 >^{**} \beta_4$	$\gamma_1 >^{***} \gamma_3$ $\gamma_2 >^{***} \gamma_4$
<i>subject matching</i>	$\beta_1 = \beta_5$ $\beta_2 = \beta_6$	$\gamma_1 = \gamma_5$ $\gamma_2 = \gamma_6$	$\beta_1 = \beta_5$ $\beta_2 = \beta_6$	$\gamma_1 = \gamma_5$ $\gamma_2 = \gamma_6$
<i>knowledge</i>	$\beta_1 <^{***} \beta_7$	$\gamma_1 = \gamma_7$	$\beta_1 = \beta_7$	$\gamma_1 = \gamma_7$

Results of two-tailed t-tests on equality of coefficients from Panel A. ">" ("<") indicates that the first coefficient is significantly larger (smaller) than the second, where ***, **, and * denote the 1, 5, and 10 percent significance levels. "=" indicates that equality of the two coefficients cannot be rejected at the 10 percent significance level.

Table 5: Market prices: Regression results (Panel A) and treatment effects (Panel B).

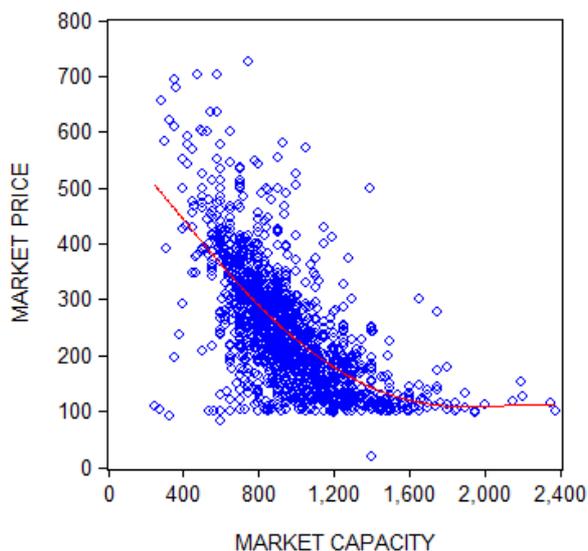


Figure 6: Scatter plot of market capacities and market prices with kernel fit.

In a second step, we add market capacity as explanatory factor to the model. The scatter plot in Figure 6 illustrates the relationship between market capacity determined at the first stage and market price determined at the second. Each circle shows the market capacity and the average market price in one specific market and period. As one expects, higher capacities involve lower prices. The plot also illustrates that subjects are very reluctant to set a price below 100 at the second stage irrespective of the installed capacity, presumably in order to avoid a sure loss. Regressions (2) and (3) in Panel A of Table 5 amend regression (1) by including terms for the installed capacity at the first stage. Specification (2) includes a set of

six capacity dummies. A capacity dummy equals one if the installed market capacity lies in the interval mentioned in its caption.¹⁸ Capacities up to 600 units are the omitted category, i.e. the β and γ coefficient estimates pertain to markets with a capacity of no more than 600 units and the dummy coefficients show the change in market price relative to that capacity interval. The estimates for the dummy coefficients are all negative and increase in absolute value, signifying a monotone negative relationship between capacity and price. The only exception is the sixth dummy coefficient for capacities larger than 1600 units which is smaller in absolute value than the fifth dummy coefficient. This, however, accords with the kernel fit line in Figure 6 indicating that the negative relationship between capacity and price is broken when capacities become very high.¹⁹

Specification (3) builds on the insight from Figure 6 indicating a convex relationship between capacity and price. It includes capacity as an exogenous factor with both a linear and a quadratic term. The regression yields a significant negative coefficient for the linear term and a significant positive coefficient for the quadratic term, thus confirming the supposed convex link. The magnitude of the coefficient estimates suggests that the first unit of installed capacity decreases the market price by approximately 0.65 ECU. If there are already 500 (1000) units of capacity installed, one further unit leads to a price decrease of about 0.46 (0.26) ECU.^{20,21}

The inclusion of capacity as explaining factor increases the share of explained variance in the regression more than sixfold; specification (3) has a slightly better fit than specification (2). Nevertheless, the inclusion of capacity does not change the implications with respect to treatment effects, as the significance test results in Panel B of Table 5 reveal. If one controls for the level of capacity installed at the first stage, price convergence levels still are significantly higher with proportional than with efficient demand rationing and also in duopoly than in triopoly. By contrast, neither subject matching nor the knowledge condition

¹⁸ Interval boundaries were chosen such that there are at least 20 observations in every capacity category. Variations in this respect leave the implications of the results unchanged.

¹⁹ All kernel fits in this study were generated with a Gaussian kernel and a bandwidth of 15 percent of the respective capacity range.

²⁰ As an artifact of the regression, a price-increasing effect of further capacity is implied if capacity exceeds 1673 units. However, this result is of little relevance since capacities are larger than this level in less than two percent of our observations.

²¹ As the regression contains no general constant, but a full set of treatment dummies without an omitted category, the β and γ coefficients function as treatment-specific constants and indicate the starting point and long-run price levels in the respective treatment when all other variables take values of zero. Hence, in regression (3) the estimates of the β and γ coefficients pertain to price levels in the respective treatment in a hypothetical state with zero capacity.

entails a significant effect. The two significant effects on pricing are in line with the results by Lepore and Shafran (2013) and Brandts and Guillen (2007).

The high capacities observed in the early periods of the experiment suggest that some subjects in the beginning fail to appreciate the impact of their capacity choice on the pricing subgame at the second stage. Le Coq and Sturluson (2012) find that the sensitivity of pricing decisions to installed capacity is higher in later periods. In order to investigate this matter in our setting, we set up two further regressions. The first, specification (4), has the form

$$P_{ht} = \sum_{j=1}^7 \left(\beta_j TD_j \frac{1}{t} + \gamma_j TD_j \frac{t-1}{t} \right) + \delta K_{ht} \frac{1}{t} + \zeta K_{ht} \frac{t-1}{t} + \varepsilon_{ht}.$$

The dependent variable is the market price P in market h and period t . The term in the brackets estimates treatment-specific price starting points and convergence levels, as known from the above regressions. In two additional terms the installed market capacity K_{ht} is interacted with period weights. The δ coefficient estimates the sensitivity of pricing to capacity in the first period while the ζ estimates the sensitivity in the long run (when t goes to infinity). We expect capacity to have a price-decreasing effect in any period, i.e. both δ and ζ should be negative. Moreover, the sensitivity should increase over periods when more subjects learn about the effect of their choices at the first stage, i.e. ζ should be smaller (larger in absolute value) than δ . The results presented in Table 6 confirm these suppositions.²² Both δ and ζ are significantly negative and ζ is significantly smaller than δ at the one percent level (t -test).

In specification (5) capacity is interacted with both period weights and treatment dummies TD_j . We thus can consider the sensitivity of pricing to capacity on the treatment level. The regression equation is

$$P_{ht} = \sum_{j=1}^7 \left(\beta_j TD_j \frac{1}{t} + \gamma_j TD_j \frac{t-1}{t} + \delta_j K_{ht} TD_j \frac{1}{t} + \zeta_j K_{ht} TD_j \frac{t-1}{t} \right) + \varepsilon_{ht}.$$

The regression results in Table 6 show that the δ coefficient is not significantly different from zero in four of the seven treatments (treatments 1, 2, 3, and 5). That is, we cannot reject the null hypothesis that subjects choose prices while completely ignoring installed capacities in the first period of those treatments. In treatments 4 and 6 installed capacity significantly decreases prices, but the effect is rather small. Only in treatment 7 do we find a significant and sizable negative effect. In contrast, in the long run higher capacities lead to lower prices

²² As they are not in the focus of our interest in these regressions, the estimates for the β and γ coefficients are omitted in Table 6.

in all treatments as all ζ coefficients are negative and significant. The sensitivity of pricing to capacity significantly increases from early to later periods in five treatments (null hypothesis $\zeta_j = \delta_j$ rejected in favor of $\zeta_j < \delta_j$, $p < 0.01$, t -tests). The two exceptions are treatment 3, where the sensitivity is still small in the long run, and treatment 7, where the sensitivity is large from the beginning. It appears that the extensive instructions in the high-knowledge treatment have been successful in teaching the subjects the connection between capacity and price choices *ex ante*, while many subjects in the other treatments had to learn about that connection by experience.

Sensitivity of Pricing to Installed Market Capacity				
	(4)		(5)	
	δ	ζ	δ_j	ζ_j
	-0.109***	-0.280***		
	(0.022)	(0.009)		
Treatment 1			-0.087	-0.286***
<i>n</i> = 2, eff. rat., fixed match.			(0.055)	(0.020)
Treatment 2			0.021	-0.284***
<i>n</i> = 2, prop. rat., fixed match.			(0.065)	(0.032)
Treatment 3			-0.023	-0.085***
<i>n</i> = 3, eff. rat., fixed match.			(0.038)	(0.016)
Treatment 4			-0.114**	-0.260***
<i>n</i> = 3, prop. rat., fixed match.			(0.045)	(0.024)
Treatment 5			-0.028	-0.419***
<i>n</i> = 2, eff. rat., random match.			(0.076)	(0.028)
Treatment 6			-0.165***	-0.430***
<i>n</i> = 2, prop. rat., random match.			(0.057)	(0.017)
Treatment 7			-0.369***	-0.329***
<i>n</i> = 2, eff. rat., fixed match., high knowledge			(0.059)	(0.037)
Observations	1,458		1,458	
Adjusted R ²	0.512		0.607	

GLS regressions with clustered errors on the market level. Clustered standard errors in parentheses. ***, **, and * denote significance at the 1, 5, and 10 percent level.

Table 6: Sensitivity of pricing to capacity: Regression results.

We now compare our pricing data to the Nash equilibrium prediction in the pricing subgame given capacity choices. A testable theory prediction is available only if individual capacities k_i are so small that market-clearing pricing is the pure-strategy equilibrium of the second-stage subgame. If, in contrast, chosen capacities at the first stage are too large, the equilibrium strategies involve randomization over prices. The conditions for market-clearing pricing to be the equilibrium of the second-stage subgame depend on both the demand

rationing scheme and the number of firms in the market. Under efficient rationing, in a market with n firms there are n conditions which must simultaneously hold. In our setting those conditions are

$$k_i \leq 500 - \sum_{j \neq i} k_j / 2, i = 1, \dots, n.$$

Under proportional rationing the condition is that the sum of individual capacities must not be greater than 500,

$$\sum_i k_i \leq 500.^{23}$$

It is immediate that the condition under proportional rationing is more restrictive; that is, the region of capacity vectors for which market-clearing pricing is the equilibrium under proportional rationing is a real subset of the respective region under efficient rationing.²⁴

The data in the first column of Table 7 show how often subjects at the first stage choose capacities that entail market-clearing pricing as the equilibrium of the second-stage subgame. Averaged over all treatments, such capacity vectors are chosen in only 8.2 percent of all instances. Pricing subgames with a pure-strategy equilibrium are especially rare in the treatments with proportional rationing (3.2 percent).

In the further analysis we consider only those subgames where market-clearing pricing is the pure-strategy equilibrium and investigate whether markets actually clear at the second stage and whether subjects choose the exact market-clearing Cournot price of $1000 - \sum_i k_i$. The results are displayed in the remaining columns of Table 7. We observe that market clearing occurs only in somewhat less than half of the instances where it is predicted. Nevertheless, the theoretical equilibrium possesses some predictive power since market clearing occurs in less than five percent of those cases where it is not predicted by first-stage capacities (data not reported). Our result stands in contrast to Lepore and Shafran (2013) who find that their KS markets clear in roughly 90 percent of those cases where it is predicted.

²³ See Lepore (2009) for an extensive treatment of these conditions.

²⁴ Iskakov and Iskakov (2014) apply their concept of equilibrium in secure strategies to capacity-constrained price competition. Equilibrium in secure strategies is a broad pure-strategy equilibrium concept which contains Nash equilibrium in pure strategies as a special case, i.e. every pure-strategy Nash equilibrium is also an equilibrium in secure strategies. The conditions for a strategy bundle to be an equilibrium in secure strategies are that (i) no player i can impose a threat to another player j , i.e. i cannot change her strategy such that i 's payoff increases and j 's payoff decreases and (ii) no player can profitably and securely deviate from the given strategy bundle, where a deviation is secure if the execution of any threats the deviating player exposes herself to by her deviation cannot reduce her payoff below her initial payoff level. Using this equilibrium concept, Iskakov and Iskakov show that under proportional rationing an equilibrium in secure strategies exists if capacities fulfill the conditions given above for *efficient* rationing and that the equilibrium implies market-clearing pricing. While they consider only proportional rationing, their arguments go through also in the case of efficient rationing. Hence, interestingly, they provide an equilibrium concept where the conditions for existence of a pure-strategy equilibrium do not depend on demand rationing.

However, most of their observations stem from their high-cost treatments and the few observations from their low-cost treatments comparable to ours show substantially lower rates. Muren's (2000) KS markets clear in 50 to 60 percent of those cases where it is predicted, which is reasonably close to our rate of 46.2 percent. As regards exact market-clearing pricing, we find that the respective price of $1000 - \sum_i k_i$ is set by a subject in 35.8 percent of those cases where this price is the pure strategy equilibrium.^{25,26} Considering this issue on the market level, it occurs in 12.6 percent of the cases that the exact market-clearing price is set by all firms in the market.

	Market Clearing in the Pricing Subgame			
	<i>market-clearing pricing is Nash equilibrium</i>	<i>market cleared</i>	<i>exact market-clearing price chosen...</i>	
			<i>...on individual level</i>	<i>...by all subjects in the market</i>
Treatment 1	0.156	0.625	0.400	0.075
<i>n = 2, eff. rat., fixed match.</i>	(40/256)	(25/40)	(32/80)	(3/40)
Treatment 2	0.019	0.750	0.250	0.000
<i>n = 2, prop. rat., fixed match.</i>	(4/216)	(3/4)	(2/8)	(0/4)
Treatment 3	0.093	0.300	0.233	0.000
<i>n = 3, eff. rat., fixed match.</i>	(20/216)	(6/20)	(14/60)	(0/20)
Treatment 4	0.009	1.000	0.167	0.000
<i>n = 3, prop. rat., fixed match.</i>	(2/216)	(2/2)	(1/6)	(0/2)
Treatment 5	0.079	0.294	0.559	0.294
<i>n = 2, eff. rat., random match.</i>	(17/216)	(5/17)	(19/34)	(5/17)
Treatment 6	0.069	0.333	0.100	0.000
<i>n = 2, prop. rat., random match.</i>	(15/216)	(5/15)	(3/30)	(0/15)
Treatment 7	0.146	0.429	0.524	0.333
<i>n = 2, eff. rat., fixed match., high knowledge</i>	(21/144)	(9/21)	(22/42)	(7/21)
Treatments 1-7	0.082	0.462	0.358	0.126
	(119/1458)	(55/119)	(93/260)	(15/119)

Table 7: Market clearing in the pricing subgame.

²⁵ Here decisions on the individual level are analyzed. The number of analyzed cases is therefore given by the number of markets where market clearing is the equilibrium multiplied by the number of firms in each market.

²⁶ Davis (1999) finds that subjects in the initial periods of his KS treatment choose the market-clearing price in about 20 percent of those cases where market-clearing pricing is the equilibrium. This share increases throughout the experiment and circulates around 85 percent in the last periods. However, Davis' results are not directly comparable to ours for several reasons. The price grid Davis employs is coarser than ours. The maximum capacity each firm can install is smaller relative to market demand than in our setting, which renders capacity choices where market clearing is the equilibrium of the pricing subgame more likely. Subjects in Davis' study may therefore have more chances to learn the optimal pricing strategy in such a situation. Moreover, prices which are no more than two cents below the exact market-clearing price are considered by Davis as unsizable deviations from equilibrium and are included in the above shares.

Examining the data in the third column of Table 7, we notice that subjects in the treatments with efficient rationing have a higher propensity to choose the exact market-clearing Cournot price than subjects in the treatments with proportional rationing. The relative frequencies are 40.3 percent under efficient rationing and 13.6 percent under proportional rationing. A chi-squared test rejects the independence of frequencies from the rationing scheme at the one percent significance level.²⁷ This result parallels the finding from section 6.2 that under efficient rationing exact Cournot capacities are more likely to be installed at the first stage. Yet, whereas the influence of the rationing scheme at the first stage corresponds to the equilibrium prediction, at the second stage we find an unpredicted significant effect of the rationing scheme. Since we consider only those markets with a pure-strategy equilibrium in the pricing subgame, the equilibrium predictions are now identical under both rationing schemes. We can provide a somewhat speculative explanation for our finding at the second stage. Since capacities which entail market-clearing pricing as equilibrium at the second stage are chosen more rarely under proportional rationing, subjects in those treatments had less opportunity to experience market-clearing pricing to be optimal when capacities are sufficiently small. This explains the observed difference in pricing behavior if one assumes that experience is needed to understand how to price optimally. This is a plausible assumption in the rather complex KS framework and is also corroborated by our above finding that subjects need experience to appreciate the link between their decisions at the first and second stage.

6.4 Profits

After examining capacity and price choices separately, we now analyze how those choices interact and which profit levels they bring about. Table 8 provides summary statistics of profits per firm in each treatment. Although the individual profit data exhibit very high variances, it becomes clear that in all treatments profits are substantially lower than predicted by the respective Cournot outcomes.²⁸ Moreover, profits are somewhat greater under proportional than under efficient demand rationing. This contrasts with the theory prediction, but is in line with the results from the low-cost treatments by Lepore and Shafran (2013).

²⁷ This result remains valid if we exclude the data from treatment 7 following the argument given in fn. 17.

²⁸ Considering only the raw individual profit data in Table 8, due to the high variances the mean individual profits in the treatments are not significantly different from their respective Nash prediction. However, putting up a regression similar to that in section 6.2 with individual profit as endogenous variable, we find that in all treatments profit convergence levels are significantly smaller than the Nash equilibrium profit (all p 's < 0.01, t -tests).

Profits per Firm				
	<i>periods</i>	<i>1-18</i>	<i>1-9</i>	<i>10-18</i>
Treatment 1 <i>n</i> = 2, eff. rat., fixed match.		35,819.797 (43,860.007) [40,000.000]	26,553.708 (44,606.965) [25,100.000]	45,085.89 (41,144.603) [53,625.000]
Treatment 2 <i>n</i> = 2, prop. rat., fixed match.		60,001.518 (37,421.334) [61,953.100]	53,699.266 (39,506.239) [55,980.550]	66,303.77 (34,156.647) [67,556.500]
Treatment 3 <i>n</i> = 3, eff. rat., fixed match.		5,384.717 (29,451.782) [5,617.500]	-1,973.653 (31,908.973) [850.000]	12,743.09 (24,712.170) [12,000.000]
Treatment 4 <i>n</i> = 3, prop. rat., fixed match.		18,823.696 (30,791.982) [17,525.000]	14,784.639 (31,241.118) [14,623.050]	22,862.75 (29,841.103) [19,775.000]
Treatment 5 <i>n</i> = 2, eff. rat., random match.		34,128.109 (35,494.925) [35,070.500]	30,721.520 (36,747.108) [29,600.000]	37,534.70 (33,948.457) [38,557.500]
Treatment 6 <i>n</i> = 2, prop. rat., random match.		42,031.159 (51,634.314) [34,855.350]	34,501.973 (49,213.397) [32,242.300]	49,560.35 (52,997.567) [42,914.000]
Treatment 7 <i>n</i> = 2, eff. rat., fixed match., high knowledge		n.a.	49,834.674 (31,995.914) [52,399.500]	n.a.
Standard errors in parentheses. Medians in square brackets.				

Table 8: Profits per firm: Summary statistics.

It is a common finding in KS experiments that subjects earn considerably less than the theoretical equilibrium profits. This is apparently due to excessive capacity installation at the first stage which, on the one hand, entails costs for units that cannot be sold and, on the other hand, triggers low sales prices at the second stage. Since subjects choose such high capacities even after many rounds when they have gained experience, the question arises whether they draw some latent utility from that behavior.

Davis (1999) suggests that subjects may strategically set high capacities in order to bully the other players in their market so that their competitors install less capacity and leave a large share of the market to the bully. He argues that such attempts are often successful, referring to the strong positive correlation between capacity shares and profit shares within markets in his KS treatment data. While we find a similar correlation in our data,²⁹ we do not consider this to provide sufficient evidence for bullying to be strategically successful. After all, a larger

²⁹ Spearman's rank correlation coefficient between the capacity share and the profit share of a subject in her market, both averaged over all periods, is 0.56. The data of 14 subjects are disregarded in that calculation because the average market profit is negative in those cases.

capacity share leads to more market power and therefore is likely to induce a higher profit share within a market. To judge success, however, also performance across markets is to be considered. Davis acknowledges that average profits are lowest in those markets with the highest capacities.

The relationship between absolute individual capacities and absolute individual profits is suitable to investigate whether choosing high capacities is eventually a successful strategy. The results are clearly negative. Spearman's rank correlation coefficient between the mean individual capacity and the mean individual profit per period, averaged over all periods, is -0.47 in duopolies and -0.39 in triopolies. The scatter plots in Figure 7 allow for a more precise analysis. The kernel fits show that there exists an interior profit-maximizing capacity. According to the kernel regressions, the profit-maximizing capacity is 312 in duopoly and 274 in triopoly. The profit-maximizing capacity in duopoly is remarkably close to the Cournot capacity whereas in triopoly it is roughly 20 percent above Cournot. The corresponding profit is about 56,300 in duopoly and about 17,400 in triopoly. In other words, the profit that can be attained on average with an empirically optimal capacity choice is still considerably lower than the equilibrium profit. Furthermore, subjects on average set substantially higher capacities than optimal *ex post*. We therefore do not share Davis' view of excess capacities as successful bully behavior.

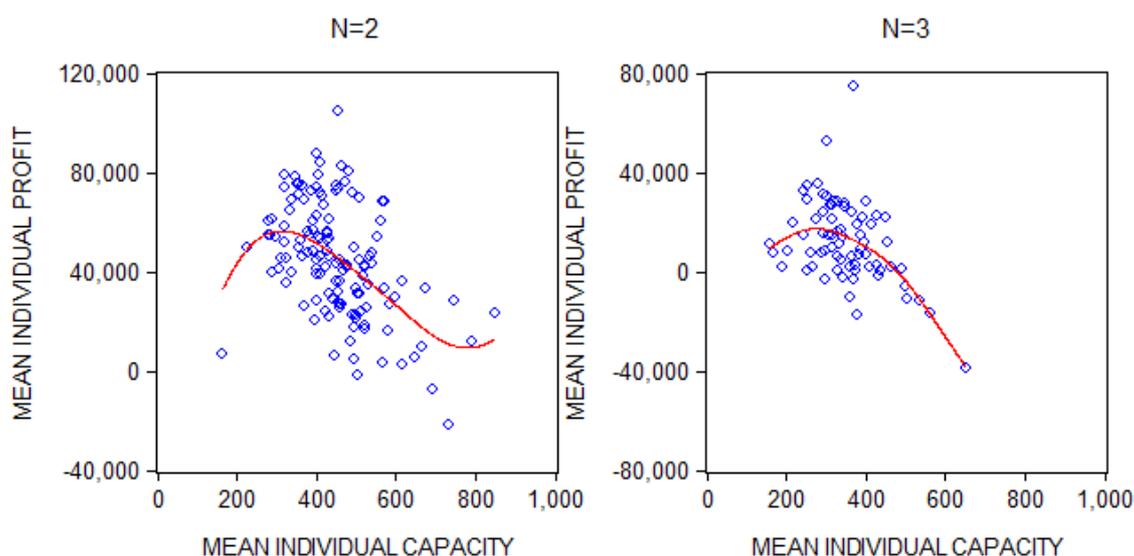


Figure 7: Scatter plots of individual capacities and individual profits with kernel fits.

Another explanation for excess capacities is put forward by Lepore and Shafran (2013). They argue that when the cost for installing capacity is low subjects lose little by choosing a

too high capacity that is not fully utilized at the second stage. However, high capacities bring about the chance of a large gain in case the other players happen to install low capacities. High capacity choices may thus be considered gambling behavior. The kernel fits in Figure 7 illustrate that on average subjects lose by gambling. Moreover, we find that high profits in one period do not correlate with high capacity choices. Table 9 categorizes individual capacity choices in single periods by profit. We see that the highest individual profits are actually realized at below-average individual capacities. High profits are apparently more triggered by implicit collusion than by exploiting opportunities induced by a high individual capacity.

Individual Capacities Categorized by Individual Profits		
	<i>n</i> = 2	<i>n</i> = 3
All	455.562 (166.869) [431.000]	349.674 (168.973) [310.000]
Top profit decile	438.000 (134.388) [400.000]	324.566 (99.762) [300.000]
Bottom profit decile	579.483 (195.877) [600.000]	551.369 (201.295) [500.000]

Standard errors in parentheses. Medians in square brackets.

Table 9: Individual capacities categorized by individual profits.

Our analysis to some extent questions the explanations for excess capacities in the literature since it shows that choosing high capacities is rarely fruitful, no matter whether one assumes a strategic or an opportunistic rationale behind such decisions. It is then an unresolved question why even in the long run subjects do not realize the failure of their tactics and commence to choose lower capacities.

7 Conclusion

This study adds to the so far limited literature of experimental Kreps-Scheinkman oligopoly markets by providing a comprehensive analysis on how different experimental conditions affect market outcomes. Seven treatments are conducted with different combinations of the four treatment variables: number of firms in a market, demand rationing, subject matching, and subjects' understanding of KS markets (referred to as "knowledge"). Theory predicts the

Cournot outcome under efficient demand rationing and a more competitive mixed-strategy equilibrium under proportional rationing.

We find that both capacity choice and pricing is considerably more competitive than the Cournot outcome in all treatments. Larger market capacities are chosen in triopoly than in duopoly. Subjects with high knowledge first set lower capacities, but the effect vanishes when low-knowledge subjects reduce their capacities after the initial rounds. Neither demand rationing nor subject matching affects capacity choice. At the pricing stage, both the number of firms and demand rationing entail persistent effects. Prices are higher in duopoly than in triopoly and with proportional than with efficient rationing. Again knowledge makes a difference only in the initial rounds. Investigating the sensitivity of price choices on installed capacity, it appears that subjects in the low-knowledge condition have to learn by experience about the relationship between capacity and price choices. Exact Cournot capacities as well as exact market-clearing pricing are more likely to occur under efficient than under proportional demand rationing. Subjects' earnings substantially fall short of equilibrium profits due to excessive capacity installation. It still remains a puzzle why subjects do not reduce capacities even after many rounds of experience.

Appendix A

English translation of the written instructions in treatments 3 and 7.

Treatment 3:

Welcome to the Laboratory for Economic Experiments at Kiel University.

You are about to participate in an economic experiment where you will have to make some decisions. You can also earn some money. The amount of money will depend on both your own decisions and the decisions of the other participants in the experiment.

Please read these instructions carefully. If after reading there are any questions, please raise your hand. An experimenter will approach you and answer your questions in private. Please do not communicate with other participants during the experiment.

All participants receive the same instructions.

In this experiment you represent a **firm** producing and selling a single good. On a market you **compete with two other firms** being represented by two of the other participants. All firms produce and sell the same good.

For your participation you will receive a fee of 6 Euro. You can earn further money during the experiment. The amount will depend on both your own decisions and the decisions of the other participants in the experiment.

Throughout the experiment, all money amounts will be accounted in **Experimental Currency Units (ECU)**. Your **start balance** at the beginning of the experiment is **240,000 ECU**. Your final balance at the end of the experiment will be paid off to you at an **exchange rate of 40,000 ECU/Euro**, i.e. for each 40,000 ECU of your final balance you will receive 1 Euro. Additionally, you will receive your participation fee.

The experiment consists of **18 periods**. At the beginning of the experiment, the participants will be randomly matched into groups of three. The other members of your group will be your competitors. The grouping will be fixed throughout the experiment.

Each **period** of the experiment **consists of two stages**:

First stage: At the first stage you and the other firms must simultaneously and independently **decide on your production quantity** of the good. All **integers from 0 up to 1000 are admissible** quantities.

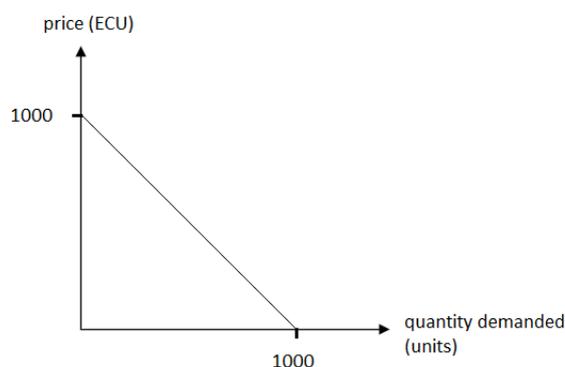
Production incurs **costs of 100 ECU per unit produced**. Your production costs will be subtracted from your balance.

After the first stage all firms will be informed about the production quantities of the other firms within their group.

Second stage: At the second stage you and the other firms must simultaneously and independently **decide on the price** at which to offer the good on the market. All **integers from 0 up to 1000 are admissible** prices.

Your sales revenue will be added to your balance. Your sales revenue is equal to the price chosen by you multiplied by your sales quantity. Your sales quantity depends on both the production quantities and the prices chosen by you and the other firms. In the following paragraphs you can find a precise description of how sales quantities are determined.

The market demand describes the quantity of the good that can at most be sold in each period at a given price. Demand will be simulated by the computer and will be identical in all periods. There are many different potential customers with different willingnesses to pay for the good. A sale will come about if the offer price is no higher than a customer's maximum willingness to pay. At a price of 0 ECU, there will be a total demand for 1000 units on the market. With a price increase by 1 ECU, demand is reduced by 1 unit. So, for example, at a price of 500 ECU, 500 units will be demanded. At a price of 1000 ECU no one will be ready to buy the good. Here is a graphical representation of this relationship:



Each firm in each period can sell at most as many units of the good at the second stage as it has produced at the first stage.

Customers will first try to purchase the good from the firm offering at the lowest price. Customers with the highest willingness to pay will be served first. In case several firms choose the same price, demand will be divided equally among them.

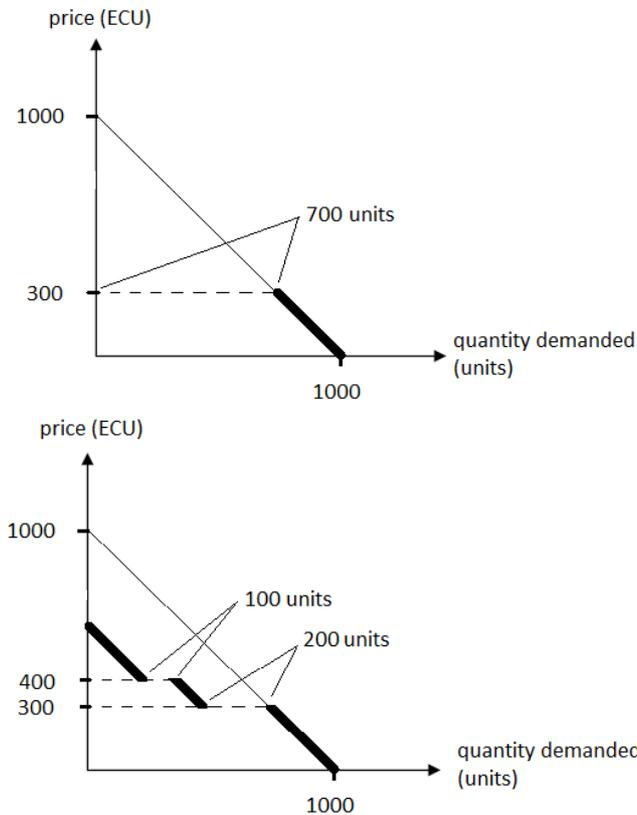
If a firm has not chosen the lowest price within its group, there may be some residual demand left over for it or not. This is illustrated by the following two examples. For both examples assume that firms 1 and 2 have chosen prices of 300 ECU and 400 ECU respectively. The production quantity of firm 1 be 700 units in the first example and 200 units in the second. The production quantity of firm 2 be 100 units in both cases.

We are interested in the remaining demand for firm 3 at different price choices of firm 3. In the figures below, the remaining demand for firm 3 is represented by the bold line.

If firm 3 chooses a price below 300 ECU, its price is the lowest and it is confronted with the entire market demand.

If firm 3 chooses a price above 300 ECU, firm 1 sells its units first. In the first example (upper figure), the production quantity of firm 1 satisfies the whole demand at this price, so the other firms are unable to sell any

units at prices above 300 ECU. In the second example (lower figure), however, the production quantity of firm 1 is not enough to satisfy all market demand. Therefore, a residual demand is left over at prices above 300 ECU. If firm 3 then chooses a price above 400 ECU, also firm 2 will sell its units previous to firm 3. In the example, firm 2 again cannot satisfy all remaining demand at a price of 400 ECU, so there is some residual demand left for firm 3 even at prices higher than 400 ECU.



In case a firm cannot sell its entire production from the first stage at the second stage, the units that could not be sold will forfeit and cannot be transferred to the next period. However, production costs are incurred for all units produced, no matter whether they can be sold or not.

Your **profit** or loss (in ECU) in a period is equal to your **sales revenue** (= offer price chosen by you multiplied by your sales quantity) **at the second stage minus** your **production costs** (= production quantity chosen by you multiplied by 100 ECU) **at the first stage**.

Your balance in each period is increased or decreased by your profit or loss respectively in this period. If your balance should become negative during the experiment, you can nevertheless go on producing and selling, i.e. your firm has a credit without limit in the experiment. If your final balance at the end of the experiment should be negative, you will only be paid your participation fee. Your participation fee will not be charged against any losses.

To get a general idea of your sales quantities and profits resulting from different price and quantity combinations, you may use the "**profit calculator**". Simply enter a hypothetical production quantity and a hypothetical offer price for each firm and click on "Calculate". Then your resulting sales quantity and profit will be displayed. You may use the profit calculator throughout the whole experiment.

Moreover, after each period a screen with a "**history**" will inform you about the outcomes so far. For all past periods the "history" shows the production quantities and prices chosen by the firms, your resulting sales quantity, your sales revenue, your production cost and your profit. Moreover, your current money balance will be displayed.

The following two screenshots illustrate the use of the profit calculator and the history:

Here you can enter three hypothetical production quantities and offer prices and have the results calculated.

Here you can enter and submit your choice for the current period (production quantity at the first stage, offer price at the second stage).

My production quantity	Other production quantity 1	Other production quantity 2	My price	Other price 1	Other price 2	My sales quantity	My profit
220	120	500	250	220	500	220	33000
220	120	500	400	220	500	220	60000
220	120	500	500	220	500	190	73000
220	120	500	550	220	500	0	-22000
220	305	500	300	220	299	0	-22000

The results of the profit calculator are displayed here.

Your total profit so far is displayed here.

The results of the last period are displayed here.

Period	My production quantity	Other production quantity 1	Other production quantity 2	My price	Other price 1	Other price 2	My sales quantity	My sales revenue	My cost	My profit
-2	0	0	0	0	0	0	0	0	0	0
-1	0	0	0	0	0	0	0	0	0	0
0	0	0	0	0	0	0	0	0	0	0
1	450	158	542	450	455	145	8	3600	45000	-41400
2	450	500	250	333	412	154	417	138861	45000	93861

This is the history showing the outcomes of all past periods. Zeros are displayed for trial periods.

To practice there will be **three trial periods** before the actual periods start. The procedures in the trial periods are the same as in the actual periods described above, but the outcomes of the trial periods will not add to your payoff. Your start balance for the trial periods is 100,000 ECU. After the trial periods your balance will be set to 240,000 ECU – your start balance for the actual periods – independent of your profits or losses in the trial periods. After the trial periods, the participants will be randomly re-matched into new groups. The new groups will stay fixed throughout all actual periods of the experiment. The profit calculator will be available to you during both trial and actual periods. The history, however, will not be displayed after trial periods and will only contain the data of the actual periods.

Your **final balance** will be **paid to you** after the experiment at the above-mentioned exchange rate. Additionally, you will receive your participation fee. None of the other participants will come to know your payoff, and neither will you come to know the payoff of any other participant. Furthermore, no participant will come to know with whom he or she interacted during the experiment.

If you click on the "Continue" button on your screen, some statements will appear to check whether all participants have understood the instructions. Please decide whether these statements are right or wrong. As soon as all participants will have evaluated the statements correctly, the first trial period will start.

If there are any questions concerning the experiment, please raise your hand. Enjoy the experiment!

Treatment 7:

Welcome to the Laboratory for Economic Experiments at Kiel University.

You are about to participate in an economic experiment where you will have to make some decisions. You can also earn some money. The amount of money will depend on both your own decisions and the decisions of the other participants in the experiment.

Please read these instructions carefully. If after reading there are any questions, please raise your hand. An experimenter will approach you and answer your questions in private. Please do not communicate with other participants during the experiment.

All participants receive the same instructions.

In this experiment you represent a **firm** producing and selling a single good. On a market you **compete with one other firm** being represented by one of the other participants. All firms produce and sell the same good.

For your participation you will receive a fee of 6 Euro. You can earn further money during the experiment. The amount will depend on both your own decisions and the decisions of the other participants in the experiment.

Throughout the experiment, all money amounts will be accounted in **Experimental Currency Units (ECU)**. Your **start balance** at the beginning of the experiment is **200,000 ECU**. Your final balance at the end of the experiment will be paid off to you at an **exchange rate of 40,000 ECU/Euro**, i.e. for each 40,000 ECU of your final balance you will receive 1 Euro. Additionally, you will receive your participation fee.

The experiment consists of **9 periods**. At the beginning of the experiment, the participants will be randomly matched into groups of two. The other member of your group will be your competitor. The grouping will be fixed throughout the experiment.

Each **period** of the experiment **consists of two stages**:

First stage: At the first stage you and the other firm must simultaneously and independently **decide on your production quantity** of the good. All **integers from 0 up to 1000 are admissible** quantities. Production incurs **costs of 100 ECU per unit produced**. Your production costs will be subtracted from your balance.

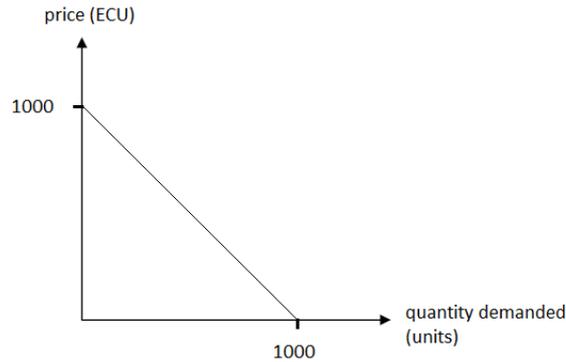
After the first stage all firms will be informed about the production quantity of the other firm within their group.

Second stage: At the second stage you and the other firm must simultaneously and independently **decide on the price** at which to offer the good on the market. All **integers from 0 up to 1000 are admissible** prices. Your sales revenue will be added to your balance. Your sales revenue is equal to the price chosen by you multiplied by your sales quantity. In the following paragraphs you can find a precise description of how sales quantities are determined.

The market demand describes the quantity of the good that can at most be sold in each period at a given price. Demand will be simulated by the computer and will be identical in all periods. The market demand function $MD(p)$ is given by

$$MD(p) = 1000 - p.$$

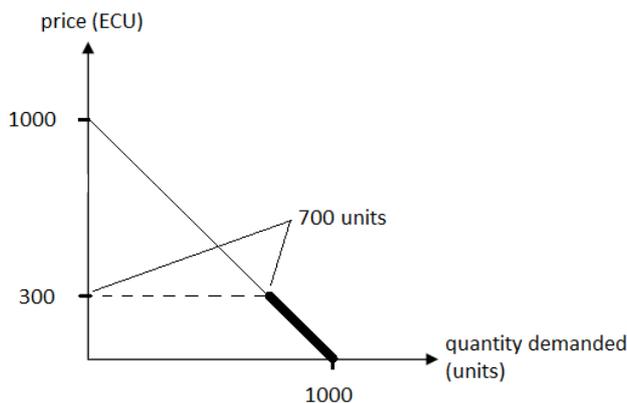
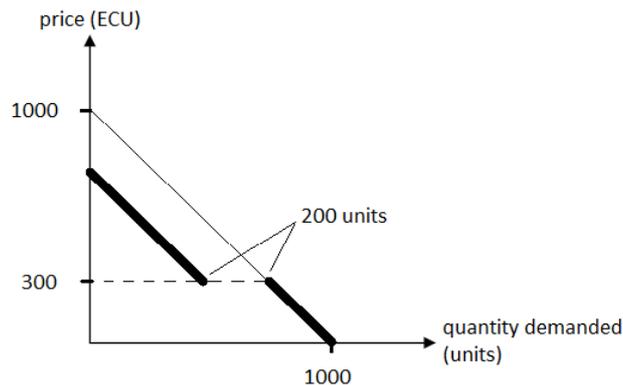
At a price of 0 ECU, there will be a total demand for 1000 units on the market. With a price increase by 1 ECU, demand is reduced by 1 unit. At a price of 1000 ECU no one will be ready to buy the good. Here is a graphical representation of this relationship:



Customers will first try to purchase the good from the firm offering at the lowest price. Customers with the highest willingness to pay will be served first. In case several firms choose the same price, demand will be divided equally among them.

Your sales quantity depends on both the production quantities and the prices chosen by you and the other firms. Three cases are to be distinguished: **1.** You choose the lower price. You are confronted with the entire market demand. **2.** Both firms choose the same price. Market demand is divided equally. **3.** You choose the higher price. First the other firm sells up to its capacity. There may be some residual demand left for you or not.

This is illustrated by the following two examples. For both examples assume that the other firm has chosen a price of 300 ECU. The production quantity of the other firm be 200 units in the first example and 700 units in the second.



In the figures above, the remaining demand for your firm is represented by the bold line. If your price is lower than 300 ECU, your price is the lowest and you are confronted with the entire market demand. If your price is higher than 300 ECU, the other firm sells its units first. The bold line in the figures is shifted leftwards by this quantity. In the first example (upper figure), the production quantity of the other firm is not enough to satisfy all market demand. Therefore, a residual demand is left over for you at prices above 300 ECU. In the second example (lower figure), however, the production quantity of the other firm satisfies the whole demand at this price, so you are unable to sell any units at prices above 300 ECU.

Formally, the demand a firm is confronted with can be expressed as follows. Let q be your production quantity, \tilde{q} the production quantity of the other firm, and p and \tilde{p} the prices chosen by you and the other firm, respectively. The demand you are confronted with is given by

$$D = \begin{cases} 1000 - p & p < \tilde{p} \\ 500 - p/2 & p = \tilde{p} \\ \max\{0, 1000 - p - \tilde{q}\} & p > \tilde{p} \end{cases}$$

D cannot be negative. In case you have chosen the higher price and $1000 - p - \tilde{q}$ yields a negative value, your residual demand is zero.

At the second stage you can sell at most as many units as are demanded. Moreover, you can sell no more than you have produced at the first stage. That is, your sales quantity V is given by the minimum of these two values:

$$V = \min\{D, q\}.$$

Regard the following two examples to see how your sales quantity is determined.

<i>Example 1</i>	Production quantity (units)	Price (ECU)
My firm	300	400
Other firm	200	300

You have chosen the higher price. Your residual demand is $D = 1000 - p - \tilde{q} = 1000 - 400 - 200 = 400$. As you have produced no more than 300 units, you can sell no more than those units: $V = \min\{400, 300\} = 300$.

<i>Example 2</i>	Production quantity (units)	Price (ECU)
My firm	300	350
Other firm	700	250

You again have chosen the higher price. $1000 - p - \tilde{q} = 1000 - 350 - 700 = -50$ yields a negative value. Your residual demand is $D = \max\{0, -50\} = 0$. You therefore cannot sell any units, $V = \min\{0, 300\} = 0$.

In case a firm cannot sell its entire production from the first stage at the second stage, the units that could not be sold will forfeit and cannot be transferred to the next period. However, production costs are incurred for all units produced, no matter whether they can be sold or not.

Your **profit** or loss Π (in ECU) in a period is equal to your **sales revenue** (= offer price chosen by you multiplied by your sales quantity) **at the second stage** minus your **production costs** (= production quantity chosen by you multiplied by 100 ECU) **at the first stage**:

$$\Pi = Vp - 100q.$$

This profit function due to the case discriminations in determining the sales quantity is a piecewise function. Given the production quantities q and \tilde{q} and the price of the other firm \tilde{p} , the following prices are possible candidates for the price which maximizes your profit:

- (I) $p = 1000 - q,$
- (II) $p = 1000 - q - \tilde{q},$
- (III) $p = 500$
- (IV) $p = 500 - \tilde{q}/2,$ round to next integer if applicable,
- (V) $p = \tilde{p},$
- (VI) $p = \tilde{p} - 1.$

When you will make your price choice in the experiment, you will know about q and \tilde{q} , but not about \tilde{p} . However, you may interpret \tilde{p} as the price which you expect the other firm to choose.

The following example illustrates how to find your profit-maximizing price given q, \tilde{q} , and \tilde{p} .

<i>Example 3</i>	Production quantity (units)	Price (ECU)
My firm	400	?
Other firm	400	300

The candidates for your profit-maximizing price are (I) 600, (II) 200, (III) 500, (IV) and (V) 300, and (VI) 299. The price candidates yield the following sales quantities and profits:

- (I) $V = 1000 - 600 - 400 = 0$ $\Pi = 0 * 600 - 100 * 400 = -40000.$
- (II) $V = \min\{1000 - 200, 400\} = 400$ $\Pi = 400 * 200 - 100 * 400 = 40000.$
- (III) $V = 1000 - 500 - 400 = 100$ $\Pi = 100 * 500 - 100 * 400 = 10000.$
- (IV) and (V) $V = 500 - 300/2 = 350$ $\Pi = 350 * 300 - 100 * 400 = 65000.$
- (VI) $V = \min\{1000 - 299, 400\} = 400$ $\Pi = 400 * 299 - 100 * 400 = 79600.$

A comparison of the profits reveals that 299 is your profit-maximizing price choice in this example.

Your balance in each period is increased or decreased by your profit or loss respectively in this period. If your balance should become negative during the experiment, you can nevertheless go on producing and selling, i.e. your firm has a credit without limit in the experiment. If your final balance at the end of the experiment should be negative, you will only be paid your participation fee. Your participation fee will not be charged against any losses.

To get a general idea of your sales quantities and profits resulting from different price and quantity combinations, you may use the **"profit calculator"**. Simply enter a hypothetical production quantity and a hypothetical offer price for each firm and click on "Calculate". Then your resulting sales quantity and profit will be displayed. You may use the profit calculator throughout the whole experiment.

Moreover, after each period a screen with a **"history"** will inform you about the outcomes so far. For all past periods the "history" shows the production quantities and prices chosen by the firms, your resulting sales quantity, your sales revenue, your production cost and your profit. Moreover, your current money balance will be displayed.

The following two screenshots illustrate the use of the profit calculator and the history:

Here you can enter two hypothetical production quantities and offer prices and have the results calculated.

Here you can enter and submit your choice for the current period (production quantity at the first stage, offer price at the second stage).

The results of the profit calculator are displayed here.

Your total profit so far is displayed here.

The results of the last period are displayed here.

This is the history showing the outcomes of all past periods. Zeros are displayed for trial periods.

To practice there will be **three trial periods** before the actual periods start. The procedures in the trial periods are the same as in the actual periods described above, but the outcomes of the trial periods will not add to your payoff. Your start balance for the trial periods is 100,000 ECU. After the trial periods your balance will be set to 200,000 ECU – your start balance for the actual periods – independent of your profits or losses in the trial periods. After the trial periods, the participants will be randomly re-matched into new groups. The new groups will stay fixed throughout all actual periods of the experiment. The profit calculator will be available to you during both trial and actual periods. The history, however, will not be displayed after trial periods and will only contain the data of the actual periods.

Your **final balance** will be **paid to you** after the experiment at the above-mentioned exchange rate. Additionally, you will receive your participation fee. None of the other participants will come to know your payoff, and neither will you come to know the payoff of any other participant. Furthermore, no participant will come to know with whom he or she interacted during the experiment.

If you click on the "Continue" button on your screen, some tasks will appear to check whether all participants have understood the instructions. As soon as all participants will have solved the tasks, the first trial period will start.

If there are any questions concerning the experiment, please raise your hand. Enjoy the experiment!

Appendix B

Translation of the control questions.

Control questions in treatments 1-6:

Binary choice: correct or wrong.

1. The participants will be re-matched into new groups before each period of the experiment. – Correct if random matching condition; wrong if fixed matching condition.
2. Each firm in each period at first chooses its production quantity. Firms are informed about all production quantities in their group. Then each firm chooses its sales price. – Correct.
3. Production costs per unit are equally large for every unit. – Correct.
4. A firm which has not chosen the lowest price within its group can never sell any units in the respective period. – Wrong.
5. In case a firm is unable to sell its whole production in one period, it can offer the unsold units in the next period again. – Wrong.

Control tasks in treatment 7:

<i>Task 1</i>	Production quantity (units)	Price (ECU)
My firm	500	300
Other firm	500	350

My sales quantity: ___ (correct answer: 500)

My profit: ___ (correct answer: 100,000)

<i>Task 2</i>	Production quantity (units)	Price (ECU)
My firm	700	400
Other firm	400	500

My sales quantity: ___ (correct answer: 600)

My profit: ___ (correct answer: 170,000)

<i>Task 3</i>	Production quantity (units)	Price (ECU)
My firm	400	450
Other firm	500	400

My sales quantity: ___ (correct answer: 50)

My profit: ___ (correct answer: -17,500)

<i>Task 4</i>	Production quantity (units)	Price (ECU)
My firm	300	?
Other firm	300	400

My profit-maximizing price: ___ (correct answer: 400)

My maximum profit: ___ (correct answer: 90,000)

<i>Task 5</i>	Production quantity (units)	Price (ECU)
My firm	250	?
Other firm	200	200

My profit-maximizing price: ___ (correct answer: 550)

My maximum profit: ___ (correct answer: 112,500)

<i>Task 6</i>	Production quantity (units)	Price (ECU)
My firm	400	?
Other firm	400	500

My profit-maximizing price: ___ (correct answer: 499)

My maximum profit: ___ (correct answer: 159,600)

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IV Demand Rationing in Bertrand-Edgeworth Markets with Fixed Capacities: An Experiment^{*}

Abstract. This study is the first to investigate the effect of demand rationing in experimental Bertrand-Edgeworth markets with fixed exogenous capacities. It is found that prices and profits are significantly higher under proportional than under efficient demand rationing. Moreover, the amount of capacity available to each firm is varied. In accordance with earlier studies, prices and profits are significantly higher when capacities are lower. Those effects accord qualitatively with the Nash equilibrium predictions of the corresponding stage games. However, the Nash equilibrium concept does poorly at quantitative predictions. Prices are significantly higher than the Nash prediction in all treatments, irrespective of whether the Nash equilibrium is in mixed or in pure strategies. Profits are higher than the Nash prediction with high capacities, but may converge to the equilibrium prediction in the long run with low capacities. The data of individual price choices feature dynamic patterns that can potentially be explained by both Edgeworth price cycles and imitation of the price set by the competitor.

JEL classification: C72, C90, D43, L13.

Key words: Bertrand-Edgeworth, demand rationing, Edgeworth cycles, oligopoly, laboratory experiment.

^{*} Corresponding working paper: Martin Jacobs and Till Requate (2016), Demand Rationing in Bertrand-Edgeworth Markets with Fixed Capacities: An Experiment, Economics Working Paper 2016-03, University of Kiel.

1 Introduction

Edgeworth's (1925) enhancements of Bertrand's (1883) price competition model tremendously complicate its game-theoretic solution. Specifically, the Nash equilibrium becomes contingent on the rule according to which demand is rationed. Compared to other market institutions, few experiments have so far been conducted to investigate price oligopoly with capacity constraints.

We follow the seminal experiment by Kruse et al. (1994) in that we consider price competition in markets with fixed, exogenously given capacity constraints. Our study, to the best of our knowledge, is the first to investigate the effect of demand rationing in such a setting. The two demand rationing rules most prominent in the literature are considered: proportional and efficient rationing. Moreover, we vary the amount of capacity available to each firm in the market.

We find that the seemingly minor change in demand rationing entails a significant effect. Prices and profits are higher with proportional than with efficient rationing. Furthermore, in accordance with earlier findings, lower capacities yield higher prices and profits. These effects are predicted qualitatively by the Nash equilibria for the single treatments. However, our data do not fit the Nash predictions quantitatively. This holds even for the treatment with low capacities and efficient rationing where the Nash equilibrium is in pure strategies. The empirical mean price is significantly higher than the expected mean price in Nash equilibrium in all treatments. Profits are significantly higher than the Nash predictions in the treatments with high capacities, but approach Nash predictions in the long run in the treatments with low capacities. Alternative, off-equilibrium concepts that may rationalize the empirical data are Edgeworth price cycles brought about by myopic best response pricing and imitation of the price set by the competitor in the past. Both of these concepts are able to partially explain empirical price choices; Edgeworth's myopic best response theory does somewhat better. A simultaneous test of the two alternative concepts is impeded by excessive collinearity. Therefore, a definite answer cannot be given to the question which of the two alternative concepts is the true driver of the dynamic pricing patterns in the data.

The remainder of this study is structured as follows. Sections 2 and 3 briefly review the theory and existing experimental evidence concerning Bertrand-Edgeworth markets with fixed capacities. Section 4 describes the market setting of our experiment, and section 5 presents the associated outcomes for prices and profits in benchmark situations. The

experimental procedures are explained in section 6. Section 7 states our hypotheses. Our experimental results are presented in section 8. Section 9 concludes.

2 Theoretical background

Bertrand's (1883) model of simultaneous price oligopoly yields the odd prediction that there will be perfect competition with marginal cost pricing and zero profits as soon as there are at least two firms in the market. That results from the assumption that each firm can produce any quantity at constant marginal costs. Edgeworth (1925) abandoned this assumption in order to receive a model with more realistic results. He assumed instead that firms' production capacities are constrained. The capacity constraints may be either exogenously given or endogenously imposed by introducing increasing marginal production costs, such that further production becomes unprofitable from some point.

While Edgeworth's modifications make the model more realistic, they also make it considerably more complicated. There is in general no Nash equilibrium in pure strategies. The existence of Nash equilibrium in mixed strategies in the Bertrand-Edgeworth setting has been proven only in the 1980s (Dixon, 1984; Dasgupta and Maskin, 1986; Maskin, 1986).

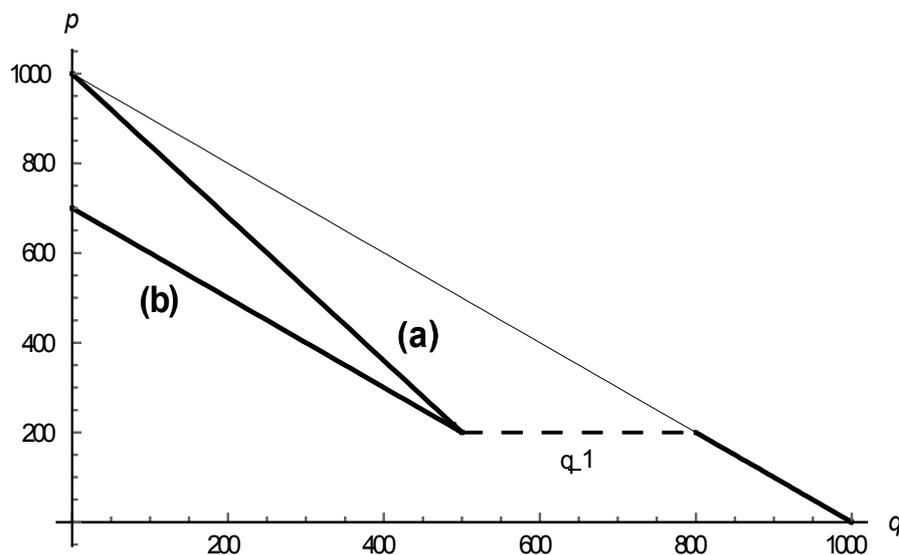


Figure 1: Proportional (a) and efficient (b) demand rationing.

Equilibrium depends on how demand is rationed if firms choose different prices. Two assumptions about demand rationing are prominent in the literature: proportional and efficient rationing. Proportional rationing means that customers are served in random order, such that a

fraction of randomly ordered customers, if any, is left for firms with higher prices. In contrast, under efficient rationing those customers with the highest willingness to pay are served first. Customers with a lower willingness to pay, if any, are then left for firms with higher prices. The quantity a firm is able to sell at a higher price will therefore be higher under proportional than under efficient rationing. Figure 1 illustrates the rationing rules graphically for a duopoly with linear total demand $D(p) = 1000 - p$. The bold line represents residual demand for firm 2 at varying prices p_2 , given that firm 1 is willing to sell quantity $q_1 = 300$ at price $p_1 = 200$. Whereas under proportional rationing the demand curve is rotated around the reservation price to obtain residual demand, it is shifted leftwards in parallel under efficient rationing.

For several special cases of Bertrand-Edgeworth markets with exogenous capacity constraints the mixed-strategy Nash equilibrium has been characterized. Beckmann (1967) dealt with the symmetric duopoly with linear total demand under proportional demand rationing. The equilibrium for the otherwise same situation under efficient demand rationing was described by Levitan and Shubik (1972). Vives (1986) generalized the analysis under efficient rationing to markets with more than two firms and non-linear demand (retaining the symmetry assumption). Osborne and Pitchik (1986) provided a generalization for the duopoly under efficient rationing to firms with asymmetric capacities and non-linear demand. Beckmann's results were corrected and generalized by Allen and Hellwig (1993) who provided a complete characterization of the equilibrium pricing strategies in the duopoly with proportional rationing and established uniqueness of equilibrium under weak conditions. More recently, Hirata (2009) and De Francesco and Salvadori (2010) analyzed the triopoly under efficient demand rationing. De Francesco and Salvadori (2013) established uniqueness of the equilibrium characterized by Vives and extended the analysis to some asymmetric capacity combinations.¹

3 Related experiments

Compared to the multitude of market experiments concerned with the standard models by Cournot and Bertrand, experiments in the more complex Bertrand-Edgeworth setting are still scarce. The first experimental investigation of Bertrand-Edgeworth markets with exogenous capacities is the seminal study by Kruse et al. (1994). They varied capacities and the amount

¹ The analysis of a special case of the duopoly with linear demand and asymmetric capacities can also be found in the appendix of Levitan and Shubik (1972).

of information firms have about cost and demand in symmetric oligopolies with four firms. An isoelastic demand function and proportional demand rationing were employed. The different capacity conditions gave rise to different mixed-strategy pricing Nash equilibria. Kruse et al. found that higher capacities led to lower prices while the variation of information did not entail a significant effect on prices. Subjects did not price according to the Nash equilibrium, but on average set higher prices. The dynamics of individual price choices can be explained by a partial adjustment to the myopic best response to the other firms' prices in the last period (as proposed by Edgeworth's price cycle theory).

Fonseca and Normann (2013) also found a negative effect of capacities on prices in a similar setting. Moreover, they conducted both duopoly and triopoly treatments and observed that prices in the latter are lower when total capacity in the market is held constant. Again, prices exceeded the equilibrium predictions. On the whole, Edgeworth's theory of myopic price adjustment rationalized the data better than the Nash equilibrium concept. In another study, Fonseca and Normann (2008) also considered asymmetric capacity distributions. Asymmetry had a clear price-declining effect and even resulted in average prices below the Nash equilibrium prediction.

The finding that behavior according to the Nash equilibrium is too demanding in Bertrand-Edgeworth settings is corroborated by Heymann et al. (2014). They found that the pricing dynamics in their experiment can be explained by a simple heuristic: A subject raises her price by a certain increment as long as she does not fully utilize her capacity. In contrast, she lowers her price by that increment when she cannot sell up to her capacity.

This study is to our knowledge the first to investigate the effect of demand rationing in Bertrand-Edgeworth markets with exogenous capacities.² However, demand rationing has been varied in related experiments of price competition with convex costs (Kruse, 1993; Jacobs and Requate, 2016) and two-stage games of capacity-setting and subsequent pricing à la Kreps and Scheinkman (1983) (Lepore and Shafran, 2013; Jacobs, 2016). All those studies have found that, independently of the direction of the predicted effect, proportional demand rationing yields higher prices than efficient demand rationing.

² Buchheit and Feltovich (2011) conducted a Bertrand-Edgeworth market experiment with exogenous capacity constraints and employed six different demand rationing schemes. However, demand rationing is not a treatment variable in their design. Rather, they investigated the effect of sunk fixed costs. In all their treatments, before each round one of the six rationing schemes is randomly determined to be effective in the next round. While Buchheit and Feltovich inform their subjects in the instructions which six rationing schemes can occur, subjects do not know the effective rationing scheme in the current round when they make their price choice.

4 The market setting

We consider experimental duopoly markets where the two symmetric supplying firms offer a single homogenous good and compete in prices. Competition takes place repeatedly over several periods. In each period, firms are confronted with the linear market demand function $D(p) = 1000 - p$. Each firm can supply at most k units in each period; market capacity thus equals $2k$. Capacity per firm is one of the two treatment variables. In the high-cap treatments $k = 500$ while in the low-cap treatments $k = 300$. For simplicity, production costs are set to zero, but the setting can easily be generalized to constant marginal costs up to the capacity constraint by appropriate rescaling. Any unsold capacity forfeits and cannot be transferred to the next period. For example, the setting may resemble competition between two retail firms which sell a perishable and are bound by long-term wholesale contracts, such that both quantity and unit price of their purchases are given and the accompanying costs are sunk.

The quantity a firm sells in a given period can be at most its capacity. Furthermore, the sales quantity q_i of firm i depends on the prices p_i and p_j the two firms choose and on the effective demand rationing scheme, the second treatment variable. Under efficient demand rationing, it is given by

$$q_i^{eff}(p_i, p_j, k) = \begin{cases} \max\{0, \min\{1000 - p_i, k\}\} & p_i < p_j \\ \max\{0, \min\{500 - p_i/2, k\}\} & p_i = p_j \\ \max\{0, \min\{1000 - k - p_i, k\}\} & p_i > p_j \end{cases}$$

In case i has chosen a higher price than j , a different sales quantity for i results under proportional demand rationing:

$$q_i^{prop}(p_i, p_j, k) = \begin{cases} \max\{0, \min\{1000 - p_i, k\}\} & p_i < p_j \\ \max\{0, \min\{500 - p_i/2, k\}\} & p_i = p_j \\ \max\left\{0, \min\left\{\left(1000 - p_i\right)\left(1 - \frac{k}{1000 - p_j}\right), k\right\}\right\} & p_i > p_j \end{cases}.$$

As there are no costs, firm i 's profit in a given period is equal to its revenue: $\Pi_i(p_i, p_j, k) = p_i q_i(p_i, p_j, k)$.

5 Benchmark outcomes

As benchmark cases we calculate the market outcome in our model under full competition as well as under full collusion. While those outcomes depend on the market capacity, they do not

depend on the effective rationing scheme since both firms charge the same price in the benchmark settings.

The outcome under full competition is described graphically by the intersection of market supply and market demand in Figure 2. If there is enough capacity in the market to satisfy all demand, the competitive price is zero; otherwise the competitive price is given by the price at which market demand equals market capacity. Formally, $p^{comp} = \max\{0, 1000 - 2k\}$. The corresponding individual sales quantities and profits are $q_i^{comp} = \min\{500, k\}$ and $\Pi_i^{comp} = \max\{0, k(1000 - 2k)\}$.

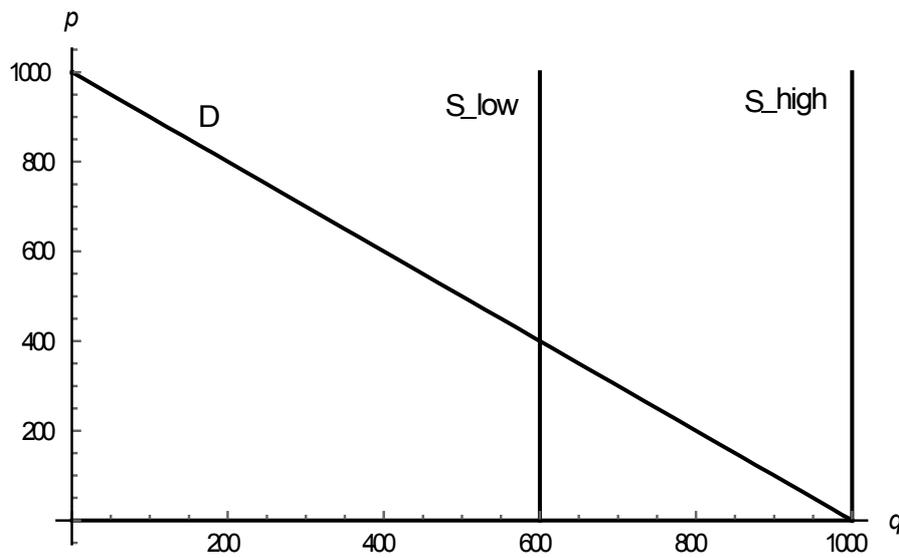


Figure 2: Market supply and market demand.

Under full collusion, in case the market capacity is at least 500, both firms charge a price of 500 maximizing joint profit. Even higher prices occur if market capacity is smaller, $p^{coll} = \max\{500, 1000 - 2k\}$. Quantities and profits under full collusion are $q_i^{coll} = \min\{250, k\}$

$$\text{and } \Pi_i^{coll} = \begin{cases} 125000 & k \geq 250 \\ k(1000 - 2k) & k < 250 \end{cases}$$

Another benchmark is the Nash equilibrium outcome of the stage game.³ For the relatively simple case of duopoly with symmetric capacities we consider, for both proportional and efficient rationing Nash equilibrium has been characterized and its uniqueness has been

³ In principle, since interaction takes place repeatedly over several periods, one may also expect supergame effects that lead to a more collusive conduct than predicted by the Nash equilibrium of the stage game. However, our experiment lasted for a given number of periods that was disclosed to the subjects *ex ante*. Thus, backward induction applies and the predictions for the stage game remain valid. Moreover, Kruse (1993) found that the stage-game Nash equilibrium can be a good predictor of behavior in a Bertrand-Edgeworth market experiment even if subjects are not informed about the number of periods to be played.

proven. The analysis of the case with efficient demand rationing is due to Levitan and Shubik (1972).⁴ For $k \leq 1000/3$, which is fulfilled in our low-cap condition, the Nash equilibrium is in pure strategies with $p^{NE} = 1000 - 2k$ and $\Pi_i^{NE} = k(1000 - 2k)$. For $k \geq 1000$, price and profit in Nash equilibrium are zero. In the remaining interval, $1000/3 < k < 1000$, there is a Nash equilibrium in mixed strategies. For our high-cap treatment condition with $k = 500$, the cumulative distribution function (cdf) describing the mixed-strategy pricing Nash equilibrium is given by

$$\Phi^{high,eff}(p) = \begin{cases} 0 & p < 125 \\ \frac{500p - 62500}{p^2} & 125 \leq p < 250. \\ 1 & 250 \leq p \end{cases}$$

Under proportional demand rationing, we have $p^{NE} = 1000 - 2k$ and $\Pi_i^{NE} = k(1000 - 2k)$ for $k \leq 250$ and again zero price and profit for $k \geq 1000$. Nash equilibrium is in mixed strategies in the capacity range $250 < k < 1000$, that is, in both our high-cap and in our low-cap condition. From the more general analysis by Allen and Hellwig (1993) it emerges that the cdf in our market setting is described by the integral equation

$$\Phi^{prop}(p) = \begin{cases} 0 & p < p_0 \\ 1 - p_0 k \sqrt{\frac{1000 - p}{2k - 1000 + p}} \int_p^{500} \frac{1000 - 2\pi}{\pi^2 (1000 - \pi)^{1.5} \sqrt{2k - 1000 + \pi}} d\pi & p_0 \leq p < 500, \\ 1 & 500 \leq p \end{cases}$$

where p_0 is determined by the condition $\Phi^{prop}(p_0) = 0$. For the case $k = 500$, a closed-form solution is provided by Beckmann (1967),⁵

$$\Phi^{high,prop}(p) = \begin{cases} 0 & p < p_0 \\ 1 - \frac{p_0}{1000} \left(1 - \frac{4}{3} \sqrt{\frac{1000 - p}{p}} + \frac{1}{3} \left(\frac{1000 - p}{p} \right)^2 \right) & p_0 \leq p < 500, \\ 1 & 500 \leq p \end{cases}$$

with $p_0 \approx 171.773$. For $k = 300$, we calculated the closed-form solution

⁴ Levitan and Shubik as well as Beckmann below dealt with the normalized demand function $D(p) = 1 - p$. The results presented here have been adapted to fit the demand function in our experiment.

⁵ Beckmann considered the case of symmetric duopoly with linear market demand and provided closed-form solutions for any capacity. Yet, since he erroneously assumed $(1000 - k)$ instead of 500 to be the upper limit of the integral, his results are invalid except for the case $k = 500$.

$$\Phi^{low,prop}(p) = \begin{cases} 0 & p < p_0 \\ 1 - p_0 \frac{3}{10000} \sqrt{\frac{1000-p}{p-400}} \left[\frac{-\sqrt{5}}{0.5^{1.5} 12} \left(\sqrt{0.5} + 2.25 \arctan\left(\frac{1}{\sqrt{8}}\right) \right) \right. \\ \quad \left. + \frac{5^{3.5}}{3p \left(1 - \frac{p}{1000}\right)^{1.5}} \sqrt{\frac{1000-p}{5p-2000}} \left(4 \sqrt{1 - \frac{p}{1000}} \left(\frac{7}{200000} p^2 - \frac{29}{1000} p + 6 \right) \right. \right. \\ \quad \left. \left. - \frac{9}{1000} \left(\frac{p^2}{1000} - p \right) \sqrt{\frac{p}{100}} - 4 \arctan\left(\frac{4000 - 7p}{2\sqrt{2000 - 2p}\sqrt{5p - 2000}} \right) \right] \right\} & p_0 \leq p < 500 \\ 1 & 500 \leq p \end{cases},$$

with $p_0 \approx 404.644$.

Figure 3 shows a plot of the Nash equilibrium cdf's for all four treatments. Given the cdf's, we can calculate the expected price and profit in Nash equilibrium. Table 1 summarizes the benchmark prices and profits for all treatments.⁶

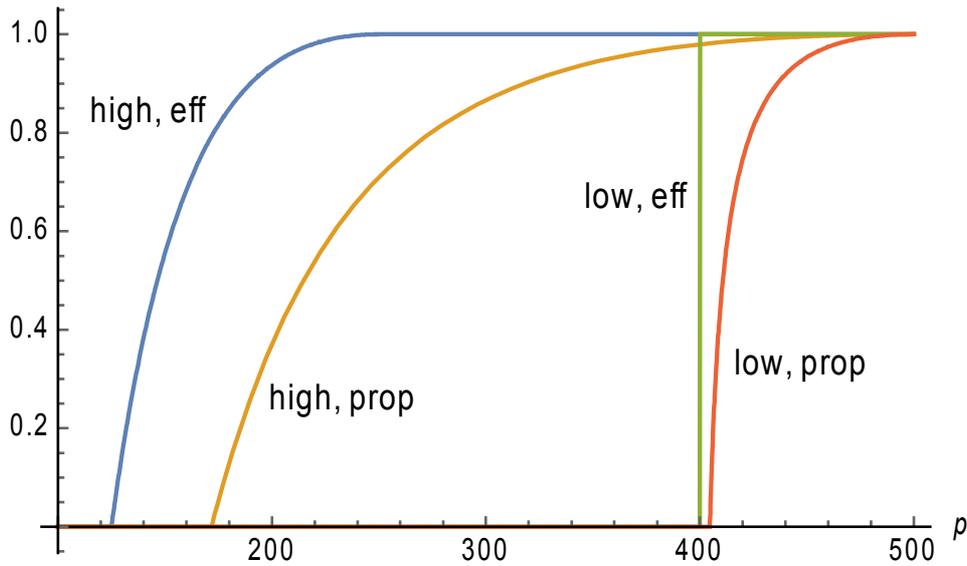


Figure 3: Cumulative distribution functions of the pricing Nash equilibria.

⁶ Calculating expected prices and profits in the mixed-strategy Nash equilibria, we took into account that in our actual experimental setting price is not a continuous choice variable, but only integer prices were permissible. In order to discretize the setting, we took as probability that an integer price p^* is chosen the value of the cdf at this price minus the value of the cdf at $(p^* - 1)$, i.e. $prob(p^*) = \Phi(p^*) - \Phi(p^* - 1)$. The stated expected prices and profits as well as the accompanying standard deviations therefore differ slightly from the results with continuous pricing.

	Full Competition	Full Collusion	Nash Equilibrium
high, eff	$p^{comp} = 0$	$p^{coll} = 500$	$E(p^{NE}) = 153.929$ $\sigma_p = 24.570$
	$\Pi_i^{comp} = 0$	$\Pi_i^{coll} = 125000$	$E(\Pi_i^{NE}) = 62667.833$ $\sigma_{\Pi} = 9518.569$
high, prop	$p^{comp} = 0$	$p^{coll} = 500$	$E(p^{NE}) = 233.779$ $\sigma_p = 58.378$
	$\Pi_i^{comp} = 0$	$\Pi_i^{coll} = 125000$	$E(\Pi_i^{NE}) = 86017.658$ $\sigma_{\Pi} = 20128.786$
low, eff	$p^{comp} = 400$	$p^{coll} = 500$	$p^{NE} = 400$
	$\Pi_i^{comp} = 120000$	$\Pi_i^{coll} = 125000$	$\Pi_i^{NE} = 120000$
low, prop	$p^{comp} = 400$	$p^{coll} = 500$	$E(p^{NE}) = 416.933$ $\sigma_p = 14.655$
	$\Pi_i^{comp} = 120000$	$\Pi_i^{coll} = 125000$	$E(\Pi_i^{NE}) = 121433.871$ $\sigma_{\Pi} = 2223.262$

Table 1: Benchmark outcomes.

6 Experimental procedures

Combining two capacity conditions with two rationing schemes, we obtained four different treatments. The treatments are named according to their capacity condition ("high" or "low") followed by their effective rationing scheme ("eff" for efficient or "prop" for proportional). Two sessions of each treatment were conducted at the economics experimental lab at Kiel University during April and May 2015. Due to no-shows of subjects, the number of markets per treatment varies between 10 and 14. 96 subjects from all fields of study participated in the experiment;⁷ each subject took part only once.

At the beginning of each session, subjects were randomly allocated to computer terminals in the lab. They received printed instructions that provided full information about the market setting, but were not told any of the benchmark outcomes mentioned in the last section.⁸ After reading, the key features from the instructions were repeated by the experimenter in a short presentation. Then the actual experiment started which was computerized using the z-tree software (Fischbacher, 2007). Subjects first played three unpaid trial rounds, then were re-matched and played 20 paid rounds in fixed groups. The number of rounds to be played was common knowledge. In each round, each subject had to decide on her sales price. Integers

⁷ The hroot software package (Bock et al., 2014) was used for subject recruitment.

⁸ Instructions for a sample treatment are included in Appendix A.

from 0 to 1000 were admissible prices. Buyers were simulated by the computer. Throughout the experiment, subjects could use a profit calculator implemented in the software. The profit calculator returned a subject's sales quantity and profit when two hypothetical sales prices were entered. After each round, subjects were informed about the prices chosen in that round and about their resulting own sales quantity and profit. In the end, subjects were paid the sum of their earnings from all paid rounds at a predefined exchange rate.⁹ Payments were made in private. An average session took about 80 minutes, including time for instructions and payment. The average payment per subject amounted to 16.1 Euro.

7 Hypotheses

Taking into account the benchmark outcomes from section 5 and the results of the related experiments mentioned in section 3, we set up hypotheses about the experimental outcome. The first two hypotheses concern the effects of the two treatment variables.

Hypothesis 1: Prices and profits will be higher in the low-cap than in the high-cap condition.

Hypothesis 2: Prices and profits will be higher with proportional than with efficient demand rationing.

The hypotheses follow from the Nash equilibrium predictions stated in Table 1. Moreover, evidence from the experiments by Kruse et al. (1994) and Fonseca and Normann (2013) supports Hypothesis 1. The effect of demand rationing has not yet been investigated experimentally in the market setting we employ, but support for Hypothesis 2 stems from the experiments by Kruse (1993) and Jacobs and Requate (2016) which consider the closely related setting of price competition with convex production costs.

While the first two hypotheses are only qualitative, the Nash equilibrium predictions allow us to set up also quantitative hypotheses. Assuming behavior in accordance with Nash equilibrium, we formulate the next two hypotheses:

Hypothesis 3: The distribution of individual price choices in each treatment will follow the probability distribution predicted by the respective Nash equilibrium.

Hypothesis 4: The mean price and profit in each treatment will equal the expected mean price and profit in the respective Nash equilibrium.

As it is quite demanding to expect that the experimental data will fit the Nash prediction even in their distributions, we take Hypothesis 4 as an alternative to test a somewhat weaker accordance of the data with Nash equilibrium theory. However, the studies by Kruse et al.

⁹ The exchange rate was 1 Euro per 120,000 experimental currency units (ECU) in the high-cap treatments and 1 Euro per 150,000 ECU in the low-cap treatments.

(1994) and Fonseca and Normann (2013) support neither of the two hypotheses. They found that pricing within the same market across periods is not independent as the Nash predictions purport. Instead, they observed dynamic pricing patterns that can be explained by Edgeworth's off-equilibrium theory of price cycles which come about when subjects myopically optimize taking past prices as fixed. Hence, our next hypothesis:

Hypothesis 5: Individual price choices will follow a myopic best response rationale as purported by Edgeworth's price cycling theory.

Yet, as investigated in Jacobs and Requate (2016) for Bertrand-Edgeworth markets with convex costs, still another possible behavioral pattern is that subjects just imitate past prices of their competitor:

Hypothesis 6: Individual price choices will be based on imitation of the other firm's price observed in the last round.

We thus have two opposing hypotheses of off-equilibrium behavior that provide alternatives to the Nash predictions.

8 Results

We divide the presentation of our results into two parts. Hypotheses 1 and 2 are concerned with how the market outcome is affected by variations of the treatment variables in the experiment. We therefore use data aggregated on the market level to evaluate those hypotheses. Hypotheses 3 to 6, in contrast, pertain to individual price choices and the resulting individual profits. Consequently, data on the individual level have to be considered for an assessment.

8.1 Market performance

As measures to assess market performance, we use the market price and the average profit per firm in a market. The market price is the quantity-weighted average of the prices the single firms in a given market and period set; it equals total revenue divided by total sales quantity in a given market and period. The average profit per firm equals the total profit realized in a given market and period divided by two, the number of firms in each market. Thus, for both price and profit we have one observation per market and period.

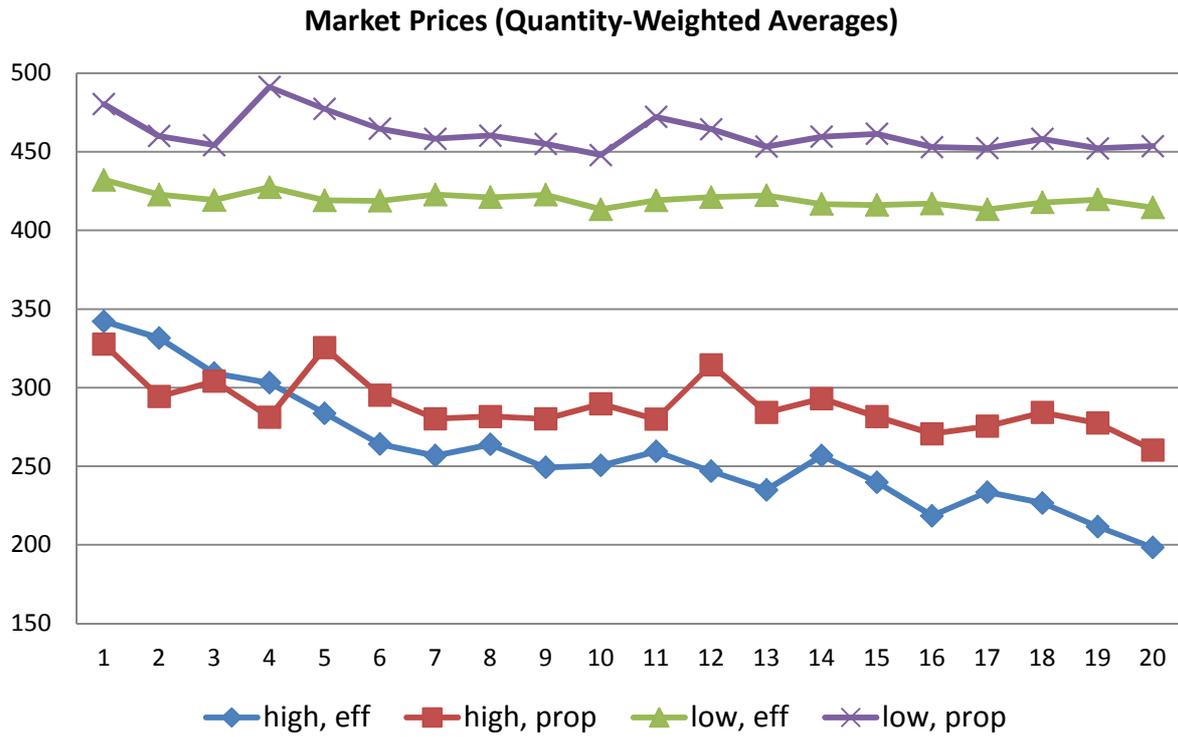


Figure 4: Series of market prices.

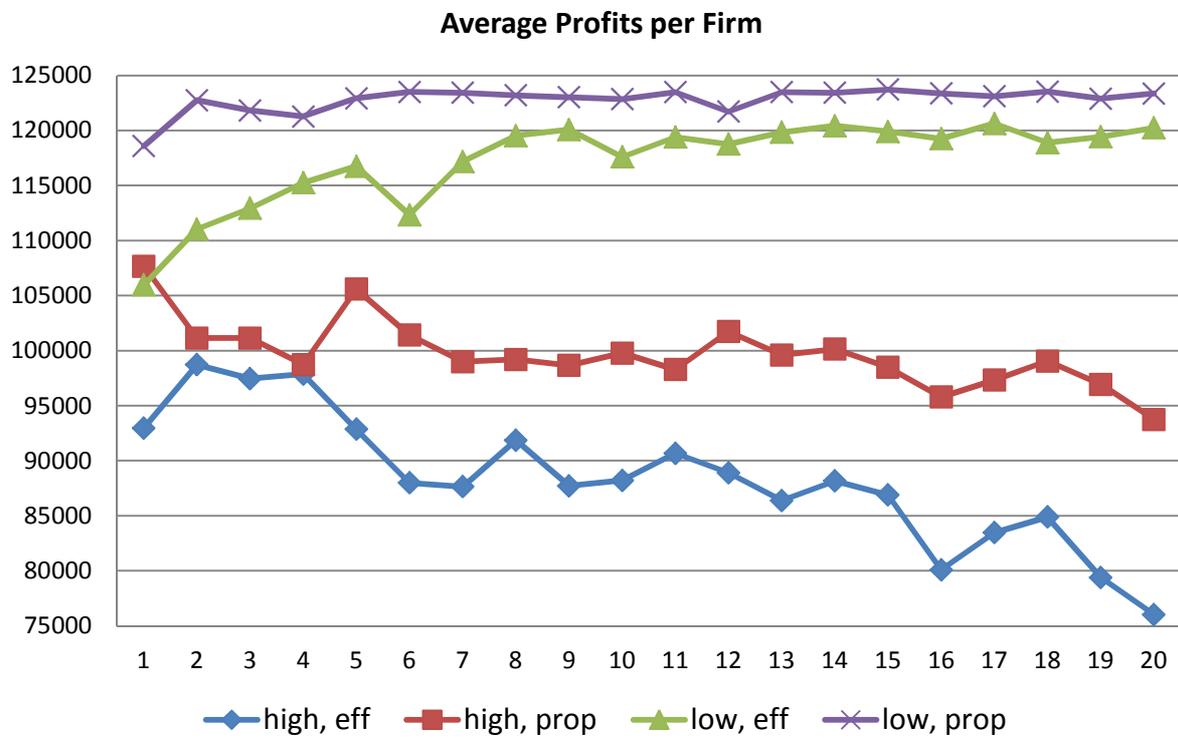


Figure 5: Series of average profits per firm.

Panel A: Market Prices			
<i>periods</i>	<i>1-20</i>	<i>1-10</i>	<i>11-20</i>
high, eff	259.007 (77.847) [232.878]	285.382 (82.528) [277.292]	232.633 (62.864) [211.335]
high, prop	289.050 (65.447) [276.611]	295.967 (68.715) [284.716]	282.133 (61.495) [269.512]
low, eff	419.820 (30.444) [416.076]	421.907 (38.118) [423.053]	417.732 (20.009) [411.957]
low, prop	461.447 (36.394) [462.162]	464.901 (39.530) [467.435]	457.994 (32.798) [453.909]

Panel B: Average Profits per Firm			
<i>periods</i>	<i>1-20</i>	<i>1-10</i>	<i>11-20</i>
high, eff	88,409.2 (16,266.9) [85,039.750]	92,330.6 (16,813.2) [93,119.750]	84,487.8 (14,746.8) [80,836.000]
high, prop	99,673.1 (11,865.9) [98,871.325]	101,233.6 (12,955.0) [101,379.125]	98,112.6 (10,485.6) [95,987.100]
low, eff	117,270.9 (8,461.3) [120,000.000]	114,864.8 (11,255.9) [119,077.000]	119,677.1 (2,337.4) [120,000.000]
low, prop	122,772.4 (3,648.2) [123,680.325]	122,336.0 (4,632.4) [123,648.475]	123,208.7 (2,215.7) [123,708.525]

Standard errors in parentheses. Medians in square brackets.

Table 2: Market prices (Panel A) and profits (Panel B): Summary statistics.

Aggregating data from different markets in the same treatment, Figures 4 and 5 illustrate how prices and profits develop throughout the experiment in the different treatments.¹⁰ Table 2 presents the corresponding numbers, combining data from several periods for conciseness. The graphical impression from Figures 4 and 5 supports Hypotheses 1 and 2. Prices and profits are considerably higher in the low-cap than in the high-cap treatments. Whereas the effect of demand rationing is somewhat less sizable, the results appear clear-cut also in this respect. Holding capacity constant and comparing the data points in Figures 4 and 5 pairwise, we find that prices and profits are almost always higher under proportional than under efficient rationing. The only exception to this can be found in the first four periods where in

¹⁰ Figures showing the development of market prices within single markets can be found in Appendix B.

the high-cap condition the market price is somewhat higher under efficient than under proportional rationing.

For a formal statistical analysis, we compare market prices and profits between two treatments within single periods with nonparametric Mann-Whitney tests. For conciseness, we restrict our attention to the data from the second half of the experiment (periods 11 to 20). The implications of our results do not change if all periods are considered. For each treatment variable, two pairwise treatment comparisons apply: To investigate the effect of capacity, data from treatment "high, eff" ("high, prop") are compared to data from "low, eff" ("low, prop"). The effect of the rationing scheme is examined by comparing data from "high, eff" ("low, eff") to data from "high, prop" ("low, prop"). That is, for both prices and profits we conduct 20 significance tests per treatment variable (2 pairwise treatment comparisons multiplied by 10 periods).

Panel A: Market Prices				
	<i>significance test results</i>			
	$p < 0.01$	$0.01 \leq p < 0.05$	$0.05 \leq p < 0.1$	$p \geq 0.1$
<i>effect of capacity</i>				
"high, eff" vs. "low, eff"	10/10	0/10	0/10	0/10
"high, prop" vs. "low, prop"	10/10	0/10	0/10	0/10
<i>effect of the rationing scheme</i>				
"high, eff" vs. "high, prop"	2/10	5/10	2/10	1/10
"low, eff" vs. "low, prop"	6/10	4/10	0/10	0/10

Panel B: Average Profits per Firm				
	<i>significance test results</i>			
	$p < 0.01$	$0.01 \leq p < 0.05$	$0.05 \leq p < 0.1$	$p \geq 0.1$
<i>effect of capacity</i>				
"high, eff" vs. "low, eff"	10/10	0/10	0/10	0/10
"high, prop" vs. "low, prop"	10/10	0/10	0/10	0/10
<i>effect of the rationing scheme</i>				
"high, eff" vs. "high, prop"	5/10	4/10	0/10	1/10
"low, eff" vs. "low, prop"	10/10	0/10	0/10	0/10

Pairwise comparison of treatments in periods 11 to 20. Frequencies of results of two-tailed Mann-Whitney tests. Table entries in the first three columns indicate the number of cases in which the null hypothesis of equal medians is rejected at the displayed significance level in favor of the alternative hypothesis that the median is higher in the second treatment.

Table 3: Market prices (Panel A) and profits (Panel B): Results of significance tests for treatment effects.

The test results are summarized in Table 3. The results concerning the effect of capacity are as clear-cut as they can be: Every single test result is significant at the one percent level.¹¹ Both prices and profits are significantly higher in the low-cap than in the high-cap condition. We also find a consistently significant effect of the rationing scheme although the results are somewhat less powerful. Comparing prices in the two high-cap treatments, the effect of rationing becomes significant in period 12 and remains significant thereafter. In the low-cap treatments there is a significant effect in all periods. The same pattern holds for the profit data. Hence, Hypotheses 1 and 2 are strongly supported, and we conclude:

Result 1: Prices and profits are significantly higher in the low-cap than in the high-cap condition.

Result 2: Prices and profits are significantly higher with proportional than with efficient demand rationing.

8.2 Individual-level data

As the Nash equilibrium makes predictions about individual pricing, we examine our data on the individual level in order to test the accordance of our subjects' behavior with the Nash equilibrium prediction. Hypothesis 3 purports that the distribution of individual prices in each treatment will follow the respective Nash distribution. Figure 6 for each treatment plots the cdf of empirical price choices against the predicted cdf. It can be seen that the empirical cdf's almost entirely lie to the right of the predicted cdf's. That is, subjects largely choose higher prices than predicted. Kolmogorov-Smirnov tests allow us to clearly reject Hypothesis 3. In each treatment, the maximum difference between the values of the empirical and the predicted cdf is at least 0.43. The accompanying probability that the empirical and the predicted price distribution are identical is smaller than 10^{-80} in each case. If one assumes that subjects need some incentivized trials to learn optimal pricing behavior and hence considers only the price choices from period 11 onwards for the empirical cdf's, the test results remain strongly significant.

Result 3: The distribution of individual price choices does not follow the probability distribution predicted by the respective Nash equilibrium.

However, it still can be that some qualities of the Nash equilibrium predictions are contained in the empirical data. We first test for each treatment whether the empirical mean price equals the predicted mean in Nash equilibrium. As mentioned in the above paragraph, from Figure 6 it appears that higher prices than predicted are chosen. That impression is

¹¹ All p -values refer to two-tailed tests.

confirmed by the test results. The null hypothesis of equal means can be rejected at the one percent level for each treatment (t -tests). This still holds if only price choices from periods 11 to 20 are included. The test results are visualized in Figure 7. Figure 7 for each treatment shows the 99 percent confidence interval of the empirical mean price (from all periods) together with the Nash equilibrium prediction. The result that prices are higher than the Nash equilibrium prediction is in accordance with the findings in Kruse et al. (1994) and in Fonseca and Normann (2013). Moreover, it can be seen that the difference between the empirical mean price and its Nash prediction is larger in the high-cap treatments, which confirms another observation by Fonseca and Normann.

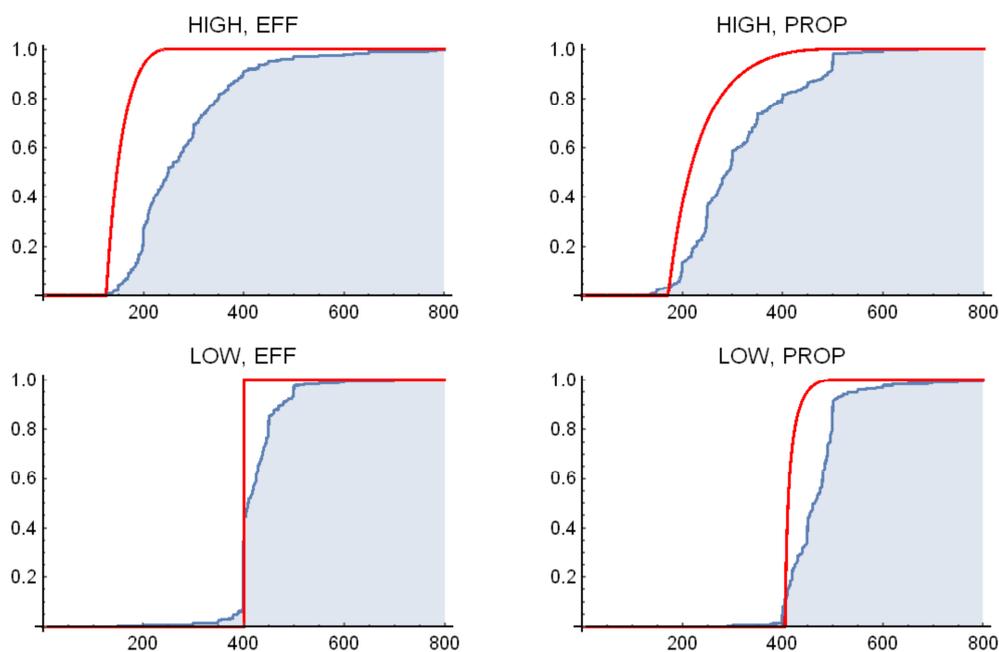


Figure 6: Empirical cumulative distribution functions (blue) and Nash equilibrium predictions (red).

At first sight, it appears especially noteworthy that pricing behavior significantly deviates from the Nash prediction even in the "low, eff" treatment where the Nash equilibrium is in pure strategies and pricing according to that equilibrium promises fairly high profits. Yet, while the chosen prices are significantly higher than the Nash equilibrium prediction, subjects may have little incentive to price exactly according to the Nash equilibrium when the payoff function is flat. This pertains especially to our low-cap treatments where the (expected) profits in all benchmark outcomes are close to each other (recall Table 1). Therefore, we also compare realized profits to their equilibrium predictions. For the two high-cap treatments, the test results are clear. Profits are significantly higher than the Nash prediction, no matter

whether one considers all periods or only periods 11 to 20 (all p 's < 0.01 , t -tests). The results for the low-cap treatments are somewhat ambiguous. In the "low, eff" treatment, profits are significantly *lower* than predicted by the Nash equilibrium if data from all periods are included ($p < 0.01$). However, if one includes only the data from periods 11 to 20, profits are not significantly different from the Nash equilibrium profit ($p = 0.45$). It may be that learning takes place in the first periods of the experiment and subjects adjust their pricing behavior towards the Nash equilibrium, such that profits then converge towards the equilibrium level. Due to the flatness of the payoff function, this can lead to a situation where prices are still significantly different from the benchmark, but profits are not. In the "low, prop" treatment, profits are higher than the Nash prediction. The difference is weakly significant ($p < 0.1$) for both periods 1 to 20 and periods 11 to 20.

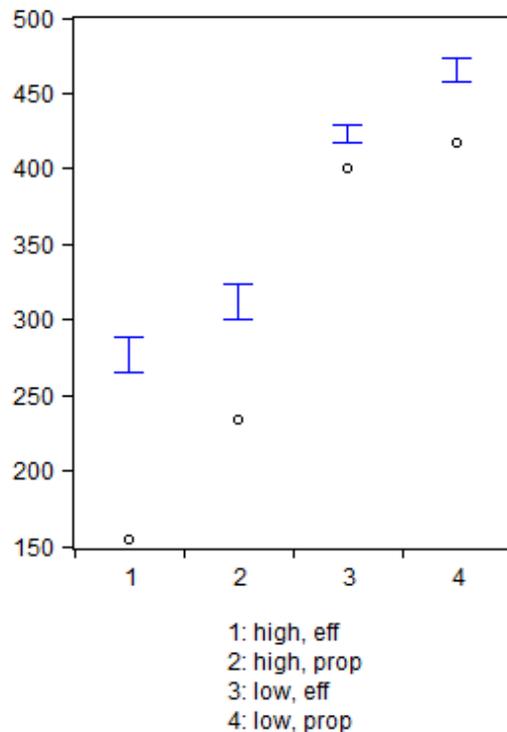


Figure 7: 99 percent confidence intervals of empirical mean prices and Nash equilibrium predictions (circles).

These results indicate that we cannot generally exclude *ex ante* that there are dynamic patterns which lead to long-run convergence to Nash equilibrium prices and profits. In order to investigate such possible dynamics, we set up two regressions. The first regression equation has the form

$$p_{i,t} = \sum_{j=1}^4 \left(\beta_j TD_j \frac{1}{t} + \gamma_j TD_j \right) + \varepsilon_{i,t}.$$

The endogenous variable $p_{i,t}$ is the individual price chosen by subject i in period t . TD_j , $j = 1, \dots, 4$, is a set of treatment dummy variables. In the first summand in the brackets on the right hand side of the equation, the treatment dummies are interacted with the inverse of the period, $1/t$. The β_j coefficients hence account for treatment-specific time trends. The γ_j coefficients measure the long-run price convergence level in treatment j . An analogous regression equation is set up for profits. In the second equation the endogenous variable is the profit subject i realizes in period t ; the right hand side of the equation remains unchanged.

Dependent Variable	Individual Price	Individual Profit
	(1)	(2)
$\beta_{high,eff}$	195.341*** (19.044)	8,139.560 (7,688.602)
$\beta_{high,prop}$	25.340 (19.763)	11,187.087 (7,978.840)
$\beta_{low,eff}$	42.505** (21.485)	-18,185.939** (8,673.913)
$\beta_{low,prop}$	43.652* (22.534)	-2,014.018 (9,097.277)
$\gamma_{high,eff}$	229.562*** (7.007)	83,319.644*** (1,591.999)
$\gamma_{high,prop}$	301.549*** (7.271)	96,701.571*** (1,652.096)
$\gamma_{low,eff}$	415.193*** (7.905)	120,575.874*** (1,796.017)
$\gamma_{low,prop}$	453.817*** (8.290)	123,536.027*** (1,883.679)
Observations	1,920	1,920
Adjusted R ²	0.258	0.181
GLS regressions with clustered errors on the individual level. Clustered standard errors in parentheses. ***, **, and * denote significance at the 1, 5, and 10 percent level.		

Table 4: Convergence of individual prices and profits: Regression results.

Generalized least squares (GLS) estimations are performed to account for possible autocorrelation and heteroskedasticity of prices set by the same subject in different periods. The regression results are presented in Table 4.¹² The β coefficient estimates in the first

¹² We also ran an extended version of the two regressions which included individual characteristics of the subjects (age, gender, and field of study) as exogenous variables. While these characteristics have no significant effect on price choices, there are two significant, albeit small, effects on realized profits: Females earn about

regression indicate that there is a significant downward time trend for prices in three of the four treatments. The price decline over time is most pronounced in the "high, eff" treatment. In the "high, prop" treatment there is no observable time trend in prices. The picture is more diverse in the second regression concerning the profit data. For the two high-cap treatments, we find a downward time trend as for prices, but the effect is not significant. In the two low-cap treatments, in contrast, profits increase over time. The time trend is significant only in the treatment with efficient rationing.

The estimates of the γ coefficients allow us to test whether prices and profits converge to their Nash equilibrium levels in the long run (t -tests). We find that the price convergence levels are significantly higher than the Nash equilibrium predictions in all treatments. Whereas the result is significant at the one percent level in the three other treatments, we observe only weak significance at the ten percent level in the "low, eff" treatment. One can speculate that this is due to the fact that the Nash equilibrium for this treatment is in pure strategies and the equilibrium price of 400 has some attraction. Indeed, the exact equilibrium price accounts for 34 percent of all price choices in the treatment. In periods 11 to 20, the share is even higher; we then observe 44 percent exact Nash equilibrium price choices.

The profit convergence levels are significantly higher than the Nash equilibrium predictions at the one percent level in the two high-cap treatments. In contrast, we cannot reject equality of the convergence levels to equilibrium profits in the low-cap treatments ($p = 0.75$ in "low, eff", $p = 0.26$ in "low, prop"). Moreover, for the "low, prop" treatment we can neither reject equality to the benchmark profit under full collusion ($p = 0.44$).¹³

On the whole, we have found very limited support for Hypothesis 4. Prices are considerably higher than the Nash equilibrium predictions and do not converge to equilibrium levels either. As to profits, these conclusions hold only for the high-cap treatments. In the treatments with low capacities, however, profits are close to their equilibrium predictions in the long run.

Result 4: With high capacities, the mean price and profit are higher than the expected mean price and profit in the respective Nash equilibrium. With low capacities, the mean price is higher than the expected mean price in the respective Nash equilibrium, but the mean profit converges to a level close to the expected Nash equilibrium profit.

Having found that the Nash equilibrium concept has little explaining power for our empirical data, we turn to the alternative off-equilibrium concepts formulated in Hypotheses 5

4,500 ECU less per round than males ($p < 0.01$, t -test), and subjects who study economics or a related subject earn about 3,600 ECU more per round than subjects with other fields of study ($p < 0.05$).

¹³ In the three other treatments, the profit convergence levels are smaller than the collusive benchmark profit at least at the five percent level. Price convergence levels are significantly smaller than the collusive benchmark price at the one percent level in all four treatments.

and 6. In order to investigate the presence of myopic best response pricing or price imitation in our data, we estimate the regression equation

$$p_{i,t} - p_{i,t-1} = \alpha + \beta(BR_i(p_{j,t-1}) - p_{i,t-1}) + \gamma(p_{j,t-1} - p_{i,t-1}) + \varepsilon_{i,t}.$$

The endogenous variable $p_{i,t} - p_{i,t-1}$ is the change of subject i 's price choice compared to her choice in the last period. $BR_i(p_{j,t-1})$ is the best response price of subject i to the price set by subject j , the other subject in the same market, in the last period. The β coefficient thus measures the share of adaptation to the best response price. In case each subject in each period sets the price which is the best response to the price she has observed in the last period, i.e. $p_{i,t} = BR_i(p_{j,t-1})$, we have perfect myopic best response pricing as Edgeworth's theory suggests. The β then takes a value of one. Imitation is included as an additional explaining factor. The γ coefficient measures the share by which subject i adapts her price choice in period t to the price chosen by subject j in period $t - 1$. The γ takes a value of one if each subject in each period mimics the price the other subject in the market has set one period before.

The regression model originates from Kruse et al. (1994), but has been enlarged to consider also imitation, such that Hypotheses 5 and 6 can be tested against each other. A similar approach is pursued in Jacobs and Requate (2016). The results from those studies indicate that one should not expect that one of the two factors completely explains empirical pricing patterns. Coefficient estimates have not been close to one for either factor. Instead, it has been observed that, depending on the exact experimental setting, only one of the factors or both factors can have a significant impact, but in any case adjustment has been only partial.¹⁴ That is, we expect that either both the β and the γ coefficient or only one of them will be significant, and that any significant coefficient will be positive, but smaller than one.

Note that our specification includes both adaptation to subject j 's price and adaptation to the best response price, which, in turn, is a function of subject j 's price. In all treatments, there is some price interval in which the best response is to underbid subject j 's price by one ECU, the smallest admissible increment. Collinearity may therefore be a serious issue with this specification. As a consequence, we consider two more regressions with a restricted version of the model in which either of the two factors is dropped, i.e. we let either $\beta = 0$ or $\gamma = 0$.

¹⁴ The Figures in Appendix B show price fluctuations in some markets, but a more or less monotone price decline in others. Note that in the "low, eff" treatment Edgeworth's myopic best response rationale does not predict price cycles either since there is a Nash equilibrium in pure strategies.

Dependent Variable	$p_{i,t} - p_{i,t-1}$		
	(1)	(2)	(3)
Restriction	None	$\beta = 0$	$\gamma = 0$
α	-1.659 (1.912)	-5.026*** (1.921)	-2.605 (1.883)
β	0.713*** (0.076)		0.513*** (0.020)
γ	-0.194*** (0.072)	0.451*** (0.019)	
Observations	1,824	1,824	1,824
Adjusted R ²	0.272	0.238	0.270

Ordinary least squares regressions. Standard errors in parentheses. ***, **, and * denote significance at the 1, 5, and 10 percent level.

Table 5: Best response pricing and imitation: Regression results.

Table 5 presents the regression results. Specification (1) allows for both myopic best response and imitation effects. Whereas the β coefficient is significant and lies between zero and one as expected, the γ coefficient is significantly *negative*. This would mean that subjects react to other prices with counter-adaptation, i.e. if a subject observes that the other firm in her market has set a lower price than her, she would raise her price, and vice versa. Such a behavioral pattern does not appear to make any sense. In synopsis with regressions (2) and (3), the results of regression (1) appear as an artifact of overfitting due to excessive collinearity in our data. The bivariate correlation coefficient between the two factors is as high as 0.965; the corresponding variance inflation factors of the two coefficients in regression (1) are around 15, which indicates considerable collinearity. If only one factor is included in the estimation, the respective coefficient is roughly equal to the sum of the two coefficients in regression (1),¹⁵ i.e. the joint effect of the two factors in regression (1) can be reproduced in the more parsimonious specifications (2) and (3). Comparing specifications (2) and (3), the best response pricing rationale supposed in regression (3) explains a higher share of the variance in the data than price imitation in regression (2). The difference between models (1) and (3) in the share of explained variance is negligible; thus, specification (3) appears to provide the best fit to the data.¹⁶

¹⁵ Letting subscripts denote the regression from which a coefficient estimate originates, we can reject the null hypothesis $\beta_1 + \gamma_1 = \gamma_2$ ($p < 0.01$, t -test), but we cannot reject that $\beta_1 + \gamma_1 = \beta_3$ ($p = 0.77$).

¹⁶ We could not find any treatment-specific effects. Substituting the respective exogenous variable in regressions (2) and (3) by interactions of this variable with dummy variables for all treatments does not improve the fit of the regressions. Neither can the hypothesis be rejected that all four treatment-specific coefficients are equal ($p = 0.22$ and $p = 0.67$, respectively, F -tests).

As to our Hypotheses 5 and 6, our conclusions have to remain reluctant. After all, the pricing patterns in our data can be explained by either myopic best response behavior or imitation (or a mixture of both). Although Edgeworth's myopic best response rationale fits the data better if only one explaining factor is considered, it does not follow that an imitation effect is absent since the two factors are highly collinear. In regressions (2) and (3), an effect caused by the omitted factor can add to the coefficient of the contained factor. Model (1) which considers both myopic best response and imitation simultaneously suffers from overfitting, such that the effects of the two factors cannot be disentangled.

Results 5 and 6: The data contain dynamic pricing patterns that can be explained by both myopic best response pricing and imitation. The effects of the two explaining factors cannot be disentangled due to the high collinearity between the factors. Confining the analysis to one factor, myopic best response pricing explains the data better than imitation.

Nevertheless, the comparison of the results from regressions (2) and (3) lets us conjecture that Edgeworthian myopic best response pricing is the prevailing factor.¹⁷ This is corroborated by the result by Jacobs and Requate (2016) that imitation becomes important only in triopolies where finding the best response is more complex whereas best response pricing is the prime driving factor in duopolies.

9 Conclusion

This study adds to the so far quite limited evidence of pricing behavior in experimental Bertrand-Edgeworth markets. It is the first to investigate the effect of demand rationing in Bertrand-Edgeworth markets with fixed exogenous capacities. The second treatment variable besides the demand rationing scheme is the amount of exogenous capacity per firm.

We find that both treatment variables entail significant and sizable effects on prices and profits. Prices and profits are higher with proportional than with efficient demand rationing and with low than with high capacities. This accords qualitatively with the Nash equilibrium predictions. However, the quantitative Nash predictions are not reflected by the data. Mean prices are significantly higher than the equilibrium level. This holds even when the Nash equilibrium predicts pure-strategy pricing. The gap between predicted and observed prices is more pronounced with high capacities. As to profits, empirical profits significantly exceed the

¹⁷ If one applies the procedure suggested by Kruskal (1987) and Lipovetsky and Conklin (2001) to attribute shares of explained variance to the factors, myopic best response pricing accounts for 15 percentage points and price imitation for 12 percentage points of the total R^2 of 0.27. The procedure is analogous to the calculation of the Shapley value in cooperative games.

equilibrium level with high capacities, but converge to the equilibrium level with low capacities.

Two alternatives to the Nash equilibrium concept are considered: Edgeworth's theory of myopic best response pricing that leads to price cycles and simple imitation of the competitor's price in the last period. Considered separately, each alternative is able to explain individual pricing dynamics partially, where Edgeworth's concept does somewhat better than simple imitation. A simultaneous analysis of the two alternative off-equilibrium concepts is inhibited by the tremendous collinearity between them. Therefore, we cannot say whether myopic best response behavior or imitation is the underlying driver of individual pricing. This shortcoming suggests that further research is needed in order to disentangle the different effects. Although collinearity between myopic best response pricing and price imitation is to some degree an inherent feature of Bertand-Edgeworth markets since for some prices it is the best response to just underbid them, a suitable parameterization might trigger more price choices outside the price range where underbidding is the myopic best response and thus allow for a clear discrimination between Edgeworth cycles and imitation.

Appendix A

English translation of the written instructions in treatment "high, eff".

Welcome to the Laboratory for Economic Experiments at Kiel University.

You are about to participate in an economic experiment where you will have to make some decisions. You can also earn some money. The amount of money will depend on both your own decisions and the decisions of the other participants in the experiment.

Please read these instructions carefully. If after reading there are any questions, please raise your hand. An experimenter will approach you and answer your questions in private. Please do not communicate with other participants during the experiment.

All participants receive the same instructions.

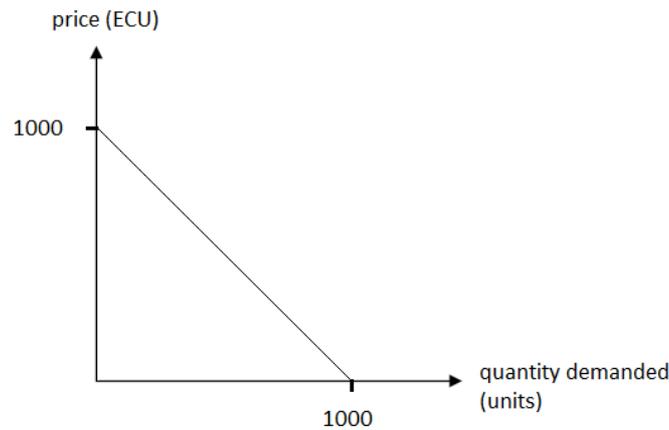
In this experiment you represent a **firm** which sells a single good. On a market you **compete with one other firm** being represented by one of the other participants. Both firms sell the same good.

The experiment consists of **20 periods**. At the beginning of the experiment, the participants will be randomly matched into groups of two. The other member of your group will be your competitor. The grouping will be fixed throughout the experiment.

In each period you and the other firm must simultaneously and independently **decide on the price** at which to offer the good on the market. All **integers from 0 up to 1000 are admissible** prices.

Throughout the experiment, all money amounts will be accounted in **Experimental Currency Units (ECU)**. Your total profit will be paid off to you after the experiment at an **exchange rate of 120,000 ECU/Euro**, i.e. for each 120,000 ECU earned in the experiment you will receive 1 Euro.

The demand side of the market will be simulated by the computer and will be identical in all periods. There are many different potential customers with different willingnesses to pay for the good. A sale will come about if the offer price is no higher than a customer's maximum willingness to pay. At a price of 0 ECU there will be a total demand for 1000 units on the market. With a price increase by 1 ECU, demand is reduced by 1 unit. At a price of 1000 ECU no one will be ready to buy the good. Here is a graphical representation of this relationship:



Customers will first try to purchase the good from the firm offering at a lower price. Those customers with the highest willingness to pay will be served first. In case one firm does not meet its whole demand, the remaining demand will be left for the other firm that offers the good at a higher price. If both firms choose the same price, demand will be divided equally among them.

Each firm can sell up to 500 units of the good in each period. Firms do not incur any costs for those units. In case a firm cannot sell all 500 units in a given period, the units that could not be sold will forfeit and cannot be transferred to the next period.

Your **profit** (in ECU) per period equals the offer **price chosen by you multiplied by your sales quantity**. Your profit thus depends on the offer prices chosen for this period by you and the other firm.

To get a general idea of your sales quantities and profits resulting from different price combinations, you may use the "**profit calculator**". Simply enter two hypothetical prices (one for each firm) and click on "Calculate". Then your resulting sales quantity and profit will be displayed. You may use the profit calculator throughout the whole experiment.

Moreover, after each period a screen with a "**history**" will inform you about the outcomes so far. For all past periods the "history" shows the prices chosen by the two firms, your resulting sales quantity, your resulting profit per period, and your total profit over all previous periods so far.

The following two screenshots illustrate the use of the profit calculator and the history:

Here you can enter two hypothetical prices and have the results calculated.

Here you can enter and submit your price choice for the current period.

Profit calculator

My price:

Other price:

Calculate

Decision

My price:

Submit

My price	Other price	My sales quantity	My profit
400	399	100.0	4000.0
400	400	300.0	120000.0
399	400	500.0	199500.0
499	400	1.0	499.0

The results of the profit calculator are displayed here.

Your total profit so far is displayed here.

The results of the last period are displayed here.

History of results

Your total profit so far: 172000.0

My price: 299

Other price: 444

My sales quantity: 500.0

My profit: 149500.0

Go to next period

Period	My price	Other price	My sales quantity	My profit
-2	0	0	0.0	0.0
-1	0	0	0.0	0.0
0	0	0	0.0	0.0
1	400	354	50.0	22500.0
2	554	350	0.0	0.0
3	299	444	500.0	149500.0

This is the history showing the outcomes of all past periods. Zeros are displayed for trial periods.

To practice there will be **three trial periods** before the actual periods start. The procedures in the trial periods are the same as in the actual periods described above, but the outcomes of the trial periods will not add to your total profit or payoff. After the trial periods, the participants will be randomly re-matched into new groups. The new groups will stay fixed throughout all actual periods of the experiment. The profit calculator will be available to you during both trial and actual periods. The history, however, will not be displayed after trial periods and will only contain the data of the actual periods.

Your **total profit** is the **sum of your profits in the single periods** of the experiment (not including the trial periods).

Your total profit will be **paid to you** after the experiment at the above-mentioned exchange rate. None of the other participants will come to know your profit, and neither will you come to know the profit of any other participant. Furthermore, no participant will come to know with whom he or she interacted during the experiment.

If you click on the "Continue" button on your screen, some statements will appear to check whether all participants have understood the instructions. Please decide whether those statements are right or wrong. As soon as all participants will have evaluated the statements correctly, the first trial period will start.

If there are any questions concerning the experimental procedures, please raise your hand. Enjoy the experiment!

Appendix B

Evolution of market prices in single markets.

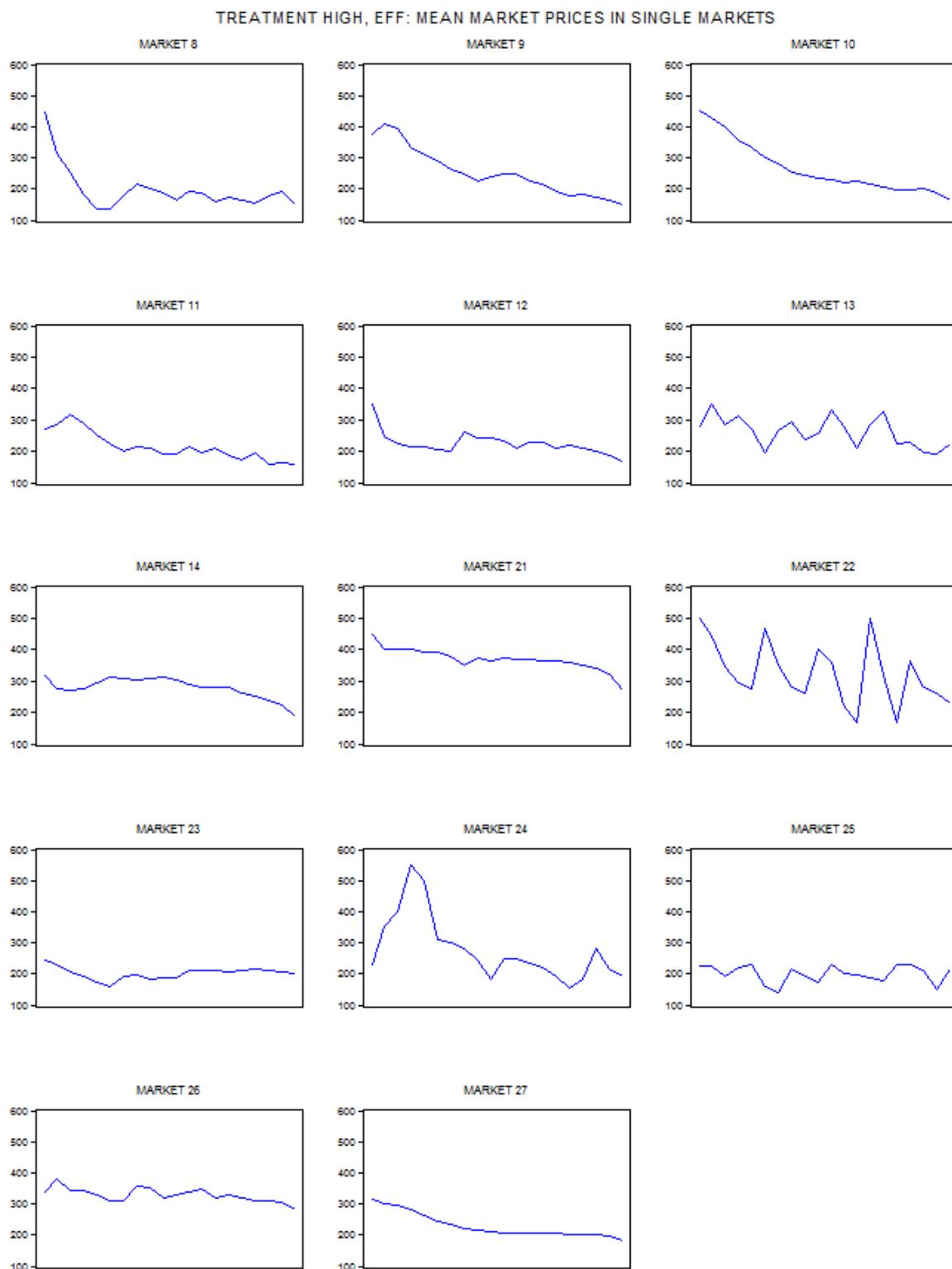


Figure B1: Development of market prices within single markets: treatment "high, eff".



Figure B2: Development of market prices within single markets: treatment "high, prop".

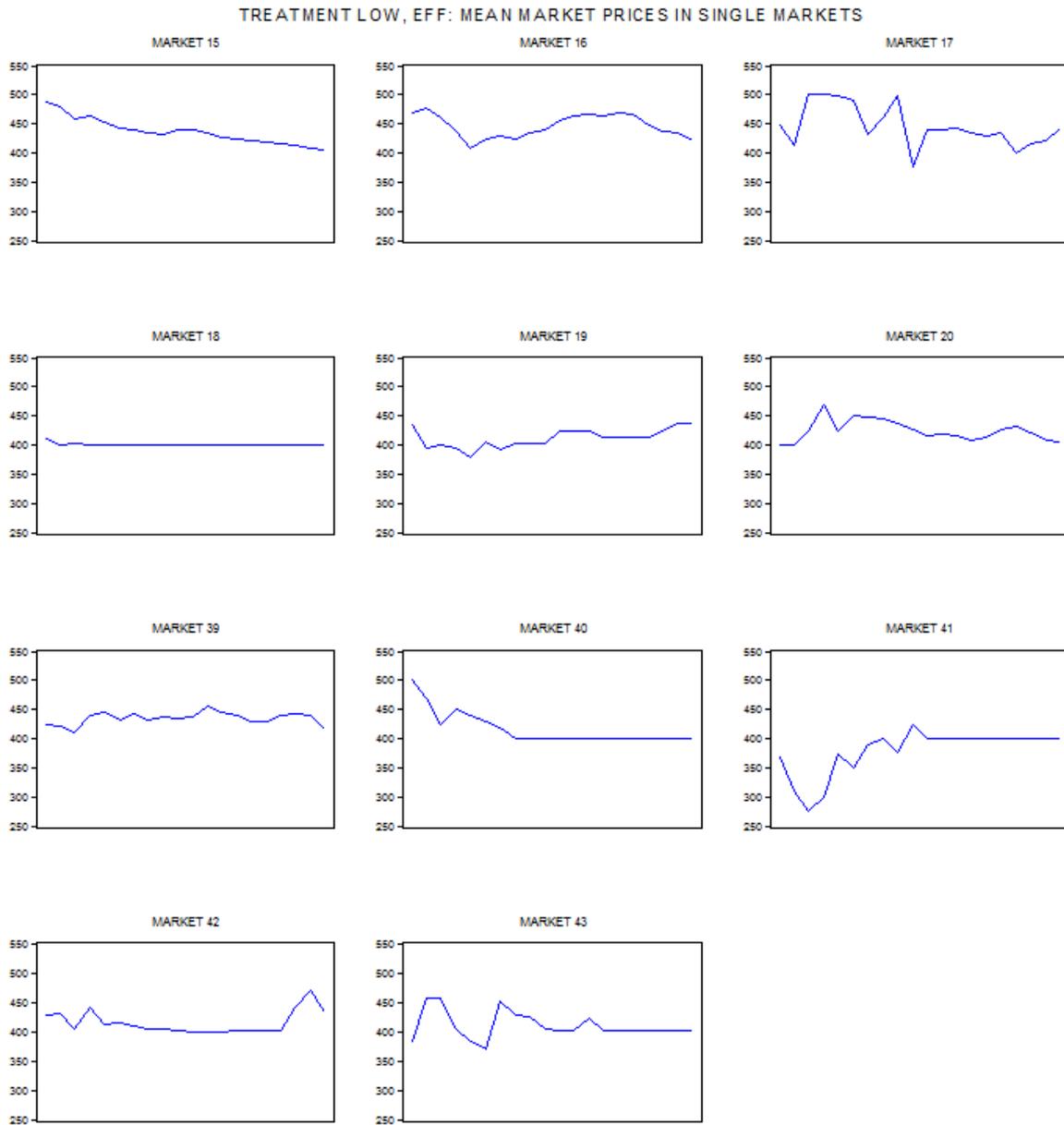


Figure B3: Development of market prices within single markets: treatment "low, eff".

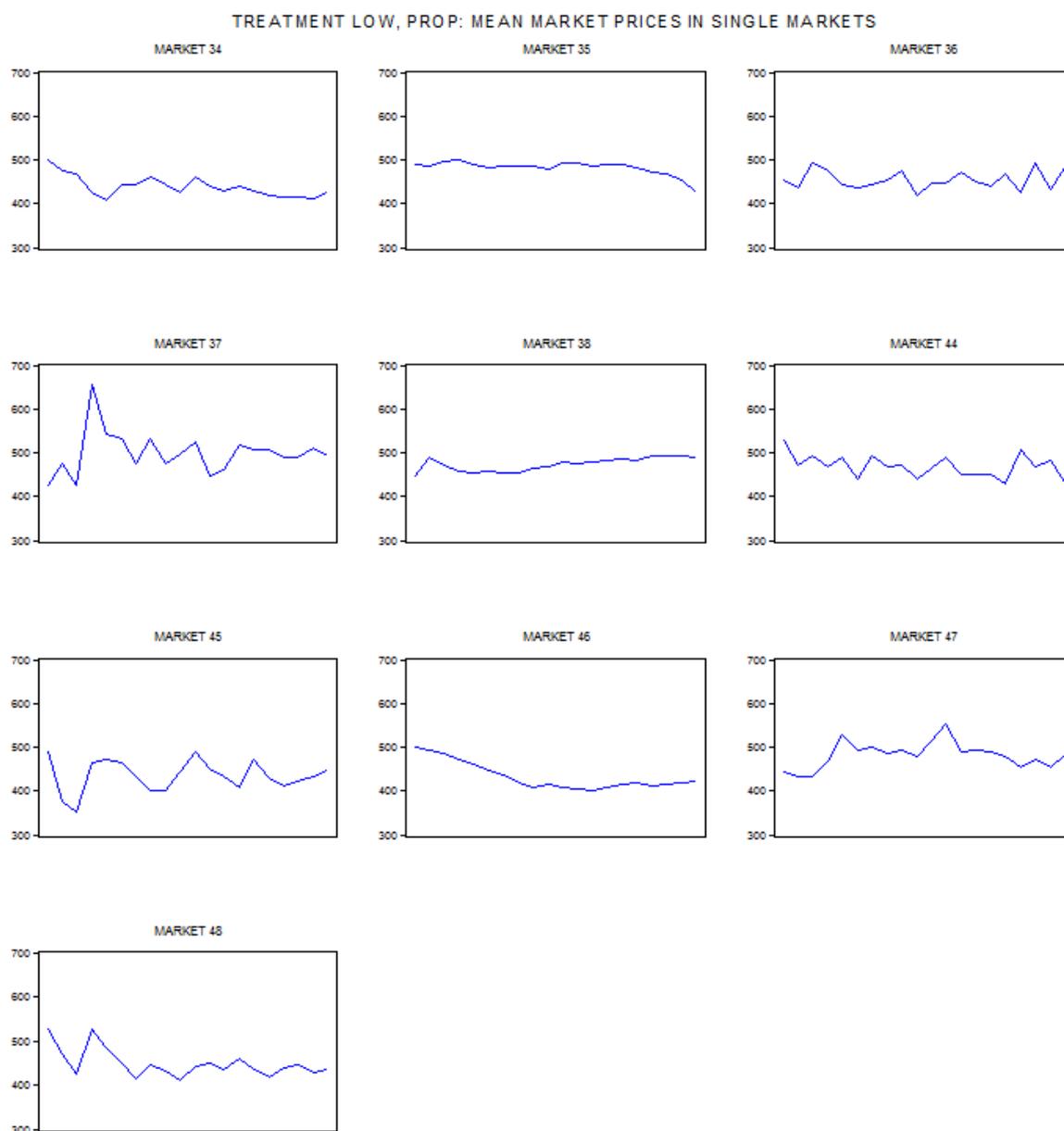


Figure B4: Development of market prices within single markets: treatment "low, prop".

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V Conclusion

The results of the single experiments have already been summarized in the introduction of this dissertation and in the concluding sections of the respective chapters. Therefore, this overall conclusion shall provide a synopsis of the three experiments and highlight some similarities and differences in our findings.

The first comment concerns the overall level of competitiveness in the three experiments as measured by the benchmarks of full competition, full collusion, and Nash equilibrium. The large differences in the degrees of competitiveness among such narrowly related market settings appear remarkable in themselves. The most competitive conduct is observed in the Kreps-Scheinkman setting where capacity choices are close to the fully competitive benchmark in most treatments. While above the fully competitive level, price choices are still rather competitive. The same is observed for price choices in Bertrand-Edgeworth markets with increasing marginal costs. By contrast, a more collusive conduct can be found when capacities are fixed. The same relationship between the two Bertrand-Edgeworth experiments holds when the Nash equilibrium of the corresponding stage game is employed as benchmark. Whereas prices are consistently below the Nash predictions in markets with increasing marginal costs, they are consistently above the Nash predictions in markets with fixed capacities.

The second significant result pertains to the effect of demand rationing, the only treatment variable considered in all three experiments in the dissertation at hand. Proportional demand rationing yields significantly higher price choices than efficient demand rationing in all three experiments. Thus, collusion is consistently fostered by rationing schemes which leave more residual demand to firms with higher prices. It is notable that this effect occurs independently of (i) which market model underlies the experiment, (ii) the degree of absolute competitiveness observed in the setting, and (iii) the direction of the predicted effect of demand rationing. The third point is especially noteworthy: We find that conduct is more

collusive under proportional rationing even in the Kreps-Scheinkman setting, where the Nash equilibrium predicts *less* collusion under proportional than under efficient demand rationing.

The third remark is about the irrelevance of subject matching. Subject matching is varied in Bertrand-Edgeworth markets with increasing marginal costs and in Kreps-Scheinkman duopolies. The effects of the subject matching are largely insignificant and always too small to be regarded as relevant determinants of the competitiveness level. At first sight, this may appear as a non-result, but it might actually have more relevant implications than the large and significant effect of demand rationing since the stability of the composition of oligopoly markets can – via institutional regulations which lead to more or less severe entry barriers – be influenced by policy whereas this is hardly possible for the sort of demand rationing. The results imply that, at least in the artificial laboratory setting where subjects are anonymous and the only possibility to communicate is signaling via the choice variable(s), it does not matter whether the same firms interact repeatedly or the composition of firms is changed, provided that the total number of firms in the market remains constant. This gives a hint that frequent invasions of new firms into a market need not lead to a more competitive market if old firms are crowded out by the entrants such that the total number of firms does not change.

The last comment considers the dynamics of behavior. The dynamics cannot satisfactorily be explained by convergence to the Nash equilibrium. While prices in Bertrand-Edgeworth markets with fixed capacities and both capacities and prices in the Kreps-Scheinkman setting come closer to equilibrium in later than in the initial periods, our estimations reject complete convergence to equilibrium. In Bertrand-Edgeworth markets with increasing marginal costs, prices even depart more from equilibrium in later periods. Myopic best response pricing and simple price imitation are investigated as alternative off-equilibrium explanations for the observed dynamics in the two Bertrand-Edgeworth experiments. In the setting with fixed capacities the conclusions remain ambiguous. With increasing marginal costs both alternative theories entail predictive power, and it crucially depends on the number of firms in the market which theory is the prime explaining factor. The results suggest that there is no simple and general answer to the question what subjects do instead of playing the Nash equilibrium when finding the equilibrium becomes too demanding. Rather, the choice which alternative rationale to follow depends on further factors of the setting as those further factors can possibly render even some simpler off-equilibrium behavioral rules too complex to be played.

Erklärung zum selbstständigen Verfassen der Arbeit

Ich erkläre hiermit, dass ich meine Doktorarbeit "Price Competition in Oligopolies with Demand Rationing: Experimental Studies" selbstständig und ohne fremde Hilfe angefertigt habe und dass ich als Koautor maßgeblich zu den weiteren Fachartikeln beigetragen habe. Alle von anderen Autoren wörtlich übernommenen Stellen, wie auch die sich an die Gedanken anderer Autoren eng anlehnenden Ausführungen der aufgeführten Beiträge, wurden besonders gekennzeichnet und die Quellen nach den mir angegebenen Richtlinien zitiert.

Datum

Unterschrift