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Stackelberg beats Cournot — On collusion and efficiency in experimental markets*

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Abstract

We report on an experiment designed to compare Stackelberg and Cournot duopoly markets with quantity competition. For each market we implement both a random matching and a fixed-pairs version. Stackelberg markets yield, regardless of the matching scheme, higher outputs than Cournot markets. Under random matching total quantities in Stackelberg markets are higher than predicted by the subgame perfect equilibrium while total quantities in the Cournot markets match the equilibrium prediction. Under fixed pairs aggregate output for both, Cournot and Stackelberg, is lower than under random matching, but there is less collusion on Stackelberg markets and, hence, higher efficiency.

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1. Introduction

The Stackelberg (1934) model is among the most frequently applied models of oligopolistic interaction. In duopoly, it refers to a situation in which one firm, the Stackelberg leader, can commit to its output first.¹ The second mover, the Stackelberg follower, produces his quantity knowing the output of the Stackelberg leader.² Actual markets may indeed exhibit such sequential order of moves. Incumbency, sequential entry, R&D races—all these phenomena can, even if in a simple fashion, be captured by the Stackelberg model.

An important implication of the Stackelberg model is that it improves market efficiency. Daughety (1990) considers a parameterized class of Stackelberg markets and shows that all sequential-move structures are beneficial in comparison with simultaneous-move Cournot. The intuition of this result is simple. Switching from a Cournot to a Stackelberg market and holding the number of firms constant³ increases aggregate output. While there is a loss in the sum of profits, the gain in consumers' surplus more than compensates for this loss such that total welfare increases. Concentration measures (like the Herfindahl index) increase in the Stackelberg case due to the asymmetry but it is precisely the sequentiality of moves that leads to the increase in welfare.

The purpose of our paper is to explore the basic consequences of a Stackelberg structure in an experimental market. To our knowledge, this has not been done yet. Our special interest is directed to the question whether observed efficiency relations will resemble the theoretically predicted ones. Therefore, we analyze both, Stackelberg and Cournot duopolies, and we have two treatments: In the first, subjects are randomly matched in every period such that interaction is one-shot. In the second, pairs of subjects play together for the entire course of the experiment such that repeated-game effects can arise.

¹Note that a sequential order of moves is today's interpretation of Stackelberg's model. Stackelberg's original idea was a behavioral difference between the firms. The Stackelberg follower is a firm which reacts according to the Cournot best reply logic. The Stackelberg leader realizes this and takes advantage of the adaptive behavior of the follower. See Stackelberg (1934, pp.16–24).

²In most models (and in our experiment) the order of moves is exogenously fixed. Recently, however, there has been some interest in the literature in investigating under which conditions a sequential move Stackelberg game results *endogenously*. See Robson (1990) and Hamilton and Slutsky (1990). A recent paper by van Damme and Hurkens (1996) contains further references. Currently, we conduct a new series of experiments exploring this issue.

³Daughety (1990) analyzes a generalized n -firm Stackelberg oligopoly with $m \leq n$ Stackelberg leaders and $n - m$ Stackelberg followers.

In Cournot duopolies, experimental results confirm the theory very well, though this depends on the matching scheme. Generally speaking, most papers with random matching, see e.g. Fouraker and Siegel (1963) or Holt (1985), report convergence to the Cournot–Nash equilibrium. Holt observes no successful collusion in a ten–period Cournot duopoly market with random matching. Instead he observes that most choices coincide with the quantity predicted by the Nash equilibrium. This is contrasted by Holt’s findings for repeated Cournot settings with fixed pairs of participants where play often converges to the collusive outcome.⁴ Will Stackelberg markets exhibit the same pattern? This is one of our main questions. There are reasons to assume that they might not. First of all, there are *many* equilibria in a Stackelberg game and one has to apply the notion of subgame perfection to single out a unique solution. In this sense the Stackelberg prediction relies on a higher degree of rationality.⁵ Secondly, as it is known from the ultimatum bargaining literature (for surveys see Güth (1995) and Roth (1995)), many subjects exhibit an aversion against disadvantageous inequality. If so, second movers may, when confronted with first movers trying to exploit their strategic advantage, deviate from subgame perfect play and engage in costly punishments. In other words, if fairness plays a role on Stackelberg markets this may induce deviations from the standard theoretical prediction.⁶

While fairness considerations may cause the theory to fail in experimental Stackelberg markets, the sequential order of moves could also be an obstacle to collusion. Theoretically, collusion should be easier to establish in Stackelberg than in Cournot markets as that the sum of profits in the subgame perfect Stackelberg equilibrium is lower than in the Cournot equilibrium. Hence, the surplus from collusion is larger and one can find distributions of the jointly maximized profits such that the incentives to deviate from collusion are lower than in Cournot markets. However, this effect may be offset by the asymmetries that are involved when joint profits are maximized in a Stackelberg market. There is no way to avoid of inequality—either with respect to actual profits or with respect to gains. If actual profits are balanced, the follower will gain from collusion much more than the leader.⁷ If gains are balanced, the leader will have higher total profits than the follower. Thus, inequality aversion may not only cause the failure of the subgame–perfect equilibrium prediction in single–period games but may also prevent collusive outcomes in repeated games.

According to these considerations the impact of the leader–follower structure on efficiency in experimental markets is ambiguous. On the one hand, fairness

⁴While there are many more papers on Cournot duopoly, there are only few experiments with more than two firms. Fouraker and Siegel (1963) ran triopoly experiments. Recently, Rassenti *et al.* (1996), Huck, Normann, and Oechssler (*in press*), and Offerman, Potters and Sonnemans (1997) conducted Cournot oligopoly experiments with more than three firms.

⁵Alternatively, one could first eliminate dominated strategies and then solve for the equilibrium.

⁶For more experimental evidence and a theoretical framework capturing this evidence, see Fehr and Schmidt (*in press*) or Bolton and Ockenfels (1998).

⁷With linear demand and cost (which we used in our experimental markets), the Stackelberg leader has no incentive to engage in such symmetric collusion. All of the extra profit goes to the follower whereas the Stackelberg leader’s profit remains constant.

effects might induce the Stackelberg leader to refrain from exploiting his first-mover advantage. Then the welfare advantage of Stackelberg markets might be smaller than theoretically expected. On the other hand, if the leader-follower structure proves to be an obstacle to collusion, consumers' surplus and welfare could be higher when firms decide sequentially.

In our experiment, we find that asymmetry increases the level of output. Stackelberg markets yield, regardless of whether subjects are randomly matched or play in fixed pairs, higher outputs, higher consumer rents and higher welfare levels than Cournot markets. Under random matching Stackelberg markets yield total quantities which are even higher than theoretically expected while Cournot markets match the theoretical predictions very accurately.

Under fixed pairs aggregate output is lower than under random matching. This holds for both, Cournot and Stackelberg. But there is much less collusion on Stackelberg markets and, hence, they yield again higher efficiency.

The remainder of the paper is organized as follows: Section 2 introduces the markets which are explored experimentally and presents the basic experimental design as well as the theoretical predictions. Section 3 is concerned with the experimental procedures, while Section 4 first presents the key research questions and then the experimental results. Section 5 concludes.

2. Markets, treatments, and theoretical predictions

In a series of experiments we study two homogeneous duopoly markets with quantity competition, the Stackelberg and the Cournot duopoly. In both markets the two firms, firm 1 and firm 2, face linear inverse demand

$$p(Q) = \max\{30 - Q, 0\}, \quad Q = q_1 + q_2, \quad (2.1)$$

while linear costs are given by

$$C_i(q_i) = 6q_i, \quad i = 1, 2. \quad (2.2)$$

The two markets differ in the timing of decisions. In the Cournot market firms decide simultaneously. Nash equilibrium play implies $q_i^C = 8$, $i = 1, 2$. In the Stackelberg market firms choose their quantities sequentially. First, the Stackelberg leader (L) decides upon his quantity q^L , then—knowing q^L —the Stackelberg follower (F) decides upon his quantity q^F . The subgame perfect equilibrium (SPE) solution is given by $q^L = 12$ and $q^F = 6$.⁸ Finally, joint profit maximization implies an aggregate output of $Q^J = 12$. On a symmetric Cournot market, one would expect—if collusion is observed at all—to observe the symmetric joint profit maximizing outputs $q_1^J = 6$ and $q_2^J = 6$. An overview over all relevant predictions concerning quantities, consumers' surplus and total welfare is given in Table 2.1.

As mentioned, we study random matching as well as fixed pairs. This creates a 2×2 -design as shown in Table 2.2. We explore Stackelberg and Cournot

⁸Note that this is not the complete description of the SPE as it does not specify the follower's strategy completely. This is given by $q^F(q^L) = 12 - \frac{q^L}{2}$.

	Cournot	Stackelberg	Collusion
Individ. Quantities	$q_i^C = 8$	$q^L = 12; q^F = 6$	$(q_i^J = 6)_{sym.}$
Total Quantities	$Q^C = 16$	$Q^S = 18$	$Q^J = 12$
Profits	$\Pi_i^C = 64$	$\Pi^L = 72; \Pi^F = 36$	$(\Pi_i^J = 72)_{sym.}$
Consumers' Surplus	$CS^C = 128$	$CS^S = 162$	$CS^J = 72$
Total Welfare	$TW^C = 256$	$TW^S = 270$	$TW^J = 216$

Table 2.1: Theoretical predictions

markets—each under both matching schemes. As the number of participating subjects shown in Table 2.2 indicates, the Cournot markets serve mainly as a control treatment while our main focus is on the Stackelberg markets.

The above Nash equilibrium solution for the Cournot market and the subgame perfect solution for the Stackelberg market apply—from a game theoretic point of view—to a situation in which these games are played only once. Hence, these are the predictions for sessions in which we matched subjects randomly. Our matching scheme ensured that no subject would meet any other subject twice. The participants were informed about this in the instructions. They were also informed about the exact number of repetitions. With random matching no rational punishments or any other form of repeated interaction was possible.

When fixed pairs of subjects interact over several periods, collusion may arise. Theory requires an infinite horizon to make collusion possible. In experiments this theoretical requirement is often met by using a random stopping rule for the termination of the experiment. However, as for example Selten *et al.* (1997) point out this can be problematic since an infinite horizon cannot credibly be implemented in the lab. Moreover, in experimental markets with fixed pairs collusive play is quite frequently observed even with a fixed horizon and is typically maintained till the second to last period (see, e.g., Selten and Stoecker, 1983). Accordingly, we preferred a commonly known finite horizon for both matching schemes.

3. Methods and procedures

The experiments reported here were conducted at Humboldt University in June and July 1998. One hundred and thirty-four subjects participated in altogether seven sessions. They were students from various fields, mainly students of eco-

	Random Matching	Fixed Matching
Stackelberg	STACKRAND (44 = 24 + 20)	STACKFIX (48 = 18 + 14 + 16)
Cournot	COURRAND (20)	COURFIX (22)

Table 2.2: The 2-by-2 factorial design of markets and matching procedures and the numbers of subjects participating in the four treatments.

nomics, business administration and law.⁹

The experiments were run in large lecture rooms with pen and paper. Subjects were seated with enough space between them to prevent communication. After having read the instructions, participants were allowed to ask questions privately to the experimenters. In the Stackelberg treatments player positions were randomly assigned to subjects and were kept constant during the whole session. All sessions consisted of ten rounds with individual feedback between rounds. Sessions lasted between 60 and 75 minutes. Subjects' average earnings were DM 15.67 (including a show-up fee of DM 5) which is about \$ 9.

In the instructions (see Appendix A) subjects were told that they will act as a firm which together with another firm produces one and the same product and that in each round both have to decide which quantity to produce. Depending on whether subjects were in a FIX treatment or in a RAND treatment, they were informed about the kind of matching as explained above.

Participants got a payoff table (see Appendix B) in which all possible combinations of quantity choices and the according profits were shown. The numbers given in the payoff table were measured in a fictitious currency unit called "Taler". Each firm could choose a quantity from the set $\{3, 4, \dots, 15\}$. The payoff table was generated according to the demand and the cost functions given in (2.1) and (2.2). Due to the discreteness of the strategy space such a payoff table typically induces multiple equilibria (see Holt, 1985). To avoid this, the bi-matrix representing the payoff table was slightly manipulated. By subtracting one Taler in 14 of the 169 entries we could ensure uniqueness of the Cournot-Nash equilibrium and of the subgame perfect Stackelberg equilibrium as given in Table 2.1.

Subjects were informed that at the end of the experiment two of the ten rounds would be randomly selected in order to determine the actual monetary profit in German marks. The latter was computed by using an exchange rate from 10:1 and adding a flat payment of DM 5 for showing up.¹⁰ Before the first round started, subjects were asked to answer a control question (which was checked) in order to make sure that everybody had full understanding of the payoff table.

⁹Subjects were either randomly recruited from a pool of potential participants or invited by leaflets distributed around the university campus.

¹⁰The show-up fee was relatively high as subjects could have made losses in the game.

For the Stackelberg treatments the firms were labelled A (Stackelberg leader) and B (Stackelberg follower). In each of the ten rounds the Stackelberg leaders received a decision sheet on which they had to note their code number and their decision by entering one of the possible quantities in a box. Then these sheets were passed on to the subjects acting as followers.¹¹ Followers had to enter their code number, too, and then made their decision on the same sheet. Doing so they immediately had complete information about what happened in the course of the actual round. Afterwards the sheets were collected and passed back to the Stackelberg leaders which now were also informed about this round's play.¹² After recollecting the sheets the next round started.

For the Cournot markets no labels were assigned to firms. The instructions simply used the words “you” and the “other firm”. In each of the ten rounds all subjects received a perforated two-part decision sheet on which they twice had to enter their code number and their decision. Afterwards the two parts of the sheet were separated. One part was collected by the experimenters, the other part was kept by the subjects. The parts of the decision sheets collected by the experimenter were then (according to the matching scheme of the session) passed on to the respective subjects. Thus, all subjects immediately had full information about what happened in this round. After collecting all sheets the next round started.

4. Experimental Results

We start this section by stating the key questions we will analyze.

Question 1 Do we replicate earlier results on Cournot duopolies, i.e. static Nash equilibrium play for random matching and partial collusion for fixed pairs?

Question 2 Will there be a similar pattern in Stackelberg games, i.e. static sub-game perfect equilibrium play with random matching and partial collusion with fixed pairs?

Question 3 Will Stackelberg markets yield higher outputs at smaller prices than Cournot markets, thus increasing total welfare?

Question 4 How will behavior change over time?

Table 4.1 provides essential summary statistics on an aggregate level for all treatments.¹³ In the following we will typically either work with average data (taking into account that many observations are not independent from each other) or with data from round 9 when subjects have gathered a lot of experience. Like

¹¹Subjects were not able to observe how the leaders' decision sheets were allocated to the followers. After collecting the sheets from the Stackelberg leaders one experimenter left the room to bring the sheets in the final order.

¹²Again one of the experimenters left the room with the decision sheets.

¹³More detailed information is given in Tables C.1 and C.2 in the Appendix. Table C.1 shows for each round mean individual quantities and mean industry outputs while Table C.2 shows the distribution of individual quantities aggregated over all ten rounds and in parentheses the distribution for round 9 only.

	STACKRAND	STACKFIX	COURRAND	COURFIX
Average Indiv. Quant.	10.19 / 8.32	9.13 / 7.92	8.07	7.64
Average Total Quant.	18.51	17.06	16.14	15.27
Average Total Profits	93.48	105.01	122.12	122.55
Average Cons. Surpl.	175.37	152.14	132.62	122.00
Average Total Welf.	268.85	257.16	254.74	244.55

Table 4.1: Aggregate data. (Note that for the Cournot markets under random matching average profit and surplus depend on the actual matching.)

Fouraker and Siegel (1963) and Holt (1985) we prefer the second to last round to the last one due to possible (and actual) end game effects. From Table 4.1 it can be seen that the following relation between the four treatments holds:

$$Q^{\text{STACKRAND}} > Q^{\text{STACKFIX}} > Q^{\text{COURRAND}} > Q^{\text{COURFIX}}. \quad (4.1)$$

This implies that the same relation holds for welfare levels. While this already provides a partial answer to one of our research questions, let us proceed step by step.

Question 1. Do we replicate the basic results on Cournot duopolies in our experiment? The answer is yes, we do. Under random matching quantities are right from the start and till the end very close to 8 (see Tables C.1 and C.2). In contrast, average quantities under the fixed-pairs treatment are usually below 8 and the modal choice is the collusive quantity 6. Over all rounds the collusive action is chosen in more than 40% of all instances and in round 9 half of the decisions are collusive. Comparing collusion rates (defined by the number of successfully colluding pairs) it shows that there is a highly significant difference between the two matching schemes ($p = .015$ in round 9). In all, the results are virtually the same as in Holt (1985).

Dealing with the Cournot data we also get a first answer to Question 4 concerning behavior over time. As we already pointed out, this is very stable under random matching. On the other hand, we find with fixed pairs a slow and slightly downwards trend in quantities but also a clear end effect with average quantities rising and collusion rates dramatically dropping.

We summarize these observations in

Result 1. *Behavior on Cournot markets depends crucially on the matching scheme. As in earlier studies we find stable equilibrium play under random matching*

and partial collusion with fixed pairs. However, collusion virtually breaks down in the last round.

Question 2. Can we find a similar behavioral pattern in the Stackelberg data? The answer to this question is both, yes and no. It is no with respect to the SPE prediction for random matching. Tables 4.1 and C.1 show that average quantities chosen by the Stackelberg leaders are distinctly away from the SPE prediction. Over all rounds they produce on average nearly two units less than predicted. Furthermore, there is no trend whatsoever towards the “rational” exploitation of the first mover advantage. How can this be explained? As indicated by the inverted commas in the previous sentence, producing 12 units is only rational if the Stackelberg followers play standard best replies but this is not the case—neither in the earlier nor in the later rounds of the experiment. Estimating the followers’ actual response function by a simple linear regression model for the random matching treatment, one gets $q^F = 10.097 - 0.175q^L$ which is far away from the standard rational best reply function, $q^F = 12 - 0.5q^L$. Given the empirical response function we next examine whether the Stackelberg leaders adapt to it, i.e., whether they choose a best reply to this function. Interestingly, they do not since the payoff maximizing strategy against the empirical response function of the followers would have been $q^L = 8.422$. It is interesting how close this number is to the Cournot quantity. In fact, it seems that Stackelberg followers try to educate the leaders to play symmetric Cournot instead of exploiting their first-mover advantage. This is illustrated by the fact producing $q^L = 12$ yielded an average profit of 44.60 for the Stackelberg leaders, while producing $q^L = 8$ yielded one of 60.42.

All this gives rise to the conclusion that other motivations besides earning absolute money must play a role in any explanation of the Stackelberg data. This is not surprising. As discussed in Section 2, it is well-known that asymmetric games like the one at hand frequently invoke fairness concerns. The Stackelberg followers clearly exhibit a kind of inequality aversion while the Stackelberg leaders seem to compromise between exploiting their theoretical advantage and adjusting fully to the response pattern of followers. This is also well-known from ultimatum bargaining data (see, e.g., Güth (1995) and Roth (1995)).¹⁴

Before comparing the random matching data with the fixed pairs data, it is worthwhile to have a look at Table C.2 which shows three important things. First, behavior is quite dispersed and a closer analysis easily shows that it varies considerably among Stackelberg leaders and among followers. Second, though leaders’ average quantity is smaller than predicted by the subgame perfect equilibrium ($q^L = 12$), the mode of the Stackelberg leaders’ choices over all rounds is given by it. However, for more experienced Stackelberg leaders this does not hold any longer which seems to be a consequence of experienced punishments.¹⁵

¹⁴Note that Stackelberg followers can punish (or give) leaders very effectively. For example, after observing a quantity of 12 the follower loses only one payoff unit by producing one unit more (less) than prescribed by the best response function. The leader, on the other hand, suffers a loss (or gains a win) of 12 payoff units in that case.

¹⁵While Stackelberg leaders seem to learn over time, the empirical response function of followers remains more or less stable. Again this can be compared to findings in ultimatum games

Third, it is remarkable that there were virtually no attempts of collusion. The latter two observations are crucial for the comparison with the fixed-pairs treatment where the pattern is roughly the other way round: There is a considerable amount of attempted collusion¹⁶ and choices in line with the SPE are not the mode. These are the first two reasons why one part of the answer to Question 2 is yes instead of no. The next reason can be seen by inspecting Table 4.1 again: Average total quantities are 1.5 units smaller when pairs are fixed. Looking at separate rounds this difference is also highly significant, especially in the end (round 9, $p = .004$). To understand these patterns we again estimate the followers' empirical response function. A regression yields $q^F = 6.413 + 0.165q^L$. Note that the plus sign is not a typo. The empirical response function under fixed pairs is indeed upward sloping. Interestingly, it intersects with the rational best response function at $(q^L, q^F) = (8.4, 7.8)$, i.e. when Stackelberg leaders play Cournot, followers react with Cournot. If quantities are below Cournot they produce less than the payoff-maximizing quantity, if quantities are above they produce more. In other words, the empirical response function reveals a clear-cut reward-for-cooperation and punishment-for-exploitation scheme. We summarize by

Result 2. *In Stackelberg markets under random matching, behavior does not settle down on the theoretical prediction—in contrast to Cournot markets. Instead, behavior of Stackelberg leaders seems to be a compromise between the SPE and the symmetric Cournot equilibrium prediction whereas Stackelberg followers produce on average about one unit more than predicted by the best reply function. However, as on Cournot markets, behavior on Stackelberg markets becomes considerably less competitive when pairs are fixed. This is mainly driven by an upward sloping response function of the followers which rewards cooperation and punishes exploitation.*

Question 3. In Question 3 we asked whether Stackelberg markets exhibit in an experiment the same welfare advantage over Cournot markets they exhibit in theory. The obvious answer is, yes, they do. The difference in average total output is nearly 2.5 units under random matching and roughly 1.5 units under fixed pairs. Total welfare increases from 254.74 to 268.85 under random matching and from 244.55 to 257.16 when pairs are fixed. This can also be statistically validated. For the fixed-pairs treatment we can compare average welfare levels by taking each pair as one observation. Here, the significance level is $p = .053$ (one-sided Mann-Whitney-U test). This procedure cannot be applied for the random-matching data as the average observations based on pairs are not independent. However, we can do comparisons between STACKRAND and COURRAND by analyzing each round separately and, in fact, in nearly all rounds the significance levels are below 5%.¹⁷ Moreover, Stackelberg markets yield even under a fixed-pair matching

where the learning dynamics seem to be driven by faster adjustments on the side of proposers.

¹⁶Again a comparison of the collusion rates in the two Stackelberg treatments shows that this difference is significant ($p = .045$ in round 9).

¹⁷The p -values are: .059; .026; .032; .006; .000; .055; .102; .006; .000; .005 (all with one-sided MWU).

scheme higher total outputs than Cournot markets do in theory (and in the lab). Thus, we have

Result 3. *Stackelberg markets yield higher welfare than Cournot markets. This is independent from the matching scheme.*

Question 4. To conclude this section we answer the last question concerning the behavior over time in our experiment. First of all inspection of Table 4.1 reveals that already first round behavior is rather sophisticated and that the relations given in (4.1) hold. Besides that it is remarkable that virtually all decisions in all rounds of all treatments are rationalizable¹⁸ which shows that subjects must have understood the rules of the game from the very beginning pretty well.

Moreover, the data show that there is not much learning going on in markets with random matching. Behavior in these markets is fairly stable over the rounds. On markets with fixed matching, however, we observe that quantities decrease over time. It seems that punishments in treatment STACKFIX reduce the number of the Stackelberg leaders' attempts to exploit their first-mover advantage. The decline of quantities in treatment COURFIX is due to the increasing number of successfully colluding pairs. Moreover, in this treatment, where collusion works best, there is a noticeable end-game effect. This is summarized in

Result 4. *Whereas behavior in random matching markets is quite constant over time, we observe decreasing total quantities over rounds in markets with fixed matching.*

5. Conclusion

Many economists and especially competition practitioners are worried by concentration in general and dominant firms in particular. Daughety (1990) shows that such concerns about concentration are not warranted if concentration results from asymmetry. We find support for Daughety's point in our experiments. In Stackelberg duopolies aggregate output is higher than in Cournot duopolies. Though Stackelberg leaders do not exploit their first-mover advantage as strongly as the theory predicts, Stackelberg markets exhibit higher welfare levels. Hence, not only theory but also experimental markets suggest that the Stackelberg leader-follower structure is beneficial for welfare.

Our results can nicely be compared with two other experimental studies in the literature. Asymmetric Cournot oligopoly has been the subject of experimental testing in Rassenti *et al.* (1996) and Mason, Phillips, and Nowell (1992). Both studies concentrate on cost asymmetries while we focus on an asymmetric (sequential) order of moves. Rassenti *et al.* (1996) find no convergence to equilibrium quantities at the individual level, only at the aggregate level. Since they do not conduct a reference treatment with symmetric firms, it is difficult to assess the impact of the asymmetric costs in their experiment. Mason, Phillips and Nowell (1992) compare a treatment with cost differences with a symmetric treatment.

¹⁸In COURRAND, for example, only 1% of all cases subjects chose a never-best reply.

They find that in asymmetric duopolies there is a higher level of output as compared to symmetric duopolies. Note that, in contrast to our results, this increase in output is not predicted by the theory.

There remain some open questions. The first is whether our findings are robust to a greater number of firms deciding on the two stages or deciding in a more complicated sequence. Our guess is that such structures would still be more efficient than a Cournot market with the same number of firms. The second is what would happen if there were different kinds of asymmetries, i.e. what would happen if we introduced in our setup additional cost asymmetries. Here it is much more difficult to formulate conjectures, especially when the cost advantage favors the second mover. Finally, it would be interesting whether one can observe the emergence of Stackelberg leadership in experimental markets. As mentioned in the introduction there has recently been a theoretical interest in endogenous Stackelberg leadership tackling the question how one can explain that one firm becomes strong and another weak. In a subsequent study we plan to explore whether these explanations are valid in laboratory settings.

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APPENDIX

A. Translated Instructions

Welcome to our experiment! Please read these instructions carefully! Do not talk to your neighbors and be quiet during the entire experiment. If you have a question, give notice. We will answer them privately.

In our experiment you can earn different amounts of money, depending on your behavior and that of other participants who are matched with you.

You play the role of a firm which produces one and the same product as another firm in the market. Both firms always have to make a single decision, namely which quantities they want to produce. In the attached table, you can see the resulting profits of both firms for all possible quantity combinations.

The table is read as follows: The head of the row represents one firm's quantity (*A*-firm) and the head of the column represents the quantity of the other firm (*B*-firm). Inside the little box at which row and column intersect, the *A*-firm's profit matching this combination of quantities stands up to the left and the *B*-firm's profit matching these quantities stands down to the right. The profit is denoted in a fictitious unit of money, which we call Taler.

So far, so simple. But how do you make your decision? Take a look at your code-number: if it starts with an *A*, you are an *A*-firm, if it starts with a *B*, you are a *B*-firm. The procedure is that the *A*-firm always starts. This means that the *A*-firm chooses its quantity (selects a line in the table) and the *B*-firm is informed about the *A*-firm's choice. Knowing the quantity produced by the *A*-firm, the *B*-firm decides on its quantity (selects a column in the table). The *B*-firm then of course already knows her own profit. The *A*-firm will be informed about it (or rather *B*'s choice). The decisions are marked on a separate decision-sheet, which we will hand out to all participants with role *A* soon.

The table is read as follows: The head of the row represents your firm's quantity and the head of the column represents the quantity of the other firm. Inside the little box at which row and column intersect, your profit matching this combination of quantities stands up to the left and the other firm's profit matching these quantities stands down to the right. The profit is denoted in a fictitious unit of money, which we call Taler. You and the other firm decide simultaneously about the quantities.

After each round you will be informed about the quantity of the other firm. The decisions are marked on a separate decision-sheet which we will hand out soon.

This procedure is repeated over ten rounds. You do not know with which participant you serve the market. You will be matched with a different participant each round and we will ensure that you will be matched with ten different participants during the ten rounds.

This procedure is repeated over ten rounds. You do not know, with which participant you serve the market, but: you will stay matched with the same participant during all rounds.

The following
two ¶ only in
STACK
treatments.

The following
two ¶ only in
COUR
treatments.

This ¶ only in
RAND
treatments.

This ¶ only in
FIX treatments.

During the entire experiment, anonymity among participants and instructors will be kept since your decisions will only be identified with your code number. Therefore you have to carefully keep your code card. Only upon showing the code card you will later receive your payment.

Concerning the payment, note the following: At the end of the experiment two of the ten rounds will be randomly chosen to count for payment. The sum of your profits in Taler of (only) these two rounds determines your payment in DM. For each ten Taler you will be paid 1 DM. In addition to this you will receive 5 DM independent of the course of the ten rounds.

B. Payoff table¹⁹

Quant.	3	4	5	6	7	8	9	10	
3	54 54	51 68	48 80	45 90	42 98	39 104	36 108	33 109	
4	68 51	64 64	60 75	56 84	52 91	48 96	44 99	40 100	
5	80 48	75 60	70 70	65 78	60 84	55 88	50 89	45 90	
6	90 45	84 56	78 65	72 72	66 77	60 80	54 81	48 80	
7	98 42	91 52	84 60	77 66	70 70	63 72	55 71	49 70	
8	104 39	96 48	88 55	80 60	72 63	64 64	56 63	48 60	
9	108 36	99 44	89 50	81 54	71 55	63 56	54 54	45 50	
10	109 33	100 40	90 45	80 48	70 49	60 48	50 45	40 40	
11	110 30	99 36	88 40	77 41	66 42	55 40	44 36	33 30	
12	108 27	96 32	84 35	72 36	60 35	48 32	36 27	24 20	
13	104 24	91 28	78 29	65 30	52 28	39 24	26 18	13 10	
14	98 21	84 24	70 25	56 24	42 21	28 16	14 9	0 0	—
15	90 18	75 19	60 20	45 18	30 14	15 8	0 0	−15 −10	—

C. Further data

¹⁹The head of the row represents one firm's quantity and the head of the column represents the quantity of the other firm. Inside the box at which row and column intersect, one firm's profit matching this combination of quantities stands up to the left and the other firm's profit stands down to the right. As mentioned in the text, 14 entries were manipulated in order to get unique best replies. For the quantity combination (7, 9) a mistake was made in that a 56 rather than 55 appeared in the table.

Round	STACKRAND			STACKFIX			COURRAND		COURFIX	
	q^L	q^F	Q	q^L	q^F	Q	q	Q	q	Q
1st	10.09 (2.69)	8.27 (2.37)	18.36 (2.82)	8.83 (2.73)	8.04 (2.05)	16.88 (3.43)	8.25 (1.83)	16.5 (2.88)	7.91 (2.14)	15.81 (3.25)
2nd	9.36 (2.79)	8.27 (1.78)	17.64 (2.68)	10.12 (2.29)	8.29 (1.97)	18.42 (3.06)	7.65 (2.23)	15.3 (2.79)	8.27 (2.05)	16.54 (3.62)
3rd	10.68 (2.59)	8.05 (2.01)	18.73 (2.98)	9.88 (2.66)	8.42 (2.26)	18.29 (4.10)	8.35 (1.69)	16.7 (2.50)	7.27 (2.29)	14.54 (4.44)
4th	10.23 (2.84)	8.32 (2.23)	18.54 (2.99)	9.17 (2.48)	7.88 (1.70)	17.04 (3.33)	7.85 (1.79)	15.7 (2.26)	8.14 (2.51)	16.27 (4.13)
5th	11.36 (1.79)	8.41 (2.24)	19.77 (2.49)	9.38 (3.09)	7.83 (2.43)	17.21 (4.24)	8.3 (1.56)	16.6 (1.78)	7.41 (2.22)	14.82 (2.79)
6th	9.86 (2.61)	8.27 (2.21)	18.14 (3.24)	9.79 (2.65)	8.00 (2.41)	17.79 (3.90)	8.1 (1.29)	16.2 (1.87)	7.86 (2.19)	15.72 (3.93)
7th	10.36 (2.04)	7.64 (1.65)	18.00 (2.18)	8.67 (2.60)	8.08 (2.06)	16.75 (3.66)	8.25 (1.68)	16.5 (2.72)	7.14 (1.83)	14.27 (2.83)
8th	10.09 (2.54)	8.77 (2.47)	18.86 (3.33)	8.38 (2.48)	7.62 (2.04)	16.00 (3.80)	8.05 (1.54)	16.1 (2.33)	7.09 (1.66)	14.18 (2.52)
9th	10.09 (1.93)	8.89 (2.32)	18.91 (3.22)	8.50 (2.84)	7.38 (1.58)	15.88 (3.81)	7.85 (1.18)	15.7 (1.42)	7.55 (1.92)	15.09 (3.05)
10th	9.77 (2.39)	8.36 (1.29)	18.14 (2.47)	8.62 (2.63)	7.67 (1.4)	16.29 (2.85)	8.05 (1.19)	16.1 (1.29)	7.73 (1.39)	15.45 (2.07)
Mean	10.19	8.32	18.51	9.13	7.92	17.06	8.07	16.14	7.64	15.27

Table C.1: Summary of experimental results: Means of individual and total quantities per round (Standard deviations in parentheses)

Quantity	STACKRAND				STACKFIX				COURRAND		COURF	
	Leader		Follower		Leader		Follower					
3	1.8	(.0)	.0	(.0)	1.3	(.0)	.8	(.0)	1.0	(.0)	1.4	
4	1.4	(.0)	.5	(.0)	0.4	(.0)	.8	(.0)	.0	(.0)	0.5	
5	.5	(.0)	5.5	(4.5)	0.0	(.0)	2.5	(4.2)	.5	(.0)	0.9	
6	2.3	(.0)	13.2	(9.1)	20.8	(33.3)	23.8	(33.3)	12.0	(5.0)	41.8	(50
7	4.1	(4.5)	16.8	(9.1)	7.9	(16.7)	17.1	(20.8)	21.5	(30.0)	8.6	(4
8	17.3	(22.7)	27.7	(36.4)	18.8	(12.5)	24.2	(25.0)	35.5	(55.0)	19.5	(18
9	10.5	(9.1)	5.5	(4.5)	7.1	(8.3)	11.7	(.0)	14.5	(5.0)	6.8	(13
10	14.5	(27.3)	17.7	(18.2)	12.5	(8.3)	7.9	(12.5)	5.5	(.0)	6.8	
11	8.6	(13.6)	7.7	(9.1)	3.3	(.0)	5.4	(4.2)	5.0	(.0)	9.1	(9
12	27.3	(13.6)	2.3	(.0)	19.6	(12.5)	2.9	(.0)	4.0	(5.0)	3.6	(4
13	4.1	(.0)	0.9	(4.5)	2.1	(.0)	1.7	(.0)	0.5	(.0)	0.5	
14	5.5	(9.1)	0.5	(.0)	3.8	(.0)	.8	(.0)	.0	(.0)	0.5	
15	2.3	(.0)	1.8	(4.5)	2.5	(8.3)	0.4	(.0)	.0	(.0)	.0	

Table C.2: Percent of Decisions for each quantity (Results of the ninth round in parentheses)