Düsseldorf Institute
for Competition Economics

DISCUSSION PAPER

No 283

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March 2018

HEINRICH HEINE
UNIVERSITÄT DÜSSELDORF

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IMPRINT

DICE DISCUSSION PAPER

Published by

düsseldorf university press (dup) on behalf of
Heinrich-Heine-Universität Düsseldorf, Faculty of Economics,
Düsseldorf Institute for Competition Economics (DICE), Universitätsstraße 1,
40225 Düsseldorf, Germany
www.dice.hhu.de

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ISSN 2190-9938 (online) – ISBN 978-3-86304-282-0

Collusion and Bargaining in Asymmetric Cournot Duopoly—An Experiment*

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March 2018

Abstract

In asymmetric dilemma games without side payments, players face involved cooperation and bargaining problems. The maximization of joint profits is implausible, players disagree on the collusive action, and the outcome is often inefficient. For the example of a Cournot duopoly with asymmetric cost, we investigate experimentally how players cooperate (collude implicitly and explicitly), if at all, in such games. We find that, without communication, players fail to cooperate and essentially play the static Nash equilibrium, confirming previous results. With communication, inefficient firms gain at the expense of efficient ones. When the role of the efficient firm is earned in a contest, the efficient firm earns higher profits than when this role is randomly allocated. Bargaining solutions do not satisfactorily predict collusive outcomes. Finally, when given the choice to talk, the efficient firms often decline that option.

Keywords: asymmetries, bargaining, cartels, communication, Cournot, earned role, experiments

JEL-class.: C7, C9, L4, L41

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*We thank Joe Harrington, Claudia Keser, Wieland Müller, Ronald Peeters, Chris Snyder, and audiences at Copenhagen Business School, EARIE Lisbon, ESA Jerusalem, Göttingen University, IIOC Philadelphia, and Lenzerheide Winter School for helpful comments. We also thank Gary Charness, Marie Claire Villeval, and Elven Priour for making their helpful z-Tree scripts available to us.

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1 Introduction

When it comes to cooperation and bargaining, economists often adopt a convenient compartmentalization by analyzing them as separate phenomena. Bargaining games and experiments typically take a certain pie as given and abstract from problems that might arise when the pie has to be generated through cooperation in the first place. Likewise, cooperation games are frequently bland in the bargaining dimension of the problem. In the standard prisoner's dilemma, for example, mutual cooperation implies the maximum joint payoffs and a symmetric payoff division, so in terms of bargaining there is not much to disagree on.

The starting point of this paper is that players often have to resolve bargaining and cooperation problems in one go. Cartels need to simultaneously resolve bargaining frictions and ensure reliable cooperation, using one action such as price or output. Contributions in public-good games determine the pie size and the split of the pie *uno actu*. The same holds for resource extraction in common-pool games. Put more broadly, players only have one instrument available (their stage-game action) to achieve two goals—the level of cooperation and the division of the surplus.

Resolving bargaining and cooperation problems with only one action will be particularly troublesome in the presence of asymmetries. In symmetric settings, this is not much of a problem because players have the same preferences about, say, the cartel price, the level of public-good provision or the rate of resource extraction. Also, the joint-profit maximum could plausibly be implemented. In asymmetric games (see, for example, Schmalensee 1987, Cox et al. 2013, and Keser et al. 2017), the preferred bargaining outcomes might not be supported by stable cooperation, and outcomes that are incentive-compatible may not be desirable from a bargaining perspective. Without side payments, the maximization of joint payoffs may not be incentive-compatible at all.[1]

Our paper illustrates these problems for the example of an asymmetric Cournot duopoly. Elaborating on the aforementioned general problems, we note that joint-payoff maximization in the Cournot case is not possible without side payments, as the inefficient firms would need to shut down.[2] If, on the other hand, all firms produce positive amounts, the outcome is inefficient. Firms may produce quantities on the Pareto frontier but, as pointed out by Bishop (1960), Schmalensee (1987), and Tirole (1988), the bargaining frontier is convex due to the cost asymmetries and firms will disagree on the collusive price. Superior payoffs can be obtained if the firms alternatingly adopt the monopoly position in the market, however, coordinating on the sequence and frequency of the alternating moves may be intricate and may raise antitrust suspicions.

At the same time, the output decisions also determine how to divide the surplus. Schmalensee (1987) suggests axiomatic theories of bargaining at this point. Players could agree on one of

[1]When side payments are allowed, players have a second instrument available: they can use the stage-game action to generate a cooperative pie and the side payment to split the pie.

[2]In Keser et al.'s (2017) linear public-good experiment with asymmetric endowments (and without side payments), high-endowment players have no interest in achieving the joint-payoff maximum as their Nash profit is larger than their profit in the joint-payoff maximum.

several possible bargaining solutions (Kalai-Smorodinski, equal relative gains, equal split, etc.) in order to determine the cooperative outcome. While firms may possibly coordinate on such outcomes, there are open questions. Schmalensee (1987) shows that different solutions imply different levels of joint payoffs and different gains (compared to the non-cooperative outcome) for asymmetric firms. So the bargaining problems players face are more severe than in symmetric markets.[3] Moreover, Schmalensee (1987) suggests that to coordinate on one of the many solutions players would presumably require explicit communication.

Still in addition to these problems, asymmetric players typically find it difficult to sustain a collusive agreement as a non-cooperative Nash equilibrium of a repeated game. The repeated-game incentive constraint is more severe with cost asymmetries than in the symmetric case. A conventional wisdom maintains that asymmetries hinder collusion (see, for example, Ivaldi et al. 2003). This problem is not specific to Cournot or oligopoly in general, but occurs for other types of (asymmetric) social dilemmas.

It seems fair to conclude that cooperating in asymmetric games, especially in asymmetric Cournot markets, is a formidable task. The Folk Theorem suggests many subgame-perfect equilibria in the repeated game but there are no focal points. Instead, firms face involved coordination, bargaining and inefficiency problems while the incentive-compatibility and individual rationality of repeated game equilibria curbs the set of possible outcomes. So, can asymmetric firms collude successfully at all? And, if so, how?

We suggest a positive approach to answer these questions. We study how asymmetric Cournot duopolies collude in experiments. We believe that controlled laboratory settings with exogenously varied treatments can complement the game-theoretic analysis in that they can deliver behavioral equilibrium selection.

Previous experiments on Cournot duopolies with asymmetric costs (Mason et al. 1992, Mason and Philips 1997, Selten et al. 1997, Fonseca et al. 2005, and Normann et al. 2014) have concentrated solely on implicit collusion, and they document consistently that participants fail to reach supra-competitive payoffs throughout.[4] In these experiments, subjects play asymmetric

[3]Experimentally, the intricacies of asymmetric bargaining have been documented for various economic environments. See Roth and Malouf (1979) for unstructured bargaining, Kagel et al. (1996) and Gneezy and Guth (2003) for ultimatum bargaining with asymmetric exchange rates, and Beckenkamp et al. (2007) for a repeated dilemma game.

[4]Mason et al. (1992) were the first to observe that asymmetric Cournot markets are less cooperative and take longer to reach an equilibrium than symmetric markets. In a companion paper, Mason and Philips (1997) study the interaction of cost asymmetries and two different information environments: with private information, players only know their own payoffs. The results show that asymmetric markets are unaffected by private vs. public information but symmetric markets are more cooperative when payoffs are common knowledge. Fonseca et al. (2005) analyze endogenous timing in an asymmetric duopoly, that is, the endogenous emergence of a Stackelberg leader and follower. They also conduct control treatments with standard simultaneous-move (and cost-asymmetric) Cournot duopolies which can be compared to the previous experiments and this paper. Finally, Normann et al. (2014) use symmetric and asymmetric Cournot duopolies to investigate the impact of the duration of an experiment.

Selten et al. (1997) asked student participants of a game-theory seminar to program repeated-game strategies for asymmetric Cournot duopolies. The setup differs to the other experiments because of an additional asymmetry of a fixed-cost parameter. The statistical significance of the results is somewhat difficult to assess because the group of subjects is relatively small and interacted repeatedly over the course of the entire term.

Asymmetries among firms can alternatively be modeled as differences in production capacities. For recent experiments with capacity-asymmetric firms, see Harrington et al. (2016) or Fonseca and Normann (2008). In

Cournot duopolies without any possibility of communication. Behavior settles roughly around the static Nash equilibrium, but there are some discrepancies: high-cost firms produce more than predicted, low-cost firms less. Aggregate output is, if anything, above (rather than below) the level predicted by static Nash equilibrium. The failure of asymmetric Cournot duopolies to collude tacitly is in contrast to the symmetric duopolies reported in these papers where typically some level of tacit collusion can be maintained (Huck et al. 2004).

We depart from these existing papers in two dimensions. First, we investigate whether asymmetric Cournot duopolies can overcome the failure to collude by using explicit communication. We know that unrestricted communication usually leads to very effective cooperation in symmetric oligopolies (recently, for example, Cooper and Kühn 2014, Fonseca and Normann 2012, and Harrington et al. 2016). It thus seems promising to extend the literature by studying how asymmetric Cournot duopolies operate when direct communication is available. We also run experiments where firms choose whether or not to communicate with one another.

A second innovation is that we introduce a contest for the role of the efficient firm in some of our treatments. As mentioned, existing asymmetric Cournot experiments found that quantities and payoffs are more equitable than predicted. We expect this effect to be less acute when players need to invest effort to become efficient (just as in the field, where firms engage in costly R&D to obtain a cost advantage). This aspect is missing in existing laboratory studies. In our "earned role" variants subjects have to conduct a tedious real-effort task (taken from Charness et al. 2013) before playing the Cournot part.[5] The best-performing participants then produce at low cost, the others at high cost. We expect the discrepancies to static Nash to disappear in these treatments.

Our results are as follows. Without communication, firms fail to collude and essentially play the static Nash equilibrium, as suggested by the previous experiments. When express communication is available, joint profits increase (insignificantly), and the inefficient firms benefit whereas the efficient player lose. Bargaining solutions do not predict collusive outcomes well. Consistent with these findings, when the two firms are given the option to talk (that is, when communication is endogenous), it is the efficient firms that are more often disinclined to communicate. When the role of the efficient firm is earned in a contest, competitiveness is reduced without communication but increased with chat. Moreover, with earned roles and when firms can talk, they often collude by producing equal amounts—a collusive strategy unknown in the existing literature. We further employ coding analysis (Houser and Xiao 2011) to investigate the nature of the agreements reached in the treatments with communication and text-mining analysis (Moellers, Normann and Snyder 2017) to identify the language suitable for successful collusion.

both studies, firms have the same preferences regarding prices, despite the asymmetry. Argenton and Müller (2012) study the role of cost asymmetry on collusion in experimental Bertrand duopolies with convex costs.

[5]There are several other experiments with "earned roles," see, for example, Konow (2000), Fahr and Irlenbusch (2000), Gächter and Riedl (2005), and Oxoby and Spraggon (2008).

2 Model and benchmarks

The stage game is a Cournot duopoly market with cost-asymmetric firms. Two firms, firm 1 and firm 2, choose non-negative quantities, $q_i,\ i = 1,2$, as their actions. Their production costs are linear

$$C_i(q_i) = \theta_i q_i \quad i = 1,2. \tag{1}$$

Assume $\theta_1 < \theta_2$, that is, let firm 1 be the low-cost firm. Firms face linear inverse demand

$$p(Q) = \max\{\alpha - Q, 0\} \tag{2}$$

where $Q = q_1 + q_2$ denotes aggregate output. In the unique static Nash equilibrium (NE) of this game, firms produce

$$q_i^* = \frac{\alpha - 2\theta_i + \theta_j}{3}, \quad i,j = 1,2; \quad i \neq j \tag{3}$$

provided cost asymmetries are not too large. Nash equilibrium profits are $\pi_i^{NE} = (q_i^*)^2$, $i,j = 1,2$. The monopoly output of firm i is

$$q_i^M = \frac{\alpha - \theta_i}{2}, \quad i,j = 1,2 \tag{4}$$

and the corresponding monopoly profit is $\pi_i^M = (q_i^M)^2$.

For collusion (tacit or explicit) to be a subgame-perfect Nash equilibrium, we need a repeated game. Accordingly, we consider infinitely many repetitions of the stage game where future payoffs are discounted by some discount factor $\in (0,1]$.

Consider now the three different ways of producing collusive outputs, as mentioned in the introduction. First, the joint-profit maximum (JP) would require $q_1 = q_1^M$ and $q_2 = 0$ and imply a joint profit of $(q_1^M)^2$. Only when side payments are feasible can this profit be freely allocated between firms. Second, firms may alternatingly produce their preferred q_i^M (AM). The joint profit would be $\gamma(q_1^M)^2 + (1-\gamma)(q_2^M)^2$ where $\gamma \in [0,1]$ denotes the likelihood or relative frequency of the low-cost firm being the monopolist. (Equivalently, firms could divide the market and allocate individual consumers to firms.) Third, when both firms produce positive amounts in each period, they should produce Pareto-efficient quantities. To derive the Pareto frontier, rewrite the payoff functions as $q_i = \pi_i/(p - \theta_i)$. Summing this expression up over both firms gives:

$$Q = \frac{\pi_1}{p - \theta_1} + \frac{\pi_2}{p - \theta_2} \tag{5}$$

Solving for π_2 and using $Q = \alpha - p$ yields the Pareto frontier:

$$\overline{\pi}_2(\pi_1) = \max_p \left(\alpha - p - \frac{\pi_1}{p - \theta_1}\right)(p - \theta_2) \tag{6}$$

Equation (6) states that for any given level of payoffs of the low-cost firm, $\pi_1 \in [0, (q_1^M)^2]$, the payoffs of the high-cost firm on the Pareto frontier, $\overline{\pi}_2$, can be found by adjusting the market price p such that π_2 is maximized. This Pareto frontier is convex (Bishop 1960, Schmalensee 1987, Tirole 1988).

Next, firms need to resolve the bargaining problem of how much either firm is going to earn. We follow Schmalensee (1987) who suggests bargaining theory here. We assume min-max payoffs here as the disagreement points, denoted by $\underline{\pi_1}$ and $\underline{\pi_2}$, not least since we analyze infinitely repeated games. The solution proposed by Kalai and Smorodinsky (1975)[6], henceforth KS, maintains the ratios of the maximal payoffs (π_i^M) players can obtain in addition to $\underline{\pi_i}$:

$$\frac{\pi_1^{KS} - \underline{\pi_1}}{\pi_1^M - \underline{\pi_1}} = \frac{\pi_2^{KS} - \underline{\pi_2}}{\pi_2^M - \underline{\pi_2}}, \tag{7}$$

where $\pi_i^{KS}, i = 1, 2$, are the payoffs of the low-cost and high-cost firm under the KS criterion. Roth's (1979) equal relative gains (ERG) suggests payoff increases proportional to the payoffs earned in the disagreement point:

$$\frac{\pi_1^{ERG}}{\underline{\pi_1}} = \frac{\pi_2^{ERG}}{\underline{\pi_2}}. \tag{8}$$

The equal split (ES) solution (Roth 1979)

$$\pi_1^{ES} = \pi_2^{ES} \tag{9}$$

has both firms earning the same absolute amount.

	Quantities			Profits			Welfare	
	q_1	q_2	Q	π_1	π_2	$\pi_1 + \pi_2$	CS	TS
NE	30	18	48.0	900	324	1224	1152	2376
JP	39	0	39.0	1521	0	1521	761	2282
AM	19.5	16.5	36.0	761	545	1306	653	1959
KS	18.6	17.0	35.6	790	516	1306	634	1940
ERG	23.2	13.2	36.4	965	391	1356	662	2018
ES	14.7	20.4	35.1	631	631	1262	616	1878

Table 1: Benchmarks. Notes: AM refers to an alternating monopoly with $\gamma = 0.5$ here, implying that firms produce half of their preferred monopoly outputs (39 and 33) on average. CS and TS denote consumer surplus and total surplus, respectively.

The numerical values of our benchmarks given the experimental parametrization are reported in Table 1. We use $\alpha = 91$, $\theta_1 = 13$ and $\theta_2 = 25$ and players choose from $q_i \in$

[6]The KS solution exists even though, as pointed out by Schmalensee (1987), the bargaining set is non-convex in the case of asymmetric, linear costs. As was shown by Conley and Wilkie (1991) it is sufficient for the existence of the KS solution that the bargaining set is *comprehensive*, which holds for our model. In a later paper, Conley and Wilkie (1996) also extend the Nash (1950) solution to non-convex but comprehensive bargaining sets. This solution coincides with the KS solution and we omit it here.

$\{0, 1, ..., 45\}$. The table presents the quantities, payoffs, consumer surplus (CS) and total surplus (TS, defined as the sum of profits and CS). The first row shows the static Nash equilibrium with continuous actions, as derived above. Due to the discretization of the action space in the experiment, two further Nash equilibria close to this equilibrium exist ($(q_1, q_2) = (29, 19)$ and $(31, 17)$) and have the same aggregate output. Table 1 also contains the joint-profit maximum, and the alternating monopoly solution (AM). For Pareto-efficient outcomes, we show the values for KS, ERG, and ES.

Figure 1 illustrates the three methods of producing collusive outputs and the bargaining solutions for the parameters of the experiment. The figure shows all three Nash equilibria, the Pareto frontier, the joint-profit maximum with side payments, and the alternating monopoly solution for $\gamma = .5$ (each firm is the monopolist with 50 percent probability). Note that AM is not on the Pareto frontier but is (marginally) superior to it. The dotted lines delimit the outcomes that are Pareto superior to mutual min-max payoffs $\underline{\pi} = (\underline{\pi_1}, \underline{\pi_2})$, with $\underline{\pi_1} = 272.25$ and $\underline{\pi_2} = 110.25$ (due to the maximum quantity in the experiment being 45). We also include the bargaining solutions (KS, ERG, ES) given firms are on the Pareto frontier (when both firms produce a positive amount).[7]

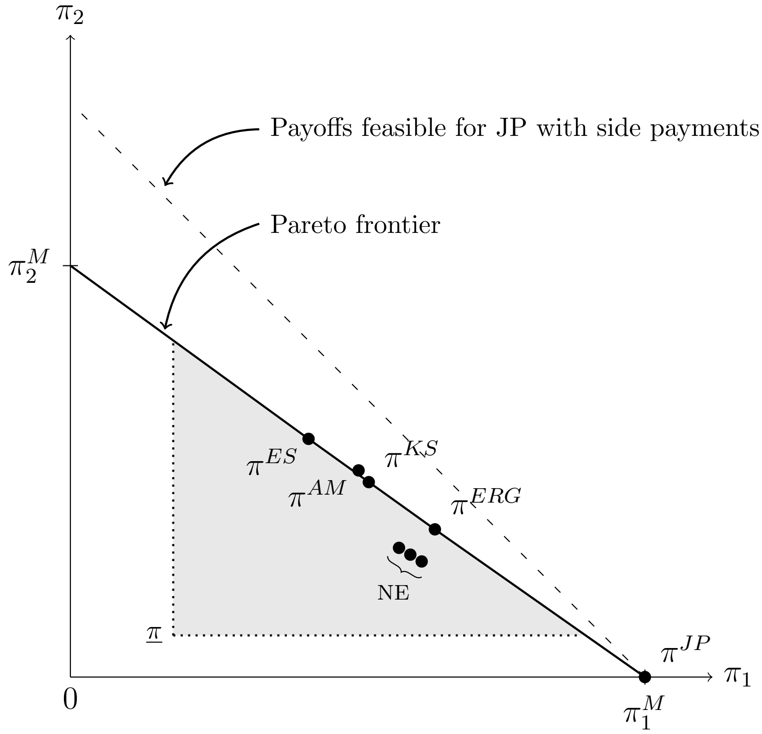

Figure 1: Feasible payoffs and bargaining solutions, plot for experimental parameters.

Concluding, we comment on the incentive-compatibility of the collusive solutions in the infinitely repeated game. From Figure 1, it is evident that all benchmarks—with the exception of JP—can be sustained as subgame perfect equilibria in the repeated game. With optimal

[7]The bargaining solutions could also be applied to JP and AM. We refrain, however, from reporting these outcomes here because JP is not feasible and (any form of) AM turns out to be empirically irrelevant in our data.

penal codes (players min-max each other for one period upon deviation), these outcomes would require a minimum discount factor of 0.28, 0.24, and 0.56 for KS, ERG, and ES, respectively. A formal derivation of the minimum discount factor with optimal penal codes can be found in Appendix A. To make AM incentive-compatible for both firms would require $\gamma \in [0.179, 0.899]$, that is, the low-cost firm would need to be the monopolist between 18 and 89 percent of the time. These AM solutions would be Pareto superior to the min-max payoffs.

3 Experimental design

The experiments were implemented as a sequence of repeated games. The stage game of these repeated games are the exact Cournot duopoly outlined above. Participants had to play a total of five supergames. The reason for repeating the supergames is that subjects may need several attempts to learn how to play infinitely repeated supergames (Dal Bó 2005). Each supergame was played with the same partner (fixed matching) but different supergames were played with a different player (absolute stranger re-matching, Dal Bó 2005). Players kept the role of either the low-cost or the high-cost firm for the entire experiment. At the end of every stage game, play would continue for another period with a probability of 75 percent. When the computer determined the end of a supergame, a new supergame with a different partner started. The actual number of periods in every supergame was determined ex ante and was the same in all sessions. The duration of the five supergames were 6, 2, 5, 4, and 3, respectively.

		Communication		
		No	Yes	Choice
Earned Roles	No	NoTalk-Random (110)	Talk-Random (108)	—
	Yes	NoTalk-Earned (106)	Talk-Earned (106)	ChooseTalk-Earned (66)

Table 2: Treatment design and the number of participants for each treatment.

We have treatments with and without the opportunity to communicate (Talk and NoTalk), and with and without the possibility to earn the role of the efficient low-cost firm (Earned and Random). We add to these four main treatments an additional variant (ChooseTalk) where subjects could choose whether or not to talk. (After a first look at the results, it became apparent that the potential sixth variant, ChooseTalk-Random, would not lead to any additional insights, so we decided to keep that treatment cell empty.) This creates the design in Table 2.

The communication between players was implemented as follows. In the Talk treatments, subjects were allowed to communicate with the other firm once, prior to the start of each supergame. Communication was via typed messages, using an instant-messenger communication tool. Communication was unrestricted and subjects were allowed to exchange as many messages as they liked, however, they were not allowed to identify themselves. Unrestricted communication was important here because we wanted to allow players to discuss entire strategies, not just quantity targets (which could also be communicated through simple numeric announcements). The time to communicate was limited to three minutes in the first supergame, two minutes in the second supergame, and 90 seconds in supergames three, four, and five. In the ChooseTalk treatment, subjects had the opportunity to choose whether or not to talk. Communication was implemented only if both players opted for it (as in the Talk treatments) and a minor cost was imposed.[8] Finally, in the NoTalk-treatments, subjects had to post quantities in each period without being able to communicate with each other.

Our second treatment variable varied how the role of the efficient firm was allocated. In the Random treatments, one subject was assigned the role of the more efficient firm by a random computer move. In the Earned treatments, winners of a pre-play contest were awarded that role. Here, subjects participated in a real-effort task at the beginning of the experiment to determine their role. During a period of five minutes subjects were instructed to translate letters into numbers using a translation table with one column of letters and a second column displaying the corresponding numbers (Charness et al. 2013). In each session, the better-performing half of the participants were assigned the role of the low-cost firm and the rest the role of the high-cost firm. Participation in the effort task was voluntary and subjects could use their time as they wished as long as they did not interfere with the other subjects in the room. However, all our subjects decided to actively participate in the effort task.

4 Procedures

We provided written experimental instructions which informed subjects of all the features of the market (the instructions are available in Appendix B). Subjects were told they were representing one of two firms in a market. The experiments were computerized, using z-Tree (Fischbacher 2007). Subjects learned their role (firm with high or low costs) from the computer screen.

In every period, subjects had to enter their quantity in a computer interface. On the decision screen, subjects also had access to a profit calculator which allowed them to compute firms' payoffs and the market price from the hypothetical quantity choices of the low-cost and high-cost firm. Once all subjects had made their decisions, the period ended and a screen displayed the quantity choices of both firms and the market price. On the screen was also displayed the individual payoff of the current period and the accumulated payoffs up to that point but not the payoffs of the other firm. When a supergame ended, a subsequent screen informed the subjects that they would now be re-matched with a new partner.

The experiments were conducted at the DICElab of Heinrich Heine University from April

[8]The cost was kept deliberately small, such that the cost itself should not deter subjects from talking. Our design resembles that of Andersson and Wengström (2007), except that they impose a cost for each message. Our design differs from those experiments with a fully-fledged system of cartel fines and leniency (as in Hinloopen and Soetevent 2008 or Bigoni et al. 2012). See our note in the Conclusion.

to November 2015 and from March to December 2017. A total of 496 subjects participated in 23 sessions (five for the four main treatment, three for ChooseTalk). Subjects were mainly students and were randomly recruited (using ORSEE, Greiner 2015) from a pool of potential participants. Sessions lasted between 45 and 65 minutes.

Payments consisted of a show-up fee of 5 € plus the sum of payoffs attained during the course of the experiment. For payments, we used an experimental currency unit ("Taler"); 2,200 Taler were worth 1 €. Since the Earned treatments lasted longer, subjects received a participation fee of 4,000 Taler for taking part in the effort task. Average earnings were 11.47 €.

5 Results

We begin in section 5.1 by reporting treatment effects. Section 5.2 answers our first research question, whether firms collude at all, by comparing the data to the point predictions of our benchmarks. Section 5.3 investigates how exactly firms collude in the Talk treatments.

Whenever we conduct non-parametric tests in our empirical analysis, we will count one session as one single observation. For regression analysis, we cluster the data at the session level. We report two-sided p-values throughout.

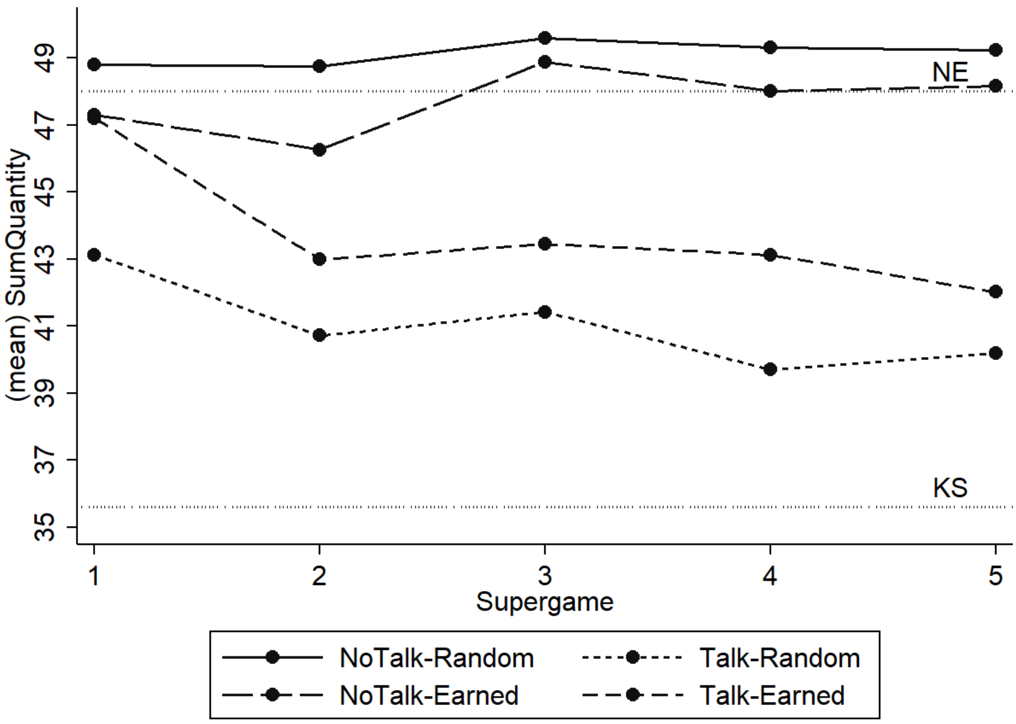

Figure 2: Aggregate quantities, Q, across supergames. For comparison, the static NE has $Q = 48$ whereas in the various collusive solutions Q is approximately equal to 35 to 36.

5.1 Treatment effects

We will initially focus on the four treatments NoTalk-Random, NoTalk-Earned, Talk-Random and Talk-Earned and postpone the discussion of ChooseTalk-Earned to section 5.1.5 because these results can be understood more easily once the impact of communication and earned roles become clear.

	Quantities			Payoffs			Welfare	
	Q	q_1	q_2	π_1	π_2	$\pi_1 + \pi_2$	CS	TS
NoTalk-Random	49.3	28.8	20.5	794	319	1113	1245	2358
	(0.71)	(0.64)	(0.74)	(22.6)	(16.8)	(18.5)	(32.7)	(19.1)
NoTalk-Earned	48.1	28.4	19.7	811	322	1132	1190	2322
	(0.76)	(0.92)	(1.59)	(36.1)	(20.4)	(25.9)	(27.5)	(21.1)
Talk-Random	40.5	18.7	21.9	666	541	1207	846	2053
	(1.74)	(1.38)	(0.89)	(20.6)	(34.7)	(27.7)	(78.3)	(51.3)
Talk-Earned	43.1	22.8	20.2	759	447	1207	955	2162
	(1.39)	(0.88)	(0.74)	(16.5)	(20.2)	(20.1)	(59.7)	(41.6)
NE	48.0	30.0	18.0	900	324	1224	1152	2376
KS	35.6	18.6	17.0	790	516	1306	634	1940

Table 3: Summary of treatment averages for outputs, profits, and welfare measures (standard deviations in parenthesis, based on session averages), employing data from supergames 2 to 5.

5.1.1 Overview and behavior across supergames

Figure 2 shows the total output (Q) for each treatment across the five supergames. The effect of communication is immediate: Talk reduces output. The effect of Earned is less strong and depends on the communication mode: for NoTalk, the earned role reduces output but for Talk it increases it.

Figure 2 also shows that some learning is going on. In the Talk treatments, aggregate output decreases after the first supergame. Appendix C contains a more disaggregated version of Figure 2, documenting average aggregate quantities, Q, for all sessions and periods separately. The learning effect we observe confirms that some repetitions of the repeated game are warranted to ensure that participants understand the experimental setting. In order to take this effect into account, we often discard the first supergame from the data in this section. Specifically, we do so for all non-parametric tests. Our results do not change qualitatively when we include the first supergame or when we exclude more supergames. The comprehensive regression analysis in section 5.1.4 includes data from all sessions and employs “session” as an explanatory variable.

Table 3 is the summary statistics of our study. It shows our main variables: aggregate and individual-level output, profit, consumer surplus, and total surplus. We will repeatedly refer to this table in this results section.

5.1.2 The effect of communication

Table 3 shows that, compared to the NoTalk treatments, Talk reduces aggregate outputs by 8.8 (Random) and 5.0 (Earned) quantity units, respectively. For both Random and Earned, this effect is statistically significant: non-parametric exact rank-sum tests yield $p = 0.008$ in either case.

A look at the outputs of high- and low-cost firms separately (columns 2 and 3 of Table 3) indicates that the collusive effect of Talk is largely due to the low-cost firms reducing their output. Comparing Talk and NoTalk, we note that q_1 is reduced by 10.1 (Random) and by 5.6 quantity units (Earned). These effects are significant (rank-sum tests, $p = 0.008$). Outputs produced by high-cost firms, by contrast, even increase in Talk, namely by 1.4 (Random, $p = 0.095$) and 0.5 (Earned, $p = 0.421$). We conclude that the collusive (Q-reducing) effect of communication is foremost due to the efficient firm reducing its output.

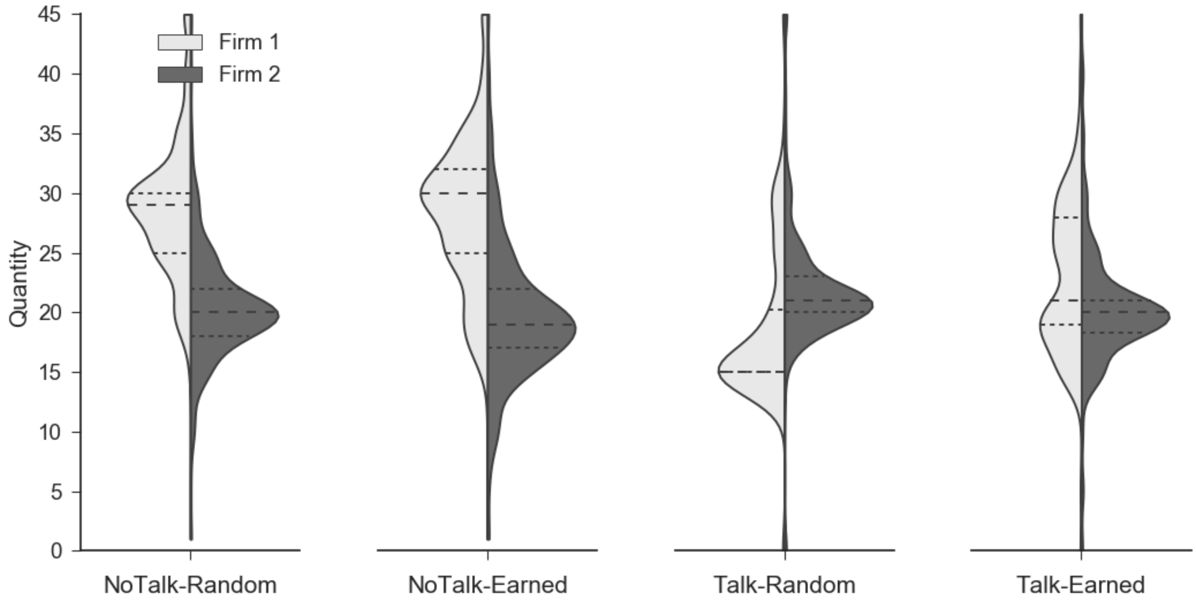

Figure 3: Kernel density estimates (KDE) for outputs by treatment and firm type. Medians are indicated by dashed lines, lower and upper quartiles by dotted lines. For comparison, recall that $(q_1, q_2) = (30, 18)$ in the static Nash equilibrium and $(q_1, q_2) = (18.6, 17.0)$ in the KS solution.

The Kernel density estimates (KDE) of the distribution of outputs in Figure 3 illustrate the changes of q_1 and q_2. In Talk-Random, the median outputs of the low-cost firm is even lower than for the high-cost firm.

We now turn to an analysis of profits, consumer surplus, and total surplus. Industry profits increase significantly in Talk compared to NoTalk (rank-sum tests, $p = 0.008$ for both Random and Earned). As expected from the analysis of q_1, the low-cost firms earn less than in the NoTalk treatments ($p = 0.008$ for Random and $p = 0.056$ for Earned). In contrast, high-cost firms strongly benefit from the possibility to talk (both $p = 0.008$). The Kernel density estimates (KDE) of the distribution of profits in Figure 4 nicely illustrate this: there is a substantial downward shift in the density of the low-cost firm's profit and a corresponding upward shift for the high-cost firm. In our setup, consumer surplus is affected by aggregate output only: any reduction of Q also reduces CS. It follows that consumers lose when Talk is introduced (both $p = 0.008$). Total surplus generally depends not only on aggregate output but also on how efficiently this output is produced (that is, how much the low-cost firm produces). In our data, however, the reduced aggregate output involves the efficient firm producing less, and so there cannot be any efficiency gains in production due to the quantity reduction in Talk. As a result, total surplus is significantly reduced in the Talk treatments (both $p = 0.008$).

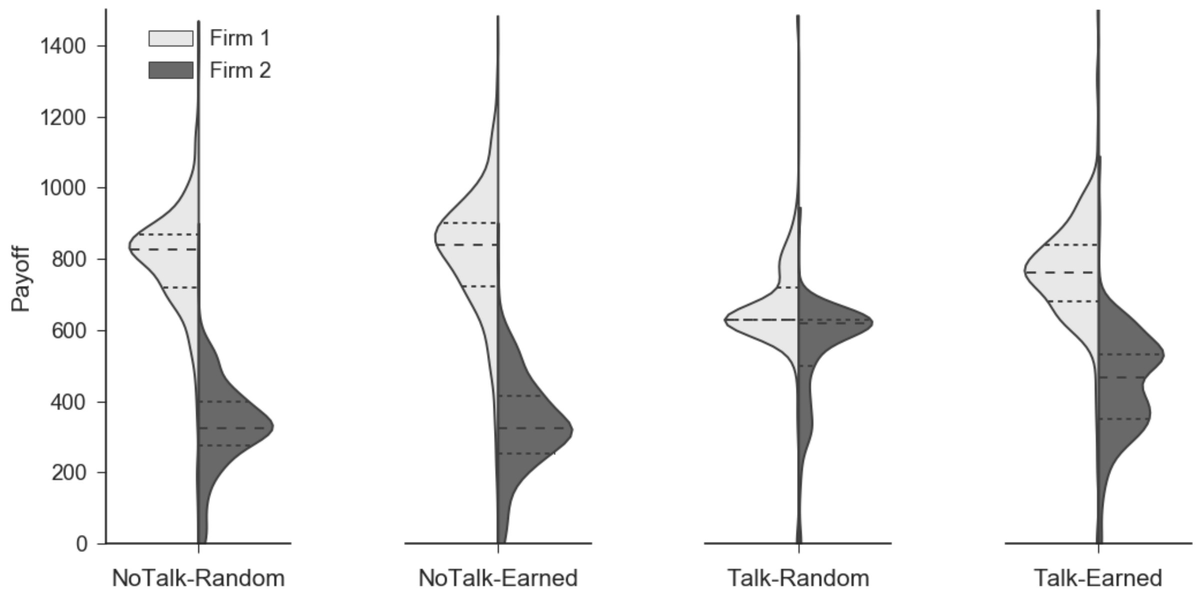

Figure 4: Kernel density estimates (KDE) for profits by treatment and firm type. Medians are indicated by dashed lines, lower and upper quartiles by dotted lines. For comparison, recall that $(\pi_1^{NE}, \pi_2^{NE}) = (900, 324)$ and $(\pi_1^{KS}, \pi_2^{KS}) = (790, 516)$.

Result 1. *Comparing Talk to NoTalk, (i) aggregate output decreases, and (ii) industry profits increase whereas consumer surplus and total surplus decrease. (iii) The reduction of aggregate output in the Talk treatments is due to the low-cost firm reducing its output.*

5.1.3 The effect of earned roles

Figure 2 and Table 3 show the ambiguous effect the contest for the role of the efficient firm has: Q is smaller when comparing NoTalk-Earned to NoTalk-Random but is larger when comparing Talk-Earned to Talk-Random. Non-parametric rank-sum tests confirm these effects are significant (rank-sum tests, $p = 0.032$ for NoTalk and $p = 0.056$ for Talk). The significant reduction of Q in NoTalk-Earned (compared to NoTalk-Random) results from both firm types insignificantly reducing their output ($p = 0.310$ for high-cost firms and $p = 0.548$ for low-cost firms). For Talk-Earned vs. Talk-Random, the increase is due to the low-cost firm increasing its output ($p = 0.008$).

As for profits, consumer and total surplus, we find that industry profits do not vary significantly between Earned and Random for either communication treatment (rank-sum tests, $p = 0.222$ for NoTalk and $p = 1.000$ for Talk). While the earnings of both firm types do not vary between NoTalk-Earned and NoTalk-Random ($p = 0.310$ for the low-cost firm and $p = 0.841$ for the high-cost firm), in Talk-Earned a profit increase of the low-cost firm ($p = 0.008$) and a profit decrease of the high-cost firm ($p = 0.008$) result. Consumer surplus decreases significantly in NoTalk-Earned compared to NoTalk-Random ($p = 0.016$) and increases significantly in Talk-Earned ($p = 0.056$). The effects for total surplus have the same signs and are also significant ($p = 0.056$ for NoTalk-Earned vs. NoTalk-Random and $p = 0.016$ for Talk-Earned to Talk-Random).

Kernel density estimates of the distributions of profits are shown in Figure 4. They suggest the following interpretation: whereas in Talk-Random the low-cost firm's profits are very much centered around the equal split and have little density toward higher profits, the Talk-Earned treatment has a substantial amount of density for profits of 800 and above toward the NE level. Nevertheless, even in Talk-Earned a large fraction of low-cost firm subjects still reach profits close to the equal split.

Result 2. *(i) Comparing NoTalk-Earned to NoTalk-Random, aggregate output decreases, industry profits do not vary, and consumer surplus and total surplus decrease. (ii) Comparing Talk-Earned to Talk-Random, aggregate output increases due to the low-cost firm increasing its quantity, industry profits do not vary, and consumer surplus and total surplus increase.*

Below, we discuss the—apparently contradicting—effect of earned roles on aggregate output (decrease in NoTalk, increase in Talk). See section 6.

5.1.4 Regression analysis

To complement the non-parametric tests, we run linear regressions (clustered at the session level). We include the data from all supergames. As regressors, we use treatment dummies (Talk, Earned, Talk $\times$ Earned) and the cardinal variable Supergame (from 1 to 5) plus the interaction Supergame $\times$ Talk to capture learning across the five repetitions of the repeated game.

Table 4 reports the results. Regression (1) regresses Q and confirms that the treatment effect due to Talk is statistically highly significant. Comparing regressions (2) and (3) reveals that the reduction of total output can be traced back to the low-cost firm that significantly reduces its output. This confirms the results of the non-parametric tests summarized in parts (i) and (ii) of Result 1. Regressions (6) to (8) regress industry profits, consumer surplus and total surplus and are in line with the effects stated in part (iii) of Result 1. Importantly, the interaction term Supergame $\times$ Talk is significant in these regressions which documents a substantial learning effect that augments the respective treatment effect over the course of the supergames.

The regressions also confirm the treatment effects due to Earned as documented in Result 2. In regression (1), the Earned dummy as well as Talk × Earned are highly significant and show that aggregate output decreases in NoTalk but increases in Talk. This interaction term is also significant in regression (2) which suggests that the output increase in Talk stems from an increase in output of the low-cost firm. Regression (6) shows that Earned has weakly significant effects on the industry payoffs, however, these are of minor magnitude (less than 24 payoff points for both, NoTalk and Talk). Finally, regressions (7) and (8) strongly confirm the findings of Result 2 on consumer surplus and total surplus.

	Quantities			Payoffs			Welfare	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. var.	Q	q_1	q_2	π_1	π_2	$\pi_1 + \pi_2$	CS	TS
Talk	-4.338***	-6.390***	2.052***	-97.11***	122.5***	25.36	-194.2***	-168.8***
	(0.724)	(0.907)	(0.571)	(15.54)	(21.59)	(19.05)	(35.68)	(22.66)
Earned	-1.298***	-0.527	-0.770	22.28	0.752	23.03*	-57.50***	-34.47***
	(0.313)	(0.623)	(0.709)	(19.04)	(14.82)	(11.76)	(14.90)	(9.025)
Talk × Earned	4.213***	4.829***	-0.616	60.86**	-100.3***	-39.48**	187.8***	148.3***
	(0.828)	(0.868)	(0.861)	(21.66)	(20.58)	(16.36)	(36.24)	(25.42)
Supergame	0.223	0.267*	-0.0441	7.497**	-1.284	6.214*	5.844	12.06**
	(0.133)	(0.130)	(0.0976)	(3.337)	(2.439)	(3.492)	(6.091)	(4.283)
Superg. × Talk	-1.246***	-1.011***	-0.236	-6.142	26.33***	20.19***	-57.29***	-37.10***
	(0.242)	(0.176)	(0.149)	(4.061)	(4.554)	(6.634)	(11.70)	(6.479)
Constant	48.54***	27.88***	20.66***	764.4***	324.6***	1,089***	1,225***	2,314***
	(0.486)	(0.663)	(0.349)	(11.27)	(15.76)	(12.60)	(24.10)	(16.25)
Observations	4,300	4,300	4,300	4,300	4,300	4,300	4,300	4,300
R^2	0.134	0.241	0.020	0.078	0.210	0.031	0.121	0.195

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4: Ordinary least-squares regressions for output, payoff, and welfare variables, clustered at the session level, including supergame 1.

5.1.5 Endogeneous communication

In the four treatments reported so far, the feasibility of communication was exogenously imposed by the experimenter. What happens when firms can freely choose whether or not to chat? We saw that the efficient firms do not gain from communication, so will they choose to talk here at all?

In the ChooseTalk-Earned treatment (see Table 2), subjects had the choice of whether to communicate with the other firm in the market. At the beginning of every supergame, subjects were asked whether they wanted to communicate. Exchanging messages was only enabled when both subjects chose to talk. Only if they agreed to chat, a cost of 5 Taler was deducted from the account of both firms. As in the other Talk treatments, communication (if enabled) was possible at the beginning of each supergame but not in-between rounds of the same supergame.

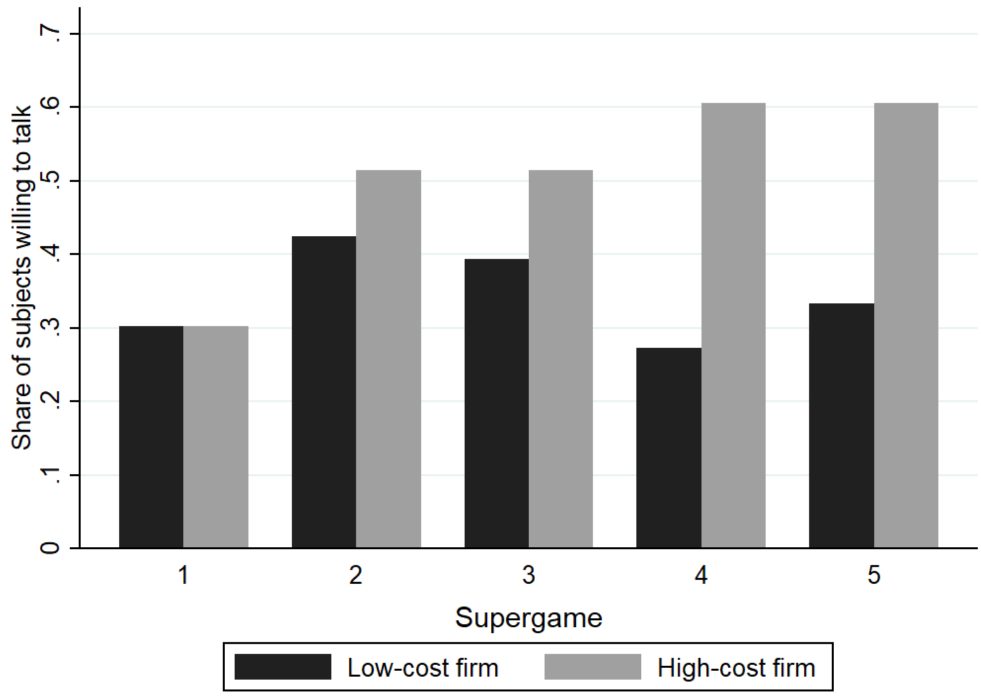

Figure 5: Willingness to talk over supergames in ChooseTalk, black bars indicate efficient firms.

Figure 5 shows for both cost types and the supergames the share of subjects willing to talk. The following observations are statistically significant in the regressions in Table 5. Regressions (1) and (2) of Table 5 show that inefficient firms (see the coefficient of "High Cost") are more willing to talk. Over the course of the supergames, the proportion of inefficient firms that are willing to talk increases (positive and significant High Cost $\times$ Period coefficient in regression (3)). It follows that the difference between firm types becomes larger in later supergames.

Of course, the low-cost firms' limited inclination to talk implies that the two firms rarely agree to talk. The fraction of markets where communication is enabled is 6.1% in the first supergame and stabilizes between 18.1% and 21.3% in the subsequent supergames.

Concluding this section, we briefly compare the outcomes of the duopolies with communication enabled to those without chat and find that, by and large, results are consistent with the corresponding treatment with exogenously imposed communication mode. Without communication, average quantities are rather close to the values of NoTalk-Earned ($Q = 50.2$ (std. err. $= 0.26$), $q_1 = 29.5$ (0.27), $q_2 = 20.7$ (0.29)) although it runs out the differences in Q are significant (rank-sum test, $p = 0.036$). With communication, results resemble those of the Talk-Earned treatment ($Q = 42.6$ (0.61), $q_1 = 21.4$ (0.71), $q_2 = 21.1$ (0.66); rank-sum test for Q, $p = 1.000$). We conclude that selection effects in the treatment with endogenous communication are moderate.

Result 3. *When firms can choose whether to communicate, (i) high-cost firms are more inclined to do so than low-cost firms and (ii) only roughly one in five duopolies agree to talk.*

	(1)	(2)	(3)
Dependent variable		Willingness to talk	
High Cost	0.420**	0.422**	-0.0654
	(0.188)	(0.188)	(0.109)
Period		0.0206***	-0.0542**
		(0.00741)	(0.0225)
High Cost × Period			0.0496***
			(0.0131)
Constant	-0.818***	-1.022***	-0.287***
	(0.274)	(0.247)	(0.0575)
Observations	330	330	330
Robust standard errors in parentheses			
*** p<0.01, ** p<0.05, * p<0.1			

Table 5: Probit regressions in ChooseTalk, dummy *Willingness to talk* is the dependent variable, clustered at the session level. The variable High Cost takes a value of zero for the low-cost firm, and a value of 1 for the high-cost firm. Data from all supergames are used.

5.2 Do firms collude at all?

Following Phlips (1995), we argue that an outcome is collusive whenever aggregate output is below the level suggested by the static Nash equilibrium (NE). We also investigate the power of the point predictions of our collusive benchmarks KS, ERG and ES.

In the NoTalk treatments, the aggregate output, Q, fits reasonably well with the NE prediction of $Q = 48$. From Table 3, we find $Q = 49.3$ for NoTalk-Random and $Q = 48.1$ for NoTalk-Earned. An exact Wilcoxon signed-rank test suggests that the average Q in NoTalk-Random is significantly larger than the Nash prediction ($p = 0.063$, two-sided) whereas NoTalk-Earned does not differ from Nash ($p = 0.813$). We conclude that there is no tacit collusion in the NoTalk-treatments.[9]

While our aggregate output in our NoTalk-Earned treatment exactly matches the NE predictions, some discrepancies with the static Nash equilibrium at the cost-type level remain. In Table 3 we see that the efficient low-cost firm produces less than its NE quantity (signed-rank test, $p = 0.063$ for both Random and Earned), the high-cost firm's output is higher than its NE choice (signed-rank test, $p = 0.063$ for Random, $p = 0.125$ for Earned). In Figure 3, we see that for both NoTalk treatments the median low-cost firm output is less than the predicted 30, for the high-cost firm the median is above 18. These results are in line with the experiments cited above.[10]

[9]For the sake of completeness, we also note that aggregate outputs in NoTalk are statistically different from the levels suggests by KS, ERG or ES (all tests $p < 0.1$).

[10]Since all existing asymmetric Cournot experiments were also conducted with linear demand and cost, we can normalize the results by using the ratio of actual and Nash equilibrium output, q_i/q_i^{NE}, for a detailed comparison. For q_1, we find that this ratio varies between 0.96 and 0.99 in previous studies compared to 0.95 (Earned) and 0.96 (Random) for our two NoTalk treatments. For q_2, the q_i/q_i^{NE} ratio in previous studies varies between 1.03 and 1.09, and it is 1.09 (Earned) and 1.14 (Random) in our case. This confirms that our results

The outputs in the Talk treatments are clearly below the Nash point prediction. Averages of $Q = 40.5$ (Talk-Random) and $Q = 43.1$ (Talk-Earned) are significantly below the $Q = 48$ benchmark (signed-rank tests, $p = 0.063$). Having said that, the outputs are significantly above the level suggested by KS, ES and ERG (signed-rank test, $p = 0.063$).

Figure 6 shows the profit outcomes in the π_1-π_2 space for all four treatments at the session level. Each dot represents the average of one session across supergames 2 to 5. The figure shows that there is relatively little heterogeneity at the session level. NoTalk outcomes (square and triangular dots) are close to the static Nash equilibrium, the communication in Talk shifts payoffs toward the equal split (ES) outcome.

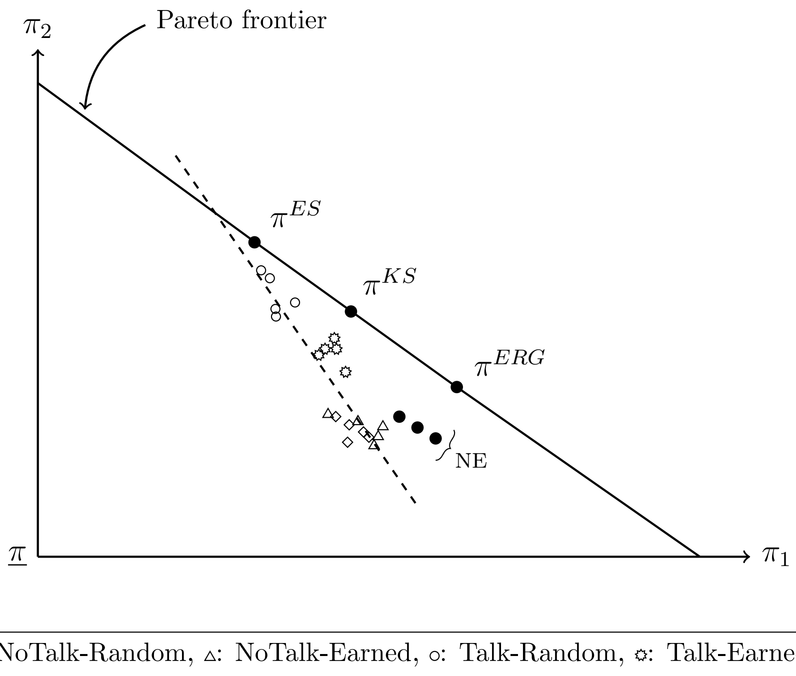

Figure 6: Session means in the π_1-π_2 space and benchmarks. The dashed line shows a linear regression of session averages, suggesting $\pi_2 = 1499 - 1.44\pi_1$ with an R^2 of 0.83. Some of the data points are slightly tilted for better readability.

Average profits are remarkably well organized by an (ad hoc) linear regression line – the dashed line in Figure 6. Starting from the triangular NoTalk-Earned dots, session averages in the other treatments "move" along this line toward ES where the round Talk-Random dots are close to the ES point. The slope of the regression line is -1.44, indicating that joint profits increase when the efficient firm earns more. All Talk session averages, however, lie to the north-west of the triangular NoTalk-Earned dots, indicating that only the high-cost firm gains from talking. The regression line suggests that sessions settle between the polar points NE and ES. In any event, we find significant differences in terms of the Euclidean distance of treatment profits to our various benchmarks ($p = 0.063$ for all benchmark-treatment combinations).[11]

Result 4. *(i) The No-Talk treatments are not collusive with aggregate output at (Earned)*

are in line with previous results. In terms of aggregate output, however, our NoTalk-Earned is the only dataset to hit $Q/Q^{NE} = 1.00$.

[11]For benchmark j the Euclidean distance to a session average (π_1, π_2) was calculated as $\Delta^j = ((\pi_1^j - \pi_1)^2 + (\pi_2^j - \pi_2)^2)^{0.5}$.

or above (Random) the static Nash equilibrium level. (ii) The Talk treatments are collusive with aggregate output below the NE. (iii) The theoretical benchmarks do not successfully predict bargaining outcomes.

5.3 How do firms collude in the Talk treatments?

To answer our second research question, we examine the nature of the agreements the firms coordinated on. Since there are no tacit agreements in NoTalk, we focus here on Talk-Random and Talk-Earned.

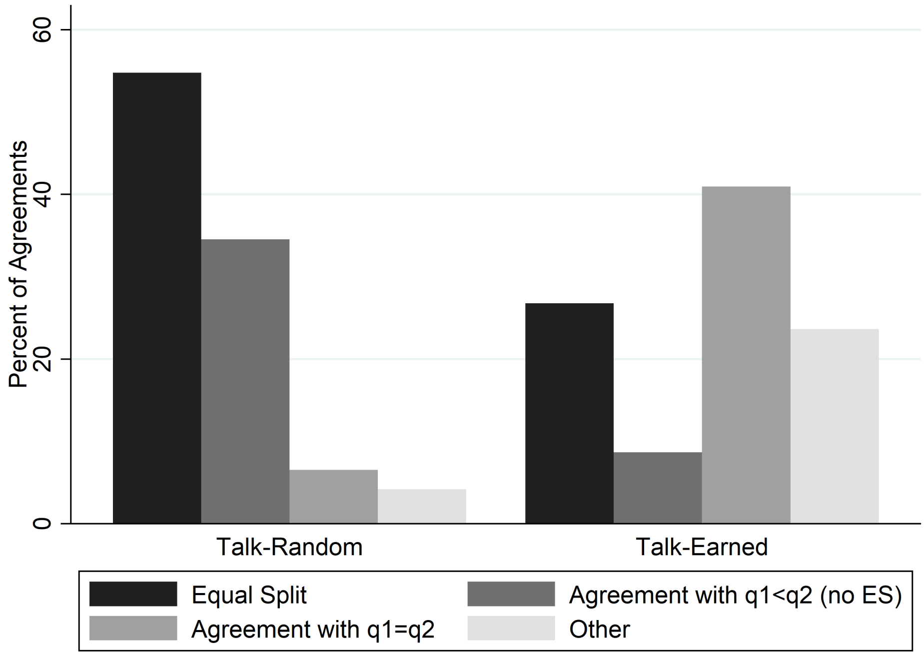

Figure 7: Distribution of agreement types, conditional on reaching an agreement.

To identify agreements in the chat of our 535 market pairs in Talk, we recruited two coders and provided them with incentives to code the chat data (Houser and Xiao 2011). We asked them to independently judge every chat dialog for whether a mutual and clearly specified agreement was present. Both were paid 10 cents for every evaluated market pair if and only if both drew the same conclusion regarding the agreement status. Their evaluations matched for 91% of market pairs. A measure of inter-coder reliability, Cohen's kappa, is $\kappa = 0.80$ which can be considered high.

The majority of market pairs in our Talk treatments decided to agree on a clearly specified joint production plan. In the Talk-Random sessions 77.8% of the markets came to an agreement whereas in the Talk-Earned sessions this rate was 59.9%. The difference is not significant (probit, clustered at the session level, $p = 0.336$, details available upon request). Only 2.8% of market pairs did not make any use at all of the possibility to exchange messages.

Figure 7 gives an impression of the different and most prominent types of agreements in the two Talk treatments. In Talk-Random, we note that a substantial share of firms (89.3%) agreed on either the equal split or other allocations in which the more efficient low-cost firm produces a smaller amount than the high-cost firm.[12] Those agreements with $q_1 < q_2$ that do not fully imply the equal split nevertheless have a strong similarity to it. Outputs in these cases were, on average, $(q_1, q_2) = (15.6, 22.4)$. Most prominent among the agreements not covered by the previous bins are agreements on equal quantities ($q_1 = q_2$) with about 6.6% of the agreements in that treatment. In Talk-Earned, agreements at or close to the equal split still amount to about 35.4% but are clearly much more rare. With 40.9%, the most frequent type of agreement has both firms producing the same output. The vast majority of these agreements (73.1%) involve quantities of 19 or 20 and almost all other observations have slightly lower quantities. In a multinomial logistical regression (with "Other" as the base category, details available upon request), Earned significantly reduces the likelihood of equal-split agreements ($p = 0.003$) and the likelihood of other agreements with $q_1 < q_2$ ($p = 0.001$).

The equal-output agreements are somewhat surprising at first sight and they do not appear in our list of benchmarks. In our data, equal quantities give the low-cost firm a payoff advantage when compared to the equal split outcomes. In fact, almost all agreements of this type involve $q_1 = q_2 = 19$ or $q_1 = q_2 = 20$ whereas the Pareto-efficient agreements (conditional on $q_1 = q_2$) range from $q_1 = q_2 = 16$ to $q_1 = q_2 = 20$. In other words, among the plausible $q_1 = q_2$ agreements, those favoring the low-cost firm are chosen almost throughout. To a certain extent, the efficient firm seems to make use of its cost advantage when roles in the Talk treatments are Earned.

In Appendix D, we provide further details concerning the agreements in the Talk treatments. We study the stability of the agreements over the course of the supergame. We will see that, even though players often agree on the ES solution, it is the deviations of low-cost players that prevent them from achieving an equal split (as seen in Figure 6).

Result 5. *(i) In the Talk variants, firms frequently come to an agreement during the chat phase. (ii) The modal agreement is $\pi_1 = \pi_2$ in Talk-Random and $q_1 = q_2$ in Talk-Earned.*

Concluding this section, we refer the reader to Appendix E where we offer a text-mining analysis of the chat data. We analyze what kind of language supports collusive behavior.

6 Discussion

At a general level, our analysis suggests that players often make use of symmetry criteria (equal profits, equal outputs) to reach an agreement. Related results are known from the literature. Roth and Malouf (1979) analyze an asymmetric, dynamic, and unstructured bargaining experiment. Two participants have to agree on a division of one hundred lottery tickets which

[12] We included the following agreements in the bin for the equal split: $(q_1, q_2) \in \{(14, 20), (14, 21), (15, 20), (15, 21)\}$.

determine the probability of winning a prize, worth three times as much to one of the players. Communication between players was possible and subjects could agree on any division within a time frame of 12 minutes. Roth and Malouf (1979) detect two types of agreements, the equal probability of winning (implying different expected payoffs) and equal expected payoff (three-quarter probability of winning for the low prize subject). These findings bear similarity to our equal quantities and equal payoffs agreements, respectively. Keser et al. (2014) and Keser et al. (2017) study linear public-good experiments in which players have asymmetric endowments. They note that subjects tend to follow a "fair-shares" rule: players coordinate on equal contributions relative to the endowment. This is necessary to make the high-endowment players benefit from the cooperation. When the degree of asymmetry in the endowments becomes too large (such that the efficient players would fail to benefit from the agreement), however, the norm shifts from equal relative to equal absolute contributions (Keser et al. 2014). The last result seems comparable to the equal-quantity scheme we observe.

The use of symmetry criteria may be explained by other-regarding preferences, especially in the Talk variants. In Fehr and Schmidt (1999) and Bolton and Ockenfels (2000), all else equal, players prefer the equal split, and, indeed, in most symmetric bargaining experiments, the equal split is ubiquitous. Our players are asymmetric, though. Nevertheless, the frequently observed equal split in our Talk data may be due to the fact that players are ex-ante symmetric when they arrive in the lab and only a random move gives some of them a cost advantage. Also, the communication may remove social distance. When the cost advantage is earned in a contest, the situation differs: low-cost firms invest costly effort; high-cost firms either invest less effort or are less capable. The role of the efficient firms is not due to luck any more but merit. As a result, we see fewer equal-split outcomes and efficient firms earn more. In these cases, we have a more obvious pluralism of fairness norms. Consider the following excerpt from the communication data of Talk-Earned:

```
Firm 1:  Hello
Firm 2:  21 me and 15 you, then we both get 630.  That's fair.  And like
this in every round.  Ok?
Firm 1:  Not bad ..  but I need to have an advantage for being firm 1
[...]
Firm 2:  Why?  That was luck.  Why not fair?
Firm 1:  No.  I have won.
```

The quote illustrates the conflict between what is perceived or declared as a fair norm. It also shows that statements about fair norms will often be self serving. See Konow (2000) and Capellen et al. (2007) for a detailed theoretical and experimental analyses of such norm pluralism.

Other-regarding preferences may also contribute to the understanding of our NoTalk results. The static Nash equilibrium brings along substantial payoff asymmetries of $\pi_1^{NE} - \pi_2^{NE} = 900 - 324 = 576$ (see Table 3). Reducing this payoff inequality is rather cheap for the inefficient firm: increasing its output by one output unit implies a payoff difference of only $\pi_1 - \pi_2 =$

$870 - 323 = 547$. In other words, at a cost of only one payoff unit, inefficient firms can reduce the payoff inequality by 29. This can explain why outputs are close yet significantly above the static Nash equilibrium in NoTalk-Random (and the literature).

Finally, we note that Result 2 suggests an apparently contradicting effect of earned roles (decreased Q in NoTalk, increased Q in Talk). Either way, however, play is pushed towards our benchmark for competition, namely the static Nash equilibrium. Earned roles reduce collusion in Talk (output increase) and reduces the excess competition in NoTalk (output decrease). In NoTalk-Earned, the role of the efficient firms is won by merit. This may reduce players' inequality aversion and reduce aggregate output towards Nash.

External validity is always a crucial issue in laboratory experiments, particularly for experiments in industrial organization. We found some striking parallels between lab and field evidence when it comes to this paper's results. With explicit communication, our finding that the inefficient firm is able to command more of the gains from cooperation has a parallel in the cartel case against lysine producers. In that case, relatively small firms were able to hold-up the larger firms for a larger percentage share of the market. Eichenwald (2000, ch. 8) provides a detailed description of the bargaining process made by the colluding asymmetric firms and is closely related to our main research question. Symeonidis (2003) analyzes a large set of legal cartels that occurred in the United Kingdom. He finds that explicit cartels are rare in concentrated industries. Symeonidis (2003) explains this finding by noting that concentration often comes along with asymmetries and that these hinder collusion. This corresponds to our finding that explicit cartels seldom emerge when communication is an option.

There are also parallels between lab and field data when it comes to cooperation without communication. Our data and previous work show that there is no tacit collusion in asymmetric duopolies. The reason why we did not investigate oligopolies with more firms is that tacit collusion in the lab is largely confined to duopolies (Huck et al. 2004). In the field, Kovacic et al. (2007) investigate the efficacy of tacit collusion after episodes of explicit cartelization. Specifically, they analyze for the vitamins cartels the pre- and post-plea prices for various products involving different numbers of firms. They find that duopolies successfully colluded tacitly after the explicit cartel was busted by the authorities. By contrast, three- and four-firm markets quickly lowered prices. So "two are few and four are many" (Huck et al. 2004, cited in Kovacic et al. 2007) also for post-cartel behavior in the field. Indirect evidence supporting the same notion comes from Davies, Olczak and Coles (2010). They analyze EU merger decisions to identify the criteria the European Commission employs in coordinated-effects cases, that is, in cases where a merger is likely to facilitate coordination in the post-merger market. They find that, from the perspective of the Commission's decisions, tacit collusion is restricted to duopolies. As for (a)symmetries, they note that tacit collusion can occur only with "more or less symmetric players." They emphasize that both findings are consistent with the experimental evidence (for example, Fonseca and Normann 2008, or recently Harrington et al. 2016).

7 Conclusion

How should firms collude in asymmetric Cournot oligopolies? There is no straightforward answer to this question. Asymmetric Cournot markets are intricate because there are no focal points and such oligopolies are accompanied by bargaining problems, inefficiencies (Bishop 1960, Schmalensee 1987 and Tirole 1988) and tight incentive constraints (Ivaldi et al. 2003). The main research questions of this paper are simple: Do firms manage to collude at all in asymmetric Cournot markets and, if so, how?

The first set of findings concerns our duopolies with (exogenously imposed) express communication. They have both firms produce positive amounts in each period (an alternative technology, alternating monopoly, is occasionally discussed in the chats but are largely dismissed and are not present in the data). Often, firms fix output combinations implying equal profits or revert to an equal-output strategy. As for the set of results concerning the bargaining outcome, we find that almost all the gains from explicit communication go to the inefficient firm. Only when the role of the efficient firm is earned in a contest do the low-cost firms perform somewhat better.

When subjects endogenously choose whether to communicate, we observe a tendency of the efficient firms to favor communication less often than the inefficient firms. Firms rarely agree to talk. Efficient firms could ask for more in the chat but, instead, they avoid communication altogether and play the non-cooperative solution.

Existing asymmetric Cournot experiments (Mason et al. 1992, Mason and Philips 1997, Fonseca et al. 2005, and Normann et al. 2014) are without communication and report average behaviors close to the static Nash equilibrium, with some discrepancies at the aggregate and individual output level. We add to this literature by demonstrating that allocating the role of the efficient firm through a contest is suitable for removing some of those discrepancies: aggregate output exactly matches the Nash prediction in our "earned role" treatment.

Our experimental design does not involve a (hypothetical) cartel authority imposing cartel fines for talking and possibly offering leniency (Hinloopen and Soetevent 2008, Bigoni et al. 2012). If it did, we would not be able to compare the outcomes of the communication and no-communication treatments. As a result, however, the frequency of cartelization we observe is merely an upper bound of what could be expected in the field. We also acknowledge that the nature of agreements may change depending on how the detection probability and fines depend on the agreement.[13]

[13]See Bos et al. (2017) for a recent theory paper on how competition policy affects cartel overcharges. In a field-data study, Normann and Tan (2014) show that the profitability of a cartel differed during a period where it was officially exempted from cartel prosecution.

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Appendix

A Optimal penal codes

In this Appendix, we derive the minimum discount factor that allows to implement any benchmark j as a subgame-perfect equilibrium of the infinitely repeated game (usually $j \in \{KS, ERG, ES\}$). Assume that players discount future payoffs with a discount factor of δ. We consider strategy profiles in which a deviator is punished by being min-maxed for one period. After punishment, play reverts to the benchmarks. For benchmark j, the payoffs of firm i are denoted as π_i^j. On the equilibrium path, benchmark j will be incentive-compatible for firm i if

$$\frac{\pi_i^j}{1-\delta} \geq \pi_i^{D,\text{on}} + \delta\pi_i^{-MM} + \frac{\delta^2}{1-\delta}\pi_i^j, \tag{IC-on$_i$}$$

where $\pi_i^{D,\text{on}}$ denotes firm i's optimal deviation from equilibrium play and π_i^{-MM} its payoffs when being min-maxed. Punishment will only be executed if doing so is incentive-compatible for the punisher. For the case where player i is the punisher it must hold that:

$$\pi_i^{+MM} + \frac{\delta}{1-\delta}\pi_i^j \geq \pi_i^{D,\text{off}} + \delta\pi_i^{-MM} + \frac{\delta^2}{1-\delta}\pi_i^j, \tag{IC-off$_i$}$$

where π_i^{+MM} denotes the punisher's payoff when min-maxing the other firm. Instead of punishing, the punisher can deviate himself, resulting in payoff $\pi_i^{D,\text{off}}$. Upon deviating, he would choose the best reply to the punished firm's minmax output.

When (IC-on$_i$) and (IC-off$_i$) hold for both firms, benchmark j will be an equilibrium of the repeated game. We obtain the minimum discount factor by solving the binding IC-constraints for δ. For firm i this gives:

$$\underline{\delta}_i^{\text{on},j} = \frac{\pi_i^{D,\text{on}} - \pi_i^j}{\pi_i^j - \pi_i^{-MM}} \qquad \underline{\delta}_i^{\text{off}} = \frac{\pi_i^{D,\text{off}} - \pi_i^j - \pi_i^{+MM}}{\pi_i^j - \pi_i^{-MM}}$$

The minimum discount factor for benchmark j is then given as:

$$\underline{\delta}^j = \max\{\underline{\delta}_1^{\text{on},j}, \underline{\delta}_2^{\text{on},j}, \underline{\delta}_1^{\text{off},j}, \underline{\delta}_2^{\text{off},j}\}.$$

Numerical values of $\underline{\delta}^j$ for the different benchmarks $j \in \{KS, ERG, ES\}$ are reported in section 2.

B Instructions (Talk-Earned treatment)

Part 1 (only used in the Earned treatments)

Welcome to our experiment. Please read these instructions carefully. Please do not speak with your neighbor and remain silent during the entire experiment. If you have any questions please raise your hand. We will come to your seat and answer your questions in person.

In this experiment you will repeatedly make decisions and thereby earn money. How much you earn depends on your decisions and the decisions of the other participants. You will receive a show up fee of 5 € for participating in the experiment. This amount will increase by the earnings that you make during the course of the experiment. At the end of the experiment the Taler you earned will be exchanged at an exchange rate of 2200 Taler = 1 €. All participants receive (and currently read) the same set of instructions. You will remain anonymous to us and all other participants of the experiment. We do not save any data relating to your name.

In this experiment you will repeatedly make decisions for a firm in a market. There are two firms in the market: firm 1 and firm 2. The experiment consists of two parts. In the first part of the experiment your decisions and the decisions of all other participants will determine whether, in the second part of the experiment, you will take the role of firm 1 or firm 2. In part 2, one participant in the role of firm 1 will always interact with one participant in the role of firm 2. Both firms produce and sell the same product in a market, however firm 1 has lower production costs than firm 2. The payoff of a firm depends on its production costs.

Your task in part 1 is to translate letters into numbers during a period of five minutes. Your screen will show a table with two columns, where the first column shows letters and the second column shows the corresponding numbers. The computer will provide you with a letter and you have to enter the corresponding number into the box on your screen. Subsequently you click on "OK".

When you confirm your answer you will be informed immediately whether your answer was right or wrong. In case the answer is wrong you will have to re-enter a number until your answer is correct. A new letter will only be shown once the current letter has been correctly translated into a number.

As soon as you have confirmed a correct answer the translation table will be recompiled with new letters and numbers and a new letter will be displayed for translation. You can translate an arbitrary amount of letters during the given time of five minutes.

In order to acquaint yourself with the program a test period will take place before the process starts.

For participating in part 1 of the experiment you will receive 4000 Taler, independently of the amount of translated letters.

Your role for part 2 of the experiment will be assigned as follows:

- After the five minutes have passed all participants will be assigned to one of two groups depending on the amount of letters that they translated. You will be assigned to group 1 if you conducted more correct translations than at least half of all participants. Otherwise you will be assigned to group 2. Thus, all participants in group 1 have translated more letters than all participants in group 2. In case several participants have translated the same amount of letters the computer will order them at random in order to guarantee an assignment to a group.

- If you were assigned to group 1 you will take the role of firm 1 in the following (firm 1 has lower production costs than firm 2)
- If you were assigned to group 2 you will take the role of firm 2 in the following (firm 2 has higher production costs than firm 1)

Immediately after the completion of the translation task you will be informed whether you have been assigned the role of firm 1 or firm 2. You will keep this role until the end of the experiment.

Please notice that in part 1 of the experiment participation in the translation task is not mandatory. Alternatively, you can e.g. read something or surf the Internet. However, please do not speak with your neighbor and please keep quiet.

Part 2 (only shown to subjects after completion of Part 1)

In this part of the experiment you repeatedly make decisions for a firm in a market. There are two firms in the market: firm 1 and firm 2. You have been assigned to the role of either firm 1 or firm 2 in the first part of the experiment. You will keep this role until the end of the experiment.

Both firms produce and sell the same product on a market. Every game of the experiment consists of several periods and in every period both firms must simultaneously make the same decision: Which quantity of the good do you want to produce? In every period, both firms can each produce a maximum quantity of 45 units of the good. Thus, the quantity you choose must be in between 0 and 45.

Information about the market and the firms:

- There is a uniform market price for both firms. The price (in Taler) which you receive for every unit of your product is calculated as follows:

  $$\text{market price} = 91 - \text{production quantity firm 1} - \text{production quantity firm 2}$$

  With every unit that your firm produces the market price for both firms is reduced by one Taler. Please keep in mind that the decision of the other firm has the same effect on the price: Every additional unit produced by the other firm also reduces the market price by one Taler.

- Both firms have different production costs: Firm 1 has per unit production costs of 13 Taler. Firm 2 has per unit production costs of 25 Taler.

- Your payoff per unit sold is the difference between the market price and your per unit production costs

$$\text{Payoff per unit} = \text{market price} - \text{production costs per unit}$$

  Please note that you make a per unit loss if the market price is below your per unit production costs.

- Your per period payoff is equal to your per unit payoff multiplied by the number of sold units:

$$\text{Payoff per period} = \text{Payoff per unit} * \text{Number of sold units}$$

  You can assume that all the units you produce can also be sold.

- In order that you can see which quantities lead to which payoffs we provide you with a payoff calculator. With it you can calculate the profits which result from different quantity combinations on your screen before you make your actual decision. Before the beginning of the first game you will have the opportunity to acquaint yourself with the profit calculator.

- At the end of every period you will receive feedback about the quanity decisions of both firms, the realized market price and your payoff. Additionally, the computer will show the total payoffs that you obtained so far.

Example: Suppose that you are firm 1. Thus, your production costs are 13 Taler per produced unit. You decide to produce 30 units of the good. Subsequent to your decision you receive the information that firm 1 decided to produce 18 units of the good itself. Hence, the resulting market price is 43 Taler and your per unit payoff in this period is 43 Taler - 13 Taler = 30 Taler. Thus, your payoff in this period is 30 Taler * 30 = 900 Taler.

Course of action: Every game consists of one or several periods. After every period, chance decides whether another period takes place: The computer randomly draws a number between 1 and 4. If a "1", "2", or "3" is drawn then another period takes place; with a "4" no further period is conducted and the current game ends. Hence, it can happen that a game is over already after a single period. Equally it can happen that a game continues for many rounds. As soon as a game ends a new game will be started. The experiment consists of a total of 5 games for which the following holds:

- All games of the experiment have the same structure. This means that in every period of every game the above-described production decision has to be made.

- If a game ends you will be assigned to a new partner at the beginning of the next period. You will not meet any previous partner in any future game.

Communication: At the beginning of every game the two firms have the possibility to communicate with each other. For this purpose a text box will appear on your screen. You and the other firm can exchange typed messages in it in which you can talk about anything. The only restriction is that you must not identify each other (e.g. do not write your name). There will be no further possibility to communicate during or after the periods.

The following Figure 8 gives a schematic summary of part 2 of the experiment.

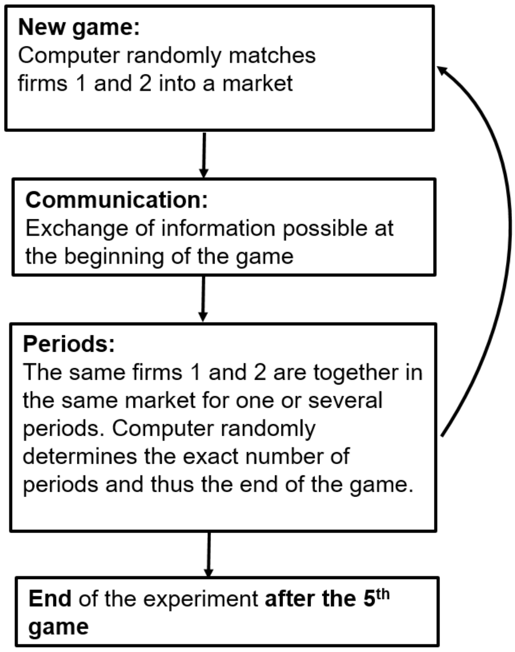

Figure 8: Schematic summary

C Additional materials

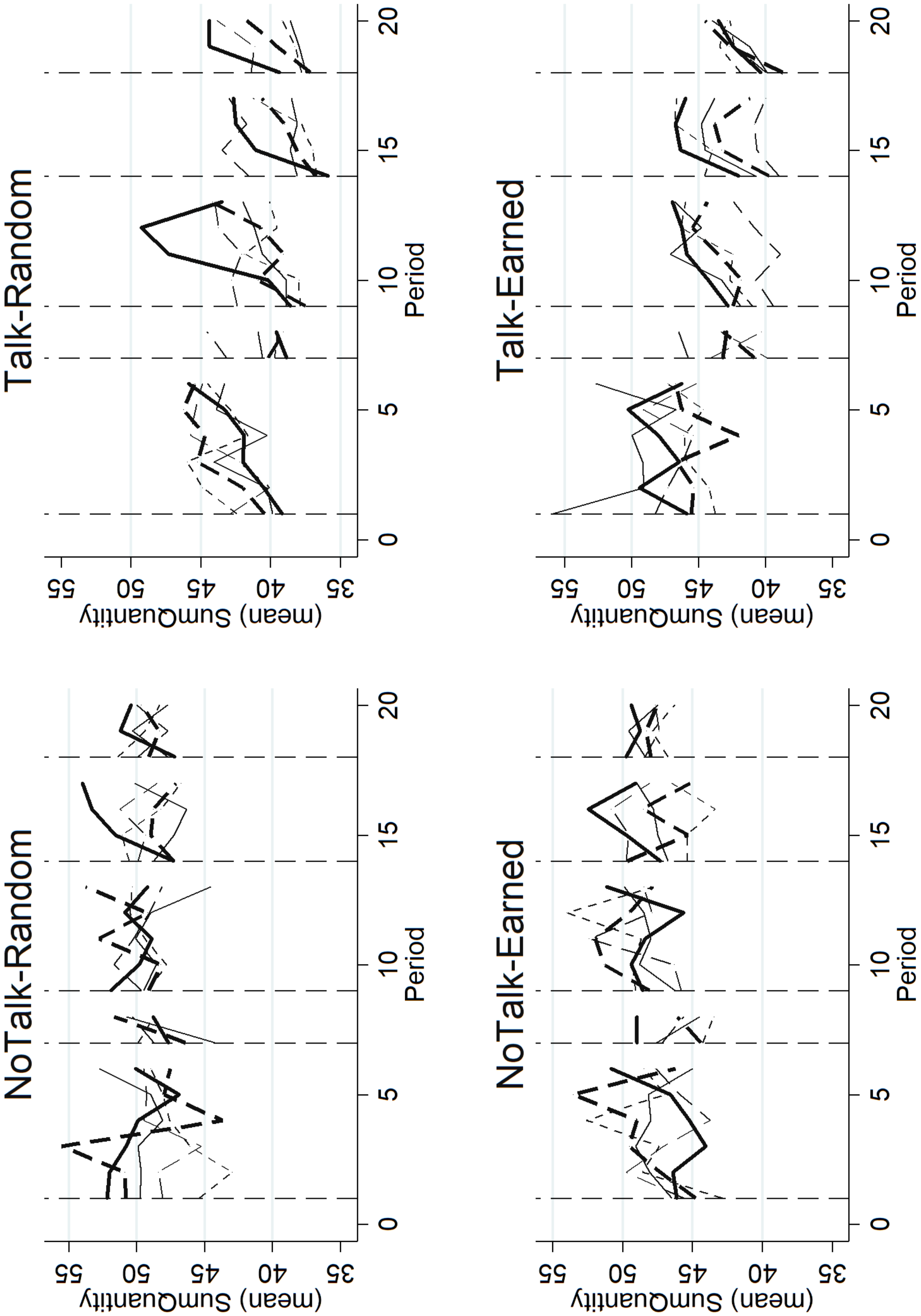

Figure 9: Aggregate quantities, Q, across periods, by sessions and treatment.

D Stability of the agreements in the Talk treatments

In this Appendix, we analyze how stable the agreements made in the Talk treatments were over the course of the supergame. Figure 10 shows by treatment, point in time, and firm type the share of periods where a (unilateral or bilateral) deviation from an agreement occurred in a market. Several interesting patterns emerge. First, significantly fewer deviations from agreements occur in the first period of the supergames when compared with the later periods. This relationship emerges in both treatments and for both firm types. Second, deviations tend to occur more often in the Talk-Earned than in the Talk-Random treatment, particularly for the non-initial periods of the game. Third, the efficient firms deviate substantially more often than the inefficient firms. The probit regressions in Table 6 confirm these effects are statistically significant.

To understand these patterns in the deviations, it is instructive to look at the best responses (and therefore optimal deviations) given the other firm chooses its agreed upon quantity. Taking the two most popular agreement types – equal split and equal quantities – and comparing the size of the optimal deviations for low-cost and high-cost firms indicates that the low-cost firm should adopt a much larger deviation from its agreed quantity (+3 units when ($q_1 = 15$, $q_2 = 21$) and +6 units when $q_1 = q_2 = 19$). Comparing the magnitude of the actual average deviations between firms confirms this pattern. The average deviation of the low-cost firm from its agreement quantity is +2.11, while for the high-cost firm it is significantly lower ($p = .003$), namely +0.30 . There is not much of a difference between the size of the average deviations in the two Talk treatments.

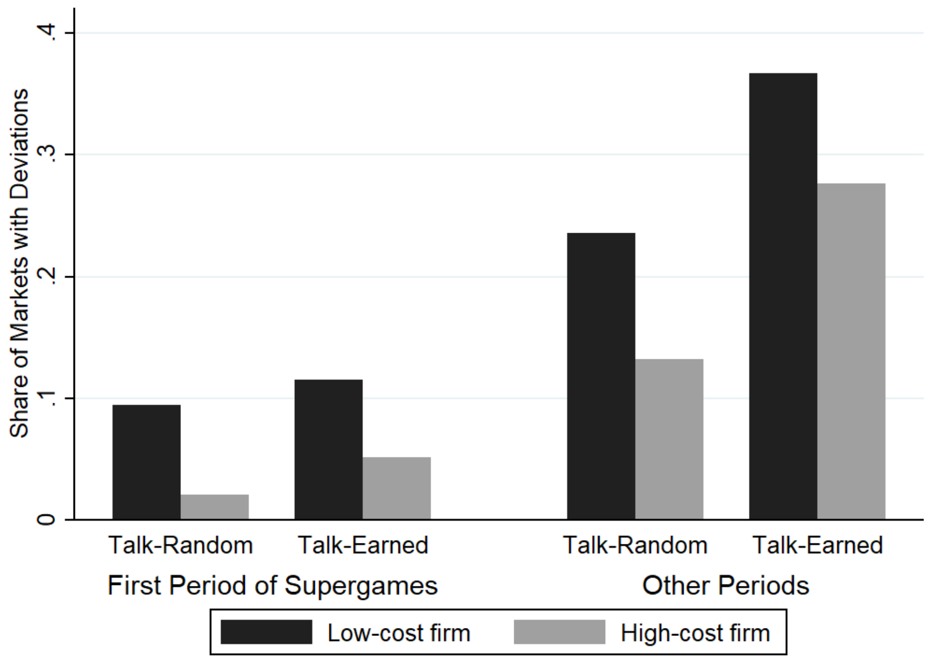

Figure 10: Deviation frequencies from agreements

Dependent variable	Deviation
Earned	0.481**
	(0.229)
Low Cost	0.372***
	(0.0808)
Initial period	-0.924***
	(0.285)
Earned × Low Cost	-0.180*
	(0.109)
Earned × Initial Period	0.182
	(0.371)
Initial Period × Low Cost	0.102
	(0.227)
Initial × Low Cost × Earned	-0.395
	(0.439)
Constant	-1.039***
	(0.114)
Observations	1,996

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6: Probit regression with dummy *deviation* as the dependent variable, clustered at the session level.

E Text-mining analysis

In this section, we want to analyze in more detail the use of language in the chat treatments. What kind of language is useful to support collusion? Do efficient and inefficient firms differ in the content of their communication? And how do the Random vs. the Earned treatments affect chat?

Our method of analyzing the text will be *text mining*. To our knowledge, Moellers, Normann and Snyder (2017) were the first to use this method in experimental economics. Text-mining methods extract keywords from a body of text, referred to as a corpus. We will compare the most frequently used keywords for two corpora in order to find out how the corpora (the chats) differ. To be more precise, we will use Huerta's (2008) *relative rank difference* which tells us which keywords are comparatively more frequently used in corpus c relative to c'. Formally, we measure the keyness of word w in corpus c relative to c' by generating ranks $r_c(w)$ for all words w in corpus c according to frequency (and in descending order). The difference in the rank of w in corpus c relative to corpus c' is defined as

$$rd_c^{c'} = \frac{r_{c'}(w) - r_c(w)}{r_c(w)}$$

We restrict ourselves to keywords that are among the top 50 most common in corpus c, avoiding

Efficient firm				Inefficient firm			
Random		Earned		Random		Earned	
word	rd	word	rd	word	rd	word	rd
14	7.94	very	9.78	14	13.11	units	1.46
21	7.25	only	7.41	;-)	2.93	firm 1	1.44
equal	5.20	round	6.06	equal	2.83	us	1.21
23	4.77	both	4.25	21	2.00	20	1.17
17	4.53	us	3.93	630	1.24		
ok	1.33	let	2.36				
15	1.20	quantity	1.98				
		each	1.58				
		19	1.40				
		20	1.33				
		best	1.19				

Efficient firm				Inefficient firm			
Collusive		Non-collusive		Collusive		Non-collusive	
word	rd	word	rd	word	rd	word	rd
firm 2	8.11	25	12.22	14	2.67	30	6.82
firm 1	2.24	me	9.81	thanks	2.66	25	2.82
14	2.10	produce	2.43	19	1.93	have	1.46
19	2.09	20	2.00	too	1.84	17	1.38
yes	1.50	30	1.33	yes	1.75	20	1.17
make	1.14	but	1.31	has	1.26		
	1.14	output	1.26	have	1.05		
	1.06	not	1.09				

Table 7: Text-mining analysis. We report words with absolute rank $r_c \leq 50$ and relative rank differential $rd \geq 1$.

keywords with a high $rd_c^{c'}$ that are nevertheless rarely used. We omit conjunctions, prepositions, and articles and report keywords with $rd_c^{c'} > 1$.

We are interested in how treatments Random vs. Earned are reflected in the communication and how collusive supergames differ from non-collusive ones. We further expect different firm types to use different language.

The top panel of Table 7 reveals some interesting insights into the differences in chat in Random vs. Earned. In Random, the output pairs that yield the equal split ("15" and "21") occurs in the list, as does the neighboring "14" and the word "equal" (payoffs). There are some differences between firm types: inefficient firms point out the resulting payoff of "630" that both players would earn and use the wink emoji whereas efficient firms suggests higher quantities or merely confirms ("ok"). Chat in the Earned treatments differs substantially. Keywords used by the efficient firms literally result in the equal-output strategy: “let us both [produce] 19 (20) in each round.” The words “very” and “only” appear in different contexts. When inefficient firms want to move away from the equal-output strategy, they have to suggest different outputs for different firms and hence use “firm 1.”

Language used in (un)successfully colluding duopolies can be obtained from the bottom panel of Table 7. Both types of successfully colluding firms use the confirming “yes” and the equal-payoff output combination (“14”, “19”). Successful collusion requires strategies that draw a distinction between “firm 1” and “firm 2.” Inefficient firms add “thanks,” indicating that they are aware that the efficient firm could earn more by playing non-cooperatively. The non-collusive groups mention higher output targets among their keywords. Words like “but” and “not” hardly indicate successful collusion, whereas “me” hardly indicates the mutuality inherent to agreements.

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ISSN 2190-9938 (online)
ISBN 978-3-86304-282-0