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Explicit vs. Tacit Collusion – The Impact of Communication in Oligopoly Experiments*

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August 2012

Abstract

We explore the difference between explicit and tacit collusion by investigating the impact communication has in experimental markets. For Bertrand oligopolies with various numbers of firms, we compare pricing behavior with and without the possibility to communicate among firms. We find strong evidence that talking helps to obtain higher profits for any number of firms, however, the gain from communicating is non-monotonic in the number of firms, with medium-sized industries having the largest additional profit from talking. We also find that industries continue to collude successfully after communication is disabled. Communication supports firms in coordinating on collusive pricing schemes, and it is also used for conflict mediation.

Keywords: cartels, collusion, communication, experiments, repeated games.

JEL classification: C7, C9, L4, L41

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1 Introduction

A central principle of antitrust policy is that firms must not communicate with each other. If firms in an industry talk about prices, they engage in cartel activities that are illegal in most jurisdictions. Evidence that firms explicitly talked to each other is often the smoking gun in antitrust procedures, as it usually presents a per se violation of competition law. Without communication firms may still coordinate on prices, but such tacit agreements are treated in an entirely different way from a legal perspective. Thus, whether or not firms communicate is absolutely central to antitrust policy.

The significance of explicit communication in policy is in stark contrast to its relative lack of importance in economic theory. Since talk between firms is illegal and cannot be enforced, the incentives to adhere to collusive agreements are the same with and without communication. This implies that the set of equilibria in repeated oligopoly is often the same regardless of whether firms talk[1] — which seems at odds with the fundamental distinction in policy. As Harrington (2008, p. 6) puts it, "there is a gap between antitrust practice which distinguishes [between] explicit and tacit collusion and economic theory which (generally) does not."

One impact of explicit communication that has been acknowledged by economic theory is that it may help firms to coordinate (see Crawford and Sobel, 1982; and Farrell and Rabin, 1996, for a review). In repeated games, there are many collusive equilibria and therefore firms face a coordination problem. Cheap talk seems useful as it can enable firms to coordinate on a certain equilibrium. Whereas the positive impact of such cheap talk in pure coordination games (like the battle of the sexes) is undisputed, its effect on dilemma games like oligopoly is subject to debate among theorists (see Farrell and Rabin, 1996; Whinston, 2008).

Even if we accept the notion that communication may facilitate coordination in repeated oligopoly, details and rigorous analysis remain elusive. Whinston (2008, p. 21) re-

[1] Explicit communication is analyzed in only few papers: Athey and Bagwell (2001), Athey, Bagwell and Sanchirico (2004), and Athey and Bagwell (2008). Harrington and Skrzypacz (2011) propose a set of conditions for which non-binding announcements of sales can sustain collusive equilibria when firms' prices and quantities are private information.

marks that "[i]t is natural to think that talking may help with this coordination, but exactly to what degree, and in what circumstances is less clear." Similarly, Porter (2005) argues that industrial economists do not fully comprehend how communication affects market outcomes. If talking indeed helps resolve coordination problems, one would like to know when demand for such coordination (and therefore communication) arises and what the effect of overcoming coordination problems is.

The Whinston (2008) quote rather accurately summarizes the research goal of this paper: when and to what degree does communication help? To this end, we report on a series of experiments comparing Bertrand oligopoly markets with and without explicit (cartel-like) communication. We vary the number of firms as the "circumstances" under which communication may help. Studying the number of firms seems promising to us as we can employ the conventional wisdom that fewer firms find it easier to collude as a testable hypothesis. Our experiment is the first to conduct a comparative-statics analysis of firm numbers with communication and hence also the first to investigate the interaction of firm numbers and communication. We design markets with two, four, six and eight firms, and we analyze to what extent communication effectively aids collusive practices.

Previous experiments (see Dawes et al., 1977; Isaac et al., 1984; Isaac and Walker, 1988; or surveys by Crawford, 1998, and Balliet, 2010), have shown that non-binding communication can facilitate cooperation in dilemma games. The main focus of more recent experiments on communication have been on the mechanisms through which talking can induce cooperation. Andersson and Wengström (2007) find for Bertrand duopolies that prices are higher and collusion is more stable when communication is costly, but the prices under costless communication are still above the static Nash equilibrium. Cooper and Kühn (2012) study a simple collusion game which is followed by a coordination game, and they analyze what type of communication is most effective in achieving cooperation in this setup. (See also the influential paper by Charness and Rabin, 2006, which is on a trust game, not a dilemma.)

In our view, there is a strong case for an experimental investigation of the effect of communication on collusion. One advantage of running an experiment is the control the experimenter has over the communication between firms in this case. Under laboratory conditions, disallowing communication very effectively ensures that there will be no talk among subjects. If communication is allowed, the experimenter has perfect transparency about what is said by whom. By contrast, in field data, all we know is whether a cartel has been detected in the past. We never know whether firms actually talked and, moreover, firms have incentives to conceal and bias information due to the illegal nature of explicit collusion. A second key advantage is that an experimental investigation can avoid the sample-selection problems empirical cartel studies face (Posner, 1970). The empirical cartel literature can only analyze industries that (i) decided to set up an illegal cartel in the first place, (ii) were detected, and (iii) were prosecuted. The resulting sample of industries may be not representative regarding the workings of cartels.

Several questions may remain unanswerable if we limit ourselves to field data, one of them pertaining to the long-run effects of communication. Is tacit collusion easier to sustain after a period of communication? If so, the number and the effectiveness of cartels in activity could be higher than presumed. The only evidence from the long-term effects of communication comes from an experiment on the effect of communication on the provision of a linear public good. Isaac and Walker (1988) find that provision of public goods increased when subjects communicated face-to-face. Further, cooperation remained high even after communication was no longer allowed. We extend this finding by looking at the benefits of (non-face-to-face) communication in the context of a price-setting market.

We have four main results. First, like previous experimental research on communication (Isaac et al., 1984; Isaac and Walker, 1988), we find clear evidence that talking leads to higher profits in markets with any number of firms. Second, we confirm the conventional wisdom that collusion is easier the fewer the firms holds under both the no communication and the communication conditions. This results is novel for the communication condition; it has only been observed in the literature for the case of no communication (see Huck et al., 2004, and the literature therein).

Third, by comparing the two communication conditions, our perhaps most significant finding emerges: the gain from communicating does not monotonically decline in the number of firms but is inversely U-shaped, with the medium-sized industries having the largest additional profit from talking. This raises interesting questions for antitrust policy. If indeed the gain from communication determines the frequency of cartels (as should be the case in the field), the suitability of the conventional wisdom for structural cartel screening would be reduced.

A fourth result is that we find a hysteresis effect (see Isaac and Walker, 1988) in that industries continue to collude successfully after communication is disabled—a result perhaps worrisome for anti-trust authorities. Our data suggests that after a period of collusion supported by regular communication, firms are able to maintain collusive prices even when communication is no longer possible. Harrington (2004) argues that the methods for calculating antitrust damages in price-fixing cases create a strategic incentive for firms to maintain non-competitive prices after the cartel has been dissolved. While this incentive is not present in our experiments by design, our data show that there are further reasons to worry that industries maintain high price after cartel dissolution.

2 Experimental design and procedures

We analyze Bertrand oligopoly markets with inelastic demand and constant marginal cost of production (Holt et al., 1986; Dufwenberg and Gneezy, 2000). There are $m = 300$ consumers who demand one unit of the good up to the reservation price of $p^{max} = 100$. The $n \in \{2, 4, 6, 8\}$ firms simultaneously and independently select a price (p) for a homogeneous good. Their action sets are the integers $\{0, 1, ..., 100\}$. Each firm is able to supply all consumers at production costs of zero. The firm charging the lowest price earns pm (in the case of ties, the firms split that profit evenly) whereas high-price firms earn nothing.

We have treatments with and without the opportunity to communicate, labeled "Talk" and "NoTalk". In NoTalk, subjects had to post prices in each period without being able to communicate to each other. In Talk, subjects were allowed to communicate with one another for one minute in every period of the experiment via typed messages, using an instant-messenger communication tool. The limit of one minute was sufficiently long for the communication phase as most talk ended (also in the larger oligopoly markets) before the one-minute period was over. Subjects were free to post as many messages they liked, but they were not allowed to identify themselves or to post offensive messages. Subjects were aware that they communicated to their entire group and nobody outside the group. This form of communication is one of the least restrictive forms available and is one of the most effective in terms of facilitating cooperation (Crawford, 1998; Brosig et al., 2003.) While potentially being a noisier form of communication, it may also be natural to participants. It seems appropriate for cartel negotiations (as opposed to one-sided pre-formulated announcements). Free chat may also reduce the potential for experimenter demand effects — restricting messages to be of a particular nature may signal to participants the research objective of the experiment, which could bias their behavior. Furthermore, Crawford (1998) indicates that communication in experiments works as a means by which participants can reassure each other and reduce uncertainty about their decisions. Such reassurance will be most effective when expressed in free-form language (Brosig et al., 2003).

We compare the Talk and NoTalk conditions within subjects. Each experimental session was divided in two parts. Similar to Brandts and Cooper (2007), communication was impossible in the first part of most treatments (NoTalk) whereas talking was allowed in the second part of the experiment (Talk). Subjects only read the instructions for the second part of the experiment after the first part had ended. Also, at no point prior to the beginning of part two was it mentioned that there would be the possibility for communication. In order to control for order effects, we ran an additional treatment with $n = 4$ firms where communication was allowed in the first part of the experiment but not in the second part (here, the order is Talk–NoTalk). Furthermore, in order to control for the effect of experience, we ran a control treatment where firms were not able to communicate in either part of the experiment (NoTalk–NoTalk). Subjects were matched with the same participants in both

Talk and NoTalk conditions. Table 1 summarizes our treatments.

Phase 1	Phase 2	$n = 2$	$n = 4$	$n = 6$	$n = 8$
NoTalk	Talk	✔	✔	✔	✔
Talk	NoTalk		✔		
NoTalk	NoTalk		✔		

Table 1: Experimental Design

All treatments were implemented as a repeated game (fixed-matching scheme), and there were at least 20 periods in both parts of the experiment. From the 21^{st} period on, a random stopping rule determined whether the experiment would go on or stop. We chose a continuation probability of 5/6.[2] The actual number of periods of the two phases (29 and 24 periods, respectively) were determined ex ante and was the same in all sessions.

We generated six markets (or groups) for each treatment. We conducted all duopolies in one session. We had two sessions for the four-firm and six-firm markets, and we had three sessions for the eight-firm markets. The sessions were run in the FEELE lab at the University of Exeter and were programed in z-Tree (Fischbacher, 2007).

We provided written experimental instructions (which were read out loud) which informed subjects of all the features of the market. Sample instructions are available in the Appendix. Specifically, subjects were told they were representing a firm in a market with other firms. In each period, after communicating in the Talk parts, subjects had to enter their price at a computer terminal. Once all subjects had made their decisions, the period ended and a screen displayed the prices chosen by all firms in the market and the profit of each individual firm in that particular period. The screen also displayed the accumulated

[2]The continuation probability of 5/6 ensures that the conventional wisdom is actually predicted in the repeated game for both communication conditions if the continuation probability is interpreted as the discount factor. For probabilities smaller than 0.5, none of our oligopolies have collusive equilibria, while the opposite is true for probabilities of at least 0.875 (see below); in this case all oligopolies have a collusive equilibrium. Dal Bó (2005) presents evidence that the continuation probability may have an impact in repeated prisoner's dilemma experiments.

profits of the individual participant up to that point but not those of other participants.

Payments consisted of a show-up fee of £5 plus the sum of the profits over the course of the experiment. For payments, we used an "Experimental Currency Unit (ECU)"; 50,000 ECU were worth £1 in the duopolies and accordingly with higher n such that maximum possible earnings were equalized across treatments. Sessions lasted for about 60 minutes and the average payment was £13.71 (roughly \$22).

In total, 168 students participated in our experiments. Subjects participated in one experiment only and had not participated in similar oligopoly experiments before.

3 Hypotheses

We begin with the impact of the number of the firms. Starting with Chamberlin (1929), there is a firm belief in the industrial organization literature that the fewness of firms facilitates collusion. This conventional wisdom appears on the common lists of factors facilitating collusion (Scherer, 1980; Tirole, 1989; Ivaldi et al., 2003; Levenstein and Suslow, 2006). That collusion is easier with fewer firms is intuitive and can be easily formalized. In our setup, the collusive profit an individual firm makes is pm/n when all firms charge the same price p—which is decreasing in n. By defecting from the collusive agreement, a firm can make a profit of no more than pm, regardless of the number of firms. The static Nash equilibrium has all firms obtaining zero profit, again independently of the n.[3] Thus, in an infinitely repeated game with Nash-reversion trigger strategies and where firms discount future profits by a factor δ, we need $pm/(n(1-\delta)) \geq pm$ for collusion to be a subgame-perfect equilibrium, or

$$\delta \geq \frac{n-1}{n}. \tag{1}$$

[3] In the static (one-shot) variant of this game, all n firms charging a price equal to the marginal cost of zero is a Nash equilibrium. Since prices are integers, there is an additional equilibrium where all firms set a price of one. In any event, firms make zero (or near zero) profits in static Nash equilibrium.

As the number of firms in the market increases, the minimum discount factor required for collusion to be successful also rises.[4]

Note that the formalization of the conventional wisdom as summarized in (1) has been applied to both tacit collusion (e.g., Tirole, 1989; Ivaldi et al., 2003) and explicit cartels (Levenstein and Suslow, 2006). We therefore conjecture that the conventional wisdom will hold for both the NoTalk and the Talk conditions. Regarding NoTalk, several experiments without communication have already confirmed this hypothesis, although in different settings.[5] For the Talk condition, we are unaware of a systematic experimental investigation of the effect of the number of firms.

Hypothesis 1: *Without communication, prices will decrease with the number of firms.*

Hypothesis 2: *With communication, prices will decrease with the number of firms.*

Then consider the impact of communication. While our paper is among the first that analyze the infinitely repeated game with communication (independently, Camera et al., 2010, study infinitely repeated prisoner's dilemma experiments), some previous research is relevant. Several experiments have shown that communication can improve cooperation in dilemma games, but its effectiveness depends on the format of the communication. In repeated settings, one-sided communication (like unilateral price announcements) typically loses its impact over time (Holt and Davis, 1990; Cason, 1995) whereas multilateral communication can lead to persistently higher prices (see the posted-offer markets with face-to-face communication in Isaac et al., 1984).[6] Crawford (1998, p. 294) argues that multilateral

[4]This is the standard result. For some Bertrand-Edgeworth models, the minimum discount factor required for collusion *de*creases when there are more firms. See Compte et al. (2002) and Kühn (forthcoming).

[5]Fouraker and Siegel (1963) study Bertrand duopolies with two- and three-firm oligopolies (and with a demand function different from ours). Dolbear et al. (1968) study Bertrand oligopolies with differentiated products with two, four and 16 firms where the incentive to collude is constant by design. Isaac and Reynolds (2002) analyze posted-offer markets with two and four firms. Dufwenberg and Gneezy (2000) study the same game, however, they look at treatments employing random matching. Finally, Cournot oligopolies are studied in Fouraker and Siegel (1963) with two and three firms, as well as in Huck, Normann and Oechssler (2004) with two, three, four and five firms. Holt (1985) studies infinitely-repeated Cournot duopolies. Waichman, Requate and Siang (2010) study cournot duopolies and triopolies with and without pre-play communication, and with students and manager subjects. Huck, Normann and Oechssler (2004) provide a meta analysis of Cournot experiments.

[6]Friedman (1967) is, to our knowledge, the first oligopoly experiment with communication, but the paper

communication has a "reassurance effect" which helps to coordinate on more efficient equilibria. Since our experiment employs open, free and simultaneous communication, we expect communication to lead to higher prices:[7]

Hypothesis 3: *Prices will be higher when communication is possible.*

We are particularly interested in the *gain* from communication, that is, the additional profit firms generate from being able to talk. The gain from communication should be decisive in the field for the decision to establish a cartel. Whether firms in an industry collude tacitly or set up an explicit cartel is not exogenously given. If they set up a cartel, they reject the opportunity to collude tacitly. Put differently, a cartel not only faces direct costs (in terms of the expected penalty and the cost of organizing the cartel), but also opportunity costs (the profit forgone by not colluding tacitly). Thus, if both tacit collusion and talking explicitly are an option (which is the case in the field), then the additional profit from communicating should matter for cartelization.

How the gain from communication depends on the number of firms is an open question. Even if Hypotheses 1 to 3 turn out to hold, this does not imply anything regarding the relationship between the gain from talking and n. The gain from talking is the difference between two monotonically decreasing functions, but this difference could be anything—monotonically increasing, monotonically declining or indeed non-monotonic. Thus we do not have a testable hypothesis here.[8] We formulate instead:

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does not have a no-communication control treatment. Seminal in the social psychology literature is Dawes, MacTavish and Shaklee (1977).

[7]One may counter-argue that communication enables renegotiations which make punishments that are necessary to sustain collusion non-credible (Bernheim and Ray, 1989; Farrell and Maskin, 1989). Such weakening of punishment possibilities should work against the positive effect communication is hypothesized to have on cooperation. However, a negative impact of renegotiation possibilities does not clearly come out in experiments. Cooper and Kühn (2012) find that price agreements are even more likely when they allow for renegotiation. Camera et al. (2010) also find no drop in cooperation due to renegotiation possibilities.

[8]In the fiels, the likelihood of cartel detection possibly increases with the number of firms. If so, large oligopolies would face higher expected cartel fines, and then the gain from communication minus the expected cartel fine may decrease with n, suggesting the conventional wisdom will hold. While this argument is intuitive, we are not aware of any research systematically documenting it. Moreover, the conventional wisdom has not been confirmed in field-data cartel studies (see below), which means that this argument probably does not have much force.

Exploratory Research Question 4: *How does the gain from communication depend on the number of firms?*

The control treatment with the reversed order of treatments (Talk-NoTalk) allows to analyze how the communication conditions affect each other. From previous experiments, it is known that communication may have a positive effect on cooperation even after it has been disabled; see Balliet (2010) for a meta study on dilemma experiments with communication and Isaac and Walker (1988) for a public-goods experiment. Cooperation may decrease after communication is removed (Frohlich and Oppenheimer, 1998), but the cooperation level is typically still higher than if there is no occasion to communicate at all, as pre-play communication experiments also show (Brosig, Ockenfels and Weimann, 2003; Costa-Gomes, 2002). If this effect materializes, the NoTalk–NoTalk treatment serves to control for the mere effect of repetition. We have:

Hypothesis 5: *Prices will be higher in the no-communication phase if this condition is preceded by a phase of communication.*

4 Main treatment effects

We report our results in two parts. Section 4 will focus on the (quantitative) effects our treatments have on selling prices, whereas Section 5 is about how firms collude (qualitatively) in terms of their pricing strategies and the communication content.

A few remarks on how we handled the data and on the statistical tests applied are warranted. In most parts of the analysis, the data we report are *selling prices.* Selling prices, or the winning bids, are equivalent to industry profits in our setup. Unless otherwise indicated, we employ non-parametric tests where we conservatively count each of the six groups (markets) we have for each treatment as one single observation and take the average selling price across all periods in a given market. We exclude the data from periods 1 to 5 from the analysis because we find a time trend that is particularly pronounced in the early NoTalk periods (see below). None of our results change qualitatively if we include the first five periods though. Finally, we analyze the treatment with the reversed order (Talk–NoTalk, which has $n = 4$ firms) and the control treatment NoTalk–NoTalk (also $n = 4$) separately from the other treatments because of significant differences we observe. That is, in this subsection and the next the next subsections, we only consider treatments where the sequence was NoTalk-Talk.

4.1 The number of firms and the impact of communication

First, we test whether the conventional wisdom holds in our experiment and we begin with NoTalk (Hypothesis 1). Figure 1 shows the average selling prices across all treatments. We see that average selling prices monotonically decline as the number of firms increases in NoTalk. The correlation of average selling prices and n is significant (Spearman's $\rho = -0.9318, p < 0.001$, based on 24 group averages). We find that, despite the only minuscule differences between the $n \geq 4$ markets, all treatments differ also in pairwise comparisons (two-sided Mann-Whitney U tests, all *p-values* < 0.05).

Quite obviously, only the duopolies are able to maintain somewhat collusive prices. This confirms the findings in the literature (see footnote 5) that "two are few and four are many" (Huck, Normann and Oechssler, 2004), meaning that markets are unlikely to be (tacitly) collusive when there are more than three firms.[9] Put differently, one could interpret the prices above one as very low collusive prices but probably a more sensible interpretation is that prices reflect the non-collusive solution for $n > 2$.[10]

Result 1: *Without communication, selling prices decline in the number of firms, although they are close to marginal cost for $n \geq 4$.*

[9]Interestingly, this result is consistent with what appears to be the model implicitly used by the European Commission in their coordinated-effects merger decisions. Davies et al. (2011) estimate this model and conclude that, from the point of view of the EC's merger policy, tacit collusion is confined to duopoly.

[10]If we look at posted prices rather than selling prices, we observe some heterogeneity: the average posted prices in the NoTalk condition we equal to 54.23, 18.18, 23.41 and 29.36 for $n = 2, 4, 6$ and 8, respectively. Posted price are not monotonic in the number of firms, mainly because of high-price signalling becomes more frequent with large n.

With communication (Talk), we also see a significant decline in average selling prices when the number of firms increases (Spearman's $\rho = -0.5076, p = 0.011$). Hence we find support for Hypothesis 3. The effect of the number of firms on prices appears to be linear and qualitatively different than the relationship between number of firms and prices when communication is absent. This finding is new to the experimental literature. There is also some heterogeneity across groups, reflected in the fact that treatments do not differ in pairwise comparisons, except for $n = 2$ vs. $n = 8$ (two-sided Mann-Whitney rank-sum, $p = 0.036$).[11]

Result 2: *With communication, selling prices decline with the number of firms.*

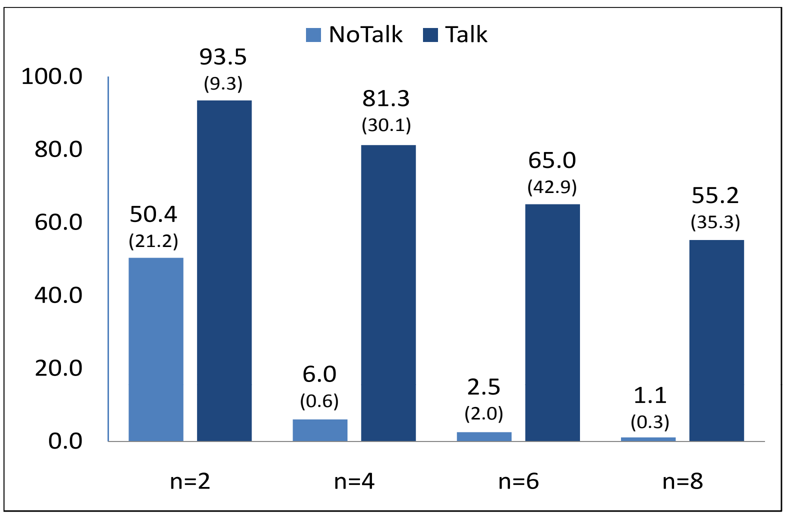

Figure 1: Selling prices by treatment (standard deviations based on market averages in parenthesis).

For all n, we see a sharp rise in selling prices under the Talk condition (dark bars) compared to NoTalk (light bars). Indeed, all 24 individual markets have higher prices in the Talk phase. These differences are highly significant (two-sided Wilcoxon signed-rank test, *p-value* < 0.001), rejecting the null hypothesis that communication has no effect and supporting our Hypothesis 3.

[11] A conspicuous finding in Figure 1 is that eight firms with communication results in a level of competition commensurate with the duopolies without communication — which of course is consistent with Results 1 to 3.

Result 3: *Prices are higher with communication.*

4.2 The gain from communication

We now turn to our Explorative Research Question 4, the relationship between the gain from communicating and the number of firms. Figure 1 shows that the gain from communicating is not monotonic. Selling prices increase by 43.2 for $n = 2$, by 75.3 for $n = 4$, by 62.5 for $n = 6$ and by 54.1 for $n = 8$. That is, the highest gain from communication is realized in the four-firm oligopolies whereas duopolies and eight-firm markets have the lowest incentive to communicate. In other words, we observe an inversely U-shaped relationship between the gain from communication and n. Statistical support for this result is that the four-firm oligopolies have a higher gain from talking than the duopolies (two-sided Mann-Whitney U test, $p\text{-}value = 0.055$) whereas the six- and eight-firm markets do not ($p\text{-}value = 0.337$ and $p\text{-}value = 0.631$).[12]

We can exploit the heterogeneity of the data at the market level to illustrate which share of industries would choose to talk, had this choice been available in our experiment rather than exogenously imposed, and were the choice to talk associated with a fine. If an industry chooses not to talk, it may be tacitly collusive (if the price is above marginal cost) or it may be competitive (if not). For simplicity assume that firms are risk-neutral and face an expected cartel fine of 50: one would obtain the pattern in Figure 2, which plots the relative frequency of firms whose gain from talking outweighs the fine (dark blue = explicit cartel); whose gain from talking is not sufficient to cover the expected fine, but can coordinate implicitly (light blue = tacit collusion); whose gain from talking is not larger than the expected fine, and whose price is close to Bertrand-Nash (grey = competition). We observe the inverse U-shaped pattern again and that the $n = 4$ oligopolies exhibited

[12] Our perspective is that firms generally do not talk but that they may choose to do so if the gain from communication is sufficiently high. However, Martin Dufwenberg pointed out to us that it could well be the other way round; that the opportunity to talk is always there in the field and that the experimental NoTalk condition is better thought of as a fictitious antitrust authority's ideal state. From this perspective, our results can be reinterpreted in that we show that the antitrust authorities' preoccupation with communication is warranted.

the highest frequency of explicit cartels. The duopolies are collusive (either with or without talk), and the share of competitive industries increases in n. Remarkably, Harrington (2010, p. 39-40) conjectures that this very picture will emerge in the field. This is merely an illustrative example. The picture looks qualitatively the same for other levels of the cartel fine, unless fines are prohibitively high (in which case no industry would choose to talk) or sub-deterrent (so that they all choose to talk).

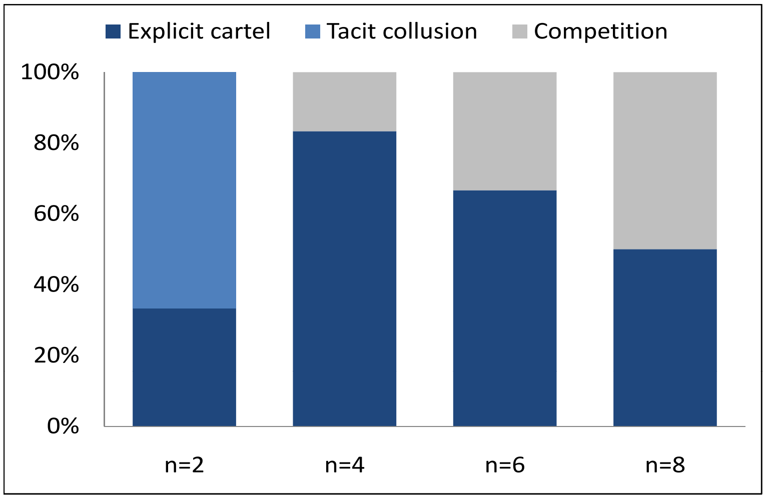

Figure 2: The share of industries for which talking pays ("Explicit cartel") given a hypothetical expected cartel fine of 50. If the gain from talking is less than 50, the industry will either be tacitly collusive (price above marginal cost) or competitive.

Result 4: *The gain from talking is inversely U-shaped with the number of firms.*

We wish to emphasize that our results should be not interpreted quantitatively but qualitatively. In this context, Holt (1995) was already skeptical that there would be a "magic" number of firms beyond which markets would be competitive because structural factors other than the number of firms (e.g., the underlying oligopoly model) would also have an impact, in and outside the lab. The same point concerns our finding that industries with four firms have the highest likelihood of being explicitly collusive. Both in the field and in different experiments, the incentives to collude explicitly may turn out to be different, especially since the average selling prices for $n \geq 4$ are already quite close to zero and statistically not different from each other. Qualitatively, though, we expect our findings to be a useful basis for further research.

Comparing our results to the cartel literature, it is (at least at first sight) irritating that the empirical evidence regarding the conventional wisdom in the field is mixed. Already, Posner (1970, p. 410) found that a "large proportion" of the cartels he studied were "in industries not normally regarded as highly concentrated." Hay and Kelly (1974, p. 21) observe that "in many cases larger groups conspire." And Levenstein and Suslow (2006, p. 58) conclude that "there is no simple relationship between industry concentration and the likelihood of collusion" in their influential recent meta study on cartels.

Why is there no clear-cut evidence? The empirical cartel literature only observes those industries that decided to form a cartel and were subsequently detected. Thus, even if the conventional wisdom were true, it may not be observable in cartel field data since there is a sample-selection bias. In the field, only those industries that do not find the opportunity to collude tacitly attractive will cartelize. Note at this stage a key advantage of the experiment: by imposing the communication condition exogenously, we can avoid the sample-selection problem of cartel field data. In other words, while our data confirm that "collusion is easier with fewer firms" both with and without communication, they also show that this finding may not imply that there are more explicitly colluding cartels with fewer firms in the field where the decision to talk is endogenous. Thus, the conventional wisdom may not be a useful guiding principle for structural cartel detection in the field.[13] Our results suggest that looking for non-linear effects could be promising.

4.3 The long-run effects of communication

In this section, we will focus on the variant in which we reversed the order of the treatments. For $n = 4$ firms, we ran six markets where subjects had the opportunity to talk in the first phase but they did not have the same opportunity in the second phase. So, the sequence

[13] In his survey paper, Harrington (2008) dismisses structural methods of cartel defection and analyzes behavioral methods instead.

was Talk–NoTalk in this variant.

The order effect we observe is remarkably strong. If we compare selling prices under the NoTalk condition, we find an average selling price of 6.0 (standard deviation: 0.6) when the order is NoTalk-Talk, but 67.0 (33.8) when the sequence is Talk–NoTalk. This difference is significant (two-sided Mann-Whitney rank-sum test, p-$value = 0.055$). By contrast, when we compare prices under the Talk conditions, we do not find any differences. Average selling prices are 81.3 (30.1) for NoTalk-Talk and 89.0 (8.7) for Talk–NoTalk (two-sided Mann-Whitney rank-sum test, p-$value = 0.873$). The data thus support Hypothesis 5. We have an *hysteresis* effect: when the order is Talk–NoTalk, firms are significantly better at tacitly colluding without communication. We even observe one market in Talk–NoTalk where the average selling prices are higher in the NoTalk phase.

The data of the NoTalk–NoTalk treatment confirm that the hysteresis result is not simply a learning effect. In the NoTalk–NoTalk control treatment, subjects were unable to communicate in either phase, but they could have learned from their experience in the first phase—just as in Talk-NoTalk. The average selling price was 2.84 (3.63) in the first phase of the experiment, and 5.69 (8.42) in the second phase. Neither are significantly different from the average price in the NoTalk condition in our main four-firm oligopoly treatment. This shows that it is the communication that matters in Talk–NoTalk and not the repetition.

Result 5: *There is an hysteresis effect from communication. Prices are higher without communication if this condition is preceded by a phase of communication.*

The hysteresis effect has an implication for policy. Fines and private damages may be calculated by taking the difference between the price when the cartel was active and the post-detection price. If hysteresis effects are present, fines and damages would be underestimated.

4.4 The evolution of prices

Concluding this section, Figure 3 presents the time trends for all treatments. In periods 1 to 29 (NoTalk for the standard treatments), oligopolies with $n > 2$ converge near the Nash equilibrium within five periods. In periods 30 to 53 (Talk for the standard treatments), there is a negative time trend in all treatments. The time trend is insignificant for all treatments except $n = 8$ ($p = 0.0313$, sign test on six group-level Spearman correlation coefficients per treatment), suggesting some heterogeneity at the group level.[14] In treatment Talk-NoTalk (see Section 4.3 below), there is no negative time trend in the first (Talk) phase but a negative time trend in the second (NoTalk) phase.

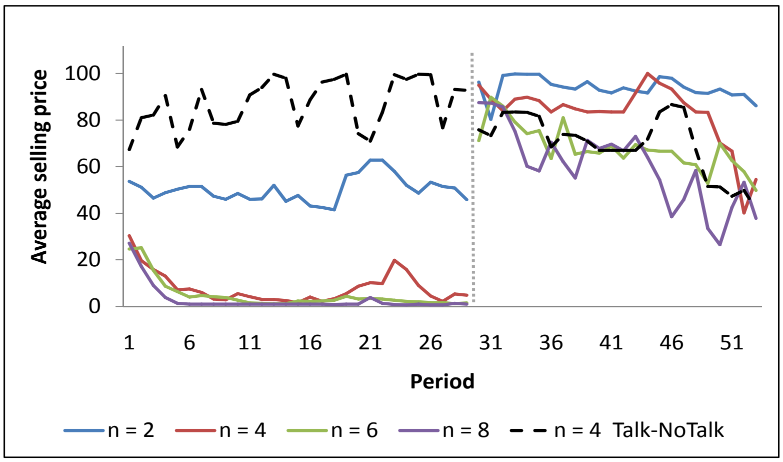

Figure 3: The evolution of average selling prices.

5 Collusive pricing strategies

5.1 The distribution of selling prices

In this section, we will take a look at the pricing strategies firms employ over time. Upfront, we report the distribution of selling prices, denoted henceforth by $\underline{p} := \min\{p_1, ..., p_n\}$. Table 2 shows the relative frequency of selling prices from period six onwards. For NoTalk, Table 2 shows that there are virtually no observations with $\underline{p} \geq 99$, which seems—at least for the

[14]If we include the last 5 periods of the Talk phase in the analysis only because of this time trend, the results in this section do not change. Profits in Talk would be 90.57, 62.97, 58.63, 38.73 for $n = 2, 4, 6$ and 8, respectively; so we still confirm the conventional wisdom. The gain from collusion is still inversely U-shaped, with 40.21, 56.95, 56.09, 37.64 profit gain for $n = 2, 4, 6$ and 8, respectively.

duopolies—surprising. The frequency of competitive prices ($\underline{p} \leq 1$) in NoTalk increases in the number of firms (Spearman's $\rho = 0.867$, $p\text{-}value < 0.001$). In the Talk treatments, there are many outcomes where $\underline{p} \geq 99$, although the frequency of prices with $\underline{p} \geq 99$ decreases with the number of firms (Spearman's $\rho = -0.400$, $p\text{-}value = 0.054$).

	NoTalk				Talk			
	$n = 2$	$n = 4$	$n = 6$	$n = 8$	$n = 2$	$n = 4$	$n = 6$	$n = 8$
$\underline{p} = 100$	0.0	0.0	0.0	0.0	60.5	74.6	23.7	12.3
$\underline{p} = 99$	2.1	0.0	0.0	0.0	14.9	2.6	28.9	23.7
$2 \leq \underline{p} \leq 98$	97.9	64.9	35.0	5.6	24.6	11.2	28.1	45.6
$\underline{p} \leq 1$	0.0	36.1	66.0	94.4	0.0	11.4	19.3	18.4

Table 2: Distributions of selling prices (relative frequency in percent)

5.2 Pricing strategies as Markov processes

We analyze the pricing dynamics in Talk as first-order Markov processes. We consider three states: (i) $\underline{p} = 100$, (ii) $\underline{p} = 99$, and (iii) $\underline{p} < 99$, and we use the data from all periods. State (i) is modal for $n = 2, 4$ and state (iii) is modal for $n = 6, 8$. State (iii) is rather broad, but including an additional state does not yield further insights or results that differ qualitatively (the analysis with $\underline{p} \in [50, 98]$ as an additional state is available from the authors upon request).

Table 3 displays the transition matrices for each of the four treatments. The ij^{th} element of each matrix is the relative frequency with which a market will move from state i in period t to state j in period $t + 1$. The diagonal elements indicate stable behavior, while off-diagonal elements indicate transitions away from a given state.[15]

[15] Based on the transition matrices, we compute the ergodic distribution of states for each treatment. Table 4 in the Appendix shows the observed likelihood for each of the three states and the corresponding ergodic distribution. The table shows that the transition matrices accurately capture the dynamics of the underlying process, despite the coarsening of the state space.

$n=2$		To		
		$\underline{p}=100$	$\underline{p}=99$	$\underline{p}<99$
	$\underline{p}=100$	0.92	0.02	0.06
From	$\underline{p}=99$	0.04	0.87	0.09
	$\underline{p}<99$	0.19	0.00	0.81

$n=4$	To		
	$\underline{p}=100$	$\underline{p}=99$	$\underline{p}<99$
$\underline{p}=100$	0.95	0.03	0.02
$\underline{p}=99$	0.10	0.70	0.20
$\underline{p}<99$	0.09	0.03	0.88

$n=6$		To		
		$\underline{p}=100$	$\underline{p}=99$	$\underline{p}<99$
	$\underline{p}=100$	0.85	0.03	0.12
From	$\underline{p}=99$	0.05	0.82	0.13
	$\underline{p}<99$	0.04	0.09	0.87

$n=8$	To		
	$\underline{p}=100$	$\underline{p}=99$	$\underline{p}<99$
$\underline{p}=100$	0.78	0.19	0.03
$\underline{p}=99$	0.07	0.72	0.21
$\underline{p}<99$	0.01	0.04	0.95

Table 3: Transition matrices (Talk treatments).

A key observation in Table 3 is that all three states are rather stable. The likelihood to stay in state (i) depends on n and varies between 78 and 95 percent. The probabilities to stay in state (ii) range between 70 and 87 percent, and the third state is the most stable one for the $n = 6$ and $n = 8$ treatments (with 81 to 95 percent stability). Confirming this point, we compute an index of mobility (Shorrocks, 1978; see also Riener, 2011, for a discussion) for this transition matrix, given by $\phi = (k - tr(M))/(k-1)$, where k is the number of states and $tr(M)$ is the trace of the transition matrix M. We find $\phi = 0.20$ for $n = 2$, $\phi = 0.24$ for both $n = 4$ and $n = 6$, and $\phi = 0.28$ for $n = 8$. The Shorrocks index of stability increases in the number of firms, which implies a greater mobility of transitions from state to state for higher n. But the general level of the index suggests low mobility.

The counterpart to behavior being stable within states is, of course, that firms rarely move between states. Specifically, firms find it difficult to (re-)establish successful collusion (state (i)) when being in states (ii) and (iii). Even with communication, firms do not often manage to get back to successful collusion with all firms charging 100 once the price has dropped.

5.3 Pricing strategies in chat discussions

We will now illustrate the findings in Tables 2 and 3 with a comprehensive discussion of the collusive pricing strategies. Kimbrough, Smith and Wilson (2008) demonstrate, in a different setting, that quotations from the chat transcripts can be rather illuminating and complement the quantitative analysis. We follow them here by quoting from the chat discussions.

Collusion where all firms charge the same price. In state (i), all firms collude at $p_1 = p_2 = ... = p_n = 100$. There are only six observations where firms set a common price that is not 100. Thus, if there is common-price collusion in Talk, it is in the $p_1 = p_2 = ... = p_n = 100$ state.

How long do these agreements last? We take the average length of the $p_1 = p_2 = ... = p_n = 100$ agreements for each market as the unit of observation. Averaging across markets yields the average length of collusive agreements as 10.8, 16.0, 4.3 and 3.6 for $n \in \{2, 4, 6, 8\}$, respectively. The $n = 4$ stand out here with longer collusive agreements than the duopolies. Despite this non-monotonicity, the correlation of duration and n is significant (Spearman's $\rho = -0.4734, p = 0.020$). In pairwise comparisons, the duration of cartels in $n = 2$ vs. $n = 6$ ($p = 0.092$), $n = 2$ vs. $n = 8$ ($p = 0.076$) and $n = 4$ vs. $n = 6$ ($p = 0.037$) differ (all two-sided Mann-Whitney tests). In the chat discussions of almost all Talk markets, subjects often immediately suggest a price of 100. Occasionally, some player would suggest a price floor as a collusive strategy but such proposals are quickly overturned. Indeed, more than half of our markets (14 of 24) also successfully implement $p_1 = p_2 = ... = p_n = 100$ right in the first period. Here is an example ($n = 4$, market 1, period one):

```
Firm A: everyone sell for 100
Firm B: then split the money!
Firm D: yeah 100 sounds good
Firm A: eventually we can all get 50000 ecu
Firm C: no point going down to 0-10 its worth pence
Firm A: exactly
Firm B: because we're all here to get money!
Firm D: exactly
Firm C: yeh, ok, so everytime split between 4?
Firm A: yes
```

```
Firm B: wicked 100 it is?
Firm D: yes
```

Next, consider state (ii) and the Talk treatments. To understand behavior in this state it is important to note that $\underline{p} = 99$ always coincides with at least one firm charging $p = 100$ in our data. Hence, state (ii) may indicate deviations from state (i) or instances where firms explicitly collude by taking turns (leaving aside the third possibility of coordination failure due to disagreements or misunderstandings).

Collusion where firms take turns in being the low-price firm. Explicit agreements where (typically) one firm becomes the low-price firm ($p = 99$) whereas all other firms charge $p = 100$ are a second popular collusive strategy. Taking turns is slightly less efficient than all firms charging 100 and this inefficiency is often mentioned in the markets whose firms take turns.[16] We have five markets which take turns repeatedly: one duopoly (over 20 periods), two six-firm markets (over 12 and 18 periods), and one eight-firm market (14 periods). Interestingly, we do not observe a single instance of deviation when firms take turns (see below our general discussion of deviations), so this strategy is rather successful.[17] We find that these five markets explain roughly half of the $\underline{p} = 99$ observations (64 of 120).

Here is an exemplary discussion ($n = 2$, market 2, period one) for the taking-turns strategy:

```
...
Firm A: surely, we should take it in turns putting 99 and 100?
Firm B: so choose 100 each time?
Firm A: that would make more profit?
```

[16]One can show that the minimum discount factor required for such a taking-turns strategy to be subgame-perfect Nash equilibrium in the infinitely repeated game is implicitly defined by $1 - \delta^n - \delta^{n-1} = 0$. That is, the minimum discount factor varies between 0.62 (for $n = 2$) and 0.91 ($n = 8$) which is slightly above the threshold of $(n - 1)/n$ required for common-price collusion (see (1)). Also, note that the taking-turns strategy would be even less efficient if the action space was coarser.

[17]As an aside, we note that the $n = 4$ Talk data stand out in various dimensions. They are more likely to be in state (i), more likely to stay in state (i), and their agreements also have a longer duration than the duopolies. However, selling prices (profits) are higher with $n = 2$ than with $n = 4$. There are two explanations for this. First, there is no $n = 4$ market where firms take turns but there is one duopoly. As taking turns occurs in state (ii), the focus on state (i) in this comparison underestimates the performance of the duopolies. Second, selling prices with $n = 4$ are rather low whenever $\underline{p} < 100$. So pricing in the $n = 4$ Talk markets is rather polarized, and they are rather unforgiving after collusion collapses.

```
Firm B: oh okay, alright you go first, choose 99
Firm A: ok
Firm B: and then I will afterwards, deal?
Firm A: when i put 99 you put 100, deal
```

Defections. As can be seen in Table 3, defections occur. The likelihood of moving from state (i) to states (ii) or (iii) varies between 4.7 percent ($n = 4$) and 22.6 percent ($n = 8$). (We find no defections when firms take turns.) This raises the question of what kind of behavior such defections trigger in the subsequent periods. Here is an example of a defection where communication is used—successfully, as it turns out—to coordinate on compensating the cheated-upon firm ($n = 2$, market 1, period 5):

```
Firm A: sorry!
Firm B: sneaky!
Firm A: just felt like we should be bidding or something....!
Firm A: ill do 100 next time if you want to put 99, then we're fair ...
Firm B: ok, yeah that would work
```

It is useful to take a look at Markov chains of length 2, where a market moves from state (i) to state (ii). We observe 21 cases where all firms charge a price of 100 in $t-2$ and at least one firm charges 99 in $t-1$. In ten of these cases, firms manage to maintain $\underline{p} \geq 99$ in period t, as in the above example.[18] Thus even in what appears to be textbook cases of defection from common-price collusion, firms only trigger punishments in about half of the cases. Note that these "punishments" are rather broadly defined as $\underline{p} < 99$ occurring at least once, which is rather different from permanently triggering $\underline{p} \leq 1$. This reluctance to start punishment phases seems reasonable since $n \geq 4$ firms find it difficult to move back to higher states (see Table 3).[19] Here is another example, with $n > 2$ firms. Without saying much, players quickly suggest resuming the collusive strategy, and they do manage to re-establish

[18]A look at the chat data reveals that two cases were deliberate moves from common-price collusion to a strategy of taking turns; thus there are only 19 deviations, and in eight cases a decline to $\underline{p} < 99$ was prevented. Oligopolies with $n = 4$ are least likely to stay in $\underline{p} \geq 99$ (1 of 5 cases) after a defection whereas $n = 8$ oligopolies are most likely to do so (5 of 7 cases).

[19]Consistent with these results, Genesove and Mullin (2001) find that cheating did occur frequently but only rarely triggered punishments in the extensively communicating sugar cartel (1927-1936).

$p_1 = p_2 = p_3 = p_4 = 100$ in this case ($n = 4$, market 4, period 15):

```
Firm C: HOW COULD YOU?!?!?!?!
Firm B: sorry, i thought d was bluffing
Firm A: B - that is not fair!
Firm D: come on i am goin fr 100 again
Firm C: YOU'VE BETRAYED MY TRUST COMPLETELY
Firm D: B its not fair
Firm A: 100 last try
Firm D: be with 100
...
Firm C: 100 all of us pls
```

Threats, punishments and price wars. In any model of repeated interaction in oligopoly, threats are essential to sustain collusive agreements. We find some evidence of threats, but they are rare. There are examples of ex-ante conditional strategies that failure to collude will result in a price war ("`cause if you choose 99 we will end up fighting`"; $n = 2$, market 2, period one). Often threats are uttered only after a deviation ($n = 8$, market 2, period 3):

```
Firm G: E!!!
Firm C: reeeally?
Firm H: what was that?
Firm F: if you dont play fair 1 i will set 1 each time
...
```

State (iii) includes severe price wars where $\underline{p} \leq 1$. Table 2 suggests that such price wars do occur regularly in Talk with $n \geq 6$ firms. Here is an example that documents the complex interaction of threats, punishments and prices wars over several periods.

Market 4 of the eight-firm oligopolies successfully established a common price of 100 in periods 1 to 12. In period 13, Firm A deviates with a price of 99. In the following chat, Firm G threatens to trigger a price war ("`if you do it again - its heading for 1's`"), Firm A apologizes, and the firms agree to resume collusion rather than triggering the punishment. In period 14, Firm A sets 99 and Firm B charges 98. Several players suggest returning to colluding at a common price of 100. Firm G threatens directly again ("this is your last chance before i start hitting 0") and so does Firm F (''if this happens again, we'll all undercut and you'll be the losers"). Firm E does not agree to trigger the punishment ("we all get more in the end if we stick to 100") and some confusion results (Firm D: "who's hitting what?"), but, in the end, players agree to get back to 100. In period 15, Firm C deviates with a price of 99 while all others choose 100. Despite the third deviation in a row, there are new suggestions to set prices of 100. These are first dismissed by Firm G ("it's passed that"); nevertheless, in the end Firm G also agrees again to the common price of 100. In period 16, Firm A and Firm H deviate with prices of 99 and 88, respectively. Despite several pleas ("not zero!", Firm B), Firms F and G finally trigger the punishment, a price of zero. But to no effect: for the remaining seven periods of the experiment, an average price of 38.4 results, with Firm A mostly being the low-price firm, and successful collusion could not be established any more.

The breakdown of this cartel can be attributed to the persistent deviations by Firm A and other firms trying to get their share, but disagreement among players to trigger a punishment and the subsequent failure of Firm G to carry out its threat may have encouraged such behavior. The threats lost their credibility, and all firms except for Firm D made at least one attempt to undercut in these periods in the end. In our data, price wars often appear to be a result of coordination failure or of a cartel in decline (as in this example) rather than a consciously triggered coordinated punishment (as the single zero-price punishment in period 16).

Observation 6: *In Talk, if industries successfully collude, they do so mostly by all firms charging* $p_1 = p_2 = ... = p_n = 100$*, or less often by taking turns in being the low-price firm. Defections occur but they frequently do not lead to a decline of the prices as communication is used for conflict mediation.*

6 Conclusion

Antitrust law and practice suggest that communication is crucial for collusion. Laboratory experiments (including ours) confirm that communication clearly leads to higher prices. But how much does communication help firms to establish collusive prices? And in which circumstances it is particulary helpful? By studying Bertrand oligopolies with various number of firms both with and without the possibility to communicate, we can investigate these issues.

We find that the medium-sized industries benefit the most from talking as they are rather competitive without communication, but are still able to maintain some collusion by talking. In contrast, the duopolies have little to gain from talking as they already earn decent profits without talking, and the large oligopolies gain less because they find it difficult to collude successfully even with communication. This result sheds an interesting light on the conventional wisdom that increasing the number of firms in a market reduces their ability to collude on supra-competitive prices (Chamberlin, 1929; Scherer, 1980; Tirole, 1989; Ivaldi et al., 2003). We find that the conventional wisdom holds regardless of whether firms can communicate, but the gain from communicating is non-monotonic. This may explain the puzzle in the empirical cartel literature that the conventional wisdom often does not materialize (Posner, 1970; Levenstein and Suslow, 2006): when firms can endogenously choose between tacit and explicit collusion (as is the case in the field), they may face incentives that are not monotonic in the number of firms.

Our experimental data also illustrate *how* communication supports collusion. There are three main channels. Communication helps firms coordinating on a price or more sophisticated pricing patterns (like taking turns in placing the low bid). This is in stark contrast to the treatments without communication where firms virtually never coordinated successfully, not even the duopolies. It appears that talking removes the strategic uncertainty present otherwise and only with communication do firms manage to coordinate on a price, sometimes even among a large numbers of firms. Communication is, secondly, frequently used for dispute mediation in our experiments. Defections occur, but they do frequently not lead to price wars. In fact, conflict mediation to avoid the decline of prices appears to be among the central uses of communication. While Genesove and Mullin (2001) report similar findings for the Sugar Institute cartel case, it is probably fair to say that this issue is underexplored by standard theory.

Finally, we find that communication has a long-lasting effect on cooperation (hysteresis): collusion is more effective without communication if it is preceded by a phase of communication, as has been observed in other social dilemmas (Isaac and Walker, 1988 – although in that paper, communication was done face-to-face, while ours was anonymous and via a computer terminal). This is consistent with Crawford's (1998) argument that communication provides reassurance about players' intentions. These findings help to fill the void between the way economic theory approaches the value of communication in repeated oligopoly games and the way antitrust law and practice view communication among firms.

In our experiments firms are symmetric, there is no uncertainty and players receive complete feedback on all firms' actions. This setting thus abstracts from some of the core problems cartels face (Rotemberg and Saloner, 1986; Athey and Bagwell, 2001; Athey, Bagwell and Sanchirico, 2004; Athey and Bagwell, 2008; Harrington and Skrzypacz, 2011). While some of these issues have been analyzed in experiments without communication, it does seem promising to study the impact of communication in these modified settings.[20]

Another open research question is what happens when the choice of the communication condition is endogenous. In such a setting, firms may choose whether to talk but, if so, a cartel fine arises with a certain probability. The experimental literature on leniency programs (Apesteguia, Dufwenberg and Selten, 2007; Hinloopen and Soetevent, 2008; Bigoni et al., 2009) analyzes similar settings, however, in these studies the focus is on the effect of leniency and not how the number of firms affects the decision to set up a cartel (all studies employ three-firm markets). We leave this issue for future research.

[20]Imperfect monitoring in prisoner's dilemma experiments (without communication) has been studied by Feinberg and Snyder (2002) and Aoyagi and Frechétte (2009); Ruffle (2010) investigates the impact of demand fluctuations on tacit collusion; and Fonseca and Normann (2008) analyze tacit collusion when firms have asymmetric capacities.

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Appendix

Appendix A: Instructions

Instructions - Part 1

Hello and welcome to our experiment. Please read this instruction set very carefully, since through your decisions and the decisions of other participants, you may stand to gain a significant amount of money. We ask you to remain silent during the entire experiment; if at any point in time you require assistance, please raise your hand.

In the first part of this experiment you will be in the role of a firm, which is in a market with another firm. The firms produce some good and there are no costs of producing this good.

This market is made up of 300 identical consumers, each of whom wants to purchase one unit of the good at the lowest price. The consumers will pay as much as 100 Experimental Currency Units (ECU) for a unit of the good.

In each market there will be two firms, A and B. You can find your type written on the top right-hand corner of this instruction set. Each firm will be able to supply 300 consumers.

The market will operate as follows. In the beginning of each period, all firms will set their selling prices. Then the firm who set the lowest price will sell its capacity at the selected price. The other firm will not have any customers left to supply.

If more than one firm set the lowest price, then they will split the available consumers. In order to fix ideas, let us go over a couple of illustrative examples:

Example 1: Suppose that the two firms choose the following prices: Firm A sets a price of 85 and firm B chooses a price of 75. Firm B set the lowest price and therefore sells its 300 units first at a price of 75, making a profit of 22,500 ECU. Firm A therefore will not supply any customers, therefore making 0 ECU.

Example 2: Suppose that the two firms choose the following prices: Firm A and firm B both set a price of 70. Given that firms A and B set the same price, they will have to share the available customers equally. Hence, both firms will sell 150 units at a price of 70 each unit, therefore making a profit of 10,500 ECU.

At the end of each period, all the firms are informed of the chosen prices by all firms and their own profits.

There will be at least 20 periods in this part of the experiment; once the 20th period is over, the computer will throw a virtual dice that will determine whether the experiment continues. If a value of six is shown, the experiment is over. If any other value is shown, the experiment continues.

You will be matched with the same participants in every period.

At the end of the experiment, you will be told of the sum of profits made during the experiment, which will be your payment. You will receive £1 for every 50,000 ECU you earn during the experiment. You will also receive £5 for participating.

Instructions - Part 2 (only shown to subjects after Part 1 was complete)

In this part of the experiment, you will be required to make the same decisions as in Part 1. The difference to Part 1 is that now you will be able to communicate with the other person you are matched with. To this effect, we will provide you with a chat box, which you can use to send messages to the other person. Only the person with whom you are matched will be able to see the messages you post.

In each period, you will be allowed to send messages to the other firm in your market for 1 minute. You are allowed to post how many messages you like. There are only two restrictions on messages: you may not post messages which identify yourself (e.g. age, gender, location etc.) and you may not use offensive language.

After the minute expires, the chat box will close and you will have to choose your price. Like in Part 1, at the end of each period, all the firms are informed of the chosen prices by all firms and their own profits.

There will be at least 20 periods in this part of the experiment; once the 20th period is over, the computer will throw a "virtual" dice that will determine whether the experiment continues. If a value of six is shown, the experiment is over. If any other value is shown, the experiment continues.

You will be matched with the same participants you were matched with in Part 1, and they will remain matched with you for the whole of Part 2.

At the end of the experiment, you will be told of the sum of profits made during the experiment, which will be your payment. You will receive £1 for every 50,000 ECU you earn during the experiment.

Appendix B: Ergodic distributions

$n = 2$	observed	ergodic	$n = 4$	observed	ergodic
$\underline{p} = 100$	0.609	0.610	$\underline{p} = 100$	0.685	0.689
$\underline{p} = 99,$	0.167	0.175	$\underline{p} = 99$	0.109	0.121
$\underline{p} < 99$	0.225	0.215	$\underline{p} < 99$	0.207	0.192

$n = 6$	observed	steady-state	$n = 8$	observed	steady-state
$\underline{p} = 100$	0.239	0.239	$\underline{p} = 100$	0.225	0.265
$\underline{p} = 99$	0.275	0.275	$\underline{p} = 99$	0.203	0.183
$\underline{p} < 100$	0.486	0.486	$\underline{p} < 100$	0.572	0.583

Table 4: Observed and ergodic distributions of the three states

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