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# The leniency rule revisited: experiments on cartel formation with open communication\*

Maximilian Andres<sup>†</sup>      Lisa Bruttel<sup>‡</sup>      Jana Friedrichsen<sup>§</sup>

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## Abstract

The experimental literature on antitrust enforcement provides robust evidence that communication plays an important role for the formation and stability of cartels. We extend these studies through a design that distinguishes between innocuous communication and communication about a cartel, sanctioning only the latter. To this aim, we introduce a participant in the role of the competition authority, who is properly incentivized to judge the communication content and price setting behavior of the firms. Using this novel design, we revisit the question whether a leniency rule successfully destabilizes cartels. In contrast to existing experimental studies, we find that a leniency rule does not affect cartelization. We discuss potential explanations for this contrasting result.

*JEL-codes:* C92, D43, L41

*Keywords:* cartel, judgment of communication, corporate leniency program, price competition, experiment

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# 1 Introduction

The formation and stability of cartels heavily relies on communication between cartel members. For example, [Harrington Jr \(2006\)](#) reports empirical data that cartel members typically meet on a monthly or at least quarterly basis, [Awaya and Krishna \(2016\)](#) show in a formal model that such frequent monitoring indeed leads to higher prices, and [Fonseca and Normann \(2012, 2014\)](#) find that communication facilitates cartel formation in experimental markets. Similarly, other experiments support the idea that communication facilitates coordination (see, e.g., [Cooper et al., 1992](#); [Gomez-Martinez et al., 2016](#)).

While the aforementioned studies emphasize that communication in cartels serves as a means of information exchange, coordination, and monitoring,<sup>1</sup> communication also contains a lot of content that is not directly related to cartel formation.<sup>2</sup> Such innocuous communication may not directly target cartelization, but may nevertheless facilitate it in more subtle ways. For example, it seems plausible that informal chat and regular meetings create an atmosphere of “knowing each other,” thereby reducing strategic uncertainty, which may facilitate and stabilize cartels.<sup>3</sup> If this is indeed the case, firms may use innocuous communication as an alternative to explicit price coordination and monitoring because a competition authority cannot rely on innocuous communication as sufficient evidence for a violation of competition law.

In this paper, we focus on the role of such innocuous communication on the effect of a leniency rule on cartelization. By implementing leniency rules, competition authorities can grant cartel members fine reductions if they deliver information that helps the authority to uncover the cartel. We argue that the possibility to engage in legally innocuous communication is likely to mediate the desired deterring effect of a leniency rule. This is because a firm that reports the cartel to the competition authority is only exempted from paying fines if it delivers sufficiently hard evidence for the cartel to be sanctioned. Such hard evidence typically comes in the form of some protocols of agreements about concerted

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<sup>1</sup>See also [Clark and Houde \(2014\)](#) and [Odenkirchen \(2018\)](#) as well as the therein cited literature.

<sup>2</sup>In general, estimates by [Dunbar \(1998\)](#) suggest that such side-content accounts for about 75% of all communication.

<sup>3</sup>In line with that argument, [Levenstein and Suslow \(2006\)](#) and [Davies and De \(2013\)](#) report empirical evidence that regular organizational structures facilitate cartel formation.

practices of the firms. If no such explicit agreements exist—because communication was sufficiently innocuous—the leniency rule might lose its bite against cartels.

Authorities claim that leniency rules successfully impede cartelization in the long run (OECD, 2012). However, perverse effects of the leniency rule are possible because it has an option value that reduces the expected future fine (see Motta and Polo, 2003) and because reporting may be used to threaten potential deviating firms. Further, Emons (2020) argues theoretically that a leniency rule may not be effective if firms choose their degree of collusion. As empirical studies evaluating the total effect of such leniency rules on cartelization from observational data<sup>4</sup> have an issue with the fact that undetected cartels are unobserved, research on the effectiveness of leniency policies prominently features experimental studies, where both detected and undetected cartels are observed by the researcher. These studies report that fewer cartels are formed, more cartels are reported, and average market prices are lower with a leniency rule than without it.<sup>5</sup>

Previous experimental studies on cartel formation could not study the subtle effect of innocuous communication on the effectiveness of a leniency rule because most of them use interactions with structured price communication.<sup>6</sup> Other studies allow for free-form communication,<sup>7</sup> but have the firms vote on whether they want to communicate with each other. These studies treated a unanimous binary decision to communicate immediately as a cartel—thereby replacing cartel formation in the chat by the vote before the chat. This makes it unattractive for the firms to communicate innocuously because if

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<sup>4</sup>Brenner (2009) does not find evidence that the EU leniency program was effective whereas the estimations by Ormosi (2014) suggest that the introduction of a leniency rule in the EU at least had a small impact on cartel detection rates. Miller (2009) finds that the US leniency program is effective in deterring cartel formation.

<sup>5</sup>These results are found in three player one-shot Bertrand price competition (Apesteguia et al., 2007) and in repeated price competition with three players and homogeneous-goods (Hinloopen and Soetevent, 2008) as well as two players with differentiated goods (Bigoni et al., 2012, 2015). Relatedly, Hinloopen and Onderstal (2014) compare the effectiveness of a leniency rule in first-price auctions and English auctions. Further experimental studies focus on the exclusion of cartel ringleaders from amnesty (e.g. Bigoni et al., 2012; Hesch, 2012; Wandschneider, 2014; Clemens and Rau, 2019) or on the effect of private damage claims (Bodnar et al., 2019). See Marvão and Spagnolo (2014, 2018) for more detailed overviews of these and further studies on cartels and leniency.

<sup>6</sup>Firms iteratively enter acceptable price ranges (Hinloopen and Soetevent, 2008) or directly enter their minimum acceptable price (Bigoni et al., 2012, 2015).

<sup>7</sup>Apesteguia et al. (2007) study a one-shot framework where firms can engage in free-form chat communication for ten minutes. Dijkstra et al. (2020) combine repeated homogeneous-goods Bertrand duopolies with free-form chat communication at the beginning of each round if firms agreed to communicate.

communication—as soon as it takes place—is subject to fines anyway, they will likely prefer explicit cartel formation over more subtle communication content.

Our paper introduces an innovative experimental design that includes the human judgment of communication and competitive conduct in order to give firms the option to communicate without automatically running the risk of being fined for forming a cartel. In our experiment, firms interact repeatedly and a chat window opens automatically at the beginning of each round. Each market includes a participant in the role of the competition authority as an active player. Whenever a control takes place, this experimental competition authority reads the written free-form chat communication of the experimental firms, observes price setting, and decides about the fines for each firm, such that fines are positively correlated to the severity of the firm’s violation of the law. Decisions of the competition authority are incentivized properly by paying the participant in this role in proportion to the overlap between their judgments and those of an expert cartel lawyer, who judges communication content and prices after the experiment. In this experimental design, communication is only sanctioned if it serves the purpose of coordinating prices. In other words, if firms chat about innocuous topics, they may reach a relatively high level of price coordination without risking a fine.

We find a leniency rule to be ineffective. The frequency of cartel formation and average prices do not differ significantly between the two treatments with and without a leniency rule in our setup. Further, the communication content is surprisingly similar in the two treatments. This is in contrast with previous experimental studies and needs explanation. In the discussion of our results (in Section 5), we identify design features that may be causal for the failure of a leniency rule to reduce cartelization in our setup. As we depart from previous designs in various ways, we cannot attribute this difference to one single design aspect. Indeed, only the combination of different aspects might fully explain the discrepancy. In particular, we argue that the voting stage and the inhibition of innocuous communication in previous studies jointly amplified the effect of a leniency rule on cartelization.

In the following, we describe our experimental design in Section 2 and develop hypotheses in Section 3. We then present the experimental results in Section 4 and provide a discussion of them in Section 5. We conclude in Section 6.

## 2 Experimental design and procedures

In the experiment, participants are matched into groups of four. Each group represents a market, in which three participants take the role of firms and a fourth participant takes the role of the competition authority. Each group interacts for at least 25 rounds as described below. Role assignments and matching groups remain fixed throughout the experiment.

**Structure of each round** In each round except the first, participants in the role of firms can communicate in free form chat for 60 seconds before price setting.<sup>8</sup> The chat window opens automatically without firms voting for communication beforehand. In each round, including the first, firms choose prices in a discrete three-player Bertrand price-setting game with differentiated products as detailed in Appendix A. Figure 1 illustrates how we presented payoffs to the participants. As can be seen in Figure 1, the Nash equilibrium price of the stage game is 3 and the symmetric joint-profit maximizing price of the stage game is 9. The firms are informed about each others' prices immediately after the price setting stage. Firms can self-report their market to the competition authority during price setting and then again when they receive the feedback about each other's prices.<sup>9</sup>

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<sup>8</sup>Communication starts only from the second round on because we use the price level in the first round as a benchmark for price setting in the absence of communication.

<sup>9</sup>As [Bigoni et al. \(2012\)](#) argue, making reporting possible at these two different instances allows for distinguishing between reports with the intention to punish another firm's deviation from a cartel agreement (during feedback) and self-reports in order to pre-empt such punishment (during price setting). Both reporting instances are located in the same price-setting round, so that deviations can be punished with a report instantaneously.

		Average price of the two other firms												
		0	1	2	3	4	5	6	7	8	9	10	11	12
Your own price	0	0	0	0	0	0	0	0	0	0	0	0	0	0
	1	29	<b>38</b>	47	56	64	73	82	91	100	109	118	127	136
	2	36	53	<b>71</b>	89	107	124	142	160	178	196	213	231	249
	3	20	47	73	<b>100</b>	127	153	180	207	233	260	287	313	340
	4	0	18	53	89	<b>124</b>	160	196	231	267	302	338	373	409
	5	0	0	11	56	100	<b>144</b>	189	233	278	322	367	411	456
	6	0	0	0	0	53	107	<b>160</b>	213	267	320	373	427	480
	7	0	0	0	0	0	47	109	<b>171</b>	233	296	358	420	482
	8	0	0	0	0	0	0	36	107	<b>178</b>	249	320	391	462
	9	0	0	0	0	0	0	0	20	100	<b>180</b>	260	340	420
	10	0	0	0	0	0	0	0	0	0	89	<b>178</b>	267	356
	11	0	0	0	0	0	0	0	0	0	0	73	<b>171</b>	269
	12	0	0	0	0	0	0	0	0	0	0	0	53	<b>160</b>

Figure 1: Payoffs presented to the participants.

After the price-setting has ended, a control may take place. It can either be caused by report of a firm as mentioned above or randomly with 10% probability in each round.<sup>10</sup> If a control takes place, the participant in the role of the authority does not know whether a

<sup>10</sup>According to [Ormosi \(2014\)](#), 10% constitutes a lower bound of the annual cartel detection rate in the European Union between 1985 and 2009. We intentionally decided for a fixed control probability as opposed to one that is increasing with prices because the fixed control probability allows for a cleaner comparison of price setting behavior across treatments. If we had used a control probability that is, e.g., increasing with prices, we could not clearly attribute treatment differences in prices to the main treatment variable. To see why this would be problematic, suppose the main treatment variable had an effect on prices. Then, as a side effect, it also had an effect on the detection risk. Now we would vary two things at a time: the main treatment variable (intentionally) and the detection risk (accidentally). In order to avoid such confounds, we decided for a fixed control probability.

self-report or chance caused it. He or she gains access to the history of chats and prices in his or her group and then makes a judgment about the duration of a potential cartel. The authority also decides about the extent of fines (0%, 50%, or 100%) for each of the three firms that will be applied to the firms' profits from the cartel. The profit to which the fine is applied is calculated by the experimental program based on the authority's input regarding the duration of the cartel and the firms' profits during the rounds that have passed since the last control. Participants in the role of firms receive a message that a control takes place and are asked to wait until the authority has recorded their judgment. Reporting is no longer possible after an investigation has started.

At the end of each round, participants receive feedback about their own and the other two firms' prices, their own profit, and—if applicable—the results of a control, i.e. the reporting behavior and penalties for all firms in their market.

**Repetition and termination rule** Participants interact for a minimum of 25 rounds. Starting from round 25, the game ends with the respective round with a probability of  $1/3$ ; with the complementary probability of  $2/3$  the game continues for another round. Thus, participants interact for 27 rounds in expectation. The random termination rule serves the purpose of blurring the time horizon to minimize endgame effects. We informed participants in the instructions that we would stop the experiment after 2.5 hours if the random continuation mechanism had not stopped it before. This event did not occur.

**Payment** During the experiment, participants receive payoffs in points. The final payoff is converted to euros at an exchange rate of 1 euro = 125 points for payment at the end of the experiment.

Participants in the role of firms are paid their cumulative earnings from the entire interaction. Perfectly competitive behavior, i.e. playing the Nash equilibrium of the stage game in all rounds, would yield an expected 2700 points; symmetric joint profit maximization, i.e. choosing a price of 9 in each round, would yield 4860 points in expectation. Participants in the role of a firm receive their payment from the experiment as well as a show-up fee of 5 euros immediately after the experiment in cash.



Participants in the role of the competition authority are paid based on the overlap of their judgment with the judgment of an expert in competition law with whom we contracted to independently evaluate the chat messages and the price setting behavior of the firms.<sup>11</sup> After each session, the expert received the full chat protocols as well as the history of prices of all firms in all rounds. A few days later, he provided us with a round-wise classification of whether a firm participated (0%, 50%, or 100%) in a cartel.

Whenever a control happens during the experiment, the competition authority takes four decisions (the extent of the fine for firms 1, 2, and 3 (0%, 50%, 100%), and the duration of the cartel in rounds). For each correct decision, i.e., agreement with the expert, this participant receives 900 points.<sup>12</sup> Thus, in each control, they can earn up to 3600 points. Their final payment is the average over the points they achieve per control across all controls that they took part in. In case no control ever takes place in his or her group, the respective authority receives a payoff of 1875 points (15 euros). Participants in the role of the competition authority receive their payment 2-3 weeks after the experiment by bank transfer. Additionally, they receive a show-up fee of 10 euros immediately after the experiment in cash.

**Treatments** We ran two treatments in a between-subjects design that varied the presence of a leniency rule at the session level: in the FINE treatment, no amnesty is granted to any firm. In the LENIENCY treatment, the first firm reporting receives full amnesty from fines.<sup>13</sup>

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<sup>11</sup>The expert holds a law degree (German: “Volljurist”), is writing a dissertation in the field of competition law, and also has practical experience in this area.

<sup>12</sup>In order to compare the duration stated by the experimental competition authority with the expert’s round-wise judgment, we computed the sum of rounds since the last control in which the expert reported a cartel. Similarly, we computed the average of the expert’s judgment of cartel activity by each firm over that interval and counted the decision of the experimental competition authority as correct if the expert’s average judgment comes closer to this judgment than to the other two (0%, 50%, or 100%) categories.

<sup>13</sup>The computer program recorded the exact timing of the report so that we could always identify the first firm reporting, even if two or all three firms reported in the same stage. The second and third firm reporting did not get a fine reduction. [Spagnolo \(2008\)](#) argues that it is important to grant amnesty only to the first reporting firm and not to later applicants. Further, [Harrington Jr \(2008\)](#) argues that, in most cases, leniency should be granted only to the first reporting firm.

**Procedures** In the pre-registration, we included a power analysis and specified a target sample size of 24 groups per treatment to be collected in 10 to 12 sessions with 12 to 20 subjects each. Using G\*Power, we computed that this sample size would be sufficient to detect effect sizes of  $d = 0.8$  or larger with a power of at least 0.8 in one-tailed Wilcoxon-Mann-Whitney tests with  $\alpha = 0.05$ , and effect sizes of  $d = 0.5$  still with a power of 0.5.<sup>14</sup> We collected our actual data in 12 experimental sessions à 12 to 20 subjects that took place in the experimental laboratories at the University of Potsdam and at TU Berlin in February and March 2019. We balanced the sessions per treatment across the two involved laboratories.<sup>15</sup> Our final sample consists of 23 groups in the FINE treatment and 27 groups in the LENIENCY treatment. On average, the experiment lasted for about 2 hours and participants earned 31.97 euros including the show-up fee. The experiment was computerized with z-Tree (Fischbacher, 2007) and the recruitment process was conducted using ORSEE (Greiner, 2015)

Participants were given detailed instructions<sup>16</sup> at the beginning of the experiment, which describe the structure of the interaction, the impact of own and others' prices on profits on the basis of a profit table, and the determination of penalties. Participants are not given the exact mathematical formula according to which profits depend on the three firms' prices. In addition, participants in the role of an authority were given an information sheet that explains in detail when firm behavior is to be considered as illegal cartel formation and how the duration of the cartel is to be determined. For example, this information sheet made clear that all prices above 3 can constitute a cartel if agreed

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<sup>14</sup>In pretests using a similar setup with a differentiated products Bertrand market and three firms, we observed cartel activity (counting rounds with 50% and with 100% fines) in about 26% of all rounds in the LENIENCY treatment and 87% of all rounds in the FINE treatment, with a standard deviation of 0.32 and 0.12. Further, the mean price in the pretest was 6.56 in the LENIENCY treatment and 8.74 in the FINE treatment, with a standard deviation of 1.2 and 0.2. In Bigoni et al. (2012), which had a similar market environment, the average price was 4.85 ( $SD = 1.67$ ) in LENIENCY and 5.35 ( $SD = 2.03$ ) in FINE. Their outcome variable "incidence of cartels," which is comparable to our measurement of cartel activity, implies a large treatment effect of about 30 percentage points (26.4% in LENIENCY vs. 58.3% in FINE). With respect to prices, which seem to react less than cartel activity, our own pretest data suggests an effect size of  $d = 2.53$  whereas the results in Bigoni et al. (2012) have an effect size of  $d = 0.27$ .

<sup>15</sup>Participants signing up for experiments in German are required to be proficient in German, but may ignore this and still sign up. Our pre-registration specified that a group of participants would be excluded from the analysis if the experimenter detected during the experiment or during payment that at least one participant in the respective group was not proficient in German. This led us to exclude two groups from the FINE treatment.

<sup>16</sup>An English translation of the instructions is provided in Appendix C.

upon in the chat, that convoluted descriptions of prices are to be treated as the prices they refer to, or how to deal with cartels that become more or less severe over time (for details see Appendix C.3).<sup>17</sup>

Furthermore, participants in both roles were trained with computerized tools that made them familiar with their role in the experiment before the payoff-relevant interaction started. Participants in the role of firms had access to a tool in which they could enter any combination of their own price and two prices for their competitors and the tool gave them feedback on the resulting profits. Participants in the role of an authority interacted with a training tool that confronted them with three archetypical constellations that they had to audit. After they had given their judgment, participants received feedback about the expert’s decision and an explanation for the correct answers. The experiment only started once everyone had finished their use of the respective training tool.

### 3 Research questions and hypotheses

In this section, we introduce the theoretical background and our research questions before developing a corresponding set of testable hypotheses for our experimental design. The first two hypotheses relate to the main measures for the success of a leniency rule, which are its impact on cartelization and prices. Thereafter, we turn to the use of the self-report option and to the way firms communicate with each other, respectively. Both are important to understand how a leniency rule affects firm behavior. In Appendix A, we develop a theoretical framework that captures the main features of our experimental design and use this framework to derive hypotheses on treatment differences in cartel formation. We verify the theoretical predictions by numerically computing expected payoffs from our

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<sup>17</sup>We intentionally did not provide this information to participants in the role of firms because we wanted to mimic real conditions in which most firms (except very large ones having their own legal department) are not aware of the precise legal situation. For a similar reason, we also did not elicit beliefs of the firms about the judgments. From the experimental literature we know that the mere question about beliefs can influence behavior (see [Croson, 2000](#); [Erev et al., 1993](#)). In our case, it seems likely that asking firms about whether they think their communication would be subject to sanctions if detected would have affected the way the communicated and also made them more reluctant to form a cartel.

specific experimental design under equilibrium strategies. The design, the hypotheses, and the way we would test these were preregistered on OSF.

First, we want to better understand how a leniency rule affects the formation and stability of cartels because these are important criteria for the success of the leniency rule. In our model, we determine the discounted present value from colluding and deviating to compute the critical discount factor for collusion to be stable. The treatment difference arises because deviations in LENIENCY are optimally combined with self-reports (as fines are waived for the reporting firm) whereas they are not in FINE (as the reporting firm is also fined). In this setup, deviating is less profitable in FINE than in LENIENCY: in FINE, a cartel simply ends with a deviation, but it is not immediately fined because no firm has an incentive to self-report. If a random control takes place in later rounds after the deviation, the cartel members—including the deviator—may still be fined for previously being a member in the cartel. In LENIENCY, the cartel also ends, but the self-report of the deviator causes immediate fines for the other firms and, therefore, there is no risk of future fines for the deviator any more.

As a result of the above argumentation, the incentive compatibility constraint for the perfectly collusive outcome is tighter in LENIENCY than in FINE. The critical discount factor is higher in LENIENCY than in FINE. This means that collusion can be sustained as an equilibrium for a larger range of discount factors, and supposedly is more stable, in FINE than in LENIENCY. Additionally, the leniency rule may also behaviorally undermine trust among (former) cartel members, which would strengthen this treatment difference. Thus, the incentive to step away from a cartel is always higher in LENIENCY than in FINE. Further, the leniency rule may also hinder the (re-)establishment of a cartel in LENIENCY compared to FINE because using the self-report option to achieve amnesty from fines may undermine trust in a market.

Therefore, we expect that there is less cartelization in LENIENCY than in FINE (Hypothesis 1a). We also expect that cartels that were formed are more likely to endogenously break down (Hypothesis 1b), and are less likely to re-establish (Hypothesis 1c) in the presence of a leniency rule than without one.

**Hypothesis 1a.** *The extent of cartelization is higher in FINE than in LENIENCY.*

**Hypothesis 1b.** *Cartels are less stable in LENIENCY than in FINE.*

**Hypothesis 1c.** *After a break-down, cartels are re-established later in LENIENCY than in FINE.*

The reasoning behind Hypothesis 1c is that we expect at least some of the cartels in LENIENCY to break down due to controls triggered by a self-report, i.e. through use of the leniency rule, which will lower mutual trust, thereby impeding the re-establishment of a cartel (see also A.3). Assuming that the incentives remain unchanged for randomly detected cartels, this implies on average later re-establishment in the presence of the leniency rule than without it.

Second, we address the overarching question of how a leniency rule affects market prices, which is an important outcome for policy makers. We start by asking how a leniency rule affects average market prices, i.e., not distinguishing between cartel and competitive phases. As we hypothesize that there will be less cartelization in LENIENCY than in FINE (see above) and cartels are characterized by supra-competitive prices, we expect that the leniency rule decreases average market prices (Hypothesis 2a). Further, we expect that prices during cartel phases are also lower in LENIENCY than in FINE (Hypothesis 2b) because the incentive compatibility constraint is tighter: cartels may try to coordinate on a lower price than the joint profit-maximizing price because this typically relaxes incentive compatibility due to a lower unilateral deviation profit. Again, this will decrease prices in cartel phases.

**Hypothesis 2a.** *Average prices are lower in LENIENCY than in FINE.*

**Hypothesis 2b.** *Average prices during cartel phases are lower in LENIENCY than in FINE.*

We have no directed hypothesis regarding the treatment effect on prices in rounds that are not characterized by cartel behavior according to the expert judgment. Prices may be higher or lower with a leniency rule than without it.

Next, we turn to the channels through which a leniency rule may affect firm behavior. Making cartels report themselves is the main lever that the leniency rule intends to activate. Thus, we ask how a leniency rule affects the frequency of reports of existing cartels. In the absence of a leniency rule, self-reports reduce own payoffs. If a leniency rule is in place, however, fines are waived for the first self-reporting firm. Thus, self-reports are cheaper in LENIENCY than in FINE. At a higher level of reasoning, firms might also anticipate that others are more likely to self-report and, in turn, become more likely to self-report preemptively. Thus, we expect self-reports only in the LENIENCY treatment. In the experiment, there may also be few self-reports in the FINE treatment, but given the high expected fine to be paid by the self-reporting firm we expect them to be very rare.

**Hypothesis 3.** *Self-reports are used more often in LENIENCY than in FINE.*

Finally, we want to understand how a leniency rule affects the way firms communicate with each other. Based on the reasoning behind hypothesis 3, we posit that participants understand the leniency rule as increasing the probability of controls. Due to the increased risk of being controlled and fined, we subsequently expect that participants are more careful in their chat messages in LENIENCY than in FINE because they try to avoid punishment for explicit price coordination. This means that we expect them, e.g., to use the word 'price' (and its synonyms) less often than participants in FINE, and to write the number '9' or the word 'nine' (the joint profit maximizing price) as a description of the desired cartel price less often.

**Hypothesis 4.** *Communication in LENIENCY is less explicit about prices than communication in FINE.*

## 4 Analysis and results

In the following, we use our experimental data to test the specified hypotheses. We restrict ourselves to the data from the first 25 rounds of play as specified in the preregistration.

We use this restriction because these rounds are played in all sessions, irrespective of the realizations of the random continuation mechanism, thus allowing for the cleanest treatment comparison. We first investigate cartelization and prices because these are the outcomes most relevant for competition authorities. We then investigate reporting behavior and communication as the channels that a leniency rule might affect.

**Cartelization** In order to examine how a leniency rule affects the formation of cartels, we compare cartelization across treatments. Our measure for the extent of cartelization is the ratio of rounds in which a cartel existed according to the judgment of the expert.<sup>18</sup> As our primary measure, we compute the weighted ratio where the extent of the fine (50% vs. 100%), as decided by the expert, is used to weight the respective cartel rounds to account for the fact that cartel behavior may be more or less severe. The average weighted cartelization rate is 0.34 in LENIENCY ( $N = 27$ ,  $SD = 0.33$ ) and 0.32 in FINE ( $N = 23$ ,  $SD = 0.25$ ). This measure of cartelization does not differ significantly between the two treatments at the 5%-level ( $p = 0.57$  in a one-tailed Wilcoxon-Mann-Whitney test).<sup>19</sup> The same holds if we use the unweighted ratio of rounds in which a cartel existed as cartelization measure (mean = 0.39,  $N = 27$  and  $SD = 0.34$  in LENIENCY, mean = 0.43,  $N = 23$  and  $SD = 0.29$  in FINE,  $p = 0.38$  in a one-tailed Wilcoxon-Mann-Whitney test). Hence, our data does not support Hypothesis 1a that the extent of cartelization is larger in FINE than in LENIENCY.

We further analyze the distribution of cartelization in the two treatments. Figure 2 shows that the histograms of our cartelization measures are relatively similar in both treatments. While the unweighted measure that does not take into account the severity of

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<sup>18</sup>If we consider the binary judgment of whether or not a firm participated in a cartel, participants in the role of the competition authority come to the same judgment as the expert in 76.49% of the cases. If we consider the weighted judgment, which takes into account the severity of an infringement and the duration of a cartel, the overlap between participant and expert judgment still amounts to 61.05%. Most important for us, the *difference* between the judgment of the participant and the one of the expert is not systematically different in the two treatments, neither with the former ( $p = 0.75$ ) nor with the latter measure ( $p = 0.31$ ) in a two-tailed Wilcoxon-Mann-Whitney test. Note that it is not crucial for the validity of our design that the experimental authorities' judgments coincide with those of the expert in all cases. As the judgment of legal experts is typically not perfectly predictable (otherwise, they would have been replaced by algorithms already), some uncertainty about the most likely judgment actually increases the external validity of our design.

<sup>19</sup>For all statistical tests, we treat each market as an independent observation.

cartel infringements appears to be very similar across treatments, the weighted measure suggests subtle differences. However, the distributions do not differ significantly in a one-tailed Kolmogorov-Smirnov test for either of the two measures ( $p = 0.45$  for the weighted and  $p = 0.58$  for the unweighted cartelization measure). Figure 2 also seems to suggest that there are more markets without any cartelization in LENIENCY than in FINE. However, this is driven by the binning of cartelization rates from 0% to 10%. The fraction of markets without any cartelization is virtually identical across treatments and the hypothesis that there are more cartel-free markets with a leniency rule is clearly rejected (5 out of 27 markets or 18.52% in LENIENCY, 5 out of 23 markets or 21.74% in FINE,  $p = 0.74$  in a one-tailed Fisher exact test).

We continue with the investigation of cartel duration. Our measure for the stability of cartels is the number of rounds that a cartel existed before break-down according to the unweighted expert judgment. The average duration of the first cartel formed in a group (if a cartel is formed at all) is 7.09 rounds in LENIENCY ( $N = 22$ ,  $SD = 8.34$  rounds) and 8.33 rounds in FINE ( $N = 18$ ,  $SD = 6.31$  rounds). The decrease in duration in the presence of a leniency rule is not statistically significant at the 5%-level based on a one-tailed Wilcoxon-Mann-Whitney test ( $p = 0.12$ ). Therefore, we do not find support for Hypothesis 1b.

Next, we look at cartel recidivism and compute how long it takes for a cartel to re-establish after it has broken down. We count the number of rounds that pass between the break-down of the first cartel and the establishment of the second cartel, again according to the unweighted expert judgment. In groups where no second cartel has been formed through round 25, we assume that the cartel is reestablished in round 28, i.e., after the expected number of rounds is over.<sup>20</sup>

Using this imputed measure of recidivism, we find that cartels are re-established after 6.06 rounds in LENIENCY ( $N = 18$ ,  $SD = 6.86$  rounds) and 6.23 rounds in FINE ( $N = 13$ ,  $SD = 6.29$  rounds); if we consider only cartels that are actually re-established until round

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<sup>20</sup>We use this assumption both for groups in which the cartel was not reestablished at all during the actual number of rounds played and for groups in which a cartel was reestablished after round 25. We believe our assumption is a clean way to account for the fact that we commit to only using data from rounds 2 to 25 for our analysis.



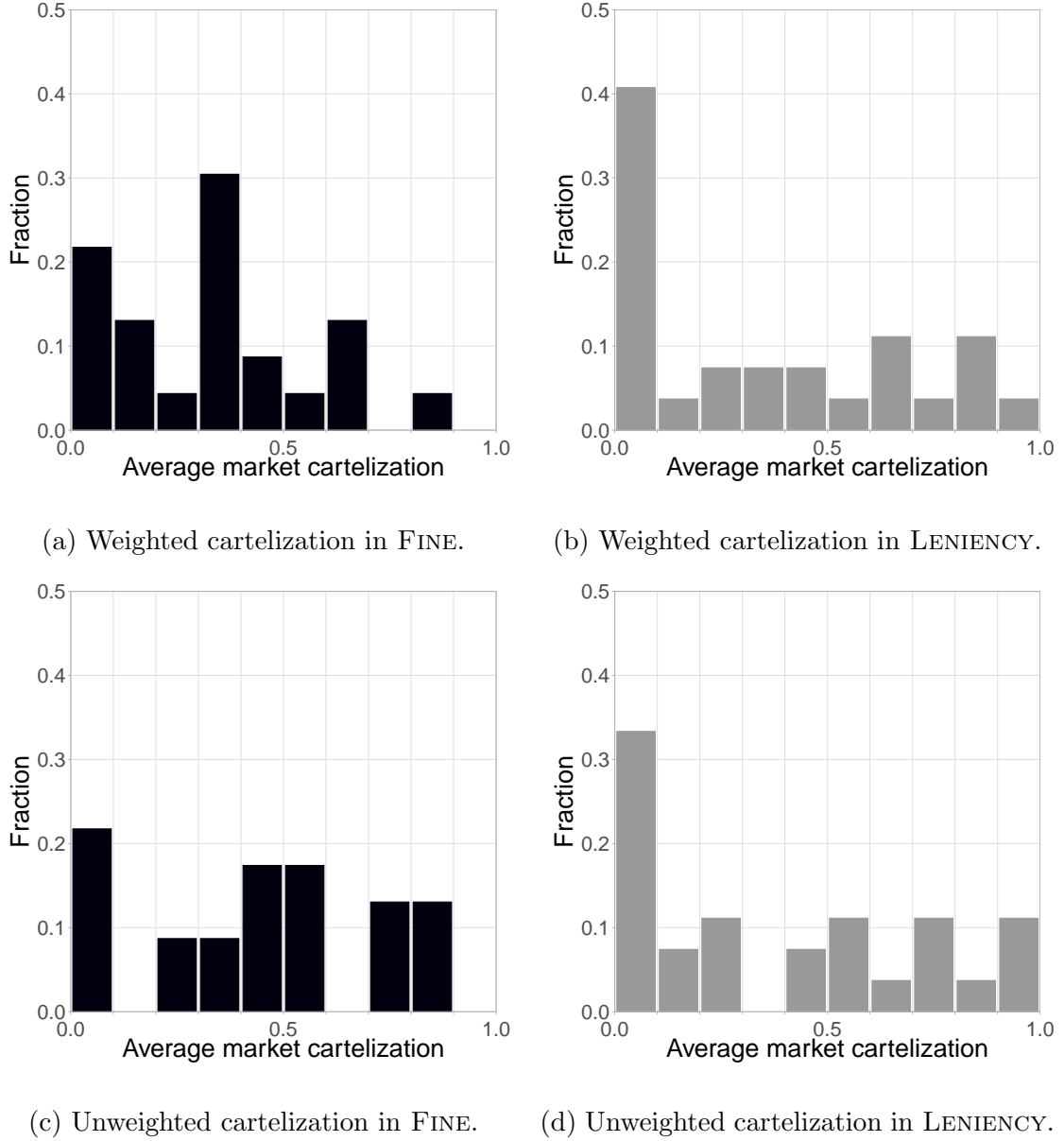


Figure 2: Histogram of cartelization by treatment.

25, we find that they re-establish after 3.39 rounds in LENIENCY ( $N = 13$ ,  $SD = 3.75$  rounds) and after 3.33 rounds in FINE ( $N = 9$ ,  $SD = 2.87$  rounds), on average. With neither measure do we find that re-establishment occurs significantly later in LENIENCY than in FINE ( $p = 0.39$  (imputed measure) and  $p = 0.5$  (actual values) in a one-tailed Wilcoxon-Mann-Whitney test). Overall, we find that 13 out of 18 cartels (72.22%) are re-established after a break down in LENIENCY and that 9 out of 13 (69.23%) cartels are re-established in FINE. In line with the above findings, we cannot, based on the overall frequency of recidivism, reject the hypothesis that cartels are re-established equally often

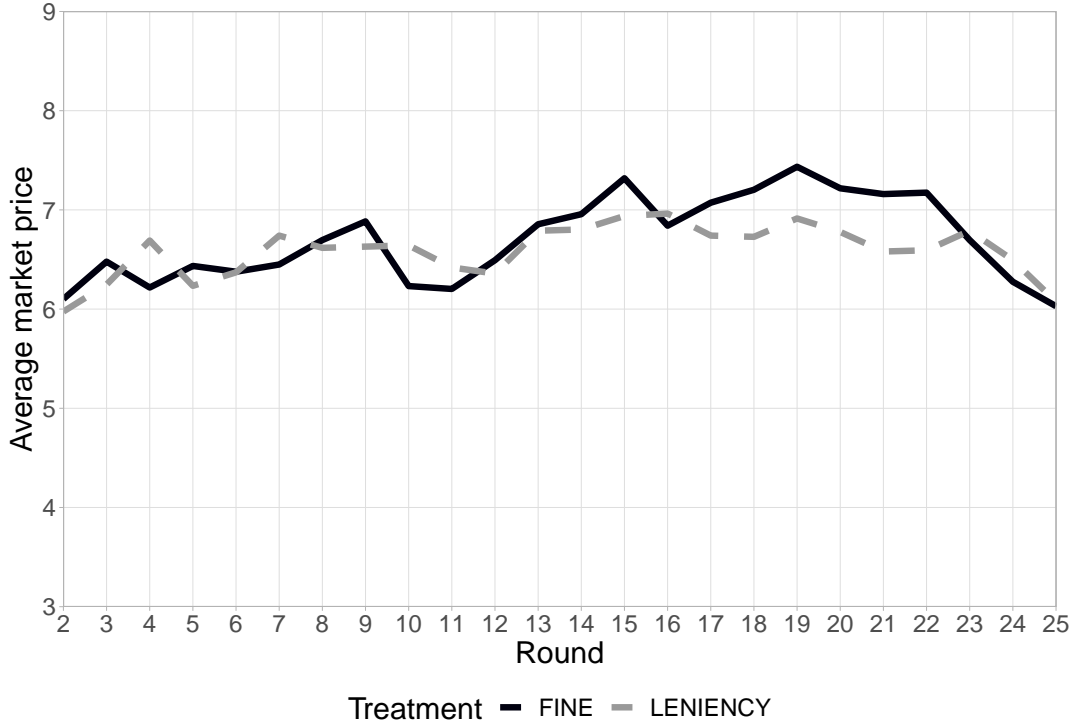


Figure 3: Average market prices over time by treatment.

in the presence of a leniency rule than in its absence ( $p = 0.64$ , one-tailed Fisher exact test). Thus, our data does not support Hypothesis 1c.

**Prices** We first average prices per market over time (rounds 2 to 25) and then compare whether these average market prices differ across treatments according to a one-tailed Wilcoxon-Mann-Whitney test. We do not find statistically significant evidence ( $p = 0.49$ ) that the average market price in LENIENCY (mean = 6.59 points,  $N = 27$ ,  $SD = 1.45$  points) is lower than that in FINE (mean = 6.7 points,  $N = 23$ ,  $SD = 1.03$  points). Thus, our data does not yield support for Hypothesis 2a. As shown in Figure 3, prices in LENIENCY and FINE develop very similarly over time, indicating that the averages over time do not hide relevant differences.<sup>21</sup>

We move on to compare average market prices between phases that are characterized by competitive or cartel behavior according to the expert judgment, i.e., we average market prices over competitive and cartel rounds at the group level, compute averages by treatment and competitive conduct, then compare the averages across treatments

<sup>21</sup>This impression is confirmed if we look at the average market price over time at the individual market level, see Appendix B for details.

separately for competitive and cartel conduct. Figure 4 illustrates that average prices during cartel phases are 7.82 points in LENIENCY ( $N = 22$ ,  $SD = 1.16$  points) and 7.86 points in FINE ( $N = 18$ ,  $SD = 0.95$  points); indeed, pricing during cartel phases does not differ significantly ( $p = 0.56$  in a one-tailed Wilcoxon-Mann-Whitney test). Therefore, the data does not support Hypothesis 2b that average prices during cartel phases are higher in FINE than in LENIENCY.

Figure 4 also illustrates that prices during phases that are not characterized by cartel behavior are also very similar across treatments. Indeed, the average price of 5.74 points during competitive phases in LENIENCY ( $N = 27$ ,  $SD = 1.11$ ) is very close to the average price of 5.68 during competitive phases in FINE ( $N = 23$ ,  $SD = 1.22$ ) and we find no evidence for a statistically significant difference ( $p = 0.6$  in a two-tailed Wilcoxon-Mann-Whitney test).

However, there is a significant difference between average prices during cartel phases and phases with competitive behavior in LENIENCY, FINE, and in both treatments pooled (mean = 7.84 points,  $N = 40$  and  $SD = 1.06$  points during cartel phases, mean = 5.71 points,  $N = 50$  and  $SD = 1.15$  points with competitive behavior). The respective differences are all significant ( $p < 0.001$ ) at the 1%-level in a two-tailed Wilcoxon-Mann-Whitney test using the full sample and in a two-tailed Wilcoxon matched pairs test where we restrict the sample to 22 (LENIENCY) and 18 (FINE) groups in which we observe both competitive and cartel conduct. This result illustrates that the expert identified cartels based on evidence of unlawful communication in combination with anticompetitive pricing behavior. This reliance on combined evidence from chat and prices does not only reflect the current legal approach to cartel prosecution but is also economically meaningful: we care about cartel agreements not because of the agreement *per se* but because of its effect on prices and the resulting negative impact on consumers' welfare.

**Self-reporting** On average, self-reporting (during rounds 2 to 25) occurred in 0.93 rounds in LENIENCY ( $N = 27$ ,  $SD = 1.33$ ) and in 0.52 rounds in FINE ( $N = 23$ ,  $SD = 0.73$ ) and, therefore, very rarely. We do not find that self-reporting occurs significantly

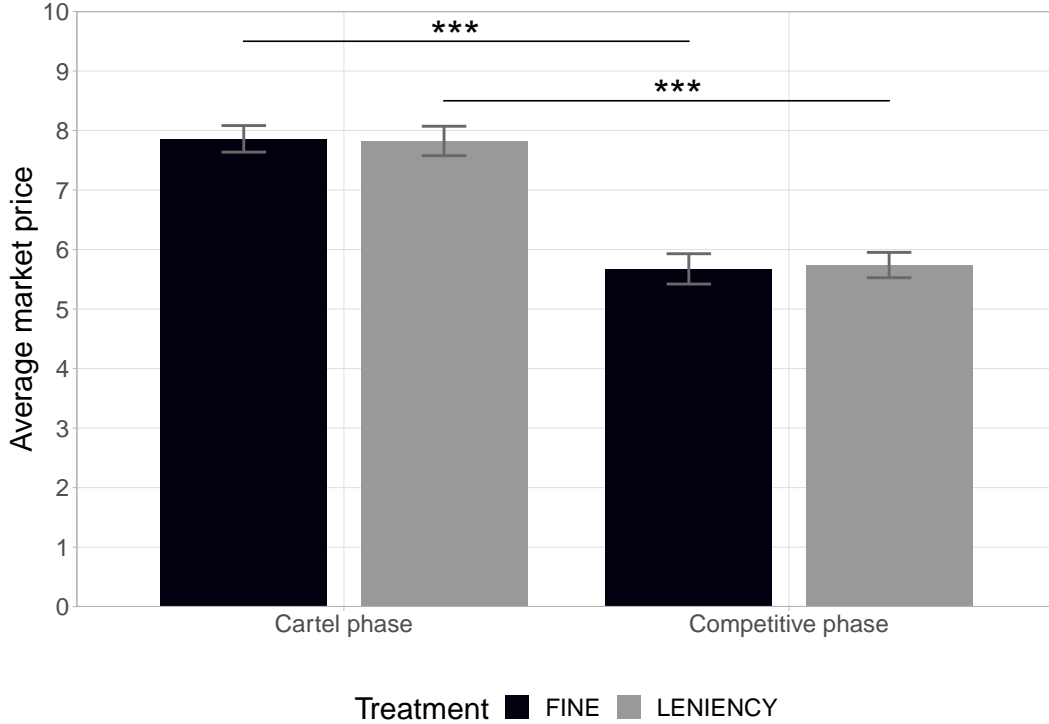


Figure 4: Average market prices by competitive conduct and treatment. '\*\*\*' shows statistical significance at the 1% level. Error bars indicate standard errors.

more often in LENIENCY than in FINE ( $p = 0.28$  in a one-tailed Wilcoxon-Mann-Whitney test). The same holds if we consider that two or all three firms can self-report in the same stage game. We do not find a statistically significant difference between the average ratio of rounds with controls triggered by self-reports between LENIENCY (mean = 0.89 rounds,  $N = 27$  and  $SD = 1.25$ ) and FINE (mean = 0.48 rounds,  $N = 23$  and  $SD = 0.59$ ) in a one-tailed Wilcoxon-Mann-Whitney test ( $p = 0.26$ ). This suggests that self-reporting across the two treatments is very similar and that our data yields no support for Hypothesis 3.

We further investigate self-reports conditional on a cartel being active because self-reporting is meaningful only if a cartel exists that can be dismantled through this report. Overall, 22.73% or 10 out of 44 cartels are reported in LENIENCY and 12.5% or 4 out of 32 cartels are reported in FINE. According to a Fisher exact test, the frequency of a cartel being reported is not significantly higher with a leniency rule ( $p = 0.2$ , one-tailed). This finding is confirmed if we investigate the average number of cartels reported per market. The average rate of cartels reported per market with cartelization does not differ

significantly in a one-tailed Wilcoxon-Mann-Whitney test (mean = 0.25,  $N = 22$  and  $SD = 0.36$  in LENIENCY, and mean = 0.17,  $N = 18$  and  $SD = 0.34$  in FINE,  $p = 0.16$ ).

Next, we restrict our attention to only the first cartel in each group to avoid dynamic effects from past reporting. The frequency of a first cartel being reported is higher in LENIENCY with 31.82% or 7 out of 22 first cartels than in FINE with 16.67% or 3 out of 18 first cartels. Again, we find no significant evidence that cartels are reported more often in the presence of a leniency rule in a Fisher exact test ( $p = 0.23$ , one-tailed). Thus, even a more exploratory analysis of the data does not provide evidence supporting Hypothesis 3. However, the relatively low incidence of reporting does not allow us to explore this dismantling through a report in more detail. In particular, we acknowledge that self-reporting might have long-run effects on recidivism and stability of cartels that our design is unable to capture.<sup>22</sup>

**Communication** To investigate communication, we follow two separate approaches. First, we classify communication manually into explicit attempts to coordinate<sup>23</sup> and then (a) count rounds in which communication has or has not occurred and (b) count messages with explicit communication. We then compare the amount of (explicit) communication across treatments by first averaging group-level communication over time. On average, we find slightly more rounds with communication in LENIENCY than in FINE, with groups communicating on average in 83.03% of the rounds in LENIENCY ( $N = 27$ ,  $SD = 0.23$ ) and in 70.65% of rounds in FINE ( $N = 23$ ,  $SD = 0.31$ ). The number of rounds with communication is not statistically significantly different between the two treatments according to a two-tailed<sup>24</sup> Wilcoxon-Mann-Whitney test ( $p = 0.21$ ). Next, we investigate whether participants communicate less explicitly in the presence of a leniency

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<sup>22</sup>According to a sensitivity analysis using G\*Power (Faul et al., 2007), our design would have picked up an effect at a power of 0.5 and  $\alpha = 0.05$  (which is the level we targeted in the preregistration) if the share of reports in LENIENCY would have exceeded 45.6%, which would still have been a much smaller effect than that observed in Bigoni et al. (2012). The measured effect is also sensitive to small changes in numbers. For instance, the treatment difference in the probability of a first cartel being reported drops to 5 percentage points if we had observed only one report less in FINE and one more in LENIENCY.

<sup>23</sup>Two of the co-authors of this paper independently coded all messages, with an inter-coder agreement of 97.55%. We decided to do the classification ourselves because the coding task is quite challenging and requires substantial understanding of the setup.

<sup>24</sup>We use a two-tailed test because we do not have a hypothesis on the total amount of communication in both treatments.

rule. We find that 5.97% of all messages in FINE contain explicit price communication ( $N = 23$ ,  $SD = 0.08$ ), whereas in LENIENCY this holds for 6.71% of the messages ( $N = 27$ ,  $SD = 0.06$ ). These averages are not statistically different in a one-tailed Wilcoxon-Mann-Whitney test ( $p = 0.77$ ). Thus, we find no support for Hypothesis 4.

Second, we compare the content of the chats after having followed a systematic natural language processing procedure, which aims to represent the chat data by tokens that are most likely to confer the content of text (Manning and Schütze, 1999; Gentzkow et al., 2019; Penczynski, 2019). Tokens are strings that are extracted out of the text, e.g. words, numbers, or what remains from a word after the stemming procedure described below. We define each group chat as a document so that all group chats together constitute the corpus on which the procedure operates. We correct spelling mistakes in the chat, clean the text by removing stop words, and reduce all words to their stem such that, e.g., 'preference' and 'prefers' both become 'prefer'.<sup>25</sup>

Consistent with the null effects in terms of economic variables, we find that communication in both treatments is surprisingly similar. Figure 5 shows word clouds and plots the 15 most frequent tokens in the chat protocols by treatment after natural language processing.<sup>26</sup> It is apparent that participants in both treatments talk about very similar content because the word clouds and the list of most frequent words overlap almost entirely. Overall, 14 out of the 15 most frequent tokens are identical.<sup>27</sup> Interestingly, 'firm' (=firma) and the corresponding name '1', '2' and '3' are present in both figures. Closer inspection of figure 5 shows that only the 15th token varies: 'competition authority'

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<sup>25</sup>We use the *R* package *tm* published by Feinerer et al. (2008) to process the chat data. We use the list of *stop words*, which are tokens that appear frequently in all texts and are unlikely to confer meaningful content such as 'the' or 'at,' provided by Feinerer et al. (2008) and append tokens typical for chat messages in German, such as 'wat' meaning 'what' in Berlin and Brandenburg. We use the *R* package *SnowballC* published by Bouchet-Valat (2019) to *stem* words.

<sup>26</sup>Subjects write 'yes' (=ja), 'okay' and 'no' (=nein) very often, they talk about firms (=firma) and the name of firms (1,2,3), they mention rounds of play (=rund), the 'competition authority' (=kartellamt), a price of nine and discuss how to improve outcomes typing 'always' (=imm), 'get' (=geht), more (=mehr) as well as 'yet' or 'nice' (=scho(ö)n), 'time' (=mal) and 'good' (=gut). We refrain from translating the depicted tokens because the translation of tokens after stemming and out of context is difficult and may be misleading. For instance, German 'schon' can be used as 'already', 'yet', or (transcribed from 'schön') 'nice', 'good'. Picking one of these as the translation would miss part of the meaning.

<sup>27</sup>Also an in-depth text mining analysis (including a machine learning approach) did not deliver any indication that the communication content would differ between the FINE and the LENIENCY treatments. Details are discussed in a companion paper (see Andres et al., 2020).

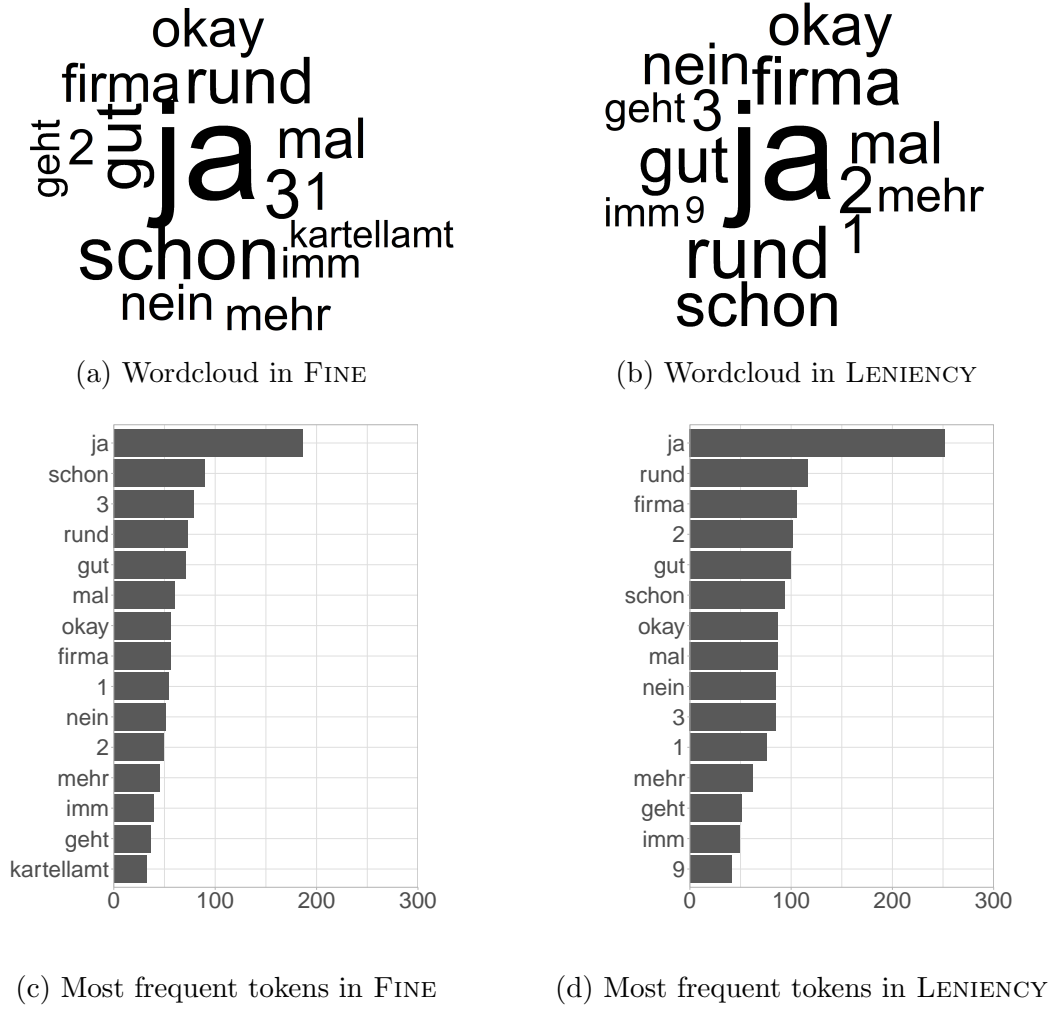


Figure 5: Descriptive analysis of communication data.

(=kartellamt) in figure 5c and '9' in figure 5d. This is intuitive, as we classified slightly more communication as being explicit in LENIENCY than in FINE, although the difference is not significant. Thus, this observation of a small difference rather supports our classification of text messages as meaningful. To conclude, also at this more disaggregated level of analysis, we find no evidence for the leniency rule affecting the way firms communicate with each other.

## 5 Discussion

Almost all previous experimental studies find that a leniency rule is an effective instrument to hinder cartel formation. Our study did not replicate this result. We attribute the difference to an important design innovation, the inclusion of a competition authority as an active player in the experiment. This innovation has two main consequences. First, this feature allows us to remove the artificial voting element, used in previous studies, from the course of actions by the firms; the task to judge what behavior is going to be punished as a cartel is taken over by the participant in the role of the authority. Second, it gives firms the option to communicate innocuously because the authority is incentivized to punish only communication that resulted in the formation of a cartel. In the following, we explain why we think that these aspects are crucial for the null effect we find.

We start with a discussion of the voting element. In previous experimental studies on the leniency rule, firms vote in a first stage whether they want to communicate with each other. If all firms in a market agree to communicate, this joint decision counts as a cartel being formed and participants know this when they decide about communication. Importantly, having unanimously voted for communication counts as a cartel regardless of whether the firms succeed in coordinating their prices or not.

Such a voting stage is likely to exaggerate the extent of cartel formation because forming a cartel by clicking a button on the computer screen is presumably far easier than doing so by coordinating on a specific joint pricing behavior via free-form communication. Furthermore, the unanimity rule may have reduced the perceived immorality of cartel formation due to a diffusion of responsibility. In line with this reasoning, studies using a voting design found a high degree of cartel formation. Among the cartels that form through voting, there are likely some that are inherently unstable because commitment to the cartel is low and that would not form in a less structured environment. With respect to these unstable cartels, a leniency rule is an ideal playing field to unfold its effect as firms that are less committed and expect low commitment to the cartel from



their fellow firms will find a self-report particularly attractive.<sup>28</sup> Put differently, removing the voting element in our setup made it harder for a leniency rule to deter cartels because (a) fewer cartels are formed and (b) the cartels that do form can be expected to be more stable.

Furthermore, the voting element has the unintended consequence that one single firm can veto communication and, thus, cartel formation in its market. Consequently, even a small increase in the number of firms opting against communication due to a leniency policy will result in an over-proportional decline in cartels being formed. Indeed, [Hinloopen and Soeteven \(2008\)](#) report that such “near unanimity” situations are especially frequent in their leniency treatment while the total share of firms wishing to form a cartel is not different across their two treatments with and without leniency policy.

Following our above argumentation, the voting element is a likely intensifier of a deterring effect of the leniency rule on cartel formation. However, the voting stage alone does not seem to be sufficient to produce such a deterring effect: in a study by [Dijkstra et al. \(2020\)](#) that included a voting stage, the leniency rule was largely ineffective.<sup>29</sup> Thus, in order to fully explain why we did not find an effect of the leniency rule, there has to be at least one other consequence of our design with the active competition authority that counteracts the leniency rule.

We think that free-form communication is the second important element of our design that counteracts deterrence by the leniency rule in our setting. Note first that open communication is shown to be more effective in terms of cooperation than structured communication (see [Harrington Jr et al., 2016](#); [Cooper and Kühn, 2014](#)). However, this only explains why we observe far higher prices, fewer deviations, and fewer self-reports

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<sup>28</sup>Recall that cartel incidence is 58.3% without and 26.4% with a leniency rule in [Bigoni et al. \(2012\)](#) while we find an incidence of about 32.19% without and 34.11% with a leniency rule. They find that about 50.7% of cartels are reported in the presence of a leniency rule, while we find that this is the case for only 22.73%. Empirically, the relatively low extent of self-reporting that we observe even in the presence of a leniency rule is consistent with the findings of [Ormosi \(2014\)](#), who estimates an upper bound of 20% of all existing cartels being detected.

<sup>29</sup>In general, the setup of [Dijkstra et al. \(2020\)](#) is still likely to yield biased results on the extent of cartelization and the content of communication. This is evident from their observation that (some) firms agree to tacitly collude after detection and the corresponding finding of price hysteresis after cartel detection. According to judicial practice, this would be a continued cartel and should be fined but is not in their experimental study.

than previous studies *in both treatments*.<sup>30</sup> In order to explain why there is no treatment difference, the cartel stabilizing effect of free-form communication has to “neutralize” the deterring effect of a leniency rule. In our view, innocuous communication plays an important role for this neutralization as it can reduce strategic uncertainty and enhance trust. A high level of trust then provides an environment in which a leniency rule turns out to be particularly ineffective because this rule intends to create distrust by making deviations relatively more attractive (see also [Dijkstra et al., 2020](#)). In other words, we think that innocuous communication might counteract the previously reported destabilizing effect of a leniency rule that intends to undermine trust among cartel members.<sup>31</sup> In the FINE treatment, the *marginal* effect of free-form communication on cartelization is smaller because the strategic risk is lower than in the LENIENCY treatment. Further, as argued in our theoretical exploration of the setting, the lack of trust is likely to mostly affect cartelization in the presence of a leniency rule. In this way, free-form communication would further reduce any potential treatment difference.

Finally, by giving firms the choice of how explicitly to communicate about their intent to form a cartel, our design comes closer to a setting where firms can adjust the degree of collusion to the trade-off between higher expected profits and higher expected fines instead of making a binary choice of whether or not to collude. [Emons \(2020\)](#) shows theoretically that a leniency rule may be ineffective in such a setting thus giving some theoretical backing for why a leniency rule might work differently in our context than in previous studies. Previous experimental designs did not allow for, and could not capture, any choice on the degree of collusion as modeled in [Emons \(2020\)](#) because they only

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<sup>30</sup>While average prices in our experiment are 6.7 in FINE and 6.59 in LENIENCY, [Bigoni et al. \(2012\)](#) report average price levels of 5.35 in FINE and 4.85 in LENIENCY, while using very similar payoff functions with prices also being restricted to the same range between 0 and 12. The difference is particularly striking when taking into account that they had markets with two firms, which should have facilitated coordination in comparison to our setup with three firms per market.

<sup>31</sup>The leniency policy increases the “strategic risk” ([Marvão and Spagnolo, 2018](#)) from forming a cartel. [Spagnolo \(2004\)](#) provides some theoretical considerations into this direction, but a rigorous formal treatment or experimental analysis of the trust channel with respect to leniency rules and cartel deterrence, to our best knowledge, does not exist. However, several papers provide theoretical and experimental evidence that higher “strategic risk”—understood as a lower sucker’s payoff—reduces cooperation in repeated prisoner’s dilemma games ([Blonski et al., 2011](#); [Breitmoser, 2015](#); [Dal Bó and Fréchette, 2018](#)) such that a deterring effect on cartel formation should be expected.

included collusion as a binary choice variable. Our design allows firms to communicate more or less explicitly, thus coming closer to that model than previous experiments.

## 6 Conclusion

In this study, we revisit the effect of a leniency rule on cartelization. The main innovative element of our experiment is that we introduce a competition authority as an active player into the setup, in addition to participants in the role of firms. Thus, in comparison to previous studies, we replace the binary and unanimous decision whether to form a cartel or not with a free-form communication possibility that precedes the price-setting stage. This free-form chat allows firms to communicate without explicitly forming a cartel. We obtain a reliable measure of cartelization in the form of the independent judgment of an expert in competition law.

We find that a leniency rule affects neither cartelization nor pricing behavior in this experimental design. This finding is in stark contrast to results from previous experimental studies. We argue that previous studies likely overestimate the size of the effect of a leniency rule on cartelization because their setup created an environment that makes it particularly easy for the leniency rule to reduce cartelization by a) setting the decision to communicate equivalent to the decision to form a cartel and b) allowing communication only in a highly structured form. Based on our data and a careful consideration of the previous findings in light of the respective designs, we conclude that a leniency policy may not be as effective in destabilizing cartels as suggested by previous experimental evidence.

Taking these results literally, one would recommend endowing competition authorities (that currently invest a large share of their resources into sanctioning reported cases) with sufficiently large budgets and investigative rights to allow more investigations to take place. However, a leniency rule may have advantages for cartel prosecution that are not reflected in our experimental design. For example, in practice, a leniency rule benefits competition authorities by increasing the chance that they obtain useful evidence of unlawful agreements among the firms once an investigation is already opened—irrespective

of how the investigation was started (see [Bundeskartellamt, 2016](#)). Our design did not allow for reports and fine reductions after an investigation was opened. In practice, such reports are common and of high importance.

While we believe that our setup provides a reasonable setting to study the short-term effects of a leniency rule on cartelization, we are more cautious when drawing conclusions about the long-term effect of a leniency rule. Indeed, one of our findings suggests that we might possibly underestimate the latter: the probability that a cartel is reported, though at a very low level in both treatments, seems to be higher with a leniency rule than without. It seems plausible that such reporting would impede or delay cartel recidivism in the long run. However, our setup cannot entirely capture such effects because of the limited number of consecutive market interactions. Therefore, some of our tests on these more subtle differences are not well powered. Whether the null result regarding the effect of the leniency rule would survive in the long run, when subjects have more time to gain experience with the complex strategic situation, remains an open question that we leave—as well as the question to what extent trust relates to cartelization—for future research.

Finally, the way in which we implemented the active competition authority in the experiment offers a methodological contribution that may be useful in experimental studies with communication among participants in general. This innovation allows researchers to connect a participant’s payoff in the experiment with the content of their communication during the experiment. Thus, this feature bears potential for increasing the external validity of experimental studies. For example, in a cartel context, it may allow judging whether a cartel has actually re-formed after being fined, but it can be useful when studying other forms of criminal behavior such as, e.g., corruption. While the accuracy of the experimental authorities is already high, future studies could adjust the design to further increase it. One potential change would be to have authorities also judge the entire interaction—as the expert does—which would make the two judgments more comparable and, therefore, is likely to increase overlap.

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# Appendix

## A Theoretical background

In this section, we derive the critical discount factors for an infinitely repeated game that provides the background for our experimental design. We note that the experimental design differs from this theoretical setup in that the first rounds are undiscounted and a random termination rule only kicks in from round 25 onward. However, this is one established way to implementing infinite games in laboratory experiments (see [Fréchette and Yuksel, 2017](#)) so that we view the chosen repeated game model as an appropriate modeling framework.

### A.1 Modeling framework

In the experiment, participants interact in groups of four, consisting of a market of three firms and one competition authority. The interaction between the firms is characterized by Bertrand competition with differentiated products. The same firms play the following stage game repeatedly.

**Stage game:** We let the quantity sold by each firm  $i$  given its own price  $p_i$  and the prices of its two competitors  $j$  and  $k$ ,  $p_j$  and  $p_k$ , be given by:

$$(1) \quad Q_i[p_i, p_j, p_k] = 40 - \frac{100}{9}p_i + \frac{80}{9}(p_j + p_k),$$

where firms may choose only integer prices so that  $p_i, p_j, p_k \in \mathbb{N}_0$ .

Per period profit for each firm is computed as  $(p_i - c)Q_i$  where  $c$  is the unit cost of production that we normalize to zero for simplicity. Then firm  $i$ 's profit as a function of its own and the competitors' prices is given by:

$$(2) \quad \Pi_i[p_i, p_j, p_k] = 40p_i - \frac{100}{9}p_i^2 + \frac{80}{9}p_i(p_j + p_k)$$

Deriving the individual best-response functions and solving for the symmetric Nash equilibrium yields  $p = 3$  as the equilibrium price of the stage game with a corresponding per firm profit of  $\Pi = 100$ . If we instead consider the maximization of joint profits, we find a symmetric joint profit maximizing price of  $p = 9$ , which yields a per firm profit of 180. Given that the other two firms choose a price of  $p = 9$ , the optimal unilateral undercutting price is  $p = 5$ . Deviating to  $p = 5$  yields a deviation profit of 322.22 (rounded to 322 in the profit table of the experiment). The other two firms that continue to charge the collusive price  $p = 9$  make a profit of only 20 in the respective period.

For the implementation in the laboratory experiment, we restrict the price setting range to the integers from 0 to 12. All prices above 12 are at least weakly dominated by those prices in the restricted range. Thus, this only helps to simplify the experiment.

**Controls and fines:** A cartel can be detected and fined during its existence and after its end. In each round a control of the competition authority is launched with an exogenous probability of 10% or because a firm self-reported its cartel. If a control is launched an existing or past cartel is detected and fined with certainty.

A cartel member is fined based on its cumulative profits during the participation in a collusive agreement as judged by the competition authority. However, past profits can only to some extent be reduced by a fine. For the computation of the cumulative profits on which the fine is applied, profits from period  $t$  are taken into account with 100%, profits from period  $t-1$  with 80%, profits from period  $t-2$  with 60%, profits from period  $t-3$  with 40%, and profits from period  $t-4$  with 20%. Profits from period  $t-5$  or earlier are only relevant for the computation of a potential fine (chosen by the authorities and the expert as 0%, 50% or 100% that will be applied to the cumulative profits), but the fine is not applied to these profits. This ensures that fine sizes in our setup correspond approximately to the magnitude of real cartel cases.

However, the experimental program does not know in which rounds a cartel existed because the authorities are only asked to evaluate for how many rounds since the last control a cartel existed but do not specify the rounds. Therefore, the program uses

the following approximation: Based on the cartel duration as specified by the authority and the number of rounds that passed since the last control, the program computes an adjustment factor in the form of the percentage of rounds since the last control during which a cartel existed. This factor is then multiplied with the discounted cumulative profits from the five rounds preceding the control as detailed above. In the case where firms either always collude or always compete, the program yields exactly the fines specified above.

**Feedback, fines, punishment of deviations:** We assume that a deviation from a cartel is detected by the other firms immediately due to the complete feedback about each firm's price setting. Expected fines are increasing during the first five rounds of each cartel phase. For the computations that relate to perfectly collusive behavior, we assume that the fine is perceived as a fixed fine with the size that can be expected in our setup when the collusive agreement is perfect, i.e., all members always set the joint profit-maximizing price which results in per-period-per-firm profits of  $\Pi^c = 180$ . Then, using the linear depreciation of fine-relevant profits as introduced above, the expected fine in an infinitely repeated game when colluding perfectly equals  $F = 540$ . We further assume that deviations as well as reports will be punished by playing Nash forever after.

**Repetition:** Suppose that time is discrete and that the stage game is repeated infinitely often with the participants discounting future payoffs with a discount factor  $\delta$ .<sup>32</sup> For the analysis of the repeated game, we restrict attention to the following set of stage game payoffs: the payoff from the Nash equilibrium in the stage game,  $\Pi^n = 100$ , the payoff from the joint-profit-maximizing price in the stage game (the collusive or cartel payoff),  $\Pi^c = 180$ , the deviation payoff that is made from an optimal unilateral deviation from the collusive agreement,  $\Pi^d = 322$ , and the payoff that is made by the remaining cartel members when one member deviates,  $\Pi^b = 20$ . It holds that  $\Pi^b < \Pi^n < \Pi^c < \Pi^d$ .

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<sup>32</sup>We restrict attention to a standard stationary repeated game because we see our experimental design as one way to bring the repeated game to the laboratory even though it diverges from theory in certain aspects. To account for the fact that subjects may perceive the game slightly differently, we include below a discussion of implicit discount factors that subjects can compute in each round from the expected continued duration of the game (see A.2).

We only consider settings with law enforcement and compare the games that ensue with and without a leniency rule that exempts the firm that first self-reports a cartel from paying any fine for past collusive behavior.

## A.2 Participation and incentive compatibility constraints

Firms will only choose the collusive equilibrium if this will yield a greater payoff than playing the Nash equilibrium. Furthermore, in a collusive equilibrium, it does not pay for any firm to deviate unilaterally in any round. In this subsection, we investigate these conditions for both treatments.

**Participation constraints:** First, consider the participation constraint for collusion. This reads in both the leniency and the no-lenieny setting as

$$(3) \quad \frac{\Pi^c - \Pi^n}{1 - \delta} > \frac{\alpha F}{1 - \delta}$$

With the parameters in the experiment, this is clearly fulfilled because  $80 > 54$ . Next, consider the incentive compatibility constraints of collusion.

**Incentive compatibility without a leniency rule:** Without a leniency rule, the value of the strategy “sticking to the collusive agreement”, i.e., setting each period the joint-profit-maximizing price and doing so even if the cartel has been detected through the exogenous detection mechanism, is:

$$(4) \quad V^c = \frac{\Pi^c + \alpha(\delta V^c - F)}{1 - (1 - \alpha)\delta} = \frac{\Pi^c + \alpha\delta V^c - \alpha F}{1 - (1 - \alpha)\delta}$$

Solving for  $V^c$  this yields

$$(5) \quad V^c = \frac{\Pi^c - \alpha F}{1 - \delta}$$

We assume that as part of the strategy “sticking to the collusive agreement” cartel members continue to collude if their cartel has been detected due to a control that was triggered by the exogenous detection probability. This implies that their cartel continues to exist after such a control; and it also continues to face the exogenous risk of being detected and fined in every single period.

Consider now the possibility of deviating from the collusive agreement. Any such deviation is immediately observed by the cartel members (there is feedback on all prices set in a period, making it easy to observe the deviation). We assume that a deviation is punished by reverting to the Nash equilibrium of the stage game forever after. The value from deviating once and being punished is

$$(6) \quad V^d = \Pi^d + \delta \frac{\Pi^n}{1 - \delta} - \frac{\alpha F}{1 - (1 - \alpha)\delta}$$

The third term results from the possibility of a cartel being detected and fined with exogenous probability also after it has broken down. As the cartel is assumed to never reform, the cartel can only be detected once after the deviation.

The incentive compatibility constraint in a setting without leniency (our treatment named FINE) is therefore

$$(7) \quad \frac{\Pi^c - \alpha F}{1 - \delta} > \Pi^d + \delta \frac{\Pi^n}{1 - \delta} - \frac{\alpha F}{1 - (1 - \alpha)\delta}$$

From this constraint, we compute the critical discount factor which determines the range of discount factors for which, given all the other parameters in our experiment, collusion can be sustained as an equilibrium.

Solving the above constraint for  $\delta$ , we obtain a quadratic equation which has only one solution that lies in the interval  $[0, 1]$  and therefore has a unique admissible solution  $\delta_N = 0.682711$ .

**Incentive compatibility with a leniency rule:** Consider now a setting with a leniency rule, i.e., the first firm that self-reports a collusive agreement is exempt from paying



a fine. This implies that any deviation from the collusive agreement is coupled with a self-report in order to pre-empt the other firms that would report the cartel once they learn about the deviation. Thus, the value from defecting from the collusive agreement becomes:

$$(8) \quad V^d = \Pi^d + \delta \frac{\Pi^n}{1 - \delta}$$

Reporting the cartel leads to an immediate fine to the other cartel members but not the self-reporting deviator. Moreover, the self-report implies that the cartel, which is assumed not to be reformed because of the Nash reversion punishment, does not face any detection risk in the future.

Thus, the incentive compatibility constraint in a setting with a leniency rule (named LENIENCY) is

$$(9) \quad \frac{\Pi^c - \alpha F}{1 - \delta} > \Pi^d + \delta \frac{\Pi^n}{1 - \delta}$$

From this constraint, we also compute the critical discount factor given all other parameters. Setting the above incentive constraint to bind and solving for  $\delta$ , we obtain the unique solution  $\delta_L = 0.883$ .

Note that the critical discount factor of an infinitely repeated discounted game with punishment by Nash reversion exceeds  $2/3$  in the cases with and without leniency. Collusion on the symmetric joint-profit maximizing price of the stage game is therefore not an equilibrium of the continuation game starting in round 25, neither in FINE nor in LENIENCY. According to a strict backward induction argument, this type of collusion in the repeated game starting from the first round cannot be supported as a subgame-perfect equilibrium in either treatment. When we, however, compute implicit discount factors for the still to be expected duration of the interaction in a given period, assuming that the uncertain end prevents unraveling of cooperation, we find that collusion on the symmetric joint-profit maximizing price is incentive compatible throughout the first 23

rounds of play in treatment FINE and through the first 18 rounds in treatment LENIENCY as illustrated in Table 1.

We further note that the experimental setting also allows for asymmetric collusive strategies. Specifically, the three firms may alternate in choosing the prices 7 – 7 – 12 yielding an average per-period profit of 217.778 for each firm. Assuming again that any deviation will be punished by reversion to the Nash equilibrium of the stage game, the incentive compatibility constraint for this strategy yields a critical discount factor clearly below  $2/3$  in both treatments. Specifically, in the leniency setting, for a firm supposed to set a price of 7, the optimal unilateral deviation is  $p = 5$  with a one-time deviation profit of 344.444 which – using these values in the incentive compatibility constraint (9) – yields a critical discount factor of 0.613, and for a firm supposed to set a price of 12, the optimal unilateral deviation is also  $p = 5$  with a deviation profit of 233.33 which yields a critical discount factor of 0.292. The analogous critical discount factors are even lower in the setting without a leniency rule and are easily derived from the incentive compatibility constraint (7). The repeated game may have additional asymmetric equilibria that we have not identified.

**Collusive price and incentive compatibility** In principle, collusion may occur at prices different from the jointly optimal price of nine. This will lead to lower expected profits but relaxes the incentive compatibility constraint. In Table 2, we have compiled an overview of the critical discount factors that result per treatment for different symmetric collusive prices. For the computation, we otherwise assume the parameters of the experiment,  $\alpha = 0.1$ , and a fine equal to the expected fine in a steady state equilibrium with stable collusion  $F = 3\Pi^c$ , where  $\Pi^c$  is the per-firm profit per period from continue collusion on the respective price. The computed values are derived directly from the incentive compatibility constraints as derived above. Values that are set in bold lie below the continuation probability of two thirds.

Table 1: Implicit and critical discount factors in treatments FINE and LENIENCY.

round	expected rounds to go	implicit $\delta$	exceeds critical $\delta$	
			FINE	LENIENCY
1	26	0.962	yes	yes
2	25	0.960	yes	yes
3	24	0.958	yes	yes
4	23	0.957	yes	yes
5	22	0.955	yes	yes
6	21	0.952	yes	yes
7	20	0.950	yes	yes
8	19	0.947	yes	yes
9	18	0.944	yes	yes
10	17	0.941	yes	yes
11	16	0.938	yes	yes
12	15	0.933	yes	yes
13	14	0.929	yes	yes
14	13	0.923	yes	yes
15	12	0.917	yes	yes
16	11	0.909	yes	yes
17	10	0.900	yes	yes
18	9	0.889	yes	yes
19	8	0.875	yes	no
20	7	0.857	yes	no
21	6	0.833	yes	no
22	5	0.800	yes	no
23	4	0.750	yes	no
24	3	0.667	no	no
25	2	0.667	no	no
26	2	0.667	no	no
following	2	0.667	no	no

*Notes:* For the first 24 rounds, the implicit discount factor is computed based on the expected duration of the interaction of 27 rounds. From round 25 onwards, the implicit discount factor is replaced with the actual continuation probability of 2/3. The critical discount factor refers to the equilibrium where firms collude on the symmetric jointly optimal price of 9 in the stage game with Nash reversion after any deviation. As discussed in the text, the critical discount may be lower with collusion using asymmetric strategies and may be higher if firms do not trust each other sufficiently.

Table 2: Critical discount factors per treatment for different collusive prices.

Treatment	FINE	LENIENCY
price=9	0.6827	0.8829
price=8	<b>0.6011</b>	0.8618
price=7	<b>0.5014</b>	0.8519
price=6	<b>0.4071</b>	0.875

### A.3 Extended model version with trust

We believe that collusion is helped by the participating firms trusting each other. While trust does not play a role in the above theory of collusion in a repeated game setting, the model can be extended to allow for it. In line with a large experimental literature on trust, we understand the decision to continue to collude as the trusting action.<sup>33</sup> For our purposes, we follow Falk et al. (2018) who argue that “trust constitutes a belief rather than a preference” and interpret as trust the subjective belief about the probability that the others are “trustworthy,” i.e. in our context also decide to continue colluding. Therefore, we extend the above simple model with a belief variable,  $\mu$ , that captures a firm’s belief that the other two firms will stick to the symmetric joint-profit maximizing price.

We reconsider the value of the strategy “sticking to the collusive agreement” with a belief element. Specifically, denote by  $\mu$  the probability that—in a given round—both competitors collude, with probability  $1 - \mu$  they do not collude. Recall that we have simplified the model such that firms can only choose to compete, to collude, or to deviate, where the decision to compete is represented by the outcome from choosing the Nash price, collude by the outcome from choosing the jointly optimal price of 9, and deviate by the outcome from choosing the optimal unilateral deviation price. Note that now the payoff of a firm that continues to collude when another firm unilaterally deviates,  $\Pi^b$ , becomes relevant. We continue to assume that collusion on a price of 9 is supported by

<sup>33</sup>We commonly say to trust someone or something if we believe the person will not harm us or something is safe and reliable (see for instance the entry *trust* in the Cambridge dictionary, <https://dictionary.cambridge.org/us/dictionary/english/trust>). According to this understanding, the decision to trust can be understood as a decision under uncertainty where individuals hold subjective beliefs about the risk that their trust will be justified or betrayed. The literature has taken different stands on the relative importance of subjective beliefs, risk preferences, and betrayal aversion (see, e.g., Karlan, 2005; Bohnet et al., 2008; Fehr, 2009; Falk et al., 2018).

punishing any deviation from this price with playing the Nash equilibrium of the stage game forever after. Subsequently, we can replace the stage payoff from one further round of choosing to collude with the expected value from choosing to collude in this round and write the continuation value as:

$$(10) \quad V^c = \frac{\mu\Pi^c + (1 - \mu)(\Pi^b + \frac{\delta\Pi^n}{1-\delta}) - \alpha F}{1 - \delta\mu}$$

With this trust-adjusted continuation values, we can rewrite the incentive compatibility constraints for both the setting with and without a leniency rule. Solving for  $\delta$  as a function of the belief  $\mu$  yields the following critical discount factors:

$$(11) \quad \tilde{\delta}(\mu) = \frac{267+20\mu-\sqrt{71289-156930\mu+89200\mu^2}}{333\mu} \quad \text{in FINE}$$

$$(12) \quad \tilde{\delta}(\mu) = -\frac{2(40\mu-89)}{111\mu} \quad \text{in LENIENCY}$$

In Figure 6, we plot these critical discount factors for beliefs about continued collusion ranging from 0.8 to 1.<sup>34</sup> The figure further includes a horizontal line at 0.96, which is the implicit discount factor at the end of round 2, i.e. the round when the firm participants have the option to chat for the first time. As can be seen from the figure, the critical discount factor in treatment FINE is always much lower than that in LENIENCY and increases more slowly when the belief  $\mu$  decreases. While the critical discount factor in FINE remains below the implicit discount factor in round 2, even for with substantial doubt about continued collusion of the other two firms, the critical discount factor in LENIENCY exceeds the round 2 implicit discount factor already when the belief  $\mu$  falls below 0.954. Thus, the viability of collusion is more reactive to the belief into collusion and, thus, more fragile in the treatment with a leniency rule than without it. Furthermore, the leniency rule is intended to create distrust among firms that each have an incentive to self-report if they have some doubt that the others will continue to collude. In other

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<sup>34</sup>If a firm expects other to collude with probability of at least 0.8, this implies that the firm expects to collude for at least 5 rounds, so that the steady-state approximation of the fine payment, which takes profits from 5 rounds of collusion as input is applicable.

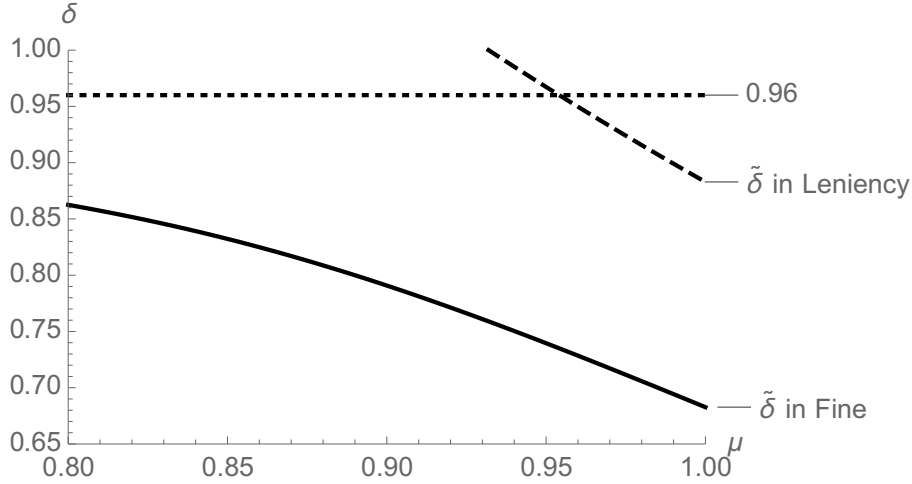


Figure 6: Critical discount factors as functions of the belief  $\mu$  in treatments with and without a leniency rule.

words, a leniency policy intends to lower the belief  $\mu$ . Therefore, we hypothesize that a leniency rule will reduce the extent of cartelization also through its detrimental effect on the belief in continued collusion, which makes it more difficult to sustain a cartel as shown above.

#### A.4 Predictions from theoretical background

We conclude from the above framework and its analysis that collusion can be potentially supported as an equilibrium in either treatment but that this should be expected to be more difficult in the treatment with a leniency rule as the critical discount factor is consistently higher with a leniency rule. Further, we note that, later in the game, it becomes increasingly difficult to sustain collusion and this is the case earlier in LENIENCY than in FINE. Thus, firms in LENIENCY have less opportunity to collude. On this, we base Hypothesis 1a. Further, we show that the viability of the symmetric collusive equilibrium is more reactive to potential distrust in the treatment with a leniency rule. This leads us to Hypothesis 1b. Finally, we use the model extended by trust also as a basis for Hypothesis 1c because the use of the leniency rule will likely lead to a decrease in the trust among firms, and we have shown above that cartelization is more difficult to achieve for lower values of  $\mu$ .

Further, the theoretical background also allows us to make predictions about prices. First, as a consequence of the predicted difference in the extent of cartelization, we expect that prices will, on average, be lower in treatment LENIENCY than in FINE because the price during a cartel phase is higher with a theoretical prediction of 9 than during competitive phases where theory predicts a price of only 3 in the Nash equilibrium of the stage game (Hypothesis 2a).

Furthermore, we investigate the possibility that firms may collude on a price below the theoretical optimum of 9 to relax the incentive compatibility constraint. The results with respect to the ensuing critical discount factors are collected in Table 2.

What can be seen is that the critical discount factor decreases sharply in the price on which firms coordinate symmetrically in the treatment FINE whereas there is only a moderate decrease in treatment LENIENCY. However, whereas in FINE, the critical discount factor drops below the continuation probability already for a collusive price of 8, in treatment LENIENCY, the critical discount factor reaches a minimum at a price of 7. Further, in the presence of a leniency rule, lowering the collusive price may extend the range of rounds during which collusion may still be supported if the implicit discount factor is used to judge its attractiveness by two more rounds until round 20. In treatment FINE, already moving to a price of 8 would allow to support collusion throughout the continuation game starting in any round. Based on this analysis, we argue that the average price on which firms collude may be lower in the presence of a leniency rule (Hypothesis 2b).

Finally, it is obvious from the design of the fine and leniency treatments, that the use of the self-report option is costly to a firm in the fine treatment whereas it reduces a firm's expected fine payment in the leniency treatment (Hypothesis 3).

## B Additional results

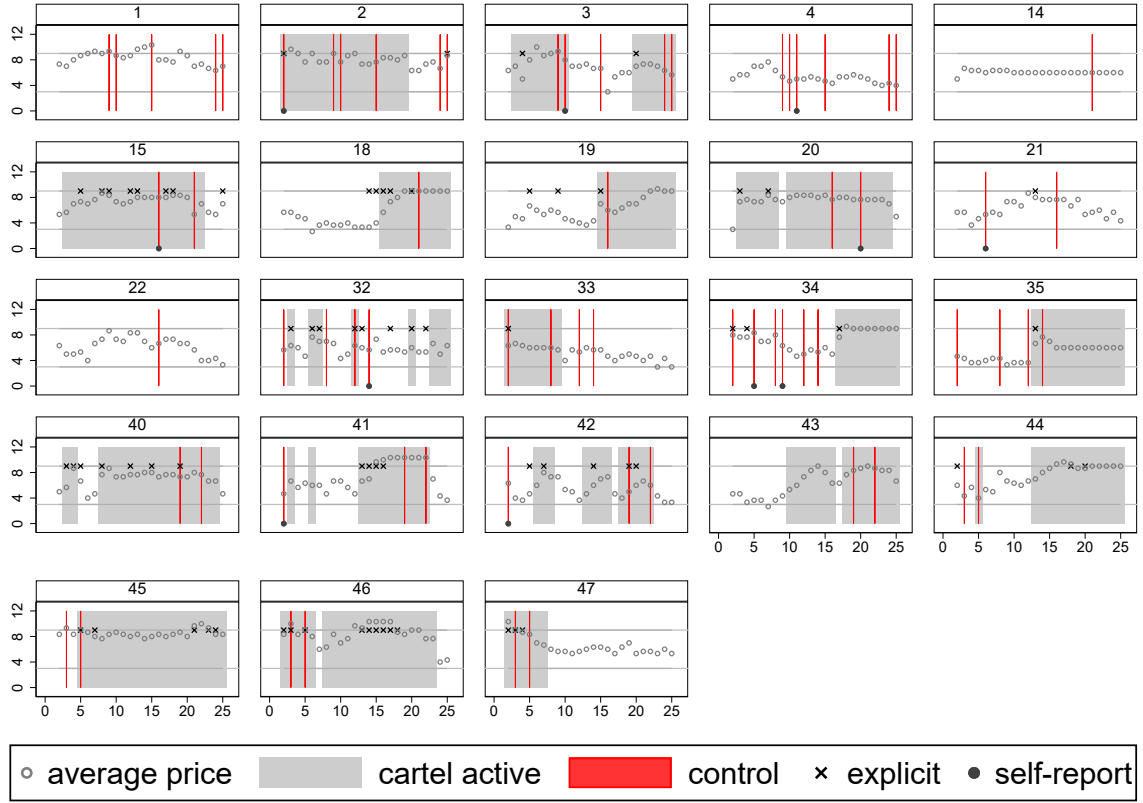


Figure 7: Prices by market (each group of three firms forms a market): FINE. Gray shaded areas mark periods in which a cartel existed according to the expert judgment. Vertical red lines mark rounds in which a control took place. Black balls indicate that a control was triggered by a self-report. Black crosses at the height of the collusive price of 9 indicate that in this round, the group engaged in what we classified as “explicit” communication.



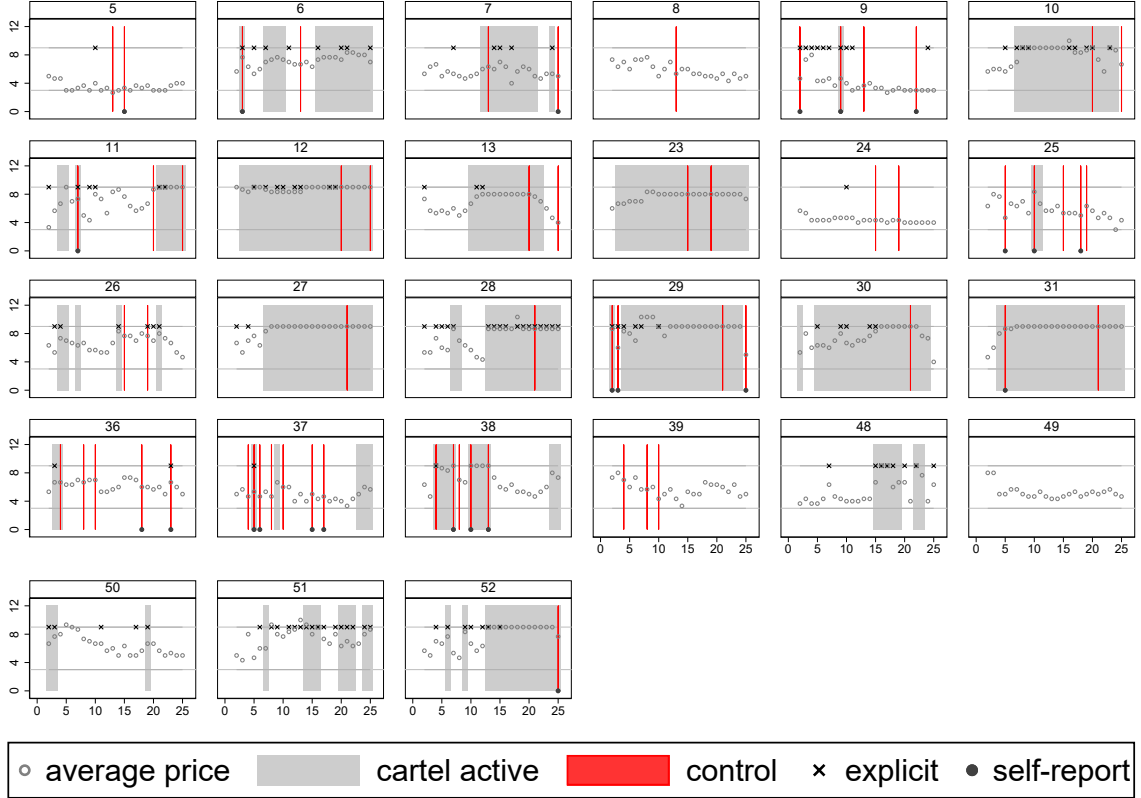


Figure 8: Prices by market (each group of three firms forms a market): LENIENCY. Gray shaded areas mark periods in which a cartel existed according to the expert judgment. Vertical red lines mark rounds in which a control took place. Black balls indicate that a control was triggered by a self-report. Black crosses at the height of the collusive price of 9 indicate that in this round, the group engaged in what we classified as “explicit” communication.

## C Instructions

In the following, we present our instructions for firms in Section C.1 and for authorities in Section C.2. Please note that text in *italics* only appears in instructions for the LENIENCY treatment.

### C.1 Instructions for firms

Today you are participating in a decision-making experiment. If you read the following instructions carefully, you can earn money. The amount of money you receive depends on your decisions and the decisions of other participants.

For the entire duration of the experiment it is prohibited to communicate with other participants. Therefore, we ask you not to talk to each other. Violation of this rule will result in exclusion from the experiment and payment.

If there is something you do not understand, please have another look at these instructions or give us a hand signal. We will then come to your seat and answer your question personally.

During the experiment, we do not talk of euros but of points. The number of points you earn during the experiment will be converted into euros as follows:

$$\mathbf{125\ Points = 1\ euro}$$

At the end of today's experiment, you will receive the points earned in the experiment converted into euros in **cash** plus 5 euros as basic endowment.

On the following pages we will explain the exact procedure of the experiment to you, starting with the general procedure. We will then familiarize you with the procedure on the screen. Then, you will have the opportunity to familiarize yourself on the computer screen with the calculation of profits in the experiment before the experiment begins.

## The experiment

At the start of the experiment, you will be matched randomly into a group with three other participants. During the experiment, you will make decisions within this group of four persons in total. The composition of your group remains the same throughout the entire experiment. Neither you nor the other participants will be informed about the identity of the participants in the group – neither during nor after the experiment.

The experiment consists of at least 25 rounds. You will receive more information on the number of rounds on page 5 of this document.

Every participant in your group represents either a firm or the competition authority. There are three firms (firm 1, 2 and 3) and one competition authority. **In all rounds, you take the role of a firm.** At the start of the experiment, you will be informed onscreen about which firm you are. You will be the same firm during the entire experiment.

The firms 1, 2 and 3 sell the same (fictional) good on the same market. Production of this good is costless for the firms. All firms decide simultaneously what price they want to charge for the good in a round. The price must be an integer between 0 and 12. If a firm does not enter its own price and clicks the OK button within 30 seconds (60 seconds in the first round only), a price of 0 is automatically set for this firm.

Your profit depends on your own price and the average price of the other two firms. Your profit is larger the higher the prices of the other two firms are. Your own price has two effects on your own profit: If you increase your own price, the quantity you sell decreases, but at the same time your earnings per unit sold increases. Depending on which effect is larger, your profit increases or decreases. The table on the following page shows your profit, depending on your own price and the averages prices of the other two firms. (This table is the same for all three firms, read from their perspective.)

Your profit, depending on the average price of the two other firms

		Average price of the two other firms												
		0	1	2	3	4	5	6	7	8	9	10	11	12
Your own price	0	0	0	0	0	0	0	0	0	0	0	0	0	0
	1	29	<b>38</b>	47	56	64	73	82	91	100	109	118	127	136
	2	36	53	<b>71</b>	89	107	124	142	160	178	196	213	231	249
	3	20	47	73	<b>100</b>	127	153	180	207	233	260	287	313	340
	4	0	18	53	89	<b>124</b>	160	196	231	267	302	338	373	409
	5	0	0	11	56	100	<b>144</b>	189	233	278	322	367	411	456
	6	0	0	0	0	53	107	<b>160</b>	213	267	320	373	427	480
	7	0	0	0	0	0	47	109	<b>171</b>	233	296	358	420	482
	8	0	0	0	0	0	0	36	107	<b>178</b>	249	320	391	462
	9	0	0	0	0	0	0	0	20	100	<b>180</b>	260	340	420
	10	0	0	0	0	0	0	0	0	0	89	<b>178</b>	267	356
	11	0	0	0	0	0	0	0	0	0	0	73	<b>171</b>	269
	12	0	0	0	0	0	0	0	0	0	0	0	53	<b>160</b>

From the second round on, you have the option to communicate with the other firms via chat messages at the beginning of each round. The duration of a chat cannot exceed 60 seconds in one round. In this chat, you can write anything you want with the exception that you are not allowed to reveal hints on your identity.

§1 GWB of the Act against Restraints of Competition prohibits price agreements and the attempt of price agreements (for the exact wording, see the box).

§ 1 Prohibition of Agreements Restricting Competition

Agreements between undertakings, decisions by associations of undertakings and coordinated practices which have as their object or effect the prevention, restriction or distortion of competition are prohibited.

At the end of a round, the chat messages can be subject to an audit. In an audit, the competition authority judges whether the texts you and the other firms wrote in the chat are in accordance with §1 GWB. Such an audit can be initiated in two ways, by a random mechanism and by the firms:

- In each round, a random mechanism decides whether an audit takes place or not. This random mechanism is programmed so that an audit takes place with a probability of 10% (i.e. on average in 10 out of 100 cases).
- In addition, in each round the firms have the opportunity to initiate an audit themselves, both while setting their price and after they have learned the prices of the other firms. You can initiate an audit by clicking on a small white box at the bottom left of the screen. Initiating an audit cannot be undone. As soon as you click on the small white box, the box for that round disappears and an audit will definitely take place. The same applies to the other two firms in your group.

When an audit takes place, the competition authority has insight into all communication in the previous chats in your group as well as into the pricing since the first round. The competition authority imposes penalties on firms that have violated §1 GWB. It decides on the individual penalties for each of the three firms and for how long an agreement has been in place.

The penalty may be 0%, 50% or 100% of a firm's accumulated pecuniary profit during the agreement. 0% (no penalty) means that the firm has acted in accordance with §1 GWB, 100% means a clear, serious violation. 50% should be chosen for less serious violations.

The pecuniary profit is measured according to your profit that you have earned and the duration of the agreement. However, if the agreement has been in place for more than five rounds, the penalty will only be applied to the profits of the last five rounds. Previous rounds are included in the calculation of the penalty, but will not be punished themselves.

The competition authority has three minutes to reach its decision.

*The active initiation of an audit by a firm leads to the possibility that that firm is exempted from punishment. If only one firm has initiated the audit, that firm will automatically receive full amnesty. If two or three firms have initiated an audit, the penalty will only be waived for the firm that first initiated the audit.*

After each round, the firms are informed about their own price, their profit and, if applicable, their penalty. In addition, each firm is informed about the prices set by the other two firms in the current round and, if applicable, their penalties. *You will also be informed on whether a firm has initialized an audit by the competition authority and has thus received an exemption of its penalty.*

From the 25th round on, a random mechanism decides in each round whether the experiment ends with the last round completed. With a probability of 33.3% (i.e. in an average of 1 out of 3 cases) the experiment ends with the last round completed. With a probability of 66.7% (i.e. in 2 out of 3 cases) another round takes place. In addition, it is ensured that the experiment does not last longer than 2 hours and 30 minutes.

After the last round, you will see an overview screen showing you how many points you have earned in total. You will receive all points converted into euros directly after the experiment.

If something is not clear to you, please give a clear hand signal. We will then come to your seat.

After the experiment we will ask you to fill out a short questionnaire on the computer.<sup>35</sup>  
You will then receive your payment.

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<sup>35</sup>The questionnaire asked for some statistical information which we use for the administration of the computer laboratory. Otherwise, we did not use it for the evaluation of the data.

## C.2 Instructions for authorities

Today you are participating in a decision-making experiment. If you read the following instructions carefully, you can earn money. The amount of money you receive depends on your decisions.

For the entire duration of the experiment it is prohibited to communicate with other participants. Therefore, we ask you not to talk to each other. Violation of this rule will result in exclusion from the experiment and payment.

If there is something you do not understand, please have another look at these instructions or give us a hand signal. We will then come to your seat and answer your question personally.

During the experiment, we do not talk of euros but of points. The number of points you earn during the experiment will be converted into euros as follows:

$$\mathbf{125\ Points = 1\ euro}$$

As an exception, this time you will not receive your payment for today's experiment in cash at the end of the experiment, but in about 2-3 weeks via bank transfer. You will receive more information on the bank transfer on page 6 of these instructions. In addition to your other earnings in this experiment, you will receive 10 euros in cash.

On the following pages we will explain the exact procedure of the experiment to you, starting with the general procedure. We will then familiarize you with the procedure on the screen. Then, you will have the opportunity to familiarize yourself on the computer screen with your task in the experiment before the experiment begins.



## The experiment

At the start of the experiment, you will be matched randomly into a group with three other participants. During the experiment, you will make decisions within this group of four persons in total. The composition of your group remains the same throughout the entire experiment. Neither you nor the other participants will be informed about the identity of the participants in the group – neither during nor after the experiment.

The experiment consists of at least 25 rounds. You will receive more information on the number of rounds on page 6 of this document.

Every participant in your group represents either a firm or the competition authority. There are three firms (firm 1, 2 and 3) and one competition authority. **In all rounds, you take the role of the competition authority.**

The firms 1, 2 and 3 sell the same (fictional) good on the same market. Production of this good is costless for the firms. All firms decide simultaneously what price they want to charge for the good in a round. The price must be an integer between 0 and 12. If a firm does not enter its own price and clicks the OK button within 30 seconds, a price of 0 is automatically set for this firm.

The profit of a firm depends on its own price and the average price of the other two firms. The profit is larger the higher the prices of the other two firms are. The own price has two effects on the profit of a firm. If the own price increases, the quantity sold by this firm decreases, but at the same time the earnings per unit sold increases. Depending on which effect is larger, a firm's profit increases or decreases. The table on the following page shows the profit of a firm, depending on its own price and the averages prices of the other two firms. (This table is the same for all three firms.)

A firm's profit, depending on the average price of the two other firms

		Average price of the two other firms												
		0	1	2	3	4	5	6	7	8	9	10	11	12
Own price	0	0	0	0	0	0	0	0	0	0	0	0	0	0
	1	29	<b>38</b>	47	56	64	73	82	91	100	109	118	127	136
	2	36	53	<b>71</b>	89	107	124	142	160	178	196	213	231	249
	3	20	47	73	<b>100</b>	127	153	180	207	233	260	287	313	340
	4	0	18	53	89	<b>124</b>	160	196	231	267	302	338	373	409
	5	0	0	11	56	100	<b>144</b>	189	233	278	322	367	411	456
	6	0	0	0	0	53	107	<b>160</b>	213	267	320	373	427	480
	7	0	0	0	0	0	47	109	<b>171</b>	233	296	358	420	482
	8	0	0	0	0	0	0	36	107	<b>178</b>	249	320	391	462
	9	0	0	0	0	0	0	0	20	100	<b>180</b>	260	340	420
	10	0	0	0	0	0	0	0	0	0	89	<b>178</b>	267	356
	11	0	0	0	0	0	0	0	0	0	0	73	<b>171</b>	269
	12	0	0	0	0	0	0	0	0	0	0	0	53	<b>160</b>

From the second round on, the firms have the option to communicate via chat messages. The duration of chat cannot exceed 60 seconds.

§1 GWB of the Act against Restraints of Competition prohibits price agreements and the attempt of price agreements (for the exact wording, see the box).

§ 1 Prohibition of Agreements Restricting Competition

Agreements between undertakings, decisions by associations of undertakings and coordinated practices which have as their object or effect the prevention, restriction or distortion of competition are prohibited.

At the end of a round, the chat messages can be subject to an audit. In an audit, you as the competition authority judge whether the texts the firms wrote in the chat are in accordance with §1 GWB. Such an audit can be initiated in two ways, by a random mechanism and by the firms:

- In each round, a random mechanism decides whether an audit takes place or not. This random mechanism is programmed so that an audit takes place with a probability of 10% (i.e. on average in 10 out of 100 cases).
- In addition, in each round the firms have the opportunity to initiate an audit themselves, both while setting their price and after they have learned the prices of the other firms. A firm can initiate an audit by clicking on a small box on the screen.

When an audit takes place, you will not be informed on how it was initiated. You have insight into all communication in the previous chats in your group as well as into the pricing since the first round. Your task is to impose penalties on firms that have violated §1 GWB. You decide on the individual penalties for each of the three firms and for how long an agreement has been in place. The duration is the number of all rounds since the last audit (or since the start of the experiment) in which, in your opinion, an agreement had a visible effect on the prices.

The penalty may be 0%, 50% or 100% of a firm's accumulated pecuniary profit during the agreement. 0% (no penalty) means that the firm has acted in accordance with §1

GWB, 100% means a clear, serious violation. 50% should be chosen for less serious violations.

The pecuniary profit is measured according to the profit of the respective firm and the duration of the agreement. However, if the agreement has been in place for more than five rounds, the penalty will only be applied to the profits of the last five rounds. Previous rounds are included in the calculation of the penalty, but will not be punished themselves. You, in the role of the competition authority, nevertheless enter the entire duration of the cartel; the computer program proportionally calculates the penalties for the last five rounds.

Your payment as an competition authority depends on the consistency of your penalty decisions with those of a real competition law expert. After today's experiment, in the same way as you do today, this expert (a licensed lawyer specialized in competition law) will see the chat messages and prices and will assess the extent to which they contain violations of §1 GWB. You will receive 900 points for each match between your decision and the expert's decision. You will also receive 900 points if you have correctly specified the duration of a possible agreement. Since you make four decisions for each penalty decision (one for each of the three firms and one for the total duration of the agreement), you can earn up to 3600 points. You will only receive points if you make exactly the same decision as the expert, otherwise (e.g. if you impose a 50% penalty on a firm and the expert would impose 100%) you will not receive any points for this partial decision. At the end, the **average** score of all rounds in which you were able to impose penalties is determined. This then determines your payment, which we will transfer to your bank account within 2 to 3 weeks. If there is no audit during the entire experiment, you will receive a fixed bank transfer of 15 euros in addition to your cash payment of 10 euros.

You have 3 minutes for each of your penalty decisions. If you do not specify the height of the penalty during this time, you will not receive any payment for your judgment and the computer program will assume for the calculation of the firms' profits that you have not imposed any penalties. **Please remember to submit your decision at the end by clicking the OK button.**

*The active initiation of an audit by a firm leads to the possibility that that firm is exempted from its punishment. If only one firm has initiated the audit, that firm will automatically receive full amnesty. If two or three firms have initiated an audit, the penalty will only be waived for the firm that first initiated the audit. This exemption will also be automatically implemented by the computer program, if necessary, and will not be relevant to your penalty decisions.*

After each round, the firms are informed about their own price, their profit and, if applicable, their penalty. In addition, each firm is informed about the prices set by the other two firms in the current round and, if applicable, their penalties. *The firms will also be informed on whether a firm has initialized an audit by the competition authority and has thus received an exemption of its penalty.*

From the 25th round on, a random mechanism decides in each round whether the experiment ends with the last round completed. With a probability of 33.3% (i.e. in an average of 1 out of 3 cases) the experiment ends with the last round completed. With a probability of 66.7% (i.e. in 2 out of 3 cases) another round takes place. In addition, it is ensured that the experiment does not last longer than 2 hours and 30 minutes.

Directly after the experiment you will receive 10 euros in cash. Your additional earnings from the experiment will be transferred to your bank account. Please enter your name and address as well as your bank details in the form and sign it. (You are welcome to fill in the form during the experiment, if you have nothing to do on the screen.)

If something is not clear to you, please give a clear hand signal. We will then come to your seat.

After the experiment we will ask you to fill out a short questionnaire on the computer. You will then receive your payment.

### C.3 Assistance for participants in the role of a competition authority | How does the expert punish?

What counts as an agreement?<sup>36</sup>

- If a firm explicitly suggest a price above 3 and then charges this price, the firm gets a 100% penalty.
- Convolved descriptions of prices are punished in the same way as if the corresponding price was given as a number.
- Agreements on prices not higher than 3 do not distort competition and therefore do not count as an agreement.
- If a firm doesn't write anything in the chat (but of course can read what the others write) it can still be punished. The amount of the penalty depends on the price and can be up to 100%, e.g. if the other two firms make a clear agreement and this firm sets exactly the price agreed by the other two firms over a long period of time.
- If the firms make an agreement that no one will abide by afterwards, there will be no penalty.
- Prices above 3, which have come about without any agreement, cannot be punished.

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<sup>36</sup>In order to clarify what behavior is defined to constitute a cartel we rely on legal practice. According to article 101 of the Treaty on the Functioning of the [European Union \(2012\)](#), a cartel covers "all agreements between undertakings, decisions by associations of undertakings and concerted practices [...] which have as their object or effect the prevention, restriction or distortion of competition [...]." With the beginning of the new millennium, the [European Union \(2002\)](#) began to constantly refine article 101 (formerly article 81) from a rule-based approach (that makes the per se assumption that all agreements between undertakings harm social welfare) to an effects-based approach ([European Commission, 2004, 2011; European Court of Justice, 2004; European Union, 2012, 2019](#)) (that prohibits only those agreements which indeed harm social welfare, see [Chiriță, 2014; Colomo, 2016; European Commission, 2004; European Court of Justice, 2004; European Union, 2012; Jones, 2006, 2010; Jones and Kovacic, 2017; Jones and Sufrin, 2016; Whelan, 2012](#)). The phrase *object* in article 101 essentially allows authorities to assume that a proven agreement was *causal* for an observed distortion of competition without having to prove this causal relationship legally. Also note that if a market participant who does not agree to take an expressed action but behaves as if she did, a concerted practice can be assumed by the authority ([Albors-Llorens, 2006; European Union, 2019; Odudu, 2010; Whish and Bailey, 2015](#)).

For determining the duration:

- For determining the duration of a cartel, all rounds in which the agreement was visibly effective in the prices count.
- If a company receives a 50% penalty for part of the total duration of the cartel and a 100% penalty for the remainder of the total duration, then the amount of the penalty that applies for a longer period will apply for the total duration (because the computer program does not allow for further gradation).
- If a firm joins an agreement already in place between the two other firms at a later round (or leaves the agreement earlier than the others), the longer overall duration of the cartel still applies to it. In order to prevent the fine from becoming unreasonably high, the amount of the fine can then be adjusted accordingly. (Example: Anyone who was involved in a 100% agreement in 5 out of 10 rounds receives a 50% penalty for the duration of 10 rounds.)
- If, after a penalty, prices remain at the same level as before the audit, a penalty may be imposed again at a later audit, even if there has been no new agreement.