Post-Cartel Tacit Collusion: Determinants, Consequences, and Prevention*

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Abstract

We experimentally investigate the determinants of post-cartel tacit collusion (PCTC), the effects of PCTC on market outcomes, and potential policy measures aimed at its prevention. PCTC occurs robustly with or without fines or leniency and is determined both by collusive price hysteresis and learning about cartel partners' characteristics and strategies. As a result, it is also strongly related to the preceding cartel success. PCTC generates a downward bias in the estimated cartel overcharges. This threatens the effectiveness of deterrence induced by private damage litigation and fines imposed on colluding firms based on the overcharge. This bias further increases with preceding cartel stability such that especially more stable sets of colluding firms may be deterred less when PCTC is present. Rematching colluding subjects with strangers within a session prevents PCTC. This indicates that barring colluding managers from their posts could help impede PCTC in the field.

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1 Introduction

Post-cartel tacit collusion (PCTC) occurs when firms tacitly collude after an explicit cartel, in which they were involved in before, breaks down. Such PCTC intensifies the negative welfare effects of collusion and at the same time undermines the effectiveness of policies aimed at the deterrence of cartels. In the presence of PCTC prices do not immediately return to the level of competition even after the cartel is detected. As a result, firms continue to earn supernormal profits and the harm induced by the cartel on welfare extends to post-cartel periods. Moreover, fines that are strongly related to the cartel price gains (called overcharges) in the cartel periods cannot fully deter collusion. These cartel overcharges are predominantly used by antitrust authorities to impose fines and are important in private damage litigation to calculate damages awarded to the cartel customers. Hence, given their large size and growing importance at an international level, these damages provide an important factor in deterrence. PCTC results in underestimated cartel overcharges if the supernormal markup created by PCTC is not accounted for. This leads to lower damages that are insufficient to deter collusion and to fully compensate customers. This downward bias in overcharge estimates is in particular a problem in some of the price-based approaches commonly used in court cases, in which post-cartel periods are used as competitive counterfactuals to establish the cartel overcharge (see, e.g. Davis and Garcés, 2009; Harrington, 2004).[1] Despite these important consequences of PCTC, little is known under which circumstances PCTC might occur, to what extent the overcharge estimates may be biased due to PCTC, and how antitrust law can be designed to prevent PCTC. Thus, a better understanding of the determinants of PCTC and of potential tools aimed at its prevention is vital. This study aims to add to this knowledge.

PCTC has been observed (or at least suspected) in various industries with results being based on different methodologies. Harrington (2004) provides a theoretical model, Fonseca and Normann (2012) experimental results, and Connor (1998, 2001), de Roos (2006), Ordóñez-de Haro and Torres (2014), Kovacic *et al.* (2007), and Crede (2019) empirical observations that point towards the emergence of tacit collusion after the end of cartels.[2] Connor (1998) notes that prices in the citric acid industry did not decline significantly even 18 months after the breakdown of the cartel. However, it is not certain whether this observation was triggered by rising input prices or by tacit collusion. Similar suspicions are raised in Connor (2001) and de Roos (2006) for the lysine cartel. de Roos (2006) provides two potential explanations for the lack of post-cartel price reductions in the lysine industry, in which prices actually rose after the detection of the cartel. First, it could have been possible that the conspirators learnt enough about each others' behaviour through several years of explicit communication and cooperation that enabled them to collude tacitly. Knowing that communication to dissolve disputes was no longer possible after

[1] In the last 30 years, private damage litigation related to cartels grew significantly in the United States. Currently about 90% of all cartel court cases are based on private litigation representing an important source of cartel deterrence (Lande and Davis, 2008; Wils, 2003). A similar development is in process in Europe triggered by the European Commission's Directive on Antitrust Damage Actions (December 2014).

[2] The interested reader is referred to Figures 1 in Connor (1998) and Kovacic *et al.* (2007) for illustrative examples of suspected PCTC.

such a breakdown, the firms were particularly careful to prevent a price war. Second, it is also possible that the firms simply continued to set collusive prices to reduce fines to be paid under the U.S. antitrust sentencing guidelines (that refers to post-cartel prices to determine the cartel overcharge). Harrington (2004) shows that firms have the strategic interest to keep the prices high after cartel detection during litigation, such that overcharge estimates based on post-cartel prices underestimate the true harm caused by the cartel. Erutku (2012) provides empirical evidence in support of this idea. Ordóñez-de Haro and Torres (2014) examine the breakup of several Spanish food cartels that relied on the signals of trade associations. Significant levels of price hysteresis (i.e. prices remained high and were subject to a reduced variance) can be observed in most of the cartels after antitrust intervention. This evidence suggests that the firms may have continued to post prices based on past signals from their trade associations. Fonseca and Normann (2012) provide experimental evidence for the existence of tacit collusion after periods of explicit communication that suggests that the chance of PCTC to arise in industries as well as its magnitude are negatively correlated with the number of firms in the market. Similar findings are reported by Kovacic *et al.* (2007), who empirically study multiple markets that were engaged in the Vitamins cartels.[3]

Although these studies hypothesise the possible sources of PCTC, these hypotheses have never been formally tested. This lack of empirical evidence prevents tackling inappropriate overcharge estimates and the development of policies aimed at deterring PCTC. Therefore, the aim of this study is to focus on the possibility of tacit collusion to arise after periods of explicit communication,[4] and to shed light on the following research questions: (1) Is PCTC an abnormal phenomenon for a specific competition regime, or is its occurrence robust over various competition regimes such as the existence of antitrust fines, leniency programmes, etc.? (2) What are the determinants of PCTC? (3) What effects does PCTC have on attempts to estimate cartel overcharges? (4) Can any policy measures be implemented to deter PCTC?

To our knowledge, this is the first study to systematically investigate the driving factors and the related consequences of PCTC as well as possible preventive measures aimed against it. For this, we carry out a laboratory experiment that allows for an analysis of the marginal contribution of different market characteristics to tacit collusion in a controlled environment.[5] Lack of sufficient data prevents to carry out a similar exercise in the field. Our results show that PCTC is a robust phenomenon across competition regimes. Learning about other players' collusive types through successful cartel formation as well as collusive price hysteresis are found to be the main determinants of PCTC. Further, the downward bias in cartel damage estimates induced by

[3] Isaac and Walker (1988) are the first to test the effects of communication on coordination after communication is disallowed in public goods experiments. They find that preceding communication has a negative but diminishing effect on free-riding in periods without communication.

[4] Therefore, we are not interested in pure tacit collusion, i.e. collusion established without any communication (see, e.g. Ivaldi *et al.*, 2003; Martin, 2006).

[5] This prevents other factors that undermine the identification of the occurrence and sources of PCTC in the field such as unobserved input cost changes, additional signals by market participants, or unobserved changes in demand. Also, as fines in the experiment are fixed, (unlike firms in the field) subjects have no incentive to keep prices high after the end of explicit communication to reduce fines or damages in litigation to be paid.

PCTC increases with the preceding cartel success. Rematching subjects in the experiment is found to be a promising measure to prevent or reduce PCTC.

2 Sources of post-cartel tacit collusion

Although an important legal difference exists between explicit and tacit collusion, the standard theory of collusion does not differentiate between the two. Only recently have scholars begun to close this gap with theoretical models (Bos *et al.*, 2015; Harrington, 2012; Martin, 2006). An important function of communication in collusion is that it reduces uncertainty about present and past actions (Mouraviev, 2006). Such communication may either be explicit or implicit (e.g. setting high prices repeatedly to signal intentions to collude). Throughout this article, we refer to explicit communication as *communication*, and implicit communication as *price signalling*. Price signalling enables subjects to express their intention to collude by setting prices above the market level (see, e.g. Cason, 1995; Davis *et al.*, 2010). Although there may be other forms of implicit communication in the field, signalling with price choices is the only means to express intentions to collude apart from (explicit) communication in this study.

Despite the importance of communication for establishing collusion, its link to PCTC is not well understood. The observation that PCTC frequently arises after cartels have dissolved suggests that communication can have intertemporal effects on the strategic interaction of firms: Communication among rival firms might not only reduce uncertainty in the period in which it is used, but also in the periods afterwards. Hence, PCTC can be induced through two distinct channels.

First, former cartelists often abstain from price reductions in attempts to prevent triggering a price war (de Roos, 2006). An alternative explanation for a hysteresis effect of collusive actions is an intertemporal value of a preceding signal of collusion. In the absence of renewed communication, a previous period's outcome can provide a focal point for strategic choices. We refer to this effect determined by the two factors discussed as *collusive price hysteresis*. Prime examples for this source of tacit collusion are the Spanish food cartels observed by Ordóñez-de Haro and Torres (2014). Second, past experiences in periods with communication allow firms to learn about their competitors' "types" in terms of discount factors. This knowledge helps to sustain collusion by reducing the uncertainty about the other cartel members. Hence, given successful explicit collusion, the perceived profitability of playing collusive strategies in the post-cartel periods increases. We refer to this effect as *learning in cartels*. This argument is provided by de Roos (2006) as a possible explanation for the observed tacit collusion following the detection of the lysine cartel. More formally, deviation is an important source of risk to colluders that can only be observed *a posteriori*. A firm that considers collusion needs to form subjective beliefs about this risk and incorporate them into the decision problem. The observed history of play is important and shapes the subjective beliefs and therefore a firm's decisions. *Ceteris paribus*, firms with a longer history of successful collusion should assign a higher subjective probability to other firms' actions of continuing to abide to the collusive agreement. Such belief-updating as a reaction to risk has been studied theoretically in the context of tacit collusion by Harrington and Zhao (2012) and in generic multi-agent learning models (see, e.g. Foster and Young, 2003; Young, 2007). Thus, PCTC might be a function of the preceding cartel success, and markets colluding more successfully in the past are more likely to engage in and sustain PCTC.

Fonseca and Normann (2012) provide experimental evidence for the effect of communication on collusion after the end of communication They point out that the effect's magnitude depends on the number of firms in the market. In their experiment, the gains for firms are characterised by an inverted U-shaped curve and are highest for markets with four firms. Furthermore, they find that these gains diminish over time. Fonseca and Normann (2014) find a higher level of cartel recidivism for markets with four firms than with duopolies, as the four-firm-markets profit more from re-engaging in communication after the breakdown of collusion. These two studies are the only ones to provide experimental indications on PCTC. However, they focus on the link between tacit collusion and the number of firms in the market. Thus, they neither investigate the reasons for and consequences of PCTC nor strategies that can be used to prevent it.

3 Experiment

3.1 Experimental procedure

The experiment was conducted at the Centre for Behavioural and Experimental Social Science (CBESS) at the University of East Anglia, UK. It was programmed with z-Tree (Fischbacher, 2007) and the recruitment of subjects was done using ORSEE (Greiner, 2015). The subjects were allocated into groups of three and interacted with the same two other participants throughout the experiment (except for a treatment in which subjects at some pre-announced point in time are rematched into new groups). We recruited 228 students with no prior experience in oligopoly experiments. 36 subjects participated in each treatment to obtain 12 independent market observations.[6]

Subjects were randomly seated in the laboratory at the start of each session. Each participant received a printed copy of the instructions, which were also displayed on the computer screen and were read aloud by an experimenter at the beginning of the session. Questions about the instructions could be asked in private by subjects by raising their hands. The experiment was comprised of two parts. The first part consisted of a risk elicitation task (Holt and Laury, 2002), whereas the second part was the market game. In the market game, subjects interacted in markets for 20 (30 in one treatment) regular periods, i.e. periods that are played with certainty before a random stoppage rule applies. To prevent potential end-game effects and to reflect the infinitely repeated game with discounting, a random stopping rule in the spirit of Dal Bó

[6]42 subjects participated in the Fine and the Rematching treatments. Hence, each of them features 14 independent markets.

(2005) was implemented. After the end of the regular periods, in each period there was a 20% chance that the experiment ends. Subjects' understanding of the instructions was tested with a questionnaire, in which all values used in the questions were randomised across subjects to prevent example numbers to systematically influence decisions in the experiment.[7] An example of the instructions can be found in the Appendix in Section 6.2.

Sessions lasted between 25 and 50 minutes and subjects were allowed to participate in one session only. Earnings in part one were denoted in British Pounds, whereas earnings in the second part were labelled as "experimental points". Each experimental point gained in the market game was converted into £0.15 at the end of the experiment. Payments varied from £5.63 to £28.90 with a mean of £11.35.

3.2 Experimental design

In this experiment three subjects, each representing a firm in a market, engage in homogeneous goods price competition with perfectly inelastic unit demand from a computerised buyer. The demand structure is similar to that of Dufwenberg and Gneezy (2000), the oligopoly market design is similar to that of Gillet *et al.* (2011), and it is combined with a variation of the "Talk-NoTalk" design of Fonseca and Normann (2012). We implement a three firm homogeneous goods rather than a two firms differentiated goods market (e.g. Bigoni *et al.*, 2012), as this significantly reduces the complexity of the decision making process for subjects as well as the subjects' learning effects on outcomes. Finally, triopolies are used because previous studies find that three firms are sufficient to prevent significant levels of collusion without communication in markets with both price (Dufwenberg and Gneezy, 2000; Wellford, 2002) and quantity (Huck *et al.*, 2004) competition.

Unless stated otherwise, the market game consists of four stages. In the first stage, subjects need to decide whether they would like to attempt to reach a price agreement with the other subjects in the market. In the instructions, they are informed that in this part of the experiment they "... may decide to agree with the other firms to set the highest price of **102** and share the earnings". On the computer screen, subjects in this stage are asked "*Do you want to agree on prices?*" and need to click on option "Yes" to signal their intention to form a price agreement with the others. An agreement is only reached if all three subjects in the market confirm that they want to agree on prices. If it is reached, a message is displayed that all subjects agreed to set the price of 102. However, the agreement is non-binding, i.e. subjects are not required to follow the price agreement. In the second stage subjects are asked to make a price decision. Each subject can charge a price between 90 and 102 (integer values only) facing a cost of 90 if she sells the good and of 0 otherwise. Therefore, a subject i's profit equals $\pi_i = (P_i - 90)/\#Min$ if $P_i = Min(P_j)$, where P_j denotes all other j subjects' prices and $\#Min$ is the number of subjects charging the minimum price, and equals 0 otherwise. Thus, the demand is characterised by a computerised

[7] The result of the risk preference task was only announced at the end of the experiment. An anonymous questionnaire followed at the end of the experiment.

buyer that buys either 1 or 0 units from each subject depending on whether the subject sets the lowest price in that round.[8]

Subsequently, we refer to the price entered by subjects as the *asking price*, and to the lowest price in a market as the *market price*. There are several Nash equilibria in this framework. In one equilibrium two subjects charge 90 and the remaining subject charges any of the available prices including 90. Alternatively, all subjects charge 91. However, the latter is both the payoff-dominant equilibrium as well as the unique equilibrium in strategies that are not weakly dominated. In the third stage the subjects learn about each others' prices. In this stage, they also receive additional information and face further choices that are treatment-specific. In the last stage subjects learn their profits in that period. Then the next round of the market game starts, and the sequence of stages repeats.

Figure 1: Sequence of the market game

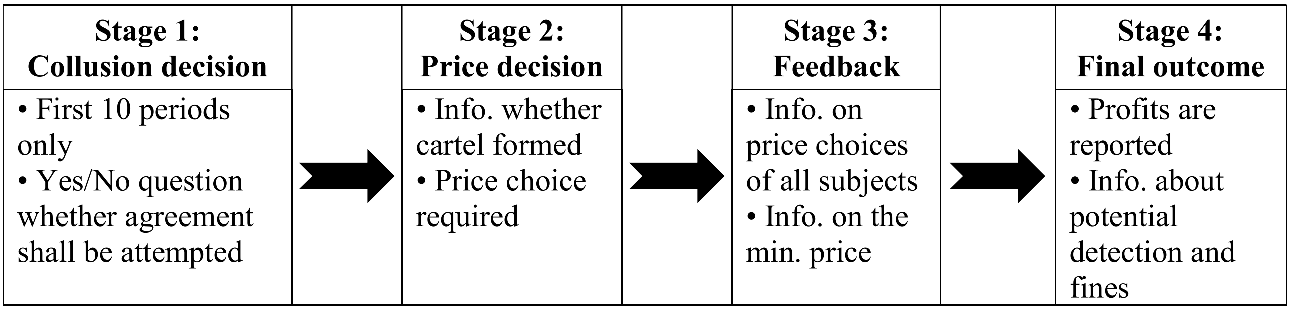


Figure 1 depicts the sequence of the experiment and shows the four stages as well as the main feedback provided to the subjects in each of them. In all treatments (except for the Baseline treatment introduced below), subjects were told in the instructions that they *may* agree on prices in the market game (i.e. the option of communication might or might not be given). Then they were allowed to communicate in the first 10 periods only – which we call the *Communication phase*. Then, without prior notice, the communication is disallowed for the rest of the game – which we call the *No Communication phase*. As such, while subjects know that they might be able to communicate with others with respect to price agreements, they also know that this option might not always be available.[9] The uncertainty with respect to the possibility to communicate ends at the beginning of period 11 when subjects are informed that from this point onward communication is not possible any more, and that previous agreements cannot be detected for the rest of the experiment. This design prevents strategic behaviour of subjects in the transition from explicit to tacit collusion and assures that no cheating is triggered by the anticipation of the end of communication in period 10. In one treatment (ExtComm), the Communication phase is preceded by 10 additional periods in which no communication is possible. An overview of the possibility to communicate in all treatments is provided in Table 1.

[8]E.g. if two subjects charge the lowest price of 92, they each earn a profit of (94-90)/2=2 experimental points and the third subject with a higher price earns a profit of 0.

[9]The word *may* is applied deliberately as it is defined as "used to express possibility". In contrast to other words such as *will* ("used to talk about what someone or something is able or willing to do" and *can* ("to be able to"), the word *may* does not imply that communication is always possible (Cambridge dictionary, available online at *http://dictionary.cambridge.org/*).

Table 1: Communication in treatments

Treatments	**No Communication phase**	**Communication phase**	**No Communication phase**
Baseline	-	×	×
Comm	-	✓	×
ExtComm	×	✓	×
Fine	-	✓	×
Leniency	-	✓	×
Rematching	-	✓	×
Periods	-9 to 0	1 to 10	11 to 20

Notes : A ✓ indicates that communication is possible in the time periods, and in periods denoted with × subjects cannot communicate. The dash (-) denotes that in all but the ExtComm treatment directly start with communication in the Communication phase.

Instead of implementing an exogenously given cutoff point for communication after 10 periods, an alternative design could have been to stop communication after the first incidence of cheating or detection in a market. We have decided against such a design for several reasons. First, both re-emergence of collusion after temporal breakdown as well as cartel recidivism are common observations in the field. Our design allows us to observe whether PCTC occurs despite both such forms of interruptions. Second, collusion in the lab has been noted to be very unstable, especially when it is not based on free-form communication. Removing the possibility to communicate after the first incidence of failure of collusion would therefore significantly limit the scope for learning. This would in turn undermine the analysis of learning, one of the main determinants mentioned in the literature. Third, our design provides a common cutoff point for all groups as well as all treatments. This greatly simplifies the analysis and allows through case-by-case comparisons for a clean identification of the sources of PCTC. In particular, it allows us to separate the effects of changing the expected length of interaction in the Rematching treatment introduced below from the effects of disrupting PCTC by ending the possibility to communicate.

We introduce the following treatments pertaining to our research questions:

Baseline: Subjects cannot communicate at any point and each round starts directly with the price decision in the *Baseline* treatment. It serves as the benchmark for tacit collusion that can be obtained without communication. Any difference in price levels between this and the other treatments in which subjects can communicate represents the effect of communication.

Comm: Subjects can agree on prices as described above for the first 10 periods during the Communication phase in the *Comm* treatment, but not afterwards in the No Communication phase. This is the equivalent of the relevant treatment in Fonseca and Normann (2012).

ExtComm: In this treatment, the Communication and No Communication phases of the Comm treatment are supplemented by 10 additional, initial periods without communication. Subsequently, we do not analyse these initial 10 periods (-9 to 0) but in line with the other treatments focus on the periods with and without communication that follow. This treatment is introduced to (i) reflect the situation that usually in markets communication occurs after the market is established and its growth rate starts to slow down, and (ii) to test whether experiencing competition before communication affects PCTC. Subjects can learn about the Nash equilibrium in the initial periods and revert back to it quickly after the end of communication. Furthermore, they might have a better understanding of the benefits of communication because of preceding exposure to low profits during competition.

Fine: The *Fine* treatment replicates the effect of an antitrust authority on illegal communication. Subjects face an exogenous detection probability of 16% if they agree to fix a price in the Communication phase. This probability is in the range of the estimated detection probabilities of cartels of between 13%-17% provided by Bryant and Eckard (1991). Detection is possible either in the period in which the agreement is formed or in subsequent periods provided that it has not been detected before. Detected subjects have to pay a fine of 5 experimental points irrespective of the number of agreements that they have reached before. Past agreements can only (jointly) be detected and fined once. Hence, additional fines are only possible if after detection subjects reach another agreement and are detected again.

Leniency: The *Leniency* treatment is an extension of the Fine treatment. It implements a leniency programme by offering subjects the option to report price agreements. This leads to the immediate detection and to fines of the other cartel members in return for a (partial or full) reduction of the fine for the reporter(s). If a cartel is formed in the same or a previous period and so far has remained undetected, subjects can report it after learning about each others' prices in Stage 3. Such a fine reduction procedure for leniency applications is standard in the experimental literature (Bigoni *et al.*, 2012; Hamaguchi *et al.*, 2009). If only one subject submits a leniency application, she is not fined but the other two subjects pay the full fine of 5 experimental points. Filing a leniency application incurs a cost of 1 experimental point for the applicant.[10] If two subjects submit leniency applications, both pay only half of the fine while the third pays the full fine. If all three subjects use the leniency scheme, they all pay 1/3 of the fine. A cartel is always detected if at least one leniency application is submitted, but subjects are not informed whether the detection occurred due to the exogenous detection probability or because of a leniency application.[11]

Rematching: The *Rematching* treatment introduces a mechanism aimed at disrupting PCTC

[10] The parameters are chosen such that the incentive compatibility constraints (ICC) for the infinitely repeated games that characterise the incentives to collude in the Fine and in the Leniency treatments are similar (given collusion on the price of 102, the critical discount factors necessary to support collusion are approximately 0.66 and 0.68, respectively, if only one subject deviates to price 101 and is the only one to submit a leniency application).

[11] Leniency has been a successful tool to deter cartels in the field, and is well analysed in the experimental literature. Apesteguia *et al.* (2007) were the first to examine the effects of leniency programmes in a one-shot setting. Hinloopen and Soetevent (2008) extend this in many different dimensions and establish the effectiveness of such programmes. Several further studies such as Hamaguchi *et al.* (2009), Bigoni *et al.* (2012), Chowdhury and Wandschneider (2018) study various additional aspects such as group size, anonymous reporting and fine levels with respect to their interactions with leniency programmes. A detailed survey on this area can be found in Marvao and Spagnolo (2018).

by targeting its source of learning. Similar to the Comm treatment, here each subject starts in a group with two other subjects but they are informed that they will be rematched with two new randomly chosen subjects at some point in the experiment. The point in which they are rematched is not revealed beforehand; it is announced immediately before the rematching is carried out. The rematching takes place at the beginning of period 11, in which communication ends as well. This ensures that subjects cannot learn about the types of the new group members. Hence, any change in behaviour observed in this treatment from period 11 onwards compared to the Comm and ExtComm treatments comes from the disruption of the effects of learning. Further, from a supergame perspective, this should yield lower rates of cooperation by reducing the horizon for cooperation itself. The uncertainty due to different expectations of the duration of cooperation in the supergame may also destabilise collusion.

This treatment is novel to the literature. The mechanism in Rematching replicates one of the indirect enforcement effects that (criminal) sanctions against managers involved in cartels have on PCTC. Sanctions against cartel managers in the form of imprisonment or debarment, i.e. disqualification from taking up managing positions in the same or similar industries after conviction, remove convicted managers from the market. Examples for regimes with imprisonment of cartel managers are the United States and Canada. Examples for the regimes of debarment are the United Kingdom, the Czech Republic and Hungary. We regard debarment to feature different direct and indirect effects with respect to deterrence. Financial sanctions, reputational damages or opportunity costs (e.g. temporary unemployment) affecting monetary incentives are direct effects that we do not capture in this experiment. Yet, debarment also shortens the expected length of interaction if subjects in the lab (or managers in the field) are not agnostic about future periods and interaction, because it occurs with a positive probability and prevents explicit or tacit collusion in subsequent periods. Market contact is characterised by repeated interactions such that any communication and actions can have intertemporal informational value and effects, as our analysis below shows. Debarment renders the decision of sending an illegal signal to establish or sustain collusion less profitable because the expected length of benefits gained by collusion is shortened, whereas the expected punishment for such communication remains unchanged (or is increased if the direct effects are taken into account). Another indirect effect of rematching subjects in our experiment is that it eliminates any knowledge about the strategies and likely actions of the other subjects, as is the likely effect of removing key managers involved in operating a cartel in the field. Hence, we regard this mechanism as a preventive measure against PCTC, because it is likely to reduce expected profitability of explicit collusion and disrupts tacit collusion that occurs nonetheless.

4 Results

We present the results of the experiment as follows. First, we carry out a descriptive analysis of the data with respect to the occurrence of PCTC across competitive regimes and its sources. Econometric estimates are then used to formally test and establish the sources of PCTC. Second, we use different approaches to show how PCTC leads to a downward bias in cartel overcharge estimates. Third, we test whether rematching subjects after the end of communication can deter PCTC.

In the analysis up to the regression analysis, all observations after the 20^{th} period are excluded from the analysis to prevent unequal sampling of the groups (towards the end of the experiment) to influence the results.[12] We distinguish between the asking and market prices as defined in the previous section. The market price serves the whole market in homogeneous goods price competition and is the relevant market outcome from a welfare perspective. The asking price captures additional information such as price signalling or failed attempts to collude. This is in particular important for periods without communication because subjects can signal their intentions to establish collusion by deviating from the Nash equilibrium and setting a price of 102.

4.1 Sources of post-cartel tacit collusion

As a first step, we test the existence and determinants of PCTC across the treatments that approximate various competition regimes. Table 2 contains the average absolute margins (Average price − 90) for both the asking and market prices separated by treatment in the Communication and No Communication phases. For Baseline, we include periods 1-10 and 11-20 into the Communication and the No Communication phases throughout the analysis, respectively.[13] As the market prices are the market-clearing prices, they are at least as low as the asking prices in all treatments. Based on the magnitude of price margins, the ranking of treatments with respect to asking prices in the Communication phase is as follows: ExtComm features the highest price margins followed by Comm, Rematching, Leniency, Fine, and Baseline.

Table 2: Asking and market price margins by communication possibility

	Communication phase						No Communication phase					
	Asking prices			**Market prices**			**Asking prices**			**Market prices**		
	Obs.	Mean	Std. dev.	Obs.	Mean	Std. dev.	Obs.	Mean	Std. dev.	Obs.	Mean	Std. dev.
Baseline	360	3.328	3.324	120	1.925	2.338	360	3.436	3.600	120	2.125	2.410
Comm	360	7.744	4.816	120	5.958	5.004	360	6.925	4.968	120	5.725	5.042
Fine	360	4.978	4.019	120	3.508	3.498	360	4.206	4.229	120	3.042	3.487
Leniency	420	5.276	4.784	140	3.429	4.125	420	4.595	4.699	140	3.021	3.888
ExtComm	360	8.078	4.769	120	6.533	5.002	360	5.817	4.979	120	4.667	4.731
Rematching	420	6.874	4.730	140	4.507	4.365	420	5.238	4.725	140	2.557	3.232

This ranking coincides with the number of markets successfully engaged in collusion in the Communication phase. Successful collusion, i.e. a cartel is formed and all subjects abide to the agreement in a period, occurs at least once in 7 markets in ExtComm, 6 in Comm, 5 in Rematching, 4 in Leniency, and 2 in Fine out of 12 markets (14 in Leniency). This link between

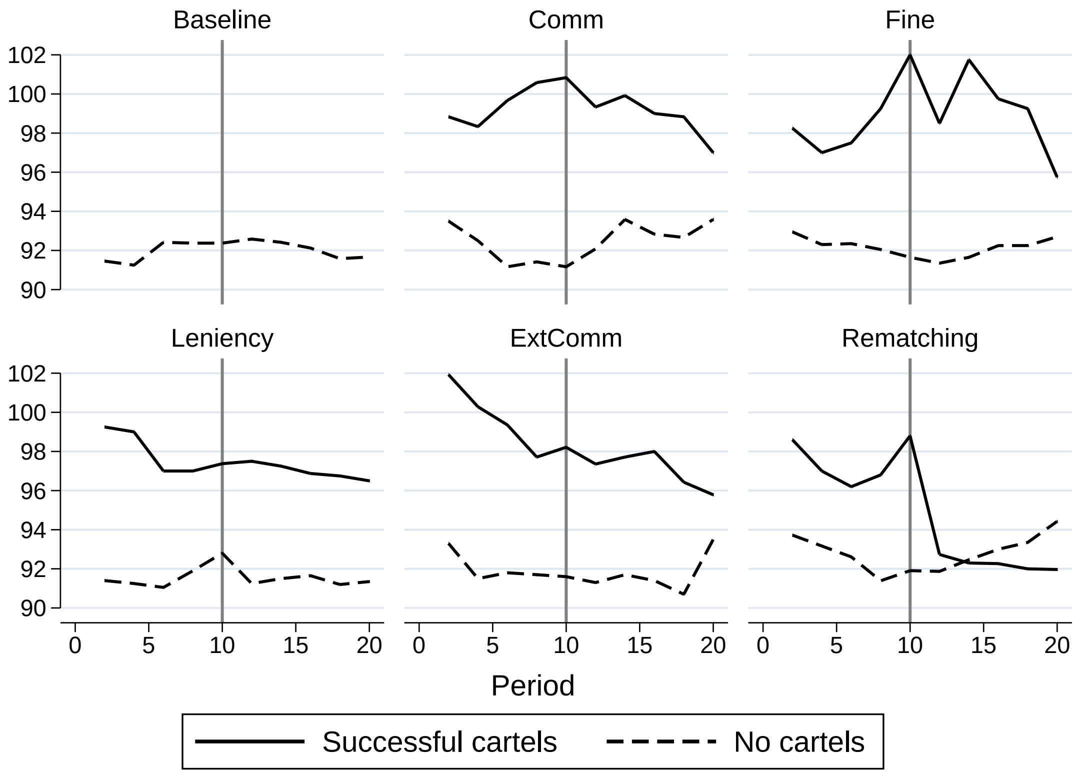
[12] For the rest of the analysis, we disregard periods -9 to 0 in the ExtComm treatment. Given the random stoppage-rule, actual termination varies between the 20th and the 25th period across sessions.

[13] The results are robust to comparing observations 1-10 from the Baseline treatment into the No Communication phase for comparison with the other treatments.

price agreements and asking and market price margins reiterates the importance of successful collusion for generating supernormal profits.

Comparing prices between the Communication and the No Communication phases in Table 2 shows a strong relationship of price margins across the two phases. Price margins in the No Communication phase are significantly higher in the treatments with communication compared to Baseline and the order of treatments remains the same apart from Rematching. The price margin in the No Communication phase relative to Baseline is an indicator of PCTC, as it is enabled by subjects' preceding ability to communicate. Therefore, the occurrence and magnitude of PCTC appears to be related with successful collusion in the Communication phase. Such a relation does not appear to exist in the Rematching treatment. Whereas market prices in Rematching are close to those of Comm and ExtComm in the Communication phase, they are subject to a significant decline in the No Communication phase and then are very close to Baseline. Thus, unlike in the other treatments, PCTC appears to be absent in Rematching. This provides the first evidence of the disruptive effect of rematching on collusion by eliminating learning that apparently drives PCTC.

Figure 2: Market prices by preceding cartel success



As the experimental design provides repeated time series of groups with fixed group composition (except for the Rematching treatment), one can plot these time series or aggregated sub-samples thereof to inspect the dynamics of play. The link between PCTC and successful collusion in the Communication phase becomes clearer if it is distinguished between the markets in which price agreements were successfully implemented and those in which no such successful collusion occurred. In Figure 2, the averages of two sub-samples of the group-level market price time series by treatment are plotted. The sub-sample "Successful cartels" contains the average market prices for those groups which successfully established a market price of 102 based on a price agreement *at least once* in the Communication phase. The sub-sample "No cartels" contains all other groups, i.e. those in which the subjects did not manage once to reach a market price of 102 based on a price agreement. The vertical grey line marks the last period of communication, and market prices are averaged over two periods.

A fixed assignment over time into the two sub-samples according to the occurrence of any successful collusion in the first 10 periods prevents changing sub-sample compositions from affecting the results.[14] Note that the particular shape of the price paths in the treatments should be interpreted with care, as only 2 and 4 cartels are formed in Fine and Leniency, respectively. Yet, while the limited data does not allow to assess whether PCTC occurs to a larger or smaller extent in Fine and Leniency compared to Comm and ExtComm, Figure 2 shows clearly that PCTC does occur in all four treatments. Subjects successfully forming a cartel are able to charge higher prices throughout the experiment in all four treatments.

For the Rematching treatment, market prices in the No Communication phase are separated between subjects previously engaging in successful collusion and those who did not. Note that market prices in Rematching immediately collapse in the No Communication phase after subjects are matched into new groups. This sudden decline in market prices does not occur in the other treatments with communication. This suggests that the positive effect of communication on PCTC is removed in the Rematching treatment.[15]

We turn to regression analysis to formally test the observations in the No Communication phase regarding the sources of PCTC and its absence in the Rematching treatment. This analysis allows us to distinguish the sources, control for the dynamics, and run analyses that capture the marginal contributions of different key determinants of prices. Asking and market prices in the No Communication phase are regressed on other market outcomes and the results are reported in Table 3. To distinguish PCTC from any tacit collusion that is established by price signalling only, we include proxy variables aimed at capturing the sources of PCTC, i.e. learning in cartels and collusive price hysteresis. The small numbers of cartels in Fine and Leniency do not allow for producing reliable treatment-specific estimates. Hence, we pool them with Comm and ExtComm to estimate averages of effects of our variables of interest among these treatments.[16] Results based on all treatments excluding Rematching are presented in Columns I to IV. We

[14]Yet, this creates some fuzziness in the plots in periods 1-10. E.g. the market prices of a group that only colludes successfully from period 4 onwards and features prices of 91 in periods 1 to 3 enter the "Successful cartels" sub-sample time series from period 1. However, as can be seen, the effect on sub-sample averages in periods 1 to 10 are very limited only. Furthermore, the focus of the Figure lies on periods 11-20, which are unaffected by it.

[15]As we argue below, market prices recover during the end of the experiment in the Rematching treatment because of an increased stability of tacit collusion in the Rematching treatment compared to Baseline. We attribute this stability to the subjects' preceding experience that a return to collusion after deviation is hard to achieve after the possibility of communication ceases to exist.

[16]If the effects are not identical across the treatments, our approach yields averages more heavily driven by Comm and ExtComm compared Fine and Leniency, because the former two feature 13 successful cartels compared to 6 in the latter two.

analyse the Rematching treatment separately due to the potentially very different dynamics of play and nature of tacit collusion in this treatment and report the results in Columns V to VIII.

In both instances, we run the regressions both at the level of asking (Columns I, III, V and VII) and market prices (Columns II, IV, VI, VIII). Whereas asking prices capture pricing behaviour at the subject level such as price signalling, market prices represent market outcome relevant form a welfare perspective. Further, we divide the data in both instances into two sub-samples. As Figure 2 shows, PCTC declines over time suggesting that the effect of learning in cartels and collusive price hysteresis might be unstable over time.[17] To model this decline without imposing particular parametric assumptions about it, we run the regressions in Columns I-II and V-VI based on periods 11 to 15 and periods 16 to the end in Columns III-IV and VII-VIII.

The regression analysis is based on multi-level hybrid models (Allison, 2009; Bell *et al.*, 2019).[18] Random assignment of subjects across groups and treatments ensures that a subject's or group's decision to engage in collusion or to charge specific prices is not subject to self-selection bias that would require corrections such as with two-stage models. The hybrid model combines advantages of fixed effects and random effects models. They allow to estimate the effects of time-invariant variables while relaxing some of the assumptions of random effects models. The multi-level hybrid model takes into account the nested panel data structure of the data, in which there are repeated observations over time (level 1) of subjects (level 2) in fixed groups of three representing markets (level 3). As such, it features time-varying independent variables at subject and market level X_{ikt} (level 1) and time-invariant variables that vary across subjects and markets. It can, e.g. for the regression in Column I, be expressed as

$$P_{ikt} = \beta_0 + \beta_1 \left(X_{ikt} - \overline{X}_{ik}\right) + \beta_2 Y_{ik} + \beta_3 Z_k + \beta_4 \overline{X}_{ik} + u_{ik} + s_k + \epsilon_{ikt} \quad (1)$$

where P_{ikt} represents the asking price of subject i in market k at time t. The hybrid model decomposes the estimated effects of time-variant level 1 variables X_{ikt} into their group-centred means $\overline{X}_{ik}$ and deviations from these group-centred means $\left(X_{ikt} - \overline{X}_{ik}\right)$. As a result, the regression coefficients β_1 and β_4 capture the isolated within and between-group centred means. No such decomposition is possible for time-invariant variables at the level of subjects Y_{ik} (level 2) or markets Z_k (level 3), for which the corresponding coefficients β_2 and β_3 capture between effects. u_{ik} and s_k represent random intercepts at the subject and the market-levels, whereas ϵ_{ikt} is the error term relying on most of the usual RE assumptions. However, no bias can arise for level 1 coefficients due to omitted variables at levels 2 and 3, because such correlations are absorbed in the between effects. The above specification is used at the subject-level regressions based on asking prices, and in the market level regressions no random intercepts are included at the subject-level.

[17]This is in line with the experimental findings of Fonseca and Normann (2014), who show that hysteresis effects are unstable over time and require renewed communication depending on the number of firms in the market.

[18]Hybrid models are also sometimes referred to as within-between random effects (REWB) models.

We include the following independent variables in the regressions.[19] *Lag price* represents a subject's own asking price or the market price from the previous period depending on the dependent variable. It captures hysteresis in price-setting behaviour of subjects. *Max price others* and *Min price others* contain the higher and lower of the other two subjects' asking prices in the previous period and are included in the asking price regressions only.[20] We use two different variables to measure the effect of preceding collusion on pricing. *Lag tacit collusion* is an indicator variable that takes the value 1 if all three subjects charged the collusive price of 102 in the previous period and is 0 otherwise. It serves as a proxy variable for collusive price hysteresis. It captures an overall effect of the different channels determining the hysteresis, which we cannot differentiate in our analysis. *No. of successful cartel periods* contains the market's number of periods of successful cartelisation (i.e. all subjects agreed to fix prices and did not cheat) in the preceding Communication phase.[21] It approximates the effect of preceding cartel success on PCTC and corresponds to the effect of learning in cartels on subsequent tacit collusion. In the Rematching treatment, the coefficient of *No. of successful cartel periods* shows whether a subject's intention to establish collusion with price signalling is driven by preceding experience of collusion in the Communication phase. Our rematching procedure allows us to observe how subjects with a history of engaging in collusion behave in a new market environment. For this treatment, the coefficient captures the average collusive experience in the new market and shows how price signalling triggered by former collusion contributes to market prices. The variable *Period* captures potential time trends. *Comm*, *Fine*, *Leniency*, and *ExtComm* are treatment indicators, with the Baseline treatment being the baseline category in the regressions in Columns I-IV. Therefore, the treatment dummies control for any treatment-specific effects on PCTC that are not captured by any of the other included regressors. *Lag Price*, *Max price others*, *Min price others* and *Lag tacit collusion* vary over time at level 1, such that for these variables following the model in Eq. 1 are decomposed into deviations from the subject/market-centered as indicated by the Δ symbol and group-centered means indicated with the $\oslash$ symbol.[22]

The results for all treatments except Rematching in Columns I to IV provide strong evidence that collusion in the preceding period has a significant positive effect on price choices both within and across subjects. This suggests that PCTC is indeed partly caused by collusive price hysteresis. Strikingly, this hysteresis effect is stable over time and does not only occur immediately after the end of communication, but also at later periods of time. A different picture emerges for *No. of successful cartel periods*. We find evidence for learning in cartels driving PCTC prices both

[19]Inclusion of lags of the dependent and independent variables yields the autoregressive distributed-lag model, which is a widespread model in applied econometrics to model dynamics in time series and panel data (Banerjee *et al.*, 1990; Pesaran and Shin, 1998).

[20]If both competitors set the same asking price, both variables contain that price.

[21]In the Rematching treatment, the three subjects in a market in the No Communication phase come from markets with different histories of collusion. Therefore, in this treatment we use the average value of the variable across the three markets that the subjects come from. This allows us to control for the effect of the average level of preceding experience of successful collusion of subjects on PCTC after rematching.

[22]For the sake of a concise model, we did not include subject characteristics such as their risk attitude into the model presented here. These characteristics were neither significant in the model nor did they affect the other results. We interpret this to suggest that subjects' actions are strategic (Dreber *et al.*, 2014).

Table 3: Prices in the No Communication phase – Multi-level hybrid model

	I	II	III	IV	V	VI	VII	VIII
	All treatments except Rematching				**Rematching treatment**			
	Periods 11-15		**Periods 16-end**		**Periods 11-15**		**Periods 16-end**	
	AP	**MP**	**AP**	**MP**	**AP**	**MP**	**AP**	**MP**
Coefficients								
Δ Lag price	0.222***	0.345***	0.385***	0.367***	0.332***	0.209	0.298***	0.099
	(0.061)	(0.076)	(0.043)	(0.068)	(0.058)	(0.175)	(0.055)	(0.089)
Δ Max price others	0.094***	–	0.110***	–	0.025	–	0.050	–
	(0.035)		(0.036)		(0.050)		(0.060)	
Δ Min price others	−0.032	–	0.074**	–	0.059	–	0.011	–
	(0.044)		(0.037)		(0.157)		(0.072)	
Δ Lag tacit collusion	2.369***	2.431***	3.023***	4.232***	−1.163	0.099	4.032***	8.727***
	(0.762)	(0.876)	(0.385)	(0.699)	(1.621)	(1.512)	(1.156)	(1.031)
No. of successful cartel periods	0.120**	0.133**	−0.073***	−0.096**	0.059	0.151	−0.113	−0.162
	(0.058)	(0.060)	(0.027)	(0.042)	(0.279)	(0.257)	(0.139)	(0.137)
Period	0.107	0.151**	−0.008	−0.024	−0.233	0.122	0.020	−0.098
	(0.094)	(0.074)	(0.019)	(0.018)	(0.224)	(0.235)	(0.092)	(0.075)
Comm	0.246	0.629**	−0.446**	−0.666***	–	–	–	–
	(0.315)	(0.314)	(0.198)	(0.213)				
Fine	0.192	0.248	0.058	−0.064	–	–	–	–
	(0.261)	(0.224)	(0.238)	(0.259)				
Leniency	0.247	0.258	−0.066	−0.143	–	–	–	–
	(0.252)	(0.193)	(0.138)	(0.115)				
ExtComm	−0.022	0.643**	0.366***	0.124	–	–	–	–
	(0.373)	(0.280)	(0.123)	(0.108)				
⊘ Lag price	0.994***	0.832***	1.042***	1.076***	1.060***	1.042*	1.009***	1.099***
	(0.072)	(0.074)	(0.040)	(0.058)	(0.189)	(0.612)	(0.129)	(0.297)
⊘ Lag tacit collusion	−1.009	0.765	0.263	−0.396	−2.626	−5.873	0.553	−0.313
	(0.987)	(0.831)	(0.599)	(0.936)	(6.827)	(8.491)	(4.636)	(3.847)
⊘ Max price others	0.110	–	−0.037	–	−0.359*	–	0.281**	–
	(0.067)		(0.039)		(0.202)		(0.120)	
⊘ Min price others	−0.080	–	0.001	–	0.374	–	−0.306	–
	(0.170)		(0.073)		(0.381)		(0.277)	
Constant	−4.147	13.004*	−0.386	−6.326	−2.400	−5.336	−0.029	−7.210
	(7.703)	(6.850)	(4.119)	(5.292)	(21.834)	(55.352)	(15.850)	(26.594)
Random intercepts								
u_{ik}	−0.839**	−24.404	−16.206	−23.966	−2.238	−25.834***	−25.571***	−29.483***
	(0.379)	(41.203)	(30.584)	(27.966)	(9.889)	(9.536)	(5.502)	(4.618)
s_k	−13.932	–	−16.130	–	−14.077***	–	−26.958***	–
	(38.878)		(39.588)		(4.018)		(3.205)	
ϵ_{ikt}	0.881***	0.420***	0.937***	0.524***	1.211***	0.692**	1.202***	0.651***
	(0.072)	(0.131)	(0.065)	(0.102)	(0.083)	(0.276)	(0.087)	(0.242)
Observations	858	286	2094	698	210	70	348	116

*Notes: Significant at * 10% level, ** 5% level, *** 1% level. Displays coefficients without and standard errors within brackets. Baseline serves as the Baseline treatment in AP columns. Cluster and autocorrelation-robust standard errors are used in Columns I-IV, in Columns V-VIII they are based on pairs cluster bootstraps with 500 iterations to take into account small cluster sizes of 12 groups in these cases. Random intercepts are included at the subject level in AP columns, and at the market level in all columns.*

at the subject and market levels. For example, a market that featured 10 periods of successful cartel periods in the Communication phase on average featured market prices increased by 1.33 experimental points in periods 11 to 15. However, this positive effect declines over time and the effect is even negative in later periods. While it is unclear why market prices are significantly lower for previously more collusive markets, a potential explanation could be that such markets could feature more severe punishment phases after breakdown. Alternatively, such a finding could result if grim-trigger punishment strategies are more likely in groups in which subjects are more likely to engage in collusion.

Result 1: PCTC is determined by both collusive price hysteresis and learning.

A similar initially positive but declining effect over time is observed for the Comm and ExtComm treatments. This suggests that non-successful attempts to collude supported by comparably higher discount factors for collusion lead to less fierce competition in these treatments. A a result, prices are initially higher in affected markets after the end of the communication periods. In later periods, however, this effect dissipates leading to relative decline in prices compared to the other treatments.

Turning to the Rematching treatment in Columns V-VIII, a large and positive coefficient of *Lag tacit collusion* suggests that the effect of tacit collusion is more pronounced after rematching. It may be because subjects are aware that re-establishing collusion after cheating is harder to achieve without communication. However, as collusion on price 102 only arises in about 6% of the observations in the No Communication phase under Rematching, the magnitude of the coefficient might be overstated due to unrepresentative outliers. The fact that no similar effect can be found in periods 11-15 implies that it could be an outcome generated by pure tacit collusion that is not affected by previous communication, i.e. it does not represent PCTC. In addition, while large in magnitude, the overall welfare effects might be limited due to the small number of cases in which tacit collusion occurs in this treatment. In Rematching, the coefficient of *No. of successful cartel periods* is insignificant and implies that the positive effect of learning about the previous partners' collusive types on PCTC is eliminated by being rematched with other subjects. This is consistent with the idea that the information obtained with past successful collusion about competitors becomes irrelevant due to a change in group composition. Therefore, the regression results are consistent with the descriptive analysis above that suggests that PCTC cannot be observed in Rematching.

Result 2: PCTC is absent in Rematching.

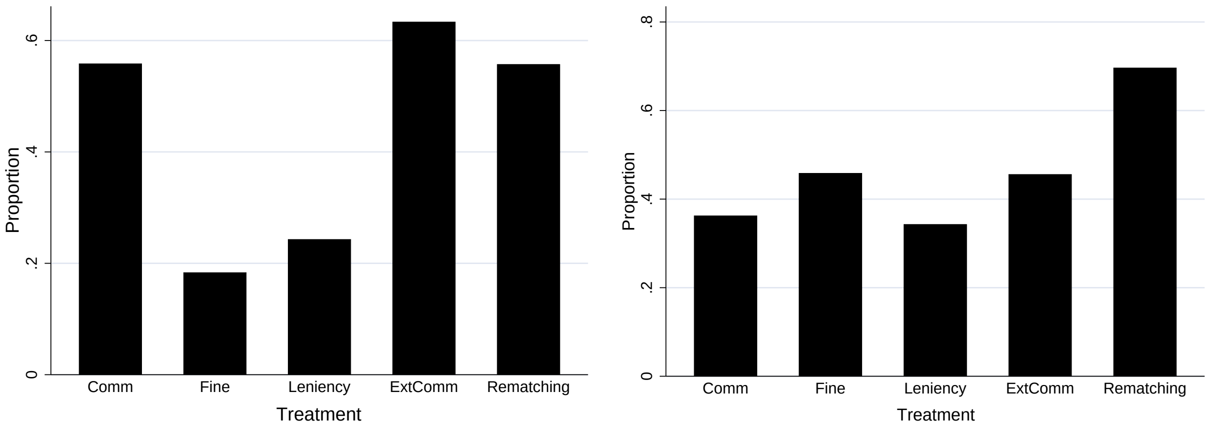
The results confirm the concerns raised in the literature about the use of post-cartel observations in empirical methods to estimate cartel damage, which can yield underestimated cartel overcharges. An insight for this literature arising from our analysis is that in fact particularly successful cartels might be subject to the biggest overcharge bias. We discuss this issue in further detail in Appendix 6.1.

4.2 The impact of re-matching on explicit collusion

Next, we focus on the effects of rematching on the performance and stability of cartels. The absolute price margin based on market prices in the Communication phase in the Rematching treatment appears to be lower than in the Communication treatment (4.507 vs. 5.958; Table 2) for markets with at least one successful cartel period. As the two treatments are identical in the Communication phase aside from the announcement of future rematching of the groups in the Rematching treatment, we can attribute the lower market prices in Rematching to a negative effect of anticipated rematching on collusion.

To determine how rematching affects cartels, we compare the incidence of collusion and cheating

Figure 3: Incidence of cartelisation and cheating in the Communication stage



(a) Proportion of markets colluding

(b) Proportion of markets with cheating

conditional on collusion in the Communication phase between the treatments. Figures 3a and 3b show differences in the proportions of markets with price agreements (irrespective of whether they are successful or whether cheating or occurs) and with cheating conditional on existing agreements, respectively.[23] We define cheating as any subject's decision to charge a price below 102 when either an agreement was reached in the same period or a previous periods' agreement has not yet been undercut by any other subject. Thus, a higher level of cheating shows a lower level of stability of cartels.

In line with the literature, Fine and Leniency feature lower levels of collusion by rendering collusion less attractive, although collusion and cheating are not much different between the two treatments. Rematching does not reduce attempts to collude (as cartel formation is unchanged) in the Rematching treatment compared to the Comm treatment (a two-sample t-test testing for differences in the proportion of the subjects colluding in Comm and Rematching reports a p-value of 0.497). Yet, the incidence of cheating in the Rematching treatment is higher than in the Comm treatment. A two-sample t-test comparing cheating between the Comm and the Rematching treatments shows a weakly significant difference between the treatments (p-value = 0.058).[24] Thus, rematching does not reduce attempts to collude, but it significantly increases the incidence of cheating. This destabilising effect is very pronounced with the proportion of firms cheating rising from 36.2% in Comm to 69.6% in Rematching.

> **Result 3**: Rematching reduces explicit collusion through its negative effect on cartel stability.

[23]Attempts to collude that fail are implied by the difference between agreements that were reached and the number of observed cases of cheating.

[24]The t-tests use cluster- and autocorrelation-robust standard errors based on pairs cluster bootstrapping with 500 iterations and compare the incidence of collusion and cheating at a market and period level. They are derived from a linear probability model. The t-tests are preferred here to Mann Whitney U tests, as the latter cannot take sample weights into account. As different markets engage to different extents in collusion and cheating, markets more active in collusion and cheating are more informative. Using this information with weighting leads to efficiency gains of the test statistic compared to non-parametric tests.

5 Conclusion

Although it is a conventional wisdom that firms may resort to tacit collusion after a cartel breaks down, little is known about the conditions under which this happens and about the determinants that drive the level and persistence of such behaviour. As a result, it is hard to assess implications of such firm behaviour for competition policy and how to counteract PCTC. Given the importance of PCTC for deterrent fines, welfare effects of cartels, and the right design of antitrust legislation, this study aims at adding to the knowledge on the existence, determinants, consequences, and prevention of PCTC.

We run experiments in which groups of three firms, each controlled by a subject, compete in homogeneous goods price competition and can establish price agreements. These price agreements can be renewed in the following periods or remain active absent new agreements that were neither detected nor cheated upon. After this initial phase of communication, the ability to agree on price-fixing ends and subjects are only able to collude tacitly. Such an approach contributes to our understanding on how cartels react to detection when continued communication is deemed too risky. We test the existence of PCTC in different competition regimes to establish whether it is a common phenomenon unrelated to a particular policy tool. Conducting an econometric analysis we study the different sources of PCTC. We then show how under PCTC the standard procedures to estimate cartel damages may be biased and test the use of rematching to disrupt the positive effects of learning on PCTC.

The results suggest that firms are able to profit frequently from PCTC irrespective of different antitrust laws. We identify two sources of PCTC: collusive price hysteresis and learning in cartels. The former describes a firm's strategy to continue charging preceding cartel prices after the end of the cartel in order to avoid triggering a price war or collusive focal point pricing in the absence of renewed communication. The latter describes how communication and a cooperative history facilitate PCTC by reducing uncertainty about the actions of the other firms. Moreover, the magnitude of PCTC is positively linked to preceding cartel success. In line with Bigoni *et al.* (2015), this stresses the importance of beliefs for successful collusion in infinitely repeated games. Rematching in the experiment is found to be an effective mechanism to prevent PCTC as well as to reduce cartel stability. The Rematching treatment emulates one indirect enforcement effect that debarment (disqualification orders for convicted cartel managers and imprisonment) has on collusion. Note, however, that we do not fully replicate such mechanisms. Our focus is on the indirect enforcement effects of sanctions against managers. As such, stronger (deterrent) effects are likely to arise if the *direct enforcement effects* linked to these punishment mechanisms were modelled as well.

Several implications arise from our analyses. Antitrust laws that reduce the formation and stability of cartels lessen the negative welfare effects of PCTC, as the incidence of tacit collusion is linked to the preceding cartelisation of the industry. Cartels that do not break down due to cheating but are detected exogenously might realise supercompetitive profits long after the end of communication. Therefore, competition agencies should rely on leniency programmes to reduce cartel formation as much as possible to reduce the negative welfare effects of PCTC on top of the standard benefits of leniency programmes.

In addition, provided that debarment programmes and imprisonment have similar disruptive indirect enforcement effects on collusion in the field as indicated by the Rematching treatment in the lab, these policy tools may help to minimise the harm caused by PCTC. Note that our implementation of a debarment focuses on some indirect effects only, such that the effect of these programmes with direct monetary or reputational punishment (and in conjunction with fines and leniency programmes) could be even more effective. Debarment of managers so far has been limited to few countries such as the USA, UK, Sweden, and Slovenia (Ginsburg and Wright, 2010). Our results suggest that this policy tool might offer the potential to reduce the damage caused by cartels in other ways than the direct effect on individuals that has been discussed in the literature, and should receive greater attention. As such, other actions aimed at disrupting PCTC could improve welfare as well. Similar policy interventions could aim at facilitating practices used to support collusion, such as attempts to improve market transparency. An example of such interventions could be the extensive prohibition for trade associations in formerly cartelised industries to gather and report statistical data on relevant market outcomes. Finally, our analyses suggest that post-cartel prices should be used with caution as competitive counterfactuals to determine cartel overcharges as this creates the risk of a downward bias in these estimates that increases with preceding cartel success. As such, the most harmful cartels could be those deterred the least in instances in which (a substantial part of) the fines are based on such overcharge estimates.

There are several ways to extend our analysis. We focus on learning as a source of PCTC abstracting from focal points in the spirit of Scherer (1967) as a source of collusion. After rematching, subjects could try to establish tacit collusion by setting the price last charged in markets in previous periods with collusion. A limitation of this study's stylised design is its inability to assess under which circumstances PCTC is more likely to arise or to last. Therefore, the effects of the variations of market characteristics including firm numbers and product differentiation or incomplete cartels on PCTC and its identified sources should be studied. These factors should render PCTC more difficult to sustain by reducing transparency or increasing the burden to establish a consensus among market participants. This could shed further light on the instances in which PCTC is likely to be of concern in the field. Furthermore, the limited sample size in our study did not allow us to test for potential differences in the magnitude of the effects of the sources of PCTC among different antitrust regimes. Finally, the complete effects of debarment are not tested experimentally because our implementation only captures an indirect effect on uncertainty but not direct monetary punishments of individuals.

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6 Appendix

6.1 Implications for cartel overcharge estimations

We use the "before-after approach" to calculate the damages caused by all cartels formed in the experiment and study the relationship between preceding cartel success and overcharge bias. This estimator is one of the most common methods used in the field to estimate cartel overcharges. The cartel overcharge is calculated by comparing the price during the cartel with a counterfactual price under competition from a benchmark period. Three different variants of this approach are commonly used (see, e.g. Baker and Rubinfeld, 1999; Davis *et al.*, 2010). *Pre-Cartel* denotes the overcharge estimate that compares the price during periods of cartelisation to a price benchmark based on prices before the cartel. *Post-Cartel* denotes the estimate based on post-cartel prices serving as benchmark prices, and *Whole sample* uses prices both before and after a cartel as the counterfactual for competition.

As we observe pre-communication prices only for the Baseline and ExtComm treatments, we use the average market price of the ExtComm treatment observations from periods -9 to 0 as the benchmark of competition for all treatments.[25] To calculate the overcharges, a reasonable assessment has to be made about the periods that should be regarded as cartel periods. In the Comm, ExtComm, and Rematching treatments we include only those periods in which subjects communicate and reach a price-fixing agreement as cartel periods.[26] Fine and Leniency feature periods in which either a cartel forms or a previous cartel is undetected in the Communication phase. These differences in the composition of cartel periods reflect the underlying differences in incentives for cartel formation and pricing. Given that detection is possible in Fine and Leniency even if no cartel is formed in a certain period but a previous price agreement so far has remained undetected, subject behaviour might be affected by the chance of detection.

Table 4 reports the average of the estimated cartel overcharges using the different benchmark prices in the first three columns by treatment. The prices in Pre-Cartel that are not affected by any communication represent the correct counterfactual of competition. Unlike post-cartel prices, they are untainted by tacit collusion enabled by preceding communication. The last two columns report the average overcharge bias. The results show that the Post-Cartel and the Whole sample overcharge estimates are biased downwards for all treatments except for Rematching. Hence, PCTC leads to a significant underestimation of cartel damages by econometric techniques that rely on post-cartel data. It is not possible to rank the treatments with respect to the severity of the downward bias due to the limited sample size. Yet, the main implication that the problem of underestimating cartel damages does not exist in the Rematching treatment because of an absence of PCTC remains valid.[27]

[25] Market price margins are considerably larger in ExtComm with 3.475 than in Baseline with 1.925. We conjecture that the anticipation of the possibility to communicate affects the willingness to attempt establishing tacit collusion. Hence, the Baseline treatment is not a good benchmark for the calculation of the cartel overcharge as it lacks comparability with the other treatments with respect to the attainable profits before communication has taken place. Thus, we use only the ExtComm treatment for such purposes.

[26] Periods without price agreements that lie between periods with price agreements could have also been included here. Whether exclusion of such periods with potential tacit collusion increases or decreases the overcharge estimate depends on the market outcome in these periods. If the subjects collude tacitly (compete fiercely) between periods with price agreements, then the true damage would be higher (lower).

[27] In fact, the estimations point to a large overestimation of damages in this treatment. However, these results should be treated with caution, as the competitive counterfactual of ExtComm prices in periods -9 to 0 might not be good counterfactuals for Rematching. Given the destabilising effect of informing about rematching in the

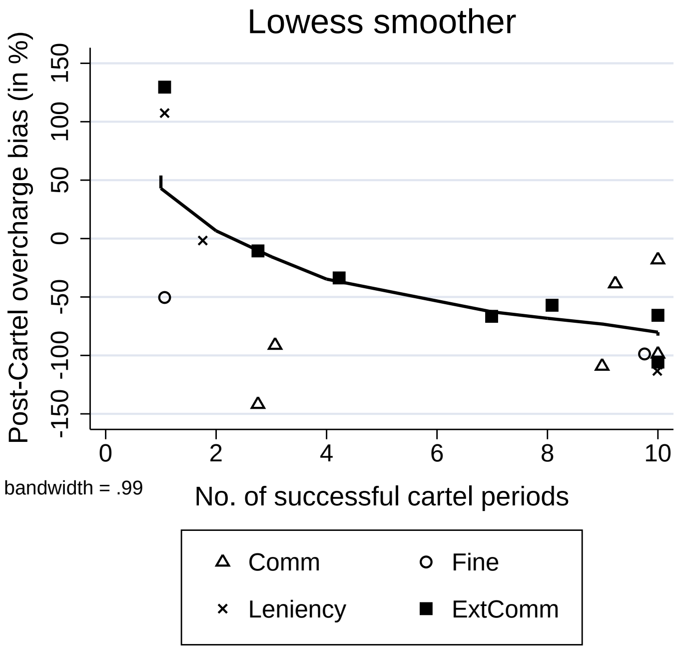
Table 4: Overcharge estimates and biases

		Overcharge estimate			Overcharge bias	
	Obs.	Pre-Cartel	Post-Cartel	Whole sample	Post-Cartel	Whole sample
Comm	6	64.45	19.91	41.12	-77.75%	-40.73%
Fine	2	55.41	10.73	32.01	-68.01%	-35.62%
Leniency	4	48.91	8.82	27.91	-20.30%	-10.63%
ExtComm	7	46.89	22.12	33.91	-24.67%	-12.92%
Pooled	19	53.76	17.42	34.72	-45.07%	-23.61%
Rematching	5	40.63	53.86	47.25	129.73%	64.87%

Notes: Pre-Cartel, Post-Cartel, and Whole sample overcharge estimates represent average values of estimated cumulated cartel overcharges by cartel based on competitive price benchmarks including periods before, after, and before and after the cartel. Pre-cartel prices serve as the counterfactuals for the calculation of overcharges biases. Pooled includes the average values of the columns excluding the Rematching treatment.

As has been shown with Table 3, post-cartel prices are correlated with preceding cartel success. Hence, the downward bias of the estimates should be increasing with the number of preceding cartel success. Figure 4 plots the relationship between the number of successful cartel periods and the bias of the Post-Cartel estimates with a lowess smoother excluding the Rematching treatment (the overcharge estimates are jittered to improve readability). Indeed, the downward bias is increasing with preceding cartel success.

Figure 4: Post-cartel overcharge bias by cartel success



Result: There is a downward bias in overcharge estimates based on the before-after approach when post-cartel prices are considered as benchmark prices. The bias increases with preceding cartel success.

future on collusion, a proper counterfactual for this treatment would likely contain lower prices.

6.2 Instructions (Leniency)

Instructions

Welcome and thank you for taking part in this experiment. In this experiment you can earn money. How much money you will earn depends on your decision and on the decision made by other participants in this room. The experiment will proceed in two parts. The currency used in Part 1 of the experiment is Pound Sterling (GBP). The currency used in Part 2 is experimental points. Each experimental point is worth 15 pence. All earnings will be paid to you in cash at the end of the experiment.

Every participant receives exactly the same instructions. All decisions will be anonymous. It is very important that you remain silent. If you have any questions, or need assistance of any kind, please raise your hand and an experimenter will come to you.

Instructions for Part 1

In the first part of the experiment you will be asked to make 15 decisions. For each line in the table that you will see on the computer screen there is a paired choice between two options ("Option A" and "Option B"). Only one of these 15 lines will be used in the end to determine your earnings. You will only know which one at the end of the experiment. Each line is equally likely to be chosen, so you should pay equal attention to the choice you make in every line. At the end of the experiment a computerized random number (between 1 and 15) determines which line is going to be paid.

Your earnings for the paid line depend on which option you chose: If you chose Option A in that line, you will receive £1. If you chose Option B in that line, you will receive either £2 or £0. To determine your earnings in the case you chose Option B there will be second computerized random number (between 1 and 20). Both computerized random numbers will be the same for all participants in the room.

Instructions for Part 2

In this part of the experiment you will form a group with two other randomly chosen participants in this room. Throughout the experiment you are matched with the same two participants. All groups of three participants act independently of each other. This part of the experiment will be repeated for at least 20 rounds. From the 20th round onwards, in each round there is a **one in five** (**20%**) chance that the experiment will end.

Your job:

You are in the role of a firm that is in a market with two other firms. In each round, you will have to choose a price for your product. This price must be one of the following prices:

90, 91, 92, 93, 94, 95, 96, 97, 98, 99, 100, 101, 102.

You will only sell the product if your price is the lowest of the three prices chosen by you and the other two firms in that round. If you sell the product, your earnings are equal to the difference between the price and the cost, which is 90:

Earnings = Price - 90.

If you do not sell the product, you will not get any earnings but you will not incur costs either. If two or more firms sell at the same lowest price, the earnings will be shared equally between them. Before you choose your price, you may decide to agree with the other firms to set the highest price of **102** and share the earnings. This agreement is only valid if all three firms want to agree on it. After you made your choice, you will be informed whether the price agreement is reached. However, the price agreement is not binding and firms are not required to set the agreed price. After your price choice, you will be told whether you have selected the lowest price as well as the prices of the other firms.

The price agreement may be discovered by the computer. In that case, a fine of **5** points has to be paid. The computer can detect it in 16 out of 100 cases (a chance of 16%). A price agreement remains valid – and can be discovered – as long as it has not been discovered in a previous round. Once this has happened, you will not be fined in the future, unless you make a price agreement again. If you have reached a price agreement in this period, or a past agreement has not been detected by the computer, you must decide whether to report it. You can do this by choosing between the "Report" and "Not report" buttons. If you report it, you are charged additional costs of **1**.

In case one or more group members reports the agreement, it is discovered and a penalty of **5** has to be paid by all group members. However, in case you report your penalty gets reduced as follows:

- If you are the only one to report, you will not pay the penalty but the others will pay the full penalty.
- If you report and exactly one of the other two reports, then your penalty is reduced by half (50%). The other reporting participant has to pay only half of his penalty, while the remaining participant will pay his full penalty.
- If you report and both the other two also report, then the penalty is reduced by one third (33%) for all three of you.

At the end of each round, you will be told the earnings you made in this round. If you agreed on prices, you will also be told whether the agreement was detected by the computer (either because it was detected by chance or by reports).

Final Payment:

At the beginning of the experiment you start with an initial endowment of 40 points = 6 GBP. If the sum of your profits from Part B is below 0, the difference is being covered by the initial endowment. The earnings you earned in each round minus any fine and penalty that you paid will be converted into cash. Each point is worth 15 pence, and we will round up the final payment to the next 10 pence. We guarantee a minimum earning of 2 GBP.