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How do antitrust regimes impact on cartel formation and managers' labor market? An experiment☆

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ABSTRACT

We explore the impacts of different antitrust regimes on managers' labor contracts, when shareholders are intent on their managers engaging in price fixing activities. We compare legal regimes that fine firms to regimes that prosecute managers. We build a theoretical model, which we take to the laboratory. We observe contract choices of shareholders for a given legal regime, as well as the probability of managers forming explicit cartels and coordinating on prices in a repeated Bertrand oligopoly, taking contract and legal regime as given. Our results suggest that there is less collusion when the legal regime prosecutes managers. High-powered contracts do not incentivize cartel formation or price coordination effectively, irrespective of legal regime. Nevertheless, high-powered contracts were most frequently chosen by shareholders, often with collusive intents.

1. Introduction

Antitrust authorities take very different approaches to prosecuting and penalizing cartels. The European Union's DG Comp (and historically many European jurisdictions) only applies corporate fines (as a function of company turnover). In contrast, the US Department of Justice (DoJ) pursues individual convictions of both civil and criminal nature in addition to firm fines. Individual penalties can be severe: between 2000 and 2009, on average, 62% of the individuals found guilty in corporate antitrust cases initiated by the DoJ were sentenced to serving jail time (with an average of 19 months of jail) (Hammond, 2010). In total, in the 2000–2009 period, this amounts to 208 individuals sentenced to jail time (US DoJ,

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2009).[1,2] Such differences in legal regimes will expose firm managers to different levels of risk in the event that they choose to form or join a cartel. Therefore, firms that wish to engage in collusive practices should have to compensate their managers accordingly.

As such, do antitrust regimes involving individual sanctions have larger cartel deterrence power than regimes where only corporate fines are levied? Do different antitrust regimes cause any type of distortion in the managers' labor market, namely by making particular types of contract more appealing?

To examine both questions, we build a theoretical model where firms' managers play an infinitely repeated Bertrand oligopoly game. Taking the antitrust regime as given, firm shareholders offer a contract to the manager. The wage contract may have a fixed component and/or a variable component that is a function of firm's revenues. When setting prices, managers may do so non-cooperatively (that is, with no cartel formation), or cooperatively through a cartel.

Our research questions are ultimately empirical. However, field data is of limited help. First, and perhaps most importantly, it is not possible to do policy experiments and generate legal regime counterfactuals. Even if that were not the case, we would still face a sample selection bias: we would only have access to data from successful cartels that were busted. We could not analyze data from markets with unsuccessful cartels, or from markets with successful, undetected cartels.

To overcome these barriers, we employ laboratory experiments, in which cartels are operationalized through the ability of managers to verbally communicate. Our design allows us to investigate endogenous cartel formation and deterrence effectiveness of the two antitrust regimes we consider. Although they are imperfect approximations to reality, they afford us insight into the role of incentive structures, which create similar trade-offs to those faced by managers (albeit with very different stakes).

The theoretical model predicted very well the probability of cartel formation as a function of legal regime in the data. While cartel formation rates were generally high, the more a manager was exposed to risk (as a function of the legal regime), the less likely cartels were to form. However, contrary to the theoretical prediction, high-powered contracts (those with a high variable component) did not lead to more cartel formation than low-powered contracts. Furthermore, conditional on cartels being active – that is, when all managers agreed to talk – high-powered contracts generally led to worse pricing coordination than low-powered contracts. It appears that flatter payoff functions helped coordination by reducing the financial temptation to deviate.

This relative lack of effectiveness did not stop 60% of shareholders in the two main treatments from offering high-powered contracts – many who did justified it on the basis of motivating managers to collude. In contrast, many shareholders who picked the low-powered contract stated cost-minimization as a motive, likely not believing the effectiveness of the high-powered contract to promote cooperation, or not believing in the stability of cooperation. Both cases are consistent with equilibrium play. In other words, legal regimes that prosecute managers are better able to deter them from price fixing, but they do not discourage firms from trying to incentivize managers to fix prices.

The remainder of the paper is structured as follows. Section 2 contextualizes our paper in the literature. Section 3 outlines our theoretical model. Section 4 presents the experimental design, and Section 5 summarizes the results. Section 6 concludes.

2. Relation to the literature

Our paper contributes to the experimental literature on indefinitely repeated market games and communication. Experimental work has long established that communication is a very effective catalyst for cooperation in social dilemmas (see Crawford, 1998; Balliet, 2010, for a review and meta-analysis, respectively). Andersson and Wengström (2007) show that costly communication is more effective than cheap talk to promote collusion in Bertrand duopolies. Fonseca and Normann (2012) show that communication aids price coordination in Bertrand oligopolies, but that the gains from communication are non-monotonic in the number of firms. Cooper and Kühn (2014) study three forms of communication and their effect on collusion in duopoly experiments. One of their key findings is that the type of communication involving contingencies facilitates collusion to a greater extent. Harrington et al. (2016) address what forms of communication are most effective in spurring collusion and how such forms of communication depend on market structure. They consider two methods of communication: i) price announcements, which may be less effective in spurring collusion but may give rise to lower probability of prosecution, and ii) unrestricted communication, which can be more effective to collusive behavior but associated with higher chance of antitrust prosecution. They find evidence that price announcements lead to price coordination consistent with collusive equilibrium only for symmetric (in terms of firms' costs) duopolies; whereas, unrestricted communication determines collusive behavior for all market structures and its incremental value increases in more asymmetric and more fragmented oligopolies. Gomez-Martinez et al. (2016) study the effect of information about individual firms' actions

[1] Our paper focuses on (incentives for) cartel behavior in jurisdictions where penalties are imposed on the firm (corporate fines) vis-à-vis jurisdictions where sanctions are individual in nature (individual fines and/or jail terms). In doing so, for the case of individual sanctions, we follow Becker (1968) and assume that jail terms have a monetary equivalent that allows them to be compared to fines.

[2] Among the countries that have criminalized cartel behavior, the US is the exception insofar as it makes more frequent use of jail sentences for cartel offenses (OECD, 2009; 2020a; 2020b; Norton Rose Fulbright, 2016), but only from around 2000 onwards (Connor, 2011; 2014).

on cartel stability. Freitag et al. (2019) examine the role of exchanging verifiable vs. unverifiable information in determining the success of explicit cartels in a Cournot market. More recently, Andres et al. (2021) implement an experimental design that distinguishes between innocuous communication, which cannot be sanctioned, and communications on price coordination (which can be sanctioned). They show that innocuous communication reduces the effectiveness of leniency rules: the frequency of cartel formation is similar with or without leniency rules.[3]

In addition, Dijkstra et al. (2021) are the first to investigate a leniency experiment within a repeated game with free-form communication where subjects can discuss anything they want using a computer chat. Their main results suggest that leniency programs do not deter cartels, but only delay them. Moreover, compared to the rest of the literature on leniency programs that use restricted communication, defections and reporting rates are rather low. Moreover, Dechenaux and Mago (2019) study experimentally the role of communication and side payments in facilitating collusion and fostering productive efficiency in repeated Bertrand duopoly markets. The authors find experimental evidence that prices are higher with the combination of both communication and side payments from a low-cost firm to a high-cost one.

Following this literature, a body of work emerged that sought to understand under what circumstances are cartels most likely to emerge. Fonseca and Normann (2014) study the endogenous formation of cartels in Bertrand supergames. They find evidence that firms in four-firm markets form cartels more often than duopolists because of a smaller hysteresis gain if the cartel breaks down. Chowdhury and Crede (2020) study the possibility of post-cartel tacit collusion once a cartel breaks down. The ability of firms to collude tacitly after a cartel is busted may lead to a downward bias in the estimates of damages, and a reduced deterrence effect of fines that are estimated on overcharges. They find evidence of post-cartel tacit collusion; they also find that rematching colluding subjects with strangers after a cartel is detected (which proxies imprisonment or disbarment) can be a sufficient remedy.

Another interesting contribution is that by Gillet et al. (2011), whose experiment studies how the process of decision making within firms affects cartel formation and pricing. The main treatments differ in the rule process by which each firm decides whether to form a cartel and chooses price. In one treatment, one member of the group has the role of CEO, and only his/her decision matters. A second treatment considers the majority rule, according to which the decision is realized by reaching the majority of members. In the third treatment, the authors assume a consensus decision-making rule whereby each member has veto power. The authors find evidence that (i) there is no difference in cartel formation across treatments, (ii) the decision making rule influences price decisions, and (iii) CEOs determine higher prices.

We build on this literature by implementing for the first time in the lab a more general game in which firm managers operate under contracts that include fixed and revenue-based components. This allows us to then examine the role of legal regimes that penalize one or both components.

Our work also builds on and extends the experimental literature on principal-agent games. Regarding the design of contracts, Anderhub et al. (2002) analyze a menu of contracts including fixed and flexible wages for studying incentive compatibility along with 'fair sharing' and reciprocity. Fehr et al. (2007) study behavior within a principal-agent framework where principals can choose among an incentive contract with enforceable monitoring, a bonus contract without fining, and a trust contract. They find that about 90% of principals prefer to choose the bonus contract, in contrast to the standard theories of efficiency; also, effort exerted by the agents and average payoff for both principals and agents is higher than in the incentive contract with monitoring. The authors interpret these results as subjects caring about fairness and opposing situations of distrust and hostility created by incentive contracts with fining mechanisms. Moreover, Karakostas et al. (2017) study efficiency and fairness building on the work of Anderhub et al. (2002) and Fehr et al. (2007). They consider a revenue-sharing contract, a bonus contract and a trust contract among which principals can choose from. The vast majority of experimental subjects choose the revenue sharing contract, and evidence suggests that such contract results in higher effort and a fairer distribution of profits, on average, than the bonus contract. We take the experimental principal-agent literature in a different direction by examining a set up where the objective of the principal is for the agent to coordinate prices with other agents in a supergame; we do not allow monitoring to happen, nor any renegotiation of contracts.[4]

Our work also relates to a significant strand of a literature dedicated to the understanding of the incentives underlying labor contracts of firms' top management and their (labor) relationship with shareholders, typically within a principal-agent framework (Murphy, 1999; Bebchuk and Fried, 2003; Core et al., 2003; Aubert, 2009). Empirically, Murphy (1999) notes that US executives are paid more, and their variable salaries have a larger weight in their total pay than in other countries. Fonseca et al. (2022) explore the consequences to contract design if shareholders are intent on their managers engaging in price-fixing activities under different legal regimes (corporate fines vs. individual sanctions). The authors put forward the idea that (at least part of the) differences in CEO compensation in the US vis-à-vis other countries could be related to the fine regime for collusive behavior because managers, knowing in advance that they may end up being ultimately responsible for collusive behavior will 'demand' from shareholders a contract that protects them adequately from that eventuality.

[3] A leniency rule allows a cartel member to receive a reduced fine in exchange for reporting (or providing evidence on) the cartel to the antitrust authority.

[4] Our paper is also tangentially relevant to the literature on managerial incentives and collusion. See, among others, Spagnolo (2005); Bernhardt and Chambers (2006); Han (2010).

3. The model

3.1. The set-up

Consider a market with n firms selling homogeneous goods to K homogeneous consumers for an infinite number of periods. Consumers have an inelastic demand: they are willing to purchase (at most) one unit of the good in each period $t \in \{0, 1, 2, \ldots\}$ if the price is lower than or equal to their reservation price, $\bar{p}$.

In each period, managers simultaneously and independently set prices. More precisely, the manager of firm $i \in \mathcal{I} = \{1, \ldots, n\}$ decides p_i, and receives a wage equal to $w_i = a + bR_i$, where: $a \geq 0$ is the fixed component (i.e., independent of the firm's market performance), and $0 \leq b < 1$ is the variable component that remunerates the manager with a percentage of the firm's revenue, R_i. Managers are symmetric, risk-averse, and their utility function is given by $u(w) = w^{\alpha}$, with $0 < \alpha < 1$. The higher the value of α is, the less risk-averse the managers are.

For simplicity, firms have no other costs than the manager's wage. Thus, the per-period period profit of firm i is $\pi_i = (1-b)R_i - a$, where R_i depends on the price charged by the firm, p_i, and the rivals' prices. Each firm has sufficient capacity to supply the whole market. Thus, (i) if firm i sets a price above a rival, it sells nothing and gets zero revenues, $R_i = 0$;[5] (ii) if firm i sets the lowest price in the market, $p_i \leq \bar{p}$, along with $m-1$ rivals, the market is evenly split between those firms, and the revenues of firm i are $R_i = \frac{p_i K}{m}$; finally, (iii) if firm i sets (alone) the lowest price in the market $p_i \leq \bar{p}$, it serves the entire market and obtains revenues of $R_i = p_i K$.

At the beginning of the period $t = 0$, the firms' shareholders simultaneously and independently offer their managers a wage contract. The contract consists of a pair of fixed and variable components, (a, b), that prevails for *all* the periods and does not depend on the manager's conduct.[6]

Managers set prices within an infinite time horizon to maximize their expected utility flow, which we denote by U.[7] We assume that all managers attach the same weight to future gains, denoting their common discount factor by $\delta \in (0, 1)$.

As products are homogeneous and firms compete in prices, if managers choose prices non-cooperatively, the equilibrium is the infinite repetition of the Bertrand–Nash equilibrium of the stage game, which we denote using the superscript 'N'. More precisely, in each period, managers set the price equal to the firms' marginal production cost: $p^N = 0$.[8]

Thus, shareholders may offer contracts to managers that foster price coordination to get a positive discounted flow of profits. We will focus on the sustainability of collusive agreements involving the managers of all firms in the market (full collusion). If all managers set price p^c, they equally supply the K consumers and the per-period revenue of each firm is $R^c = \frac{Kp^c}{n}$. The highest price managers may set is the maximum price consumers are willing to pay, $\bar{p}$. Thus, the collusive price p^c ('c' stands for collusion) is such that $p^N < p^c \leq \bar{p}$. Let U^c denote the discounted expected utility flow of each manager if all managers cooperate in all periods.

Managers use grim trigger strategies: they set the cooperative price, p^c, if all managers set this price up to the current period; and they set the Bertrand–Nash price, p^N, if any manager set a price other than p^c in the past. It follows straightforwardly that the optimal deviation is to slightly undercut the collusive price, i.e., to set $p^d = p^c - \epsilon$, with $\epsilon > 0$ ('d' stands for deviation). Let U^d denote the discounted utility flow of a manager that breaks the collusive agreement.

To foster collusion, the wage contract must satisfy the managers' incentive compatibility constraint, i.e., must ensure that the managers' expected discounted utility flow is higher if abiding by the collusive agreement than if breaking it. In other words, managers will collude if and only if $U^c \geq U^d$.

Suppose an antitrust authority may investigate the market and levy a fine if it detects a cartel. If the cartel is detected in a period, t, the collusive agreement may be restarted in the following period, $t+1$. We assume that, in the case of recidivism, there is no aggravated fine. Furthermore, the probability that the antitrust authority investigates a market does not depend on whether a cartel has been detected in that market in a previous period.[9] Thus, in any period under price coordination, the antitrust authority detects the cartel with probability $\tau \in (0, 1)$, and imposes a fine. Following Buccirossi and Spagnolo (2007), we assume that the fine is imposed also on deviators.[10]

[5] We assume that, to be operating, a firm must necessarily hire a manager and, if the manager accepts the contract, he/she cannot be fired in that time period. In other words, in every period, no firm can avoid paying the fixed wage component of its manager.

[6] We restrict the analysis to contracts that managers are willing to accept, i.e., satisfy their participation condition. In a companion paper (Fonseca et al., 2022), we consider a more complete principal-agent problem, where the principal (shareholder) must offer a contract to the manager that satisfies both the participation and incentive compatibility constraints.

[7] Aligning the manager's finite time horizon (which coincides with his/her spell as the firm's manager) with the typically infinite time horizon for the firm is a key challenge for shareholders (see, e.g., Edmans and Gabaix 2016). This alignment is often achieved by compensating the manager with equity. We bypass this issue by assuming that the time horizons of shareholders and managers are equal.

[8] Thus, in the absence of managers' coordination, shareholders might incur losses (if the fixed-wage component is strictly positive, $a > 0$). Indeed, in the best scenario (wherein the managers' contract is purely variable, i.e., $a = 0$), shareholders get a zero payoff.

[9] We recognize this assumption may not be entirely realistic, but treating recidivism differently would introduce significant complexity into the analysis.

[10] This assumption is different from Dargaud et al. (2013), who assumed that no fine is paid in the deviation period. However, as pointed out by Buccirossi and Spagnolo (2007), in neither the US nor in the EU, "defecting from the cartel agreement is deemed an alleviating circumstance", with the fine imposed on deviators being the same as on those that abided by the agreement. Leniency programmes exist but we do not incorporate leniency programmes in our model.

We consider two antitrust policies that differ in the target of the fine: (i) the case where the fine is imposed on the firm; and (ii) the case where it targets the manager. In practice, antitrust authorities may employ a combination of these two policies, levying fines on both the shareholder and the manager. However, to better understand the mechanisms underlying each policy, we examine these two extreme cases separately.

3.2. Firm revenue-based fine

In most EU jurisdictions, cartel behavior is punished with a fine that targets firms' profits, usually in the form of a percentage of its revenues. In this case, if the antitrust authority discovers the cartel, each firm must pay a fine that is a percentage $F \in (0,1)$ of its revenues (independently of the cartel duration). We will use the subscript 'f' (firm) to denote this scenario. If managers coordinate prices in a given period $t \in \{0,1,2,\ldots\}$:

- with probability $1-\tau$, the cartel is undetected, and the managers' wage is: $a + b\frac{Kp^c}{n}$;
- with probability τ, the cartel is detected, and the managers' wage is: $a + b\frac{(1-F)Kp^c}{n}$.

Thus, if managers coordinate prices in all periods, their discounted expected utility flow is:[11]

$$U_f^c = (1-\tau)\left[\left(a + b\frac{Kp^c}{n}\right)^{\alpha} + \delta U_f^c\right] + \tau\left[\left(a + b\frac{(1-F)Kp^c}{n}\right)^{\alpha} + \delta U_f^c\right]$$

$$\Leftrightarrow U_f^c = \frac{(1-\tau)\left(a + b\frac{Kp^c}{n}\right)^{\alpha} + \tau\left[a + b\frac{(1-F)Kp^c}{n}\right]^{\alpha}}{1-\delta}.$$

If a manager disrupts the collusive agreement by undercutting the price and supplying the entire market, he/she has a short-run deviation gain. However, this will trigger a permanent reversion to the Bertrand–Nash equilibrium and the manager will only receive the fixed-wage component (as the firms' revenues will drop to zero) in all the following periods. Thus, the expected discounted utility flow of a manager that disrupts the collusive agreement is:[12]

$$U_f^d = (1-\tau)(a + bKp^c)^{\alpha} + \tau[a + b(1-F)Kp^c]^{\alpha} + \frac{\delta}{1-\delta}a^{\alpha}.$$

Managers will coordinate prices if their expected discounted payoff under collusion exceeds their payoff if breaking the agreement, i.e., $U_f^c \geq U_f^d$.

An increase in the severity of the fine (corresponding to an increase in F) or in the probability of cartel detection (corresponding to an increase in τ) has two countervailing effects on the incentives for managers to collude: (i) on the one hand, managers' expected gains from collusion decrease, which makes the collusive agreement more difficult to sustain; (ii) on the other hand, as deviations are also punished by the antitrust authority, the gain from a defection also decreases, which makes the agreement easier to sustain.

3.3. Manager salary-based fine

Inspired by the approach taken by US antitrust authorities, consider now that cartel behavior is punished with sanctions on the manager (e.g., because it is considered a criminal offense). We use the subscript 'm' (manager) to denote this case. Following Dargaud et al. (2013), we assume this sanction is equivalent to a monetary reduction of the manager's remuneration (e.g., the reputational damage caused by the fine). Assume that the fine is a percentage $\Phi \in (0,1)$ of the managers' wage and is independent of cartel duration. Thus, in a period under price coordination:

- with probability $1-\tau$, the cartel is undetected, and the managers' wage is: $a + b\frac{Kp^c}{n}$;
- with probability τ, the cartel detected, and the managers' wage is: $(1-\Phi)\left(a + b\frac{Kp^c}{n}\right)$.

Assuming that, if the cartel is detected, managers may restart the agreement in the following period,[13] the managers' expected discounted utility flow under price cooperation is:

$$U_m^c = (1-\tau)\left[\left(a + b\frac{Kp^c}{n}\right)^{\alpha} + \delta U_m^c\right] + \tau\left[\left[(1-\Phi)\left(a + b\frac{Kp^c}{n}\right)\right]^{\alpha} + \delta U_m^c\right]$$

$$\Leftrightarrow U_m^c = \frac{\phi\left(a + b\frac{Kp^c}{n}\right)^{\alpha}}{1-\delta},$$

[11] Recall that we are assuming that, after the cartel being detected, managers can restart the agreement in the following period. Without the existence of an augmented fine for recidivism, it follows that it is optimal for managers to set the collusive price (instead of the Bertrand prices) in the periods following the cartel detection.

[12] Recall we are assuming that, in case of cartel detection, the fine is also imposed on deviators.

[13] This assumption allows us to directly compare the outcomes between the two jurisdictions, but it is naturally unrealistic if there are non-monetary implications of being caught in a cartel (e.g., jail time).

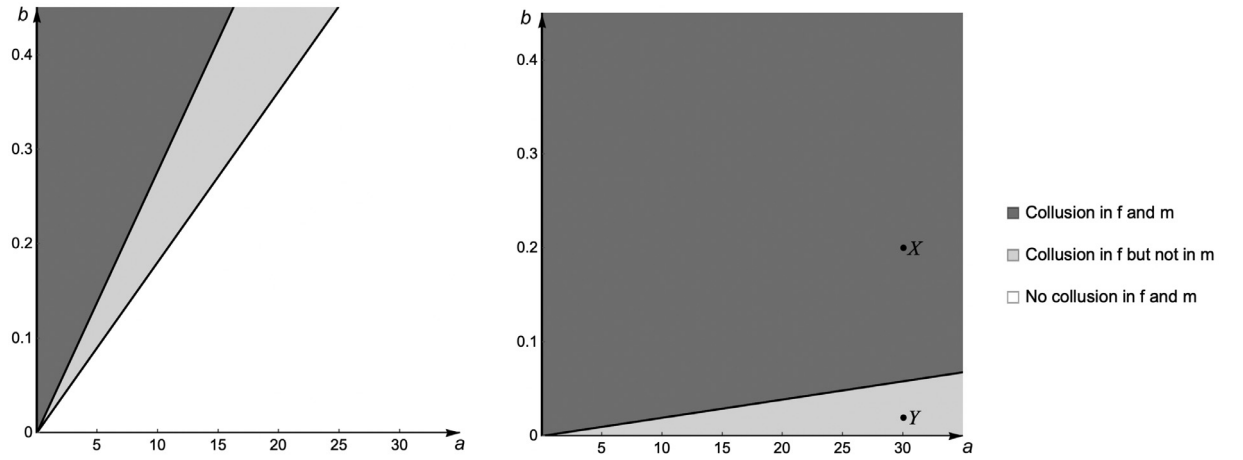


Fig. 1. Sustainability of collusion in the two legal regimes given wage contracts (a, b), for $\alpha = 0.7$, $K = 24$, $n = 4$, $p^c = 10$, $\tau = 0.2$ and $F = \Phi = 0.35$. Points X and Y correspond to the contracts implemented in the experiment.

where $\phi = 1 - \tau[1 - (1 - \Phi)^{\alpha}]$, with $0 < \phi < 1$, denotes the (net of fine) share of utility that managers expect to receive in each period.

As in the case of the revenue-based fine, if a manager disrupts the collusive agreement, he/she gets a short-run deviation gain, but this will trigger the permanent reversion to Bertrand–Nash competition. Thus, the expected discounted utility flow of a manager that breaks the agreement is:

$$U_m^d = (1-\tau)(a + bKp^c)^{\alpha} + \tau[(1-\Phi)(a + bKp^c)]^{\alpha} + \sum_{t=1}^{+\infty} \delta^t a^{\alpha} \;=\; \phi(a + bKp^c)^{\alpha} + \frac{\delta}{1-\delta} a^{\alpha}.$$

As before, collusion is sustainable if and only if the managers' expected payoff is higher under collusion than if they break the agreement, i.e., $U_m^c \geq U_m^d$.

3.4. Comparison of legal regimes

We expect the antitrust regime, i.e., the target of the fine, to affect the terms of the wage contracts that incentivize managers to coordinate prices. Fig. 1 represents incentives for managers to collude in the two regimes, for a given wage contract (a, b).

As illustrated in Fig. 1, if the magnitude of the fine is similar across regimes ($\Phi = F$), the set of wage contracts that induce collusion is smaller when the fine targets the manager (regime m) than when it targets the firm (regime f). This is a natural result, as under regime f, if the cartel is uncovered, the manager is harmed to the extent that his/her variable payment is affected (because the fine reduces the firm's revenues). However, under regime m, if the cartel is uncovered, both wage components (fixed and variable) are affected. Therefore, contracts that make collusion under regime f appealing do not necessarily induce collusion under regime m. The reverse, however, is not true: all else equal, a contract that induces collusion under m also induces collusion under regime f.

4. Experimental design and hypotheses

4.1. Experimental design

Our experimental design builds directly on the model of the previous section. The experiment was divided in two phases. In Phase 1, participants played the role of firm's managers. Each manager set the price for a homogeneous good that was produced at zero cost. A market consisted of 24 computerized consumers, each of whom demanded one unit of the good up to a reservation price of $\bar{p} = 10$. Each market was served by four firms; in every period, firm managers simultaneously and independently chose a price, denominated in Experimental Currency Units (ECU) from the set $\{0.00, 0.01, \ldots, 9.99, 10.00\}$.[14]

[14] We chose four-firm markets on the grounds that Fonseca and Normann (2012) found that four-firm Bertrand oligopolies are the biggest beneficiaries from communication in terms of their ability to collude. Fonseca and Normann (2014) also find that four-firm Bertrand markets are more likely to choose to communicate than Bertrand duopolies.

Table 1
Experimental design.

Contract type	NoChat	ChatNoLaw	Chat-m	Chat-f
High-Powered (30, 20%)	(12, 144, 5472)	(12, 144, 5472)	(12, 144, 5472)	(12, 144, 5472)
Low-Powered (30, 2%)	(12, 144, 5472)	(12, 144, 5472)	(12, 144, 5472)	(12, 144, 5472)

(# of sessions, # of markets, # pricing decisions); 12 subjects per session.

The firm that set the lowest price served all 24 consumers. In case of a tie, the market was equally split between the firms that set the lowest price, while the other firm(s) earned 0.

Markets did not have a deterministic duration; at the end of each period, there was a continuation probability of 90% that was common knowledge among participants. Note that the probability of continuation in the experiment (90%) proxies the discount rate. We generated a sequence of random draws before data collection began and used it in all sessions. Once a market ended, we randomly reassigned participants to new markets.[15] Setting a continuation probability of 90% means that the expected duration of each market was 10 periods. This is a sufficiently long expected duration to allow for collusion to take place. Since Phase 1 consisted of four consecutive markets, the expected total number of periods in the session as a whole (i.e., 40) was low enough not to induce subject fatigue, which could have compromised decision quality.

At the start of the first period in each market, the computer informed managers of the parameters of their contract, (a, b). We implemented two wage contracts, which differed in the revenue component. The two contracts were $X = (30, 20\%)$ and $Y = (30, 2\%)$, where the first term is the fixed wage, a, and the second term is the proportion of revenues that accrued to the manager, b. As Fig. 1(b) illustrates, the X-contract satisfies the incentive compatibility constraint of the manager in both legal regimes. In contrast, for the Y-contract the manager's incentive compatibility constraint is satisfied only in the firm revenue-based regime. We opted for the same fixed-wage parameter in both types of contracts to minimize the number of parameters that change across legal regimes. This makes the two antitrust regimes comparable. We will henceforth refer to X as the 'high-powered contract' and to Y as the 'low-powered contract'.

Every manager in a four-firm market had the same contract. To control for the possibility that the order in which we implement contracts might affect subjects' decision-making, we implemented two orderings at the session level: XYXY and YXYX. Participants were paid on the basis of their total earnings in two randomly chosen markets. Table 1 summarizes our experimental design.

To address the second research question posed at the beginning of the paper (*Do different antitrust regimes cause any type of distortion in the managers' labor market, namely by making particular types of contract more appealing?*), we considered that, in Phase 2, the subjects who acted as managers in Phase 1 took the role of shareholders. Their task was to decide which contract (X or Y) they would like to offer to a future manager of their firm.[16] Their firm would exist in a future session of the experiment (Phase 2). Their earnings as shareholders would be equal to the revenue of the firm minus the manager salary minus any potential fine in all rounds of a market. Participants were called to the lab to receive their payment from their decisions as shareholders in cash a few days after completing the Phase 2 sessions. There was an implicit limited liability rule for shareholder payoffs, because we could not enforce losses. In the event of a loss, we informed a shareholder of the loss and stated that their payoff as an shareholder was zero.

Phase 2 participants only played the role of firm managers. Their instructions were nearly identical to Phase 1; the only difference was an explicit mention that their contract was chosen by a shareholder, who was a participant in an earlier session. The instructions also specified that the profits resulting from their pricing decisions would go to the shareholder. The purpose of Phase 2 was to ensure that the contract choices made by shareholder at the end of Phase 1 were real and no deception occurred. As in Phase 1, participants took part in four indefinitely repeated markets with a continuation probability of 90%. In each market they were assigned to a different shareholder in Phase 1: we assigned Phase 1 shareholders to Phase 2 managers using a random draw without replacement, based on shareholders' contract choices. We repeated the draw in the event that there was excess demand for a given contract. This is because each session in Phase 2 required an equal number of X and Y contracts, but there was not necessarily an equal number of Phase 1 shareholders who chose contract X and Y. We also implemented two orderings at the session level: XYXY and YXYX. Fig. 2 summarizes the timeline of the experiment.

We implemented four treatments: NoChat, ChatNoLaw, Chat-f and Chat-m. The NoChat treatment is a benchmark treatment where communication between managers was not possible at any time. The ChatNoLaw treatment is a second benchmark treatment, where communication among managers was possible, but markets were not audited and hence no punish-

[15] It is widely acknowledged in the experimental economics literature that participants require repetition of a game in order to learn what the optimal course of action is. Dal Bó and Fréchette (2011) show that in the context of repeated games, it is also necessary for subjects to learn how to play the repeated game.

[16] We do not attempt to model potential choices of shareholders of choosing a different firm by selling shares or divesting their investment according to firm's performance or behavior, because this would be beyond the scope of the paper; it would also complicate further our experimental design without much benefit. Even if we had modified our experimental design to allow shareholders to choose a specific firm in Phase 2, those firms were all equal; that is to say, each firm would be ex-ante indistinguishable from one another, because those contracts would be offered to (ex ante unknown) managers in a future date.

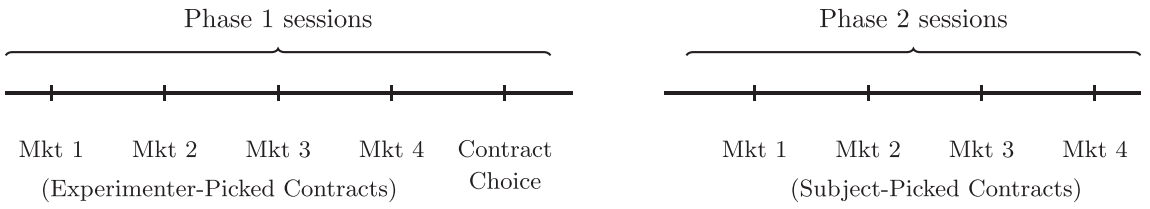


Fig. 2. Timeline of the experiment.

ment was enforced. If in a given period, all managers agreed to talk (at no cost), a chat box would appear for 30 seconds before managers made their pricing decisions in that period and every period thereafter. We did not allow for partial cartels because they are ineffective in a homogeneous good Bertrand setup. This setup also allows us to relate our findings directly to the existing literature. If there was no consensus in a given period, the chat box would not appear prior to setting prices, but managers would again have the option to chat in the next period.

In the Chat-f and Chat-m treatments, communication among managers was possible as in ChatNoLaw; however, each market was audited in every period with an i.i.d. probability of 20%. If an audit occurred and a chat box was active in that period (irrespective of whether it was used or not), a fine was levied on all four managers and the chat box was not automatically available anymore: managers would have to unanimously agree to chat in the following period for it to become available again. In the Chat-f treatment, the fine was 35% of the variable wage component, proxying a fine to corporate revenue. In the Chat-m treatment, the fine was 35% of both components of the manager's salary.

Upon arrival at the laboratory, each participant sat at a booth which ensured their decisions were taken in private. The experimenters handed out instruction sets and requested no verbal communication to take place among participants. Participants read the instructions in their own time and had to solve a quiz to verify their understanding of the experimental environment. The experimenters checked each participant's answers. Once all quizzes were checked and any clarification questions were answered, the experiment started. See the Appendix for copies of the instructions and quizzes.

Other than the first period of a market (in which communication was not allowed),[17] an experimental period unfolded as follows. Managers were asked if they wished to communicate or not (except in NoChat, or if the chat box was already available). If all managers agreed (or if the cartel was active), a chat box was available on-screen for 30 seconds. Managers could talk about anything they wished, but they could not identify themselves. Once the 30 seconds elapsed, managers set prices. After that, the computer randomly determined whether the market was audited (except NoChat and ChatNoLaw treatments). The period was then over: the software displayed prices and revenues for all managers/firms in the market. The software then announced whether the market would continue for an additional period or if that was the last period of the market.

At the end of the experiment, we measured risk aversion using the instrument developed by Gneezy and Potters (1997). Subjects received an endowment of 100 ECU. They could allocate any proportion of that endowment to a 50/50 lottery that either paid out 2.5 times the amount invested or nothing, and keep the rest. A risk neutral/risk seeking individual should allocate their whole endowment to the lottery; the more risk averse the decision-maker is, the less they should allocate to the lottery.

In both Phase 1 and Phase 2, we ran three sessions of each ordering type, with 12 participants per session in each treatment, for a total of 576 participants (288 in each Phase). We ran the experimental sessions in the FEELE lab at the University of Exeter Business School. Phase 1 sessions took place between 21 February and 5 March 2018; Phase 2 sessions took place between 6 March and 20 March 2018. The software used to run the experiment was *z*-Tree (Fischbacher, 2007). We recruited participants from a pool of volunteers using the FEELE lab's ORSEE system (Greiner, 2015). Sessions lasted on average one hour. The average earnings were £13.10 (min: £9.00, max: £19.00).

4.2. Hypotheses

As illustrated in Fig. 1(b), the low-powered contract should incentivize collusion under a corporate fines regime (Chat-f) but not under an individual fines regime (Chat-m). In other words, the low-powered contract satisfies the incentive compatibility constraint in Chat-f, but not in Chat-m. In contrast, the high-powered contract incentivizes collusion in both regimes. Under the NoChat treatment, explicit cartel formation is not allowed, as there is no communication. By contrast, in the ChatNoLaw treatment, there is no punishment associated with cartels. Therefore, the incentive compatibility constraint is satisfied under either contract.

[17] Disallowing communication in the first period of each market allows us to have a baseline measure of competitiveness without communication in all markets.

There is an important caveat to the aforementioned theoretical predictions: participants in the experiment may be heterogeneous in key behavioral parameters such as risk aversion.[18] As such, the parameters of the high-powered contract may not be sufficiently high to incentivize collusion for some participants. Therefore, if one manager in a given market is particularly risk averse, he or she may block the formation of a cartel. This means that we may observe fewer cartels being formed in the Chat-m and Chat-f treatments than predicted. We will therefore use two outcome measures from the data: instances where cartels were formed, and individual managers' votes for cartel formation. Building on the theoretical results and on the possible heterogeneity in risk attitudes in our sample, we posit the following hypotheses.

Hypothesis 1 (probability of forming cartels, I)**.**

1. Under low-powered contracts, the probability of a manager voting for a cartel and the probability of forming a cartel in the different treatments can be ranked as follows: ChatNoLaw $\geq$ Chat-f $>$ Chat-m.
2. Under high-powered contracts, the probability of a manager voting for a cartel and the probability forming a cartel in the different treatments can be ranked as follows: ChatNoLaw $\geq$ Chat-f $\geq$ Chat-m.

Hypothesis 2 (probability of forming cartels, II)**.**

1. In the Chat-m treatment, the probability of forming a cartel will be strictly higher under the high-powered contract than under the low-powered contract.
2. In the Chat-f treatment, the probability of forming a cartel will be at least as high under the low-powered contract than under the high-powered contract.

In the setup of our experiment, we would typically expect managers to coordinate on the maximum price whenever they choose to communicate. While it is possible that managers coordinate on any supra-Nash price, it stands to reason that managers would prefer to maximize their collusive payoffs by coordinating on the monopoly price. The maximum price is also arguably focal.

Hypothesis 3 (Cartel Prices)**.** Communication will lead to better price coordination and higher prices, regardless of treatment or contract type.

As mentioned above, at the end of Phase 1, managers were asked to take on the role of shareholders and choose a particular contract that would be offered to managers in Phase 2. As shareholders, they would receive further payoffs depending on managers' decisions in Phase 2. Recall that the manager's incentive compatibility constraint is always satisfied in the NoChat, ChatNoLaw, and Chat-f treatments regardless of the type of contract. Therefore, in those treatments, managers always have the incentives to collude — although they must do so tacitly in the NoChat treatment. As such, profit-maximizing shareholders should choose the low-powered contract, as it should achieve the same revenues, but at a lower cost. In contrast, in the Chat-m treatment, the low-powered contract does not satisfy the incentive compatibility constraint. As such, in order to foster collusion, shareholders would need to choose the high-powered contract. This leads to our final hypothesis.

Hypothesis 4 (Contract Choice)**.** The probability of shareholders choosing the high-powered contract is higher in Chat-m than in NoChat, ChatNoLaw and Chat-f.

5. Results

We will report on results from Phase 1 and Phase 2 jointly, as we did not find systematic differences across the two in the key behaviors of interest. We break down the analysis for each Phase separately in the Appendix. We report results from (i) random effects probit estimations using period/market-level observations, bootstrapping standard errors at the session level whenever our dependent variable is dichotomous; and otherwise (ii) random effects GLS estimations using period/market-level observations, bootstrapping standard errors at the session level. When reporting results from non-parametric tests, the unit of observation is the session.

5.1. Cartel formation

We begin by looking at the probability with which participants agreed to form a cartel as a function of treatment. For each of the 432 markets in the ChatNoLaw, Chat-f and Chat-m treatments (72 markets per treatment per phase), we created an indicator variable that equals one if at least one cartel was initiated in that market. An initiated cartel is an instance in which all four managers agreed to communicate in a given period. It could occur anytime from the second period a market was in existence onwards. We note that we may observe several cartels in a single supergame in either Chat-f or Chat-m. This is because communication is terminated following an audit, and manager have the option of restarting it. In contrast, cartels in ChatNoLaw are never broken up, therefore the frequency is limited to be 0 or 1. Therefore, in order to have a meaningful comparison, we focus on the probability of forming at least one cartel, rather than the average number of

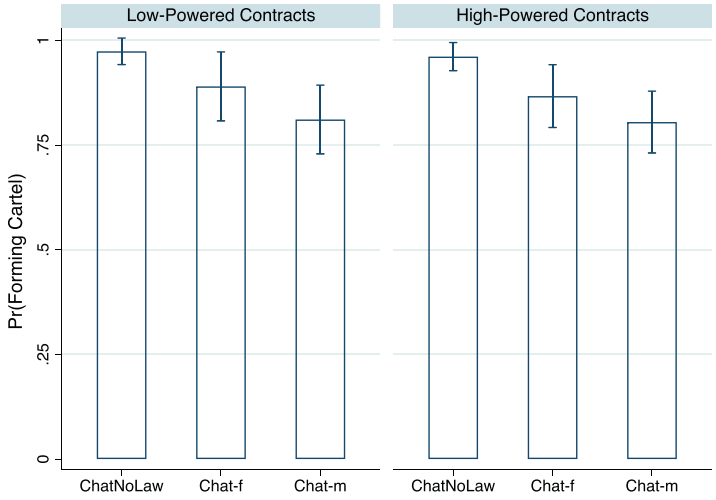


Fig. 3. Estimated probabilities of cartel formation by treatment.

cartels. Fig. 3 displays the estimated probability of cartel formation during the lifetime of a market in each of the treatments with communication.

Result 1 (probability of forming cartels, I)**.** Treatments are ranked on their probability of cartels being formed as: ChatNoLaw $\geq$ Chat-f $=$ Chat-m.

Support: Under low-powered contracts, we observe significantly lower propensity to form cartels in Chat-m than in ChatNoLaw ($z = 2.337$, $p = 0.020$, Mann-Whitney (MW) test) but not relative to Chat-f ($z = 1.049$, $p = 0.294$, MW test). We do not observe a significant difference between ChatNoLaw and Chat-f ($z = 1.141$, $p = 0.254$, MW test). Under high-powered contracts, we observe the same pattern, with a significant difference between Chat-m and ChatNoLaw ($z = 2.311$, $p = 0.021$, MW test), a weakly significant difference between Chat-f and ChatNoLaw ($z = 1.948$, $p = 0.051$, MW test) and no significant difference between Chat-f and Chat-m ($z = 0.679$, $p = 0.497$, MW test).

Result 2 (probability of forming cartels, II)**.** The probability of forming a cartel is not affected by contract type in ChatNoLaw, Chat-f and Chat-m.

Support: We compare the probability of cartels being formed across contract types, keeping treatment constant. We find no significant difference in collusion propensity between low-powered and high-powered contracts in any treatment (ChatNoLaw: $z = 0.503$, $p = 0.615$; Chat-f: $z = 0.586$, $p = 0.558$; Chat-m: $z = 0.152$, $p = 0.879$, MW test).

We complement the analysis that supported the testing of our hypotheses by analyzing in more detail the voting decisions taken by managers whenever the opportunity to form a cartel arose. We estimated a random effects probit model of the decision by manager i in market k to vote in favor of forming a cartel in period t, whenever that vote was available to managers in a market —that is, excluding the very first period in a market when voting was never allowed, as well as any period in which the cartel was already active.

Our first specification includes as regressors treatment dummies (Chat-f and Chat-m), a dummy for high-powered contract (HP) and all relevant interactions. We consider a second specification that also includes a dummy variable for whether the cartel had been caught before (Caught), which allows us to distinguish votes for cartel re-starts from votes on initial cartel formation, as well as, supergame dummies (Supergame t), and a linear time trend (Period). The supergame dummies and time trend examine learning effects both within supergame and across supergames. Choosing to form a cartel is a risky decision, so it stands to reason that risk attitudes should play a prominent role in determining whether or not to vote for the formation of a cartel. As such, our second specification also includes a dummy variable that equals one if the manager was risk averse (Risk Averse), based on the risk-aversion elicitation done after the experiment. Table 2 summarizes the estimation results.

The voting data broadly confirms the evidence on the probability of at least one cartel being formed. Estimation (1) shows that the probability of voting in favor of a cartel being formed is significantly lower in Chat-m than in ChatNoLaw under both types of contracts (LP: $\chi^2(1) = 77.98$, $p < 0.001$; HP: $\chi^2(1) = 86.41$, $p < 0.001$). The probability of voting for a

[18] Rabin (2000) makes the important point that risk aversion is an implausible explanation for behavior in laboratory experiments, or any low-stakes decision. Nevertheless, it has been a prevalent explanation for the failure of risk-neutral Nash equilibria in settings like first-price auctions (Cox et al., 1988) or Tullock contests (Milner and Pratt, 1991; Sheremeta, 2011), so we consider it here also.

Table 2
Determinants of individual votes for cartel formation.

DV: Vote in favor of cartel formation				
	(1)		(2)	
Chat-f	−0.783***	(0.085)	−0.570***	(0.092)
Chat-m	−0.846***	(0.096)	−0.641***	(0.075)
HP	−0.003	(0.131)	0.087	(0.125)
Chat-f × HP	−0.055	(0.144)	−0.145	(0.132)
Chat-m × HP	−0.026	(0.150)	−0.113	(0.133)
Caught			−0.480***	(0.062)
Risk Averse			−0.236***	(0.054)
Period			−0.033***	(0.007)
Supergame 2			0.973***	(0.080)
Supergame 3			1.330***	(0.127)
Supergame 4			1.531***	(0.192)
Constant	1.415***	(0.085)	1.290***	(0.096)
N (markets)	6876 (432)		6876 (432)	

*Notes:*Bootstrapped standard errors clustered at session level in parentheses. Data excludes the initial period of each market or any period in which a cartel was already active. ***, **, *: $p < 0.01$, $p < 0.05$, $p < 0.10$, respectively.

cartel is also significantly lower in Chat-f than ChatNoLaw under both types of contract (LP: $\chi^2(1) = 85.09$, $p < 0.001$; HP: $\chi^2(1) = 71.51$, $p < 0.001$).

We next extend our analysis to consider dynamic effects in estimation (2). The sign, magnitude and significance of the coefficients of treatments, contract type and their interactions are unchanged. The probability of voting for a cartel is significantly lower once a cartel was busted ($\chi^2(1) = 59.28$, $p < 0.001$). The linear time trend is small, close to zero but significant ($\chi^2(1) = 23.97$, $p < 0.001$). From that estimated coefficient, we infer a marginal effect of −0.008, which indicates that for every period in the supergame there was a 0.8 percentage point decrease in the probability of voting for a cartel. This suggests little scope of intra-supergame learning above and beyond the effect of being caught (the marginal effect for Caught is −0.123, implying a 12.3 percentage point decrease in the probability of voting for a cartel). In contrast, the supergame dummies all have large positive and significant coefficients ($\chi^2(1) \geq 63.82$, $p \leq 0.001$). This supports the conjecture that participants learn the strategic incentives of the supergame with more experience.

We conclude by examining the role of risk aversion. The estimated coefficient on the risk aversion dummy is negative and significant ($\chi^2(1) = 19.36$, $p < 0.001$). This is consistent with the intuition that risk averse managers are less likely to vote for a cartel.

5.2. Prices

We now turn to pricing behavior conditional on the cartel being active or not. We start by testing Hypothesis 3. To do so, we quantify the proportion of periods in which firms were able to coordinate and charge the same price above marginal cost in a given period. We denote these instances as price coordination.

Result 3 (Prices and Communication)**.** Communication leads to better price coordination and higher prices in all treatments and both contract types.

Support: When managers could not or did not form a cartel, they were able to coordinate on a price 6% of the time (*N=172*). In all but two observations (99% of cases), the price they coordinated on was 10.[19] In contrast, when cartels were active, managers were able to charge the same price in a significantly higher proportion of periods (63%, $z = 11.104$, $p < 0.001$). In 99% of those instances ($N = 1,562$), managers coordinated on a price of 10.[20]

Table 3 presents estimates of the rate of coordination on prices conditional on cartels being active as a function of contract type and treatment. The unit of observation is the relative frequency of price coordination in market *j*. We include random effects for session.

We find no significant difference in the average rate of price coordination between Chat-f and ChatNoLaw under both types of contracts (low-powered contracts: $\chi^2(1) = 0.52$, $p = 0.470$; high-powered contracts: $\chi^2(1) = 0.72$, $p = 0.397$). Chat-m has a marginally lower rate of coordination than ChatNoLaw under low-powered contracts ($\chi^2(1) = 3.76$, $p = 0.053$) but not under high-powered contracts ($\chi^2(1) = 1.12$, $p = 0.291$). We find no difference in the rate of price coordination in Chat-m and Chat-f (low-powered contracts: $\chi^2(1) = 2.63$, $p = 0.105$; high-powered contracts: $\chi^2(1) = 0.43$, $p = 0.514$).

[19] In those two cases, all managers set prices of 0.1 and 1.

[20] There were 11 other instances in which all four managers coordinated on a price other than 10. In those cases, the coordinated prices were 9.99, 7.5, 7, 5, 4.5, 4, and 3.

Table 3
Estimated rates of price coordination in active cartels.

Dependent variable: price coordination		
Chat-f	−0.039	(0.054)
Chat-m	−0.134**	(0.069)
HP	−0.126***	(0.058)
Chat-f × HP	0.075	(0.067)
Chat-m × HP	0.206**	(0.098)
Supergame 2	0.137**	(0.058)
Supergame 3	0.228***	(0.063)
Supergame 4	0.301***	(0.053)
Constant	0.506***	(0.056)
var(Session RE)	0.029	(0.005)
var(Residual)	0.109	(0.006)
N (sessions)	382 (36)	

Notes: DV = 1 if all firms charge the same price in period t and zero otherwise. Bootstrapped standard errors at session level. ***, ** indicate $p < 0.01$, $p < 0.05$, respectively.

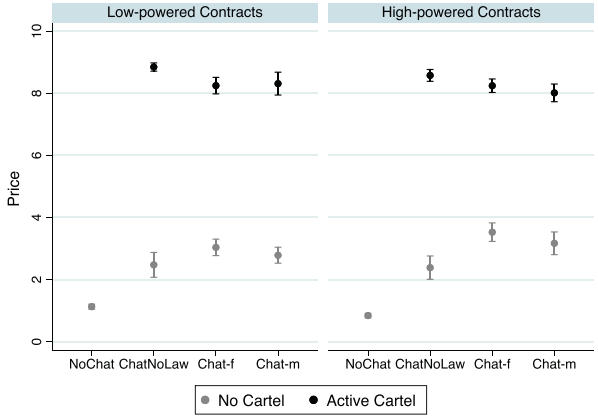


Fig. 4. Estimated selling prices.

We next analyze average selling prices. Because of the large number of parameters being estimated, we opted to show our estimation results visually. Fig. 4 displays estimated average selling prices conditional on treatment, on a cartel being active that period, and contract type. As expected, average selling prices are the lowest in NoChat, irrespective of contract type. Also as expected, conditional on treatment, average selling prices are higher when a cartel is active, irrespective of contract type.

5.3. Exploratory analysis

Managers engage in explicit collusion at a very high rate despite reasonably large financial penalties. The fact that managers persist in doing so even after their cartel is busted merits some additional attention. In particular, it suggests that price agreements between managers were frail, despite the large increase in selling price reported in Fig. 4.

We looked at the average selling price conditional on the first cartel being active and compared it to the average price in the period immediately after the cartel was busted for all relevant treatments and both contracts. Table 4 summarizes this information.

We note that the average selling price under the first cartel ranges between 8 and 8.5 depending on the treatment. However, the average selling price in the period immediately after the first audit declines by 33–50% if managers do not choose to re-form the cartel right away. Admittedly, post-bust cartel prices without communication are substantially higher than the prices set before the first cartel was formed. However, they reveal that in the absence of communication, managers are unable to maintain the same average price level without communication, even in the period immediately following the cartel break-up. This is also the case if we focus on the first period after the first cartel is busted or all periods thereafter.

It is therefore reasonable to presume that subjects anticipated prices would continue to decline, and this expectation would result in the desire to re-form the explicit cartel, even accounting for the expected cost of doing so.

Table 4
Average selling prices pre-cartel, during first cartel, period immediately after first audit, all periods after first audit, Chat-f, Chat-m treatments. Standard deviations in parentheses.

	Chat-f		Chat-m	
	LP contract	HP contract	LP contract	HP contract
Pre-First Cartel	1.57 (1.62) $N = 198$	1.68 (1.69) $N = 194$	2.26 (2.76) $N = 255$	1.54 (1.78) $N = 233$
First Cartel	8.10 (3.30) $N = 198$	8.47 (3.10) $N = 209$	8.55 (2.66) $N = 147$	7.93 (3.37) $N = 187$
Round Post-First Cartel Bust, No Chat	5.27 (3.83) $N = 33$	5.68 (4.07) $N = 32$	3.94 (3.64) $N = 40$	5.02 (3.97) $N = 35$
Round Post-First Cartel Bust, Chat	9.10 (1.65) $N = 10$	7.89 (3.62) $N = 9$	9.99 (0.04) $N = 7$	9.31 (2.26) $N = 11$
Post-First Cartel, No Chat (All Rounds)	5.66 (4.21) $N = 114$	6.21 (4.26) $N = 123$	3.48 (3.93) $N = 132$	5.49 (4.22) $N = 112$
Post-First Cartel, Chat (All Rounds)	8.42 (2.90) $N = 174$	8.04 (3.41) $N = 158$	8.24 (3.22) $N = 150$	8.71 (2.79) $N = 152$

Table 5
Relative frequency of chat topics.

	ChatNoLaw	Chat-f		Chat-m	
	First cartel	First cartel	Later cartels	First cartel	Later cartels
$P = 10$ S/P/A	0.51	0.34	0.37	0.33	0.45
$P \neq 10$ S/P/A	0.02	0.02	0.03	0.03	0.02
Price floor S	0.01	0.01	<0.01	0.01	0.01
Turn take S	0.08	0.02	0.04	<0.01	0.01
Cooperation appeal	0.15	0.13	0.13	0.10	0.12
Collective benefit	0.06	0.06	0.05	0.06	0.04
Grim trigger threat	0.02	0.02	0.03	0.02	0.01
Audit	0.00	0.04	0.07	0.03	0.05
Contract	0.09	0.04	0.04	0.02	0.02
Communication	0.01	0.05	0.07	0.05	0.07

Notes: S, P, A stand for suggestion, promise, and agreement, respectively.

This leads us to ask: what did managers talk about when in a cartel? Did the content of the conversations change post-detection? To answer these questions, we did an extensive analysis of the 19,650 chat entries in our data set. We identified the following non-mutually exclusive categories, which have theoretical relevance:

- Suggestion/promise/agreement (S/P/A) of price =10 (e.g., “Yeah, 10 and split everytime”)
- Suggestion/promise/agreement of price $\neq$ 10 (e.g. “all do 7”)
- Suggestion of a price floor (e.g. “do not price under 5”)
- Suggestion to take turns at pricing 9.99 (e.g. “We could give everyone a chance to win big alternatively”)
- Generic appeals to coordinate prices/cooperate (e.g., “don’t scam please”)
- Appeals to collective benefit/fairness (e.g., “long run we all win”)
- Grim trigger-type threats (e.g., “ill put 0 forever next time someone snakes haha (sic)”)
- Mentions of being audited (e.g., “i hope our market doesnt get audite (sic)”)
- Mentions of the variable pay (e.g., “It’s 2% variable”)
- Mention of communication (e.g., “yeah don’t chat next time”)

Table 5 summarises the relative frequency with which groups made remarks consistent with these categories in a given round. The most frequently observed categories are suggestions/promise/agreement to set $p = 10$ ($P = 10$ S/P/A), followed by appeals for price coordination/cooperation (Cooperation appeal). All other categories have very small relative frequencies. We also do not observe large changes pre- and post-first cartel, with the exception of Chat-m, where we see an increase in $P = 10$ suggestions/promises/agreements in later cartels.

The fact that we do not observe a large change in theoretically-relevant content is likely to be due to the fact that price coordination was indeed fragile. As such, subjects were constantly reminding each other of the need to cooperate, and the price at which they should do so.

5.4. Contract offers

We conclude our analysis by looking at contract choices made by shareholders at the end of Phase 1.

Result 4 (Contract Choice)**.** The probability of shareholders choosing the high-powered contract is higher in Chat-m than in ChatNoLaw. There is no difference in frequency of high-powered contract choice between Chat-m and any other treatment.

Table 6
Frequency of self-reported motives for contract choice.

	Self interest	ICC	Incentives (Non ICC)	Altruism	Own experience	Risk	Reciprocity	Other
LP	79	1	15	0	12	12	1	14
HP	20	45	4	21	23	7	0	34
Total	99	46	19	21	35	19	1	48

*Notes:*Numbers in each cell denote the number of cases in which the response by participants could be coded under a given category. Categories are not always mutually exclusive.

Support: In NoChat and Chat-m, 58% of shareholders offered high-powered contracts; in Chat-f, 61% of shareholders offered high-powered contracts. There is no statistically significant difference when doing any pairwise test of proportions (NoChat vs. Chat-m: $p = 1.000$; NoChat vs. Chat-f, $p = 0.865$; Chat-m vs. Chat-f, $p = 0.865$, Fisher's exact test). In contrast, in ChatNoLaw, the proportion of high-powered contracts chosen was equal to 36%, significantly lower than in any other treatment (ChatNoLaw vs. NoChat, ChatNoLaw vs. Chat-m: $p = 0.012$; ChatNoLaw vs. Chat-f: $p = 0.004$, Fisher's exact test).

To further our analysis of shareholder motives, we use data from a short unincentivized survey we administered to subjects after each session had concluded. One of the open-ended questions in Phase 1 sessions pertained to the reasoning behind their contract choice when in the role of shareholder. Two coders independently coded the responses based on a set of non-mutually exclusive, pre-determined categories, which we feel capture the potential motives for contract choice. Coders had an 92% agreement rate; a third coder adjudicated on the disagreements.

The first category is *Self Interest*, which includes mentions of cost minimization, or maximizing profits, or simply stating selfishness as a motive. Examples of responses which were coded under this heading include: "More profit for me."; "Because the manager only getting 2% of the revenue means more revenue for me."; "I'm greedy". Entries in which the writer stated that collusive equilibria could be sustained without incentives, so the least costly contract made the most sense, were also included under this category: "most people will eventually reach the "go 10 to win" conclusion regardless of their starting contract, so as the owner, I will gain more profit at 2% rather than 20%."

The second category is *ICC*, which refers to instances in which subjects explicitly referred to giving high-powered contracts in order to incentivize collusion. Examples included: "Realised that if managers were acting rationally then my profit would be higher if contract has 20% of revenue in, as it should encourage collusion."; "20% people more likely to cooperate, thus profits should be higher."

Interestingly, a non-trivial proportion of subjects mentioned incentives, though not in the way the ICC is predicated to operate. Some subjects argued that low-powered incentives were *less* likely to induce deviations, which in turn should lead to more price stability. We categorized these instances as *Incentives (Non-ICC)*. Examples include: "With a lower variable component, people tended to 'go rogue' less as it payed off much less and so wasn't worth it, so everyone stuck to putting 10. This means there would be a greater chance of picking a firm that had some form of profit."

The fourth category we generated was *Altruism*, which refers to instances where the subject expressed concern for the welfare of the manager, or general welfare. Examples include: "20% is the nice choice for managers."; "Give the other manager an opportunity to earn as much as possible." or "Feels more ethical even in a simulation. Plus it means that the people who play in the future have more of a chance of receiving ECU in revenue."

The fifth category was *Own Experience*, which included instances where subjects referred to their own behavior or outcomes in the markets they took part as managers as a justification for their choice. Examples include: "More money was made when variable pay was higher in my groups"; "I chose it because I earned more ECU in these sessions and so would hope that other people did too."

A non-trivial proportion of subjects mentioned risk-taking, or the riskiness of contracts, which became our sixth category, *Risk*. Example responses included "i chose 2% because it was less riskier (sic)." All responses that we could not code systematically were assigned to *Other*.

Table 6 reports the frequency of subjects' motives conditional on contract choice. The distributions of reported motives conditional on contract choice are significantly different ($\chi^2(7) = 117.9$, $p < 0.001$). Overall, the most frequently cited motive was narrow self-interest, expressed either via minimizing the amount paid to managers, or as maximizing own payoff in a narrow, non-strategic sense (in that the potential gains from collusion are not explicitly mentioned). Interestingly, a few subjects justified cost minimization on the basis that they did not believe that high-powered contracts would make a behavioral difference.[21] The overwhelming proportion (79/99 or 80%) of subjects stating this motive picked the low-powered contract; we reject the hypothesis that this outcome was the result of chance ($p < 0.001$, binomial test). The second most cited motive were ICC considerations, and almost all those subjects selected the high-powered contract. Again, we reject the hypothesis that such an outcome occurred from random chance ($p < 0.001$, binomial test). The third most cited motive was own experience as a firm manager. Two thirds of subjects citing this motive picked the high-powered contract ($p = 0.045$,

[21] The following survey responses are illustrative: "My own payoffs/decision werent (sic) really affected by my contract so as an owner i chose the most beneficial one (2%)" ; "Differences in decision-making on price were minute if at all present between the different contracts offered, so I offered the lower % paid to the managers in order to maximise my own potential profits." ; "Whether it was 2 or 20% the firms acted in the same way, this means that I will maximise my profits by giving the manager less and me (the owner) more".

binomial test). Non-ICC Incentives and Risk were cited more often by those who picked the low-powered contract ($p = 0.01$, $p = 0.19$, respectively, binomial test). We only found one response that made explicit mention to reciprocity concerns; surprisingly, it was from a subject who picked the low-powered contract: "The only downside would be if they performed low just to spite me for giving them a bad contract, but hopefully they won't do that because they still earn more from doing well."

Observation 1 (Contract Choice). *Shareholders who choose low-powered contract choices are mostly motivated by self-interest/cost minimization. Shareholders who choose high powered contracts most often cite ICC concerns and altruism as the drivers of their choices.*

6. Conclusion

The present paper extends our understanding of theory and behavior in a repeated Bertrand oligopoly in two separate, yet complementary directions. The first is by considering the Bertrand game to be part of a more general game in which the incentives of the firms' managers are determined prior to the start of the pricing supergame. The standard Bertrand game assumes away any incentive misalignment between shareholders and managers. Yet, this need not be the case for a variety of reasons. We explore the possibility that shareholders care for profit maximization enough to wish for their firms' managers to establish a price-fixing cartel.

The second, related direction is the role of antitrust legislation in curbing the propensity of firm managers to engage in collusion. We consider two forms of antitrust legislation. They are loosely modeled after the historical practices in the US on one hand, where managers are directly penalized; and most EU countries on the other hand, where, historically, only firm fines have been imposed. These differences in legal regimes expose managers to different degrees of risk, which have important consequences on the type of contract shareholders should offer to managers in order to incentivize them to engage in collusion.

The questions we address in this paper have not only academic relevance, but they also shed important insights for antitrust policy. Our model predicted very well the probability of cartel formation as a function of the legal regime. While cartel formation rates were generally high, the more a manager was exposed to risk (as a function of the legal regime), the less likely cartels were to form. Our findings suggest that legal frameworks that prosecute managers will be more successful than those that focus solely on corporate fines. This result is likely to be a lower bound on this effect since our experimental design cannot capture adequately the reputation and utility loss from imprisonment. In addition, properly implementing imprisonment would require a convicted manager to not take part in a given number of periods of a given market and some other manager to take his/her place.

Our paper thus contributes to a recent debate involving the possible criminalization of EU antitrust policy: Shaffer et al. (2015) point out that there is a world-wide trend in adding a criminal law component to antitrust policy in many areas of the world. For example, Brazil and South Africa have started to introduce individual sanctions (fines and imprisonment) for cartel offenses; also, a growing number of Latin American and Asian countries have been doing so. Moreover, our findings are consistent with Wils (2002, 2005), who discusses individual sanctions and the criminalization of cartels with reference to the EU context, concluding that imprisonment (manager liability in our paper) can increase cartel deterrence. In our future research, we plan to look more closely at the dichotomy between individual fines and jail terms in jurisdictions that have criminalized cartels. Such research could serve as a testbed of whether the US DoJ's policy shift around 2000, with increased emphasis on imprisonment, is indeed a more effective tool for cartel deterrence.

Perhaps surprisingly, high-powered contracts were not more effective than low-powered contracts at promoting cartel formation. This is the case despite the fact that the high-powered contract provided strong incentives to collude across all treatments. Assuming managers employed grim trigger strategies, they could expected to gain 14–21% greater profits by colluding on a price of 10 than by deviating. In contrast, the low-powered contract provided weaker incentives to collude – only a 2% payoff gain for NoChat, ChatNoLaw, and Chat-f, and a loss of 3% in payoffs for Chat-m, as the incentive compatibility constraint is not satisfied in that case (see Table B1 in the Appendix).

Ultimately, the incentive compatibility constraint is not as important as we hypothesized. When it is violated in the low-powered contract in Chat-m, we see only a small and non-significant drop in the capacity of firms to coordinate prices and no change in the average selling price. This could be due to other-regarding preferences that are triggered by communication. Indeed, the chat transcripts reveal that our participants often made promises to cooperate with one another ('I did 10 like i promised'; 'i'll go 10, I promise ;)') and there is evidence that individuals assign utility to keep promises (Charness and Dufwenberg, 2006; Vanberg, 2008). Furthermore, the chat data evidenced pleads for fairness when making pricing decisions ('best for us all'; 'if we all actually do the same its fair').

In fact, high-powered contracts generally led to worse pricing coordination than low-powered contracts. It could be that flatter payoff functions helped coordination by reducing the financial temptation on managers to undercut their fellow cartel members. This relative lack of effectiveness did not stop 60% of shareholders in the two main treatments from offering high-powered contracts. Many who did justified it in their answer to an open-ended post-experimental questionnaire on the basis of motivating managers to collude. In contrast, many shareholders who picked the low-powered contract stated cost-minimization as a motive, likely not believing in the effectiveness of the high-powered contract to promote cooperation, or not believing in the stability of cooperation. We note, however that both are possible equilibrium outcomes, depending on managers' discount rates.

Our results suggest that subjects do not react as expected to clearly different incentive schemes, embedded in the type of contract. However, when given a choice, subjects appear to anticipate that other (future) managers will. This raises important questions not only on the real impact of legal regime choices on cartel deterrence, but also, within a particular regime, on the effectiveness of harsher penalties. This topic certainly warrants future research.

Declaration of Competing Interest

Authors declare that they have no conflict of interest.

Data availability

Data will be made available on request.

Appendix A. Analysis of results conditional on phase

In this section we report on results from Phase 1 and Phase 2 separately. Unless noted otherwise, we will report results from random effects probit regressions (for dichotomous dependent variables) or random effects GLS regressions (for continuous dependent variables) using period/market-level observations, bootstrapping standard errors at the session level.

A1. Cartel formation

We begin by looking at the probability with which participants agreed to form a cartel as a function of treatment and phase. For each of the 432 markets in the ChatNoLaw, Chat-f and Chat-m treatments (72 markets per treatment per phase), we created an indicator variable that equals one if at least one cartel was initiated in that market. Fig. A.1 displays the estimated probability of cartel formation during the lifetime of a market for each of the four treatments across the two Phases.

In Phase 1, under both contract types, treatments are ranked on the probability of cartels being formed as: ChatNoLaw = Chat-f > Chat-m. In Phase 2, under both contract types, treatments are ranked on the probability of cartels being formed as: ChatNoLaw > Chat-f = Chat-m.

In Phase 1, under low-powered contracts, we observe significantly lower propensity to form cartels in Chat-m than either ChatNoLaw ($z = 2.512$, $p = 0.012$, MWT) or Chat-f ($z = 2.130$, $p = 0.033$, MWT). We do not observe a significant difference between ChatNoLaw and Chat-f ($z = 0.460$, $p = 0.645$, MWT). Under high-powered contracts, we see observe the same pattern, but with weaker significance (ChatNoLaw = Chat-f: $z = 0.000$, $p = 1.000$; ChatNoLaw = Chat-m: $z = 1.884$, $p = 0.060$; Chat-f = Chat-m: $z = 1.884$, $p = 0.060$, MWT).

In Phase 2, under low-powered contracts, we observe significantly higher propensity to form cartels in ChatNoLaw than Chat-f ($z = 2.302$, $p = 0.021$, MWT) and Chat-m ($z = 1.757$, $p = 0.079$, MWT). We do not observe a significant difference between Chat-f and Chat-m ($z = 0.745$, $p = 0.456$, MWT). Under high-powered contracts, we observe a similar pattern (ChatNoLaw = Chat-f: $z = 2.765$, $p = 0.006$; ChatNoLaw = Chat-m: $z = 2.302$, $p = 0.021$; Chat-f = Chat-m: $z = 0.628$, $p = 0.530$, MWT)

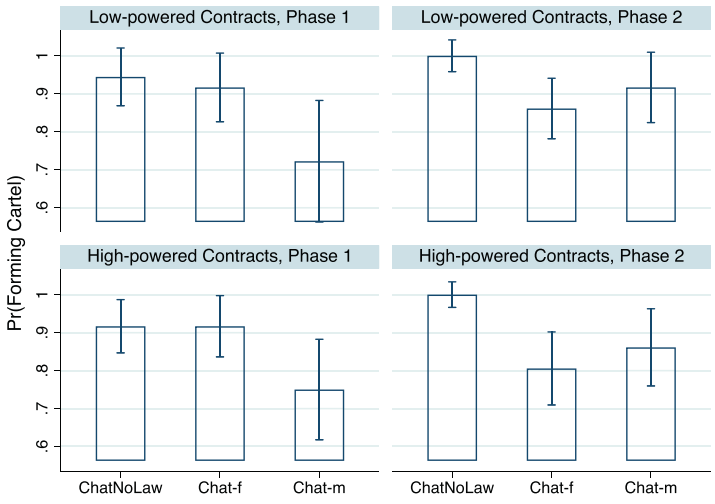


Fig. Appendix A1. Estimated probability of cartel formation by treatment.

Table Appendix A1
Determinants of individual votes for cartel formation.

Dependent variable: vote in favor of cartel formation				
	Phase 1		Phase 2	
	(1)	(2)	(3)	(4)
Chat-f	−0.497***	−0.294***	−1.120***	−0.900***
	(0.116)	(0.157)	(0.152)	(0.134)
Chat-m	−0.731***	−0.581***	−1.002***	−0.718***
	(0.111)	(0.127)	(0.139)	(0.150)
HP	−0.082	−0.016	0.176	0.314
	(0.126)	(0.150)	(0.189)	(0.229)
Chat-f × HP	−0.048	−0.112	−0.161	−0.315
	(0.157)	(0.191)	(0.212)	(0.236)
Chat-m × HP	0.169	0.103	−0.345	−0.487*
	(0.149)	(0.161)	(0.223)	(0.249)
Caught		−0.381***		−0.644***
		(0.073)		(0.091)
Risk Averse		−0.368***		−0.103*
		(0.074)		(0.075)
Period		−0.037***		−0.022*
		(0.009)		(0.013)
Supergame 2		0.891***		1.076***
		(0.103)		(0.135)
Supergame 3		1.395***		1.165***
		(0.196)		(0.263)
Supergame 4		1.600***		1.341***
		(0.272)		(0.380)
Constant	1.171***	1.191***	1.708***	1.405***
	(0.107)	(0.128)	(0.138)	(0.138)
N (markets)	3668 (216)	3668 (216)	3208 (216)	3208 (216)

Notes: Bootstrapped standard errors clustered at session level in parentheses. Data excludes the initial period of each market or any period in which a cartel was already active. ***, **, *: $p < 0.01$, $p < 0.05$, $p < 0.10$, respectively.

We next compare the probability of cartels being formed across contract types, keeping treatment constant. In Phase 1, we did not observe any significant difference in probability of cartels being formed across contract types (ChatNoLaw: $z = 0.460$, $p = 0.645$; Chat-f: $z = 0.000$, $p = 1.000$; Chat-m: $z = 0.266$, $p = 0.791$, MWT). In Phase 2, we also did not observe any significant difference in the probability of cartels being formed across contract types (ChatNoLaw: no variation in data, exact $p = 1.000$; Chat-f: $z = 0.628$, $p = 0.530$; Chat-m: $z = 0.745$, $p = 0.456$, MWT).

We complement this analysis by analyzing in more detail the voting decisions taken by managers whenever the opportunity to form a cartel arose. We estimated a random effects probit model of the decision by manager i in market k to vote in favor of forming a cartel in period t, whenever that vote was available to managers in a market—that is, excluding the very first period in a market when voting was never allowed, as well as any period in which the cartel was already active (i.e., where a chat box was opened by consensus choice).

Our first specification includes as regressors treatment dummies (Chat-f and Chat US), a dummy for high-powered contract (HP) and all relevant interactions. We consider a second specification that also includes a dummy variable for whether the cartel had been caught before (Caught), which allows us to distinguish votes for cartel re-starts from votes on initial cartel formation, as well as, supergame dummies (Supergame t), and a linear time trend (Period). The supergame dummies and time trend examine learning effects both within supergame and across supergames. Choosing to form a cartel is a risky decision, so it stands to reason that risk attitudes should play a prominent role in determining whether or not to vote for the formation of a cartel. As such, our second specification also includes a dummy variable that equals one if the manager was risk averse (Risk Averse), based on the risk-aversion elicitation done after the experiment. We estimate the two specifications separately for Phase 1 and Phase 2. Table A.1 summarizes the estimation results.

In both phases, the voting data broadly confirms the evidence on the probability of at least one cartel being formed. Estimations (1) and (3) show that the average number of votes in favor of a cartel being formed is lower for Chat-f and Chat-m than ChatNoLaw in both high-powered and low-powered cases.[22]

We next extend our analysis to consider dynamic effects. Models (2) and (5) account for whether a cartel had been busted before the voting takes place. In both Phases, the probability of voting for a cartel is significantly lower once a cartel was busted (Phase 1: $\chi^2(1) = 17.61$, $p < 0.001$; Phase 2: $\chi^2(1) = 62.37$, $p < 0.001$). The linear time trend in both Phases

[22] Phase 1, low-powered: Chat-f, $\chi^2(1) = 18.38$, $p < 0.001$; Chat-m, $\chi^2(1) = 43.03$, $p < 0.001$; Phase 1, high-powered: Chat-f, $\chi^2(1) = 18.10$, $p < 0.001$; Chat-m, $\chi^2(1) = 23.83$, $p < 0.001$; Phase 2, low-powered: Chat-f, $\chi^2(1) = 54.35$, $p < 0.001$; Chat-m, $\chi^2(1) = 52.21$, $p < 0.001$; Phase 2, high-powered: Chat-f, $\chi^2(1) = 55.62$, $p < 0.001$; Chat-m, $\chi^2(1) = 58.85$, $p < 0.001$.

Table Appendix A2
Estimated rates of price coordination in active cartels.

Dependent variable: price coordination		
	(Phase 1)	(Phase 2)
Chat-f	−0.065	−0.018
	(0.071)	(0.092)
Chat-m	−0.390***	0.109
	(0.076)	(0.071)
HP	−0.130*	−0.116
	(0.070)	(0.083)
Chat-f × HP	0.084	0.054
	(0.114)	(0.111)
Chat-m × HP	0.291***	0.126
	(0.120)	(0.098)
Supergame 2	0.093	0.181***
	(0.086)	(0.068)
Supergame 3	0.165**	0.290***
	(0.085)	(0.075)
Supergame 4	0.241***	0.360***
	(0.086)	(0.065)
Constant	0.677***	0.339***
	(0.088)	(0.079)
var(Session RE)	0.018	0.017
	(0.006)	(0.005)
var(Residual)	0.101	0.114
	(0.008)	(0.010)
N (sessions)	186 (18)	196 (18)

Notes: DV = 1 if all firms charge the same price in period t and zero otherwise. Bootstrapped standard errors clustered at session level in parentheses. ***, ** indicate $p < 0.01$, $p < 0.05$, respectively.

is very small. When estimating a marginal effect, we obtain a coefficient of −0.0095 in Phase 1 and −0.005 in Phase 2, indicating a reduction period-on-period of 0.95 and 0.05 percentage points in the probability of voting in favor of a cartel in Phase 1 and 2 respectively. The supergame dummies all have positive and highly significant coefficients. The coefficients on supergames 2–4 imply marginal effects between 0.238 and 0.393 across both phases, indicating a substantial increase in the probability of voting for a cartel vis-à-vis supergame 1. This supports the conjecture that participants learn the strategic incentives of the supergame with more experience.

We conclude by examining the role of risk aversion. The estimated coefficient on the risk aversion dummy in both Phases is negative and significant in both Phases (Phase 1: $\chi^2(1) = 22.70, p < 0.001$; Phase 2: $\chi^2(1) = 3.61, p = 0.085$). This is consistent with the intuition that risk averse managers are less likely to vote for a cartel.

In short, once detected, cartels are less likely to re-form. Experience with the indefinitely repeated game leads to more cartels being formed.

A2. Prices

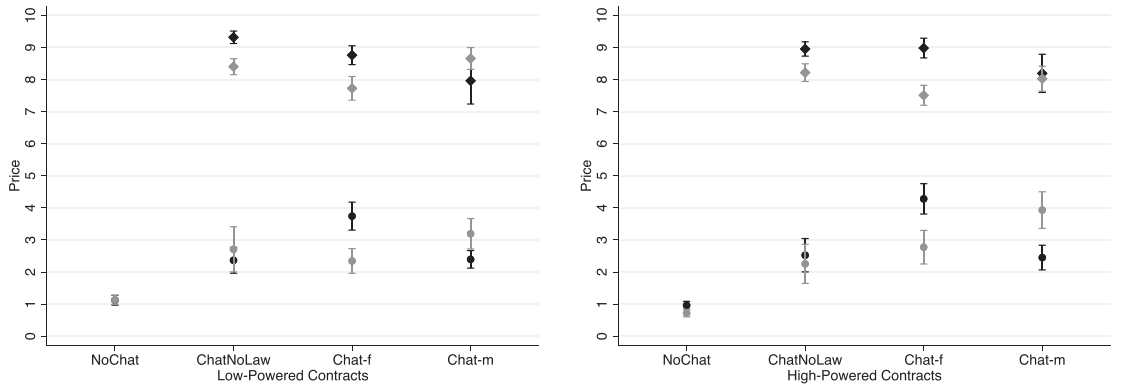
We now turn to pricing behavior conditional on the cartel being active or not. We start by testing Hypothesis 3. To do so, we quantify the proportion of periods in which firms were able to coordinate and charge the same price in a given period, which we denote as price coordination.

When firms could not or did not form a cartel, they were able to coordinate on a price 6% of the time. In all those cases, the price they coordinated on was 10. In contrast, firms were able to charge the same price in 57% of periods in which a cartel was active, a significantly higher proportion (Phase 1: $z = 5.387, p < 0.001$; Phase 2: $z = 5.465, p < 0.001$). Of those instances, the chosen price was almost always 10.[23] Table A.2 presents mixed effects estimates of rate of coordination on prices conditional on cartels being active as a function of contract type, treatment in each phase. The unit of observation is the relative frequency of price coordination in market j.

Both regressions yield qualitatively similar results. The exception is Chat-m, which in Phase 1 has a lower frequency of coordination under low powered contracts than ChatNoLaw ($\chi^2(1) = 25.82, p < 0.001$) and a higher frequency under high-powered contracts ($\chi^2(1) = 6.03, p = 0.014$). No such differences exist in Phase 2 data.

We next analyze average selling prices. Because of the large number of parameters being estimated, we opted to show our estimation results visually. Fig. A.2 displays estimated average selling prices conditional on treatment, on a cartel being active that period, and contract type. As expected, average selling prices are lowest in NoChat, irrespective of Phase and con-

[23] The cases in which firms coordinated on a different price were $p = 9.99$, 7.5, 4.5, 3 (one observation each), as well as 5 and 4 (two observations each).



(a) Low-powered contracts (b) High-powered contracts

Fig. Appendix A2. Average selling prices.

tract type. Also as expected, conditional on treatment, average selling prices are higher when a cartel is active, irrespective of Phase and contract type.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jebo.2022.10.031.

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