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Impact of Leniency Programs and Follow-on Damages on Cartel Deterrence

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Abstract

Abstract: Leniency programs encourage corporate cooperation with antitrust authorities by offering immunity or fine reductions for reporting illegal cartels. While prior studies suggest these programs discourage collusion and destabilize existing cartels, experimental evidence in environments with unrestricted communication indicates that the effectiveness of leniency is not clear-cut. We conduct a laboratory experiment in such an environment to examine the interaction between leniency programs and follow-on private damages, proposing the use of Fair Funds to maintain victim compensation and preserve incentives for leniency application. Contrary to theoretical predictions, we find that the prospect of private damages can increase cartel formation, though this effect is mitigated when our Fair Funds compensation scheme is introduced. In addition, leniency applications decline when private damages are introduced, but this decline is partially offset by the presence of Fair Funds.

Keywords: Antitrust, Illegal Cartels, Leniency Programs, Private Damages.

JEL codes: C92; D03; K21; K42; L41.

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1. Introduction

Leniency programs are designed to encourage companies to cooperate with antitrust authorities (AA) in exchange for a reduction or exemption from administrative sanctions (such as fines) that might otherwise be imposed due to their involvement in a cartel. Theoretical research has long shown that leniency reduces the number of cartels and promotes their instability (Motta and Polo, 2003; Harrington Jr, 2008). However, empirical findings from experimental studies are less clear-cut. Earlier experiments, whose designs restrict communication between participants to price messages, generally confirm that leniency lowers collusion (Apesteguia *et al.*, 2007; Bigoni *et al.*, 2012, 2015). By contrast, more recent studies that allow subjects to communicate freely report no stronger deterrent effect of leniency than a baseline antitrust enforcement (Dijkstra *et al.*, 2021; Andres *et al.*, 2021).[1]

This empirical literature treats leniency through the lens of public enforcement alone, whereas cartel policy now operates alongside an increasingly active private-enforcement layer. In the United States, this dual system dates back to the Sherman (1890) and Clayton (1914) Acts, which grant treble-damage actions to any injured party. In Europe however, private actions remain a relatively recent phenomenon, formalized by the Antitrust Damages Directive 2014/104/EU and implemented across all member states only in 2017. The Directive entitles cartel victims to full compensation and imposes joint-and-several liability on cartel members. This raises expected penalties but also exposes potential leniency applicants to sizable follow-on claims. This combination can have opposing effects. On the one hand, by increasing the expected cost of detection, it may deter firms from forming cartels. On the other hand, it can also weaken incentives to self-report, since leniency no longer eliminates the full burden of liability, as even the reporting firm remains exposed to significant private damages. Consequently, colluding firms may hesitate to apply for leniency, fearing that cooperation with antitrust authorities will amplify their exposure to follow-on claims. Building on a formal model, we conduct a laboratory experiment that isolates the behavioral responses to the incentive conflict between leniency and private damages. The design tracks how the prospect of private damages affects cartel formation and leniency requests. We also test two

[1] A comprehensive review of how such design features shape experimental findings, and of the external-validity issues they raise, is provided by Law *et al.* (2025).

policy refinements in our experiment. The first, through our *Transfer* treatment, caps the reporting firm's civil liability by reallocating damages to non-reporting cartel members. The second, through our *Fair Funds* treatment, compensates victims through an external fund that fully shields the leniency applicant from follow-on claims without transferring its liability to the remaining parties.
Private damages litigation has become highly visible in Europe. Laborde (2021) record 299 damages actions to date, almost all of them follow-on claims (57% triggered by national-authority decisions and 40% by Commission decisions, with only 2% stand-alone). A headline example is the European Truck-Manufacturers cartel: after the Commission's 2016 infringement decision, class actions were filed in the UK, Germany, the Netherlands and Spain, seeking compensation estimated at 5 to 20% of truck purchase prices; Scania's final appeal was dismissed by the Court of Justice in 2024, and claims remain open until 2029. Even MAN, the first leniency applicant that escaped all fines, faces multi-million-euro damage awards in Spain and continues to defend follow-on suits across several EU jurisdictions (Marvão and Spagnolo, 2024). Other recent high-profile cases further illustrate the growing relevance of private enforcement. In the Dutch elevators cartel (2007-2022), Kone, the leniency recipient, avoided fines but was nonetheless targeted in successful follow-on litigation initiated by a Dutch foundation acting on behalf of public housing corporations. Similarly, in the French linoleum flooring cartel, the 2017 decision resulted in a €300 million fine, and over 300 public hospitals subsequently claimed €200 million in damages. In 2024, the Paris Court of Appeal formally recognized that these cartel practices had caused compensable harm in its follow-on proceedings. Cases of this scale put the prospect of substantial civil liability front-and-center for firms. It therefore appears reasonable to consider that cartel members weigh the risk of follow-on damages when deciding whether to seek leniency.

Our experimental study is not the first to examine leniency and private damages in the laboratory. Bodnar *et al.* (2023) introduce follow-on liability into a repeated cartel game with leniency, and study two communication regimes: a structured setting where subjects may announce only intended prices, and an unrestricted chat that also lets them coordinate on whether and when to self-report. In the structured condition, Bodnar *et al.* (2023) find two effects consistent with theory: introducing damages actions lowers cartel formation, but the cartels that do form are more stable as private damages reduce incentives to seek leniency. These effects are markedly weaker in the free-chat condition. As the authors note, greater weight may be placed on the free-chat condition since "communication is often open and in person in real-world cartels". Following the framework developed by Dijkstra *et al.* (2021), we consider a homogeneous repeated Bertrand duopoly model, in which firms may engage in open-ended communication at the beginning of each round to facilitate collusion. This communication channel serves to build trust and coordinate cartel formation. Our objective is to examine how the potential for civil damage claims affects this trust, and, consequently, firms' incentives to collude or report.

According to both the theoretical model of Buccirossi *et al.* (2020) and the experimental evidence of Bodnar *et al.* (2023), reducing the lenient firm's exposure to civil damages can help preserve the effectiveness of leniency programs. Their benchmark is the 2009 Hungarian civil regulation, in force before the EU Directive, which made an immunity recipient liable for compensation only if the other cartel members were insolvent. This effectively freed the lenient party from civil liability while still allowing claimants to use the evidence it provided to the authority.[2] Buccirossi *et al.* (2020) formally demonstrate that this "liability of last resort" strengthens deterrence, and Bodnar *et al.* (2023) provide some experimental evidence that shielding the lenient party from damages mitigates the cartel-stabilizing effect observed under full joint liability. In their "PDC+" treatment, the reporting firm is granted both immunity from fines and a transfer of its private-damages liability to the remaining cartel members, which increases incentives to report relative to a baseline where liability is retained. Motivated by these results, our experiment includes a similar *Transfer* treatment and introduces a second approach, *Fair Funds*. This mechanism, inspired by the "Fair Funds" system of the U.S. Securities and Exchange Commission (SEC) and proposed by Hornkohl (2024), removes civil liability from the lenient

[2]In the United States, the Antitrust Criminal Penalty Enhancement and Reform Act of 2004 (ACPERA) limits the civil liability of leniency applicants to actual damages, rather than treble damages, and grants civil plaintiffs access to information from the DOJ's investigation. Under EU competition law, leniency applicants are exempt from joint and several liability and are only liable for the harm they directly or indirectly caused (only if other infringing parties are able to compensate the victims). Unlike the U.S. ACPERA regime, the EU framework, including its implementation in French law, strictly limits the use of lenient information in follow-on civil litigation. Specifically, leniency statements are not accessible in damages actions without the applicant's consent (Article L.481 of the French Commercial Code). This non-disclosure principle stands in clear contrast to the efficient disclosure-oriented approach advocated in Buccirossi *et al.* (2020) and Bodnar *et al.* (2023). In both our theoretical and experimental frameworks, we abstract from this institutional dimension: the potential use of leniency-related evidence in follow-on civil litigation is not considered, as our analysis focuses exclusively on the allocation of civil liability and its implications for reporting incentives and cartel formation.

party without shifting it to other cartel members. Instead, victims are compensated through a public fund financed by antitrust fines collected by the authority. The goal is twofold: ensure that victims remain fully compensated and strengthen incentives for cartel members to self-report by reducing the cost of cooperation with antitrust authorities. However, because the mechanism also reduces overall expected penalties for firms, it could inadvertently make collusion more attractive, a trade-off that we explore in the experiment.

Another originality of our framework, compared to Bodnar *et al.* (2023), is that firms may apply for leniency either before or after an investigation has been announced. This aspect is important because a substantial share of leniency applications in practice occurs after the start of an investigation, yet this timing has received little attention in previous experimental work. The incentives to report differ significantly in this context: once an investigation is underway, applying for leniency is no longer simply a deviation strategy in a repeated game but can become a rational response to minimize expected liability. We examine how the prospect of follow-on damages can reduce or even eliminate these incentives during ongoing investigations. To restore incentives, and following the approach suggested by Bodnar *et al.* (2023), we consider a mechanism that exempts the lenient applicant from civil liability and shifts this liability to non-reporting cartel members. In our setting, however, this mechanism does not change equilibrium outcomes relative to the benchmark without exemptions. The reason is that both firms have an equal opportunity to report, which distinguishes our framework from that of Bodnar *et al.* (2023), although the result is consistent with the theoretical predictions of Dijkstra *et al.* (2021).

Our empirical findings reveal an unexpected dynamic: the mere introduction of private damages is associated with an increased propensity for cartel formation. However, this effect is notably attenuated under the Fair Funds approach. Furthermore, we find that the presence of private damages does not lead to higher rates of deviation from cartel pricing. Consistent with our theoretical model, we also observe a decline in leniency applications when civil liability is imposed, an effect that is partially offset in the presence of the Fair Funds compensation scheme. When focusing on markets under investigation, we also show that reporting is higher under the Fair Funds approach in comparison with other civil liability models.

The paper is organized as follows. Section 2 presents the theoretical predictions.

Section 3 presents the design of the experiment. Section 4 outlines our main results and Section 5 concludes.

2. Theoretical foundation

In this section, we present the theoretical model and outline our research questions, followed by the formulation of a set of testable hypotheses for our experimental design.[3]

We consider an infinitely repeated Bertrand price competition game between two symmetric firms with common discount factor δ. In each period, firms first decide whether to communicate in order to coordinate on a collusive price or compete independently. Communication in the first period is both necessary for successful collusion and constitutes sufficient evidence for the authority to impose a fine if the market is investigated (or reported) at any point in the future.

In the stage game, the unique Bertrand-Nash equilibrium is for both firms to set a price of 1, yielding a per-firm profit of $\Pi_B = 0.5$. Joint-profit maximization occurs at a price of 10, which gives each firm $\Pi_C = 5$. A unilateral deviation from this collusive price, by optimally setting $p = 9$, yields $\Pi_D = 9$ for the deviator and $\Pi' = 0$ for the other firm. For our experiment, prices are restricted to integers between 1 and 10; therefore, we limit our theoretical analysis to this range.

Enforcement risk is introduced as follows. In each period, the antitrust authority initiates an investigation with probability $\rho = 0.2$. Following an investigation, the cartel is detected with probability $\alpha = 0.75$. Detection leads to an individual fine of $F = 5$ per firm. Firms may apply for leniency either before or after the authority initiates an investigation. If a cartel member applies for leniency, the report guarantees detection of the cartel. For the first leniency applicant, a pre-investigation report grants full immunity from fines, while a post-investigation report reduces the fine to $f = 1$. We assume that when both firms apply for leniency, each has an equal probability of being recognized as the first successful applicant and thus eligible for immunity. This framework constitutes our *Baseline*, which does not consider the

[3] Appendix A provides a detailed analysis of the theoretical framework, incorporating the main features of our experimental design. Based on this framework, we derive hypotheses about treatment differences in cartel formation and reporting behavior. To generate these predictions, we compute expected payoffs numerically using the parameter values from our experimental design and equilibrium strategies.

threat of private damage claims.
When designing their collusive strategy, firms can either (i) collude and commit to report if an investigation is announced (CR strategy) or (ii) collude and commit not to report even if an investigation is announced (CNR strategy). Collusion is sustained through grim-trigger strategies (Friedman, 1971): any deviation from the collusive price, any off-path leniency report under CNR, or any pre-investigation report when collusion was agreed, triggers a permanent reversion to the Bertrand-Nash equilibrium. Finally, we assume that detection does not necessarily mark the end for the repeated interaction: once a market is dismantled by the authority, firms can communicate again in later periods, so cartels may re-form.

To our *Baseline* scenario, where detected firms only face fines, we add three additional treatments introducing the possibility for cartel victims to claim damages in a civil court. In all three treatments, damages correspond to the harm caused by the cartel and are calculated as the difference between the collusive price and the competitive price in the period of detection. For simplicity, as with fines, damages are not cumulative across periods and apply only when the cartel is detected.
The three scenarios differ in how civil liability is allocated when at least one firm applied for leniency:

Shared Liability (*Shared*). Both cartel members are jointly liable for damages. Since we assume symmetric equilibrium play, each firm pays 50% of the total damage amount D, which corresponds to the harm it individually caused. A firm applying for leniency still obtains exemption from the fine, but must pay its full share of the damages.

Transfer of Liability (*Transfer*). Under this scenario, a firm applying for leniency obtains not only full exemption from the fine F (or pays the reduced fine f in the case of post-investigation reporting) but is also completely exempt from liability for private damages. The entire damages amount is then transferred to the non-reporting firm. If both firms apply for leniency, each has a 50% chance of being absolved. This mechanism is intended to reinforce reporting incentives by removing civil liability from the leniency applicant.

Fair Funds Mechanism (*Fair Funds*). To avoid placing the entire burden of damages on the non-reporting firm when the other obtains leniency, this scenario introduces a third-party fund to cover the damages exempted for the lenient firm. This ensures that victims are fully compensated without disproportionately penalizing one cartel member. The rationale is twofold: (i) to maintain some level of fairness in the allocation of liability and (ii) to strengthen incentives for self-reporting by eliminating the fear of shifting the entire financial burden onto the other firm.

Our first two hypotheses address how the introduction of private damages affects the effectiveness of the leniency program, both in terms of cartel formation and pricing behavior. We then turn to the incentives to self-report, either before or after an investigation has been initiated.
Based on our theoretical analysis (see Appendix A), the CR strategy dominates CNR in the *Fair Funds* and *Baseline* scenarios, whereas the reverse holds in *Shared* and *Transfer*. Across all scenarios, forming a cartel is a perfect subgame equilibrium and leads to a higher discounted present value than in a competitive setting. This value, however, varies by treatment: it is highest in *Baseline*, followed by *Fair Funds*, and lowest in *Shared and Transfer*, which provide the same expected value in our setting.[4]

Hypothesis 1. *Introducing private damage claims reduces the incentive to form a cartel. This effect is strongest in the Shared and Transfer scenarios, and weaker under the Fair Funds scenario.*

The viability of a cartel depends on its stability, which is itself determined by the strength of the incentive to maintain a collusive pricing scheme agreed upon by the participants. Indeed, although firms, at equilibrium, adhere to the collusive strategy in all the situations considered, the robustness of this stability may vary according to the stringency of the incentive constraint. This constraint is assessed by measuring the gap between the commitment to the collusive agreement and the temptation to deviate by reducing the collusive price and reporting the cartel to the antitrust authority. More specifically, the minimum threshold of the discount factor required to sustain a repeated collusive agreement may vary across scenarios, and according to theory, a higher threshold reflects reduced stability and an increased temptation to deviate. After measuring this gap in the theoretical model (see appendix), we

[4] This result contrasts with Bodnar *et al.* (2023) because, in our model, both firms, and not only the leniency applicant, have an incentive to report following a defection by one party.

find that the introduction of private enforcement reduces the stability of cartels.

Hypothesis 2. *Private damage claims increase the incentives to deviate. This effect is stronger in Shared and Transfer where the incentives are the same, compared to the Fair Funds scenario.*

At first glance, our hypothesis appears to contradict Bodnar *et al.* (2023), who argues that private damages reduce the incentive to deviate. This difference, however, stems from our consideration of reporting after an antitrust investigation, a scenario not addressed in their framework. In our Shared and Transfer scenarios, we will show that reporting during an investigation is not optimal, unlike in Baseline. This behavioral variation increases the deviation incentives gap, making Baseline more stable than the other scenarios, contrary to Bodnar *et al.* (2023). Furthermore, the Fair Funds scenario, which increases the incentive to report in the event of an investigation compared to Baseline (see subgame), thereby tightens the incentive constraint relative to the Baseline, resulting in a stronger incentive to deviate.

Regarding now the decision to report the cartel, it can occur at two levels: in case of a deviation and in case of an investigation. In the first scenario, the deviating party always has an incentive to report, regardless of the circumstances, but the other party also has a particular incentive to report, particularly in the Transfer scenario. However, based on the arguments presented in Bodnar *et al.* (2023), the incentive to report should decrease when the cost of reporting supported by the deviating party increases. Compared to the *Baseline* scenario, the cost to the deviating party is higher in all other scenarios. Nevertheless, the cost of not reporting is highest in the *Transfer* scenario compared to *Fair-Funds* and *Shared*, which provides a stronger incentive to report in *Transfer*. In case of an investigation, we know that the CR strategy dominates CNR in *Fair Funds* and *Baseline*, which is the opposite in *Shared* and *Transfer*. We can thus formulate the following hypotheses:

Hypothesis 3. *After an investigation, the incentive to report is stronger in Baseline and Fair Funds than in Shared and Transfer where the incentives are slightly stronger than in Shared.*

Hypothesis 4. *Following a defection, leniency request happens the most in Baseline and is less frequent in Shared and Fair Funds than in Transfer.*

Following the three previous hypotheses, we can announce:

Hypothesis 5. *a. Reporting will always be highest in the Baseline condition compared to all scenarios involving private damages. Among these, it will be less significant in the Shared condition than in the Fair Funds and Transfer conditions.*
b. Comparing the Fair Funds and Transfer conditions is more challenging; however, since defecting is not optimal in either scenario, we consider, based on Hypothesis 3, that reporting will be globally higher under the Fair Funds condition.

3. Experimental Design

Our experimental setup is close to Dijkstra *et al.* (2021), with the main difference that we introduce private damage claims in three independent treatments. Two subjects engage in a repeated Bertrand duopoly, where free communication is allowed in each round. If both subjects choose to communicate, a cartel is formed. The cartel remains undetected by the AA as long as this communication is not reported. Any reporting or detection of the cartel results in its dissolution. After communication, subjects set their prices independently, and the market price is defined as the lowest declared price. An antitrust investigation may then be initiated. Subjects are informed of the market price and whether an investigation has occurred, after which they can decide whether to apply for leniency. Opting for leniency guarantees cartel detection, while in the absence of leniency applications, the AA may still uncover evidence and impose fines.

We deliberately designed an environment conducive to cartel formation, as leniency programs are most effective in such settings. Consequently, we concentrate on Bertrand duopolies, a market structure known for its susceptibility to collusion. Furthermore, we allow subjects to apply for leniency even after an investigation has been initiated. This feature enables them, during communication, to coordinate not only on prices but also on whether they will report if an investigation occurs (CR strategy) or not (CNR strategy).

We employ fixed matching, where each subject interacts with the same competitor across all periods. A subject pair is referred to as a market. Participants play for at least 20 periods, after which the experiment ends with a 15% probability in each subsequent period, determined by an independent random draw for each group. As a result, the total duration of play varies across markets.

We describe here the different stages of a round in our experiment, with the differences between each treatment.

Stage 1: Each participant chooses whether to communicate by selecting either 'YES' or 'NO'.

Stage 2: If both participants choose 'YES', they enter a computer chat for 60 seconds. A participant who opts not to communicate does not learn the other participant's decision. However, if a participant chooses to communicate, they can inevitably infer the other participant's choice.

In our experiment, any communication implies the formation of a cartel. Therefore, participants who communicate are subject to potential fines regardless of the chat content. A cartel ends only if it is detected by the AA. Participants can be penalized for current or past undetected communication.

Stage 3: Participants select a price from the set 1, 2, ..., 10. At the end of this stage, both individual prices and the resulting market price (defined as the lowest of the two) are revealed.

Stage 4: There is a probability $\rho = 0.20$ that the AA initiates an investigation, and participants are informed when this occurs. Following this, they decide whether to REPORT or NOT REPORT. Reporting is allowed whether an investigation is initiated or not. Reporting entails a cost of $c = 1$ currency unit. If no report is made, the AA detects the cartel with probability $\alpha = 0.75$. if a report is made, detection is guaranteed. Fines are fixed at $F = 5$. If only one participant reports, their fine is reduced to $f = 1$ in the case of an investigation and to 0 otherwise. If both report, the fine is split evenly between both parties. Although bankruptcy could theoretically occur, it was not an issue in our experiment.

In the *Baseline* treatment, the round ends at this point, and participants receive a summary of all actions taken during the round along with their payoff. An additional fifth stage applies only to the *Shared*, *Transfer*, and *Fair Funds* treatments.

Stage 5 (*Shared*): After detection or reporting of the cartel, there is a risk of facing private damages with probability $\beta = 0.95$. In such cases, each participant pays 50% of the total damages, calculated as the difference between the collusive price and the competitive price.

Stage 5 (*Transfer*): After detection or reporting, with probability $\beta = 0.95$, the non-lenient participant bears the entire amount of private damages.

Stage 5 (*Fair Funds*): After detection or reporting, with probability $\beta = 0.95$, private damages owed by the lenient participant are fully covered by "Fair Funds".

The experiment was conducted at the CRESE Experimental Economics Laboratory, University Marie and Louis Pasteur, France. The participants were recruited by email from undergraduate and graduate students in various fields (including notably economics, business, law, and science). Each participant was assigned a computer upon arrival, by drawing a card in a box. No student could participate in more than one session and the experimenters were the same for all of the sessions. Clear instructions were provided verbally and in print at the beginning of the experiment. Before the start, participants had to correctly answer questions on their computer to confirm their understanding. The experiment was programmed using OTree (Chen *et al.*, 2016). We apply a conversion rate of €1 for 5 ECU (experimental currency unit). Average earnings were €24.92, including a €3 show-up fee. Sessions lasted between 60 and 90 minutes.

Nine experimental sessions were conducted with a total of 118 participants. Each session lasted at least 20 periods. From period 20 onward, we implemented a random termination rule under which the session ended with a 15% probability in each subsequent period. All sessions included two treatments, one of which was always the Baseline. At the start of period 11, new instructions were provided to announce a “change of rule”, corresponding to the introduction of the second treatment. Participants knew from the start that a rule change might occur, but the nature of this change was not disclosed. Table 1 summarizes the treatment sequence across sessions. In sessions 1 to 3, participants first played the Baseline treatment, followed by a treatment with private damage claims. Sessions 4 to 9 varied the order of treatments.

Session	**Periods 1-10**	**Periods 11+**	**Number of participants**
Session 1	Baseline	Shared	14
Session 2	Baseline	Transfer	14
Session 3	Baseline	Fair Funds	12
Session 4	Shared	Baseline	10
Session 5	Transfer	Baseline	14
Session 6	Fair Funds	Baseline	14
Session 7	Fair Funds	Baseline	14
Session 8	Shared	Baseline	14
Session 9	Transfer	Baseline	12

Table 1: Overview of the experimental treatments

4. Results

Given that our hypotheses are primarily based on a comparative statics approach, we direct our focus towards the between-subject aspect of our design in the initial ten periods of the game, where subjects have no prior experience playing together or from other contexts. In contrast, this examination becomes complex in the experiment's second stage, starting from period 10. After this point, decisions to collude and price-setting strategies may be influenced by the relationships developed under the previous treatment.

4.1. The extent of collusion We first study cartel incidence under our four experimental treatments. We focus our analysis on two measures. "Propensity to collude" is a variable defined at the individual level. It takes one if the individual express a willingness to communicate at the given period, or if the individual is already part of a detectable cartel. "Cartel incidence" is a variable defined at the independent observation level. It takes one if a cartel is formed at the given period, or if a cartel formed previously formed is still undetected at the start of the given period. By construction, the propensity to collude is always higher than the cartel incidence, as a cartel is only formed when the two matched subjects express a willingness to communicate.

Table 2: Comparative analysis of cartel incidence

Indicator	Treatment	N	Count (%)
Propensity to collude	Baseline	400	196 (49.00%)
	Shared	240	154 (64.17%)
	Transfer	260	96 (71.15%)
	Fair Funds	280	185 (54.29%)
Cartel incidence	Baseline	200	53 (26.50%)
	Shared	120	58 (48.33%)
	Transfer	130	77 (59.23%)
	Fair Funds	140	49 (35.00%)

Note: The reported figures consider only periods 1 to 10 of the experiment.

Table 2 reports, for all interactions in periods 1 to 10, the propensity to collude and the proportion of markets that form a cartel. Hypothesis 1 posits that private damage claims should diminish the incentives to collude; however, our experimental data suggests otherwise. The raw introduction of private damage claims in the

Shared treatment tend to increase the propensity to collude and thereby fosters cartel incidence (from 26.50% to 48.33%, WMW test, $p = 0.037$). Similarly, when extending the leniency program to include private damages, as seen in the *Transfer* treatment, we observe a similar increase in collusion (from 26.50% to 59.23%, WMW test, $p = 0.009$). In the *Fair Funds* treatment, the incidence of cartels appears more aligned with the *Baseline* treatment (from 26.50% to 35.00%, WMW test, $p = 0.375$) than with the other treatments with private damages. Cartel incidence is lower, although not significantly, compared to the *Shared* treatment (48.33% versus 35.00%, WMW test, $p = 0.243$), or the *Transfer* treatment (59.23% to 35.00%, WMW test, $p = 0.057$). Consistent with Hypothesis 1, we observe some evidence of equivalence in collusion between the *Shared* and *Transfer* treatments, whereas the proportion of cartels tend to be lower in the *Fair Funds* treatment.

It appears at first glance that the extent of collusion varies between treatments, especially when comparing *Baseline* and *Fair Funds* to *Shared* and *Transfer*. However, the non-parametric tests we perform provide only weak evidence for these treatment differences. This outcome is not entirely unexpected, given the nature of these tests. They necessitate data aggregation at the group level. While aggregation ensures independence between observations, it also considerably diminishes the power of the tests. Moreover, properly identifying treatment effect requires controlling for additional covariates to fully understand their impact. To gain a more nuanced understanding of our data, we turn to econometric regression analysis. This approach allows us to investigate individual-level data and a broader range of potentially confounding variables.

Table 3 reports estimates from random-effect Probit regressions on both the propensity to collude, in the first column, and cartel incidence, in the second column. We account for the evolution of behavior and decision-making over time in our experiment by using a quadratic form to model period effects. We choose to do so because collusion dynamics may follow a non-linear pattern. Indeed, the emergence of new cartels may be more common in the initial stages, diminishing as time progresses, while the collapse of cartels may only occur after a certain duration. Control for individual characteristics include gender, age, and risk attitude as measured by the Eckel and Grossman (2002) risk elicitation procedure. In the second model, which is estimated at the pair level, individual characteristics were averaged for each group. Estimates reported in Table 3 confirm our previous findings: both propensity to collude and cartel incidence increase on average in both *Shared* and *Transfer* treatments, compared to the *Baseline* treatment. The extent of this increase is lower, and show no statistical significance in the *Fair Funds* treatment. Using a linear combination test, we can also affirm that cartel incidence is significantly lower under Fair Funds than under Baseline ($p = 0.043$) or than under Transfer ($p < 0.001$). There is no significant difference in cartel incidence between *Shared* and *Transfer* ($p = 0.331$). Moreover, the progression of cartel incidence over time displays an inverted U-shaped curve. Initially, cartel incidence rises as participants reach agreements to communicate. However, in subsequent periods, once cartels are either reported or detected, they tend not to re-form. This observation does not necessarily mean that subjects stop colluding on pricing in later periods. Instead, data suggest that participants likely resort to tacit collusion after their cartels have been detected.

Table 3: Cartel incidence - Random-effect probit regressions

Treatment	Propensity to collude	Cartel incidence
Baseline	Ref.	Ref.
Shared	0.614*	1.027***
	(0.338)	(0.391)
Transfer	1.012***	1.289***
	(0.348)	(0.292)
Fair Funds	0.222	0.362
	(0.346)	(0.337)
Period	0.187**	0.291***
	(0.092)	(0.078)
Period^2	-0.024***	-0.028***
	(0.008)	(0.008)
Control for ind. characteristics	Yes	Yes
N	1,180	590

Note: Subsample considering only the first ten periods of the game. Standard errors, clustered at the session level, are displayed in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Result 1. *Contrary to our predictions, the introduction of private damage claims in the Shared and Transfer treatments leads to an increased propensity for cartel formation. However, collusive behavior in the Fair Funds treatment aligns more closely with the baseline scenario without private damage claims, as predicted.*

This result appears to contrast with the findings of Bodnar *et al.* (2023), who report that the absence of private damage claims (NoPDC) leads to a significantly higher propensity to collude compared to their PDC (equivalent to our *Shared*) and PDC+ (equivalent to our *Transfer*) treatments. A plausible explanation for this discrepancy lies in the use of structured communication in Bodnar *et al.* (2023). In our experimental setting, communication is free and unstructured, which let participants build stronger trust. Introducing private damage claims may reinforce this trust, as cartel members face lower incentives to report the cartel. Such an interpretation aligns with the observations of Dijkstra *et al.* (2021).

Moreover, in the model of Bodnar *et al.* (2023), in the event of a deviation from the agreed price, only the deviating party has an incentive to report the cartel. In contrast, in our model, following a price deviation, both the deviating party and the other party have an incentive to report, leading to a higher expected cost of reporting. Anticipating this, cartel participants understand that the incentive to report in the event of a deviation is reduced. This can strengthen mutual trust *ex-ante* and further encourage the formation of cartels.

4.2. Prices Table 4 reports the average market price in our four treatments. The market price is defined as the lowest price declared among the two matched subjects; its value ranges from 1 to 10. We observe that market prices are on average lower (although not significantly) under the *Baseline* treatment, compared to *Shared* (WMW test, $p = 0.115$), *Transfer* (WMW test, $p = 0.105$), and *Fair Funds* (WMW test, $p = 0.071$). This is a direct consequence of cartel incidence being lower in the absence of private damage claims, as documented above. Indeed, when focusing on colluding markets, we do not find any treatment difference in price-setting.

Table 4 also reports the proportion of defection, defined as interactions where at least one member of the cartel deviates from the agreed price. Note, that agreed prices have been extracted from the chat data. Because we use free-form communication, our data includes few groups (10) where cartels are formed but no agreement on price is found. Reported proportions correspond to deviation in the 212 interactions in cartels for which a collusion price has been explicitly agreed upon. Our experimental data do not provide any support for Hypothesis 2. Defection rates are low across our four treatments. The observed defection rates, lying between 4% and 13%, align with findings from Dijkstra *et al.* (2021), which highlights rather low defection rates in settings featuring free-form communication. No treatment difference is statistically significant.

Result 2. *Market prices are on average the lowest in the absence of private damage*

Table 4: Market Prices and Occurrence of Price Deviation

All markets	
Treatment	Market Price
Baseline	5.510 (3.702)
Shared	7.167 (3.665)
Transfer	7.192 (3.357)
Fair Funds	7.300 (3.417)

Colluding markets only		
Treatment	Market Price	Deviation
Baseline	8.245 (3.292)	10.64%
Shared	9.289 (1.802)	3.64%
Transfer	7.922 (3.003)	4.62%
Fair Funds	8.306 (2.639)	13.33%

claims. Contrary to our expectations, the introduction of private damages does not increase the propensity to deviate from collusive prices.

An important observation concerns the relatively high average prices in the *Fair Funds* treatment. One might expect fewer cartels to translate into lower prices. Yet, prices in *Fair Funds* remain comparable to those observed in other treatments introducing private damages. A similar pattern can be observed in related experimental studies that allow for tacit collusion. For instance, in the "CHAT" treatment of Bodnar *et al.* (2023), the introduction of private damages tends to reduce cartel formation but see at the same time a slight increase in market prices. The role of free-form communication in the occurrence of tacit collusion is extensively discussed in Law *et al.* (2025). Given the limited sample size and the absence of significant treatment differences, we caution against overinterpreting the price variations reported in Table 4.

4.3. Leniency applications Table 5 displays data regarding the reporting decisions under our four treatments for all cartels, and separating interactions with and without an announced investigation. In this part of the analysis, we consider data from the twenty first periods of the experiment. This contrasts with the previous analysis, which ignored periods 11 to 20. There are two reasons for this change in our empirical approach. First, we believe that the decision to report or not the cartel should be affected by the events (*e.g.* price-setting, investigation, defection) occurring in a given round, but not by previous interactions. This could not be said from cartel formation or price-setting, as we suspect that periods 11 to 20 are characterized by a large share of tacit collusion, dependent on the development of the relationship in the ten first periods. Second, studying decisions to report the cartel narrows our sample to those observations where a cartel is detectable. Limiting ourselves to the ten first observations would considerably reduce our sample size, and make any statistical inference difficult. For consistency, we report statistics regarding leniency applications in period 1 to 10 only in Table B.1 in Appendix B. We observe that the extent of leniency application, and how it compares between different treatments, is similar to the data reported in Table 5.

N indicates the number of observations for which a cartel member could decide to apply, or not, for leniency. It varies between treatments, since cartel prevalence differs between treatments. In line with the findings of Dijkstra *et al.* (2021), reporting rates are modest, even when an investigation is announced. Overall, we observe reporting behavior which are consistent with Hypothesis 5. Leniency applications are the highest in *Baseline*, followed by *Fair funds.* Non-parametric tests, computed at the independent observation level, do not show any significant difference between treatments.

Table 5: Leniency applications

Treatment	N	All	No Investigation	With Investigation
Baseline	196	30.10%	27.06%	50.00%
Shared	164	10.98%	6.56%	23.81%
Transfer	192	13.02%	11.39%	20.59%
Fair Funds	112	17.86%	13.95%	30.77%

Table 6 reports estimates from random-effect Probit regression on the individual decision to apply for leniency. In model (1), we keep consistent with the previous econometric analysis reported in table 3 and include as covariates the treatment variables, the round (introduced in a quadratic form) and additional control for the participant's characteristics. The regression results suggest that reporting is significantly less likely in the *Transfer*, *Shared*, and *Fair Funds* treatments, compared to the *Baseline* treatment. Model (2) introduces control for two events that may have occurred in the period where the decision to apply for leniency is made. Investigation is a dummy variable taking one if an investigation is announced in this round. Defection is a dummy variable taking one if any of the two cartel members set a price lower than the price agreed upon. Both variables are associated with positive and significant coefficient. This finding is quite intuitive, and in line with our theoretical model, even if the strategy "CR" is dominated by the strategy "CNR" in *Transfer* and *Shared.* In this second model, reporting under *Fair Funds* is no longer statistically significant, whereas both *Shared* and *Transfer* conditions continue to demonstrate a significantly lower reporting rate compared to *Baseline.* Using a linear combination test, we can also affirm that reporting under *Transfer* is significantly less likely than reporting under *Fair Funds*, with a difference in estimates of 0.326 ($p = 0.047$). This contrast is less clear-cut when comparing *Fair Funds* with *Shared* ($p = 0.091$). There is no significant difference in reporting between *Shared* and *Transfer* ($p = 0.445$).

Table 6: Reporting decision - Random-effect Probit regressions

Treatment	(1)	(2)
Baseline	*Ref.*	*Ref.*
Shared	-1.012***	-0.943**
	(0.283)	(0.421)
Transfer	-0.754***	-0.743***
	(0.197)	(0.235)
Fair Funds	-0.425 ***	-0.417
	(0.136)	(0.255)
Investigation	-	1.035***
		(0.269)
Defection	-	1.342***
		(0.217)
Period	-0.022	0.090
	(0.079)	(0.086)
Period^2	0.001	-0.003
	(0.004)	(0.004)
Control for ind. characteristics	Yes	Yes
N	664	618

Note: Subsample considering only colluding markets, during the first twenty periods of the game. Model (2) only considers interaction where members explicitly agreed on a cartel price. Standard errors, clustered at the session level, are displayed in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Result 3. *Partially in line with our Hypotheses 5a and in line with 5b, leniency application is significantly higher in Baseline and Fair Funds compared to Shared and Transfer.*

We now refine our analysis by focusing on more restricted subsamples to directly test Hypotheses 3 and 4. Hypothesis 3 concerns the decision to report the cartel once an investigation has been initiated. The first column of Table 7 presents estimates from a random-effect Probit regression on the decision to apply for leniency, focusing exclusively on colluding markets under investigation. These results partially confirm Hypothesis 3.

Table 7: Reporting decision - Subsample analysis

Treatment	Following Investigation	Following Defection
Baseline	*Ref.*	*Ref.*
Shared	-0.717**	-1.550***
	(0.307)	(0.507)
Transfer	-0.879***	-0.553*
	(0.332)	(0.311)
Fair Funds	-0.494	-1.031**
	(0.392)	(0.401)
Period	0.079	-0.050
	(0.118)	(0.088)
Period^2	-0.004	0.003
	(0.005)	(0.005)
Control for ind. characteristics	Yes	Yes
N	128	60

Note: Subsample considering only markets where an investigation has been announced (column 1) and markets where a defection took place (column 2). Standard errors, clustered at the session level, are displayed in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Result 4. *After an investigation is launched, the highest reporting rates are observed in the Baseline treatment, followed by Fair Funds, which does not significantly differ from Baseline. In contrast, in both Shared and Transfer, the likelihood of reporting is significantly lower compared to Baseline. No significant difference in reporting rates is observed between Shared and Transfer, using linear combination tests.*

The second column of Table 7 examines the decision to apply for leniency following a defection. The results partially support Hypothesis 4.

Result 5. *Following a price deviation, reporting is significantly higher without private damages than in any other treatment. Among treatments with private damages, reporting is highest under Transfer, followed by Fair Funds and Shared, though differences between these three are not statistically significant.*

Caution is needed because this last result may be due to the limited number of observations, as defections from the agreed price were relatively rare in our experiment.

5. Discussion and Conclusion

This paper examines how leniency programs interact with the threat of private damage claims in shaping cartel behavior. Using a repeated Bertrand competition framework, we conducted a laboratory experiment to assess how private enforcement affects the performance of leniency policies. Our design compares three liability regimes: Shared liability, Transfer of liability to non-reporting firms, and a Fair Funds mechanism where an external fund assumes the lenient firm's liability. The results reveal patterns that diverge from theoretical expectations. Rather than discouraging collusion, the prospect of private damages tends to foster cartel formation, though the magnitude of this effect varies across regimes. Notably, the *Fair Funds* mechanism appears to attenuate this tendency relative to *Shared* and *Transfer*.

A plausible explanation for this counterintuitive result lies in reporting incentives. Our data show that leniency applications significantly drop when private damages are introduced, particularly under *Shared* and *Transfer*, while they remain relatively higher under *Fair Funds*. Exposure to civil liability weakens the appeal of self-reporting, thereby reducing the perceived risk of defection and making collusion more sustainable. In other words, fewer leniency applications in these regimes likely contribute to the higher prevalence of the cartels we observe. By shielding lenient firms from liability without shifting the burden to non-reporting parties, the *Fair Funds* mechanism partially restores reporting incentives and, in doing so, reduces cartel stability.

These dynamics underscore the importance of considering how private enforcement interacts with leniency when designing antitrust policy. Unlike the experimental results reported by Bodnar *et al.* (2023), our Transfer treatment neither reduces the incidence of cartels nor strengthens incentives to report, nor does it materially affect cartel stability. Two factors may explain this divergence. First, in our framework, both firms retain an incentive to report following a deviation from the agreed price, even when private damages are absent. This feature is also present in Dijkstra *et al.* (2021). This structure implies that the incentive constraint is essentially equivalent under Transfer and Baseline, which is consistent with our finding of no significant behavioral difference between these two regimes. Second, our design allows for open-ended communication, as in Dijkstra *et al.* (2021) and one of Bodnar *et al.* (2023)'s treatments. This feature likely reinforces trust among participants, even when partners are initially unfamiliar, thereby facilitating cartel formation and persistence. Moreover, participants know that they will interact with the same partner throughout the game, a condition that mirrors real-world markets with stable duopolistic relationships and may amplify these trust-based dynamics.

In conclusion, our findings challenge the assumption that private damages invariably strengthen deterrence. They also reinforce Hornkohl′s proposal to combine Fair Funds with exemptions for firms benefiting from leniency programs. When firms can communicate freely, such claims may unintentionally promote collusion by reducing incentives to report. This effect is particularly pronounced under Shared and Transfer liability regimes, whereas the Fair Funds mechanism appears to mitigate the conflict between public and private enforcement by preserving leniency incentives without shifting undue burdens to non-reporting parties. These results call for caution in extending private enforcement without considering its interaction with leniency programs. Policy discussions should therefore move beyond viewing these instruments in isolation. They should explore designs aiming for a balanced approach that deters anti-competitive behavior while ensuring justice for victims.

Further research should examine the external validity of these findings, especially in multi-firm cartels and institutional settings where litigation costs, disclosure rules, and reputation concerns play a critical role. Such extensions would allow for a deeper understanding of how to design enforcement tools that are both efficient and equitable.

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A. Theoretical Model and Equilibrium Conditions

We present a theoretical framework based on an infinitely repeated game to shed light on the incentives faced by participants in our experiment. In this model, two firms compete by setting prices for a homogeneous good, which they can produce at no cost in each period. Market demand is fixed at one unit and perfectly inelastic. Firms choose prices from the discrete set 1, 2, ..., 10, and if both firms select the same price, the market is split evenly between them. Under these conditions, the stage game yields a fully collusive (monopoly) profit of $Pi_C = 5$ for each firm, whereas the Nash equilibrium outcome results in a profit of $Pi_B = 0,5$ per firm. Each firm in the model has a discount factor denoted by δ (in the experiment $delta = 0.85$). We analyze each variant of the model, corresponding to *Baseline*, *Shared*, *Transfer* and *Fair Funds*. For each scenario, we determine the conditions under which a cartel can be sustained in a pure-strategy subgame-perfect equilibrium. Firms are assumed to use classic grim trigger strategies: if the previous cartel is uncovered but no defection occurred, they coordinate again to form a new cartel (Friedman, 1971). Our objective is to derive the complete equilibrium set to define relevant and testable hypotheses.

Given our framework, we describe the possible strategies adopted by firms:

- S_0: Compete in the market at each period without colluding.
- S_{CNR}: Collude in every period and refrain from reporting in case of an investigation by the Antitrust Authority (AA). If the cartel is detected, firms reform it. However, if a firm deviates and/or reports the cartel before or after an investigation, then firms compete à la Bertrand for the remainder of the game.
- S_{CR}: Collude and report in case of an investigation by the AA. As long as no firm deviates and reports before or after an investigation, firms continue to collude, even if the cartel has been detected.

We begin our analysis by defining the discounted present value associated with each strategy.

Discounted Present Value Computation

Under strategy S_0, the discounted present value is:

$$V_0 = \frac{\Pi_B}{1-\delta}.$$

Scenario Without Private Damage Claims (*Baseline*)
Under S_{CNR}, the discounted present value is:

$$V_{CNRB} = \frac{\Pi_C - \rho\alpha F}{1-\delta}.$$

The right-hand term accounts for the expected fine when the cartel is detected, with F being the fine and $\rho\alpha$ representing the probability of detection.
Under S_{CR}, the discounted present value is:

$$V_{CRB} = \frac{\Pi_C - \rho.c - \rho \cdot 0.5 \cdot (F + f)}{1-\delta}.$$

Here, in case of an investigation, each symmetric firm has a 50% probability of paying the full fine F or the reduced fine f, with c representing the cost of reporting in case of an investigation.
For *Baseline*, V_{CRB} dominates V_{CNRB} if and only if:

$$-\alpha F < -0.5(F + f) - \rho.c,$$

which simplifies to:

$$f < \frac{(\alpha - 0.5)F - \rho.c}{0.5}.$$

Given our numerical parameters, this condition holds, making S_{CR} the dominant strategy.
Equilibrium Constraints
For collusion and either non-reporting (CNR) or reporting (CR) to be an equilibrium, the following constraints must be satisfied:
- **Individual Rationality (IR) Constraint:** Firms must obtain a higher discounted present value from collusion than from competition (S_0).[5]
- **Incentive Compatibility Condition (ICC):** Collusion must be self-enforcing, meaning the discounted present value from collusion must exceed that from deviation.

[5] All parameters in our experiment are chosen to ensure this condition is always met.

In our setting, deviation from collusion triggers full competition until the game ends. The maximum deviation profit is $\Pi_D = 9$, but subsequent collusion becomes unsustainable, leading to competition. Additionally, as leniency is available, reporting the cartel is a dominant strategy in *Baseline*. Given that prices become public before reporting, the non-deviating firm also has an incentive to report. The discounted present value from deviation is:

$$V_{D_B} = \Pi_D - c - \frac{1}{2}(F + \rho f) + \delta V_0.$$

If there is no investigation (with probability $1 - \rho$) and I report the cartel, then with probability 1/2 I am the first to report and pay no fine, while with probability 1/2 I am the second and must pay the full fine F. If an investigation occurs (with probability ρ), and I am the first to report, I only pay the reduced fine f. Otherwise, if I am not the first to report, I pay the full fine F.

Once an investigation begins, sustaining CNR as an equilibrium requires an additional constraint:
- **Incentive Compatibility Constraint in the Subgame (ICCS):** As the probability of detection increases to 0.75, the discounted present value from deviating (V_D^{AI}) must be lower than from adhering to CNR (V_{CNRB}^{AI}):

$$V_{CNRB}^{AI} = \Pi_C - \alpha F + \delta V_{CNRB},$$

$$V_D^{AI} = \Pi_C - c - f + \delta V_0.$$

Incorporating Private Damage Claims

When private damages are introduced, discounted present values become:

Shared Treatment (S)

$V_{CNRS} = \frac{(\Pi_C - \rho\alpha(F + \beta * 0.5D)}{1-\delta}$.

β represents the probability to pay private damages following a public condemnation. D represents the global damage and in *Shared*, both companies share liability for the global damage.

$V_{CRS} = \frac{\Pi_C - \rho.c - \rho * 0.5 * (F + f + \beta * D)}{1-\delta}$

In case of an investigation, if each firm decides to report the cartel, they have 50% of chance to pay the partial fine f.

If a firm decides to deviate from CNR or CR and reports [6] , we have have:

$V_{D_S} = \Pi_D - c - \frac{1}{2}(F + \rho * f) - \beta * \frac{1}{2} * D' + \delta * V_O$.

where D' is the global damage in case of deviation.

We also have: $V^{AI}_{CNRS} = \Pi_C - \alpha(F + \beta * 0.5D) + \delta * V_{CNRB}$

$V^{AI}_{D} = \Pi_C - c - (f + 0.5\beta * D) + \delta * V_0$.

Transfer Treatment (T)

$V_{CNRT} = \frac{(\Pi_C - \rho\alpha(F+\beta*0.5D)}{1-\delta}$.

$V_{CRT} = \frac{\Pi_C - \rho.c - \rho*0.5*(F+f+\beta*D)}{1-\delta}$

If a firm decides to deviate from CNR or CR and reports, we have:

$V_{D_T} = \Pi_D - c - \frac{1}{2}(F + \rho * f) - \beta * \frac{1}{2} * D' + \delta * V_O$.

$V^{AI}_{CNRT} = \Pi_C - \alpha(F + \beta * 0.5D) + \delta * V_{CNRT}$.

$V^{AI}_{DT} = \Pi_C - c - f + \delta * V_0$.

Fair Funds Treatment(F)

$V_{CNRF} = \frac{(\Pi_C - \rho\alpha(F+\beta*0.5D)}{1-\delta}$.

$V_{CRF} = \frac{\Pi_C - \rho.c - \rho*0.5*(F+f+0.5\beta*D)}{1-\delta}$

If a firm decides to deviate from CNR or CR and reports, we have:

$V_{D_F} = \Pi_D - c - \frac{1}{2}(F + \rho * f) - \beta * \frac{1}{2} * 0.5 * D' + \delta * V_O$.

$V^{AI}_{CNRF} = \Pi_C - \alpha(F + \beta * 0.5D) + \delta * V_{CNRF}$.

$V^{AI}_{DF} = \Pi_C - c - f + \delta * V_0$.

According to our specific data in the experiment, we can now compare the different discounted present values in each treatment, in order to define the best strategy in each case.

We first summarize the main data in the experiment:

Table A.1: Numerical data in the experiment

data	ρ	α	β	δ	Π_C	Π_D	c	D	D'	F	f
.	0.2	0.75	0.95	0.85	5	9	1	9	8	5	1

Interpretation of Results

[6] It should be emphasized that the numerical parameters were deliberately calibrated to ensure that, in the event of a deviation, firms always have an incentive to report, irrespective of the treatment involving private damages.

Table A.2: Discounted present values by Treatment

Treatment	Baseline	Shared	Transfer	Fair Funds
V_{CNR}	28.33	24.05	24.05	24.05
V_{CR}	29.2	23.5	23.5	26.35
V_D	9.13	5.33	5.33	7.23
V_{CNR}^{AI}	25.33	18.49	18.49	18.49
V_D^{AI}	6.73	2.45	6.73	6.73
V_0	3.33	3.33	3.33	3.33
$V_{CNR} - V_D$	.	18.72	18.72	.
$V_{CR} - V_D$	20.07	.	.	19.11
$V_{CNR}^{AI} - V_D^{AI}$	.	16.04	11.76	.

Table $A.2$ compares the discounted present values across treatments. In all treatments, the individual rationality constraint is respected. However, the discounted value within the cartel is highest in *Baseline*, followed by *Fair Funds*, and then *Shared* and *Transfer*. In *Fair Funds*, CR dominates CNR, whereas the opposite holds in *Shared* and *Transfer*, justifying **Hypothesis 1**.
Regarding the incentive to deviate from the cartel (by lowering its price compared to the cartel price), the incentive constraints are satisfied in all treatments. However, these constraints may be tighter in one treatment compared to another one, leading to different incentives to deviate. Indeed, when the collusive discounted present value is closer to the value associated with deviation, the incentive to deviate is stronger.

In *Baseline* and *Fair Funds*, the CR strategy dominates CNR even though both strategies constitute equilibria. Conversely, this relationship is reversed in *Transfer* and *Shared*, indicating that in these scenarios, the incentive to report during an AA investigation will be lower than in the aforementioned treatments. Furthermore, the table reveals that the incentive constraint is more stringent in *Transfer* and *Shared* compared to *Baseline* and *Fair Funds*, where the incentive constraint is slightly tighter than in *Baseline*. Finally, even though CNR is a dominant strategy in *Shared* and *Transfer*, the IICS constraint is tighter in *Transfer* (11.76 in comparison with 16.04), thereby providing a potential stronger incentive to deviate from CNR and report once the AA has initiated an investigation. All these observations justify our **Hypothesis 2** and **Hypothesis 3.**

After a deviation, we can confirm that the incentive to report the cartel remains a dominant strategy compared to not reporting whatever the mechanism. However, the cost of reporting is higher in the presence of private damages. Nevertheless, with private damages, the cost of not reporting whereas the other reports is more important in *Transfer* than in *Shared* and *Fair Funds*, providing more incentive to report under *Transfer*. This observation validates our **Hypothesis 4.**

B. Additional tables

Table B.1: Leniency applications in the ten first periods

Treatment	N	All	No Investigation	With Investigation
Baseline	106	19.81%	16.28%	35.00%
Shared	116	12.93%	7.32%	26.47%
Transfer	154	13.64%	11.29%	23.33%
Fair Funds	98	16.33%	11.11%	30.77%