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Conference Paper

Relative performance evaluation, sabotage and collusion☆

Matthew J. Bloomfield [a,*], Catarina Marvão [b], Giancarlo Spagnolo [c,d,e]

[a] The Wharton School of the University of Pennsylvania, USA
[b] Technological University Dublin and Stockholm School of Economics (SITE), Italy
[c] Stockholm School of Economics (SITE), Sweden
[d] CEPR, London, UK
[e] Tor Vergata and Eief, Rome, Italy

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ABSTRACT

We examine whether the potential for costly sabotage is a deterrent to firms' use of relative performance evaluation ("RPE") in CEO pay plans. We exploit illegal cartel membership as a source of variation in the potential for costly sabotage and document that firms are more likely to use RPE if they are currently cartel members. Moreover, firms frequently drop RPE from their CEOs' pay plans immediately after their cartels are detected, dissolved and punished. We further provide suggestive evidence that the potential for costly sabotage explains these patterns; cartel membership severs the empirical association between RPE and competitive aggression.

1. Introduction

Agency theory's "informativeness principle" holds that an optimal incentive contract uses every contractible metric that provides incremental information about an agent's actions (Holmström, 1979). When multiple agents are exposed to common shocks, other agents' performance outcomes are informationally valuable signals. By benchmarking performance against other agents in similar economic circumstances, the obscuring effects of common shocks can be stripped away, thereby making it easier to monitor/ascertain each agent's actions (e.g., Lazear and Rosen, 1981; Holmström, 1982; Nalebuff and Stiglitz, 1983; Prendergast, 1999). The practice of benchmarking an agent's performance against a reference group is known as relative performance evaluation ("RPE").

While RPE is often an effective tool for filtering out common shocks, it can also bring about unintended consequences. For example, if an agent's actions can affect the performance of the reference group to which they are compared, RPE can introduce a significant incentive-distorting side effect: namely, costly sabotage (e.g., Dye, 1984; Lazear, 1989; Gibbons and Murphy, 1990; Chowdhury and Gürtler, 2015). An agent with significant relative performance incentives will be inclined

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* Corresponding author. 1325 Steinberg-Dietrich Hall 3620 Locust Walk Philadelphia, 19104-6365, PA, USA.
E-mail address: mjbloom@wharton.upenn.edu (M.J. Bloomfield).

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to take actions that harm the reference group's performance in order to inflate their own relative performance—even at significant cost to their own absolute performance.

Prior evidence suggests that RPE-induced sabotage plays an important role in many contexts, ranging from corporate promotions (e.g., Chen, 2003; Harbring et al., 2007) to higher education (e.g., Royal and Guskey, 2014), sports (e.g., Del Corral, Prieto-Rodriguez, and Simmons, 2010) and electoral politics (e.g., White, 1994; Lau and Pomper, 2001a,b, 2002, 2004). We examine whether the potential for RPE-induced sabotage is also an important consideration vis-à-vis CEO incentives, and find evidence that it is. Firms are more likely to use RPE in their CEOs' pay packages when the potential for sabotage is lower.

In the context of CEO compensation, costly sabotage could take the form of overly aggressive product market strategies, such as lower prices, greater output volumes and/or excessive advertising spending. While these actions are detrimental to own-firm profits, in imperfectly competitive markets (e.g., oligopolies), these actions can be even more destructive to rivals' profits, likely making competitive aggression an attractive sabotage strategy to managers with RPE-based incentives.[1] Accordingly, when the potential for costly sabotage is substantial, rational principals may choose to withhold relative performance incentives, even at the expense of risk-sharing efficiency, so as not to encourage value-destroying excess product market aggression.

While extant theory presents a clear story as to the impact of RPE on sabotage/competitive aggression, prior empirical literature provides no compelling evidence demonstrating that firms avoid using RPE in their CEOs' pay plans because of the potential for costly sabotage. We address this gap by looking to cartels as a setting where the potential for costly sabotage is significantly reduced. In cartels, otherwise rivalrous firms collectively agree on—and commit to—product market strategies. We posit that this commitment diminishes the potential for costly sabotage, increasing the net benefits (and therefore use) of RPE. This "commitment" does not need to be iron clad; so long as cartel membership makes unilateral strategy adjustments more costly, our predictions will hold.

We examine the relation between cartel membership and the use of RPE and find that cartel members are roughly 60% more likely to use RPE than non-cartel members. Moreover, this effect is driven by concentrated product markets, where the potential for RPE-induced sabotage is greater. In industries of above-median concentration, cartel firms are roughly 130% more likely (i.e., more than twice as likely) to use RPE than non-cartel members. In industries of below-median concentration, we find no evidence to suggest that cartel membership is associated with RPE.

Cartel membership is highly endogenous. Thus, documenting an empirical association between cartel membership and managerial incentives does not imply that a causal relation exists between the two. To better identify whether RPE grants are causally influenced by cartel membership, we look to cartel dissolutions as a source of plausibly exogenous variation in cartel membership. These dissolutions are the result of regulatory interventions, such as the U.S. Department of Justice ("DOJ") successfully bringing an enforcement action against a detected cartel. While detection and enforcement are non-random, it seems unlikely that the circumstances leading to a cartel dissolution would be substantially endogenous with respect to firm-level decisions regarding the use of RPE.

We find that firms frequently drop RPE from their CEOs' incentive plans immediately after their cartel membership is terminated by such an enforcement action. These results are particularly noteworthy because contract terms tend to be 'sticky'—once a CEO is given RPE-based incentives, they are rarely taken away. Moreover, this pattern is driven by firms in concentrated industries; in industries of above-median concentration, RPE drop-rates are more than seven and a half times higher among firms from recently terminated cartels. These patterns are not driven by executive turnover, and we find no evidence that pay packages change dramatically along other dimensions (e.g., pay plan complexity; proportion of grants awarding restricted stock versus cash).

We further examine the mechanism underlying our findings by analyzing the relations among RPE, cartel membership and product market aggression. We document that, among non-cartel firms, RPE is positively and significantly associated with measures of product market aggression (sales volume, total costs, spending-to-sales, and advertising expenditures). In contrast, among cartel firms, we find no such association. This evidence is consistent with the interpretation that RPE induces more aggressive behavior (i.e., costly sabotage), and that cartel membership is effective at curtailing these destructive actions. Collectively, our evidence provides support for the notion that costly sabotage is an important deterrent to firms' use of RPE, and that explicit collusion mitigates this possibility, allowing for more efficient risk-sharing between shareholders and executives.

To facilitate sharp inferences, we use firm and year (or SIC-year) fixed effects throughout our analysis. This design choice shields our inferences from time-invariant confounding factors in the cross-section, as well as arbitrary time trends. We identify the empirical patterns from within-firm and within-(industry-)year variation in cartel membership, RPE-reliance and product market aggression. Furthermore, in placebo tests, we exploit generic RPE (i.e., benchmarking against the S&P 500); sabotage strategies will not be effective when compared against such a broad reference group, so generic RPE is not likely to induce sabotage, regardless of cartel membership. We find that generic RPE is no more common among cartel firms, nor does its use change systematically around cartel terminations. Moreover, generic RPE has no association with product market aggression.

[1] It may seem counter-intuitive that a firm's excess aggression would hurt its rivals' profits more than its own; in Appendix A, we present a stylized example demonstrating how this works in the context of a Cournot oligopoly.

Our main results establish a tight link between cartel participation and the use of RPE. However, the hypothesized channel (i.e., costly sabotage) is not the only possible explanation for such a connection. In particular, our main tests leave open two important questions: (1) does cartel membership lead firms to use RPE (as posited), or does RPE push firms to collude? And (2) if cartel membership leads firms to use RPE, is this because cartel membership decreases the costs of using RPE (as posited), or could it also be that cartel membership increases the benefits of using RPE? While we cannot definitively answer these questions, we provide descriptive evidence which supports our 'costly sabotage' interpretation. In terms of the causal direction, we find that cartel membership "Granger causes" RPE in the sense that current cartel participation explains future reliance on RPE but not past reliance on RPE. With respect to the benefits of RPE, we find no evidence that cartel membership is associated with greater exposure to common risk.

We further examine whether, among RPE users, cartel firms construct more effective peer groups, from the standpoint of optimal risk-sharing. In constructing a benchmark, firms face a trade-off between optimizing risk-sharing and minimizing sabotage costs. By selecting more economically similar peers (with whom performance shocks are more correlated), firms are better able to shield their executives from risk. By selecting more economically distant peers (whose performance is less manipulable), firms can mitigate the potential for RPE-induced sabotage. As predicted, we find that cartel members construct higher quality peer groups, from a risk-sharing perspective. This appears to be driven by cartel members' greater willingness to select RPE peers from their own product markets. These results are consistent with the notion that firms are cognizant of the trade-off between risk-sharing and costly sabotage, and that cartel membership allows firms to focus more on the risk-sharing aspect of peer selection.

Our work contributes to the literature on relative performance evaluation by providing novel empirical evidence on the downsides of RPE. Our results speak most directly to arguments laid out by Gibbons and Murphy (1990), who propose two downsides: costly sabotage and collusive shirking. In the context of CEO compensation, we find evidence that the potential for costly sabotage is a significant driving force behind firms' avoidance of peer RPE. In contrast, we find no evidence that the potential for collusive shirking factors in to this decision. We further provide evidence that the potential for costly sabotage shapes peer selection, among those firms that choose to use RPE. Among RPE-users, cartel members tend to choose more economically similar peers.

Our results shed new light on the old, but still largely unsettled question of why RPE is not ubiquitous in executive pay packages. Ample prior literature has considered the possibility that concerns about product market aggression play a role in the scarcity of its use, but so far the primary supporting evidence has been the negative correlation between industry concentration, and firms' use of RPE (e.g., Aggarwal and Samwick, 1999b; Gong et al., 2011; Vrettos, 2013; Bettis et al., 2014). We complement these prior findings by exploiting cartel membership as a source of variation in the potential for costly sabotage and showing that firms are significantly more likely to use RPE if they have credibly committed, through explicit collusion, not to sabotage each other. This relation manifests most clearly in more concentrated industries, where [absent collusion] the risk of costly sabotage is greater. Moreover, we find suggestive evidence that RPE makes firms behave more aggressively, and that cartel membership is effective at mitigating this side effect of RPE.

We further contribute to the related literature on the role that strategic product market considerations play in shaping executive incentives/corporate governance.[2] We find that firms consider their product market position, and avoid RPE when its use would likely encourage value-destroying excess aggression. By committing not to engage in such behavior through explicit collusion, firms are better able to share risk with their executives by using RPE. In a similar vein, our work relates to the oft-discussed disciplinary role of product market competition on corporate governance (e.g., Allen and Gale, 2000; Buccirossi and Spagnolo, 2008).

Most closely related to our study is a contemporaneous working paper by Zaldokas et al. (2020). Like us, they are focused on the connection between explicit collusion and RPE in CEO pay plans. However, their results are directly opposite to ours; they document that explicit collusion is associated with *reduced* reliance on RPE, which they interpret as evidence that RPE discourages collusion. While there are several differences between our studies which may, in part, explain the divergence in results, the biggest difference is the approach to measuring RPE. We look to the explicit incentive pay disclosures found in public firms' proxy statements, and code firms as using RPE if they disclose using relative performance grants in their CEOs' incentive pay plans. In contrast, Zaldokas et al. (2020) use a regression-based approach to *infer* the use of RPE by regressing compensation on own performance and industry performance—the more negative the loading on industry performance, the more RPE is inferred to be present in the CEO's pay plan. While this approach is intuitive, and has been used extensively in prior literature (e.g., Aggarwal and Samwick, 1999a; Vrettos, 2013), we do not believe it is the best methodology for examining the use of RPE, as we detail in the Online Appendix, Section OA1.

The remainder of the paper is organized as follows. In Section 2, we develop and state our predictions; in Section 3, we discuss the cartel setting; in Section 4, we detail our data sources, sample construction and variable definitions; in Section 5, we present our main analyses and results; in Section 6, we offer supplemental discussion and analyses; and in Section 7, we conclude. In Appendix A, we illustrate the intuition underlying the relation between RPE and costly sabotage; we use a

[2] See, for example: Fershtman (1985); Vickers (1985); Fershtman and Judd (1987); Sklivas (1987); Fumas (1992); Aggarwal and Samwick (1999b); Spagnolo (2000, 2005); Buccirossi and Spagnolo (2008); Vrettos (2013); Kwon (2016); Feichter et al. (2022); Bloomfield et al. (2022b); Bloomfield et al. (2021); Antón et al. (2018).

stylized example of a Cournot duopoly, to demonstrate how excess competitive aggression can improve a firm's *relative* performance, despite being detrimental to the firm's *absolute* performance.

2. Hypothesis development

Under traditional agency theory (e.g., Holmström, 1979), the optimal performance measurement system is that which best informs about the agent's actions. In situations with multiple agents all subject to common performance shocks, performance relative to other agents is a useful source of information to include for this purpose, as it purges the uncertainty arising from these common shocks (e.g., Holmström, 1982; Lazear and Rosen, 1981). However, the utility of RPE is predicated on the notion that other agents' performance outcomes are informative but not manipulable. If agents can take costly actions to harm the reference group (i.e., sabotage the other agents), then RPE may not be optimal despite the informational benefits it yields (e.g., Dye, 1984; Lazear, 1989; Gibbons and Murphy, 1990).

In many concentrated product markets, a single firm can unilaterally affect its rivals' profitability through its own strategic actions. For example, by choosing a more aggressive strategy (e.g., lower prices in Bertrand competition, or higher production volume in Cournot competition), a firm can damage its rivals' profits (as well as its own). Importantly, in imperfectly competitive markets (e.g., oligopolies) one firm's overly aggressive conduct typically damages competitors' profits *more* than it damages the firm's own profits, increasing the firm's relative performance to the detriment of absolute performance. Thus, a CEO given substantial compensation tied to RPE may be incentivized to take profit-destroying actions, so long as their actions reduce peers' profits to a greater extent than their own. Cartel membership constrains the firm's competitive actions, thereby limiting this deleterious response to RPE, and increasing the net benefits of its use. Accordingly, we predict that cartel members are more likely to use RPE, especially in more concentrated product markets. With respect to the underlying mechanism, we further predict that RPE is associated with heightened product market aggression, but only among non-cartel members.

Conditional on using RPE, firms have considerable flexibility in constructing the RPE benchmark. As with the choice of whether to use RPE or not, firms face a similar trade-off between risk-sharing and sabotage potential during the peer selection process. By selecting peers that are more economically similar (e.g., direct product market competitors), the benchmark is better able to filter out the common shocks. However, such a benchmark is also more easily manipulable through unilateral strategic conduct, exacerbating the potential for costly sabotage. By selecting peers that are more economically distant (e.g., firms in different, but related, industries), the benchmark is less effective at filtering out the systematic shocks, but the potential for costly sabotage is reduced. We posit that cartel membership limits the potential for costly sabotage, and therefore allows boards to focus on filtering out systematic shocks during the peer selection process. Accordingly, we predict that, conditional on using RPE, cartel members construct peer groups that are more effective at filtering risk, and more heavily comprised of product market rivals.

In the Online Appendix, Section OA2, we present a sketch of a stylized contracting framework, based on Holmström and Milgrom (1987, 1991), from which our predictions derive. We develop our predictions based on the assumption that cartels are well-enforced by their members, and that unilateral deviation from the cartel's chosen strategy is costly, thereby making cartel membership an effective deterrent to sabotage.[3] To the extent that this is the case, the above predictions should hold. In contrast, if unilateral deviation from the cartel strategy is costless, our predictions do not hold, and opposite predictions are likely to arise. Specifically, if cartel members can easily defect from their cartels by unilaterally choosing their own product market strategies, then RPE is likely to encourage firms to sabotage their co-conspirators. As such, cartel members would be likely to avoid using RPE, so as not to destabilize their cartels. Moreover, we would expect to see a positive relation between RPE and product market aggression irrespective of cartel membership status. We view the possibility of weak cartel enforcement as a source of tension in the predictions, contributing to the credibility of the null. Ultimately it is an empirical matter which premise is best supported in the data. As we will show, the data best support the notion that cartels function as deterrents to sabotage; all of the empirical findings line up very nicely with the predictions that arise from this premise.

3. Cartels as an empirical setting

Explicit collusion— “the supreme evil of antitrust”[4]— occurs when some or all firms in a market coordinate to soften (or end) competition. This is typically done with the aim of increasing prices and consequently, profits. This behavior is forbidden in the US by section 1 of the Sherman Act (1890), and in the EC by article 101 of the Treaty of the European Communities (1999). When firms conspire to engage in explicit collusion, the conspiring firms are known as a “cartel.”

Three canonical approaches to collusion are price-fixing (including bid-rigging), quantity fixing and market allocation. In fixing cartels, colluding firms collectively agree on their prices or production volumes, to act jointly as a monopolist. Under market allocation, firms divide the market into segments (e.g., by geography or product features), in which each firm acts separately as a monopolist. While these various approaches to collusion are economically distinct from each other, our

[3] In our model we effectively assume an *infinite* cost of deviation by taking away the agent's ability to choose the strategy, but this extreme modeling approach is not necessary; making deviation costly yields qualitatively similar implications.

[4] U.S. Supreme Court Justice Antonin Scalia, Verizon Communications, Inc. v. Law Offices of Curtis V. Trinko LLP, 2004

predictions apply equally to all types. In all cases, explicit collusion mitigates the potential for sabotage, thereby reducing the costs of using RPE.

For cartel membership to affect CEO pay plans, the cartel must be an important aspect of firm operations. If an enormous conglomerate operates in a single cartelized market that accounts for a minuscule portion of its sales, it is unlikely that the cartel arrangement would have any bearing on the optimal CEO compensation plan. In contrast, if a firm's operations are highly cartelized, such collusion would plausibly be an important consideration for CEO pay plan design.

While it is difficult to assess the exact degree to which cartelization affect any given cartel firm's operations, existing research suggests that cartelization plays a major role in firm operations. For large firms, like those in our sample, explicit collusion is rarely an anomalous occurrence in a single product market. These firms tend to “embrace explicit collusion as a business model,” (Kovacic et al., 2018) or avoid cartelization, altogether. As Marx et al. (2015) put it: “The list of firms engaged in collusion in more than one product is long,” with 71% of prosecuted cartels involving “multi-market colluders” (Marx and Zhou, 2014). Echoing this sentiment, Kovacic et al. (2018) remark that:

> “The evidence of serial collusion by major multi-product firms is readily observable from the public record … Many large multi-product firms have engaged in explicit collusion for extended periods of time across a variety of products … Large multi-product firms enjoy multiple advantages when embracing explicit collusion as part of their business model.”

Indeed, even when cartelization starts off small, as just a single product line, it can quickly grow, spreading across product lines and permeating large swaths of firm operations (e.g., Connor, 2007; Igami and Sugaya, 2022). In some cases, collusion is so pervasive as to be part and parcel of corporate culture. As Levenstein et al. (2015) note:

> “There are cases where a firm’s corporate culture encourages participation in cartels. In such a case, the leadership of the organization expects managers to collude, and we observe collusion in many markets in which the firm operates. Managers may learn to collude in one division and then take those practices to another. Firm norms and expectations of managerial behavior can encourage collusion, as the well-known ADM case illustrated (Eichenwald, 2000).”

As an additional consideration, for cartelization to affect CEO pay plans, it must be the case that decision-makers in the pay plan design process are aware of the existence of the cartel. For the purposes of our study, we see the possibility of shareholder ignorance as a major source of the “credibility of the null.” Many of our analyses are, in a sense, a joint test of two hypotheses: (1) cartel membership improves the net benefits of RPE; and (2) some decision-makers in the compensation design process are aware, at least vaguely, of the existence of a collusive arrangement. Anecdotally, there is evidence that major shareholders are aware of collusive arrangements (e.g., Marvão, 2015; Marvão and Spagnolo, 2022), but we acknowledge that it is unclear exactly how knowledge of a cartel enters into the compensation design process. That said, given how extensive collusion is within some cartel firms (e.g., Connor, 1997; Marx and Zhou, 2014; Marx et al., 2015; Kovacic et al., 2018; Levenstein et al., 2016), it seems quite plausible that knowledge of cartelization would find its way into the contracting process, at least for some meaningful fraction of cases.

4. Data, sample and key variables

4.1. Data

The cartel data employed in the empirical analysis come from two sources. Data on U.S. cartels comes from an excerpt from John Connor's Private International Cartels dataset.[5] This excerpt covers the years of 1984–2011 and is limited to publicly reported information on 180 cartels convicted between 1985 and 2011 by the DOJ, involving 470 non-anonymous individual firms.

Data on EU cartel cases was hand-collected by one of the authors through publicly available summary reports and associated press releases of the antitrust cases handled by the EC and accessible via the Commission's website.[6] The data include 81 cartels involving 613 firms convicted in the period of 1998 to December 15, 2014.

The financial and compensation data used in this study come from four sources: Compustat's Annual and Quarterly Industrial Files; Incentive Lab; ExecuComp; and the Hoberg and Phillips Data Library. Incentive Lab provides detailed, grant-level data on executive compensation contracts, including the choice of metrics, performance goals and associated payouts. Coverage is limited to the largest publicly traded firms, beginning in 1998. The Hoberg and Phillips Data Library provides a text-based network industry classification, giving each firm a list of firm-year specific competitors, with associated similarity ‘scores.’ The scores are based on the cosine similarity between two firms' product disclosures.[7]

[5] Private International Cartels spreadsheet by John M. Connor, Purdue University, Indiana, USA (January 2012). The dataset was modified in several ways: the anonymous firms and groups of firms were dropped to be able to account for different measures of recidivism; some of the variables were resized; where possible, data was checked (and corrected) against the DOJ case documents; the imprisonment variable was updated with John Connor's criminal dataset, obtained in 2016 and several other variables were dropped due to inconsistent or missing data.

[6] For a thorough description of this dataset, see: Marvão (2015) and Levenstein et al. (2016).

[7] See: Hoberg and Phillips (2010, 2016), and Hoberg et al. (2014).

4.2. Sample selection

We construct our sample using all firm-years in the intersection of Compustat and Incentive Lab, over the period of 1998−2015. We drop all observations with missing data on sales, ticker symbols, or SIC codes. We match this set, as feasible, to the cartel dataset, using firms' ticker symbols.[8] Our final sample consists of 22,276 firm-year observations from 2026 unique firms, of which 106 firms were cartel members at some point over our sample period, for a total of 708 firm-year cartel observations.

4.3. Variables

Below we outline the variables used in our main analyses. Definitions for all variables can be found in Appendix B.

4.3.1. Cartel membership

We measure cartel membership with an indicator variable equal to one for all firm-years which are identified as being part of a cartel membership window. A firm's cartel membership window spans from the first year for which the firm was successfully prosecuted for antitrust violations, to the year of the final antitrust enforcement action.[9] We refer to this measure as *CARTEL*. For supplemental analyses, we decompose *CARTEL* into *CARTEL* (*primary*) and *CARTEL* (*secondary*), which reflect whether or not a cartel operates in a firm's primary industry.

We further construct the indicator variable, *BUST*, to reflect firms' transitions from being cartel members to non-cartel members (i.e., when final enforcement actions are successfully brought against the firms).[10] *BUST* which takes a value of one if $CARTEL_{i,t-1} = 1$ and $CARTEL_{i,t} = 0$.

One overarching concern regards our measurement of cartel membership. As explicit collusion is illegal, cartels operate in secret. As such, we are only able to identify cartel members based on *detected* cartels. This problem of selection on the unobservables cannot be overcome, but its existence is acknowledged in the interpretation of the results. To the extent that undetected cartels exist, and differ from detected cartels along relevant dimensions, our results may be biased (e.g., if the use of RPE plays a role in the detection of the cartel). This problem is not unique to our study; an analogous concern applies to all studies where variable codings are jointly contingent upon both the presence and *detection* of the feature of interest.[11]

4.3.2. Executive incentives

We measure executive incentives with indicator variables equal to one if the CEO has any compensation grants tied to purely “Relative” objectives (i.e., *performancetype* = “*Rel*”).[12] Within relative incentives, we construct two measures of RPE, one for peer group benchmarks and one for a generic benchmark: the S&P 500. We refer to these variables as *RPE* (*peer*) and *RPE* (*gen.*).

Analogously to *BUST*, we also construct indicator variables to reflect when RPE is dropped: *δRPE* (*peer*) and *δRPE* (*gen.*) are equal to one in the rare instance that peer RPE or generic RPE, respectively, were dropped from the CEOs' pay package. That is, for generic and peer RPE, $\delta RPE_{i,t} = 1$ if and only if $RPE_{i,t-1} = 1$ and $RPE_{i,t} = 0$.

For most of our analyses, we use indicator measures for the existence (or recent removal) of relative incentives. We recognize that these indicator variables fail to capture potentially meaningful variation in the *intensity* of relative incentives. However, our use of indicator variables based on explicitly disclosed incentives is standard in recent studies, since reliable proxies for metric weight intensity can be difficult to construct (e.g. Gong et al., 2011; Guay et al., 2019; Ma et al., 2019; Bloomfield, 2021; Bloomfield et al., 2022a,b). In particular, regression-based approaches to identifying continuous measures of executive incentive weights are ill-advised, as we detail the Online Appendix, Section OA1. That said, we recognize the value of examining intensive margin of variation in relative incentives. For this purpose, we construct the variable *Prop*. *RPE* (*peer*), equal to the proportion of grants which include relative performance.

[8] Where possible, we use the US ticker symbols developed by Standard & Poor's (S&P) to identify each firm. We use the latest available symbol for each firm, to reflect mergers and acquisitions. For example, Exxon's US ticker symbol was “XON” but after the 1999 merger with Mobil Oil, it changed to “XOM”.

[9] Many firms are involved in multiple cartels (known as “repeat offenders” or “serial colluders”). In these cases, the cartel membership window covers involvement in all cartels that the firm is convicted of participating in. For example, if the firm involved in one cartel from 2006 to 2010, and another cartel from 2008 to 2013, the firm's cartel window spans from 2006 to 2013.

[10] It is conceivable that cartels manage to sustain even after cartel member firms are caught, convicted and fined. To the extent that regulatory interventions are ineffectual, it would reduce the power of our tests.

[11] Common examples include fraud/financial misreporting (e.g., AAER issuances); insider trading; etc.

[12] Some firms include hybrid grants which include both absolute relative components (*performancetype* = “*AbsRel*”). In our main analyses, we focus on purely relative grants, as these are most likely to induce sabotage. In robustness analyses, we find that our inferences remain unchanged if we further include these hybrid grants in our measure of RPE reliance. See Section 6.7 for more details.

4.3.3. Product market aggression

Product market aggression is difficult to measure. To mitigate this issue, we use a triangulation approach and construct four different measures of product market aggression: sales-to-assets, $log(\frac{Sales_{i,t}}{Assets_{i,t}})$; total costs-to-assets, $log(\frac{Costs_{i,t}}{Assets_{i,t}})$; spending-to-sales, $log(\frac{Spend_{i,t}}{Sales_{i,t}})$; and advertising spend-to-assets, $log(\frac{Ad.\ Spend_{i,t}}{Assets_{i,t}})$.[13]

While none of these variables perfectly reflect competitive aggressiveness, they are all closely tied to firms' strategic choices. A more aggressive strategy (e.g., greater sales quantity and/or lower product prices) is likely to result in greater revenues and costs, as well as lower expenditure efficiency (i.e., greater spending per dollar of revenue). Advertisement spending is an explicit strategic choice, but whether or not higher levels of advertising should be considered more aggressive depends on the nature of the advertisements (i.e., market stealing versus market expanding). Our analysis is predicated on the notion that greater levels of advertising reflect a more aggressive strategy, but we acknowledge the possibility of non-adversarial advertising campaigns.

5. Empirical analysis

5.1. Descriptive statistics

We present summary statistics in Table 1. In Panel A, we provide pooled summary statistics for the entire sample. In Panel B, we provide variable means, split by cartel membership status. Several descriptive facts bear mention. First, in our sample, both cartel membership and RPE are quite rare, at roughly 3.2% and 11.4% (combining peer and generic RPE), respectively.[14] Second, we note several uni-variate relations. Cartel firms are significantly more likely to use peer RPE—and rely on it more heavily, conditional on using it—and are marginally less likely to use generic RPE. Quantitatively, cartel firms are 60% more likely to use peer RPE (11.2% for cartel firms versus 7% for non-cartel firms). Moreover, among firms that use peer RPE, cartel firms use roughly 40% peer RPE grants, compared to only 28% for non-cartel firms. Cartel firms also appear to be less aggressive, as captured by lower sales and production expenditures, and lower spending per dollar of sales generated. Of note, we also document that cartel firms are quite different from non-cartel firms, with respect to a variety of firm and CEO characteristics. Cartel firms tend to be larger firms with more directors, and their CEOs tend to be somewhat shorter tenured, less likely to be founders and more likely to be chair of the board.[15]

In Table 2, we document the prevalence of cartel membership, by economic sector. We find that cartels are most common in the financial sector, accounting for almost 20% of our cartels. Next most common are organic chemicals, machinery, and electronics, collectively accounting for almost 30% of the cartels in our sample. The remaining 50% of the cartels are split among the other sectors. No single sector seems to dominate the sample. In our sample, a significant proportion of cartels (31%) are global cartels (i.e. covering the US and non-US jurisdictions), and the vast majority (73%) occur in markets with many buyers, defined as more than 100. On average, the cartels in the sample include 13 members (and have a maximum of 32 members), and last 7.4 years.

5.2. Baseline results

We begin our analysis by examining the relations among RPE use, cartel membership, and industry concentration. We do so with variants on the following regression specification:

$$RPE_{i,t} = \beta CARTEL_{i,t} + \mu_i + \tau_{j,t} + \varepsilon_{i,t}, \tag{1}$$

where $RPE_{i,t}$ is an indicator variable equal to one if firm i uses RPE in year t, $CARTEL_{i,t}$ is an indicator variable equal to one if firm i was a cartel member in year t, and μ and τ are firm and SIC-year fixed effects. Across our first set of tests, specifications differ with respect to the measure of RPE (peer versus generic), the fixed effect structure, the use of control variables, and the sample.

Pooled results for the entire sample are presented in Table 3. In Panel A (Panel B), the dependent variable is *RPE* (*peer*) (*RPE* (*gen.*)). Across both panels, the fixed effects are consistent: in the first specification, we include only year fixed effects; the second specification adds firm fixed effects; and in the third specification, we use firm and SIC-year fixed effects. In light of the

[13] The sample size is substantially smaller for advertising spending since this information is not disclosed when advertising spending is sufficiently small. Replacing missing values with zero does not improve the sample size, because of our use of the logarithm to address skewness. Tests which use $log(\frac{Ad.\ Spend_{i,t}}{Assets_{i,t}})$ as the dependent variable therefore focus on the subset of observations for which significant advertising spending is part of the business model.

[14] The average rate of RPE usage is lower in our study than in some related studies. There are two primary reasons for this. First, our sample period (1998–2015) skews earlier than many recent studies, which focus exclusively on the post-CD&A period (from 2006 onward). Reliance on RPE has grown significantly since 1998, and rapidly so since 2006. Second, we exclude hybrid grants with comingled absolute and relative components (coded as *performancetype* = "*AbsRel*" in Incentive Lab); in practice, these hybrid grants operate quite differently from the canonical type of RPE upon which our predictions are based. In robustness analyses, we assess the impact of both research design choices on our inferences (see: Section 6.7).

[15] We use firm fixed effects and/or controls for these characteristics, to mitigate their impact on our inferences.

Table 1
Summary statistics.

Panel A: Summary Statistics, Pooled

Variable	Obs.	Mean	SD	Q1	Med.	Q3
Cartel membership						
CARTEL	22,276	0.032	0.175	0.000	0.000	0.000
BUST	22,276	0.005	0.068	0.000	0.000	0.000
CARTEL (primary)	22,276	0.020	0.139	0.000	0.000	0.000
CARTEL (secondary)	22,276	0.012	0.109	0.000	0.000	0.000
Incentives						
RPE (peer)	22,276	0.071	0.257	0.000	0.000	0.000
RPE (gen.)	22,276	0.043	0.203	0.000	0.000	0.000
δRPE (peer)	22,276	0.015	0.120	0.000	0.000	0.000
δRPE (gen.)	22,276	0.009	0.094	0.000	0.000	0.000
Prop. RPE (peer)	22,276	0.020	0.092	0.000	0.000	0.000
$\underbrace{Prop.\ RPE\ (peer)}_{\text{Given } RPE\ (peer)=1}$	1,585	0.287	0.206	0.143	0.207	0.400
Aggression						
$log\left(\frac{Sales}{Assets}\right)$	21,556	−0.666	1.054	−1.230	−0.463	0.057
$log\left(\frac{Costs}{Assets}\right)$	21,571	−0.705	1.048	−1.268	−0.517	0.018
$log\left(\frac{Spend}{Assets}\right)$	19,119	−0.198	0.431	−0.316	−0.188	−0.103
$log\left(\frac{Ad.\ Spend}{Assets}\right)$	7,170	−4.537	1.593	−5.589	−4.383	−3.324
Firm characteristics						
ROA	16,029	0.031	0.169	0.012	0.040	0.079
log (SALES)	16,029	7.854	1.631	6.868	7.881	8.883
log (ASSETS)	16,029	8.408	1.629	7.374	8.321	9.421
log (#DIRECT)	16,029	2.269	0.258	2.079	2.303	2.398
CEO characteristics						
log (TENURE)	15,945	1.678	0.848	1.099	1.792	2.303
log (AGE)	15,945	3.997	0.117	3.931	4.007	4.078
FOUNDER	15,945	0.049	0.216	0.000	0.000	0.000
CHAIR	15,945	0.693	0.461	0.000	1.000	1.000

Panel B: Summary Statistics, by Cartel Membership Status

Variable	Mean	SD	Mean	SD	Dif.
	Cartel		Non-Cartel		
Incentives					
RPE (peer)	0.112	0.315	0.070	0.255	0.042***
RPE (gen.)	0.030	0.170	0.044	0.204	−0.0140*
Prop. RPE (peer)	0.044	0.143	0.020	0.090	0.025***
$\underbrace{Prop.\ RPE\ (peer)}_{\text{Given } RPE\ (peer)=1}$	0.400	0.211	0.282	0.204	0.115***
Aggression					
$log\left(\frac{Sales}{Assets}\right)$	−0.813	1.132	−0.661	1.051	−0.152***
$log\left(\frac{Costs}{Assets}\right)$	−0.887	1.167	−0.698	1.043	−0.189***
$log\left(\frac{Spend}{Sales}\right)$	−0.230	0.192	−0.197	0.436	−0.033*
$log\left(\frac{Ad.\ Spend}{Assets}\right)$	−4.720	1.558	−4.532	1.594	−0.188
Firm characteristics					
ROA	0.039	0.057	0.031	0.172	0.008
log (SALES)	9.463	1.397	7.794	1.608	1.669***
log (ASSETS)	10.299	2.060	8.338	1.568	1.961***
log (#DIRECT)	2.478	0.253	2.261	0.254	0.217***
CEO characteristics					
log (TENURE)	1.548	0.807	1.683	0.849	−0.135***
log (AGE)	4.019	0.110	3.996	0.117	0.023***
FOUNDER	0.015	0.120	0.050	0.219	−0.035***
CHAIR	0.877	0.329	0.685	0.464	0.192***

This table presents descriptive statistics for all variables used in our main analyses. Panel A presents pooled summary statistics for the full sample; Panel B presents summary statistics split by cartel membership status.

Table 2
Distribution of cartels by sector.

Industry	frequency
Finance, insurance, banking	19.6%
Organic chemicals, other	13.1%
Machinery, including electrical and parts	8.9%
Electronic devices, including computers	6.9%
Other services	6.5%
Transport services	5.8%
Wholesale, retail	5.8%
Rubber and plastic	5.5%
Instruments, miscellaneous manufacturing	4.5%
Pharmaceuticals, medicines, medical devices	3.8%
Inorganic chemicals, fertilizers	3.4%
Paper and printing	2.4%
Stone, clay, graphite, glass products	2.1%
Communication services	2.1%
Minerals, metal ores	1.7%
Food and beverage mfg.	1.7%
Fabricated metal products	1.7%
Petroleum products	1.4%
Construction	1.0%
Agricultural raw materials	0.7%
Wood, lumber	0.7%
Tobacco manufacturing	0.3%
Textiles	0.3%

This table presents the distribution of cartel activity across economic sectors. Sectors are defined based on John Connor's sector classification.

differences between cartel and non-cartel firms (as show in Table 1, Panel B), we replicate the third specification of Panel A using a variety of controls for firm and CEO characteristics, in Panel C.

We find that cartel members are significantly more likely to use peer RPE than non-cartel members (Panels A and C). This result holds in the cross-section, as well as within-firm and SIC-year, and is robust to the inclusion of a variety of controls. In terms of economic magnitudes, cartel membership is associated with an almost doubled likelihood of using peer RPE. This pattern is entirely absent in the case of generic RPE; the relation between generic RPE use and cartel membership is both statistically insignificant and economically small. These results are consistent with our predictions, and align with the notion that (on average) cartels act as deterrents to sabotage, bolstering the net benefits of RPE.

To further support the notion that these patterns are driven by concerns about costly sabotage, we split the sample in half, based on industry concentration, and replicate the analyses on each sub-sample. Results from these analyses are reported in Table 4. We find that the relation between cartel membership and the use of RPE is present only in the more concentrated industries—precisely the industries in which firms are more likely to engage in costly sabotage. Collusion occurs in much the same way in both concentrated and non-concentrated industries. In our sample, the low concentration subsamples account for almost half (~44%) of the cartel observations. As such, the notable disparities across these specifications are unlikely to be an artifact of differential statistical power across subsamples. Moreover, we observe that the coefficients are estimated with similar precision (as gauged by standard errors) across specifications.

5.3. Event study: cartel terminations

The preceding evidence demonstrates a significant positive relation between cartel membership and the use of RPE—especially in more concentrated product markets. This evidence is consistent with the 'costly sabotage' explanation for the scarcity of explicit RPE in executive pay packages (à la Gibbons and Murphy, 1990). However, cartel membership and RPE use are both endogenous firm choices, and a host of potential confounds could explain the association between the two that we document. Our use of firm and industry-year fixed effects reduces this concern, by stripping away any time-invariant firm-level endogeneities as well as any time-varying industry-level factors. However, our inferences could still be contaminated by time-varying firm-level factors that influence both cartel participation and the use of RPE.

To mitigate concerns that some correlated omitted factor explains our findings, we look to cartel terminations as a source of plausibly exogenous variation in cartel membership. In the context of our sample, a cartel termination arises because of a regulatory intervention (e.g., the cartel was discovered by the FBI and thereafter convicted/dissolved by the DOJ). While regulatory interventions are not random, it seems plausible that the specific timing of these enforcement actions would be largely exogenous with respect to firm-level executive compensation practices.

We begin with a graphical investigation, plotting RPE drop-rates, in event time, for the twelve-year period centered around the cartel termination date (Fig. 1). We find that dropping RPE is a rare event—occurring in less than 2% of the observations in our sample—but that its likelihood increases substantially in the year after a cartel's dissolution. Relative to the base rate, a firm is roughly five times more likely to drop RPE from its CEO's pay package in the first year after its cartel was

Table 3
Cartels and relative performance evaluation.

Panel A: Peer RPE

	Pred.	Outcome = *RPE* (*peer*)		
		(1)	(2)	(3)
CARTEL	+	0.064***	0.047**	0.068**
		(4.232)	(2.186)	(2.751)
Year Fixed Effects		Yes	Yes	No
Firm Fixed Effects		No	Yes	Yes
SIC-Year Fixed Effects		No	No	Yes
Observations		22,276	22,276	22,276
R-squared		0.018	0.443	0.556

Panel B: Generic RPE

	Pred.	Outcome = *RPE* (*gen.*)		
		(1)	(2)	(3)
CARTEL	0	−0.003	0.016	0.020
		(−0.334)	(0.924)	(1.231)
Year Fixed Effects		Yes	Yes	No
Firm Fixed Effects		No	Yes	Yes
SIC-Year Fixed Effects		No	No	Yes
Observations		22,276	22,276	22,276
R-squared		0.010	0.373	0.506

Panel C: Peer RPE with Added Controls

	Pred.	Outcome = *RPE* (*peer*)		
		(1)	(2)	(3)
CARTEL	+	0.078***	0.073**	0.069**
		(2.929)	(2.744)	(2.255)
ROA		−0.008		−0.005
		(−1.340)		(−0.411)
log (SALES)		0.017		0.030
		(1.245)		(1.619)
log (ASSETS)		0.001		0.019
		(0.062)		(0.775)
log (#DIRECT)		0.018		0.004
		(0.677)		(0.095)
log (TENURE)			−0.000	−0.000
			(−0.072)	(−0.076)
log (AGE)			0.008	−0.015
			(0.157)	(−0.345)
FOUNDER			−0.085	−0.120*
			(−1.312)	(−1.776)
CHAIR			−0.011	−0.023
			(−0.781)	(−1.441)
Firm Fixed Effects		Yes	Yes	Yes
SIC-Year Fixed Effects		Yes	Yes	Yes
Observations		16,029	15,945	12,511
R-squared		0.595	0.589	0.609

This table presents results on the relation between cartel membership and the use of RPE, using variants on the regression specification:

$$RPE_{i,t} = \beta CARTEL_{i,t} + \mu_i + \tau_{j,t} + \varepsilon_{i,t},$$

where *CARTEL* is a firm-year indicator for cartel membership, and *RPE* is a firm-year indicator for the use of RPE. Specifications differ with respect to fixed effect structure, controls, and the dependent variable. In Panel A (Panel B), the dependent variable is *RPE* (*peer*) (*RPE* (*gen.*)). Across Panels A and B, Specification 1 uses only year fixed effects; Specification 2 uses firm and year fixed effects; and Specification 3 uses firm and SIC-Year fixed effects. Panel C presents a replication of Panel A Specification 3, with additional controls. Specification 1 includes controls for firm characteristics (*ROA*, log(*SALES*), log(*ASSETS*) and log(*#DIRECT*)); Specification 2 includes controls for CEO characteristics (log(*TENURE*), log(*AGE*), *FOUNDER* and *CHAIR*); Specification 3 includes controls for firm and CEO characteristics, jointly. Changes in sample size are due to missing data for control variables. Below each coefficient, we report t-statistics, based on standard errors clustered by industry (Fama and French 48 Industry Classification) and year.

dissolved. Moreover, the spike in RPE drop-rates manifests only for peer RPE; drop-rates for generic RPE remain roughly flat over the entire window.

The sharp spike in peer RPE drop-rates at year $t = 1$ (the first year after the cartel bust) is preceded by a short run-up in years $t = -1$ and $t = 0$. That is, RPE drop-rates are somewhat elevated in the year before a cartel is dissolved, and substantially elevated in the year of the cartel dissolution. These plausibly reflect firms' anticipation of the dissolution, and setting CEO incentives for the year ahead with the impending dissolution in mind. Moreover, if a cartel is dissolved early enough in the year, the firm might have sufficient time to change the current year's executive incentives to account for the change. In

Table 4
Cartels and relative performance evaluation, by concentration.

	Pred.	Outcome = *RPE* (*peer*)			
		(1)	(2)	(3)	(4)
CARTEL	+/?	0.095**	0.023	0.117**	0.028
		(2.178)	(0.372)	(2.003)	(0.471)
Firm Fixed Effects		Yes	Yes	Yes	Yes
SIC-Year Fixed Effects		Yes	Yes	Yes	Yes
Sample		Few Firms	Many Firms	High HHI	Low HHI
Observations		11,146	11,130	11,142	11,134
R-squared		0.620	0.530	0.622	0.549

This table presents results on the relation between cartel membership and the use of RPE, split by industry concentration. The estimating equation exactly mirrors Specification 3 of Table 3 Panel A, but the sample is cut in half, based on concentration at the SIC-year level. In Specification 1 (Specification 2), the sample is only those firms in SICs with a below-median (above-median) number of firms. In Specification 3 (Specification 4), the sample is only those firms in SICs with an above-median (below-median) Herfindahl-Hirschman index. Firm and SIC-year fixed effects are included in all four specifications. Below each coefficient, we report t-statistics, based on standard errors clustered by industry (Fama and French 48 Industry Classification) and year.

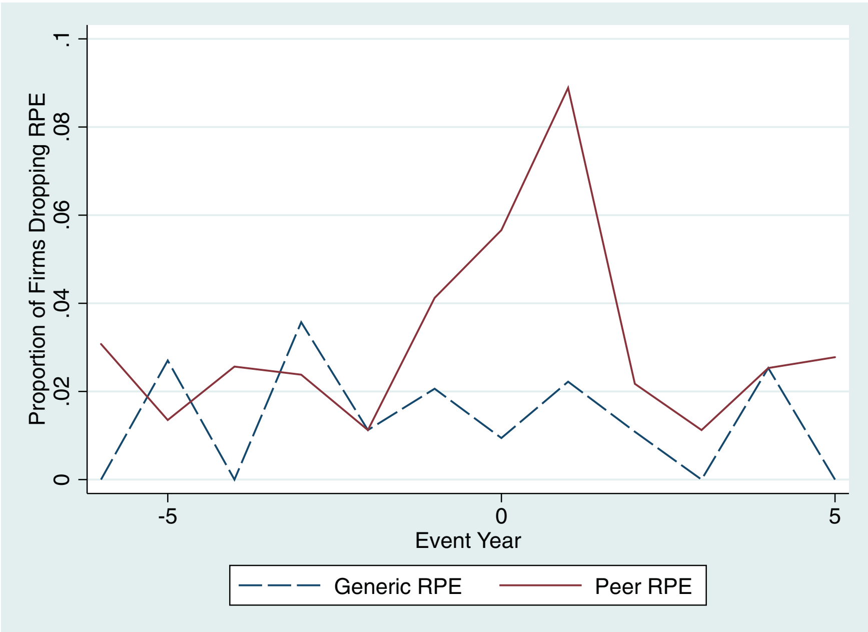


Fig. 1. Dropping RPE around Cartel Terminations. This figure presents RPE drop-rates, in event time, around cartel terminations. Year 0 represents a firm's first year after its cartel was terminated. That is, $CARTEL_{t=-1} = 1$ and $CARTEL_{t\geq 0} = 0$.

contrast, drop-rates fall sharply back to normal levels at $t = 2$; it seems that firms which remove RPE due to their cartels' dissolution do not wait beyond the first year.

We test for the effect of cartel busts using variants on the following regression specification:

$$\delta RPE_{i,t} = \beta BUST_{i,t-1} + \mu_i + \tau_{j,t} + \varepsilon_{i,t}, \tag{2}$$

where $\delta RPE_{i,t}$ is an indicator variable equal to one if firm i stopped using RPE in year t, and $BUST_{i,t-1}$ is an indicator variable equal to one if firm i was a member of a cartel terminated in year $t-1$. The tests exactly mirror those of Tables 3 and 4, with respect the fixed effect structures, controls, and sample. The only change is that *BUST* replaces *CARTEL*, while δRPE replaces *RPE*. Thus these tests identify the relation between RPE and cartel membership more sharply around plausibly exogenous shocks to cartel membership status.

Across all specifications, we find that cartel termination is associated with a significantly greater RPE drop-rate, but only for peer RPE. We find no evidence that generic RPE drop-rates change at all around cartel termination (Table 5). Moreover, the relation between cartel termination and RPE drop-rates appears to be driven by more concentrated product markets, with economically large and statistically significant effects among industries of above-median concentration, and near-zero effects among industries of below-median concentration (Table 6). We caution that cartel dissolutions are rare events, comprising

Table 5
Busted cartels and RPE drop-rates.

Panel A: Peer RPE

	Pred.	Outcome = δRPE (*peer*)		
		(1)	(2)	(3)
BUST	+	0.063***	0.044**	0.035*
		(3.136)	(2.011)	(1.945)
Year Fixed Effects		Yes	Yes	No
Firm Fixed Effects		No	Yes	Yes
SIC-Year Fixed Effects		No	No	Yes
Observations		22,276	22,276	22,276
R-squared		0.004	0.106	0.263

Panel B: Generic RPE

	Pred.	Outcome = δRPE (*gen.*)		
		(1)	(2)	(3)
BUST	0	0.009	0.006	0.006
		(0.884)	(0.655)	(0.512)
Year Fixed Effects		Yes	Yes	No
Firm Fixed Effects		No	Yes	Yes
SIC-Year Fixed Effects		No	No	Yes
Observations		22,276	22,276	22,276
R-squared		0.003	0.098	0.262

Panel C: Peer RPE with Added Controls

	Pred.	Outcome = δRPE (*peer*)		
		(1)	(2)	(3)
BUST	+	0.048**	0.039*	0.054**
		(2.628)	(2.026)	(2.606)
ROA		−0.001		0.004
		(−0.465)		(0.439)
log (SALES)		−0.002		0.001
		(−0.596)		(0.088)
log (ASSETS)		0.006		0.011
		(1.215)		(1.110)
log (#DIRECT)		−0.019*		−0.021
		(−1.969)		(−1.677)
log (TENURE)			−0.004	−0.006*
			(−1.663)	(−1.883)
log (AGE)			0.012	−0.005
			(0.563)	(−0.230)
FOUNDER			0.001	0.007
			(0.095)	(0.352)
CHAIR			0.001	0.000
			(0.175)	(0.015)
Firm Fixed Effects		Yes	Yes	Yes
SIC-Year Fixed Effects		Yes	Yes	Yes
Observations		16,029	15,945	12,511
R-squared		0.290	0.290	0.304

This table presents results on the relation between cartel membership and the use of RPE, using variants on the regression specification:

$$\delta \langle RPE \rangle_{i,t} = \beta BUST_{i,t-1} + \mu_i + \tau_{j,t} + \varepsilon_{i,t},$$

where *BUST* is a firm-year indicator for having been in a recently dissolved cartel, and *δRPE* is a firm-year indicator for whether the firm dropped RPE from the CEO's pay package that year. Specifications differ with respect to fixed effect structure, controls, and the dependent variable. In Panel A (Panel B), the dependent variable is *RPE* (*peer*) (*RPE* (*gen.*)). Across Panels A and B, Specification 1 uses only year fixed effects; Specification 2 uses firm and year fixed effects; and Specification 3 uses firm and SIC-Year fixed effects. Panel C presents a replication of Panel A Specification 3, with additional controls. Specification 1 includes controls for firm characteristics (*ROA*, log(*SALES*), log(*ASSETS*) and log(*#DIRECT*)); Specification 2 includes controls for CEO characteristics (log(*TENURE*), log(*AGE*), *FOUNDER* and *CHAIR*); Specification 3 includes controls for firm and CEO characteristics, jointly. Changes in sample size are due to missing data for control variables. Below each coefficient, we report t-statistics, based on standard errors clustered by industry (Fama and French 48 Industry Classification) and year.

only a small fraction of our sample. As such, these results reflect a reaction to extreme situations, and thus might not generalize to more typical circumstances.[16]

[16] Given the rarity of cartel busts, these tests rely heavily on small numbers of key firms to identify the coefficients—especially after splitting the sample in half, by concentration. To ensure that our findings are not driven by any small subset with undue influence, we replicate the analyses in Table 6 using a battery of jack-knifed "leave-one-out" tests. In untabulated analyses, we find that our inferences cannot be attributed to any individual year or industry.

Table 6
Busted cartels and RPE drop-rates, by concentration.

	Pred.	Outcome = $\delta RPE\ (peer)$			
		(1)	(2)	(3)	(4)
BUST	+/?	0.089**	−0.006	0.092**	−0.010
		(2.165)	(−0.219)	(2.330)	(−0.344)
Firm Fixed Effects		Yes	Yes	Yes	Yes
SIC-Year Fixed Effects		Yes	Yes	Yes	Yes
Sample		Few Firms	Many Firms	High HHI	Low HHI
Observations		11,146	11,130	11,142	11,134
R-squared		0.362	0.173	0.397	0.187

This table presents results on the relation between cartel membership and the use of RPE, split by industry concentration. The estimating equation exactly mirrors Specification 3 of Table 5 Panel A, but the sample is cut in half, based on concentration at the SIC-year level. In Specification 1 (Specification 2), the sample is only those firms in SICs with a below-median (above-median) number of firms. In Specification 3 (Specification 4), the sample is only those firms in SICs with an above-median (below-median) Herfindahl-Hirschman index. Firm and SIC-year fixed effects are included in all four specifications. Below each coefficient, we report t-statistics, based on standard errors clustered by industry (Fama and French 48 Industry Classification) and year.

While these results are consistent with our predictions, we caveat that cartel membership could affect RPE for reasons other than those hypothesized. Similar patterns might arise if cartel firms use RPE as an enforcement mechanism, credibly threatening aggression to deter defection (i.e., mutually assured destruction). Moreover, antitrust enforcement actions can entail major disruptions for affected firms. Any observed changes around these enforcement actions could be driven by a major firm-level shake-up; not necessarily our predicted sabotage versus risk-sharing trade-off.[17]

One might reasonably question why we don't do event study analyses around cartel formation, analogous to the cartel bust tests in Table 5. While such analyses are conceptually appealing, there are some major practical issues in this context. First, roughly half of the cartels in our sample began before our sample period, making it infeasible to examine changes in pay plans around their formations. Second, cartels often start out slowly, and ramp up over time, spreading across product lines (e.g., Igami and Sugaya, 2022). It may take time for knowledge of the cartel to reach decision-makers involved in the compensation design process. As such, it is unclear that changes in RPE should occur immediately around cartel formation.[18]

5.4. Mechanism tests

The preceding evidence demonstrates a close connection between cartel participation and reliance on RPE. In this subsection, we provide additional evidence to better assess the possible reason(s) for this link. We examine evidence regarding three possible explanations: (1) RPE pushes firms to collude (and not the other way around); (2) cartel participation pushes firms to use RPE because it reduces the potential for costly sabotage; and (3) cartel participation pushes firms to use RPE because it increases the extent of common shocks for which RPE is a useful risk-sharing device. We find the strongest support for explanation (2).

5.4.1. Direction of causality

We develop our predictions under the notion that cartel membership affects the net-benefits (and therefore use) of RPE. Another possibility is that RPE affects the net-benefits (and therefore prevalence) of collusion. This could occur for multiple reasons. For example, firms' reliance on RPE could push firms into hyper-aggressive product market equilibria, increasing the potential benefits of cartel formation. Alternatively, RPE could push executives to seek the quiet life through ‘collusive shirking’ (e.g., Gibbons and Murphy, 1990).

We assess these possibilities by examining the lead-lag relations among cartel participation and reliance on RPE. We do so with variants on the following regression specifications:

$$RPE_{i,t+1} = \beta_1 CARTEL_{i,t} + \beta_2 RPE_{i,t} + \mu_i + \tau_t + \varepsilon_{i,t}, \tag{3}$$

$$RPE_{i,t-1} = \beta_1 CARTEL_{i,t} + \beta_2 RPE_{i,t} + \mu_i + \tau_t + \varepsilon_{i,t}. \tag{4}$$

If cartel membership pushes firms to use RPE, we would expect $CARTEL_t$ to be associated with RPE_{t+1}; if RPE pushes firms to collude, we would expect $CARTEL_t$ to be associated with RPE_{t-1}. As shown in Table 7, we find that cartel participation explains future reliance on RPE, but not past reliance on RPE. These patterns hold with or without controlling for contemporaneous reliance on RPE. Thus, cartel membership appears to “Granger cause” RPE and not the other way around.

[17] In Section 6.5, we examine this possibility and find no evidence to support it.

[18] In lieu of an event study, we use Granger causality tests to examine whether firms are significantly more likely to incorporate RPE into CEO pay plans, after joining a cartel (see Section 5.4.1). While not as sharp as an event study, Granger causality tests are advantageous in this context because they do not require sudden changes in pay plans immediately upon joining a cartel. These tests can accommodate heterogeneity across cartel firms, in terms of the length of time required, before CEO incentives are adjusted.

Table 7
Mechanism test: Causal direction.

	Outcome = $RPE\ (peer)_{t+1}$		Outcome = $RPE\ (peer)_{t-1}$	
	(1)	(2)	(3)	(4)
CARTEL	0.045**	0.025*	0.018	−0.004
	(2.168)	(1.812)	(0.790)	(−0.250)
RPE (peer)		0.510***		0.483***
		(17.234)		(15.863)
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	20,052	20,052	20,052	20,052
R-squared	0.458	0.591	0.447	0.583

This table presents evidence on the lead-lag relation between cartel participation and the use of RPE. The specification mirrors that of Table 3 Panel A, but uses future RPE and prior RPE as the dependent variables. In specifications 1 and 2 (3 and 4), the outcome variable reflect peer RPE in year $t+1$ ($t-1$). In even-numbered columns, we add a control for RPE in year *t*. Firm and year fixed effects are included in all specifications. Below each coefficient, we report t-statistics, based on standard errors clustered by industry (Fama and French 48 Industry Classification) and year.

5.4.2. RPE, cartels and costly sabotage

In our next set of tests, we examine whether RPE and cartel membership are associated with product market aggression in the predicted manner. Aggressive strategies can take many forms, such as low prices or high output volumes. Unfortunately, we lack access to broad sample data on firms' product market actions (e.g. product prices), so we must rely on rough proxies for product market strategy, based on available data.[19]

As there is no well-accepted proxy for product market strategy, we use four different measures intended to reflect firms' product market aggressiveness: sales-to-assets; total costs-to-assets; spending-to-sales; and advertising spend-to-assets. Across firms and industry-years, variation in these measures is likely attributable to economic circumstances and/or business models. However, our use of firm and SIC-year fixed effects subsumes much of this variation, allowing residual variation to be more plausibly interpreted as variation in firm strategy (i.e., 'aggressiveness').

We test whether RPE and cartel membership are jointly associated with product market aggression in the posited manner using variants on the following regression specification:

$$<Aggression>_{i,t} = \beta RPE_{i,t} + \mu_i + \tau_{j,t} + \varepsilon_{i,t}, \quad (5)$$

where *Aggression* takes one of four different measures: sales-to-assets; total costs-to-assets; spending-to-sales; and advertising spend-to-assets. For each measure, we present four specifications, two in Panel A, and two in Panel B. In Panel A, we exclude cartel observations and present one specification for peer RPE (in odd-numbered specifications), and another for generic RPE (in even-numbered specifications). In Panel B, we examine only peer RPE, but split the sample into non-cartel observations (odd-numbered specifications) versus cartel observations (even-numbered specifications). Results are presented in Table 8.

In Panel A, we find that peer RPE is associated with substantially greater aggression across all four measures. That is, among non-cartel observations, the use of peer RPE is associated with higher sales, but also higher costs. Moreover, RPE is associated with less efficient spending, whereby firms must spend more money per dollar of revenue generated. Collectively, these results are consistent with the notion that peer RPE drives firms to compete more aggressively. In contrast, we find no evidence that generic RPE is associated with any of the four measures. The coefficient on *RPE (gen.)* is statistically insignificant in all four specifications, and economically tiny in three out of the four specifications.[20] This evidence suggests that generic RPE does not affect competitive behavior in the same manner as peer RPE.

In Panel B, we find that the relation between RPE and aggression is only present among non-cartel observations. Among cartel observations, the patterns documented in Panel A are conspicuously absent; all four estimated coefficients are statistically insignificant (and carry a negative point estimate). Jointly, the descriptive findings in Table 8 lend credence to our supposition that peer RPE induces more aggressive product market strategies, and that cartel membership is effective at curtailing this effect.

The evidence in Table 8 suggests that many non-cartelized firms choose to use RPE, despite the risk of sabotage. It is conceivable that these firms are making a mistake, failing to correctly gauge the costs of their choices. However, it seems more likely that such firms experience a net benefit from their use of RPE, despite any harm that may arise from sabotage (e.g., because the risk-sharing benefits are more than enough to offset these costs). Assessing the optimality of firms' chosen pay plans lies outside the scope of our study.

We caveat that this evidence is highly descriptive in nature. RPE use is an endogenous choice and these results do not necessarily reflect the causal effect of RPE on firms' product market strategies. Moreover, the outcome variables we examine

[19] If broad sample product pricing data become more readily available, future work could better shed light on the link between CEO incentives and product market strategy by examining the relation between CEO incentives and product market prices.

[20] In the one specification with an economically significant coefficient, it carries a negative sign.

Table 8
Mechanism test: Product market aggression.

Panel A: Peer versus Generic RPE

	Pred.	Outcome = $log(\frac{Sales}{Assets})$		Outcome = $log(\frac{Costs}{Assets})$		Outcome = $log(\frac{Spend}{Sales})$		Outcome = $log(\frac{Ad.\ Spend}{Assets})$	
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RPE (*peer*)	+	0.049**		0.053***		0.013***		0.201***	
		(2.870)		(3.828)		(6.259)		(3.164)	
RPE (*gen.*)	0		−0.000		−0.007		−0.019		0.000
			(−0.002)		(−0.308)		(−1.621)		(0.002)
Firm Fixed Effects		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SIC-Year Fixed Effects		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations		20,849	20,849	20,864	20,864	18,571	18,571	6,994	6,994
R-squared		0.913	0.913	0.932	0.932	0.699	0.699	0.933	0.933

Panel B: Cartel versus Non-Cartel Firms

	Pred.	Outcome = $log(\frac{Sales}{Assets})$		Outcome = $log(\frac{Costs}{Assets})$		Outcome = $log(\frac{Spend}{Sales})$		Outcome = $log(\frac{Ad.\ Spend}{Assets})$	
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RPE (*peer*)	+/0	0.049**	−0.030	0.053***	−0.012	0.013***	−0.008	0.201***	−0.025
		(2.870)	(−0.560)	(3.828)	(−0.253)	(6.259)	(−0.161)	(3.164)	(−0.215)
Cartel firm		No	Yes	No	Yes	No	Yes	No	Yes
Firm Fixed Effects		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SIC-Year Fixed Effects		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations		20,849	707	20,864	707	18,571	548	6,994	176
R-squared		0.913	0.991	0.932	0.990	0.699	0.967	0.933	0.992

This table presents evidence on the relations among RPE, product market aggression and cartel membership, using variants on the regression specification:

$$Aggression_{i,t} = \beta RPE_{i,t} + \mu_i + \tau_{j,t} + \varepsilon_{i,t},$$

where *Aggression* takes one of four different measures: sales-to-assets (specifications 1 and 2); total costs-to-assets (specifications 3 and 4); spending-to-sales (specifications 5 and 6); and advertising spend-to-assets (specifications 7 and 8). Panel A excludes cartel members, and presents side-by-side regressions for *RPE* (*peer*) (in odd-numbered specifications) versus *RPE* (*gen.*) (in even-numbered specifications). Panel B examines only *RPE* (*peer*), and presents side-by-side regressions for non-cartel firm-years observations (in odd-numbered specifications) and cartel firm-year observations (in even-numbered specifications). Firm and SIC-year fixed effects are included in all specifications. Below each coefficient, we report t-statistics, based on standard errors clustered by industry (Fama and French 48 Industry Classification) and year.

do not all cleanly reflect firms' strategic choice variables. Sales, costs and expenditure efficiency are equilibrium outcomes and will be affected by firms' strategic choices, as well as supply and demand shocks. But despite the descriptive nature, this evidence further supports the notion that RPE encourages sabotage, and that cartel membership mitigates this possibility, thus increasing the net benefits of RPE.

5.4.3. RPE, cartels and risk-sharing benefits

In developing our predictions, we posit that cartel versus non-cartel firms receive the same risk-sharing benefits from using RPE, and that cartel participation affects reliance on RPE only through its mitigating effect on sabotage costs. This assumption is violated if cartel participation causes firm performance to become more exposed to common shocks. In our next set of analyses, we address this possibility by examining whether cartel membership is associated with greater stock price co-movements.

For these tests, we examine the set of firms which used RPE at some point during its cartel period and did not use RPE at some point during its non-cartel period. For this set of firms, we construct peer groups using the self-selected peers that the firm chose to benchmark against during its cartel phase and examine how well this peer group explains firm returns during the cartel period versus before and after the cartel period. We use the following specification:

$$\begin{aligned} OWN\ RETURN_{i,t} = \beta_1 CARTEL_{i,t} \times PEER\ RETURN_{i,k,t} \\ + \beta_2 CARTEL_{i,t} + \beta_3 PEER\ RETURN_{i,k,t} + \mu_i + \tau_{j,t} + \varepsilon_{i,t}, \end{aligned} \tag{6}$$

where *OWN RETURN* is the focal firm's monthly return, *PEER RETURN* the contemporaneous monthly return for a peer which the focal firm selected as an RPE peer at some point during its cartel period. The unit of observation for this analysis is the firm-peer-month. We present three specifications which vary only with respect to fixed effect structure: year; year + firm; and SIC-year + firm. The results are presented in Table 9.

We find that peer returns are highly associated with contemporaneous own-firm returns. This is unsurprising given that the [ostensible] purpose of RPE is to filter out common shocks. However, we find no evidence that the risk-sharing benefits of RPE differ across cartel and non-cartel observations; the estimated coefficient on the interaction term *CARTEL* × *PEER RETURN*

Table 9
Mechanism test: Risk-sharing benefits.

	Outcome = *OWN RETURN*		
	(1)	(2)	(3)
CARTEL × *PEER RETURN*	0.024	0.026	0.034
	(0.312)	(0.344)	(0.450)
CARTEL	0.003	0.007	0.000
	(1.069)	(1.601)	(0.053)
PEER RETURN	0.444***	0.442***	0.431***
	(12.613)	(12.425)	(12.014)
Year Fixed Effects	Yes	Yes	No
Firm Fixed Effects	No	Yes	Yes
SIC-Year Fixed Effects	No	No	Yes
Observations	401,821	401,821	401,821
R-squared	0.204	0.213	0.242

This table presents evidence on the risk-sharing benefits of RPE for cartel and non-cartel observations. We use variants on the estimating equation:

$$OWN\ RETURN_{i,t} = \beta_1 CARTEL_{i,t} \times PEER\ RETURN_{i,k,t} + \beta_2 CARTEL_{i,t} + \beta_3 PEER\ RETURN_{i,k,t} + \mu_i + \tau_{j,t} + \varepsilon_{i,t},$$

where *OWN RETURN* is the focal firm's monthly return, *PEER RETURN* the contemporaneous monthly return for a peer which the focal firm selected as an RPE peer at some point during its cartel period. The unit of observation for this analysis is the firm-peer-month. Specifications vary only with respect to fixed effect structure. Specification 1 uses only year fixed effects; Specification 2 uses firm and year fixed effects; and Specification 3 uses firm and SIC-Year fixed effects. Below each coefficient, we report t-statistics, based on standard errors clustered by industry (Fama and French 48 Industry Classification) and year.

is economically small and statistically indistinguishable from zero across all specifications. This evidence suggests that cartel participation has no impact on the effectiveness of RPE from a risk-shielding standpoint.[21] Thus, any effect of cartel participation on the use of RPE seems more likely to arise from cost-side, rather than benefit-side, explanations. One might also wonder why cartel firms require risk-sharing governance tools like RPE at all, given that collusion seems like it might strip much of the uncertainty out of performance outcomes. The important point here is that collusive arrangements might alleviate some uncertainty with respect to strategic choices, but cartel firms remain exposed to many of the same sources of common risk that non-cartel firms are exposed to: demand shocks, input price shocks, etc.

5.5. Peer selection

The preceding analysis sheds light on firms' choices to use or forgo peer RPE, based on strategic product market considerations. We next examine whether, among the subset of firms that use peer RPE, those same strategic considerations explain variation in the choice of RPE peers. We predict that non-cartelized firms will choose to forgo some of RPE's risk-sharing benefits in order to mitigate the potential for costly sabotage. In particular, we predict that non-cartelized firms choose to exclude close product market competitors from their peer groups. In contrast, we predict that cartelized construct their peer groups with risk-sharing as the sole consideration.

We examine whether the above is empirically descriptive in three stages. First, we evaluate whether cartelized firms construct higher quality peer groups, from the standpoint of risk-sharing. Second, we examine whether cartelized firms are more willing to include close competitors in their peer groups. Third, as a validation, we assess whether the willingness to include close competitors explains variation in peer groups' risk-sharing quality.

To evaluate firms' chosen peer groups, we must first consider what the peer groups might have been, in a counterfactual world in which risk-sharing is the sole consideration. To do so, we follow Bloomfield et al. (2022a) and construct counterfactual RPE peer groups based on past stock return co-movements. We focus on the subset of firm-year observations in which peer RPE is used, and for each firm-year, construct an “artificial” peer group using the Bloomfield et al. (2022a) peer selection algorithm.[22]

We then compare the actual peer groups against these artificial peer groups along several dimensions. First, we examine how well the actual peer group explains the firms' stock performance, compared to the artificial peer group. Following Bloomfield et al. (2022a), we construct the variable *QUALITY* by taking the difference between the actual peer group's explanatory power and the artificial peer group's explanatory power. *QUALITY* is positive (negative) if the actual peer group is more (less) effective at filtering risk than the artificial peer group.

Second, we examine how the actual and artificial peer groups differ, with respect to the underlying peers selected. In particular, we examine disparities in the inclusion of product market competitors as RPE peers. We measure inclusion of

[21] Given that cartel firms coordinate their conduct to maximize their joint profits, it may seem surprising that cartel firms are not more exposed to common risk. Intuitively, one might presume that, due to their coordination, cartel firms become more highly correlated in their performance outcomes. However, in theory, coordinated conduct need not be any more correlated than uncoordinated, unilateral conduct. Whether firms compete or collude, their strategies and performance outcomes will be affected by the same demand and cost shocks, leading to correlations across firms, irrespective of coordination.

[22] See Appendix C for a discussion of the algorithm.

product market competitors in two different ways: proportion of peers in the same 4-digit SIC as the focal firm; and peers' average product similarity to the focal firm, as measured by the Hoberg and Phillips text-based product similarity measures.

For each artificial peer group and actual peer group, we calculate the following statistics: (1) the proportion of peers that are in the same 4-digit SIC as the focal firm; (2) the average product similarity between the focal firm and its peers, using the Hoberg and Phillips product similarity scores; and (3) the average product similarity between the focal firm at its peers, using the Hoberg and Phillips *high granularity* product similarity scores. We then calculate the differences between the actual peer group and the artificial peer group, with respect to these characteristics. Δ*SIC*4 reflects the difference in the proportion of same 4-digit SIC peers, Δ*SCORE* and Δ*SCORE* (*hg*) reflect the differences in product similarity. The two measures differ only in that the latter uses the "high granularity" version of the TNIC scores. Δ*SIC*4 is positive (negative) if the actual peer group is more (less) populated with same-industry peers than the artificial peer group; Δ*SCORE* and Δ*SCORE* (*hg*) are positive (negative) if the actual peers' product similarity to the focal firm exceeds (subceeds) that of the artificial peers'.

Summary statistics for these variables can be found in Table 10, Panel A. We find that, on average, actual peer groups filter roughly one percentage point less risk than the artificial peer groups (R^2 of 57.6% versus 58.5%, $p < 0.05$), and consist of roughly nine percentage points fewer same-industry peers (48% versus 57%, $p < 0.01$). We do not observe any significant average differences with respect to product similarity.

We test whether cartel firms construct more effective risk-shielding peer groups using variants on the following regression specification:

$$QUALITY_{i,t} = \beta CARTEL_{i,t}[\ + \phi CARTEL_{i,t} \times Concentration_{j,t}\ ] + \mu_i + \tau_{j,t} + \varepsilon_{i,t}. \tag{7}$$

In the first specification, we consider the impact of *CARTEL* on its own; in the latter two specifications, we further include an interaction with concentration (measured by *log(#FIRMS)* and *log(HHI)*). We include firm and industry-year fixed effects in Table 10, Panel B.

We find that cartel firms construct peer groups that are significantly higher quality (from the standpoint of risk-shielding). On average, firms' peer groups are 4.2 percentage points (2/3rds of a standard deviation) higher quality when they are members of a cartel, compared to when they are not. This effect is more pronounced among firms in more concentrated industries, where non-cartelized firms are more likely to be concerned about avoiding costly sabotage.

We next examine the manner in which peer group composition changes. First we check whether cartel firms' peer groups are more similar to the artificial peer groups, and find that they are. On average, 50% of the cartel firms' peers were also included in the artificial peer group, while only 40% of non-cartel firms' peers were also included in the artificial peer group. This suggests that cartel firms are deviating substantially less from the peer groups that boards would plausibly design, if focused solely on risk-shielding. We further examine whether the change in peer group composition is systematically related to features likely to be related to sabotage costs (i.e., product market proximity). We test whether cartel firms construct peer groups that rely more heavily on product market rivals by using variants on the following regression specification:

$$< Similarity >_{i,t} = \beta CARTEL_{i,t} + \mu_i + \tau_{j,t} + \varepsilon_{i,t}, \tag{8}$$

where the outcome variable takes one of three different variables: Δ*SIC*4 in Specification 1; Δ*SCORE* in Specification 2; and Δ*SCORE* (*hg*) in Specification 3. We include firm and industry-year fixed effects in all specifications. Results are tabulated in Table 10, Panel C. We find that firms construct peer groups that rely more heavily on product market rivals, when they are members of cartels. Cartel firms' peer groups are 5.3 percentage points (0.83 standard deviations) more reliant on same-industry peers, and average product similarity is significantly greater as well.

The above establishes that cartel firms construct peer groups in a manner quite different from non-cartel firms: they are more effective at shielding risk, and rely more heavily on ostensible product market rivals. This evidence is consistent with the joint suppositions that: (1) there is a trade-off between risk-shielding effectiveness and sabotage costs; and (2) cartel membership mitigates sabotage costs, allowing firms to focus on the risk-shielding aspect of peer selection. To validate that the changes in peer composition are (at least partially) responsible for the changes in peer group quality, we examine whether peer group quality varies systematically with Δ*SIC*4, Δ*SCORE* and Δ*SCORE* (*hg*), using the following regression:

$$QUALITY_{i,t} = \beta < Similarity >_{i,t} + \mu_i + \tau_{j,t} + \varepsilon_{i,t}, \tag{9}$$

where the independent variable takes one of three different variables: Δ*SIC*4 in specification 1; Δ*SCORE* in specification 2; and Δ*SCORE* (*hg*) in specification 3. We include firm and industry-year fixed effects in all specifications. Results are tabulated in Table 10, Panel D. We find that all three variables are positively associated with peer group quality.

A separate aspect of peer selection worth considering regards whether cartel firms choose to include– or avoid including– their co-conspirators in their RPE peer groups. We have no specific predictions in this regard, but acknowledge that firms could have preferences for including or excluding their co-conspirators.[23] We examine this possibility and do not observe any

[23] For example, if firms use RPE to boost cartel stability, they might populate their peer groups with co-conspirators, irrespective of risk-sharing considerations. Alternatively, firms may omit co-conspirators to downplay their linkages.

Table 10
Peer selection.

Panel A: Summary Statistics

Variable	Obs.	Mean	SD	Q1	Med.	Q3
QUALITY	819	−0.009	0.064	−0.035	−0.006	0.021
Δ*SIC4*	819	−0.088	0.269	−0.167	0.000	0.025
Δ*SCORE*	819	−0.001	0.026	−0.008	0.000	0.011
Δ*SCORE* (*hg*)	819	0.000	0.031	−0.012	0.001	0.016

Panel B: Cartels and Peer Group Quality

	Pred.	Outcome = *QUALITY*		
		(1)	(2)	(3)
CARTEL	+/?	0.042***	0.562**	0.899*
		(7.246)	(2.015)	(1.919)
CARTEL × log (*#FIRMS*)	-		−0.084*	
			(−1.844)	
CARTEL × log (*HHI*)	+			0.252*
				(1.825)
Firm Fixed Effects		Yes	Yes	Yes
SIC-Year Fixed Effects		Yes	Yes	Yes
Observations		819	819	819
R-squared		0.798	0.798	0.799

Panel C: Cartels and Peer Characteristics

	Pred.	Outcome = Δ*SIC4*	Outcome = Δ*SCORE*	Outcome = Δ*SCORE* (*hg*)
		(1)	(2)	(3)
CARTEL	+	0.053**	0.070***	0.069***
		(2.495)	(3.572)	(3.233)
Firm Fixed Effects		Yes	Yes	Yes
SIC-Year Fixed Effects		Yes	Yes	Yes
Observations		819	819	819
R-squared		0.974	0.879	0.876

Panel D: Peer Characteristics and Peer Group Quality

	Pred.	Outcome = *QUALITY*		
		(1)	(2)	(3)
Δ*SIC4*	+	0.059**		
		(2.032)		
Δ*SCORE*	+		0.355*	
			(1.822)	
Δ*SCORE* (*hg*)	+			0.329*
				(1.836)
Firm Fixed Effects		Yes	Yes	Yes
SIC-Year Fixed Effects		Yes	Yes	Yes
Observations		819	819	819
R-squared		0.803	0.802	0.802

This table presents evidence on the relation between cartel membership and peer selection, among firms that use peer RPE. Panel A presents summary statistics for the peer group characteristics, *QUALITY*, Δ*SIC4*, Δ*SCORE* and Δ*SCORE* (*hg*). Panel B presents results from a regression of *QUALITY* on *CARTEL*, with and without an interaction with concentration. Panel C presents results from a regressions of Δ*SIC4*, Δ*SCORE* and Δ*SCORE* (*hg*) on *CARTEL*. Panel D presents results from regressions of *QUALITY* on Δ*SIC4*, Δ*SCORE* and Δ*SCORE* (*hg*) on *CARTEL*. Firm and SIC-year fixed effects are included in all regression specifications. Below each coefficient, we report t-statistics, based on standard errors clustered by firm.

evidence of such preferences. We observe no evidence that cartel firms are systematically deviating from the artificial peer groups in strategic ways that may be related to their cartels (e.g., trying to enforce them or trying to destabilize them). Cartel members appear to include or exclude co-conspirators based on risk-sharing considerations, without any notable deviation; cartel firms include or exclude co-conspirators from their RPE peer groups almost perfectly in accordance with the artificial peer groups.

6. Supplemental analyses

In this section, we discuss several supplemental analyses. These tests are intended to assess the robustness of our findings, and/or provide additional descriptive insights into the patterns we document. Many of these analyses are tabulated in the Online Appendix.

Table 11
Intensive margin tests.

Panel A: Full Sample

	Pred.	Outcome = Prop. RPE (peer)		
		(1)	(2)	(3)
CARTEL	+	0.029***	0.027**	0.028**
		(5.062)	(2.479)	(2.144)
Year Fixed Effects		Yes	Yes	No
Firm Fixed Effects		No	Yes	Yes
SIC-Year Fixed Effects		No	No	Yes
Observations		22,276	22,276	22,276
R-squared		0.007	0.367	0.482

Panel B: Sub-Sample Using Peer RPE

	Pred.	Outcome = Prop. RPE (peer)		
		(1)	(2)	(3)
CARTEL	+	0.061**	0.074*	0.138
		(2.245)	(1.994)	(1.574)
Year Fixed Effects		Yes	Yes	No
Firm Fixed Effects		No	Yes	Yes
SIC-Year Fixed Effects		No	No	Yes
Observations		1,585	1,585	1,585
R-squared		0.095	0.667	0.766

This table presents a replication of Table 3 using an intensive margin measure of peer RPE. The outcome variable is the proportion of compensation grants in the CEO's pay package that are based on relative performance (i.e., *performancetype* = "Rel"). Panel A includes the full sample; Panel B includes only the sub-sample of observations for which at least one grant is based on relative performance. The analyses are otherwise identical to those presented in Table 3. Below each coefficient, we report t-statistics, based on standard errors clustered by industry (Fama and French 48 Industry Classification) and year.

6.1. *Intensive margin of RPE*

Our main analyses examine the choice to include versus exclude RPE (i.e., the 'extensive margin'). However, our model's predictions regard the *weight* placed on RPE (i.e., the 'intensive margin'). Unfortunately, identifying the weight placed on RPE is typically not feasible from our data, making a perfect examination of the intensive margin impossible. However, we can approximate such a test by assuming that the weight placed on RPE increases with the proportion of grants which include RPE components.[24] Under this assumption, we construct a firm-year intensive margin measure of RPE use, by calculating the proportion of grants in the CEO's pay package which are based on relative performance. Much like the indicator variable for RPE, we find that this proportion is positively associated with cartel membership. Moreover, this proportion is positively associated with cartel membership, even among firms that use RPE. That is, cartel membership appears to explain both whether, and to what extent, firms incorporate RPE into their CEOs' compensation plans. These results are presented in Table 11.

6.2. *Importance of cartel to firm operations*

For cartel membership to affect CEO pay plans, the cartel must be an important aspect of firm operations. If an enormous conglomerate operates in a cartelized market that accounts for a minuscule portion of its sales, it is unlikely that the cartel arrangement would have any bearing on CEO compensation. In contrast, if the firm's primary market is cartelized, the existence of the cartel might be an important consideration for CEO pay plan design.

Our data do not allow us to observe the proportion of each cartel firm's sales that come from the cartelized market. However, we can approximate the importance of the cartel to firm operations based on industry affiliations. For each cartel firm, we calculate the proportion of public companies in the cartel that are in the focal firm's primary 4-digit SIC (per Compustat). If more (less) than half of the public companies in a cartel are in the firm's 4-digit SIC, we consider the cartel to be a "primary" ("secondary") industry cartel, for that firm. We code these cartels using the variables *CARTEL* (*primary*) and *CARTEL* (*secondary*). In our sample, roughly 60% of cartel observations are primary industry cartels, with the remaining being secondary industry cartels.

In Table OA1, we replicate the baseline analyses using these refined measures. We find that *CARTEL* (*primary*) is significantly associated with *RPE* (*peer*) while *CARTEL* (*secondary*) is not. The magnitudes of the coefficients on *CARTEL* (*primary*) are about 50%—100% larger than those in Table 3. This evidence suggests that there is variation in how important cartels are to firm operations; more important cartels drive the relation between cartel membership and RPE.

[24] For example, if one firm gives its CEO four grants of which two are RPE, and another firm gives its CEO five grants of which only one is RPE, we assume that [on average] the CEO of the latter firm has weaker RPE incentives.

6.3. Cartel type and competitive mode

Cartels can operate in heterogeneous ways. Three canonical approaches to collusion are: price fixing, quantity restriction, and market segmentation. While our analytical framework provides the same qualitative predictions, irrespective of collusive style, unmodeled differences could drive a divergence in the relation between cartel membership and RPE. Our data do not allow us to cleanly observe all of this variation, but do distinguish "bid-rigging" cartels from other types.[25]

Using this variation, we separately estimate the relation between cartel membership and RPE for bid-rigging and non-bid-rigging cartels. The results are tabulated in Table OA2. We do not find any significant differences across cartel types. We find that the magnitude of the estimated relation is mostly similar across bid-rigging and non-bid-rigging cartels, but the relation is only statistically strong for non-bid-rigging cartels. The disparity in statistical significance could be due to power; non-bid-rigging cartels outnumber bid-rigging cartels by about two-to-one.

Related to the above, cartels can differ depending on the strategic mode of the product market in which they (ostensibly) compete. The model (in Section OA2) from which we derive our predictions can apply to either price competition or quantity competition, simply by changing the interpretation of the strategic choice, x. As such, we do not have different predictions for Cournot versus Bertrand product markets. That said, we recognize that unmodeled considerations could lead to divergent patterns across different competitive modes, and this could be worth examining.

Categorizing industries as Cournot (strategic substitutes) versus Bertrand (strategic complements) is not a trivial task, and there is no single agreed upon approach. Following Bloomfield (2021) and Bourveau et al. (2020), we use the Kedia (2006) measure of strategic mode, which categorizes industries based on the empirical relation between competing firms' product market aggression; if aggression levels are positively (negatively) associated, strategic actions are said to be complements (substitutes). For these tests, we code Fama and French industries as Cournot versus Bertrand exactly as tabulated in Bloomfield (2021), Table A1.[26] Results are presented in Table OA3. We do not find that the relation between cartel membership and RPE is different depending on the strategic mode.

6.4. Compensation awards

If cartel membership allows firms to better filter risk for their executives, then payout structures should adjust, shifting away from fixed pay (i.e., salary) towards incentive pay (i.e., bonus, stock awards, option awards, etc.). To test for such an effect, we construct the variable $log(\frac{Fixed\ Pay}{Total\ Pay})$, equal to the natural logarithm of salary scaled by total compensation, and examine its within-firm association with cartel membership.

We find that $log(\frac{Fixed\ Pay}{Total\ Pay})$ is negatively associated with cartel membership. This negative relation continues to hold, with the inclusion of controls for firm performance and firm size. When we decompose compensation into fixed and incentive components, *log(Fixed Pay)* and *log(Incentive Pay)*, we find that fixed pay is slightly lower, and incentive pay is slightly higher, during firms' cartelization periods. However, neither effect is statistically significant, on its own.

These results are tabulated in Table OA4. We caveat that these results are highly descriptive in nature. While they are consistent with our predictions, there are many potential reasons why cartel membership and realized compensation awards might be correlated; we cannot claim that the documented associations are necessarily attributable to improved risk-sharing.

6.5. Cartel busts, firm disruptions and optics

The event study results in Table 5 show that cartel firms frequently abandon their use of RPE immediately after their cartels are ended by a regulatory intervention. This evidence is consistent with our risk-sharing versus sabotage trade-off explanation. However, an alternative explanation is that antitrust enforcement actions simply represent major disruptions to firm operations which could potentially result in a lot of changes—including a re-jiggering of executive pay plans. While we cannot directly rule this possibility out, we take a number of steps to assess and mitigate the concern. Across all approaches, we find no evidence to suggest that major operational disruptions/changes around cartel busts drive our results.

First, we replicate the event study analyses, excluding any cases where there was CEO turnover around the cartel bust. If the event study results are driven by instances of executive turnover, it would be more likely that we are simply capturing the operational disruption surrounding these enforcement actions. We find that the event study results are unaffected by this alteration; the event study results are not driven by executive turnover, as shown in Table OA5.

Second, we examine whether firms' risk profiles change around cartel busts in a manner that would reduce the need for RPE. Using the *Peer Availability* measure developed by Bloomfield et al. (2022a), we calculate the year-over-year change in a firm's common risk that could be shielded via peer RPE (i.e., $Peer\ Availability_t - Peer\ Availability_{t-1}$), and refer to this variable as Δ*COMMON RISK*. We find no evidence that common risk exposure changes significantly around cartel busts. While there may be major operational changes around these events, they do not appear to be related to common risk exposure, and thus do not seem likely to drive firms' sudden abandonment of RPE. If anything, based on the results with firm and SIC-year fixed effects, common risk *increases* after busts (by 1.2 percentage points, or 17% of a standard deviation), which would increase the

[25] Bid-rigging is a form of price fixing in markets where transactions occur through bidding.

[26] For a detailed description of how the measure is constructed, see Bloomfield et al. (2022b).

benefits of RPE, not encourage firms to drop it, as observed. However, this effect is not statistically significant (t-statistic ≈ 1.3). These results are tabulated in Table OA6.

Third, we look for evidence of sweeping changes in executive compensation around cartel busts. In particular, we examine pay plan complexity (the total number of compensation grants in the CEO's pay plan), and the split between cash grants and restricted stock grants. Among these aspects of compensation, we observe no changes around cartel busts, as shown in Table OA7. The null results found in Table OA7 do not indicate that pay plans do not change at all, other than changes in RPE. There are many other aspects of pay plan design that we do not examine (e.g., mix of performance metrics, performance targets, delta, vega, pay duration, etc.) which may very well change around cartel enforcement actions. But we do not observe evidence of any overarching changes in pay plan design that would suggest our findings are attributable to major shake-ups in executive pay plans.

An alternative explanation for the event study results is that cartel firms use co-conspirators in their RPE peer groups, and decide to drop RPE after their cartels are detected and dissolved simply as a way to disassociate from their former co-conspirators. If this is the case, then the shift away from RPE would not relate to sabotage concerns, but would instead be a matter of optics. To assess this possibility, we drop any cartel firms that disclose including co-conspirators in their RPE groups, and replicate the Table 5 analyses on this restricted sample. In untabulated results, we find that none of our inferences are affected by this change.

6.6. Price-based versus accounting-based RPE

Most RPE used in executive compensation can be categorized into one of two types: price-based RPE (also known as “relative TSR” or “rTSR”), and accounting-based RPE (e.g., relative profit). The product market sabotage story we propose is likely more salient for accounting-based RPE. Firms' product market strategies will have much more direct effects on current profits than [forward-looking] stock prices. Moreover, cartel participation constrains the types of actions which would affect rival profits, but not necessarily the types of actions which could affect peers’ stock prices, such as peer-harming disclosures (e.g., Bloomfield et al., 2023).

In this light, we examine whether the relation between RPE and cartel participation differs across types of RPE. We find that the relation is more pronounced for accounting-based RPE than for price-based RPE, but there is no statistically significant difference between the two types of RPE. These results are presented in Table OA8.

6.7. Measurement of RPE

The source of our data on executive incentives changed substantially over our sample period. In 2006, the introduction of the CD&A section of the proxy statement forced firms to provide more detailed, clear and credible information about executive incentives. In some cases, this could lead to an *apparent* change in executive incentives, when in fact the underlying incentives were not altered.[27] While it is not obvious that any such measurement issue would systematically bias our results (other than potential attenuation), we validate that the relation between RPE and cartel membership is present, even if we examine only the post-CD&A period.

As a related measurement issue, some RPE grants include both absolute and relative components (coded as *performancetype* = “*AbsRel*″ in Incentive Lab). We exclude these hybrid grants from our primary measures of RPE. In practice, these grants do not function quite like RPE; they are absolute performance goals which “turn off” when relative performance is poor. Barring edge cases, these hybrid grants are unlikely to encourage product market sabotage. Hybrid grants are also less effective for risk-sharing, because they do not reward managers in the case of bad absolute performance, but strong relative performance (e.g., if a manager successfully guides their firm through an economic downturn).

To assess the robustness of our inferences, we consider several alternative constructions of our *RPE* (*peer*) variable using different treatments of the hybrid “AbsRel” grants. First, we treat “Rel” and “AbsRel” grants as interchangeable, and code *RPE* (*peer*) as 1 if either type of grant is present in the CEO pay plan. Second, we consider “AbsRel” in isolation, and code *RPE* (*peer*) as 1 only if there is an “AbsRel” grant in the CEO pay plan. Third, we construct a hierarchical variable which considers hybrid grants to lie in between “Rel” grants and no RPE at all. For this hierarchical alternative, we code *RPE* (*peer*) as 1 if there is a “Rel” grant; $\frac{1}{2}$ if there is an “AbsRel” grant, and 0 if there is no RPE at all.

In Table OA9, we examine the relation between each measure of RPE and cartel membership. In Panel A, we use the entire sample; in Panel B, we use only the post-CD&A observations. Overall, results seem to be stronger, in terms of both economic magnitude and statistical significance, during the post-CD&A period. With respect to the measurement of RPE, including hybrid grants weakens the relation, but it remains statistically significant. Viewed in isolation, hybrid “AbsRel” grants are not associated with cartel membership to any significant degree.

[27] For example, suppose a CEO has always had peer RPE, but this was not clear from the proxy statement until the post-CD&A period. Our analysis would incorrectly treat such a firm as adopting RPE in the first year of the post-CD&A period.

6.8. Logit analysis

In many of our specifications, the dependent variable is a 0/1 indicator. We use linear probability models, as opposed to logit models, because linear models tend to perform more reliably in the face of dense fixed effect structures, avoiding the ‘incidental parameters problem’ (e.g., Neyman and Scott, 1948; Lancaster, 2000). However, in light of the well-documented issues associated with linear probability models (e.g., Maddala, 1986; Horrace and Oaxaca, 2006), we also run analogous logit analyses to verify that the results are not sensitive to our econometric specification. We find that our inferences are unaffected by this alteration, as shown in Table OA10.

7. Conclusion

Agency theory suggests that RPE should be widespread in executive pay packages, given its ability to shield risk averse agents from common shocks. However, empirical work has found that RPE is not nearly the staple one might expect. Until fairly recently, the vast majority of firms did not use any RPE in their CEO's pay packages, at all; a fact known as the “RPE puzzle.”

To shed light on the issue, we develop a stylized model of optimal contracting in imperfectly competitive markets. Our model shows that RPE is an effective tool for improving risk-sharing, but also that it can induce agents to engage in costly sabotage, sacrificing their own performance in order to hurt their rivals. Rational principals recognize this and choose to withhold relative performance incentives when the potential for costly sabotage outweighs the risk-sharing benefits.

We test our model's predictions, empirically, by exploiting cartel participation as a source of variation in the potential for costly sabotage. Consistent with our predictions, we find that cartel members are more likely to use RPE in their CEOs' incentive contracts and construct more economically similar benchmarks, conditional on using RPE. Supporting our sabotage costs versus risk-sharing benefits interpretation, we further document that RPE is associated with more aggressive product market strategies, but only among non-cartel firms. In sum, our study provides evidence that firms avoid using RPE [in part] due to the possibility of costly sabotage, and that cartel membership is effective at curtailing this risk, thereby enhancing the net benefits of RPE.

We caveat that cartel membership could affect the use of RPE through avenues other than those hypothesized. While our empirical results are aligned in suggesting a trade-off between risk-sharing benefits and sabotage costs, we cannot definitively rule out all conceivable alternatives. As an additional caveat, we reiterate that our story is one of RPE-induced costly sabotage, but our empirical evidence on product market outcomes does not cleanly reflect firms’ competitive aggression. Future research could better establish the link between RPE and competitive aggression by examining more detailed data on product market strategy (e.g., product selling prices), though such data are difficult to acquire in a broad sample.

There are still a number of aspects of RPE use that our study is not able to explain. For instance, why wouldn't all firms [at least] use generic RPE (e.g., benchmarking against the S&P 500)? And why wouldn't firms in non-concentrated industries, where the potential for sabotage is substantially less salient, all benchmark against each other? Further study is needed to answer these questions.

Appendix A. Numerical Example of Effective Sabotage

In this Appendix, we outline the intuition for how excess competitive aggression can improve a firm's *relative* profit, despite reducing the firm's *absolute* profit. While the intuition we wish to convey is highly general, for expositional ease, we demonstrate the intuition with a simple numerical example in the context of a linear-demand Cournot duopoly, with constant marginal costs. We first derive and characterize the profit-maximization equilibrium (Section A.1). We then examine whether one firm can unilaterally deviate from this equilibrium behavior in such a way as to improve its relative profitability. We demonstrate that there exists a wide range of sabotage strategies that improve relative profitability, despite sacrificing absolute profitability (Section A.2). In Section A.3, we discuss how generalizable the underlying intuition is.

A.1 Profit-Maximization Equilibrium

We consider two Cournot rivals producing a homogenous good at a constant marginal cost of $c = 1$, and selling at a market price of:

$$p = 4 - (q_1 + q_2), \tag{10}$$

where q_1 and q_2 are the production quantity choices made by firms 1 and 2, respectively. From the above, one can express the two firms' profit functions as:

$$\begin{aligned} \Pi_1(q_1, q_2) &= q_1 \times (p - c), \\ &= q_1 \times (3 - (q_1 + q_2)), \end{aligned} \qquad \begin{aligned} \Pi_2(q_2, q_1) &= q_2 \times (p - c), \\ &= q_2 \times (3 - (q_1 + q_2)). \end{aligned} \tag{11}$$

Taking first order conditions on each profit function, with respect to each firm's production decision, yields the following pair of best response functions:

$$q_1^*(q_2) = \frac{3 - q_2}{2}, \qquad q_2^*(q_1) = \frac{3 - q_1}{2}. \tag{12}$$

The profit-maximization equilibrium, (q_1^{**}, q_2^{**}), occurs when both best response functions are simultaneously satisfied, which in this case occurs at $q_1^{**} = q_2^{**} \equiv q^{**} = 1$. Thus the profit-maximization equilibrium is such that both firms produce one unit of output, and earn one unit of profit.

A.2 Profits and Relative Profits under Overproduction

We next consider whether firm 1 can implement a successful sabotage strategy against firm 2, in such a way that firm 1's *relative* performance improves. Specifically, we presume that firm 2 will behave according to the profit-maximization equilibrium (from Section A.1), and examine whether firm 1 can unilaterally deviate from the equilibrium production quantity, q^{**}, to an alternative production quantity, q^S, which satisfies three criteria.

1. Producing q^S instead of q^{**} lowers firm 1's profits (i.e., $\Pi_1(q_S, q^{**}) < \Pi_1(q^{**}, q^{**})$).
2. Producing q^S instead of q^{**} lowers firm 2's profits (i.e., $\Pi_2(q^{**}, q^S) < \Pi_2(q^{**}, q^{**})$).
3. Producing q^S instead of q^{**} lowers firm 2's profits more than it lowers firm 1's profits (i.e., $\Pi_2(q^{**}, q^{**}) - \Pi_2(q^{**}, q^S) > \Pi_1(q^S, q^{**}) - \Pi_1(q^S, q^{**})$).

A costly sabotage strategy which is effective at boosting relative performance must satisfy all three conditions. Any strategy that violates #1 is not costly; any strategy that violates #2 is not sabotage; any strategy that violates #3 is not effective at improving relative performance.

Substituting $q_2 = q^{**} = 1$ (see Section A.1) into the profit functions from eq. (11), one can calculate profits and relative profits for firms 1 and 2, as functions of q_1:

$$\Pi_1(q_1, q^{**}) = q_1 \times (3 - (q_1 + q^{**})) = q_1 \times (2 - q_1), \tag{13}$$

$$\Pi_2(q^{**}, q_1) = q^{**} \times (3 - (q_1 + q^{**})) = 2 - q_1, \tag{14}$$

$$\Pi_1(q_1, q^{**}) - \Pi_2(q^{**}, q_1) = -q_1^2 + 3q_1 - 2. \tag{15}$$

With respect to Condition #1, we observe that $\Pi_1(q_1, q^{**}) = q_1 \times (2 - q_1)$ is a parabola which reaches its maximum at $q_1 = q^{**} = 1$. Thus, any $q^S \neq 1$ will satisfy Condition #1. This first result is obvious; naturally, any unilateral deviation away from the profit-maximizing quantity, q^{**} will by definition result in lower profits. With respect to Condition #2, we observe that $\Pi_2(q^{**}, q_1) = 2 - q_1$ is strictly decreasing in q_1, thus any $q^S > q^{**} = 1$ will satisfy Condition #2. Therefore, any $q^S > 1$ will jointly satisfy Conditions #1 and #2; it will hurt firm 1's profits, and also firm 2's profits.

Regarding Condition #3, it remains to show that there exists a range of values of q^S such that the harm to firm 2's profits exceeds the harm to firm 1's profits. We observe that $\Pi_1 - \Pi_2$ is a parabola which is equal to zero at $q_1 = 1$ and $q_1 = 2$, and strictly positive in between, reaching a maximum of $\frac{1}{4}$ at $q_1 = \frac{3}{2}$. Thus, any production decision, $q^S \in (1, 2)$ will jointly satisfy all three conditions; it will result in each firm earning lower profits than in the profit-maximization equilibrium, but firm 1 will earn greater relative profits than in the profit-maximization equilibrium. Intuitively, when firm 1 increases its production volume, the market price decreases; both firms bear this cost. However, firm 1 also gets a partially offsetting benefit: the ability to sell additional units (at a positive margin). Firm 2 gets no such off-setting benefit, so the harm to firm 2 exceeds the harm to firm 1.[28]

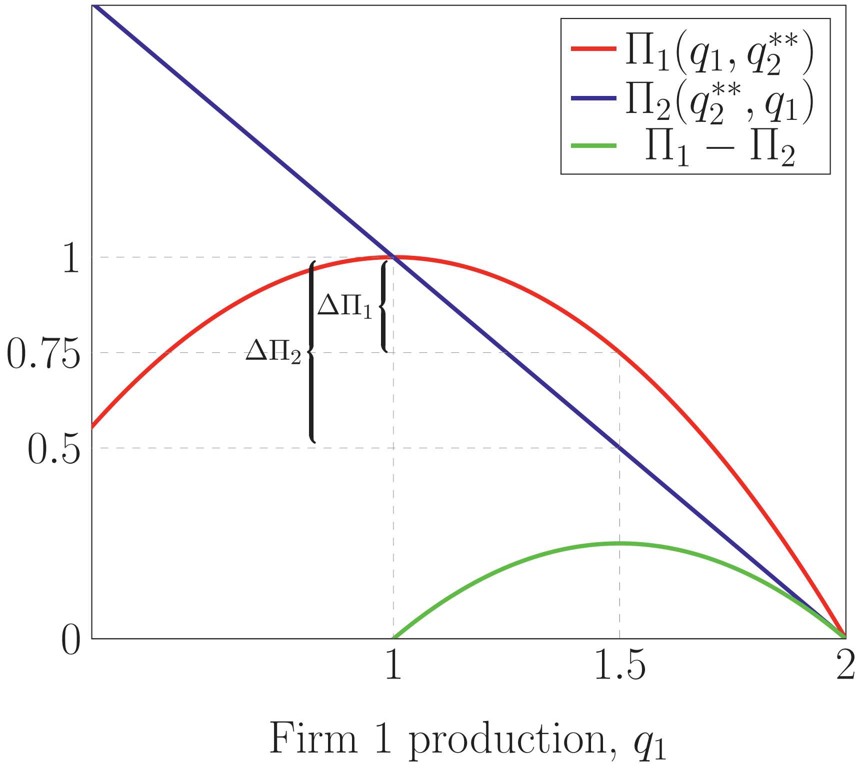
Figure A1 presents a plot of the two firm's profits, along with the difference between them, as functions of q_1. From the plot, it is clear that firm 1's (sufficiently small) increases in production quantity harm firm 1's profits, but harm firm 2's profits to a much greater extent. Relative profits are maximized when firm 1 produces 1.5 units (50% more than the profit-maximizing quantity). This production decision lowers firm 1's profits by 0.25 while lowering firm 2's profits by 0.5 (a net 0.25 increase in firm 1's *relative* profits). The above illustrates the intuition as to why a manager with RPE incentives is likely to choose actions that are more aggressive than is profit-maximizing. Doing so harms the own firm's profits a little bit while harming the competitor (ostensibly also an RPE peer) to a substantially greater degree, thereby boosting the firm's relative performance.

[28] More generally, this effect arises because (convex) profit functions are locally flat around their maxima; small changes in strategy around the profit-maximizing strategy have very small effects on the firm's own profits. Thus, there is ample scope for a firm to change its behavior away from profit-maximization in a way that slightly harms its own profit while significantly harming its rival's profit.

A.3 Discussion of Generality

In the preceding analysis, we demonstrate the existence of effective sabotage tactics that improve a saboteur firm's relative profits, within a specific example of an oligopoly. However, the underlying intuition is far more general than what we demonstrate in this Appendix. The specifics of our example are not critical; the only crucial assumption is oligopolism, as opposed to perfect competition. The above analysis does not extend to situations of perfect competition, but generalizes seamlessly to a broad class of oligopolistic games.[29] In short, the existence of relative profit-improving sabotage tactics is the norm in oligopoly models; it is quite unusual for the profit-maximizing strategy and the relative profit-maximizing strategy to perfectly coincide. As such, RPE incentives almost always distort behavior at least somewhat away from profit-maximization.

Figure A1: Profits and Relative Profits as Functions of q_1.



This figure plots profit functions for a saboteur (firm 1, in red) and a victim (firm 2, in blue), along with the difference between them (in green). The equations are all derived from a Cournot duopoly in which firms produce identical goods at a cost of $c = 1$, and sell them at a market price of $p = 3 - (q_1 + q_2)$, assuming that firm 2 produces according to the profit-maximization equilibrium.

[29] For example, qualitatively speaking, the above generalizes to an n-firm Cournot oligopoly with any arbitrary inverse-demand function, $p(Q)$, and cost curve, $c(q_i)$, so long as the demand function and cost curve are sufficiently well-behaved that standard analysis approaches are valid (e.g., $p' < 0$, $p'' \geq 0$, $c' \geq 0$, $c'' \geq 0$ and $p(0) > c(0)$). Moreover, the notion that excess competitive aggression could improve a firm's relative standing extends to alternative competitive modes (e.g., differentiated Bertrand competition instead of Cournot competition).

Appendix B. Variable Definitions

Panel A: Cartel Membership

Variable	Definition
CARTEL	Indicator variable equal to one for all firm-years which are identified as being part of a cartel membership window. A firm's cartel membership window spans from the first year for which the firm was successfully prosecuted for antitrust violations, to the year of the final antitrust enforcement action
BUST	Indicator variable equal to one if $CARTEL_{i,t-1} = 1$ and $CARTEL_{i,t} = 0$

Panel B: Executive Compensation

Variable	Definition
RPE (*peer*)	An indicator variable equal to one for firm-years in which the CEO has incentive compensation tied to a peer-based relative performance grant (i.e., *performancetype* = "*Rel*" and *performancebenchmark* = "*Peer*" in Incentive Lab)
RPE (*gen.*)	An indicator variable equal to one for firm-years in which the CEO has incentive compensation tied to a peer-based relative performance grant (i.e., *performancetype* = "*Rel*" and *performancebenchmark* = "*S&P* 500" in Incentive Lab)
δRPE (*peer*)	An indicator variable equal to one if $RPE\,(peer)_{i,t-1} = 1$ and $RPE\,(peer)_{i,t} = 0$
δRPE (*gen.*)	An indicator variable equal to one if $RPE\,(gen.)_{i,t-1} = 1$ and $RPE\,(gen.)_{i,t} = 0$
Prop. RPE (*peer*)	The proportion of grants in a CEO's pay plan that are peer-based relative performance grants (i.e., *performancetype* = "*Rel*" and *performancebenchmark* = "*Peer*" in Incentive Lab)

Panel C: Product Market Aggression

Variable	Definition
$log\left(\frac{Sales}{Assets}\right)$	Natural logarithm of revenues, scaled by avg. assets
$log\left(\frac{Costs}{Assets}\right)$	Natural logarithm of income before extraordinary items minus revenues, scaled by avg. assets
$log\left(\frac{Spend}{Sales}\right)$	Natural logarithm of EBITDA minus revenues, scaled by revenues
$log\left(\frac{Ad.\ Spend}{Assets}\right)$	Natural logarithm of advertising expense, scaled by avg. assets

Panel D: Risk Sharing Benefits

Variable	Definition
OWN RETURN	Focal firm's monthly return on its common stock
PEER RETURN	RPE Peer's monthly return on its common stock

Panel E: Firm Characteristics

Variable	Definition
ROA	Income before extraordinary items, scaled by average total assets
log (SALES)	Natural logarithm of GAAP revenues
log (ASSETS)	Natural logarithm of average total assets
log (#DIRECTOR)	Natural logarithm of the number of directors on the board

Panel F: CEO Characteristics

Variable	Definition
log (TENURE)	Natural logarithm the CEO's tenure as CEO at their current firm
log (AGE)	Natural logarithm of CEO's age
FOUNDER	Indicator equal to one if the CEO is a founder of the firm
CHAIR	Indicator equal to one if the CEO is chair of the board

Panel G: Peer Group Characteristics

QUALITY	As constructed in Bloomfield et al. (2022a): *Peer Group Synchronicity* minus *Peer Availability*, where *Peer Group Synchronicity* is the R^2 between a focal firm's daily returns and its actual RPE peer group's daily portfolio returns, and *Peer Availability* is the R^2 between a focal firm's daily returns and the artificial peer group's daily portfolio returns, with artificial peers being as defined by the Bloomfield et al. (2022a) peer selection algorithm.
Δ*SIC*4	Proportion of a focal firm's actual RPE peers in the same 4-digit SIC as the focal firm, minus the proportion of a focal firm's artificial peers in the same 4-digit SIC as the focal firm, with artificial peers being as defined by the Bloomfield et al. (2022a) peer selection algorithm

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(*continued*)

Panel G: Peer Group Characteristics	
$\Delta SCORE$	Average Hoberg and Phillips text-based similarity score between a focal firm and its actual RPE peers, minus the average Hoberg and Phillips text-based similarity score between a focal firm and its artificial peers, where artificial peers are defined using the Bloomfield et al. (2022a) peer selection algorithm
$\Delta SCORE$ (hg)	Same as $\Delta SCORE$, but using the high granularity Hoberg and Phillips text-based similarity scores
Panel H: Concentration	
log (#FIRMS)	Natural logarithm of the number of firms in an industry-year, based on primary 4-digit SICs listed in Compustat
log (HHI)	Natural logarithm of the Herfindal-Hirschman index (i.e., sum of squared market shares) for an industry-year, based on primary 4-digit SICs listed in Compustat

Appendix C. Peer Selection Algorithm

The Bloomfield et al. (2022a) peer selection algorithm works as follows.

1. For each firm-year observation, define a universe of potential peers from which to select RPE peers. Following Bloomfield et al. (2022a), we use 2-digit SIC.
2. Calculate the pair-wise correlation in daily stock returns between the focal firm and each potential peer over the preceding three years.
3. Rank potential peers based on these pair-wise correlations and then form equal-weighted portfolios consisting of the n potential peers with the highest correlations to the focal firm, for $n = 1, 2, 3 \ldots$ up to 50 or the number of potential peers in the universe, whichever is smaller.
4. Calculate the correlation between each n-peer portfolio and the focal firm, over the preceding three years, and determine the n, n^*, for which this correlation is maximized (i.e., the n which maximizes in-sample risk-shielding)
5. Consider this n^*-peer portfolio to be the "artificial" peer group, and evaluate its ability to explain the focal firm's stock returns over the subsequent three years (i.e., calculate out-of-sample R^2). This out-of-sample performance represents how much common risk a firm could reasonably be expected to shield using RPE, assuming the goal is to maximize risk-shielding. Bloomfield et al. (2022a) refer to this out-of-sample performance as *Peer Availability*.

Bloomfield et al. (2022a) shows that the peer groups generated by this algorithm are plausible counterfactuals against which to evaluate firms' RPE choices; they can be constructed entirely from backwards-looking information, and in most cases, are quite similar to the RPE peer groups that firms actually construct. Artificial peer groups and actual peer groups tend to filter similar amounts of risk (artificial peer groups are slightly better in this regard), and there is considerable overlap in terms of peer selection. For a more thorough discussion of the algorithm, and the properties of these artificial peer groups, see Bloomfield et al. (2022a).

Appendix D. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jacceco.2023.101608.

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