DISCUSSION PAPER SERIES

DP15115

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FINANCIAL ECONOMICS

INDUSTRIAL ORGANIZATION

ISSN 0265-8003

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Discussion Paper DP15115
Published 30 July 2020
Submitted 28 July 2020

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JEL Classification: G34, L22

Keywords: Compensation, Collusion, cartels, Relative Performance Evaluation, Sabotage

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Relative Performance Evaluation, Sabotage and Collusion*

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First version March 2019

This version June 2020

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, convicted 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.

Keywords: Compensation; Collusion; Cartels; Relative Performance Evaluation; Sabotage

*We gratefully acknowledge helpful feedback from Thomas Bourveau (discussant), Wayne Guay, Joseph Harrington, Mirko Heinle, Heidi Packard and Cathy Schrand as well as workshop participants at the Carnegie Mellon Accounting Mini-Conference, the Wharton School of the University of Pennsylvania, Stanford University and the Tuck School of Business at Dartmouth. C.Marvão and G.Spagnolo thank the support of a KKV grant (Dnr 409/2019).

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I. 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 one agent's performance against that of 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., Lazear, 1989; Gibbons and Murphy, 1990; Chowdhury and Gürtler, 2015). An agent with significant relative performance incentives will be inclined to take actions that harm the reference groups' 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, Irlenbusch, Kräkel, and Selten, 2007) to higher education (e.g., Royal and Guskey, 2014) and sports (e.g., Del Corral, Prieto-Rodriguez, and Simmons, 2010). We examine whether the potential for RPE-induced sabotage is also an important consideration vis-á-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 costly sabotage is lower.[1]

[1] Of note, Gibbons and Murphy (1990) state that they view costly sabotage as "unlikely" in the context of CEO compensation, arguing that "CEOs tend to have limited interaction with CEOs in rival firms."

In the context of CEO compensation, costly sabotage would likely take the form of overly aggressive product market strategies, such as sub-optimally low prices, extreme output volumes or excessive advertising spending. While these actions are detrimental to own-firm value, they can be even more destructive to peer-firm value, making sabotage an attractive strategy to managers with RPE-based incentives. Accordingly, when the potential for costly sabotage is substantial, rational principals may choose to withhold relative performance incentives, even at the expense of deprecated informational efficiency (and therefore impaired risk-sharing).

Prior 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 these explicit collusive arrangements, otherwise rivalrous firms collectively agree on—and commit to—product market strategies. We posit that this commitment diminishes the potential for costly sabotage and in so doing increases 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 contrast, in industries of below-median concentration, we find no evidence to suggest that cartel membership and firms' use of RPE are associated. These patterns hold both in the cross-section, and within firm, and are robust to controlling for a battery of firm-year and CEO-year characteristics.

We next examine whether, among RPE users, cartel firms construct more economically similar peer groups. 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 select more similar peers than do non-cartel members. Conditional on using RPE, cartel members choose a significantly greater fraction of firms from their own 2-digit SIC and select peers with more similar product offerings. 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.

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, versus driven by some correlated omitted factor, 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") detecting a cartel and successfully bringing an enforcement action against it. While detection and enforcement are, themselves, somewhat endogenous, it seems unlikely that the circumstances leading to a cartel dissolution would be substantially related to decisions about whether or not to include RPE in an executive's pay package, other than through the dissolution itself—especially not in a manner that would change sharply around dissolution events.

We find that firms frequently drop RPE from their CEOs' incentive plans when their cartel membership is terminated by such an enforcement action. Moreover, this pattern is driven by firms in concentrated industries. 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. We find that ∼2.5% of all instances of RPE being dropped coincide with cartel termination, despite these observations representing only 0.5% of the sample. That is, the likelihood that RPE gets dropped from a CEO's pay package is roughly five times higher in the year of cartel termination. Moreover, the result manifests predominantly in more concentrated product markets; in industries of above-median concentration, RPE drop-rates are more than seven and a half times higher among firms from recently terminated cartels. This pattern is not driven by executive turnover and we find no evidence that pay packages change dramatically along other dimensions (e.g., total number of grants; proportion of grants awarding restricted stock). The changes in compensation after a cartel termination appear to be specific to the use of RPE. As in our other analyses, our findings are robust to the inclusion of firm-year and CEO-year controls.

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, thereby facilitating 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 ensures that we base our inferences on within-firm and within-year variation in cartel membership, RPE-reliance and industry concentration, and thereby avoid spurious inferences arising from arbitrary time-invariant cross-sectional heterogeneity and/or sample-wide (or industry-wide) time trends. Furthermore, in placebo tests, we exploit generic RPE (e.g., benchmarking against the S&P 500) to rule out other confounds. 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. As expected, 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.

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 because cartel membership increases the benefits of using RPE? While definitive answers to these questions are difficult to determine, we provide supplemental 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 support for the notion that cartel membership is associated with greater exposure to common risk.

Our work contributes to multiple literatures. First, we contribute 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-based RPE. In contrast, we find no evidence that the potential for collusive shirking factors in to this decision. Moreover, we find 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 use is not ubiquitous in executive pay packages. Ample prior literature has considered the possibility that RPE-induced aggression plays a role in the scarcity of its use, but so far the primary supporting evidence has been the negative relation between industry concentration, and firms' use of RPE (e.g., Aggarwal and Samwick, 1999; Gong, Li, and Shin, 2011; Vrettos, 2013; Bettis, Bizjak, Coles, and Young, 2014).[2] 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 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.

Second, we contribute to the related literature on the role that strategic product market considerations play in shaping executive incentives/corporate governance.[3] 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. Moreover, our work relates to the oft-discussed disciplinary role of product market competition on corporate governance. In an influential piece, Allen and Gale (2000) note the sheer variety of approaches to corporate governance around the world, all seemingly capable of producing world-leading firms across different sectors, and posit that product

[2] See Kabitz (2017) for a recent survey.

[3] See, for example: Fershtman (1985); Vickers (1985); Fershtman and Judd (1987); Sklivas (1987); Fumas (1992); Aggarwal and Samwick (1999); Spagnolo (2000, 2005); Vrettos (2013); Kwon (2016); Bloomfield (2018); Antón, Ederer, Giné, and Schmalz (2018).

market competition alone may be sufficient to discipline managers' behavior. Following work, surveyed in Buccirossi and Spagnolo (2008), stressed that corporate governance structures would still be important, as they can limit or distort product market competition itself. This study suggests yet another angle, highlighting that product market competition may actually harm corporate governance by limiting firms' ability to adopt effective governance tools like RPE.

Third, we contribute to the literature on the welfare consequences of explicit collusion. This literature focuses predominantly on the damaging effects of cartels on pricing or output (e.g., Connor, 2014; Levenstein and Suslow, 2008). In contrast, our work highlights a potential benefit of explicit collusion: by softening competition, it allows shareholders to better share risk with their [relatively undiversified] executives, thereby improving contracting efficiency reducing agency costs. While we do not take the position that these benefits dominate the associated costs to consumers, these benefits will indeed reduce the net social costs of collusion.

The remainder of the paper is organized as follows. In Section II, we develop and state our predictions; in Section III, we detail our data sources, sample construction and variable definitions; in Section IV, we present and discuss our findings; and in Section V, we conclude. In the Appendix, we sketch the analytical framework from which our predictions derive.

II. 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).

However, the utility of RPE is predicated on the notion that other agents' performances 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., 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). 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:

P1: Cartel members are more likely to use RPE, especially in more concentrated product markets.

In setting managerial incentives, the decision to use RPE is not merely a binary choice. In addition to continuous variation in the weight of RPE, firms also have considerable leeway to construct the 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, exacerbating the potential for costly sabotage. By selecting peers that are more economically distant (e.g., firms in different industries), the benchmark is less effective at filtering out the systematic shocks, but the potential for costly sabotage is reduced. Based on the notion 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, we predict:

P2: Among firms that use RPE, cartel members select more economically similar peer firms.

Cartel membership is an endogenous firm choice. Thus, observing a relation between cartel membership and the use of RPE does not imply that a causal relation exists between the two. Perhaps some external factor drives both the utility of RPE and the likelihood of cartel membership. However, the date at which a cartel is detected and broken up is more plausibly exogenous with respect to a firm's compensation practices. Accordingly, we look to cartel dissolutions and predict:

P3: Firms are differentially likely to drop RPE from their executives' pay packages after their cartel is broken up.

The preceding predictions speak to the effect of cartel membership on the use of RPE. To better understand the mechanism underlying any such relation, we examine product market aggression and predict:

P4: RPE is associated with higher product market aggression, but only among non-cartel members.

In the Appendix, we present a sketch of a stylized LEN framework, based on Holmström and Milgrom (1991), from which our predictions derive.[4]

A. Discussion

One might reasonably question the plausibility of our predictions based on the conjecture that shareholders are unlikely to be privy to a firm's cartel membership and thus would not

[4] "LEN" stands for "linear, exponential, normal," denoting that contracts are linear, agents have negative exponential utility and all uncertainty comes from additively separable, normally distributed perturbations.

consider this information in the contracting process. To this concern, we offer two counterpoints. First, our predictions do not require the existence of sophisticated and informed principals who both: (1) understand the impact of cartel membership on optimal compensation policies; and (2) know that the firm is a member of a cartel. Consider a naïve but pragmatic principal who experiments with compensation design, abandoning policies which seem value-reducing and retaining policies which seem value-enhancing. Firm performance will respond more favorably to the use of RPE if sabotage is constrained. Thus, cartel members are more likely to retain such policies.

Second, large shareholders frequently *do* know about their firms' cartel membership. Based on data from Marvão (2015), relating to cartels convicted by the European Commission ("EC"), large shareholders' knowledge of the cartel is mentioned in 35% of cases. This is, of course, a lower bound estimate as we can only observe this for detected cartels and for the cases where it is explicitly written in the public EC cartel reports. Thus, it seems plausible that large shareholders would be privy to the necessary information and internalize this knowledge during the contracting process—even if they are not actively involved in sustaining the collusive agreement.

III. Data, Sample and Variables

A. 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]

[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.

This excerpt covers the years of 1984 to 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. See: Hoberg and Phillips (2010), Hoberg, Phillips, and Prabhala (2014) and Hoberg and Phillips (2016).

B. *Sample Selection*

We construct our sample using all firm-years in the intersection of Compustat and Incentive Lab, over the period of 1998 to 2014. 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.[7] Our final sample consists of 22,276 firm-year observations from 2,026 unique firms, of which 106 firms were cartel members at some point over our sample

[6] Thorough description of this dataset can be found in Marvão (2015) and Levenstein, Marvão, and Suslow (2016).

[7] 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.

period, for a total of 708 firm-year cartel observations.

One concern with the data is the possibility of sample selection bias. Since cartels are prohibited by the Section 1 of the Sherman Act and Article 101 of the Treaty on the Functioning of the European Union, they are secret so the available data include only cartel members that were prosecuted and convicted. This problem of selection on the unobservables cannot be overcome, but its existence is acknowledged in the interpretation of the results. Our analysis can only speak to *detected* cartels—to the extent that undetected cartels exist, and differ from detected cartels on relevant dimensions, our results will be biased.

C. Variables

Below we outline the variables used in our analyses. Table I presents summary statistics.

C.1. Cartel Membership

We measure cartel membership with an indicator variable equal to one for all firm-years which are identified as being members of a cartel. We refer to this measure as $CARTEL$.

We further construct the indicator variable, $BUST$, to reflect firms' transitions from being cartel members to non-cartel members (i.e., when enforcement actions are successfully brought against the firms).[8] $BUST$ which takes a value of one if $CARTEL_{i,t-1} = 1$ and $CARTEL_{i,t} = 0$.

C.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*").[9]

[8] 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.

[9] Some firms include hybrid grants which include both absolute relative components ($performancetype =$ "*AbsRel*"). 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

Within relative incentives, we construct two measures of RPE, one for peer group benchmarks and one for generic benchmarks (e.g., S&P 500). We refer to these variables as RPE ($peer$) and RPE ($generic$).

Analogously to $BUST$, we also construct indicator variables to reflect when RPE is dropped: δRPE ($peer$) and δRPE ($generic$) are equal to one in the rare instance that peer-based RPE or generic-based 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 $RPE_{i,t} = 0$.

C.3. Peer Selection

Conditional on using peer-based RPE, we construct three measures of economic similarity between the peer-group and the own-firm. Our first measure is the proportion of peers that reside in the same 2-digit SIC, $\frac{\#RIVAL}{\#PEER}$. The second and third measures are based on the average similarity of product offerings, based on the cosine similarity of firms' product descriptions. We rely on the similarity scores developed by Hoberg and Phillips (2010), Hoberg et al. (2014) and Hoberg and Phillips (2016), available in their TNIC database. We refer to these measures as $SCORE$ and $SCORE$ (hg), which differ only in that the latter uses the "high granularity" version of TNIC score. Peer group information is broadly available only during the post-CD&A period,[10] so we construct these measures only for post-CD&A firm-year observations.

C.4. 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

measure of RPE reliance.

[10]The Compensation Discussion and Analysis ("CD&A") section of the proxy statement was introduced by the Securities and Exchange Commission in 2006.

advertising spend-to-assets, $log(\frac{Ad.\ Spend_{i,t}}{Assets_{i,t}})$.

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.

C.5. Controls

In robustness tests, we control for several firm and executive characteristics, which could impact the provision of relative incentives (and/or the likelihood of cartel membership). Of note, we do not control for industry-year characteristics (e.g., number of firms, concentration, industry-year prevalence of RPE or cartels, etc.). Our use of industry-year fixed effects flexibly subsumes all time-varying industry-level patterns.

We control for firm profitability, ROA (income before extraordinary items, scaled by average total assets); firm size, $log(Sales)$ (the natural logarithm of GAAP revenues) and $log(Avg.\ Assets)$ (the natural logarithm of average total assets); and board size, $\#Direct$ (the total number of directors on the board). We further control for the CEO's age, $log(Age)$; tenure, $log(1{+}Tenure)$; and indicators for status as a founder, $FOUNDER$ and/or chairman of the board, $CHAIR$.

IV. Empirical Analysis

A. 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 II. In Panel A (Panel B), the dependent variable is RPE ($peer$) (RPE ($generic$)). 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 the final two specifications use firm and SIC-year fixed effects. In the fourth specification, we further include a battery of known RPE determinants as controls.

We find that cartel members are significantly more likely to use RPE than are non-cartel members (Panel A). This result holds both in the cross-section, as well as within-firm and SIC-year. In terms of economic magnitudes, cartel members are approximately twice as likely to use RPE than non-cartel members, both within and across firms. Moreover, this pattern is entirely absent for generic RPE; the relation between generic RPE use and cartel membership is both statistically insignificant and economically *de minimis.*

To further support the notion that these patterns are driven by the potential of 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 III. 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 likely better able to engage in costly sabotage.

B. Peer Selection

We next examine whether firms construct peer groups differently as cartel members than as non-cartel members. We test for such an effect using the following regression specification:

$$Peer\ Similarity_{i,t} = \beta CARTEL_{i,t} + \mu_i + \tau_{j,t} + \varepsilon_{i,t}, \tag{2}$$

where the outcome variable takes one of three different variables: $\frac{\#RIVAL}{\#PEER}$ in specification 1; $SCORE$ in specification 2; and $SCORE$ (hg) in specification 3. As the outcome variables can only be constructed for firms with peer-based RPE, we condition the sample on its use. Thus, this test examines the peer group composition among firms that have elected to use RPE. Furthermore, we restrict the sample to the post-CD&A period as the outcome variables are not broadly constructible without the peer group information contained in the CD&A.

Our results, presented in Table IV, show that cartel members select more economically similar peers. They are more likely to select peers from their own 2-digit SIC (specification 1) and firms that offer more similar products (specifications 2 and 3). In terms of economic magnitudes, the average cartel member selects peers which are 10% more likely to be from their own 2-digit SIC, and offers products which are more than twice as economically similar, as measured by TNIC scores.

Notably, these regressions include firm (and industry-year) fixed effects, so the results should be interpreted as within-firm (and within-industry-year) associations. It is not merely the case that cartel members versus non-cartel members construct different peer groups. Rather, firms appear to change the peer groups against which their executives' performances are compared based on whether or not they are currently members of a cartel.

One overarching concern regards our measurement of cartel membership; we are only able to identify cartel members based on *detected* cartels. To the extent that the type of cartel that gets caught differs from the type which remains undetected in a manner pertinent to the use of RPE, our results may fail to generalize to all cartel firms. More problematic is the possibility that firms' use of RPE causally influences the chances of detection, in which case our analysis would fail to produce unbiased estimates of the relation between cartel membership and the use of RPE. This identification concern is inherent to the nature of our data, and unfortunately not readily addressable in our setting. However, it is worth noting that 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]

C. Event Study: Cartel Terminations

The preceding evidence demonstrates a significant relation between cartel membership and the use of RPE, whereby cartel members are more likely to benchmark against economically similar firms—especially in more concentrated product markets. This evidence is consistent with a '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 contami-

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

nated 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 DOJ and thereafter convicted/dissolved). While these regulatory interventions are, themselves, endogenous, it is hard to think of a confound that would induce spurious inferences related to firms' use of RPE—especially within a short window around the cartel dissolution.

We begin with a graphical investigation, plotting RPE drop-rates, in event time, for the twelve year period centered around the cartel termination date (Figure 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 dissolved. Moreover, the spike in RPE drop-rates manifests only for peer-based RPE; drop-rates for generic RPE remain roughly flat over the entire twelve-year window.

The sharp spike in peer-based 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 set CEO incentives for the year 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 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.

Econometrically, we test for the effect of cartel busts using variants on the following regression specification:

$$\delta RPE_{i,t} = \beta BUST_{i,t-1} + \mu_i + \tau_{j,t} + \varepsilon_{i,t}, \tag{3}$$

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. Our tests exactly mirror those of Tables II and III, with respect to the measure of RPE (peer versus generic), the fixed effect structure, and the 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 V). 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 VI).

While these results are consistent with our predictions, we caveat that cartel membership affects many aspects of a firm's operations beyond merely the potential for costly sabotage, and thus might affect a firm's use of RPE through other channels than those hypothesized. That is, even if our analysis perfectly identifies the causal effect of cartel membership on RPE, we would still not be able to conclude that the change in RPE is necessarily driven by the change in the potential for costly sabotage.

D. 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 reason(s) for the documented link between cartel participation and the use of RPE. 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 strongest support for explanation (2).

D.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:

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

$$RPE_{i,t-1} = \beta_1 CARTEL_{i,t} + \beta_2 RPE_{i,t} + \mu_i + \tau_t + \varepsilon_{i,t}. \tag{5}$$

If cartel membership pushes firms to use RPE, we would expect significant results for RPE_{t+1}. If RPE pushes firms to collude, we would expect significant results for RPE_{t-1}.

We find that cartel participation predicts 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. This finding casts doubt on the alternative explanation that RPE pushes firms to collude. These results are tabulated in Table VII.

D.2. RPE, Cartels and Costly Sabotage

The preceding analyses examine whether cartel participation pushes firms to adopt RPE. While the empirical results comport with our theoretical predictions, they do not explicitly test whether RPE-induced sabotage plays any role, as posited. That is, they do not establish the *mechanism* through which cartel membership causes RPE.

In our next set of tests, we aim to provide evidence on the mechanism by examining whether RPE and cartel membership are associated with product market aggression in the manner assumed by our model/hypothesis development. Aggressive strategies can take many forms, such as low prices or high output volumes. Unfortunately, broad sample data on prices and quantities are not available. Moreover, without knowing the precise nature of the strategic game, it is difficult to determine whether prices or quantities are strategic choices or equilibrium outcomes.[12]

For these reasons, 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 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

[12]For example, if firms compete in a Cournot game, prices are not a choice firms get to make. Firms choose production quantities, which jointly determine an equilibrium price.

aggression in the posited manner using variants on the following regression specification:

$$Aggression_{i,t} = \beta RPE_{i,t} + \mu_i + \tau_{j,t} + \varepsilon_{i,t}, \tag{6}$$

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 VIII.

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-based 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 (*generic*) is statistically insignificant in all four specifications, and economically tiny in three out of the four specifications.[13] 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 VIII lend credence

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

to our supposition that peer-based RPE induces more aggressive product market strategies, and that cartel membership is effective at curtailing this effect.

We caveat that 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 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.

D.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. This could occur for a number of reasons. For example, firm performance could become more susceptible to macro shocks due to collusion-induced 'price rigidity' (e.g., Athey, Bagwell, and Sanchirico, 2004). 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:

$$OWN\ RETURN_{i,t} = \beta_1 CARTEL_{i,t} \times PEER\ RETURN_{i,k,t} + \beta_2 CARTEL_{i,t} + \beta_3 PEER\ RETURN_{i,k,t} + \mu_i + \tau_{j,t} + \varepsilon_{i,t}, \quad (7)$$

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 IX.

We find that peer-firm 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 $CARTEL \times PEER\ RETURN$ 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. Thus, any effect of cartel participation on the use of RPE seems more likely to arise from cost-side rather than benefit-side explanations.

E. Sensitivity Analyses

In this subsection, we examine whether the documented relations between cartel membership and RPE are robust to alternative research design choices. First, we examine whether the associations between cartel membership and the use of RPE are robust to the inclusion of additional firm-year and/or CEO-year controls. Next, we examine whether (1) similar results attain using an alternative definition of RPE which includes hybrid "AbsRel" grants;

and (2) similar results attain in the post-CD&A period, by itself. And last, we examine whether the event study results can be explained by contemporaneous CEO turnover events.

E.1. Analyses with Added Controls

To assess whether the relation between cartel membership and RPE might be driven by some correlated omitted factor, we replicate our analyses with additional controls for firm and CEO characteristics. We present these results in Table X. In Panel A, we re-examine the relation between cartel membership and RPE, by replicating specification 3 of Table II Panel A. In Panel B, we re-examine the relation between cartel terminations and RPE drop-rates, by replicating specification 3 of Table V Panel A.

In the first specification, we present the analyses without controls (exactly replicating the earlier analyses). In the second specification, we include controls for the firm's current profitability, size (both sales and assets), and board size. In the third specification, we instead include controls for the CEO's current age, tenure, and status as a founder and/or chairman of the board. In the fourth specification, we include all controls jointly. We find that our results remain statistically significant and economically comparable across all specifications.

E.2. Measurement of RPE variable

The source of our data on executive incentives changed substantially over our sample period. In 2006 and 2007, 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 have not been altered.[14] While it is not obvious that any such measurement issue would systematically bias our results, we examine whether the relation

[14]For example, suppose a CEO has always had peer-based 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.

between RPE and cartel membership is present in the post-CD&A period. We find that our results are qualitatively similar—and perhaps even stronger—in the post-CD&A period. These results are presented in Table XI, Specifications 1 and 2. Specification 1 uses the full sample, while Specification 2 includes only the post-CD&A observations.

As a related measurement issue, many RPE grants include both absolute and relative components (coded as awardtype="AbsRel" by Incentive Lab). Such grants are likely to ameliorate the potential for costly sabotage, as they implicitly downplay the *relative* component of the pay plan. Nonetheless, we examine whether our results are affected by including these hybrid grants in our measure of RPE utilization. In sensitivity analysis, we code firms as using RPE if a firm uses peer-based "Rel" or "AbsRel" grants in the CEO's incentive compensation plan. We find that our results are similar, though perhaps a little weaker, using this broader measure of RPE use. Moreover, the relation is present in the full sample, as well as in the post-CD&A period. These results are presented in Table XI, Specifications 3 and 4. Specification 3 uses the full sample, while Specification 4 includes only the post-CD&A observations.

E.3. No CEO Turnover

Contract terms are often quite sticky in the sense that once given, they are rarely taken away. One conceivable explanation for our event study results is that contract terms are less sticky around the incidence of CEO turnover. In this case, RPE drop-rates could increase around cartel busts simply because of CEO turnover around cartel busts. To rule out this possibility, we replicate the event study on a no-turnover sample. We find that our results are robust to this alteration, as shown in Table XII.

F. Supplemental Analyses

In this subsection, we discuss several additional analyses, for which results have been left untabulated.

F.1. 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 sabotage story we propose is likely more salient for accounting-based RPE, as firms’ product market strategies will have much more direct impacts on contemporaneous earnings 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 rival stock prices (e.g., adversarial disclosure practices).

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 is significantly positive across both types.

F.2. Other Aspects of Compensation

Our main analyses show a within-firm and industry-year relation between cartel participation and peer-based RPE. Moreover, we find that in the immediate aftermath of a cartel bust, busted cartel firms are disproportionately likely to drop RPE from their CEOs’ pay packages. One explanation for this pattern is that cartel membership (and cartel busts) are responsible for major shake-ups in executive pay practices, of which the use of RPE is only a small component.

We examine several other aspects of pay, and find no evidence of similar associations with cartel membership. In particular, with the inclusion of firm and (SIC-)year fixed effects, cartel participation is statistically unassociated with the total number of compensation grants, as well as the proportion of grants using restricted stock awards versus cash awards. In sum, we find no dramatic shifts in compensation structure, other than the use of RPE.

F.3. 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).

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.

F.4. Benchmarking Against Cartel Firms

Our model does not offer any predictions as to whether firms would prefer to choose cartel versus non-cartel firms as RPE peers. Nonetheless, one might reasonably be curious about whether the cartel firms which use RPE benchmark against other firms in the same cartel. To this end, we examine whether a firms' participation in a cartel is associated with its likelihood of being selected as a peer, and find that cartel firms are disproportionately likely to be selected as RPE peers by other firms in the same cartel. This relation holds even after controlling for the industry-level prevalence of cartel participation.

F.5. Intensive Margin

Related to the above, our 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.[15]

Under this assumption, we construct a firm-year pseudo-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.

V. 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, while used with some regularity, is not nearly the staple one might expect. A strong majority of firms do not use any RPE in their CEO's pay packages, a fact which has come to be known as the "RPE puzzle."

We develop a stylized model of optimal contracting in imperfectly competitive markets showing these two results, i.e., 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 peers. Rational principals recognize this and choose to withhold relative performance incentives when the potential for costly sabotage outweighs the risk-sharing benefits.

[15]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.

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. Consistent with the proposed mechanism, we provide evidence 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 possibility and thereby enhancing the net benefits of RPE.

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 highly competitive industries, where the potential for sabotage is substantially less salient, all benchmark against each other? Further study is needed to answer these questions.

Finally, we reiterate that cartel membership could affect the use of RPE through avenues other than our hypothesized 'costly sabotage' channel. Thus, even if our analysis accurately reflects the causal effect of cartel membership on the use of RPE, this does not necessarily imply that we have demonstrated the effect of costly sabotage potential on RPE. While our mechanism tests support our interpretation, we view this study as providing an important first step towards understanding this issue, rather than the final say on it. Future quasi-experimental work could utilize different settings to better triangulate the extent to which the 'costly sabotage' explanation drives firms' avoidance of RPE.

Appendix: Stylized Model

We develop our hypotheses using a simple LEN framework, based on Holmström and Milgrom (1987, 1991). We consider a risk and effort averse agent who may or may not have control over firm strategy. Throughout our analysis, we assume that the agent can choose to expend a level of effort, e, at quadratic personal cost. In Sections A2 and A3.2, we further assume that the agent can choose the firm's level of product market aggression, x, at a quadratic cost to the firm.

Profits for firm i and a representative peer firm, j, are described by:

$$\Pi^i(e,x) = \pi^i + e + x - \tfrac{x^2}{2} + \epsilon^i + \eta, \tag{8}$$

$$\Pi^j(x) = \pi^j - \gamma x + \epsilon^j + \eta, \tag{9}$$

where π^i and π^j are the baseline profit levels, x and $\frac{x^2}{2}$ are the benefits and costs (to firm i) of aggression level x, and γ is the extent to which firm i's aggression hurts the representative peer's profit.[16] Perturbations ϵ^i, ϵ^j, and η represent normally distributed performance shocks, with ϵ's being idiosyncratic, and η being systematic (i.e., the 'common shock').

The only contractible metrics are absolute profit, Π^i, and relative profit, $\Pi^i - \Pi^j$, for which the agent is given a linear incentive contract:

$$\begin{aligned} w &= S + \beta\left((1-\alpha)\Pi^i + \alpha(\Pi^i - \Pi^j)\right) \\ &= S + \beta\left(\Pi^i - \alpha\Pi^j\right), \end{aligned} \tag{10}$$

where S is the base salary, β is the contract's 'incentive intensity,' and α is the relative

[16] This method of modeling aggressiveness is consistent standard game-theoretic models of competition, such as Cournot and differentiated Bertrand. In both settings, aggression carries a linear benefit and a quadratic cost, vis-á-vis own-firm profit, and imposes a linear penalty on rival profit, moderated by the degree of product similarity.

weight on RPE. If $\alpha = 0$ ($\alpha = 1$), the contract is purely based on own-profit (relative profit).

In what follows, we consider three cases: (1) a single-task scenario, in which the agent only has control over effort; (2) a multi-tasking scenario in which the agent has full control over both personal effort, e, and the firm's strategy, x; and (3) an extension where the principal chooses not only the compensation contract, but also the peer group against which relative performance is measured.

A1 Single-task Case

The agent's problem is to choose e^* to maximize his expected utility (i.e., certainty equivalent), taking the incentive contract as given. An agent with negative exponential utility and risk aversion, r, has a certainty equivalent, ACE:

$$\begin{aligned} ACE &= E[w] - \tfrac{e^2}{2} - \tfrac{r^2}{2} Var(w) \\ &= S + \beta\left(\pi^i + e + x - \tfrac{x^2}{2} - \alpha(\pi^j - \gamma x)\right) - \tfrac{e^2}{2} - \tfrac{r^2}{2}\beta^2\left(\sigma^2_{\epsilon^i} + \alpha^2\sigma^2_{\epsilon^j} + (1-\alpha)^2\sigma^2_{\eta}\right), \end{aligned} \tag{11}$$

thus making the agent's problem:

$$\arg\max_e \left\{ S + \beta\left(\pi^i + e + x - \tfrac{x^2}{2} - \alpha(\pi^j - \gamma x)\right) - \tfrac{e^2}{2} - \tfrac{r^2}{2}\beta^2\left(\sigma^2_{\epsilon^i} + \alpha^2\sigma^2_{\epsilon^j} + (1-\alpha)^2\sigma^2_{\eta}\right) \right\}. \tag{12}$$

The first order condition on e implies that the agent's certainty equivalent admits the following incentive compatibility constraint:

$$e^* = \beta. \tag{13}$$

The principal seeks to construct a contract (S^*, α^*, β^*) which maximizes her firm's [after compensation] profit, subject to the agent's incentive compatibility and individual rationally constraints. As ex ante surplus can always be efficiently shifted from agent to principal, and vice versa, through the salary, S, the optimal incentive parameters (α^*, β^*) are those which maximize the combined 'certainty equivalents' of principal and agent (Holmström and Milgrom, 1991). The total certainty equivalent, TCE, is:

$$\begin{aligned} TCE \equiv & E[\Pi^i - w] + ACE \\ = & E[\Pi^i] - \cancel{E[w]} + \cancel{E[w]} - \tfrac{e^{*2}}{2} - \tfrac{r^2}{2}\beta^2 \left(\sigma^2_{\epsilon^i} + \alpha^2\sigma^2_{\epsilon^j} + (1-\alpha)^2\sigma^2_\eta\right) \\ = & \pi^i + e^* + x - \tfrac{x^2}{2} - \tfrac{e^{*2}}{2} - \tfrac{r^2}{2}\beta^2 \left(\sigma^2_{\epsilon^i} + \alpha^2\sigma^2_{\epsilon^j} + (1-\alpha)^2\sigma^2_\eta\right), \end{aligned} \tag{14}$$

thus making the principal's problem:

$$\arg\max_{\alpha,\beta} \left\{ \pi^i + e^* + x - \tfrac{x^2}{2} - \tfrac{e^{*2}}{2} - \tfrac{r^2}{2}\beta^2 \left(\sigma^2_{\epsilon^i} + \alpha^2\sigma^2_{\epsilon^j} + (1-\alpha)^2\sigma^2_\eta\right) \right\}, \tag{15}$$

$$\text{subject to} \quad e^* = \beta. \tag{16}$$

First order conditions on α and β reveal that:

$$\begin{aligned} \alpha^* &= \frac{\sigma^2_\eta}{\sigma^2_\eta + \sigma^2_{\epsilon^j}}, \\ \beta^* &= \frac{1}{1 + r^2\left(\sigma^2_{\epsilon^i} + \alpha^2\sigma^2_{\epsilon^j} + (\alpha-1)^2\sigma^2_\eta\right)}. \end{aligned} \tag{17}$$

Thus, the optimal extent of RPE is that which best shares risk between principal and agent (i.e., α^* is the minimizer of $Var(w)$). This result is unsurprising, as α only appears in the total certainty equivalent through its impact on $Var(w)$, and is wholly aligned with standard results in the extant theoretical literature.

A2 Multi-task Case

Suppose instead that the agent has full control over both personal effort and firm strategy. In this case, the agent's problem is to choose (e^*, x^*) to maximize his expected utility (i.e., certainty equivalent), taking the incentive contract as given.

$$\arg\max_{e,x} \left\{ S + \beta\left(\pi^i + e + x - \tfrac{x^2}{2} - \alpha(\pi^j - \gamma x)\right) - \tfrac{e^2}{2} - \tfrac{r^2}{2}\beta^2 \left(\sigma^2_{\epsilon^i} + \alpha^2\sigma^2_{\epsilon^j} + (1-\alpha)^2\sigma^2_\eta\right) \right\}. \tag{18}$$

In this case, the agent's maximization problem yields two incentive compatibility constraints, one for effort and one for firm strategy:

$$e^* = \beta, \tag{19}$$

$$x^* = 1 + \gamma\alpha. \tag{20}$$

As in Section A1, greater incentive intensity pushes the agent to exert more effort (a standard result in the agency literature). More novel is the second constraint: that RPE pushes the agent to a sub-optimally high level of aggression (from the principal's perspective). The more effectively his actions hurt the peer's performance (i.e., the higher γ is), the more RPE causes the agent's action to depart from the optimal level, $x = 1$.

Now the principal's problem is more complicated, as it must adhere to both constraints. The principal's augmented problem is now:

$$\arg\max_{\alpha,\beta} \left\{ \pi^i + e^* + x^* - \tfrac{x^{*2}}{2} - \tfrac{e^{*2}}{2} - \tfrac{r^2}{2}\beta^2 \left(\sigma^2_{\epsilon^i} + \alpha^2\sigma^2_{\epsilon^j} + (1-\alpha)^2\sigma^2_\eta\right) \right\}, \tag{21}$$

$$\text{subject to} \quad e^* = \beta, \tag{22}$$

$$x^* = 1 + \gamma\alpha. \tag{23}$$

Differentiating by β yields the first order condition for incentive intensity:

$$\beta^* = \frac{1}{1 + r^2\left(\sigma^2_{\epsilon^i} + \alpha^2\sigma^2_{\epsilon^j} + (1-\alpha)^2\sigma^2_\eta\right)}. \tag{24}$$

Substituting e^*, x^* and β^* into the total certainty equivalent yields the total certainty equivalent expressed purely as a function of α (the choice variable of interest), and the exogenous parameters, γ r, π^i and the σ's:

$$TCE(\alpha; \cdot) = \pi^i + \tfrac{1}{2} + \frac{1}{2}\left(\underbrace{\frac{1}{1 + r^2\left(\sigma^2_{\epsilon^i} + \alpha^2\sigma^2_{\epsilon^j} + (1-\alpha)^2\sigma^2_\eta\right)}}_{\beta^*(\alpha)} - \alpha^2\gamma^2\right). \tag{25}$$

$TCE(\alpha; \cdot)$ decomposes neatly into three components: a constant term; a risk-sharing benefit, $\beta^*(\alpha)$; and a sabotage cost, $\alpha^2\gamma^2$. The first order condition implies that the optimal incentive parameter, α^*, is that which equalizes the marginal risk-sharing benefit and the marginal sabotage cost, satisfying:

$$\frac{r^2\left((1-\alpha^*)\sigma^2_\eta - \alpha^*\sigma^2_{\epsilon^j}\right)}{\left(1 + r^2\left((1-\alpha^*)^2\sigma^2_\eta + \sigma^2_{\epsilon^i} + \alpha^{*2}\sigma^2_{\epsilon^j}\right)\right)^2} = \alpha^*\gamma^2. \tag{26}$$

While this optimal incentive parameter is difficult to express in closed form, generally, it exhibits a number of intuitive features. If $\gamma = 0$, then there is no risk of sabotage, and the optimal α is that which best shields the agent from risk: $\alpha^* = \frac{\sigma^2_\eta}{\sigma^2_\eta + \sigma^2_{\epsilon^j}}$ (as in Section A1). In contrast, if $r = 0$ or $\sigma_\eta = 0$, then there is no risk-sharing benefit, so the optimal incentive contract involves no RPE whatsoever, $\alpha^* = 0$.

In general, α^* will be no larger than $\frac{\sigma^2_\eta}{\sigma^2_\eta + \sigma^2_{\epsilon^j}}$ (α^* from the single-task case), and will be strictly smaller than $\frac{\sigma^2_\eta}{\sigma^2_\eta + \sigma^2_{\epsilon^j}}$ as long as $\gamma \neq 0$, $\sigma_\eta \neq 0$ and $r \neq 0$. Moreover, α^* decreases as γ becomes larger in magnitude. That is, the more the firm's actions affect its peers' profits, the lower the optimal use of RPE. Figure A1 presents plots of $\alpha^*(\gamma)$ for various values of σ_η

and σ_{ϵ^j}.

A3 Extension: Endogenous Peer Selection

Thus far in the analysis, we have assumed that the peer group is exogenous, and not a choice for the principal. In this Section, we depart from this assumption, and allow the principal to choose the economic similarity of the peer group. We capture this decision with the choice variable $\rho \in [0,1]$, which determines both the degree of common shock overlap, and the extent to which firm i's strategy affects the representative peer's profits.

$$\Pi^i(e,x) = \pi^i + e + x - \tfrac{x^2}{2} + \epsilon^i + \eta, \tag{27}$$

$$\Pi^j(x) = \pi^j - \rho\gamma x + \epsilon^j + \rho\eta. \tag{28}$$

By choosing a more similar peer group (i.e., ρ closer to one), benchmarking against the peer better filters out systematic uncertainty, as the representative peer will be more exposed to the same common shock. However, the more similar peer is also more strongly affected by the firm's product market strategy, thus making costly sabotage a more viable option.

A3.1 Single-task Case

As in Section A1, the agent chooses effort, e, to maximize his certainty equivalent, taking the contract terms (including the peer group's similarity) as given, yielding the incentive compatibility constraint:

$$e^* = \beta. \tag{29}$$

Thus, the principal's problem is:

$$\arg\max_{\alpha,\beta,\rho} \left\{ \pi^i + e^* + x - \tfrac{x^2}{2} - \tfrac{e^{*2}}{2} - \tfrac{r^2}{2}\beta^2 \left(\sigma^2_{\epsilon^i} + \alpha^2 \sigma^2_{\epsilon^j} + (1-\rho\alpha)^2 \sigma^2_\eta \right) \right\} \tag{30}$$

$$\text{subject to} \quad e^* = \beta. \tag{31}$$

First order conditions on the total certainty equivalent show that the optimal incentive parameters (α^*, β^*) are:

$$\alpha^* = \frac{\rho\sigma^2_\eta}{\rho^2\sigma^2_\eta + \sigma^2_{\epsilon^j}}, \tag{32}$$

$$\beta^* = \frac{1}{1 + r^2 \left(\sigma^2_{\epsilon^i} + \alpha^2 \sigma^2_{\epsilon^j} + (1-\rho\alpha)^2 \sigma^2_\eta \right)}. \tag{33}$$

Substituting these into the total certainty equivalent, and differentiating by ρ, yields:

$$\frac{dTCE(\rho)}{d\rho} = \frac{r^2 \rho \sigma^4_\eta \sigma^2_{\epsilon^j}}{\left(\rho^2 \sigma^2_\eta \left(r^2 \sigma^2_{\epsilon^i} + 1 \right) + \sigma^2_{\epsilon^j} \left(r^2 \left(\sigma^2_\eta + \sigma^2_{\epsilon^i} \right) + 1 \right) \right)^2}, \tag{34}$$

which is strictly non-negative. Thus, the first order condition for ρ is never satisfied, and the principal will optimally choose the corner solution: $\rho^* = 1$. The optimal contract is identical to that derived in Section A1; when the agent has no control over firm strategy (only personal effort), the optimal contract is designed to optimize risk-sharing, by setting α^* to the optimal risk-sharing level, and using a peer group which is as similar as possible.

A3.2 Multi-task Case

As in Section A2, the agent maximizes his certainty equivalent, taking the contract terms (now including the peer group's similarity) as given, yielding the incentive compatibility constraints:

$$e^* = \beta, \tag{35}$$

$$x^* = 1 + \rho\gamma\alpha. \tag{36}$$

Thus, the principal's problem is:

$$\arg\max_{\alpha,\beta,\rho} \left\{ \pi^i + e^* + x^* - \tfrac{x^{*2}}{2} - \tfrac{e^{*2}}{2} - \tfrac{r^2}{2}\beta^2 \left(\sigma^2_{\epsilon^i} + \alpha^2\sigma^2_{\epsilon^j} + (1-\rho\alpha)^2\sigma^2_\eta\right) \right\} \tag{37}$$

$$\text{subject to} \quad e^* = \beta \tag{38}$$

$$x^* = 1 + \gamma\alpha. \tag{39}$$

The first order condition for β is:

$$\beta^* = \frac{1}{1 + r^2\left(\sigma^2_{\epsilon^i} + \alpha^2\sigma^2_{\epsilon^j} + (1-\rho\alpha)^2\sigma^2_\eta\right)}, \tag{40}$$

which, when substituted back into the total certainty equivalent yields:

$$TCE(\alpha, \rho) = \pi^i + \tfrac{1}{2} + \frac{1}{2}\left(\underbrace{\frac{1}{1 + r^2\left(\sigma^2_{\epsilon^i} + \alpha^2\sigma^2_{\epsilon^j} + (1-\rho\alpha)^2\sigma^2_\eta\right)}}_{\beta^*(\alpha,\rho)} - \alpha^2\rho^2\gamma^2 \right). \tag{41}$$

As in Section A2, $TCE(\alpha, \rho)$ decomposes into a constant term, a risk-sharing benefit ($\beta^*(\alpha, \rho)$), and a sabotage cost ($\alpha^2\rho^2\gamma^2$).

Explicit representations for α^* and ρ^* do not readily admit closed-form expressions, but for a given value of α, ρ^* satisfies:

$$\frac{r^2\sigma^2_\eta(1-\rho^*\alpha)}{\left(1 + r^2\left(\alpha^2\sigma^2_{\epsilon^j} + \sigma^2_\eta(1-\rho^*\alpha)^2 + \sigma^2_{\epsilon^i}\right)\right)^2} = \rho^{*2}\gamma^2\alpha, \tag{42}$$

if such an interior solution exists on $\rho \in [0, 1]$, and $\rho^* = 1$ otherwise. Figure A2 presents plots of $\rho^*(\alpha)$ for various values of σ_η. Notably, $\rho^*(\alpha)$ falls below 1 (i.e., the first order condition is satisfied on the interior of $[0, 1]$) for a considerable domain of α, suggesting that it is often optimal for firms to intentionally construct a peer group that is considerably less economically similar than feasible, so as to mitigate the extent of costly sabotage.

A4 Discussion: Application to Cartel Setting

We model optimal incentives and peer group construction under two different assumptions about the agent's choice set: (1) the agent has full control to decide both personal effort and the firm's strategy; and (2) the agent takes the firm's strategy as given, choosing only personal effort. We find that, in the presence of externalities, whereby the firm's strategy affects the representative peer's profits (i.e., when $\gamma \neq 0$) the optimal use of RPE is strictly lower if the manager has control over firm strategy. This arises due to the adverse sabotage incentives RPE provides.

In many cartels, firms jointly agree on, and commit to, their product market strategies (e.g., 'price fixing'). Thus, cartel membership effectively removes the choice over x from the agent's choice set, severing the potential for RPE to induce sabotage, thereby increasing the net benefits of its use.

Alternatively, some cartels work by dividing the market into non-overlapping monopolistic market segments, with each firm controlling one (but otherwise retaining autonomy over firm strategy). While this type of cartel does not fall neatly into the single-task/multi-task framework, our model still captures it just as well. By dividing the markets into non-overlapping segments, γ is effectively set to zero (or nearly so), which results in the exact same use of RPE as the single-tasking case: $\alpha^* = \frac{\sigma_\eta^2}{\sigma_\eta^2 + \sigma_{\epsilon^j}^2}$ and $\rho^* = 1$.

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Figure A1. Optimal Use of RPE

This figure plots the optimal weight on RPE, $\alpha^*(\gamma)$, as specified implicitly by eq. (26). We present the relation for three values of σ_{ϵ^j} and four values of σ_η. In Panel A (Panel B) [Panel C], $\sigma_{\epsilon^j} = 0$ ($\sigma_{\epsilon^j} = \frac{1}{2}$) [$\sigma_{\epsilon^j} = 1$]. Within each panel, we present four relations, for each of: $\sigma_\eta = 0$ (in blue); $\sigma_\eta = \frac{1}{2}$ (in orange); $\sigma_\eta = 1$ (in green); and $\sigma_\eta = 1$ (in red). Throughout the figure, we set $r = \sigma_\epsilon^i = 1$.

Panel A: $\sigma_{\epsilon^j} = 0$ Panel B: $\sigma_{\epsilon^j} = \frac{1}{2}$ Panel C: $\sigma_{\epsilon^j} = 1$

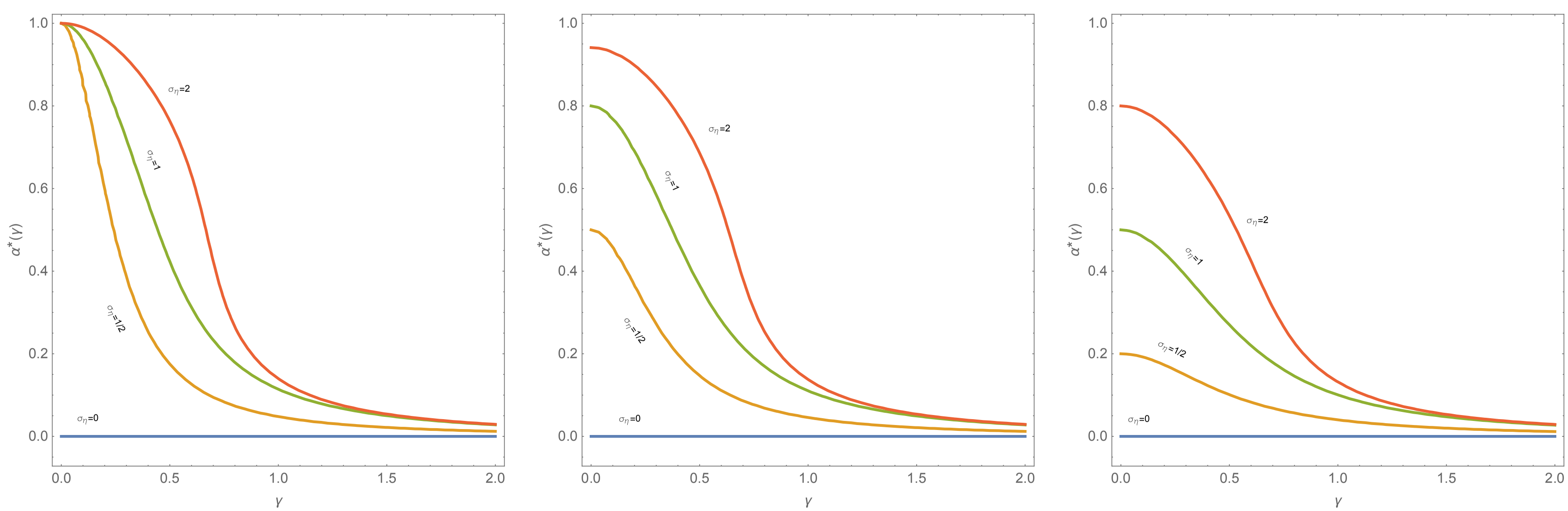

Figure A2. Optimal Peer Similarity

This figure plots the optimal peer similarity, $\rho^*(\alpha)$, as specified implicitly by eq. (42). we present four relations, for each of: $\sigma_\eta = 0$ (in blue); $\sigma_\eta = \frac{1}{2}$ (in orange); $\sigma_\eta = 1$ (in green); and $\sigma_\eta = 1$ (in red). Throughout the figure, we set $r = \sigma_\epsilon^i = 1$, and $\gamma = \frac{1}{2}$.

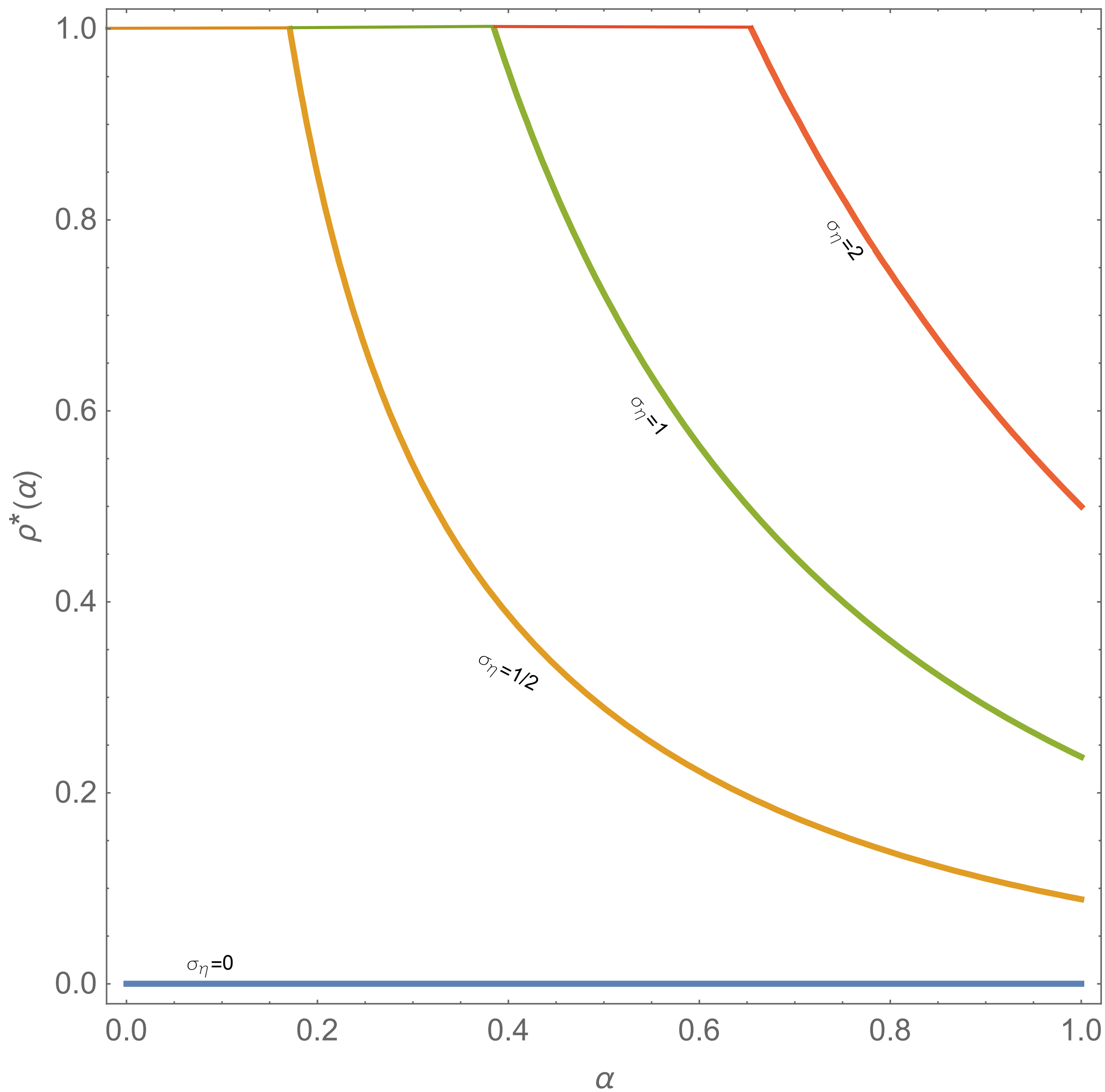

Figure 1. Dropping RPE around Cartel Terminations

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 $CARTEL_{t \geq 0} = 0$.

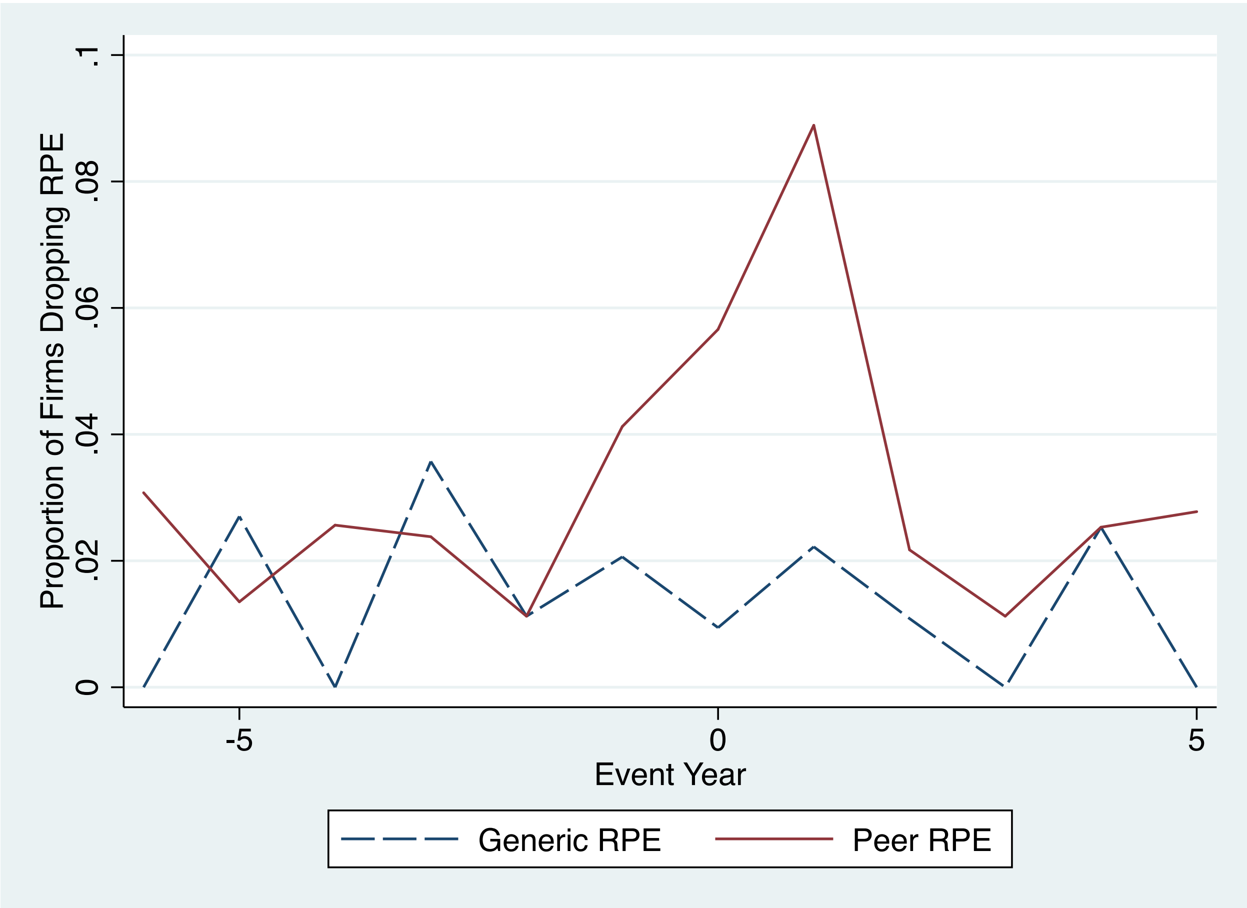

Table I. Summary Statistics

This table presents descriptive statistics for all variables used in our main analyses.

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
Incentives						
RPE (peer)	22,276	0.071	0.257	0.000	0.000	0.000
RPE (generic)	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 (generic)	22,276	0.009	0.094	0.000	0.000	0.000
Peer selection						
$\frac{\#\text{RIVAL}}{\#\text{PEER}}$	906	0.724	0.320	0.471	0.882	1.000
SCORE	906	0.050	0.059	0.000	0.027	0.083
SCORE (hg)	906	0.074	0.069	0.003	0.062	0.118
Aggression						
$\log(\frac{\text{Sales}}{\text{Assets}})$	21,556	−0.666	1.054	−1.230	−0.463	0.057
$\log(\frac{\text{Costs}}{\text{Assets}})$	21,571	−0.705	1.048	−1.268	−0.517	0.018
$\log(\frac{\text{Spend}}{\text{Sales}})$	19,119	−0.198	0.431	−0.316	−0.188	−0.103
$\log(\frac{\text{Ad. Spend}}{\text{Assets}})$	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
#DIRECT	16,029	9.991	2.633	8.000	10.000	11.000
CEO characteristics						
log(1+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

Table II. Cartels and Relative Performance Evaluation

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 and the dependent variable. In Panel A (Panel B), the dependent variable is RPE (*peer*) (RPE (*generic*)). Across both panels, 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.

Panel A: Peer RPE

		Outcome = RPE (peer)		
	Pred.	(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

		Outcome = RPE (generic)		
	Pred.	(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

Table III. Cartels and Relative Performance Evaluation, by Concentration

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 II Panel A, but the sample is cut in half, based on concentration at the SIC 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.

		Outcome = RPE (peer)			
	Pred.	(1)	(2)	(3)	(3)
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

Table IV. Peer Selection

This table presents evidence on the relation between cartel membership and peer selection, among firms that use peer-based relative performance evaluation. The estimating equation is:

$$Peer\ Similarity_{i,t} = \beta CARTEL_{i,t} + \mu_i + \tau_{j,t} + \varepsilon_{i,t}.$$

We use three different measures of peer similarity: in Specification 1, we use proportion of same-industry peers (by 2-digit SIC); in Specification 2, we use the average Hoberg and Phillips TNIC score and in Specification 3 we use the average Hoberg and Phillips TNIC high granularity score. Each specification includes firm and SIC-year fixed effects. The sample consists of peer-based RPE users (i.e., $RPE\ (peer) = 1$) from the post-CD&A period. Below each coefficient, we report a t-statistic based on standard errors clustered by firm and year (clustering by industry is not econometrically feasible for these regressions).

	Pred.	Outcome = $\frac{\#RIVAL}{\#PEER}$ (1)	Outcome = SCORE (2)	Outcome = SCORE (hg) (3)
CARTEL	+	0.047***	0.083***	0.073**
		(3.314)	(2.929)	(2.377)
Firm Fixed Effects		Yes	Yes	Yes
SIC-Year Fixed Effects		Yes	Yes	Yes
Observations		906	906	906
R-squared		0.937	0.694	0.688

Table V. Busted Cartels and RPE Drop-Rates

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

$$\delta < RPE >_{i,t} = \beta BUST_{i,t-1} + \mu_i + \tau_{j,t} + \varepsilon_{i,t},$$

where $BUST$ is a firm-year indicator for having been in a recently dissolved cartel (i.e., $CARTEL_{t-2} = 1$ & $CARTEL_{t-1} = 0$), and δRPE is a firm-year indicator for whether the firm dropped RPE from the CEO's pay package that year (i.e., $RPE_{t-1} = 1$ & $RPE_t = 0$). Specifications differ with respect to fixed effect structure and the dependent variable. In Panel A (Panel B), the dependent variable is δRPE (*peer*) (δRPE (*generic*)). Across both panels, 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.

Panel A: Peer RPE

		Outcome = δRPE (peer)		
	Pred.	(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

		Outcome = δRPE (generic)		
	Pred.	(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

Table VI. Busted Cartels and RPE Drop-Rates, by Concentration

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 V Panel A, but the sample is cut in half, based on concentration at the SIC 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.

		Outcome = δRPE (peer)			
	Pred.	(1)	(2)	(3)	(3)
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

Table VII. Mechanism Test: Causal Direction

This table presents evidence on the lead-lag relation between cartel participation and the use of RPE. The specification mirrors that of Table II 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.

	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

Table VIII. Mechanism Test: Product Market Aggression

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 includes excludes cartel members, and presents side-by-side regressions for *RPE* (*peer*) (in odd-numbered specifications) versus *RPE* (*generic*) (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.

Panel A: Peer versus Generic RPE

		Outcome = $\log(\frac{\text{Sales}}{\text{Assets}})$		Outcome = $\log(\frac{\text{Costs}}{\text{Assets}})$		Outcome = $\log(\frac{\text{Spend}}{\text{Sales}})$		Outcome = $\log(\frac{\text{Ad. Spend}}{\text{Assets}})$	
	Pred.	(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 (generic)	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

		Outcome = $\log(\frac{Sales}{Assets})$		Outcome = $\log(\frac{Costs}{Assets})$		Outcome = $\log(\frac{Spend}{Sales})$		Outcome = $\log(\frac{Ad.\ Spend}{Assets})$	
	Pred.	(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

Table IX. Mechanism Test: Risk-Sharing Benefits

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.

	Outcome = OWN RETURN (monthly)		
	(1)	(2)	(3)
CARTEL x PEER RETURN (monthly)	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 (monthly)	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

Table X. Sensitivity Analysis: RPE and Cartels, Added Controls

This table exactly replicates Specification 3 of Panel A from Tables II and V, but with additional control variables. Specification 1 includes no additional control variables; Specification 2 includes additional controls for firm characteristics ($\#Direct$, ROA, $log(Sales)$ and $log(Avg.\ Assets)$); Specification 3 includes additional controls for CEO characteristics ($log(Age)$; $log(1 + Tenure)$; $FOUNDER$ and $CHAIR$); and Specification 4 includes additional controls for firm and CEO characteristics, jointly. Below each coefficient, we report t-statistics, based on standard errors clustered by industry (Fama and French 48 Industry Classification) and year.

Panel A: RPE and Cartel Membership

		Outcome = RPE (peer)			
	Pred.	(1)	(2)	(3)	(4)
CARTEL	+	0.068**	0.078***	0.073**	0.069**
		(2.751)	(2.932)	(2.744)	(2.256)
Firm Controls		No	Yes	No	Yes
CEO Controls		No	No	Yes	Yes
Firm Fixed Effects		Yes	Yes	Yes	Yes
SIC-Year Fixed Effects		Yes	Yes	Yes	Yes
Observations		22,276	16,029	15,945	12,511
R-squared		0.556	0.595	0.589	0.609

Panel B: RPE Drop-Rates around Cartel Busts

		Outcome = δRPE (peer)			
	Pred.	(1)	(2)	(3)	(4)
BUST	+	0.035*	0.048**	0.039**	0.053**
		(1.945)	(2.619)	(2.026)	(2.617)
Firm Controls		No	Yes	No	Yes
CEO Controls		No	No	Yes	Yes
Firm Fixed Effects		Yes	Yes	Yes	Yes
SIC-Year Fixed Effects		Yes	Yes	Yes	Yes
Observations		22,276	16,029	15,945	12,511
R-squared		0.263	0.290	0.290	0.304

Table XI. Sensitivity Analysis: Measurement of RPE

This table exactly replicates Specification 3 of Panel A from Table II, but uses different samples and a different measure of RPE. Specification 1 makes no adjustments, exactly replicating Specification 3 of Panel A from Table II. Specification 2 restricts the sample to include only post-CD&A firm-year observations; Specification 3 includes the full sample, but uses a modified measure of RPE, which includes hybrid "AbsRel" as relative performance grants; and Specification 4 uses the modified RPE measure and restricts the sample to include only post-CD&A firm-year observations. Below each coefficient, we report t-statistics, based on standard errors clustered by industry (Fama and French 48 Industry Classification) and year.

		RPE (peer)		RPE (peer), incl. "AbsRel"	
VARIABLES	Pred.	(1)	(2)	(3)	(4)
CARTEL	+	0.068∗∗	0.140∗∗∗	0.063∗	0.119∗∗
		(2.751)	(4.317)	(2.084)	(3.182)
Sample		Full	Post-CD&A	Full	Post-CD&A
Firm Fixed Effects		Yes	Yes	Yes	Yes
SIC-Year Fixed Effects		Yes	Yes	Yes	Yes
Observations		22,276	11,936	22,276	11,936
R-squared		0.556	0.677	0.625	0.724

Table XII. Sensitivity Analysis: Busted Cartels and RPE Drop-Rates, No CEO Turnover

This table exactly replicates the analysis in Table V Panel A, and Table VI, on a sample of CEOs that do not turnover from year t to $t+1$. Panel A replicates Table V Panel A; and Panel B replicates Table VI. Below each coefficient, we report t-statistics, based on standard errors clustered by industry (Fama and French 48 Industry Classification) and year.

Panel A: Full Sample

		Outcome = δRPE (peer)		
	Pred.	(1)	(2)	(3)
BUST	+	0.063**	0.048*	0.052**
		(2.659)	(1.938)	(2.564)
Year Fixed Effects		Yes	Yes	No
Firm Fixed Effects		No	Yes	Yes
SIC-Year Fixed Effects		No	No	Yes
Observations		17,796	17,796	17,796
R-squared		0.003	0.120	0.291

Panel B: Split by Concentration

		Outcome = δRPE (peer)			
	Pred.	(1)	(2)	(3)	(3)
BUST	+/?	0.095*	0.025	0.099**	0.021
		(1.920)	(1.086)	(2.077)	(0.931)
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		8,962	8,834	8,875	8,921
R-squared		0.403	0.195	0.436	0.204