Relative Performance Evaluation, Sabotage and Collusion: Quasi-experimental Evidence on the "RPE Puzzle"

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Abstract

Relative performance evaluation ("RPE") is a useful tool for shielding risk averse agents from systematic uncertainty. However, RPE can also destroy shareholder value by encouraging executives to implement excessively aggressive product market strategies to improve their relative standing through costly sabotage. We posit that explicit collusion via cartel membership restricts a firm's ability to be engage in sabotage, thereby improving the net benefits of RPE, and document that: (1) cartel members are more likely to use RPE, especially in more concentrated markets; (2) conditional on using RPE, cartel members include more economically similar firms in their peer group; (3) firms are differentially likely to drop RPE from their executives' pay package within one year of their cartel being dissolved by a plausibly exogenous intervention; and (4) RPE is associated with higher advertising spending, but only among non-cartel firms. Collectively, our findings provide evidence that the potential for costly sabotage is a significant determining factor in the 'puzzling paucity' of RPE in executive pay.

Keywords: Compensation; Incentives; Cartel; Collusion; Relative Performance Evaluation

1 Introduction

Agency theory's "informativeness principle" states that an optimal contract uses every contractible measure that provides incremental information about an agent's actions (Holmström, 1979). In particular, when multiple agents are exposed to common shocks, *other agents' performances* are informationally valuable signals, which can be used to improve risk-sharing through a practice known as "relative performance evaluation," or simply "RPE." By benchmarking performance against other agents in similar economic circumstances, the effects of systemic shocks can be stripped away, making it easier to monitor/ascertain each agent's effort (e.g., Lazear and Rosen, 1981; Holmström, 1982; Nalebuff and Stiglitz, 1983; Gibbons and Murphy, 1990; Prendergast, 1999).

However, in practice, RPE is not a particularly common feature in executive pay packages—a fact which has come to be known as the "RPE Puzzle," with Murphy (1999) remarking that "the paucity of RPE in options and other components of executive compensation remains a puzzle worth understanding."[1] While recent evidence demonstrates that RPE is used more than previously thought, its use remains the exception rather than the rule. RPE use has increased over time, but even the most recent data suggest that a strong majority of large public firms use no RPE in their CEO's pay packages, at all (e.g., Albuquerque, 2009; Gong, Li, and Shin, 2011; Bettis, Bizjak, Coles, and Young, 2014; De Angelis and Grinstein, 2019).

One explanation for this 'puzzling paucity' is that RPE can encourage agents to engage in costly sabotage, intentionally harming the reference groups' performance in order to inflate their own relative performance—even at significant cost to their own absolute performance

[1] See, also: Abowd and Kaplan (1999); Aggarwal and Samwick (1999b); Maug (2000); Core, Guay, and Larcker (2003); Garvey and Milbourn (2003); Fershtman, Hvide, and Weiss (2003); Rajgopal, Shevlin, and Zamora (2006); Celentani and Loveira (2006); Kabitz (2017).

(e.g., Lazear and Rosen, 1981; Lazear, 1989; Gibbons and Murphy, 1990; Chowdhury and Gürtler, 2015). 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 them attractive strategies to managers with RPE-based incentives. Accordingly, rational principals likely choose to withhold such incentives, even at the expense of deprecated risk-sharing.

While this explanation is intuitive, there is little compelling evidence demonstrating that the potential for costly sabotage is a driving factor behind the apparent dearth of RPE in executive pay plans. We address this gap by looking to explicit collusive arrangements (i.e., cartels) to identify the role sabotage costs play in the 'RPE puzzle.' In cartels, otherwise rivalrous firms collectively agree on product market strategies. By agreeing to product market strategies in this fashion, cartel membership substantially diminishes the potential for RPE to induce costly sabotage, thereby enhancing the net benefits of its use.

We begin our empirical investigation by examining firms' reliance on RPE, based on cartel membership and product market concentration. We 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 product markets 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 product markets of below-median concentration, we find no evidence to suggest that cartel membership and firms' use of RPE are associated.

We next examine whether RPE users construct their peer groups to mitigate the potential for sabotage. In constructing a benchmark, firms face a trade-off between optimizing risk-sharing by selecting the most economically-related peers (with whom shocks would be most correlated), and minimizing sabotage costs by selecting more economically distant firms (whose performance is less manipulable through sabotage). We find that cartel members select peer firms that are more likely to be from their own SIC, and offer more similar products. These result 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.

To better identify whether RPE decisions 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 DOJ detecting a cartel and 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 use RPE in the executives 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 product markets. These results are particularly noteworthy because contract terms tend to be very 'sticky;' once a CEO is given RPE-based incentives, they are rarely taken away. We find that 2.4% 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 (9.0% versus 1.8%). Moreover, the result manifests predominantly in more concentrated product markets; in markets 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.

Lastly, we examine the relations among RPE, product market aggression and cartel membership. We find evidence to suggest that firms behave more aggressively (as captured by advertising spending) when the CEOs are given RPE-based incentives, but only among non-cartel members. Among cartel members, this pattern is conspicuously absent. This evidence suggests that RPE does indeed induce more aggressive behavior (i.e., costly sabotage), and that cartel membership is effective at curtailing these destructive actions.

To facilitate sharp inferences, we use firm and [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 time trends. Furthermore, we exploit generic RPE (e.g., benchmarking against the S&P 500) in placebo tests to rule out other confounds. Sabotage strategies will not be effective when compared against such a broad reference sample. 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. Collectively, our evidence provides support for the 'costly sabotage' explanation of the RPE puzzle.

Our work contributes to multiple literatures. First and foremost, we contribute to the literature on relative performance evaluation by providing novel empirical evidence suggesting that the potential for costly sabotage is a significant driving force behind firms' non-reliance on peer-based RPE. In so doing, we shed new light on the old, but still largely unsettled question of why most executive pay plans don't utilize RPE. 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, 1999b; Gong et al., 2011; Vrettos, 2013; Bettis et al., 2014).[2] We complement these prior findings by exploiting cartel membership and showing that firms are significantly more likely to use RPE if they have committed, through explicit collusion, not to sabotage each other. Consistent with prior studies, this relation manifests most clearly in the concentrated industries, where the risk of costly sabotage is greatest.

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 using 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 risk-share 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 posits 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 (1999b); 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 not allowing firms in concentrated industries to adopt effective governance tools like RPE.

Finally, 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 manuscript is organized as follows. In Section 2, we develop and state our predictions; in Section 3 we detail our data sources, sample construction and variable definitions; in Section 4, we present and discuss our findings; and in Section 5, we conclude. Lastly, in the Appendix, we sketch the stylized analytical framework from which all of our testable predictions derive.

2 Hypothesis Development

Under traditional agency theory (e.g., Holmström, 1979), the optimal performance measurement system is that which best informs about the agent's actions. In situations with multiple agents, all subject to common performance shocks, performance relative to *other* agents is a useful source of information to include for this purpose, as it purges the uncertainty arising from these common shocks (e.g. Holmström, 1982).

However, the utility of RPE is predicated on the notion that other agents' performances are informative, but not manipulable. In many concentrated product markets, this is not the case; a single large firm can easily affect its rivals' profitability through its 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, an agent given substantial compensation tied to RPE may be incentivized to take profit-destroying actions, so long as their actions reduce peers' profits more-so than their own (e.g., Gibbons and Murphy, 1990). Cartel membership constrains the firm's competitive stance, 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 management incentives, the choice to use RPE is not merely a binary choice. In addition to continuous variation in the weight on 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 systemic shocks, but 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 related industries), the benchmark is less able to filter out the systemic 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 executive's 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]

2.1 Discussion

Our predictions are based on the notion that cartel membership constrains the value-destroying product market aggression which RPE can incentivize, thereby making it a more attractive tool for efficiently monitoring executive performance. However, existing literature offers several possible explanations for the apparent lack of RPE in executive pay packages, of which the potential for costly sabotage is only one. For example, prior studies argue that there is little need for RPE as managers can hedge against systemic risk through their personal investments (e.g., Maug, 2000; Garvey and Milbourn, 2003). Relatedly, Core et al. (2003) posits that RPE need not be explicit; implicit relative performance evaluation could be sufficient for the risk-sharing/incentive benefits put forth in Holmström (1982).

Taking a different tack, several divergent streams of literature suggest that the RPE puzzle might arise because executive pay packages are not, in fact, designed to minimize agency costs, as typically assumed. For example, if contracts are formulated primarily by powerful, rent-seeking managers (as opposed to powerful, value-maximizing principals), then the risk-

[4]'LEN' stands for "linear, exponential, normal," denoting that contracts are linear, agent's have CARA preferences (i.e., negative exponential utility), and all uncertainty comes from additively separable, normally distributed perturbations.

sharing benefits of RPE would not be relevant in determining its use (e.g., Bebchuk, Fried, and Walker, 2002; Bebchuk and Fried, 2003; Bertrand and Mullainathan, 2001). Alternatively, contracts could be designed for strategic purposes, such as facilitating commitment, in which case RPE could be counter productive (e.g., Aggarwal and Samwick, 1999a; Vrettos, 2013).

On a more technical note, some recent studies venture that the 'puzzle' may be a mere matter of researchers' misapprehensions of the data. Much of the early empirical work on the use of RPE was conducted without access to high quality data on explicit managerial incentives, instead relying on regression-based *inferences* about managerial incentives. Thus, the results were highly sensitive to researchers' assumptions about performance metrics, payout methods and peer groups (e.g., Albuquerque, 2009). While recent empirical evidence suggests this may partially explain the 'puzzle,' it does not appear to be the entire explanation. For example, using explicit compensation details disclosed in firms' proxy statements, recent studies demonstrate that RPE is utilized more than previously thought, though its use remains the exception, rather than the rule (e.g., Gong et al., 2011; Bettis et al., 2014; Kabitz, 2017; De Angelis and Grinstein, 2019).

We do not take the position that our work rules out these alternative explanations—nor is this our goal. Rather, our view is that a multitude of factors jointly explain firms' reluctance to compensate managers on the basis of relative performance. The purpose of this study is to identify the extent to which the potential for costly sabotage contributes to the RPE puzzle. Thus, we make no attempt to 'horserace' one explanation against the others, nor pit them against each other in any other fashion.

3 Data, Sample and Variables

3.1 Data

3.1.1 Cartel Data

The cartel data employed in the empirical analysis comes from two sources. Data on US cartels comes from an excerpt from John Connor's Private International Cartels dataset.[5] This excerpt covers the years of 1984 to 2011 and is limited to publicly reported information on 180 cartels fined 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 European Commission ("EC") and accessible via the Commissions website. Cartels are restricted to those with at least one successful LP application (81 cartels involving 613 firms), as there is no publicly available information on the value of the individual fines in the other 17 cartels fined during this period and with final decisions in the period of 1998 to the 15th of December 2014.[6]

Some of the EC reports were triggered by a previous investigation and/or fine in another jurisdiction. At least 25% of the cartels reported to the EC by a cartel member were first convicted in the US, and at least another 20% were convicted by US and EU authorities

[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 Connors criminal dataset, obtained in 2016 and several other variables were dropped due to inconsistent or missing data.

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

in the same year.[7] An additional 6% were fined by the EC before a US conviction. All of these involved leniency programme applications. The remaining cases were discovered due to other reasons, such as reporting by a third party (e.g. a customer or rival firm) or under the Commission's own initiative.

3.1.2 Firm Fundamental Data and Compensation Data

The financial and compensation data used in this study come from four sources: Compustat's Annual and Quarterly Industrial Files; Incentive Lab; ExecuComp; the Hoberg and Phillips Data Library.

Incentive Labs 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. This database is thoroughly described in Hoberg and Phillips (2010), Hoberg, Phillips, and Prabhala (2014) and Hoberg and Phillips (2016).

3.2 Sample Selection

We construct our sample using all firm-years in the intersection of Compustat and Incentive lab, over the period of 1998 and 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,

[7] A further 4% of the cases were also convicted in the EU and US but there is no information on the year of conviction in the US.

using firms' ticker symbols.[8] Our final sample consists of 20,000 firm-year observations, from 1,868 unique firms of which 105 firms were cartel members at some point over our sample period, for a total of 701 firm-year cartel observations.

One concern with the data is the possibility of sample selection bias. Since cartels are prohibited by the Sherman Act and the EU Treaty, 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 in our setting, but its existence is acknowledged in the interpretation of the results.

3.3 Constructs and Proxies

Below we outline the variables used in our regression model. Summary statistics can be found in Table 1.

3.3.1 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 an indicator variable to reflect firms' transitions from being cartel members to non-cartel members (i.e., when their cartels were fined). We use the indicator variable $BUST$ which takes a value of one if $CARTEL_{i,t-1} = 1$ and $CARTEL_{i,t} = 0$.

[8]Where possible, we use the US ticker symbols, developed by Standard & Poors (S&P) to identify each firm. We use the latest available symbol for each firm, to reflect mergers and acquisitions. Eg. Exxons US ticker symbol was XON but after the 1999 merger with Mobil Oil, this changed to XOM.

3.3.2 Executive Incentives

We measure executive incentives with indicator variables equal to one if the CEO has any compensation grants tied to purely "Relative" objectives, as coded by Incentive Lab (i.e., $performancetype$ = "*Rel*"). Within relative incentives, we construct two measure 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 ($gen.$), respectively.[9]

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

3.3.3 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 4-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.

[9] Some firms include hybrid grants which include both absolute relative components (coded as $performancetype$ = "*AbsRel*" by Incentive Lab). However, we do not have enough information to determine whether the relative component uses a peer group benchmark or a generic benchmark, so we do not factor this type of compensation into our analyses. For this reason, our reported levels of RPE use will be understated.

Peer group information is available only for the post-CD&A period,[10] so we construct these measures only for post-CD&A firm-year observations.

3.3.4 Product Market Aggression

We measure firms' aggression based on advertising intensity: $log(\frac{Ad.\ Spend_{i,t}}{Avg.\ Assets_{i,t}})$.

3.3.5 Controls

In robustness analyses, we control for several firm and executive characteristics, which could impact the provision of relative incentives (and/or the likelihood of cartel membership):

Firm Characteristics

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).

Executive Characteristics

We 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, $CHAIR$.

[10] The Compensation Discussion and Analysis was introduced by the Securities and Exchange Commission in 2006.

4 Empirical Analysis

4.1 Descriptive Evidence

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 in 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 2. In Panel A (Panel B), the dependent variable is RPE ($peer$) (RPE ($gen.$)). Across both panels, the fixed effects are consistent: in the first specification, we include only year fixed effects; the second specification adds firm fixed effects; and 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 3. 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 plausibly able to materially sabotage each other through their aggressive product market strategies.

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; $SCORE$, and 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 constructible without peer group information from the CD&A.

Our results, presented in Table 4, show that cartel members select more economically similar peers. They are more likely to select peers from their own SIC (specification 1); and firms that offer more similar products (specifications 2 and 3). In terms of economic magnitudes, the average cartel members selects peers which are 10% more likely to be from their own 4-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 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 over-arching 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 recover 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]

4.2 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 eco-

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

nomically similar firms—especially in more concentrated product markets. This evidence is consistent with Gibbons and Murphy's (1999) 'costly sabotage' explanation for the scarcity of explicit RPE in executive pay packages. However, cartel membership and RPE use are both endogenous firm choices, and a host of potential confounds could explain the association between the two.

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 Department of Justice, 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.

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 CEOs pay package in the first year after its cartel was dissolved (9% versus 1.8%). 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 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 a 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 2 and 3 exactly, 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 5). Moreover, the relation between cartel termination and RPE drop-rates appears to be driven by more concentrated product markets, with economically large and statistically significant effects among industries of above-median concentration, and near-zero effects among industries of below-median concentration (Table 6).

While these results are consistent with our predictions, we caveat that cartel membership affects many aspects of a firms' operations beyond merely the potential for costly sabotage, and thus might affect firms' 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.

4.3 Sensitivity Analyses

In this subsection, we examine whether our findings are robust to alternative research design choices. First we examine whether the event study results can be explained by CEO turnover, and second whether the associations between cartel membership and the use of RPE are robust to the inclusion of additional controls.

4.3.1 No CEO Turnover

Contract terms are quite 'sticky,' in the sense that once put in, they are rarely taken out. One 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 and find that our results are robust to this alteration, as shown in Table 7.

4.3.2 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 8. In Panel A, we re-examine the relation between cartel membership and RPE, by replicating specification 3 of Table 2 Panel A. In Panel B, we re-examine the relation between cartel terminations and RPE drop-rates, by replicating specification 3 of Table 5 Panel A.

In the first specification, we present the analyses without controls (perfectly 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.

4.4 Mechanism Tests: RPE and Product Market Aggression

The preceding analyses examine whether RPE is more common among cartel members. While our 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 and RPE relate.

In our final 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 of the strategic game, it is difficult to determine whether prices or quantities are strategic choices, or equilibrium outcomes.[12]

For these reasons, we look to advertising expenditures—for which data are readily available—as an example of explicit strategic choices made by the firm. Advertising fits well with our suppositions, as (1) advertising is costly to the own firm; (2) advertising can be costly to rival firms, by drawing customers away; and (3) cartel membership limits the extent to which advertising would be a viable sabotage strategy—in some cases through explicit restrictions on cartel members' advertising expenditures.[13]

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

$$log(\frac{Ad.\ Spend_{i,t}}{Avg.\ Assets_{i,t}}) = \beta_1 RPE_{i,t} + \beta_2 CARTEL_{i,t} + \beta_3 RPE_{i,t} \times CARTEL_{i,t} + \mu_i + \tau_{j,t} + \varepsilon_{i,t}. \quad (4)$$

We present four specifications. The first two use the peer-based measure of RPE, $RPE\ (peer)$, while the latter two use the generic-based measure of RPE, $RPE\ (gen.)$. Odd number specifications include only the main effect of RPE, while even-number specifications further include the main effect of $CARTEL$ as well as its interaction with RPE. Firm and SIC-year fixed effects are included in each specification. Results are presented in Table 9.

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

[13]We caveat that not all forms of advertising would be expected to increase in the presence of RPE. In particular, some advertisements provide spillover benefits to other firms (e.g., by increasing demand for an entire category of products). RPE could actually induce *under-investment* in these types of advertisements, as the spillover benefit directly harms the firm's relative performance.

We find that peer-based RPE is associated with substantially greater advertising intensity, but only among non-Cartel members. The interaction between RPE and $CARTEL$ is significantly negative, such that it [more than] fully offsets the the main effect of RPE. In contrast, for generic RPE none of the coefficients can be statistically discerned from zero. Jointly, these descriptive findings lend credence to our assumption that [peer-based] RPE induces more aggressive product market strategies, and that cartel membership is effective at curtailing this effect.

5 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. By exploiting cartel membership, we provide evidence that part of the reason firms avoid using RPE is that such incentives drive managers to implement excessively aggressive, value-destroying strategies.

While our findings help to address the "RPE puzzle," 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? We therefore view our results as providing evidence on one aspect of the issue.

Finally, we reiterate that cartel membership could affect the use of RPE through mechanisms other than our hypothesized 'costly sabotage' channel. Thus, even if our work accurately reflects the causal effect of cartel membership on the use of RPE, this does not imply that we have necessarily demonstrated the effect of costly sabotage potential on RPE. While our mechanisms tests provide reassurance that RPE is associated with heightened aggression—an effect which vanishes for cartel members—we view this study as providing an important first step towards understanding this issue. Future quasi-experimental work could utilize different settings to better triangulate the extent to 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:

$$\Pi^i(e, x) = \pi^i + e + x - \tfrac{x^2}{2} + \epsilon^i + \eta, \tag{5}$$

$$\Pi^j(x) = \pi^j - \gamma x + \epsilon^j + \eta, \tag{6}$$

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.[14] 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

[14] 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.

which the agent is given a linear incentive contract:

$$\begin{aligned} w &= S + \beta\left((1-\alpha)\Pi^i + \alpha(\Pi^i - \Pi^j)\right) \\ &= S + \beta\left(\Pi^i - \alpha\Pi^j\right), \end{aligned} \tag{7}$$

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

$$\begin{aligned} ACE &= E[w] - \tfrac{e^2}{2} - \tfrac{r^2}{2}Var(w) \\ &= S + \beta\Bigg(\pi^i + e + x - \tfrac{x^2}{2} - \alpha(\pi^j - \gamma x)\Bigg) - \tfrac{e^2}{2} - \tfrac{r^2}{2}\beta^2\left(\sigma^2_{\epsilon^i} + \alpha^2\sigma^2_{\epsilon^j} + (1-\alpha)^2\sigma^2_{\eta}\right), \end{aligned} \tag{8}$$

thus making the agent's problem:

$$\arg\max_{e} \left\{ S + \beta\Bigg(\pi^i + e + x - \tfrac{x^2}{2} - \alpha(\pi^j - \gamma x)\Bigg) - \tfrac{e^2}{2} - \tfrac{r^2}{2}\beta^2\left(\sigma_{\epsilon^i}^2 + \alpha^2\sigma_{\epsilon^j}^2 + (1-\alpha)^2\sigma_{\eta}^2\right) \right\}. \tag{9}$$

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

$$e^* = \beta. \tag{10}$$

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:

$$\begin{aligned} TCE \equiv & E[\Pi^i - w] + ACE \\ = & E[\Pi^i] - \cancel{E[w]} + \cancel{E[w]} - \tfrac{e^{*2}}{2} - \tfrac{r^2}{2}\beta^2\left(\sigma_{\epsilon^i}^2 + \alpha^2\sigma_{\epsilon^j}^2 + (1-\alpha)^2\sigma_{\eta}^2\right) \\ = & \pi^i + e^* + x - \tfrac{x^2}{2} - \tfrac{e^{*2}}{2} - \tfrac{r^2}{2}\beta^2\left(\sigma_{\epsilon^i}^2 + \alpha^2\sigma_{\epsilon^j}^2 + (1-\alpha)^2\sigma_{\eta}^2\right), \end{aligned} \tag{11}$$

thus making the principal's problem:

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

$$\text{subject to} \quad e^* = \beta. \tag{13}$$

First order conditions on α and β reveal that:

$$\begin{aligned} \alpha^* &= \frac{\sigma^2_\eta}{\sigma^2_\eta + \sigma^2_{\epsilon^j}}, \\ \beta^* &= \frac{1}{1 + r^2 \left(\sigma^2_{\epsilon^i} + \alpha^2 \sigma^2_{\epsilon^j} + (\alpha - 1)^2 \sigma^2_\eta \right)}. \end{aligned} \tag{14}$$

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.

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

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

$$e^* = \beta, \tag{16}$$

$$x^* = 1 + \gamma\alpha. \tag{17}$$

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:

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

$$\text{subject to} \quad e^* = \beta, \tag{19}$$

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

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

$$\beta^* = \frac{1}{1 + r^2\left(\sigma^2_{\epsilon^i} + \alpha^2\sigma^2_{\epsilon^j} + (1-\alpha)^2\sigma^2_{\eta}\right)}. \tag{21}$$

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:

$$TCE(\alpha;\cdot) = \pi^i + \tfrac{1}{2} + \frac{1}{2}\left(\underbrace{\frac{1}{1+r^2\left(\sigma^2_{\epsilon^i}+\alpha^2\sigma^2_{\epsilon^j}+(1-\alpha)^2\sigma^2_\eta\right)}}_{\beta^*(\alpha)} - \alpha^2\gamma^2\right). \tag{22}$$

$TCE(\alpha;\cdot)$ 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:

$$\frac{r^2\left(\alpha^*\sigma^2_{\epsilon^j}-(1-\alpha^*)\sigma^2_\eta\right)}{\left(1+r^2\left(\sigma^2_{\epsilon^i}+\alpha^{*2}\sigma^2_{\epsilon^j}+(1-\alpha^*)^2\sigma^2_\eta\right)\right)^2} = -\alpha^*\gamma^2. \tag{23}$$

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 it's 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 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.

$$\Pi^i(e, x) = \pi^i + e + x - \tfrac{x^2}{2} + \epsilon^i + \eta, \tag{24}$$

$$\Pi^j(x) = \pi^j - \rho\gamma x + \epsilon^j + \rho\eta. \tag{25}$$

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:

$$e^* = \beta. \tag{26}$$

Thus, the principal's problem is:

$$\arg\max_{\alpha,\beta,\rho} \left\{ \pi^i + e^* + x - \tfrac{x^2}{2} - \tfrac{e^{*2}}{2} - \tfrac{r^2}{2}\beta^2 \left(\sigma_{\epsilon^i}^2 + \alpha^2 \sigma_{\epsilon^j}^2 + (1-\rho\alpha)^2 \sigma_\eta^2 \right) \right\} \tag{27}$$

$$\text{subject to} \quad e^* = \beta. \tag{28}$$

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

$$\alpha^* = \frac{\rho\sigma_\eta^2}{\rho^2\sigma_\eta^2 + \sigma_{\epsilon^j}^2}, \tag{29}$$

$$\beta^* = \frac{1}{1 + r^2 \left(\sigma_{\epsilon^i}^2 + \alpha^2 \sigma_{\epsilon^j}^2 + (1-\rho\alpha)^2 \sigma_\eta^2 \right)}. \tag{30}$$

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

$$\frac{dTCE(\rho)}{d\rho} = \frac{r^2 \rho \sigma_\eta^4 \sigma_{\epsilon^j}^2}{\left(\rho^2 \sigma_\eta^2 \left(r^2 \sigma_{\epsilon^i}^2 + 1 \right) + \sigma_{\epsilon^j}^2 \left(r^2 \left(\sigma_\eta^2 + \sigma_{\epsilon^i}^2 \right) + 1 \right) \right)^2}, \tag{31}$$

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:

$$e^* = \beta, \tag{32}$$

$$x^* = 1 + \rho\gamma\alpha. \tag{33}$$

Thus, the principal's problem is:

$$\arg\max_{\alpha,\beta,\rho} \left\{ \pi^i + e^* + x^* - \tfrac{x^{*2}}{2} - \tfrac{e^{*2}}{2} - \tfrac{r^2}{2}\beta^2 \left(\sigma^2_{\epsilon^i} + \alpha^2\sigma^2_{\epsilon^j} + (1-\rho\alpha)^2\sigma^2_\eta\right) \right\} \tag{34}$$

$$\text{subject to} \quad e^* = \beta \tag{35}$$

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

The first order condition for β is:

$$\beta^* = \frac{1}{1 + r^2\left(\sigma^2_{\epsilon^i} + \alpha^2\sigma^2_{\epsilon^j} + (1-\rho\alpha)^2\sigma^2_\eta\right)}, \tag{37}$$

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

$$TCE(\alpha,\rho) = \pi^i + \tfrac{1}{2} + \frac{1}{2}\left(\underbrace{\frac{1}{1 + r^2\left(\sigma^2_{\epsilon^i} + \alpha^2\sigma^2_{\epsilon^j} + (1-\rho\alpha)^2\sigma^2_\eta\right)}}_{\beta^*(\alpha,\rho)} - \alpha^2\rho^2\gamma^2 \right). \tag{38}$$

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:

$$\frac{r^2\sigma_\eta^2(1-\rho^*\alpha)}{\left(1+r^2\left(\alpha^2\sigma_{\epsilon^j}^2+\sigma_\eta^2(1-\rho^*\alpha)^2+\sigma_{\epsilon^i}^2\right)\right)^2}=\rho^{*2}\gamma^2\alpha, \tag{39}$$

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 $\rho^* = 1$.

Figure A1. Optimal Use of RPE

This figure plots the optimal weight on RPE, $\alpha^*(\gamma)$, as specified implicitly by eq. (23). 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 $r = \sigma^i_\epsilon = 1$.

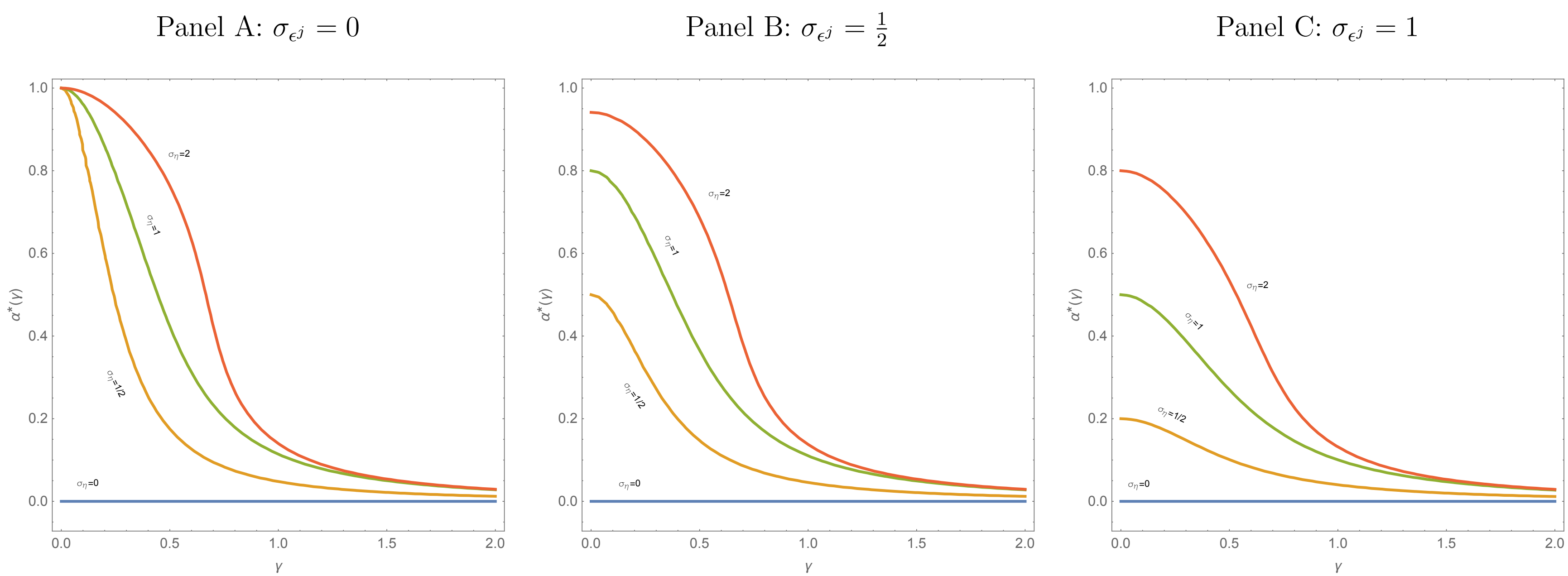

Figure A2. Optimal Peer Similarity

This figure plots the optimal peer similarity, $\rho^*(\alpha)$, as specified implicitly by eq. (39). 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 $\gamma = \frac{1}{2}$.

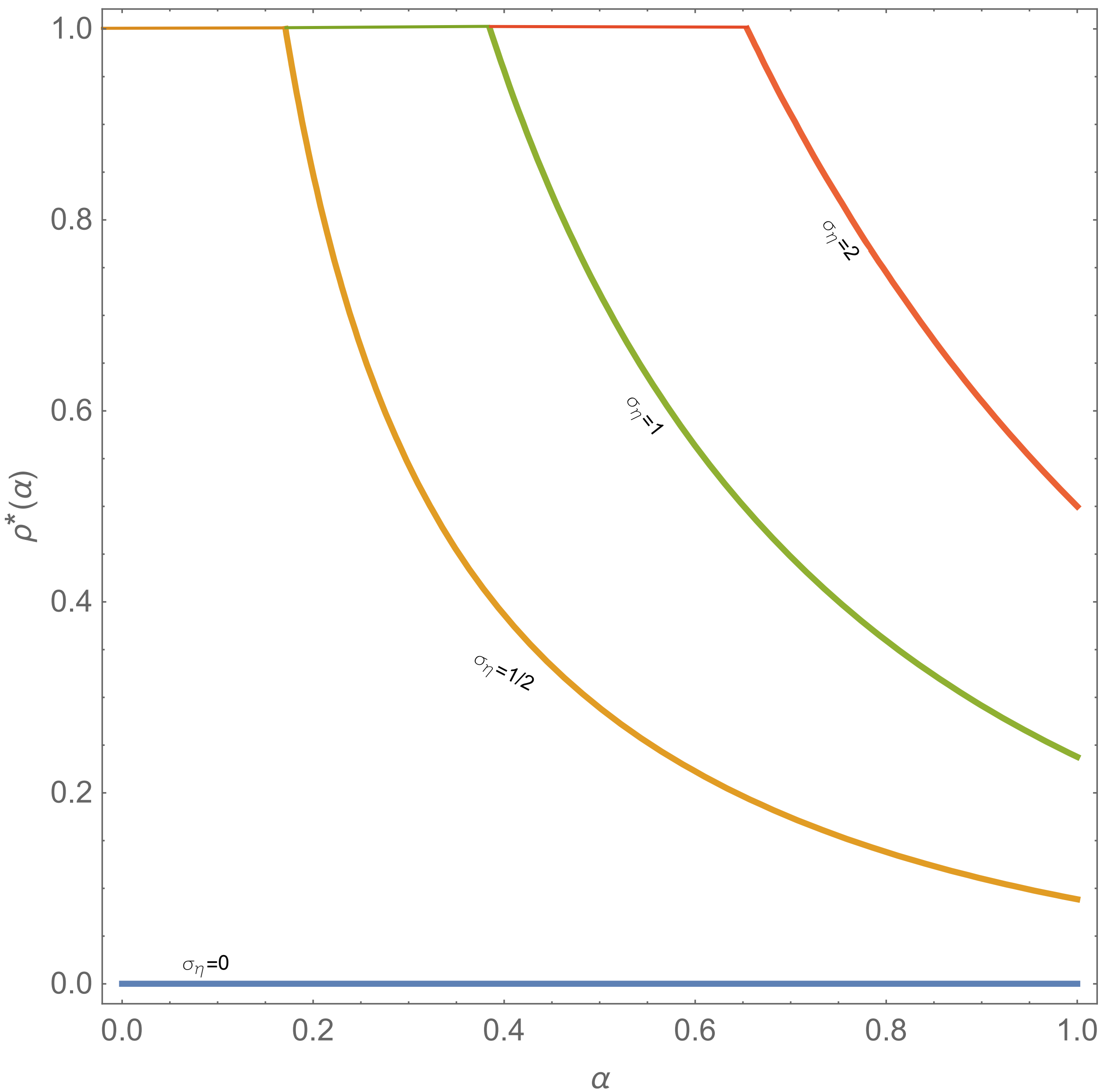

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Figure 1. Dropping RPE around Cartel Terminations

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

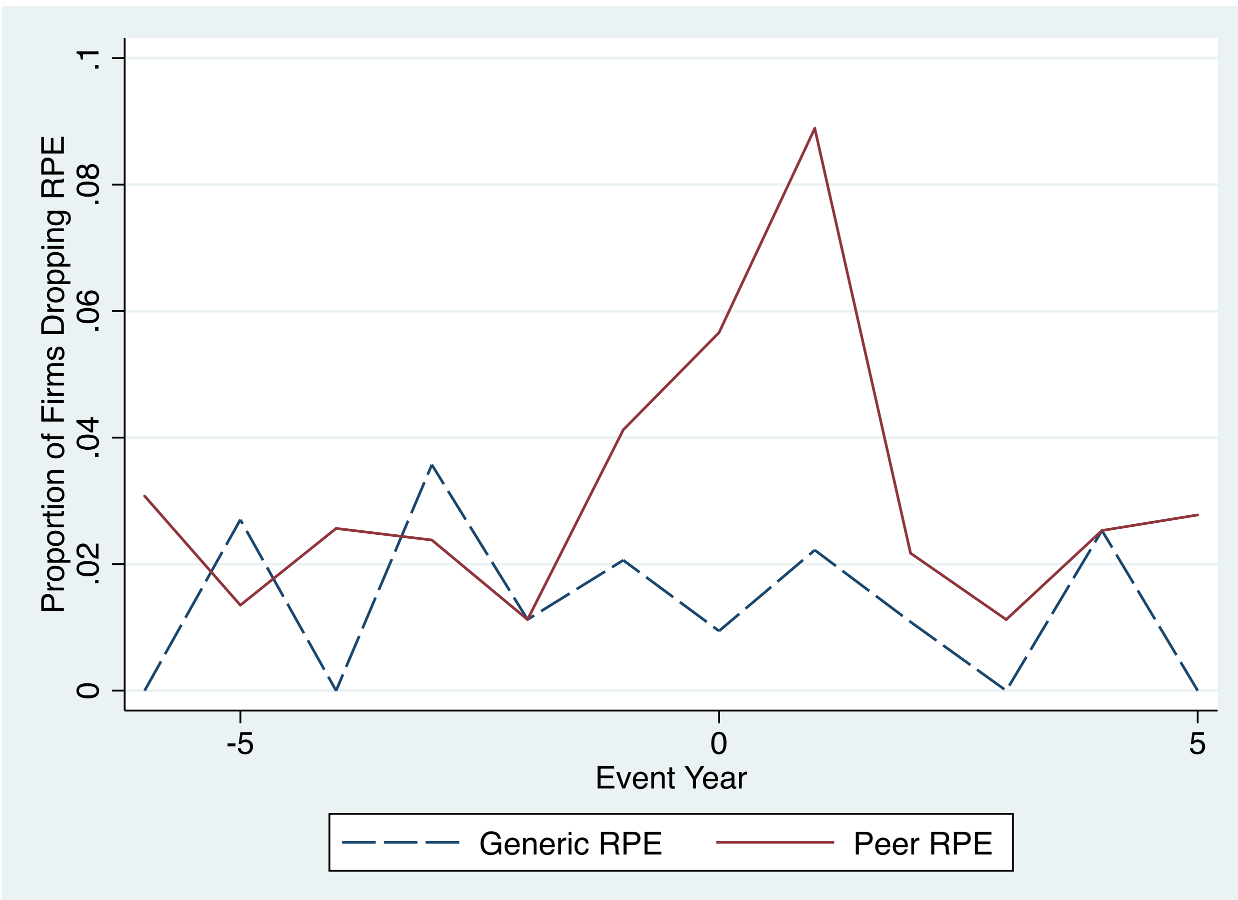

Table 1. Summary Statistics

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

Variable	Obs.	mean	sd	Q1	Med.	Q3
Cartel membership						
CARTEL	20,000	0.035	0.184	0.000	0.000	0.000
BUST	20,000	0.005	0.071	0.000	0.000	0.000
Incentives						
RPE (peer)	20,000	0.072	0.258	0.000	0.000	0.000
RPE (gen.)	20,000	0.042	0.200	0.000	0.000	0.000
δRPE (peer)	20,000	0.014	0.119	0.000	0.000	0.000
δRPE (gen.)	20,000	0.009	0.093	0.000	0.000	0.000
Peer selection						
$\frac{\#\text{RIVAL}}{\#\text{PEER}}$	919	0.376	0.360	0.067	0.235	0.750
SCORE	925	0.041	0.043	0.007	0.025	0.063
SCORE (hg)	925	0.063	0.052	0.019	0.051	0.094
Aggression						
$\log(\frac{\text{Ad. Spend}}{\text{Avg. Assets}})$	6,511	−4.519	1.587	−5.576	−4.357	−3.309
Firm characteristics						
#Direct	14,847	10.000	2.670	8.000	10.000	12.000
ROA	19,389	0.023	0.222	0.010	0.037	0.075
log(Sales)	19,957	7.734	1.671	6.764	7.781	8.801
log(Avg. Assets)	18,132	8.361	1.644	7.327	8.273	9.387
CEO characteristics						
log(Age)	15,158	3.997	0.118	3.927	4.007	4.078
log(1+Tenure)	14,868	1.664	0.843	1.099	1.792	2.303
FOUNDER	15,678	0.050	0.217	0.000	0.000	0.000
CHAIR	15,678	0.702	0.457	0.000	1.000	1.000

Table 2. 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 ($gen.$)). 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

VARIABLES	Pred.	(1) RPE (peer)	(2) RPE (peer)	(3) RPE (peer)
CARTEL	+	0.063***	0.048**	0.068***
		(4.104)	(2.396)	(2.993)
Fixed Effects				
Year		Yes	Yes	No
Firm		No	Yes	Yes
SIC-Year		No	No	Yes
Observations		20,000	20,000	20,000
R-squared		0.020	0.452	0.571

Panel B: Generic RPE

VARIABLES	Pred.	(1) RPE (gen.)	(2) RPE (gen.)	(3) RPE (gen.)
CARTEL	0	−0.004	0.013	0.020
		(−0.374)	(0.772)	(1.145)
Fixed Effects				
Year		Yes	Yes	No
Firm		No	Yes	Yes
SIC-Year		No	No	Yes
Observations		20,000	20,000	20,000
R-squared		0.008	0.374	0.509

Table 3. 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 2 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.

		(1)	(2)	(3)	(4)
VARIABLES	Pred.	RPE (peer)	RPE (peer)	RPE (peer)	RPE (peer)
CARTEL	+/0	0.100**	0.011	0.125**	0.023
		(2.241)	(0.185)	(2.120)	(0.390)
Fixed Effects					
Year		No	No	No	No
Firm		Yes	Yes	Yes	Yes
SIC-Year		Yes	Yes	Yes	Yes
Sample		Few Firms	Many Firms	High HHI	Low HHI
Observations		10,070	9,930	9,998	10,002
R-squared		0.645	0.538	0.639	0.560

Table 4. 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: proportion of same-SIC peers, in Specification 1; average Hoberg and Phillips TNIC score, in Specification 2; and average Hoberg and Phillips TNIC high granularity score, in Specification 3. 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).

VARIABLES	Pred.	(1) $\frac{\#RIVAL}{\#PEER}$	(2) SCORE	(3) SCORE (hg)
CARTEL	+	0.035***	0.063***	0.062***
		(4.352)	(3.734)	(3.883)
Fixed Effects				
Year		No	No	No
Firm		Yes	Yes	Yes
SIC-Year		Yes	Yes	Yes
Observations		919	925	925
R-squared		0.966	0.905	0.909

Table 5. 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 $(gen.)$). 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

VARIABLES	Pred.	(1) δRPE (peer)	(2) δRPE (peer)	(3) δRPE (peer)
BUST	+	0.065∗∗∗	0.049∗∗	0.040∗∗
		(3.472)	(2.533)	(2.789)
Fixed Effects				
Year		Yes	Yes	No
Firm		No	Yes	Yes
SIC-Year		No	No	Yes
Observations		20,000	20,000	20,000
R-squared		0.004	0.106	0.274

Panel B: Generic RPE

VARIABLES	Pred.	(1) δRPE (gen.)	(2) δRPE (gen.)	(3) δRPE (gen.)
BUST	0	0.010	0.004	0.002
		(0.786)	(0.339)	(0.198)
Fixed Effects				
Year		Yes	Yes	No
Firm		No	Yes	Yes
SIC-Year		No	No	Yes
Observations		20,000	20,000	20,000
R-squared		0.002	0.102	0.276

Table 6. 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 5 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.

VARIABLES	Pred.	(1) δRPE (peer)	(2) δRPE (peer)	(3) δRPE (peer)	(4) δRPE (peer)
BUST	+/0	0.100***	−0.002	0.101***	−0.003
		(3.376)	(−0.090)	(3.172)	(−0.133)
Fixed Effects					
Year		No	No	No	No
Firm		Yes	Yes	Yes	Yes
SIC-Year		Yes	Yes	Yes	Yes
Sample		Few Firms	Many Firms	High HHI	Low HHI
Observations		10,070	9,930	9,998	10,002
R-squared		0.380	0.181	0.415	0.194

Table 7. Busted Cartels and RPE Drop-Rates, No CEO Turnover

This table exactly replicates the analysis in Table 5 Panel A, and Table 6, on a sample of CEOs that do not turnover from year t to $t+1$. Panel A replicates Table 5 Panel A; and Panel B replicates Table 6. Below each coefficient, we report t-statistics, based on standard errors clustered by industry (Fama and French 48 Industry Classification) and year.

Panel A: Overall

VARIABLES	Pred.	(1) δRPE (peer)	(2) δRPE (peer)	(3) δRPE (peer)
BUST	+	0.082***	0.066**	0.071***
		(2.975)	(2.445)	(3.397)
Fixed Effects				
Year		Yes	Yes	No
Firm		No	Yes	Yes
SIC-Year		No	No	Yes
Observations		12,460	12,460	12,460
R-squared		0.004	0.147	0.340

Panel B: Split by Concentration

VARIABLES	Pred.	(1) δRPE (gen.)	(2) δRPE (gen.)	(3) δRPE (gen.)	(4) δRPE (gen.)
BUST	+/0	0.121**	0.040	0.119**	0.043
		(2.622)	(1.322)	(2.412)	(1.442)
Fixed Effects					
Year		No	No	No	No
Firm		Yes	Yes	Yes	Yes
SIC-Year		Yes	Yes	Yes	Yes
Sample		Few Firms	Many Firms	High HHI	Low HHI
Observations		6,228	6,232	6,168	6,292
R-squared		0.480	0.235	0.511	0.233

Table 8. RPE and Cartels, Added Controls

This table exactly replicates Specification 3 of Panel A from Tables 2 and Table 5, 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

		(1)	(2)	(3)	(4)
VARIABLES	Pred.	RPE (peer)	RPE (peer)	RPE (peer)	RPE (peer)
CARTEL	+	0.068***	0.077***	0.069***	0.065**
		(2.993)	(3.433)	(3.027)	(2.561)
Fixed Effects					
Firm		Yes	Yes	Yes	Yes
SIC-Year		Yes	Yes	Yes	Yes
Firm Controls		No	Yes	No	Yes
CEO Controls		No	No	Yes	Yes
Observations		20,000	14,265	14,445	11,239
R-squared		0.571	0.607	0.604	0.620

Panel B: RPE Drop-Rates around Cartel Busts

		(1)	(2)	(3)	(4)
VARIABLES	Pred.	δRPE (peer)	δRPE (peer)	δRPE (peer)	δRPE (peer)
BUST	+	0.040**	0.056***	0.042**	0.059**
		(2.572)	(3.074)	(2.452)	(2.918)
Fixed Effects					
Firm		Yes	Yes	Yes	Yes
SIC-Year		Yes	Yes	Yes	Yes
Firm Controls		No	Yes	No	Yes
CEO Controls		No	No	Yes	Yes
Observations		20,000	14,265	14,445	11,239
R-squared		0.571	0.607	0.604	0.620

Table 9. Product Market Aggression

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

$$log(\frac{Ad.\ Spend_{i,t}}{Avg.\ Assets_{i,t}}) = \beta_1 RPE_{i,t} + \beta_2 CARTEL_{i,t} + \beta_3 RPE_{i,t} \times CARTEL_{i,t} + \mu_i + \tau_{j,t} + \varepsilon_{i,t}.$$

Specifications 1 and 3 include only the main effect of RPE. Specifications 2 and 4 further include $CARTEL$ as well as its interaction with RPE. Specifications 1 and 2 measure RPE as using RPE (*peer*), while Specifications 3 and 4 measure RPE as using RPE (*gen.*). 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.

VARIABLES	Pred.	(1) $\log(\frac{\text{Ad. Spend}}{\text{Avg. Assets}})$	(2) $\log(\frac{\text{Ad. Spend}}{\text{Avg. Assets}})$	(3) $\log(\frac{\text{Ad. Spend}}{\text{Avg. Assets}})$	(4) $\log(\frac{\text{Ad. Spend}}{\text{Avg. Assets}})$
RPE	+/0	0.158**	0.185***	0.013	0.019
		(2.566)	(2.929)	(0.277)	(0.378)
CARTEL			0.095		0.062
			(0.792)		(0.523)
RPE x CARTEL	−/0		−0.287***		−0.165
			(−3.596)		(−1.129)
Fixed Effects					
Firm		Yes	Yes	Yes	Yes
SIC-Year		Yes	Yes	Yes	Yes
Type of RPE		Peer	Peer	Generic	Generic
Observations		6,511	6,511	6,511	6,511
R-squared		0.932	0.932	0.932	0.932