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COMPENSATION DISCLOSURES AND THE WEAPONIZATION OF EXECUTIVE PAY: EVIDENCE FROM REVENUE-BASED PERFORMANCE EVALUATION

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ABSTRACT

Analytical work on strategic delegation shows that Cournot competitors can boost their profitability by using revenue-based pay to commit to more aggressive behavior (Fershtman and Judd, 1987), but only if pay packages are credibly disclosed (Katz, 1991). However, no empirical evidence demonstrates that firms actually employ such strategies. I exploit a regulatory shock that forced public firms to provide detailed executive pay disclosures, and document that large Cournot competitors adopt revenue-based pay in response to the disclosure mandate. Smaller firms and Bertrand competitors do not respond in this fashion. I find no evidence that agency theory can explain these patterns. Collectively, my results are consistent with strategic delegation, and suggest that, after the mandated disclosure of executive compensation packages, firms design their incentive contracts as strategic weapons, designed to curtail their rivals' competitive actions.

1 Introduction

In many oligopolistic industries, firms stand to gain by committing to aggressive product market behavior, thereby deterring entry or discouraging the competitive actions of incumbent rivals. Aggressive commitment can take many forms. For example, firms can build up large capacities (e.g., Kreps and Sheinkman, 1983; Moreno and Ubeda, 2006), invest in marginal cost reductions (e.g., Dixit, 1980; Brander and Spencer, 1983; Raith, 2003), engage in "divisionalization" (e.g., Schwartz and Thompson, 1986; Veendorp, 1991), vertically integrate (e.g., McGuire and Staelin, 1983; Hart, Tirole, Carlton and Williamson, 1990) or take out vast amounts of debt (e.g., Brander and Lewis, 1986; Brander and Lewis, 1988). Fershtman (1985) and Vickers (1985) introduce an alternative way for a firm to commit: "strategic delegation." By delegating decision rights to a properly incentivized agent, a principal can make credible otherwise untenable product market policies. For instance, a principal can commit their firm to aggressive overproduction by using to revenue-based pay to [partially] shield the agent from the costs of production. As with other forms of commitment, the strategic alteration of an agent's incentives must be observable to rivals to be effective (Katz, 1991).

I examine whether firms incorporate revenue-based compensation into CEO pay packages as a method of credibly committing to aggressive product market behavior. I document that revenue-based pay is more prevalent when the benefits of committing to aggressive product market behavior are greater—but only when executive pay packages are credibly disclosed. Moreover, I find descriptive evidence to suggest that firms respond to their rivals' disclosed contracts, adjusting their own production based on rivals' disclosed use of revenue-based pay.

I find no similar evidence for undisclosed contracts. Collectively, I interpret these findings as evidence that CEO pay packages are structured strategically, as product market weapons, designed to curtail rivals' competitive actions.

Of the extant theoretical literature, Fershtman and Judd (1987) provide the model that is most closely tied to my study. They analyze a two-period Nash game in which rivalrous principal-agent pairs compete to sell a homogenous good. In the first period, profit-seeking principals simultaneously choose the weights on profits and revenues in their agents' compensation contracts. In the second period, agents take all contracts as given, and compete on quantity to maximize their own contractually defined payoffs. Their model implies that principals will rationally place a positive weight on revenue based pay, but this weight diminishes as the number of product market competitors grows. Intuitively, a greater weight on revenue (relative to profit) incentivizes an agent to overproduce, *ex post*.[1] *Ex ante*, this commitment to aggressive behavior disciplines rival agents, causing them to curtail their own production, and thereby exert less downward pressure on prices. The fewer the firms, the stronger the effect.

Jointly, Fershtman and Judd (1987) and Katz (1991) provide a number of testable predictions. Chief among them: (1) if an agents' incentives are more credibly disclosed to rivals, their contracts will place a greater weight on revenue; and (2) the preceding prediction is stronger for more influential firms—i.e., those with greater capacity to affect their rivals' actions. However, these predictions are not expected to hold, generally; they are specific to industries in which rivalrous firms' strategic actions are substitutes, as opposed to comple-

1. The agent 'overproduces' in the sense that they produce more than the profit-maximizing amount.

ments.[2] Hereafter, I refer to industries as "Cournot" if strategic actions are substitutes, and "Bertrand" if strategic actions are complements.[3]

I begin my empirical investigation by classifying industries as Cournot versus Bertrand. Given the difficulties in doing so, I use three different measures for the mode of competition. My primary measure is constructed using an approach similar to Kedia's (2006). This method of classification is based on the relation between a representative rival firm's aggressiveness, and the own firm's marginal profits—if the relation is negative (positive), then strategic actions are said to be substitutes (complements). Importantly, this measure was designed with the concerns of Bresnahan (1989) and Nevo (1998) in mind. In short, industries will not be classified as Cournot (Bertrand) simply because rival performances comove negatively (positively). That said, classifying industries according to the nature of their strategic game is notoriously difficult, and no single broad-sample measure is likely to do this perfectly. For this reason, I supplement Kedia's (2006) measure with two alternative measures, one based on production flexibility (i.e., the importance of capacity constraints), and another based on the degree of product homogeneity. In theory, both production flexibility and product homogeneity are closely tied to the distinction between Cournot and Bertrand competition (e.g., Kreps and Sheinkman, 1983; Singh and Vives; 1984; Dixon, 1985; Maggi, 1996; Brander and Spencer, 2015). These two alternative measures are particularly appealing because they

2. Rivalrous firms' strategic actions are substitutes (complements) if one firm's aggressive product market behavior begets less (more) aggressive behavior from its product market rivals. For example, if firms compete by setting prices (à la Bertrand, 1883), when one firm lowers its price, rivals react by lowering their own prices—their actions are strategic complements. If, instead, firms compete by choosing production quantities (à la Cournot, 1838), then one firm's choice to produce more pushes rivals to produce less—their actions are strategic substitutes.

3. Strictly speaking, firms need not compete on quantities (prices) in order for their strategic actions to be substitutes (complements). A wide variety of games beyond Cournot and Bertrand can produce similar interdependencies. I use the terms "Cournot" and "Bertrand" loosely, referring to any product market in which strategic actions are substitutes or complements.

are uncorrelated with each other, but strongly correlated with my primary measure. This suggests an effective triangulation of the underlying construct—if the measures were all highly correlated, there would be greater concern that all three measures share the same problem(s).

The strategic benefits of revenue-based pay are determined not only by the type of competition (i.e., strategic substitutes vs. complements), but also a firm's "product market influence" (hereafter, simply "influence"). I define a firm's "influence" as the extent to which their actions affect rivals' behavior. For example, in an oligopoly with only a few large players, each firm wields considerable influence; rival behavior is affected significantly by any single firm's actions. In such industries, the benefits of commitment are substantial. In contrast, in a more competitive market where each firm is [approximately] a price taker, no single firm can viably elicit a significant response from their rivals. In such industries, there is no benefit of commitment. In an industry with some large and some small players, influence is concentrated in the large players; their actions significantly affect each other and the small firms, but the small firms' actions do not affect the large firms [as much]. Thus, the large firms stand to benefit more from commitment. In my main analyses, I use market shares as a firm-level measure of influence. This measure captures variation in influence both *across* industries (e.g., number of firms in the market) and *within* industries (e.g., scale of a given firm).[4]

Finally, the strategic benefits of revenue-based pay are contingent upon the credible disclosure of the executive's pay package. If an agent's incentives are not credibly relayed

4. Similar results attain using industry-level measures of influence: number of competitors, Herfindahl-Hirschman Index, and the proportion sales attributable to an industry's 50 largest firms (including private firms, as recommended by Ali, Klasa and Yeung, 2009).

to rivals, then revenue-based pay would be unable to elicit the desired response from them.[5] Accordingly, I exploit the staggered, mandated introduction of the Compensation Discussion and Analysis section of the proxy statement ("CD&A"), in 2006 and 2007, as a plausibly exogenous shock to the credibility of pay package disclosures. Prior to the introduction of the CD&A, firms were not required to disclose detailed information about the incentives provided to their top executives. While disclosing pay package details voluntarily was not forbidden, few firms chose to. Recent evidence suggests that voluntarily disclosed pay packages—those disclosed prior to the introduction of the CD&A—would not have been sufficiently credible to function as viable precommitment tools (e.g., Morse, Nanda and Seru, 2011; Gipper, 2017). I find no evidence that pay packages are structured strategically, prior to the introduction of the CD&A, and significant evidence of strategic contracting afterwards.

Specifically, I find that in Cournot industries, the prevalence of revenue-based incentives in CEO pay packages increases substantially after the introduction of the CD&A. Roughly 10% of firms in these industries adopt revenue-based incentives (for the first time) in their first year of mandated CD&A compliance.[6] Moreover, this increase is driven by more influential firms, for whom the strategic benefits of adopting revenue-based pay are predicted to be greater. Of firms with above-median (below-median) market shares, more than 15% (less than 7%) adopt revenue-based pay in the first year of mandated CD&A compliance. The timing of these changes aligns tightly with CD&A adoption dates. The increased usage of revenue-based pay begins in fiscal 2006 for 2006 adopters (December year-end firms) and in

5. This is true even if, in equilibrium, rivals always know exactly the nature of every agent's contract (Katz, 1991).

6. Compared to the pre-CD&A base-rate of roughly 15% in these industries, this represents a 66% increase in the prevalence of revenue-based pay.

fiscal 2007 for 2007 adopters (non-December year-end firms). I find no similar patterns in Bertrand industries.

I further examine whether the changes in executives' incentives can explain firm behavior in a manner consistent with theory. Consistent with Fershtman and Judd (1987), I find that revenue-based pay is associated with increased production costs. Holding the firm fixed, the adoption of revenue-based pay is associated with a 5% increase in total production costs. Moreover, this association is similar across pre- and post-CD&A periods, as well as across Cournot and Bertrand industries. This is consistent with CEOs' pay packages affecting their own decisions, irrespective of pay package disclosures or the nature of competition. In contrast, I find no evidence that *rivals'* use of revenue-based pay affects a firm's production costs prior to the CD&A and significant evidence of an effect after the CD&A. In Cournot industries during the post-CD&A period, a firm's production costs are significantly negatively associated with rivals' reliance on revenue-based pay. Holding the firm fixed, a one standard deviation increase in rivals' reliance on revenue-based pay is associated with a 2.6% reduction in production costs. This is consistent with firms curtailing their own production in response to rivals' use of revenue-based pay—but only when the rivals' pay packages are credibly disclosed.

The primary empirical challenge is ruling out alternative explanations for the patterns of revenue-based pay that I document. In particular, moral hazard/agency theoretic concerns also provide ample justification for the use of revenue-based pay. For example, if expenses are largely uncontrollable and fluctuate unpredictably, shielding an agent from this source of uncertainty by using revenue-based pay can reduce agency costs. I rely on several research design choices intended to limit the possibility that agency theory explains my results: (1)

I use a dense fixed effect structures including (firm, [industry,] and year)[7] to identify coefficients from within-firm and within-year variation in the use of revenue-based pay. Effectively, this fixed effect structure allows each firm to act as its own control, and ensures that my findings are immune to bias arising from time invariant cross-sectional heterogeneity, as well as sample-wide time trends; (2) I exploit the staggered adoption of a compensation disclosure mandate, that serves as a plausibly exogenous shock to the credibility of pay package disclosures. Theory suggests that such a shock has a first-order impact on the benefits of strategic contracting (e.g., Katz, 1991), but not on nature of the moral hazard problem; and (3) I employ several falsification analyses, to further quell concerns about correlated omitted variables and/or violations of my identifying assumptions. For example, I find no evidence that risk sharing changed differentially for highly influential firms in Cournot industries, after the introduction of the CD&A. This null result suggests that violations of the parallel trends assumption do not drive my findings. While it is impossible to entirely rule out violations of the parallel trends assumption, any such confound would have to explain a major shift in the optimal mix of metrics while having no impact on proportion of risk borne by the agent. In another falsification analysis, I document no significant changes in the use of other common metrics ("Earnings" and "Cashflow"). Any alternative story would have to explain why the patterns I document are specific to revenue-based pay.

This study contributes to multiple streams of literature. First and foremost, this paper contributes to the empirical literature on strategic delegation as the first paper to provide evidence that firms use their executive pay packages as strategic commitment devices. Ag-

7. Most firms never change industries, making industry fixed effects moot. However, they are included to account for the occasional industry-switcher. Results are unaffected if I drop the industry fixed effects, or drop firms that switch industries.

garwal and Samwick (1999) and Kedia (2006) provide some of the only related empirical work, both providing suggestive evidence that oligopolistic interdependencies shape executive pay. Kedia (2006) shows that pay for performance sensitivity is higher in Bertrand industries, while Aggarwal and Samwick (1999) find that in more competitive industries, managers' bonuses are more highly correlated with rival firm performance. However, both results are based purely on cross-sectional correlations, and neither paper exploits any sort of plausibly exogenous variation in the contracting environment (e.g., disclosure regulation). Thus, their descriptive results, while intriguing, cannot be confidently interpreted as evidence of strategic delegation. In contrast, I utilize detailed, grant-level compensation data and a quasi-experimental design to show the use of incentive contracts as commitment devices. I find that executives are more likely to have incentive pay tied to revenue when the benefits of committing to aggressive product market behavior are greater—but only after the introduction of a pay package disclosure mandate.

This paper further contributes to the literature on the "real effects of disclosure."[8] This paper is the first to show that the mandated disclosure of pay package details led firms to alter the performance metrics used in their executives' contracts. The post-CD&A shift that I document is consistent with a disclosure-induced 'weaponization' of executive pay; consistent with theory, after the adoption of the CD&A, contracts appear to become less focused on efficiently aligning the incentives of owners and decision makers, and more geared towards strategically manipulating rival behavior.

8. "Real effects of disclosure" refer to the impact of disclosure on the *ex ante* behavior of the disclosing party. For example, Jin and Leslie (2003) show that restaurants improve their hygiene quality when forced to disclose their hygiene score cards to potential customers. As another example, Dranove, Kessler, McClellan and Satterthwaite (2003) show that the public disclosure of patient health outcomes led doctors and hospitals to systematically decline treatment to the sickest patients, because they did not want to report adverse outcomes. They conclude that the disclosure mandate had a negative impact on overall patient welfare.

Finally, this study also contributes to the growing literature on the link between executive compensation and firm behavior. This paper is the first to provide descriptive empirical evidence that firms behave more aggressively (as captured by total production costs) when the CEO is given revenue-based incentives. This paper is also the first to demonstrate that a firm's product market behavior is associated with *rivals'* incentives, but only if those incentives are credibly disclosed. Consistent with theory (e.g., Fershtman and Judd, 1987), Cournot firms curtail their production when rivals are more reliant on revenue-based pay. Prior empirical work in this area has focused predominantly on the impact of equity incentives (e.g., stock and stock options) on risk-taking behavior (e.g., Knopf, Nam and Thorton, 2002; Coles, Daniel and Naveen, 2006; Low, 2009; Dittmann, Yu and Zhang, 2017; Shue and Townsend, 2017).

The remainder of this paper is organized as follows: in Section (2), I develop my hypotheses; in Section (3) I provide a summary of the institutional setting used to test my predictions; in Section (4), I outline my data sources and describe the sample selection and variable construction procedures; in Section (5), I describe my research design and present my empirical findings; and in Section (6), I conclude.

2 Hypothesis Development and Discussion

Fershtman and Judd (1987) demonstrate, analytically, that firms in Cournot industries can increase their profitability by committing to more aggressive product market behavior, through the use of revenue-based pay. The more influential the firm, the greater the

benefits of doing so.[9] However, Katz (1991) demonstrates that commitment through an incentive contract is only effective if the contracts are credibly disclosed to rivals. Hence, I predict:

Prediction 1: In Cournot industries, firms shift towards revenue-based pay when required to credibly disclose their CEOs' pay packages.

Prediction 2: Prediction 1 is stronger for firms with greater influence.

The primary goal of this paper is to test Predictions 1 and 2. However, these predictions rely on two important presuppositions:

Presupposition 1: Firms behave more aggressively when their CEOs are given revenue-based pay.

Presupposition 2: In Cournot industries, firms behave less aggressively when influential rivals are given revenue-based pay, but only if their contracts are credibly disclosed.

Both presuppositions derive directly from extant analytical work (e.g., Fershtman, 1985; Vickers, 1985; Sklivas, 1987; Fershtman and Judd, 1987). In supplemental analyses, I provide affirmative tests of both presuppositions.

9. Fershtman and Judd (1987) focus on symmetric markets, and use the number of firms in the market as the primary source of variation in influence; the fewer the firms, the more influence each firm has. However, similar intuition attains in asymmetric markets, in which some firms have more influence than others (e.g, due to heterogenous scale or capacity constraints). In general, the more influential the firm (i.e., the more their actions affect rivals' actions), the greater the benefits of commitment.

2.1 Discussion

The central tension in traditional contract theory (i.e., agency theory) is the tradeoff between risk and incentives. Incentivizing agents to exert unobservable, costly effort requires tying their compensation to imperfectly controllable measured performance, thereby exposing them to outcome risk. A fundamental implication of agency theory is that any metric which provides incremental information about an agent's effort should be included in the optimal compensation contract. Moreover, the relative weight assigned to a metric should rise with its relative informativeness (e.g., Holmström, 1979; Baiman and Demski, 1980; Holmström and Milgrom, 1987; Lambert and Larcker, 1987; Holmström and Milgrom 1991; Holmström and Milgrom, 1994). Under this paradigm, the separation of ownership and control is a problem that must be solved, and an agency cost minimizing incentive contract is the optimal, but imperfect, solution.

Building upon Schelling's (1960) theoretical framework of "strategic delegation," I explore the complementary perspective that owners can *exploit* the separation of ownership and control for strategic gain by using compensation contracts as commitment devices. When multiple agents' actions are interdependent, the incentives of one agent affect not only that agent's actions, but also those of rivalrous agents. If an agent's incentives can be observably altered by a compensation contract, such alterations can be used, strategically, to manipulate rivals' behavior (e.g., Fershtman, 1985; Vickers, 1985; Sklivas, 1987; Fershtman and Judd, 1987; Koçkesen, 2004; Koçkesen, 2007).[10]

10. Koçkesen and Ok (2004) and Koçkesen (2007) depart from the others in that they endogenize the choice to delegate decision rights. In their framework, contracts can be used strategically, even if they are not disclosed, because the decision to delegate functions as a costly signal (à la Spence, 1973), which fully reveals the contract. In the context of my study, it seems unlikely that the owners of the largest publicly traded firms decide to whether or not to delegate decision rights to executives based on strategic incentives.

Agency theory and strategic delegation are not mutually exclusive. Pay packages can be structured jointly, by trading off the desire to efficiently align incentives against the benefits of strategic commitment. The purpose of this study is not to pit agency theory and strategic delegation against each other. Rather, the goal is to hold fixed the agency theoretic drivers of compensation and examine whether/how firms adjust their contracts when the value of strategic contracting is exogenously shocked by a disclosure mandate.

3 Setting: The Introduction of the CD&A

In 2006, the Securities and Exchange Commission (SEC) passed a new rule, requiring publicly listed firms to provide much more detailed summaries and discussion of their compensation practices for top executives. These new disclosures form the Compensation, Discussion and Analysis section of the firm's annual proxy statement ("CD&A"). In addition to disclosing the total *amount* of compensation awarded to each executive (as had been required for decades), in the CD&A, firms were also required to disclose how they determined the amount of compensation. What metrics are used to evaluate the manager, and what are the payouts for achieving a pre-specified performance target? That is, the CD&A provides a mandated public disclosure about the explicit monetary incentives provided to a firm's top management. Consistent with these requirements, Gipper (2017) shows that "the CD&A includes details on the determinants of management compensation as both a narrative and, in many cases, through direct, incentive pay calculations." Such detailed information was rarely, if ever, provided prior to 2006. Publicly listed firms were required to comply with the new rules

Accordingly, I assume that the decision to delegate is an exogenous precondition, and analyze the nature, rather than the existence, of compensation contracts. A violation of this assumption will bias against my empirical predictions.

starting in fiscal 2006 (2007) if their fiscal year ended after (before) December 15th.

One might reasonably question whether the CD&A is required for firms to use revenue-based pay strategically—given the benefits of commitment, why wouldn't firms disclose the details of their compensation contracts voluntarily? One explanation is that the voluntary disclosure of contract details was not sufficiently credible. Consistent with this explanation, Morse et al. (2011) study compensation paid to CEOs over the period of 1993-2003 (before the CD&A's introduction), and find evidence that firms routinely deviate from voluntarily disclosed pay plans. They argue that such deviations are the result of rent-seeking behavior by powerful managers, and that more credible disclosure, *ex ante*, is needed to prevent these *ex post* deviations. On the other side of the coin, Gipper (2017) documents that executive pay levels rise following the CD&A and argues that this might be because "the CD&A disclosure can bind the compensation committee to a pay plan," and that "binding to a set of performance metrics *ex ante* might become inefficient when the committed compensation determinants do not best measure the efforts of management *ex post*."

While the two studies differ in their normative implications, they are aligned in suggesting that mandatory disclosure of contract details, as required under the CD&A, are more effective than voluntary disclosures at binding firms to a pay plan, and credibly conveying executive incentives. This view is shared by industry experts, with Exequity's Robbi Fox commenting:

> "Historically, compensation committees have deviated from their stated goals to provide executives with discretionary bonuses, but it's become increasingly rare. That type of behavior was much more common before 2006, because these days you'd have to explain it in the CD&A. You had established goals, and now you're varying from those goals—and with the CD&A those sorts of deviations receive a lot of scrutiny. Certainly the disclosures around those kinds of actions have caused companies to think twice. You'd need to have a really good reason" (R.

Fox, personal communication, July 18th, 2016).[11]

Jointly, these facts point towards the CD&A improving the viability of revenue-based pay as a commitment device. The CD&A required firms to make more precise disclosures about their executive's pay plans, *and* improved the credibility of pay package disclosures by ramping up the costs of deviating.

One concern when using regulatory interventions as instruments is the endogenous nature of the regulator's decision to regulate. In my setting, one important institutional fact bolsters the claim of plausible exogeneity: the regulator responsible for the CD&A (the Securities and Exchange Comission) was concerned about a lack of transparency in executive compensation, *not* about product market competition (Securities and Exchange Commission, 2006a; 2006b). Their mission is to "protect investors, maintain fair, orderly, and efficient markets, and facilitate capital formation" (Securities and Exchange Commission, 2016), making it implausible that their decision to regulate was driven by relevant product market concerns. While this fact eliminates one major endogeneity concern, it does not guarantee that the CD&A is truly exogenous in the econometric sense that CD&A compliance is uncorrelated with omitted drivers of revenue-based pay. For this, I will rely on various parallel trends assumptions. With each of my main analyses, I explicitly state the identifying assumptions required to interpret the results causally.

11. Robbi Fox has a been a Senior Advisor with Exequity since 2010. Before that, she was a Principal and Senior Compensation Consultant with Hewitt Associates for almost 25 years.

4 Data, Sample and Measures

4.1 Data Sources and Sample Selection

Data for this paper comes from the intersection of Compustat's Annual and Quarterly Industrial Files, and Incentive Lab. I limit my attention to the years 2002–2009 (a symmetric window around CD&A adoption), and discard firms for which no data is available prior CD&A adoption. I further discard firm-year observations with vague disclosures that do not allow me to clearly determine whether or not the firm uses revenue-based pay (roughly 8% of the sample).[12] This yields a final sample of 4,156 firm-year observations from 866 unique firms. In supplemental tests, I further incorporate ExecuComp data on realized pay outcomes, and Compustat data on firm size and production costs. In these additional tests, the sample shrinks based on data availability.

4.2 Variable Construction

Below, I outline the construction of the variables used in my tabulated analyses. Summary statistics can be found in Table (1).

Executive Incentives

I measure executive incentives using an indicator variable equal to one if the CEO has any compensation tied to absolute performance "Sales" objectives, according to Incentive Lab. I refer to this variable as *RBonus*. I use an extensive margin measure because this

12. The advantage of this design choice is that it ensures that my results are driven by changes in the underlying contracts, rather than changes in the quality of my data. My results are not sensitive to this design choice.

information is available in both the pre- and post-CD&A periods. Intensive margin measures (i.e., the weight placed on revenue-based pay) cannot be reliably constructed for pre-CD&A observations. This data limitation biases *against* any results, because it precludes me from identifying changes in the use of revenue-based pay, among firms who have always used it.

Influence

In my main analyses, I measure influence using market shares, defined at the 4-digit SIC level. Because market shares are endogenous to managerial incentives, I use a static firm-level measure, equal to a firm's average pre-CD&A market share.[13] Intuitively, a firm that commands very little market share (either because it is small, or because the number of competitors is large) will not be able to manipulate rivals through precommitment. In contrast, a firm with a large market share (either because it is large, or because the number of competitors is small) stands to gain much more through precommitment. Due to the skewness of this measure, I use the natural logarithm in my empirical analyses, and refer to this measure as $log(Share)$.

One advantage of a firm-level measure is that it captures influence across two margins: (1) the number of competitors; and (2) the scale of the firm. Therefore, this measure reflects influence better than industry-level measures (e.g., number of firms, concentration, etc...), which assume a symmetric product market.[14] However, I verify that my results are robust

13. If I were to use firm-year market share, it would create a concern of reverse causality: do certain firms adopt revenue-based pay because they are influential? Or do certain firms gain market share because they adopt revenue-based pay? Using a static measure eliminates this concern. However, if I replicate my analyses using firm-year market shares, my inferences are unaffected.

14. For example, suppose an industry has one large player and many small players. An industry-level measure might suggest the industry is quite competitive, but in reality the large firm has significant influence over the actions of the small firms. In contrast, consider an industry one large firm and a few small firms. In this case, and industry-level measure might suggests that *all* the firms are quite influential, when in reality

to several industry-level measure: number of competitors, Herfindahl-Hirschman Index, and the proportion sales attributable to an industry's 50 largest firms.

Pay Package Disclosure

I exploit the mandated introduction of the CD&A as a shock to pay package disclosure. In my empirical analyses, I use *Post*, which takes a value of one during and after 2006 (2007) for December (non-December) year-end firms.

Mode of Competition

In theory, whether a firm would benefit from a commitment to more aggressive or more passive behavior depends on whether strategic actions are substitutes (e.g., Cournot) or complements (e.g., Bertrand). Intuitively, strategic actions are substitutes (complements) if one firm's more aggressive behavior begets less (more) aggressive behavior from its product market rivals. As a practical matter, estimating the mode of competition is quite difficult. Accordingly, I use three different approaches. The details of the construction for each measure are provided at the end of this subsection. Each measure of the mode of competition is estimated at industry level, as classified by Fama and French 48 industry classification.

My primary measure is based on the regression approach developed by Kedia (2006), designed to directly estimate the mode of competition from realized performance outcomes. This measure was designed in light of Bresnahan (1989) and Nevo's (1998) critiques, and won't naïvely classify industries as Cournot (Bertrand) simply because of negative (positive) correlation in rivals' performance outcomes (e.g., due to correlated supply and/or demand

only the larger one is.

shocks). Instead, it estimates the sign of the cross-partial of marginal profits with respect to both own aggressiveness, and rival firm aggressiveness. I refer to this measures as "Kedia."

This measure is *conceptually* ideal for two reasons: (1) it is based on the sign of the slopes of firms' best response functions, which dictates whether firms would choose to prefer to commit to more aggressive or more passive behavior; and (2) it is flexible enough to reflect the sign of strategic interdependencies across a wide array of possible games (e.g., Cournot; Bertrand; vertical differentiation; spacial positioning; advertising; etc...). However, in *practice*, estimating the sign of the slopes of firms' best response functions is notoriously difficult, and can be easily confounded by omitted factors. For this reason, I offer two supplemental measures to better triangulate the mode of competition.

The first supplemental measure is based on production flexibility (i.e., the importance of capacity constraints). Kreps and Sheinkman (1983) show that Bertrand competition is equivalent to Cournot if firms must commit to quantities through capacity investments. Dixon (1985) elaborates:

> "[i]t has long been recognized that flexibility of production lies at the heart of the distinction between Bertrand and Cournot models. The most natural application of the Cournot model would seem to be in the case where output in fixed in the short run."

Maggi (1996) formalizes this intuition with an analytical model in which the mode of competition is determined endogenously by a single structural parameter: the importance of capacity constraints. If capacity constraints are not important, firms will endogenously choose to engage in Bertrand competition. If capacity constrains are more binding, firms will endogenously choose to engage in Cournot competition. In this spirit, I capture Cournot vs. Bertrand competition using a measure of production flexibility based on capacity invest-

ments, and refer to this measure as "Prod. Flex." This measure produces an intuitive ranking, with low flexibility industries tending to be agricultural or industrial industries in which slowly responding capital (e.g., land and factories) play a critical role in production (e.g., agriculture, mining, fabricated products, shipbuilding/railroad equipment, etc...).

The second supplemental measure is based on the degree of product differentiation. Singh and Vives (1984) show that the benefits of Cournot competition over Bertrand competition rise as product substitutability increases. That is, given the ability to choose the mode of competition, firms would be increasingly likely to choose to compete in quantities (rather than prices) the more homogenous their goods. More importantly, the pressure to engage in endogenous product differentiation is "orders of magnitude" larger for firms engaged in Bertrand competition (Brander and Spencer, 2015). Both of these facts point towards a greater effort to differentiate in Bertrand industries than in Cournot industries. Accordingly, I construct a measure based on firms' efforts to differentiate themselves (through R&D and advertising), and refer to it as "Homog." This measure also produces an intuitive ranking. High homogeneity industries include: agriculture, precious metals, non-metallic and industrial metal mining, coal, petroleum and natural gas, and chemicals, while low homogeneity industries include: food products, consumer goods and personal services.

To demonstrate the robustness of my findings, I tabulate all of my main results for all three measures. The two supplemental measures are particularly appealing because they are both highly correlated with the Kedia measure, but *uncorrelated* with each other ($\rho = 0.0088$, $p = 0.969$). Their strong correlation with Kedia suggests that they capture the same underlying construct. Their lack of correlation with each other suggests that they provide an effective triangulation of the underlying construct. If the two were highly correlated, it

would invite the concern that the two simply share the same problem(s). Any concerns about measurement error impacting the validity of my findings would have to explain why two uncorrelated proxies both yield the same results. Table 2 tabulates all three codings for each of the 48 Fama and French Industries.

Kedia Measure Construction

Kedia (2006) proposes a methodology to directly estimate the sign of firms' response functions from Compustat's Quarterly Fundamentals data. For each firm-year, she runs the estimating equation:

$$\Delta \frac{\Delta \pi_{i,t}}{\Delta x_{i,t}} = \beta_1 x_{i,t} \Delta x_{i,t} + \beta_2 \Delta x_{i,t} + \beta_3 x_{i,t} \Delta x_{j,t} + \beta_4 \Delta x_{j,t}, \tag{1}$$

where π_i is firm i's quarterly profits, x_i is firm i's quarterly revenues, and x_j is the average contemporaneous quarterly revenue of all of firm i's rivals (defined at the 4-digit SIC level). The sign of $\hat{\beta}_3 x_i + \hat{\beta}_4$ is the estimator of the sign of the strategic interaction, which reflects the mode of competition. Aggregating over all firm-years in an industry provides an industry-level measure of Cournot versus Bertrand competition. I code industries as Cournot if the median sign is negative, and Bertrand if the median sign is positive.[15]

Intuitively, the left-hand side variable is intended to capture the returns to more aggressive actions. With this interpretation in mind, $\hat{\beta}_3 x_i + \hat{\beta}_4$ reflects the extent to which these marginal returns vary with rival aggressiveness (proxied for by x_j). If $\hat{\beta}_3 x_i + \hat{\beta}_4 > 0$, then rivals' aggressiveness is estimated to increase the gains to aggressiveness (i.e., strategic actions

15. Kedia (2006) differs in two respects: (1) she aggregates at the 4-digit SIC level, and (2) she only codes industries as Cournot or Bertrand if the F-statistic suggests that the estimator is *significantly* positive or negative. Neither alternation substantively affects my inferences.

are complements), and competition is said to be Bertrand. If $\hat{\beta}_3 x_i + \hat{\beta}_4 < 0$, then rivals' aggressiveness is estimated to decrease the gains to aggressiveness (i.e., strategic actions are substitutes), and competition is said to be Cournot. While quarterly sales are undoubtedly an imperfect proxy for aggressiveness (e.g., due to supply/demand shocks, or *other* firms' actions affecting own-firm sales), the measure can be shown to be an unbiased estimator of competition type, if marginal costs are flat and demand is linear. Moreover, under these assumptions, the measure allows for arbitrary [correlated or uncorrelated] shocks to supply and demand.[16] For more intuition on the measure, see Kedia (2006).

Prod. Flex. Measure Construction

I measure production flexibility based on industry-level capital utilization. I calculate the average ratio of Property Plant and Equipment to average total assets, and code industries as Cournot (Bertrand) if the ratio is above (below) the median.

Homog. Measure Construction

I measure homogeneity based on firms' efforts to differentiate their products through advertising and R&D. I calculate the industry-level average annual spending on R&D and advertising, and code industries as Cournot (Bertrand) if they are below (above) the median in either category (both categories).

Variables Used in Supplemental Tests

In supplemental tests, I use several additional variables. I describe their construction here.

16. Such shocks can bias the *magnitude* of the estimate, but in a linear demand system with constant marginal costs, will not bias the *sign* of the estimate. For this reason, I rely only on the sign in my empirical analyses.

Risk Sharing

I measure risk sharing using the ratio of incentive compensation to total compensation, as reported by ExecuComp. I refer to this variable as "RiskShare."

$$RiskShare = \frac{TotalCompensation - Salary}{TotalCompensation}. \tag{2}$$

Other Incentives

Similar to the construction of RevenueBased, I construct firm-year indicator variables $EBased$ ($CBased$), if a CEO has absolute incentive pay tied to "Earnings" ("Cashflow"), as reported by Incentive Lab.

Other Measures of Influence

In my main analyses, I use a firm's pre-CD&A average market share to measure a firm's influence. I supplement this measure with two additional measures of scale: average pre-CD&A total assets, and average pre-CD&A production costs. Due to the skewness of these measures, I use their natural logarithms in my empirical analyses.

Product Market Behavior

In my tests of Presuppositions 1 and 2, I examine how aggressively firms are behaving, as captured by deflated production costs (Costs of Goods Sold divided by total assets). This variable differs from the production cost measure of influence in two ways: (1) it is a firm-year measure—the measure of influence is a time-invariant pre-CD&A average; and (2) it is deflated by total assets—the measure of influence is unscaled. Due to the skewness of this measure, I use the natural logarithm in my empirical tests, and refer to it as "log(Costs)."

Rivals' Incentives

In my tests of Presupposition 2, I examine the relation between firm production and rivals' incentives. To to this, I construct two measures of rivals' incentives: *RivalsRBonus* is the firm-year influence-weighted average of *RBonus*, among a firm's 4-digit SIC rivals. *RivalsDiscRBonus* is the firm-year influence-weighted average of $RBonus \times Post$, among a firm's 4-digit SIC rivals.

$$RivalsRBonus_{i,t} = \frac{1}{\sum_{k \neq i} Share_k} \sum_{k \neq i} Share_k \times RBonus_{k,t} \tag{3}$$

$$RivalsDiscRBonus_{i,t} = \frac{1}{\sum_{k \neq i} Share_k} \sum_{k \neq i} Share_k \times RBonus_{k,t} \times Post_{k,t}. \tag{4}$$

These variables take values between zero and one, inclusive. *RivalsRBonus* takes a value of zero (one) if none (all) of a firm's rivals use revenue-based pay. Similarly, *RivalsDiscRBonus* takes a value of zero (one) if no (all) firms are using revenue based pay, *and* required to disclose.

5 Empirical Analysis

5.1 Baseline Analysis

The first step in my empirical analyses is to document, descriptively, whether the use of revenue-based pay increased after the CD&A, in Cournot industries. I further test whether this increase was stronger for more influential firms. I begin by examining the trends in revenue-based pay, graphically. I split firms, at the median, into high influence and low

influence portfolios, and plot the prevalence of revenue-based pay over time. Through 2005, revenue-based pay appears to be similarly prevalent across high and low influence firms, at about 15%. However, after the introduction of the CD&A in 2006, the two groups diverge substantially. The prevalence of revenue-based pay increases abruptly in 2006, but only for high influence firms. The two groups reach a peak difference in 2006 (21% vs. 14%) and proceed to follow parallel trends after 2007. These results are shown in Figure 1.

I test for the interactive effects of the CD&A and influence using the following regression specification:

$$RBonus_{i,t} = \beta_1 Post_{i,t} \times log(Share_i) + \beta_2 Post_{i,t} + u_j + \mu_i + \tau_t + \varepsilon_{i,j,t} \tag{5}$$

where *Post* is an indicator variable, taking a value of one if a firm has adopted the CD&A, *Share* is a firm's pre-CD&A average market share (defined at the 4-digit SIC level), and u, μ and τ are industry, firm and year fixed effects.[17] The dependent variable, *RBonus*, is an indicator variable equal to one if the CEO is given performance incentives tied to revenue. I present three specifications, which vary only with respect to the measure for the mode of competition. In Specification 1 (2) [3], I use the Kedia (Prod. Flex.) [Homog.] measure. Results can be found in Table 3.

The results align closely with my predictions. The main effect of *Post* is positive and significant throughout the table, indicating that revenue-based pay became more common in Cournot industries after the introduction of the CD&A. The interaction of *Post* and $log(Share)$ is highly significant, both economically and statistically, indicating that the post-

17. Throughout the manuscript, the intercept and the main effect of $log(Share)$ are suppressed, as they are subsumed by the fixed effects.

CD&A shift towards revenue-based pay was more pronounced for more influential firms. This result attains for all three measures of Cournot competition. On average, a one standard deviation increase in a firms' influence is associated with a 4.2-6.4 percentage point increase in the likelihood that the firm will adopt revenue-based pay, post-CD&A.

While the preceding evidence (Figure 1 and Table 3) aligns closely with my predictions, it should be interpreted cautiously. In particular, there is no benchmark for these analyses, which opens two questions: (1) how can one be confident that the documented patterns were *caused* by the CD&A, as opposed to merely coincident? and (2) even if documented patterns accurately depict the causal effect of the CD&A, how can one conclude that the effect is driven by *strategic* considerations, and not some other reason that firms of heterogenous influence might react differentially to a disclosure mandate? In what follows, I attempt to address these concerns by using appropriate benchmarks.

5.2 Benchmark: Late Adopters

Not all firms became "treated" by the CD&A simultaneously. December year-end firms (roughly three-quarters of the sample) were required to comply for fiscal 2006, while non-December year-end firms were not required to comply until fiscal 2007. If the previously documented patterns are truly caused by the CD&A (as opposed to some other coincident factor), then the effect should manifest for December year-end firms before the rest. For these analyses, I restrict the sample to firms with above-median influence, and examine whether the increase in revenue-based pay follows firms' CD&A adoption schedules.

Graphical analysis of influential Cournot firms provides visual evidence of exactly this

effect. Pre-CD&A trends are parallel and flat (but apart). In fiscal 2006, the prevalence of revenue-based pay jumped by 10 percentage points for December year-end firms, and remained constant for non-December year-end firms. Then, in the next year, the prevalence of revenue-based pay jumped by about 12 percentage points for firms with non-December year ends. See Figure 2.

I test for the effect of the CD&A on revenue-based pay using the regression specification:

$$RBonus_{i,t} = \beta Fiscal2006_t \times EarlyAdopter_i + u_j + \mu_i + \tau_t + \varepsilon_{i,j,t}, \tag{6}$$

where $Fiscal2006$ is an indicator variable, equal to one in fiscal year 2006 and $EarlyAdopter$ is an indicator variable equal to one for December year-end firms.[18] Results, presented in Table 4, provide clear evidence that variation in adoption dates explains variation in firms' use of revenue-based pay. Relative to late-adopters of the similar influence, early-adopters in Cournot industries increase their use of revenue-based pay by roughly 10-13 percentage points in the first year of compliance (Table 4, specifications 1-3). Firms in Bertrand industries exhibit no similar patterns (Table 4, specifications 4-6).

As a caveat, fiscal year ends are not assigned at random. Firms with non-December year-ends are systematically different from those with December year-ends. To the extent that these differences are constant over my sample period, firm fixed effects will eliminate the confound. But I cannot rule out the possibility that the different types of firms react differentially to omitted external factors. The coefficient on $Fiscal2006 \times EarlyAdopter$ can be interpreted causally with the following identifying assumption: absent the introduction of

18. The main effects of each are subsumed by firm and year fixed effects.

the CD&A, usage of revenue-based pay would have followed parallel trends for December and non-December year-end firms. Any alternative reason for my results would have to explain: (1) why the late adopters did not exhibit the uptick in revenue-based pay usage in 2006; (2) why the late adopters did exhibit a large uptick in their use of revenue-based pay in 2007; and (3) why these patterns are specific to Cournot industries.

5.3 Benchmark: Bertrand Industries

The baseline analysis demonstrates that, in Cournot industries, revenue-based pay became more prevalent after the CD&A—especially for more influential firms. If this same pattern emerges for firms in Bertrand industries, it would suggest that the prior results are unrelated to strategic considerations, as Bertrand competitors would not wish to commit in this fashion. Accordingly, I augment the baseline analysis to test whether influential firms' post-CD&A shift towards revenue-based pay was differentially present in Cournot industries vs. Bertrand industries.

The analysis is akin to a triple differences design, with a $Pre - Post$ difference, a $Cournot - Bertrand$ difference, and continuous variation in influence. Highly influential firms in Cournot industries form the "treatment group," and their $Pre - Post$ difference is benchmarked jointly against *two* different "control groups:" (1) less influential firms in Cournot industries, as in Table 3; and (2) similarly influential firms in Bertrand industries.

The identifying assumption is the following: absent the introduction of the CD&A, the relation between revenue-based pay and influence would exhibit parallel trends across Cournot and Bertrand industries. This identifying assumption is substantially more forgiving than

that of a conventional difference in differences design. Notably, it allows for nonparallel trends in revenue-based pay across Cournot and Bertrand industries, as well as across high and low influence firms. I must only assume that these trends, if they are nonparallel, do not *interact* with one another. Specifically, any threat to my results must explain why influence and revenue-based pay comove differentially in the pre- and post-CD&A periods, *and* why the shift in this comovement relation is systematically different across Cournot and Bertrand industries. The augmented regression specification is:

$$\begin{aligned} RBonus_{i,t} = {} & \beta_1 log(Share_i) \times Post_{i,t} \times Cournot_j + \beta_2 Post_{i,t} \times Cournot_j \\ & + \beta_3 log(Share_i) \times Post_{i,t} + \beta_4 log(Share_i) \times Cournot_j \\ & + \beta_5 Post_{i,t} + u_j + \mu_i + \tau_t + \varepsilon_{i,j,t}. \end{aligned} \tag{7}$$

I present three specifications which differ only with respect to the measure for the mode of competition. In specification 1 (2) [3], I use the Kedia (Prod. Flex.) [Homog.] measure. The main effects of $Cournot$ and $log(Share)$ are subsumed by the fixed effects.

The coefficient of interest is β_1, which captures the differential post-CD&A shift towards revenue-based pay for influential firms in Cournot industries. Across all three specifications, results align with my theoretical predictions, as can be seen in Table 5. The shift towards revenue-based pay is differentially pronounced for influential firms in Cournot industries. This suggests that the shift in revenue-based reflects *strategic* considerations, and not some unrelated reason that firms of heterogeneous influence respond differentially to the CD&A.

5.4 Supplemental Analyses

The results presented in Sections 5.1-5.3 align closely with my predictions; after the CD&A, firms in Cournot industries shifted towards revenue-based pay—especially more influential firms. Moreover, the timing of the effect lines up with the staggered timing of CD&A adoption, and the effects are present only in Cournot industries. Collectively, I interpret these findings as evidence that influential firms in Cournot industries begin using their executives' pay packages *strategically* as precommitment devices, after the introduction of the CD&A. In what follows, I offer additional tests, designed to encourage this interpretation of the prior results, and provide descriptive tests of Presuppositions 1 and 2.

Falsification Tests

The preceding analyses present evidence that after the introduction of the CD&A, the use of revenue-based pay increased, differentially, for highly influential firms in Cournot industries. The documented patterns mirror my theoretical predictions, but cannot rule out the possibility that nonparallel trends in agency theoretic concerns explain my results. In this subsection, I provide several falsification tests, intended to assess this possibility. In each test, the design exactly matches those of Tables 4 or 5, but uses a different outcome variable. The outcomes variables were chosen specifically because they would likely succeed in falsifying my identifying assumptions, if major violations occur in the sample. I find null results across all falsification tests. While these null results cannot entirely preclude violations of the parallel trends assumptions, they do substantially restrict the set of plausible alternative explanations for my findings.

Falsification 1: Other Performance Metrics

If the previously documented patterns are the result of major agency theoretic changes around the time of the CD&A, such changes would likely effect the usage of several common metrics—not just revenue. I replicate the prior analyses (Tables 4 and 5) using reliance on other common performance metrics ("Earnings" and "Cashflow") as the outcome variable and find no similar patterns. As shown in Table 6, the use of these metrics did not change differentially, after the CD&A, for influential firms in Cournot industries. For brevity, I report only the coefficients of interest. In Panel A, each element represents the estimated coefficient (and associated standard error) on $Fiscal2006 \times EarlyAdopter$. In Panel B, each element represents the estimated coefficient (and associated standard error) on $log(Share) \times Post \times Cournot$. Results are tabulated for all three performance metrics (Revenue, Earnings and Cashflow) and all three measures of the mode of competition.

Falsification 2: Risk Sharing

The proportion of risk born by the principal versus the agent is one of the most fundamental constructs in agency theory (Holmström, 1979). It is unlikely that a shock to agency theoretic considerations would cause firms to substantively alter the mix of metrics used to evaluate agent performance while leaving risk sharing unaffected.[19] In this spirit, I replicate the prior analyses using $RiskShare$ as the outcome variable. I find no evidence that risk sharing changed differentially for influential firms in Cournot industries, as shown in Table 7. I provide tabulated results for all three measures of the mode of competition,

19. For example, suppose firms shifted towards revenue-based pay because costs became more volatile. In most cases, the increase in cost volatility would also imply a shift in the optimal risk-sharing arrangement between the principal and agent.

and document null results across all of them. The results are not only statistically insignificant, but also tiny in economic magnitude. For brevity, I report only the coefficients of interest. In Panel A, I present the estimated coefficient (and associated standard error) on $Fiscal2006 \times EarlyAdopter$. In Panel B, I present the estimated coefficient (and associated standard error) on $log(Share) \times Post \times Cournot$.

Within-Industry-Year Variation (Synthetic Parallel Trends)

Causal interpretation of the analysis presented in Section 5.3 relies on a fairly forgiving parallel trends assumption. It allows for nonparallel trends in the use of revenue-based pay across more influential and less influential firms, as well as across Cournot and Bertrand industries—as long as the nonparallel trends do not *interact*. To further relax the identifying assumption, I augment the specification described by eq. (7) with industry-year and industry-post fixed effects. This analysis allows for arbitrary industry-specific time trends as well as arbitrary industry-specific responses to the CD&A. Thus, at the industry-level, the validity of the parallel trends assumption is synthetically imposed. For this analysis, the identifying assumption is: absent the introduction of the CD&A, *within-industry* trends in the use of revenue-based pay across more influential and less influential firms, would not have been *differentially* nonparallel in Cournot versus Bertrand industries. As before, this analysis allows for nonparallel trends across Cournot and Bertrand industries, as well as across high influence and low influence firms, so long as they do not interact. The primary difference is that this analysis limits the non-interaction assumption to *within-industry* trends. In this analysis, I also exploit alternative measure of influence: total assets and annual production

costs.

Results, presented in Table 8, remain consistent with my predictions across all specifications. This suggests that my prior findings cannot be easily dismissed as arising from nonparallel time trends across industries, or industry-specific responses to the CD&A. Any such effects would be subsumed by these additional fixed effects. While this design is tighter than that of Section 5.3, this tightness comes at a cost. The additional fixed effects are highly effective at eliminating potential confounds, but they also absorb much of the variation that, in theory, contributes to the treatment effect.

Decision-Relevance of Metrics (Tests of Presuppositions 1 and 2)

The theoretical foundations of this study require that executives' incentives can be materially altered by changing the mix of metrics used in performance evaluation. I offer two sets of analyses to corroborate this assumption. First, I test whether CEOs seem to care about achieving their revenue-based performance goals by examining the distribution of the difference between actual performance, and the contractually-specified "threshold goal." The threshold goal for a revenue objective specifies the amount of revenue the firm must realize in order for the CEO to receive any of the associated performance-based bonus. Compensation often jumps discontinuously upwards at the threshold, potentially creating strong incentives to "meet or beat" the goal (e.g., Murphy 1999).

I find that CEOs with revenue-based pay routinely *just barely* achieve their contractually specified revenue goals. In the distribution of the difference between actual performance and the contractually-specified threshold goal, there is a sharp discontinuity at zero (see

Figure 3).[20] It is an order of magnitude more common for a manager to barely meet the objective than barely miss the objective. This sharp discontinuity suggests that achieving the goal is important to decision makers within the firm, further suggesting that changing an executive's pay package is a viable way to affect firm behavior.

Second, I examine whether contractual terms appear to affect firm behavior by analyzing whether executive incentives can explain product market actions (namely: production decisions). To this end, I provide explicit tests of Presuppositions 1 and 2. However, given the endogenous nature of the choice to include revenue-based pay, I caveat that results from these analyses should be interpreted cautiously as *suggestive corroborating evidence*, and not a tightly identified demonstration of any causal effects. That said, I utilize tight fixed effect structures, in order to minimize the potential for supply/demand shocks to drive spurious inferences.

I test Presupposition 1 using variants on the following regression specification:

$$\begin{aligned} log(Costs_{i,t}) = \beta_1 RBonus_{i,t} + \beta_2 RBonus_{i,t} \times Cournot_j \\ + \beta_3 RBonus_{i,t} \times Post_{i,t} + \beta_4 Post_{i,t} + \mu_i + \theta_{j,t} + \varepsilon_{i,j,t}, \end{aligned} \tag{8}$$

where $\theta_{j,t}$ are industry-year fixed effects, defined at the 4-digit SIC level, and $log(Costs)$ is a firm's annual production costs, divided by total assets. While these results are primarily descriptive, I note that the tight fixed effect structure reduces the potential for confounding factors to induce spurious inferences. The inclusion of firm fixed effects ensures that time-

20. I caveat that the performance goals reported in Incentive Lab do not always map perfectly into the GAAP measures found in Compustat. Thus, this histogram includes measurement error attributable the use of non-GAAP revenue metrics in performance evaluation. This measurement error biases *away* from finding a sharp discontinuity.

invariant differences across firms are suppressed, while the inclusion of SIC-year fixed effects ensures that results are not driven by SIC-year variation in supply/demand. Results from these regressions are presented in Table 9.

Consistent with my predictions, I find that revenue-based pay is associated with increased production. Notably, this effect is not significantly different for firms in Cournot versus Bertrand industries (Table 9, specification 3), and is not significantly different in the pre- versus post-CD&A periods (Table 9, specification 4). Collectively, I view these results as affirmation of Presupposition 1; the results suggest that revenue-based pay pushes firms to behave more aggressively, irrespective of the nature of competition, or whether the pay package details are credibly disclosed to rivals.

I test Presupposition 2 using variants on the following two regression specifications:

$$\begin{aligned} log(Costs_{i,t}) &= \beta_1 RivalsRBonus_{i,t} \times Cournot_j \\ &\quad + \beta_2 RivalsRBonus_{i,t} + u_j + \mu_i + \tau_t + \varepsilon_{i,j,t}, \qquad (9) \\ log(Costs_{i,t}) &= \beta_1 RivalsDiscRBonus_{i,t} \times Cournot_j \\ &\quad + \beta_2 RivalsDiscRBonus_{i,t} + u_j + \mu_i + \tau_t + \varepsilon_{i,j,t}, \qquad (10) \end{aligned}$$

where $RivalsRBonus$ ($RivalsDiscRBonus$) is the influence-weighted average of $RevenueBased$ ($RevenueBased \times Post$) among a firm's 4-digit SIC rivals. I do not include SIC-year fixed effects in this analysis because they would be inappropriate given the construction of $RivalsRBonus$ and $RivalsDiscRBonus$.[21] Results from these regressions are presented

21. These variables are constructed as averages over all rivals in an SIC-year. For firms in the same SIC-year, all of the variation in these variables comes from their own incentives. Thus, if I were to include SIC-year fixed effects, I would be identifying off of own-firm incentives rather than rivals' incentives, which

in Table 10.

I find no relation between a firm's production, and rivals' overall use of revenue-based pay. Nor do I find any differential relation across Cournot and Bertrand industries (Table 10, specifications 1 and 2). However, for rivals' *disclosed* revenue-based pay, I find a strong relation. For firms in Cournot industries, production costs are negatively associated with their rivals' disclosed reliance on revenue-based pay (Table 10, specifications 3 and 4). The economic magnitude of the effect is substantial. Holding the firm fixed, a one-standard deviation increase in $RivalsDiscRBonus$ is associated with a 2.6% reduction in scaled production costs. This is consistent with firms adjusting their own behavior in response to rivals' disclosed incentives. Collectively, I view these results as affirmation of Presupposition 2.[22]

Sensitivity Analysis

To ensure that my results are robust to alternative design choices, I replicate my main analysis with several modifications.[23]

Logit Specification

I use a linear probability model as my main specification. This approach confers ad-

would be contrary to the purpose of the test.

22. Under imperfect competition, one player's incentives can affect other players' realized production costs, even *absent* any strategic response. For example, in Bertrand competition, if one firm unilaterally lowers its prices, rivals will incur lower production costs simply because consumers shift towards the lower priced good. However, it is unlikely that this mechanism is responsible for my findings, as the relation is specific to *disclosed* revenue-based pay. Moreover, in Cournot games, a firm's *revenues* are directly affected by its rivals actions, but its *production costs* are not. In Cournot competition, costs are driven entirely by the firm's own actions, and therefore only indirectly affected by rival behavior.

23. Results are left untabulated, but available upon request.

vantages in the form of easier interpretability and greater stability with dense fixed effect structures and lots of interactions (Neyman and Scott, 1948; Lancaster, 2000; Ai and Norton, 2003). In particular, linear probability models are robust to the "incidental parameters problem." However, given the well-documented issues associated with linear probability models (e.g., Maddala, 1986; Horrace and Oaxaca, 2006), I employ a logit analysis to verify that my results are not sensitive to my econometric specification. My inferences are not affected by this alteration.

Jackknife Analysis

To ensure that my results are not driven by any single industry, I use a jackknife analysis. I replicate my main results 48 times, dropping each of the Fama and French 48 industries, one-at-a-time, from my sample. My results remain statistically and economically significant in all tests.

Alternative Influence Measures

I use pre-CD&A market shares as my primary measure of influence. As alternatives, I also use several industry-level measures: the number of firms in an industry, an industry's Herfindahl-Hirschman index, and the proportion of sales attributable to an industry's 50 largest firms (including private firms). Results are consistent (and significant) across all approaches. I further verify that similar results attain using different industry definitions: 3-digit NAICS, and FIC-25, -50, -100, and -200.[24]

24. Fixed Industry Classifications ('FIC') are based on the cosine similarity of firm's product descriptions. For more details on the construction of the FIC industries, see Hoberg and Phillips (2010), Hoberg and Phillips (2014) and Hoberg and Phillips (2015).

6 Conclusion

This study provides evidence that firms structure their executive pay packages strategically, with the aim of conferring competitive advantages to themselves in their product markets. Specifically, I find that CEOs are more likely to have revenue-based pay when the benefits of aggressive commitment are greater. Consistent with theory, this relation only attains when compensation details are disclosed, suggesting that detailed pay package disclosures (i.e., a firm's CD&A) facilitate the use of compensation contracts as strategic commitment devices.

Moreover, I provide descriptive evidence suggesting that such strategies are effective. Firms appear to increase production when their own CEO is given revenue-based pay, and decrease their production when *rival* CEOs are given revenue-based pay. Consistent with theory, the latter result is specific to Cournot industries in the post-CD&A period.

Collectively, I interpret my findings as evidence of a disclosure-induced 'weaponization' of executive pay. After the introduction of the CD&A, firms begin to use their contracts are strategic product market weapons, designed to manipulate rivals' competitive actions. Consistent with theory, this behavior is especially pronounced among the most influential product market players.

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Figure 1: Timeseries of Revenue-Based Pay in Cournot Industries, Split by Influence

This figure plots the time series of revenue-based pay for CEOs in Cournot industries, as coded by the Kedia measure. I split the sample, at the median, into high market influence and low market influence groups, based on pre-CD&A market shares. Each line represents the proportion of CEOs who have incentive pay tied to revenue objectives. To ensure that changes are not driven by sample composition, I limit the sample to firms that provide data for all 8 years.

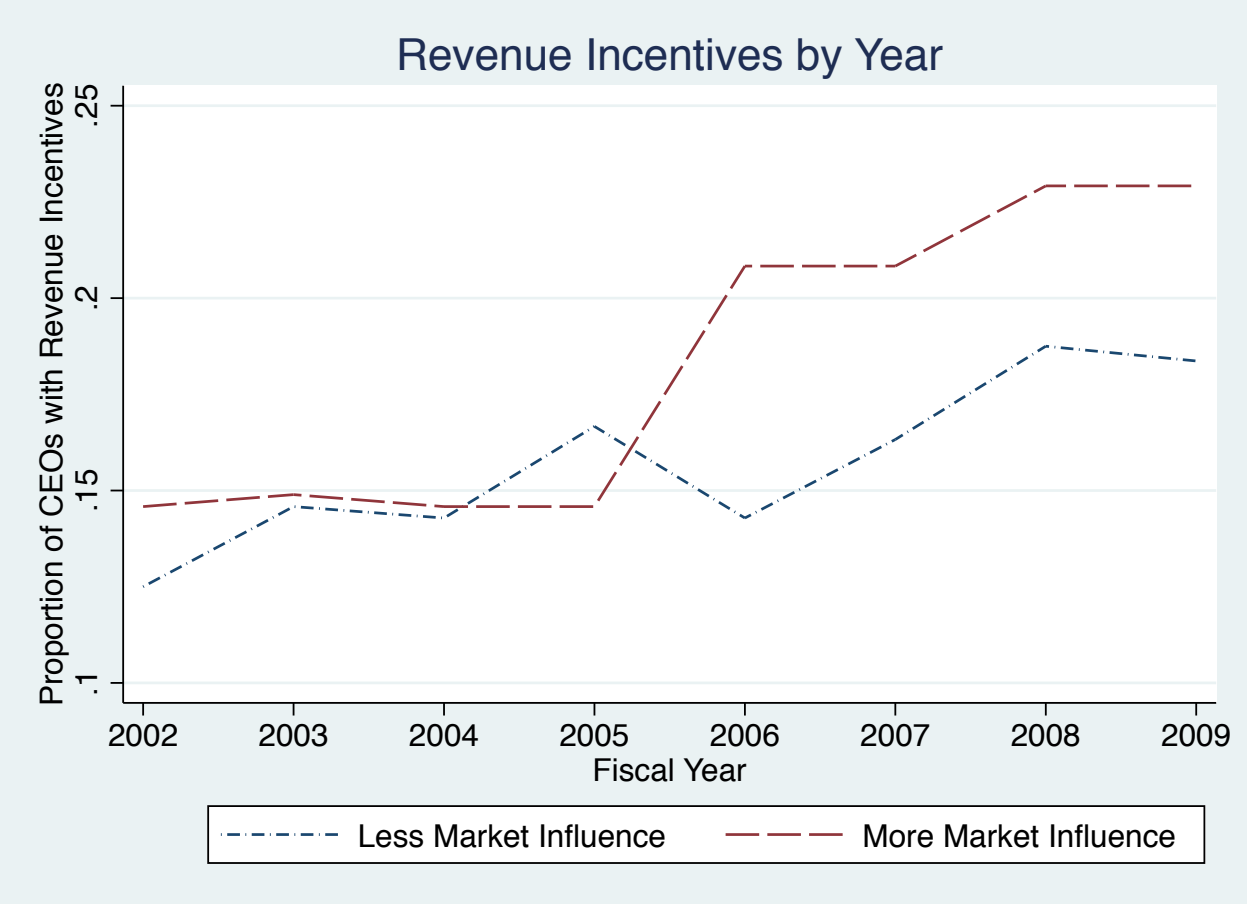

Figure 2: Timeseries of Revenue-Based Pay in Cournot Industries, Split by Adoption Year

This figure plots the time series of revenue-based pay for CEOs in Cournot industries, as coded by the Kedia measure. Only firms of above-median influence (based on pre-CD&A market shares) are included. I split the sample into "2006 Adopters" and "2007 Adopters," based on their fiscal year ends. Each line represents the proportion of CEOs who have incentive pay tied to revenue objectives. To ensure that changes are not driven by sample composition, I limit the sample to firms that provide data for all 8 years.

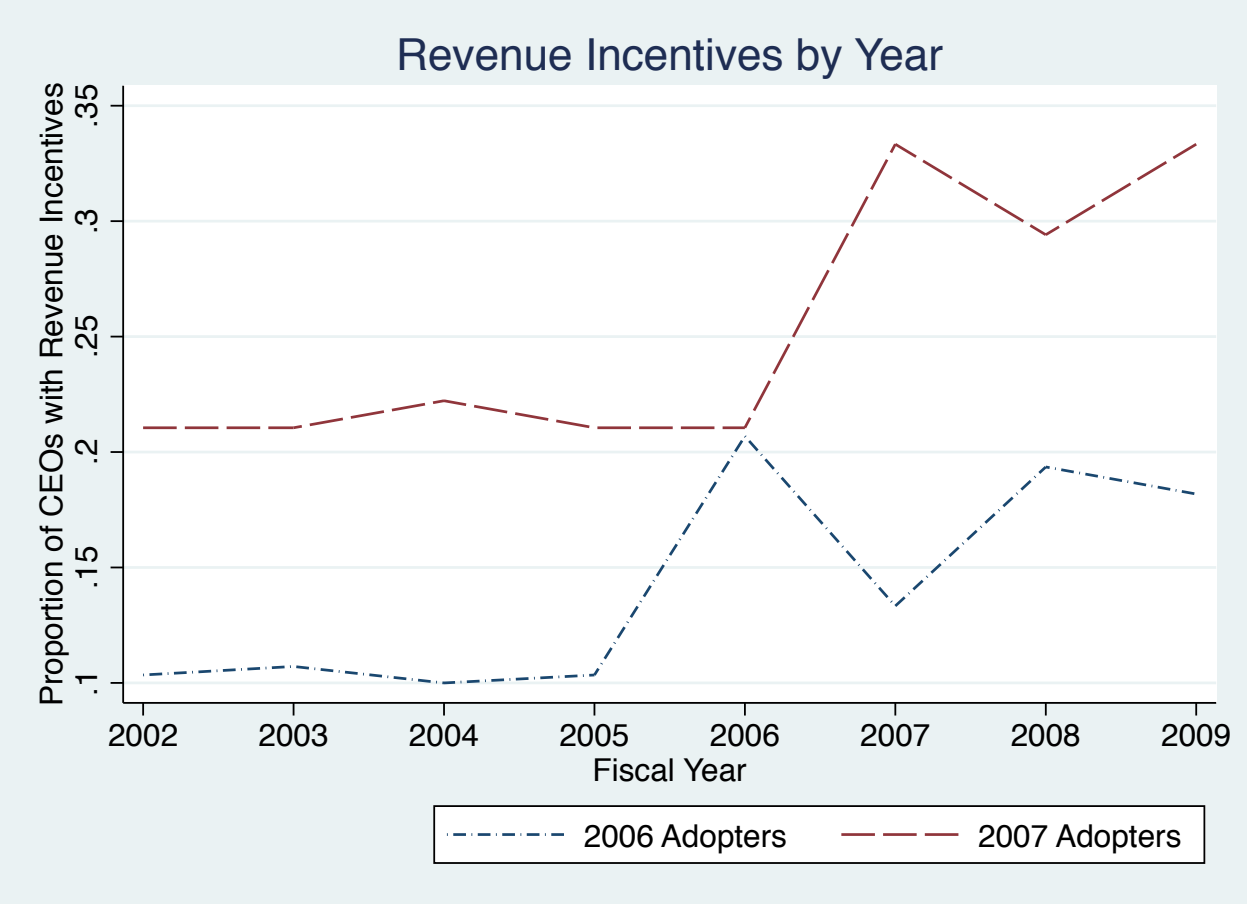

Figure 3: Distribution of Actual Revenues Relative to The CEO's Revenue Goal

This figure presents the distribution of actual sales relative to the CEO's contractually specified sales "threshold goal." The threshold goal specifies the amount the revenue the firm must achieve in order for the CEO to receive any associated bonus pay. Performance relative to the threshold goal is calculated as GAAP revenues minus the threshold goal for revenues, as a percentage of the firm's average total assets.

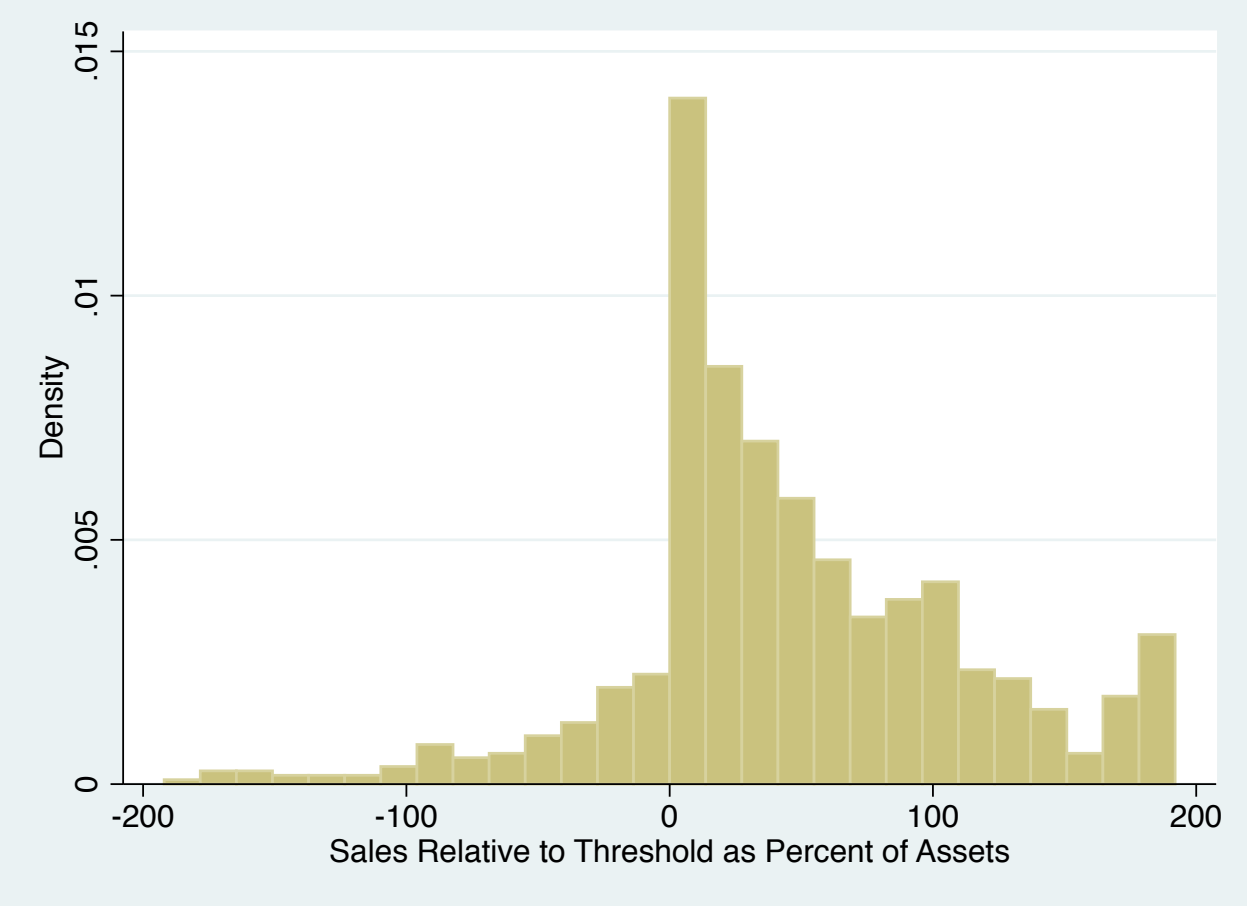

Table 1: Summary Statistics

This table presents summary statistics for selected variables used in my analyses. Panel A provides summary statistics while Panel B provides Pearson correlations. The sample spans 2002 to 2009 and comes from the intersection of Compustat and Incentive Lab. I exclude firms for which no pre-CD&A data is available. I further exclude firm-year observations for which metric choices are not clearly specified (about 8% of the sample). This produces a final sample of 4,156 firm-year observations from 866 unique firms.

Panel A: Summary Statistics

Variable	Obs.	Mean	sd	D1	Q1	Median	Q3	D9
RBonus	4,156	0.314	0.464	0.000	0.000	0.000	1.000	1.000
log(Share)	4,156	−3.095	1.912	−5.715	−4.464	−2.811	−1.573	−0.842
Cournot (Kedia)	4,156	0.531	0.499	0.000	0.000	1.000	1.000	1.000
Cournot (Prod. Flex.)	4,156	0.509	0.500	0.000	0.000	1.000	1.000	1.000
Cournot (Homog.)	4,156	0.717	0.451	0.000	0.000	1.000	1.000	1.000
Post	4,156	0.512	0.500	0.000	0.000	1.000	1.000	1.000
EBonus	4,156	0.630	0.483	0.000	0.000	1.000	1.000	1.000
CBonus	4,156	0.184	0.388	0.000	0.000	0.000	0.000	1.000
RiskShare	3,420	0.728	0.210	0.440	0.642	0.785	0.880	0.935
log(Costs)	3,569	−0.869	1.081	−2.202	−1.393	−0.684	−0.149	0.281
RivalsRBonus	3,262	0.033	0.084	0.000	0.000	0.001	0.018	0.095
RivalsDiscRBonus	3,262	0.019	0.064	0.000	0.000	0.000	0.003	0.063

Panel B: Pearson Correlations

	Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
(1)	RBonus	1.000										
(2)	log(Share)	−0.059	1.000									
(3)	Cournot (Kedia)	−0.167	0.033	1.000								
(4)	Cournot (Prod. Flex.)	−0.128	0.116	0.124	1.000							
(5)	Cournot (Homog.)	−0.216	0.081	0.544	0.009	1.000						
(6)	Post	0.077	0.018	−0.009	0.032	−0.021	1.000					
(7)	EBonus	−0.024	0.160	−0.065	−0.017	0.052	−0.032	1.000				
(8)	CBonus	0.077	0.123	0.004	0.088	−0.032	0.120	−0.033	1.000			
(9)	RiskShare	0.006	0.092	−0.011	−0.035	0.004	0.141	0.048	0.071	1.000		
(10)	log(Costs)	−0.113	0.413	0.174	0.244	0.073	0.029	0.106	−0.032	−0.028	1.000	
(11)	RivalsRBonus	0.129	0.151	−0.065	−0.056	−0.035	0.048	0.022	0.008	0.008	0.084	1.000
(12)	RivalsDiscRBonus	0.120	0.119	−0.052	−0.022	−0.044	0.258	0.000	0.019	0.015	0.054	0.724

Table 2: Fama French 48 Industries Classified as Cournot vs. Bertrand

This table presents the codings of each of the Fama and French 48 industries as a Cournot vs. Bertrand industry. For each industry, I present 3 codings. "Kedia" codes industries using the measure developed by Kedia (2006). "Prod. Flex" codes industries as Cournot (Bertrand) if industry-level average PP&E scaled by average assets is above (below) the median. "Homog." codes industries as Cournot if either industry-level average R&D or advertising spending is below the median, and Bertrand otherwise.

Industry	Description	Kedia	Prod. Flex.	Homog.
1	Agriculture	Cournot	Cournot	Cournot
2	Food Products	Bertrand	Cournot	Bertrand
3	Candy & Soda	Cournot	Cournot	Cournot
4	Beer & Liquor	Cournot	Bertrand	Bertrand
5	Tobacco Products	Bertrand	Bertrand	Bertrand
6	Recreation	Bertrand	Bertrand	Bertrand
7	Entertainment	Bertrand	Cournot	Cournot
8	Printing and Publishing	Cournot	Cournot	Cournot
9	Consumer Goods	Bertrand	Cournot	Bertrand
10	Apparel	Bertrand	Bertrand	Cournot
11	Healthcare	Cournot	Cournot	Cournot
12	Medical Equipment	Bertrand	Bertrand	Cournot
13	Pharmaceutical Products	Bertrand	Bertrand	Bertrand
14	Chemicals	Bertrand	Cournot	Cournot
15	Rubber and Plastic Products	Cournot	Cournot	Bertrand
16	Textiles	Cournot	Cournot	Cournot
17	Construction Materials	Bertrand	Cournot	Cournot
18	Construction	Bertrand	Bertrand	Cournot
19	Steel Works Etc	Bertrand	Cournot	Cournot
20	Fabricated Products	Cournot	Bertrand	Cournot
21	Machinery	Bertrand	Bertrand	Cournot
22	Electrical Equipment	Bertrand	Cournot	Bertrand
23	Automobiles and Trucks	Bertrand	Bertrand	Cournot
24	Aircraft	Cournot	Cournot	Cournot
25	Shipbuilding, Railroad Equipment	Bertrand	Bertrand	Cournot
26	Defense	Cournot	Cournot	Cournot
27	Precious Metals	Cournot	Cournot	Cournot
28	Non-Metallic and Industrial Metal Mining	Cournot	Cournot	Cournot
29	Coal	Cournot	Cournot	Cournot
30	Petroleum and Natural Gas	Cournot	Cournot	Cournot
31	Utilities	Cournot	Cournot	Cournot
32	Communication	Bertrand	Cournot	Cournot
33	Personal Services	Bertrand	Bertrand	Bertrand
34	Business Services	Cournot	Bertrand	Cournot
35	Computers	Bertrand	Cournot	Bertrand
36	Electronic Equipment	Bertrand	Bertrand	Cournot
37	Measuring and Control Equipment	Cournot	Cournot	Bertrand
38	Business Supplies	Cournot	Cournot	Cournot
39	Shipping Containers	Cournot	Cournot	Cournot
40	Transportation	Cournot	Bertrand	Cournot
41	Wholesale	Cournot	Cournot	Cournot
42	Retail	Bertrand	Cournot	Cournot
43	Restaraunts, Hotels, Motels	Bertrand	Bertrand	Cournot
44	Banking	Cournot	Bertrand	Cournot
45	Insurance	Cournot	Bertrand	Cournot
46	Real Estate	Cournot	Bertrand	Cournot
47	Trading	Cournot	Cournot	Cournot
48	Almost Nothing	Cournot	Cournot	Cournot

Table 3: Revenue-based Pay and Influence in Cournot industries, Pre vs. Post CD&A

This table presents descriptive evidence on the association between influence and the use of revenue-based incentives in CEO contracts, before and after the CD&A. The sample is firms in Cournot industries from the intersection of Compustat and Incentive Lab, over the period of 2002-2009. Each specification presents results from the following regression specification:

$$RBonus_{i,t} = \beta_1 Post_{i,t} \times log(Share_i) + \beta_2 Post_{i,t} + u_j + \mu_i + \tau_t + \varepsilon_{i,j,t},$$

where *Post* is an indicator variable, taking a value of one if a firm has adopted the CD&A, *Share* is a firm's pre-CD&A average market share (defined at the 4-digit SIC level), and u, μ and τ are industry, firm and year fixed effects. (The main effect of *Share* is excluded, because it is subsumed by the firm fixed effects.) The dependent variable, $RBonus_{i,t}$, is an indicator variable equal to one if the CEO of firm i in year t is given absolute performance incentives tied to revenue. Specifications vary only with respect to the measure of the mode of competition. In Specification 1 (2) [3] I define Cournot based on the Kedia (Prod. Flex.) [Homog.] measure. Standard errors are clustered by industry-year. Throughout the manuscript, all inferences are two-tailed.

		(1)	(2)	(3)
VARIABLES	Prediction	R. Bonus	R. Bonus	R. Bonus
log(Share)×Post	+	0.036***	0.024**	0.025***
		(0.009)	(0.010)	(0.007)
Post	+	0.095**	0.117***	0.084**
		(0.040)	(0.045)	(0.036)
Sample Restrictions:				
Cournot or Bertrand		Cournot	Cournot	Cournot
Measure for Mode of Comp.		Kedia	Prod. Flex.	Homog.
Fixed Effects:				
Industry		Yes	Yes	Yes
Firm		Yes	Yes	Yes
Year		Yes	Yes	Yes
Observations		2,165	2,084	2,916
R-squared		0.709	0.696	0.729

*** p<0.01, ** p<0.05, * p<0.1

Table 4: Benchmark Against Late Adopters

This table provides evidence that heterogeneity in CD&A adoption dates explains variation in revenue-based pay trends. In specifications 1-3 (4-6) the sample is firms in Cournot (Bertrand) industries. Specifications 1 and 4 (2 and 5) [3 and 6] use the Kedia (Prod. Flex.) [Homog.] measure for the mode of competition. In all specifications, the sample is restricted to include only firms of above-median influence, as captured by pre-CD&A market shares (defined at the 4-digit SIC level). The estimating equation is:

$$RBonus_{i,t} = \beta Fiscal2006_t \times EarlyAdopter_i + u_j + \mu_i + \tau_t + \varepsilon_{i,j,t},$$

where $Fiscal2006$ is an indicator variable, equal to one in fiscal year 2006 and $EarlyAdopter$ is an indicator variable equal to one for December year-end firms, and u, μ and τ are industry, firm and year fixed effects. The dependent variable, $RBonus_{i,t}$, is an indicator variable equal to one if the CEO of firm i in year t is given absolute performance incentives tied to revenue. Industry, firm and year fixed effects are included in every specification. Standard errors are clustered by industry-year. Throughout the manuscript, all inferences are two-tailed.

VARIABLES	Prediction	R. Bonus	R. Bonus	R. Bonus	R. Bonus	R. Bonus	R. Bonus
Fiscal 2006×Late Adopter	+/0	0.132***	0.110**	0.102**	0.023	0.040	−0.003
		(0.048)	(0.053)	(0.044)	(0.062)	(0.054)	(0.075)
Sample Restrictions:							
Cournot or Bertrand		Cournot	Cournot	Cournot	Bertrand	Bertrand	Bertrand
Only Influential		Yes	Yes	Yes	Yes	Yes	Yes
Measure for Mode of Comp.		Kedia	Prod. Flex.	Homog.	Kedia	Prod. Flex.	Homog.
Fixed Effects							
Industry		Yes	Yes	Yes	Yes	Yes	Yes
Firm		Yes	Yes	Yes	Yes	Yes	Yes
Year		Yes	Yes	Yes	Yes	Yes	Yes
Observations		1,060	1,100	1,475	961	924	545
R-squared		0.695	0.719	0.733	0.758	0.736	0.719

*** p<0.01, ** p<0.05, * p<0.1

Table 5: Benchmark Against Bertrand Industries

This table presents evidence on the differential effects of the CD&A on the use of revenue-based pay, across substitute and Bertrand industries. The estimating equation is:

$$RBonus_{i,t} = \beta_1 log(Share_i) \times Post_{i,t} \times Cournot_j + \beta_2 Post_{i,t} \times Cournot_j + \beta_3 log(Share_i) \times Post_{i,t} + \beta_4 log(Share_i) \times Cournot_j + \beta_5 Post_{i,t} + u_j + \mu_i + \tau_t + \varepsilon_{i,j,t},$$

where *Post* is an indicator variable, taking a value of one if a firm has adopted the CD&A, *Cournot* is an indicator variable, taking a value of one if a firm is estimated to reside in a Cournot industry, *Share* is a firm's pre-CD&A average market share (defined at the 4-digit SIC level), and u, μ and τ are industry, firm and year fixed effects. The dependent variable, $RBonus_{i,t}$, is an indicator variable equal to one if the CEO of firm i in year t is given absolute performance incentives tied to revenue. Specifications differ only with respect to the measure for the mode of competition. Specification 1 (2) [3] uses the Kedia (Prod. Flex.) [Homog.] measure. Industry, firm and year fixed effects are included in every specification. Standard errors are clustered by industry-year. Throughout the manuscript, all inferences are two-tailed.

		(1)	(2)	(3)
VARIABLES	Prediction	R. Bonus	R. Bonus	R. Bonus
log(Share)×Post×Cournot	+	0.054***	0.031**	0.047***
		(0.013)	(0.013)	(0.014)
Post×Cournot		0.117***	0.113***	0.062
		(0.043)	(0.043)	(0.050)
log(Share)×Post		−0.018*	−0.007	−0.021*
		(0.010)	(0.009)	(0.012)
log(Share)×Cournot		0.016	−0.370***	−0.035
		(0.083)	(0.113)	(0.098)
Post		−0.009	−0.005	0.014
		(0.042)	(0.040)	(0.051)
Sample Restrictions:				
Cournot or Bertrand?		Both	Both	Both
Measure for Mode of Comp.		Kedia	Prod. Flex.	Homog.
Fixed Effects:				
Industry		Yes	Yes	Yes
Firm		Yes	Yes	Yes
Year		Yes	Yes	Yes
Observations		4,156	4,156	4,156
R-squared		0.731	0.730	0.731

*** p<0.01, ** p<0.05, * p<0.1

Table 6: Other Common Metrics (Falsification Analysis)

This table presents evidence on the non-effect of the CD&A on the usage of two of the most commonly used performance metrics: "Earnings" and "Cashflow." Panel A replicates Specifications 1-3 of Table 4, except that the dependent variable is altered. Each element represents the coefficient (and associated standard error) on Fiscal 2006×Early Adopter. Panel B replicates Table 5, except that the dependent variable is altered. Each element represents the coefficient (and associated standard error) on log(Share)×Post×Cournot. Across both panels, the top row uses for an indicator for the use of revenue-based pay in the CEO's pay package (exactly replicating Tables 4 and 5; these results are shown for easy comparison). The middle (bottom) row replaces the dependent variable with an indicator for the use of "earnings"-based pay ("cashflow"-based pay) in the CEO's pay package. In each row, results are presented for all three mode of competition measures. Specification 1 (2) [3] uses the Kedia (Prod. Flex.) [Homog.] measure. Industry, firm and year fixed effects are included in every specification. Standard errors are clustered by industry-year. Throughout the manuscript, all inferences are two-tailed.

Panel A: Benchmark Against Late Adopters

		(1)	(2)	(3)
	Prediction	Kedia	Prod. Flex.	Homog.
Revenue	+	0.132***	0.110**	0.102**
		(0.048)	(0.053)	(0.044)
Earnings	0	0.030	0.001	0.048
		(0.042)	(0.049)	(0.036)
Cashflow	0	0.008	0.046	0.002
		(0.049)	(0.046)	(0.038)

*** p<0.01, ** p<0.05, * p<0.1

Panel B: Benchmark Against Bertrand Industries

		(1)	(2)	(3)
	Prediction	Kedia	Prod. Flex.	Homog.
Revenue	+	0.054***	0.031**	0.047***
		(0.013)	(0.013)	(0.014)
Earnings	0	−0.017	−0.000	−0.006
		(0.014)	(0.014)	(0.016)
Cashflow	0	−0.011	0.003	−0.004
		(0.010)	(0.011)	(0.010)

*** p<0.01, ** p<0.05, * p<0.1

Table 7: Risk Sharing (Falsification Analysis)

This table presents evidence of the non-effect of the CD&A on risk sharing. Panels A and B replicate Specifications 1-3 of Table 4 and Table 5, except that the dependent variable is replaced by *RiskShare*. For brevity, coefficients (and associated standard errors) are reported only for the variables of interest. In Panel A, the variable of interest is $Fiscal2006 \times EarlyAdopter$. In Panel B, the variable of interest is $log(Share) \times Post \times Cournot$. Across both panels, results are presented for all three mode of competition measures. Specification 1 (2) [3] uses the Kedia (Prod. Flex.) [Homog.] measure. Industry, firm and year fixed effects are included in every specification. Standard errors are clustered by industry-year. Throughout the manuscript, all inferences are two-tailed.

Panel A: Benchmark Against Late Adopters

		(1)	(2)	(3)
VARIABLES	Prediction	RiskShare	RiskShare	RiskShare
Fiscal 2006×Late Adopter	0	0.025	0.032	0.015
		(0.033)	(0.033)	(0.027)
Sample Restrictions:				
Cournot or Bertrand		Cournot	Cournot	Cournot
Only Influential		Yes	Yes	Yes
Measure for Mode of Comp.		Kedia	Prod. Flex.	Homog.
Fixed Effects				
Industry		Yes	Yes	Yes
Firm		Yes	Yes	Yes
Year		Yes	Yes	Yes
Observations		941	968	1,322
R-squared		0.401	0.471	0.448

*** p<0.01, ** p<0.05, * p<0.1

Panel B: Benchmark Against Bertrand Industries

		(1)	(2)	(3)
VARIABLES	Prediction	RiskShare	RiskShare	RiskShare
log(Share)×Post×Cournot	0	−0.010	−0.010	0.004
		(0.008)	(0.009)	(0.009)
Sample Restrictions:				
Cournot or Bertrand?		Both	Both	Both
Measure for Mode of Comp.		Kedia	Prod. Flex.	Homog.
Fixed Effects:				
Industry		Yes	Yes	Yes
Firm		Yes	Yes	Yes
Year		Yes	Yes	Yes
Observations		3,360	3,360	3,360
R-squared		0.498	0.497	0.500

*** p<0.01, ** p<0.05, * p<0.1

Table 8: Within-Industry-Year Analysis (Synthetic Parallel Trends)

This table replicates the analyses in Table 5, but replaces industry and year fixed effects with industry-year and industry-post fixed effects, and uses alternative measures of within-industry-year influence: total assets, and total production costs. In Specification 1 (2) [3], I use pre-CD&A average market share (total assets) [total production costs] to measure scale. Standard errors are clustered by industry-year. Throughout the manuscript, all inferences are two-tailed.

		(1)	(2)	(3)
VARIABLES	Prediction	R. Bonus	R. Bonus	R. Bonus
log(Scale)×Post×Subs	+	0.040**	0.053***	0.054***
		(0.016)	(0.020)	(0.018)
log(Scale)×Post		−0.032***	−0.030**	−0.030**
		(0.011)	(0.015)	(0.014)
log(Scale)×Subs		0.049	0.017	0.054
		(0.095)	(0.083)	(0.083)
Sample Restrictions:				
Cournot or Bertrand?		Both	Both	Both
Measure for Mode of Comp.		Kedia	Kedia	Kedia
Measure of Scale		Mkt. Share	Total Assets	Prod. Costs
Fixed Effects:				
Industry-Year		Yes	Yes	Yes
Industry-Post		Yes	Yes	Yes
Firm		Yes	Yes	Yes
Observations		4,034	3,885	3,610
R-squared		0.755	0.754	0.757

*** p<0.01, ** p<0.05, * p<0.1

Table 9: Revenue-Based Pay and Production

This table presents supporting evidence for Presupposition 1. The estimating equation is:

$$log(Costs_{i,t}) = \beta_1 RBonus_{i,t} + \beta_2 RBonus_{i,t} \times Cournot_j + \beta_3 RBonus_{i,t} \times Post_{i,t} + \beta_4 Post_{i,t} + \mu_i + \theta_{j,t} + \varepsilon_{i,j,t},$$

where *RBonus* is an indicator variable, equal to one if the CEO is given performance objectives tied to revenue, *Post* is an indicator variable, taking a value of one if a firm has adopted the CD&A, *Cournot* is an indicator variable, taking a value of one if a firm is estimated to reside in a Cournot industry (as coded by the Kedia measure), and μ and θ are firm and SIC-year fixed effects. The dependent variable, $log(Costs)$, is equal to the natural logarithm of costs of good sold divided by total assets. Standard errors are clustered by industry-year. Throughout the manuscript, all inferences are two-tailed.

		(1)	(2)	(3)	(4)
VARIABLES	Prediction	log(Costs)	log(Costs)	log(Costs)	log(Costs)
RBonus	+	0.038**	0.050*	0.057*	0.050
		(0.018)	(0.026)	(0.035)	(0.034)
Post				0.005	
				(0.037)	
RBonus×Post				−0.013	
				(0.035)	
RBonus×Cournot					0.000
					(0.051)
Sample Restrictions:					
Cournot or Bertrand?		Both	Both	Both	Both
Measure for Mode of Comp.		Kedia	Kedia	Kedia	Kedia
Fixed Effects:					
Firm		Yes	Yes	Yes	Yes
SIC-Year		No	Yes	Yes	Yes
Observations		3,503	2,593	2,593	2,593
R-squared		0.954	0.973	0.973	0.973

*** p<0.01, ** p<0.05, * p<0.1

Table 10: Rivals' Revenue-Based Pay and Own-Firm Production

This table presents supporting evidence for Presupposition 2. The estimating equations are:

$$log(Costs_{i,t}) = \beta_1 RivalsRBonus_{i,t} \times Cournot_j + \beta_2 RivalsRBonus_{i,t} + u_j + \mu_i + \tau_t + \varepsilon_{i,j,t},$$

for specifications 1 and 2, and

$$log(Costs_{i,t}) = \beta_1 RivalsDiscRBonus_{i,t} \times Cournot_j + \beta_2 RivalsDiscRBonus_{i,t} + u_j + \mu_i + \tau_t + \varepsilon_{i,j,t},$$

for specifications 3 and 4, where *RivalsRBonus* (*RivalsDiscRBonus*) is the influence-weighted average of *RBonus* (*RBonus* × *Post*) among a firm's 4-digit SIC rivals, *Cournot* is an indicator variable equal to one for Cournot industries (as coded by the Kedia measure), and u, μ and τ are SIC, firm and year fixed effects. The dependent variable, $log(Costs)$, is equal to the natural logarithm of costs of good sold divided by total assets. Standard errors are clustered by industry-year. Throughout the manuscript, all inferences are two-tailed.

		(1)	(2)	(3)	(4)
VARIABLES	Prediction	log(Costs)	log(Costs)	log(Costs)	log(Costs)
RivalsRBonus×Cournot		−0.078	−0.038		
		(0.143)	(0.130)		
RivalsRBonus		−0.110	−0.123		
		(0.104)	(0.090)		
RivalsDiscRBonus×Cournot	–			−0.408***	−0.319**
				(0.149)	(0.150)
RivalsDiscRBonus				0.274***	0.083
				(0.098)	(0.109)
Cournot		−0.083		−0.079	
		(0.084)		(0.085)	
Sample Restrictions:					
Cournot or Bertrand?		Both	Both	Both	Both
Measure for Mode of Comp.		Kedia	Kedia	Kedia	Kedia
Fixed Effects:					
Firm		Yes	Yes	Yes	Yes
SIC		No	Yes	No	Yes
Year		No	Yes	No	Yes
Observations		2,671	2,667	2,671	2,667
R-squared		0.955	0.959	0.955	0.959

*** p<0.01, ** p<0.05, * p<0.1