Do managers tacitly collude to withhold industry-wide bad news?

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That managers would choose to withhold firm-specific bad news is not only intuitive, but supported by theory, observed disclosure patterns, and survey responses. When the bad news is industry-wide, however, explaining withholding as a sustainable equilibrium is more complicated. If any one firm chooses to disclose, the news effectively becomes public, creating incentives for other firms to disclose. Withholding is only sustainable if all firms cooperate ("tacitly collude"), which depends on their own incentives, and their conjectures about the incentives of other firms in the industry to cooperate. We document cases of increased intra-industry obfuscation in the annual 10-K, controlling for changes in fundamentals, consistent with tacit collusion to hide news. The identified episodes are followed by abnormally poor industry level accounting and market performance two to three years out, suggesting that our episodes represent periods in which firms in the industry withheld bad news that eventually materialized. Tacit collusion is more likely in industries with more significant equity incentives and more concentrated industries, and less likely in industries in which observable/public macro-economic data relevant to firm valuation is available. The results have implications for understanding when market forces are sufficient to generate voluntary disclosure of industry-wide news.

First draft: February 2013

This draft: May 2013

Preliminary and incomplete

The authors thank Paul Fischer, Mirko Heinle, and audiences at Duke and MIT for helpful comments. The authors gratefully acknowledge the financial support of the University of Chicago Booth School of Business (Jonathan Rogers and Sarah Zechman) and the Neubauer family (Sarah Zechman).

1. Introduction

The idea that firms would withhold bad news is intuitive, and theoretical models predict withholding when traders are uncertain about whether the manager has received a signal (e.g., Dye, 1985; Jung and Kwon, 1988; Shin, 2003 and 2006).[1] Firms with relatively adverse news are able to hide in the pool of firms that have no news. But these models assume that firms' adverse private signals are uncorrelated. The withholding prediction in these models does not necessarily translate into a prediction that firms will withhold *industry-wide* bad news, which is by definition correlated across firms in an industry. If at least one firm in the industry receives a firm-specific valuation signal that is higher than its current stock price, despite the adverse industry-wide component, that firm will disclose, and the news effectively becomes public. The uncertainty about news arrival, which generates the partial disclosure equilibrium, disappears and adverse selection should lead to the classic unraveling "full disclosure" equilibrium result (Grossman, 1981; Milgrom, 1981). One might argue that firms could still withhold the news, hoping other firms do not disclose it. If another firm does disclose, the withholding firm simply responds with additional disclosure. However, withholding industry-wide bad news when another firm in the industry discloses it first imposes costs on the second-mover, such as increased litigation or reputational costs, that could decrease firms' willingness to follow this strategy. Ultimately, in the presence of second-mover costs, unless firms *know* that every other firm will withhold, disclosure is the best choice.[2]

We investigate whether and, if so, when firms withhold industry-wide bad news. The manager receives a valuation signal that contains an industry-wide component. When making her disclosure decision regarding this signal, the manager anticipates that she will receive revised signals in subsequent periods. Thus, adverse news, if withheld, may never materialize. The managers' disclosure decisions depend on their conjectures about the probability of new (and improved) subsequent signals of industry-wide news, the impact of the industry-wide news on other firms' valuations, the benefits firms would expect to receive from withholding adverse news (e.g., an elevated stock price, reduced borrowing costs), the expected duration of the period that the news remains hidden, the costs of withholding, and the extent to which the conjectures about each of

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[1] See empirical evidence that firms withhold bad news in Kothari, Shu, and Wysocki (2009) and survey evidence in Graham, Harvey and Rajgopal (2005). See Sletten (2012) and Tse and Tucker (2010) for evidence of clustering in bad news announcements, which theoretical models predict when firms withhold bad news (Dye and Sridhar, 1995; Acharya et al., 2011).

[2] Dye and Sridhar (1995) explain that if firms know that another firm will disclose industry-wide news, then it must do so also because of the adverse selection arguments. The firm faces a nontrivial disclosure decision… "only when it knows that no other firm intends to disclose…". p. 166.

these elements in firms' disclosure decisions are common knowledge. Managers' conjectures can depend on relatively soft factors such as whether there is a focal point to shape beliefs, and whether firms have opportunities to assess the beliefs of other firms, perhaps through frequent interactions or established relationships that create trust. Because the withholding equilibrium requires a reliance on conjectures, we refer to the cooperative behavior that leads to withholding of industry-wide bad news as "tacit collusion."[3]

Understanding whether and when firms withhold industry-wide bad news has important implications for debates about mandating disclosures. Proponents of mandated disclosure argue that firms left to their own devices will withhold private information, in particular adverse news. Opponents argue that disclosure mandates impose unnecessary costs on firms because market forces are sufficient to encourage voluntary disclosure (Easterbrook and Fischel, 1984). Capital market price pressure due to adverse selection, described previously, and the threat of litigation both encourage disclosure. Our analysis brings real data to the question of when these and other market forces are sufficient to encourage voluntary disclosure of industry-wide bad news, which informs the debate on the necessity for mandated disclosures.

In the empirical analysis, we operationalize withholding of bad news by increased industry-level opacity or obfuscation. Extant studies show that termination of previously disclosed information is costly (e.g., Chen, Matsumoto, and Rajgopal, 2011), and Reg. S-X requires firms to discuss various topics, but the content is discretionary. As a result, we expect firms will withhold industry-wide bad news by obfuscating the discussion in their reports rather than excluding it. Our measure of obfuscation is a decrease in the readability of disclosures. Li (2008) establishes the FOG score as a measure of readability, which he uses as a proxy for management obfuscation, and documents an association between firms' FOG scores and both future performance and performance persistence.

We first document periods in which firms within an industry collectively increase disclosure obfuscation (i.e., FOG score). We identify "collusion episodes" for a given industry as a year with a significant increase in the intra-industry average level of annual report FOG scores after controlling for fundamental firm characteristics that prior literature has shown to be associated with FOG scores (Li, 2008). Hence, the measured increase attempts to isolate the discretionary increase in

[3] As described in Ivaldi et al. (2003), footnote 2: ' "Tacit collusion" need not involve any "collusion" in the legal sense, and in particular need involve no communication between the parties. It is referred to as tacit collusion only because the outcome … may well resemble that of explicit collusion or even of an official cartel. A better term from a legal perspective might be "tacit coordination".'

industry-level opacity. The number of industry-year collusion episodes is small. Across the Fama-French 49 industry groups over the 15 year period from 1994 to 2008, we find between 11 and 18 collusion episodes (depending on the sample) in which the increase in intra-industry disclosure opacity suggests tacit collusion.

To provide some validation of these collusion episodes, we conduct analyses that assess whether our episodes can empirically distinguish industries that received and withheld adverse news. If our measure is capturing the withholding of bad news, we predict that the collusion episodes will be followed by poor subsequent performance, on average, relative to the industry-years that are not identified as collusion episodes. We show that the episodes we identify are followed by more performance-related delistings in the year following the episode. The collusion episodes are also followed by lower sales growth, greater write-offs and more negative special items two years after the collusion episode, and greater write-offs and goodwill impairments in the third year. Adjusted returns are significantly more negative for the colluders than for the non-colluders starting in the second and/or third year (depending on the specification) following the collusion episode. These patterns are consistent with the collusion episodes representing periods in which firms in the industry withheld bad news that eventually materialized. Although we do not have specific predictions about the timing of the subsequent poor performance, the fact that the poor performance comes two to three years after the disclosure obfuscation is sensible because the duration of the withholding period is predicted to be positively related to the likelihood of withholding. If firms had rationally anticipated that the news would be made public quickly, withholding should not have been a dominant equilibrium.

The main analysis in the paper investigates the industry characteristics associated with the identified collusion episodes. Three main results emerge. First, industries in which the news is likely to be public or become public quickly are less likely to collude. We identify industries with this characteristic based on the incremental explanatory power of observable/public macro-economic signals (i.e., short-term interest rates, default spreads, term spreads, foreign exchange rates, producer price index, and the Fama-French SMB and HML factors) for returns. When news is public, or will become public in the near future, all firms in the industry have little incentive to withhold and we observe less collusion.

Second, when the proportion of firms in the industry that trade on the major exchanges is high relative to the proportion filing annual reports, the industry is more likely to experience a collusion episode. We provide two interpretations of this finding. One interpretation is that firms in industries with a higher proportion of publicly traded firms have greater equity incentives to withhold adverse news to maintain an elevated stock price, and this would be common knowledge, making a withholding equilibrium more sustainable. A second interpretation is that firms in industries with a lower proportion of publicly traded firms (i.e., firms file annual reports despite not having publicly traded equity) are subject to significant disclosure regulation, perhaps from regulators or other users that demand financial statements. Assuming mandated disclosure is common knowledge, firms in such industries will conjecture that other firms will disclose, making a withholding equilibrium less sustainable. Supplemental analysis favors the first interpretation – that greater equity incentives are associated with collusion – but the tests are weak.

Third, more concentrated industries are more likely to collude. Industry concentration could be related to several fundamental industry characteristics that affect collusion likelihood. Greater concentration could increase the probability that firms conjecture that other firms have received a similar negative signal, such that they have individual incentives to withhold. Greater concentration could also be correlated with fewer influential firms in the industry, which provides greater opportunities to form beliefs that the other influential firms will cooperate, perhaps because of explicit collusion or simply greater interpersonal connections, or perhaps because a focal point is more likely available to facilitate cooperation. Greater concentration could also imply more significant existing barriers to entry, which decreases each firm's incentives to disclose bad news relative to industries with low entry barriers. The positive association between industry concentration and collusion, however, suggests that concentration does *not* proxy for the market's ability to infer the arrival of industry-wide news, which would decrease the sustainability of a withholding equilibrium.

We examine several other industry characteristics with mixed or no results. We examine whether a withholding equilibrium is more sustainable when an industry is subject to greater uncertainty about its propensity to experience a large adverse event. We use a measure of implied negative expected return skew from option prices ("tail risk") to identify high-risk industries. We do not find evidence supporting an association between high risk industries and collusion using one method of identifying collusion episodes, but we find that industries with greater tail risk are more likely to collude in tests that use only single segment firms to identify collusion episodes. A positive association suggests that when firms are more likely to believe that a revised signal will arrive, and that the adverse industry-wide news could reverse (i.e., not materialize), they are more likely to withhold.

We also investigate the relation between collusion and a proxy for litigation risk. While we expect that greater litigation risk, both related to withholding in general and to being the second mover relative to a peer firm, should reduce the likelihood that a withholding equilibrium is sustainable, we do not find empirical support for this conjecture. In fact, we find some evidence that industry-level litigation risk is positively associated with collusion episodes. This result could suggest that our litigation risk proxy captures uncertainty in the industry-level propensity to incur large adverse shocks. Greater uncertainty implies that firms are more likely to believe that future valuation signals may increase (i.e., the news will not be as bad as anticipated or may never materialize), and that this is common knowledge, increasing the sustainability of a withholding equilibrium. Under this interpretation, litigation risk is not a sufficient market force to encourage market disclosure for industry-wide bad news.

The final determinant of collusion episodes that we investigate is within industry heterogeneity in operations, measured as the average value of intra-industry idiosyncratic risk. We find no association with collusion episodes, potentially because of offsetting predicted associations. Industries in which firm returns have a greater idiosyncratic component are less likely to have industry-wide adverse news of a magnitude that will sustain a withholding equilibrium, but intra-industry heterogeneity also makes it less likely that investors will infer industry-wide news from the disclosure by one firm, decreasing the costs to withholding and making a collusive equilibrium more sustainable.

In section 2 we discuss firms' incentives to withhold industry-wide bad news and the conditions for a collusive equilibrium. We describe our measurement of collusion episodes in Section 3, including the subsequent performance tests meant to provide validation of this measure. Section 4 provides the main analysis of the determinants of the collusion episodes.

2. *Hypothesis development*

Section 2.1 outlines our assumptions about the nature of the signal, the timing of the managers' decisions, and the information available to managers and traders. Section 2.2 summarizes the payoffs in a two-player game, each player having two possible disclosure strategies: disclose or withhold. Section 2.3 uses the payoffs to make predictions about the likelihood that firms will tacitly cooperate and withhold industry-wide news.

A unique assumption relative to the related literature (discussed in Section 2.4) is that the manager expects to receive revised signals in subsequent periods. Two related analytical models have multiple periods, but the additional periods only represent additional opportunities to disclose the initial signal; revised signals do not arrive. This assumption is realistic. Graham et al. (2005) report that several surveyed CFOs indicate they delay bad news "…in hopes that the firm's status will improve before the next required information release, perhaps saving the company the need to ever release the bad information…". This statement suggests that firms anticipate the potential for new valuation signals when making disclosure decisions.

2.1 Assumptions about the nature and arrival of news signals and managers' disclosure decisions

At time t = 0, prior to the receipt of private signals, the expected value of a firm *j* is V_j^0. The V_j^0 values of firms in industry *i* have a common industry-wide component and an idiosyncratic component. These values and components are common knowledge to managers and traders.

At time t = 1, with probability < 1, firms receive a private signal about their firm values (V_j^1). If one firm in industry *i* receives a signal, then all firms in the same industry also receive a signal about their own firm-specific values. Like V_j^0, the V_j^1 values have an idiosyncratic component and a common industry-wide component. Examples of common components are demand shocks to products sold by firms in the industry or average credit quality shocks to customers in the industry. The assumption that the signal contains a common intra-industry component accords with intuition about within-industry valuations. Firms within an industry are likely to have relatively homogeneous production and cost functions, thus a single piece of news will affect the valuations of all firms in the same direction.[4]

Firms observe their own firm-specific valuations (V_j^1). Each firm *j* in industry *i* knows that if it receives a private signal, other firms in the industry have received a signal. While firms do not observe other firms' signals, they have expectations about the valuations based on the observation of the industry-wide signal and expectations about the common and idiosyncratic components of firms' valuations.

Traders remain uncertain at time 1 about whether firms in an industry have received a signal. Traders only learn about the news arrival if a firm, any firm, in an industry discloses its valuation. In that case, traders have expectations about the industry-wide implications and infer that other firms

[4] Consistent with the idea that firms' valuations are determined by a common industry wide component, O'Brien (1990) shows that investment analysts tend to specialize in one industry, and Dunn and Nathan (2005) show that analysts who focus on one industry produce better forecasts than analysts focusing on multiple industries.

in the industry also received a signal. The common component of the valuation effectively becomes public upon disclosure by any firm.

If a manager receives an adverse industry-wide signal at time 1, she chooses whether to disclose or withhold it. Firms make disclosure decisions contemporaneously. In the empirical analysis, we examine disclosure opacity in annual 10-K filings. While 10-Ks are not released on the same day, there is a large degree of clustering within calendar time for firms within an industry and it is unlikely that one firm would see the release of a peer firm's report and be able to adjust its own 10-K in response before filing. In this sense, we consider firms' annual 10-K disclosure decisions to be contemporaneous.

If one firm withholds the news from its annual report and another firm in the industry discloses it, effectively making the industry-wide news public, the withholding firm will need to respond with an additional disclosure, for example a press release. We call the firm that discloses the first mover and firms that are forced to respond the second movers.

Traders respond to a disclosure and to silence (i.e., non-disclosure), taking into account the strategic behavior of managers. The stock price of a firm that discloses its valuation will be the disclosed amount (V_j^1). The stock price of a non-disclosing firm will be the expected value conditional on its non-disclosure and the disclosure decisions of other firms.

2.2 Summary of payoffs

Figure 1 summarizes the payoffs to the two possible disclosure strategies at time 1: disclose or withhold.

Figure 1: Payoff matrix

		FIRM 1	
		DISCLOSE	WITHHOLD
FIRM 2	DISCLOSE	(1) Firm 1: $P_1^D \equiv V_1^1 - V_1^0$ Firm 2: $P_2^D \equiv V_2^1 - V_2^0$	(2) Firm 1: $P_1^{2M} \equiv P_1^D - (c^{2M} + c^{WH})$ Firm 2: $P_2^{1M} \equiv P_2^D$
	WITHHOLD	(3) Firm 1: $P_1^{1M} \equiv P_1^D$ Firm 2: $P_2^{2M} \equiv P_2^D - (c^{2M} + c^{WH})$	(4) Firm 1: $P_1^{WH} \equiv f_1(V_1^1 - V_1^0) - c^{WH}$ $\equiv XS\_PRICE_1^{WH} - c^{WH}$ Firm 2: $P_2^{WH} \equiv f_2(V_2^1 - V_2^0) - c^{WH}$ $\equiv XS\_PRICE_2^{WH} - c^{WH}$

If both firms disclose (Box 1 in the upper-left corner), each firm's payoff, denoted P_i^D, is the decrease in its own stock price from the pre-shock level (V_i^0) to the lower post-shock valuation (V_i^1). A critical assumption is that the valuations at time 0 and 1 (V_j^0 and V_j^1) are drawn from different distributions. This assumption, which is consistent with the news being industry-wide, allows for the possibility that $V_j^1 < V_j^0$ for *all* firms in the industry and also for the possibility that firms would conjecture that $V_j^1 < V_j^0$ for *all* other firms, which are necessary conditions for withholding to be a sustainable equilibrium. That is, we are only interested in settings where the posterior distribution of the valuations of the *j* firms in industry *i* (V_j^1) is lower as a result of the news. Said differently, prior to the shock, all firms in an industry were considered to be of one type, with individual firm valuations drawn from a known distribution. The industry-wide bad news worsens the type of all firms in the industry, but the reduction is not observable to market participants.

Box 4 shows the payoffs if both firms withhold the news at time 1, which we denote P_i^{WH}. The payoff is positively affected by the direct benefits that the self-interested manager receives from avoiding the immediate price reduction ($V_i^1 - V_i^0$) that occurs if both firms disclose. The firm-specific benefits are captured by f_i, where $f_i(V_i^1 - V_i^0) \geq P_i^D$.[5] We also refer to these benefits as $XS\_PRICE_i^{WH}$ because they represent the benefits from maintaining an artificially elevated ("excess") stock price. While the loss in value will accrue immediately following disclosure (Box 1), the benefits from withholding will continue for the duration of the period that the news is withheld. The withheld news may be revealed if either firm chooses to disclose in a subsequent period as revised signals arrive. The withheld news may also be revealed in a subsequent period by external parties (e.g., an analyst or the media) who discover it, a public announcement of macro-economic data (e.g., GDP figures that are relevant to firm valuation), or realization of the shock (e.g., sales declines due to an adverse demand shock). The news may never be revealed if the industry-wide bad news is not realized (e.g., a decline in average customer credit quality reverses prior to significant credit losses).

The Box 4 payoffs are negatively affected by an exogenous cost of withholding bad news that accrues to any non-disclosing firm (c^{WH}). The cost will be zero if the shock never materializes and is not discovered. Skinner (1994) suggests that litigation costs are increasing in the duration of withholding because a longer withholding period means a longer class period.[6]

The off-diagonal Boxes (2 and 3) show the payoffs when one firm discloses and the other firm withholds in the contemporaneous disclosure decision. The withholding firm is forced to disclose upon disclosure by the other firm and so becomes the second mover. The valuations of both the first and second movers are adjusted to the lower valuation level (V_i^1) because the news has been revealed. The second mover also absorbs the cost of withholding (c^{WH}) that accrues to any firm that withholds and represents the costs when the news is made public by external forces. In addition, the second mover absorbs an incremental cost of withholding adverse news that accrues only when the revelation is by a peer firm (c^{2M}).[7]

Litigation costs are one example of a second-mover cost. One element of a 10b-5 case is that the defendant omitted a material fact. The disclosure of industry-wide news by a peer firm in

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[5] The payoffs that occur if both firms disclose are also a firm-specific function of the stock price drop ($V_i^1 - V_i^0$), but we leave this function out of Box (1) for simplicity.

[6] This assertion motivates Skinner's prediction that firms will preemptively disclose bad news. Evidence that firms disclose to avoid such costs is mixed. Skinner (1994) suggests that firms disclose bad news early to mitigate the risk, while Francis, Philbrick, and Schipper (1994) do not find evidence that preemptive disclosure mitigates litigation risk. Skinner (1997) then examines a broader sample and finds support consistent with Francis et al. (1994) but suggests that, while issuing forecasts may not deter litigation, it may reduce the costs.

[7] We refer to these incremental costs of withholding as "externality" costs of being the second mover (c^{2M}), consistent with the terminology in Dye (1990), because the decision to disclose by one firm imposes a real externality on a non-disclosing firm.

the industry can provide useful evidence to support this claim. If one firm in the industry discloses, plaintiffs are more likely able to establish that the non-discloser knew the news, or was reckless in not knowing it, and intentionally withheld it, which is otherwise difficult to prove. It may also be easier to establish materiality, which is a subjective evaluation, given that the disclosing firm assessed the news as material. While plaintiffs can bring a 10b-5 case based on a return drop such that litigation risk affects even a disclosing firm, we assume there are incremental costs of litigation for a second mover because the plaintiff has a stronger case (Francis, Philbrick, and Schipper, 1994).

Reputation costs are another example of a second-mover cost. Individuals identified in enforcement actions lose their jobs, face restrictions on future employment, and incur pecuniary costs including fines and valuation losses on shareholdings (Karpoff et al., 2008a). Individuals can also face criminal charges and serve jail time (Karpoff et al., 2008a; Schrand and Zechman, 2012). Karpoff et al. (2008b) document reputational penalties at the firm rather than individual level. They characterize the direct legal costs as fairly minimal, but the loss in firm market value associated with the reputational effects of the misconduct as "huge."[8] We expect that reputational costs are higher for the second mover if markets assess the firm's disclosure behavior relative to its industry peers.

The second-mover costs are critical to our interpretation of observed industry-wide withholding of adverse industry-wide news as tacit collusion. If there are no second-mover costs, we might observe correlated firm decisions to hide adverse news with an industry-wide component simply due to correlated fundamentals that affect the disclosure decision. With second-mover costs, each firm's individual decision is affected by its beliefs about other firms' disclosure decisions, and withholding requires tacit collusion.

2.3 Predictions

We make four predictions about when a withholding equilibrium is more sustainable.

Prediction 1: Sustainability of a withholding equilibrium increases in the surplus from withholding relative to disclosure (P^{WH} - P^{D}), which in turn depends on the following factors:

[8] Per Karpoff et al. (2008b): "For every dollar of inflated value when a firm's books are cooked, firm value decreases by that dollar when the misrepresentation is revealed; in addition, firm value declines $0.36 more due to fines and class-action settlements and $2.71 due to lost reputation." The analysis focuses on the effects of earnings misstatements, so these dollar amounts cannot be translated as penalties for disclosure omissions, but the evidence nonetheless supports the assumption that capital markets impose reputational penalties on firms that do not communicate.

a) Amount of the shock: The greater the raw magnitude of the *industry-wide* component of the bad news, the greater the likelihood that $V_i^1 < V_i^0$ for all firms and that this relation is common knowledge.

b) Uncertainty of the shock: The more uncertain the reduction in valuation, the greater the likelihood that subsequent revised valuation signals will reverse the bad news before it is revealed. Predicting that uncertainty affects the equilibrium is consistent with the notion in Graham et al. (2005) that managers withhold bad news in the hopes that it will never materialize.

c) Expected duration of withholding: The longer the duration of the expected surplus, the greater the benefits to withholding. The duration depends on when the news becomes public. The news may become public because of 1) external factors, for example, regularly scheduled macro-economic forecasts; 2) information acquisition by outside parties such as analysts; or 3) realization of the shock. In addition, firms must also consider that future revised valuation signals can change their own equilibrium disclosure decision or that of other firms. If the firm expects to disclose in a future period, or expects a peer firm to disclose, these endogenous disclosures also decrease the duration of the surplus.

d) Exogenous costs of withholding (c^{WH}): The greater the exogenous costs of withholding bad news, such as litigation costs that accrue to all firms that withhold (not just the incremental costs to the second mover), the less likely firms are to withhold, and the less likely they are to believe other firms will withhold, making a withholding equilibrium less sustainable. We assume the costs are increasing in the magnitude of the industry-wide component of the news and the duration of the withholding period.

e) Equity incentives to withhold: A presumption of the payoff definitions is that the surplus to withholding (P_i^{WH}) is positively related to an elevated stock price ($XS\_PRICE_i^{WH}$). In industries in which more firms have equity incentives, more firms will have individual incentives to withhold and to believe that other firms share these incentives, increasing the likelihood that a withholding equilibrium is sustainable. These benefits vary with the magnitude of the elevation, as noted in (a) above, but are also related to the equity incentives of the manager.

Prediction 2: Sustainability of a withholding equilibrium decreases in the second-mover costs (c^{2M}). Recall that the second-mover costs exist only if the news becomes public due to disclosure by a peer firm. If the news exogenously becomes public, all firms in the industry bear costs of withholding (c^{WH}).

Prediction 3: Sustainability of a withholding equilibrium is more likely when firms conjecture that the surplus (P^{WH} - P^{D}) and the second mover costs (c^{2M}) are common knowledge. Consistent with predictions from the product market collusion literature,[9] we predict that such assessment opportunities are increasing in: *i*) the availability of a focal point;[10] *ii*) the frequency of interactions between the players; and *iii*) other soft issues such as trust among the firms in the industry.

Prediction 4: Sustainability of a withholding equilibrium is more likely when firms conjecture that traders cannot infer industry-wide news from the disclosure of firm-specific news by any one firm in the industry. Any industry characteristic that makes traders infer less about the industry-wide component of the news based on individual firms' disclosures of their valuations will increase the sustainability of a withholding equilibrium.

2.4 Related literature

Three voluntary disclosure studies have analyses that relate to industry-wide news. Dye and Sridhar (DS, 1995) analyze disclosure choice when the timing of the arrival of private signals is correlated, but the signal values are uncorrelated. In the special case of "industry-wide common knowledge," firms also learn whether other firms in the industry have received a signal. As DS note, if any firm "knows" that another firm will disclose, unraveling and a full disclosure equilibrium will occur (p. 166). Withholding is a possible equilibrium, but only if firms "know" that other firms will not disclose. DS do not analyze disclosure choice when firms must form beliefs about other firms' signals as well as conjectures about whether these beliefs are common knowledge.[11]

Acharya, DeMarzo, and Kremer (ADK, 2011) examine disclosure when signal values are correlated. They operationalize this assumption by assuming firm-specific news is correlated with market-wide news, thus their analysis essentially analyzes disclosure decisions when the news contains a market-wide component. If the market-wide news is already public at the time firms receive their private signals, the full disclosure equilibrium results because investors are certain that the firm has received a signal. If the market-wide news is not public, such that investors remain

[9] See relevant discussions in Levenstein and Suslow (2006), Ivaldi et al. (2003), Symeonidis (2003), and Pindyck and Rubinfeld (1989).

[10] Pindyck and Rubinfeld (1989) give the prescient example that LIBOR could serve as a focal point, allowing banks to cooperate in setting rates.

[11] DS primarily consider a case when the timing of the arrival of private signals is correlated, but the signal values are uncorrelated, and firms do not learn whether other firms have received a signal. Our Prediction 4 is related to their comparative static analysis for this case (Theorem 2a) related to the number of firms (n) in the industry. Two forces are at play. As n increases, traders infer less from the disclosure by any one firm about the probability that a non-discloser has received a private signal, but adverse selection motivates more firms to disclose. The second effect always dominates and the threshold is decreasing in n.

uncertain about whether the manager has received a signal, disclosure of bad news is delayed relative to the public information case. When the news is subsequently made public, the release can trigger disclosure by multiple firms that had withheld news.[12] In their model, the subsequent release of the news that makes it public is exogenous. ADK discuss the implications for their equilibrium if the news is made public through the endogenous disclosure by another firm, which is the primary focus of our analysis. However, they only conjecture that the results would hold, stating: "The construction of the equilibrium presents a significant computational challenge." Although ADK's setting has multiple periods, the periods after the signal arrives only provide another opportunity for the manager to disclose. No new signals arrive or are anticipated.

Jorgensen and Kirschenheiter (2012) analyze disclosure of industry-wide news with an exogenous leader and follower firm. The leader firm anticipates the disclosure decision of the follower. Their analysis focuses on how costs of disclosure, not costs of withholding affect the leaders disclosure choice. The follower does cannot receive a revised signal after the leader discloses.

Our analysis is also related to studies on collusion in product markets. The analogy to collusion in product markets is useful, but the translation is admittedly less than perfect.[13] The main distinction between our analysis and product market games relates to the determination of the payoffs. In product market games, the amount of the surplus payoffs to cooperation (Box 4) over profits in the prisoners' dilemma equilibrium (Box 1) depends on the players' strategic decisions, as does the short-term benefit to the player that deviates (Boxes 2 and 3). In our setting, strategic interactions do not affect the payoff to cooperation, nor do they affect the second-mover costs, which the player that deviates avoids. The expected valuation implications of the industry shock could ultimately depend on how firms react to the shock through their operating, investing, or financing decisions, and each firm's reaction could be related to the anticipated reactions of other firms. The focus of our analysis is on the disclosure of the shock, not on tacit collusion in a firm's

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[12] ADK predicts a clustering of bad news announcements after a public disclosure and DS predicts disclosure waves following the disclosures by other firms. The clustering occurs because multiple firms had withheld bad news until these events, which then trigger disclosure. Sletten (2012) documents increases in management forecasting following stock price declines associated with a restatement by an industry peer, suggesting that firms withheld bad news, but disclose it when the news becomes favorable relative to the new lower stock price. Tucker and Tse (2010) provide evidence on clustering in warnings about earnings shortfalls. These studies are consistent with firms withholding firm-specific private information, but they do not have implications for whether firms withhold industry-wide bad news.

[13] Another difference between our assumed payoff structure and those in models of price or quantity decisions is that tacit collusion in product market decisions typically describes the cooperative equilibrium that arises when firms can credibly retaliate against firms that deviate (Bertomeu and Liang, 2008). The threat of retaliation does not exist in the disclosure setting.

operating, financing, or investing decisions as a reaction to the shock.[14] As such, we do not formally model any specific strategic reactions, though this idea is incorporated into our setting through the assumption that firms anticipate receiving new signals in subsequent periods and they anticipate that other firms will anticipate this as well. Conjectures about the new signals can embed the firm's expectations about other firms' strategic reactions to the shock. In the empirical analysis, we consider fundamental industry characteristics that we expect will affect players' conjectures about strategic reactions and the extent to which this is common knowledge. For example, we predict that firms in more homogeneous industries are more likely to have similar payoffs in the case of cooperation relative to the prisoners' dilemma equilibrium, and to conjecture that other firms have the information and believe each firm does (and so on). This prediction mirrors those about heterogeneity in the product market literature, which affects firms' conjectures about the payoffs to cooperation assuming certain strategic reaction functions (Ivaldi et al., 2003).

A withholding equilibrium is similar to a product market "tacit collusion" equilibrium on several dimensions. An individual firm's decision to withhold is based not only on its own signal but also on its beliefs about other firms' signals and higher order beliefs. Firms' conjectures can depend on soft factors like whether there is a focal point that can sustain a cooperative withholding equilibrium. The reliance on conjectures makes these equilibria inherently unstable, as evidenced by the numerous empirical studies on when tacit collusion or cartel formation occurs in product markets and why cartels dissolve.[15]

3. Measuring annual report opacity and collusion episodes

Section 3.1 describes the analysis to identify collusion episodes. Section 3.2 reports subsequent accounting and return performance for the identified episodes compared to non-collusion industry years. The subsequent performance analysis shows that the identified episodes can empirically distinguish industries that realize poor performance following the collusion year and hence provides some validation that the ex ante identified episodes are associated with yet unrealized bad news.

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[14] Rajan (1994) is an example of a model of firms' choices about disclosures of industry-wide news in the context of a specific strategic decision. The disclosure choice is a bank's decision about recording credit reserves, which serve as a signal to other banks that make lending decisions about the overall level of credit quality in the industry. The analysis focuses on the implications of the recorded reserves for strategic lending and other decisions, which affect payoffs and business strategy, and the overall availability of credit.

[15] See Levenstein and Suslow (2006) for a review.

3.1 Detecting periods when industry-wide bad news is withheld

We define an industry-year collusion episode as an unexplained increase in the within-industry average annual report opacity. The underlying assumption of this measure is that firms increase opacity when they are attempting to hide information. Our model of opacity controls for industry-wide shocks to (disclosed) fundamentals that could also generate an observed increase in intra-industry FOG scores.[16] Thus, we interpret the measured increase in opacity as discretionary, and interpret the increase as intentional obfuscation.

We measure annual report opacity using the FOG index (Li, 2008). We determine the discretionary component using a model that controls for firm-level determinants of FOG as identified in Li (2008). The control variables include: the log of market value of equity, the market to book ratio, special items scaled by total assets, return volatility, the number of non-missing data items in Compustat as a measure of complexity, firm age, an indicator for Delaware incorporation, the log of the number of geographic segments plus one, and the log of the number of business segments plus one. (See variable definitions in Appendix A.) Including the control variables in the regression mitigates the concern that observed changes in the FOG score are due to changes in fundamentals.

We regress firms' 10-K FOG scores on the control variables by FF49 industry for rolling two year windows over the period 1993 through 2008. We require at least ten observations within the industry for each year included and all variables be specified in the regression. The model includes a year indicator for the later of the two years (YEAR2). An industry-year is considered a "collusion episode" in year 2 if the coefficient estimate on the YEAR2 indicator, controlling for industry fundamentals, is positive and significant (p-value < 0.10). This analysis generates observations of collusion episodes for the years 1994 through 2008.

We estimate the model separately for two samples of firms. The first sample includes all firms in each FF49 industry with available data for the year ("FULL" sample). The second sample excludes firms with multiple business segments per the Compustat Segment datafile ("1SEG" sample). We create the second sample because, ex ante, we expect the parameter estimates from the FOG model estimated with the 1SEG sample to provide better controls for the fundamental industry characteristics that affect FOG, thus providing a cleaner measure of the change in the

[16] Mimicking is another explanation for observing correlated behavior in corporate decisions (e.g., capital structure decisions in Leary and Roberts, 2010). In the context of choosing to make financial statements less transparent, we do not put much weight on the mimicking explanation. While firms may voluntarily mimic other firms' to improve transparency or may be compelled to follow other firms (Jung, 2011), mimicking opacity seems less plausible.

discretionary component of FOG. However, this sample has fewer observations, especially in later years due to SFAS 131, reducing its power to detect significant collusion episodes. The relative power of these samples to detect collusion episodes is ambiguous.[17]

Table 1 reports data on select parameter estimates and model statistics for estimates from a regression of FOG on the control variables, by FF49 industry and year. Industry-year averages are reported for the FULL sample (Panel A) and the 1SEG sample (Panel B). Of the 15 years over which we attempt to estimate the model between 1994 and 2008, we are able to estimate it for 12 years (6 years) per industry on average in the FULL (1SEG) sample. The grand average numbers of firms per industry-year are 63 and 17, respectively. These figures illustrate our concern about the smaller number of observations in the 1SEG sample. The grand average of the 49 industry intercepts is 19.5 in the FULL sample and 19.2 in the 1SEG sample. Although the raw magnitudes are similar, only 16.5% of the intercepts are significant in the FULL sample, versus 30.3% in the 1SEG sample. Thus, the industry dummy captures less of the variation in FOG after controlling for the fundamentals in the FULL sample than in the 1SEG sample, consistent with our expectations, which reduces its power to detect significant collusion episodes.

Table 2 provides a summary of the collusion episodes. We report the number of industries for which we are able to estimate the FOG model as well as the number and names of each industry estimated to be a colluder in a given year. Based on the FULL (1SEG) sample, there are 18 (11) estimated collusion episodes.

3.2 Performance following identified episodes

This section uses a traditional portfolio methodology to investigate the relation between collusion episodes and subsequent performance measures with the intention of providing validation for the episodes we identified. If the collusion episodes indeed represent cases of hiding adverse industry-wide news, we expect to observe poor performance, on average, following collusion episodes compared to non-collusion industry years. We do not make specific predictions about the timing of the subsequent poor performance. Our only expectation is that revelation of bad news in

[17] We also examine a third sample, which is comprised of firms in the FULL sample that cluster in their fiscal year ends. Specifically, the sample consists only of the firms within an industry that have FYEs within a five-month window (the particular 5-month window that maximizes the sample). We do not report the results for this sample given the high degree of overlap with the FULL sample (of the 12 collusion episodes identified in this sample, 11 are in the FULL sample). The degree of clustering of fiscal year ends within the FULL and 1SEG samples is similar. In the FULL sample, the average (median) percentage of firms in an industry-year observation that cluster in their fiscal year ends is 83.9% (84.1%) compared to 83.1% (84.1%) in the 1SEG sample.

the financial statements should be concurrent with or after the reflection of bad news in returns. We do not, however, expect the poor performance to be immediate. The duration of the withholding period is predicted to be positively related to the likelihood of withholding. If firms had rationally anticipated that the news would be made public quickly, withholding should not have been a dominant equilibrium. Our analysis of *ex post* realized performance rather than expected performance could understate the amount of bad news that was hidden if the bad news is either partially mitigated or never realized. Some industries likely hid industry-wide adverse news about possible future outcomes hoping that the bad outcomes would never materialize, consistent with survey responses in Graham et al. (2005).

We analyze subsequent performance-related delistings, accounting performance, and stock market returns for the firms in the colluding industry at discrete intervals over the three years following the identified collusion episode. We also examine subsequent performance for firms that were not in a colluding industry. We exclude firms identified as colluders in year y from the set of non-colluders in years $y+1$ through $y+3$, so that performance metrics for the colluding firms do not confound the $y+1$ and $y+2$ performance metrics for the non-colluding samples in the subsequent period. For example, if an industry is labeled a colluder in 2002, it is excluded from the non-colluder sample in 2003-2005.

The first performance metric we examine is the proportion of firms that delist due to bad performance in periods subsequent to the collusion episodes. We also examine delistings due to M&A because this is the single most frequent source of delistings during our sample window and because firms may use mergers to avoid bankruptcy costs (Jensen and Meckling, 1976).[18] This analysis serves two purposes. First, the delistings are a direct measure of poor performance. Second, delistings could result in a bias in the measurement of the accounting and return performance, which we analyze next. We examine accounting and return performance for all firms in the industry, including firms that were not included in the determination of the collusion episodes due to data availability and we do not require firms to have data for all periods subsequent to the collusion episode. These missing observations could create a bias in our sample that would work against finding subsequent bad news in the collusion industries if the missing observations are the firms with the most egregious bad news.

[18] The delisting codes (from CRSP) we use to identify performance-related delistings are 400 to 500 and 520 to 584 (Shumway, 1997). The delisting codes we use to identify M&A activity are codes 200 to 400.

Table 3 summarizes delistings for both the Compustat data, used in the accounting performance tests, and the CRSP data, used in the return performance tests. The missing observation bias is potentially different in the two datasets. For the firms that were identified as having a performance-related delisting, Panel A reports the percentages of single segment firms that disappear from Compustat in the three years following the 11 collusion episodes identified by the 1SEG sample, and Panel B reports the percentages of single segment firms that disappear from CRSP for month +1, months 2 to 3, months 4 to 6, months 7 to 12, months 13 to 24, and months 25 to 36 after month four (April) of the collusion episode year *y*.

In the sample used to analyze subsequent accounting performance, the colluders show significantly higher levels of both performance-related and M&A related delistings than the non-colluders in the year following the collusion episode (Table 3 Panel A). The differences are not significant in years *y+2* and *y+3*. In the sample used to analyze subsequent return performance, the two groups do not exhibit significantly different performance-related delistings (Panel B). The colluders show some evidence of a lower propensity for M&A-related delistings that could affect the returns data. The missing observations associated with performance-related delistings in the sample used to analyze subsequent accounting performance likely bias against the detection of more negative performance for the colluding industries. Less of a bias is expected in the analysis of subsequent return performance. We adjust the returns as described below, as a sensitivity analysis of the main results.

We first present the analysis of subsequent accounting performance. We analyze four accounting measures of firm performance: 1) sales growth, equal to the change in sales divided by lagged sales, 2) special items, scaled by total assets, 3) write-offs, scaled by total assets, and 4) goodwill impairments, scaled by total assets. We separately regress lead accounting performance in years *y+1*, *y+2*, and *y+3* on an indicator for collusion, control variables measured as of the collusion year, and indicators for year and industry. Our unit of observation is at the industry-year level. The control variables include *MVE*, *MTB*, firm age, and return volatility, as well as the concurrent measure of the respective accounting performance measure. All continuous variables are measured as the mean value for the observations in that industry and year. Standard errors are clustered at the year-level.

Table 4 presents the results for the coefficient on the collusion indicator and its significance for each of the four performance variables across the three lead performance periods for the 11 collusion episodes identified in the 1SEG sample. All but one of the collusion coefficients has a negative sign, consistent with collusion episodes being followed by worse accounting performance in future periods than non-collusion industry-years. In the second year after the initial collusion episode, colluders have significantly lower sales growth, approximately 11% lower than the non-colluding firms. They also have more negative special items and write-offs. Evaluating the effect at the average total assets for the firms with a single business segment of approximately $1.6 billion, the coefficient on negative special items translates to just under $20 million in additional negative special items for the average colluding firm two years out. In fact, the colluding firms have more negative write-offs in all three subsequent years. Finally, we find significantly more goodwill impairments in the third year following the collusion episode.

We next present the analysis of subsequent return performance. We measure return performance as the cumulative abnormal size-decile adjusted returns (CARs). The subsequent returns for an industry-year are CARs in months 1-, 3-, 6-, 12-, 24- and 36 after month four (April) following collusion episode year end. We estimate portfolio returns for several sets of firms. The first set includes only single business segment firms and excludes new entrants to the industry, defined as firms with returns available as of month four following the collusion episode year (*y*) but with missing returns as of December year *y*. Assuming these firms were privy to the news when they chose to enter, they are likely different from the existing firms, reducing the likelihood that the industry shock will affect them similarly.[19] The second set adds multi-segment firms to the first sample, which significantly increases the sample size in our tests, but at the cost of less specific returns related to the industry with the bad news. The third and fourth sets are similar to the first and second sets but we adjust for performance-related delistings. We replace observations missing due to performance-related delistings with the 5th percentile CAR for the same FF49 industry in year *y*. We assume delistings for other reasons (e.g., changes in exchanges) do not create a bias in our portfolio returns.[20]

Table 5 reports the mean firm-level CARs for the collusion and non-collusion portfolios for the 11 collusion episodes that were identified using the 1SEG sample. Panel A presents average CARs for the single segment firms, excluding new entrants and without adjusting for delistings. For the colluding portfolio, there are 424 individual firms with data available to compute CARs in the first month following the collusion episodes aggregated across the 11 collusion episodes; this

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[19] We also create a set of firms that includes single segment new entrants to the industry. All results (untabulated) are qualitatively the same, suggesting that new entrants and existing firms were similarly affected by the industry-wide news.
[20] In untabulated analysis, we also replace the missing returns due to M&A-related delistings with the industry average of CARs for firms in the industry for that period. The results are similar to those in Panels A and B.

number declines to 270 by month +36. The CARs turn negative in the second year following the episodes, and more negative by the third year. Panel B, which includes multi-segment firms, has substantially more observations in both colluder and non-colluder portfolios, which should increase power but could also increase noise. Nonetheless, the results show the same basic pattern as in Panel A with higher returns for the colluders through month +12 followed by more negative cumulative abnormal returns as of month +24 and month +36.

In Panels C and D we present results after adjusting for performance-related delistings. The replacement of missing values with the 5th percentile described above is made for both the colluder and non-colluder portfolios. The number of firms with available data in each subsequent period still declines as in Panels A and B because of missing data for reasons other than performance-related delistings, but the decreases (and potential selection bias) are not as great. The CARs are lower for both the colluders and non-colluders than those reported in Panels A and B, by construction. The differences between the colluders and non-colluders, however, are similar to those presented in Panels A and B in all periods. In summary, the delistings affect both samples similarly.

Overall, we find evidence of poor subsequent performance in both accounting measures and returns with similar timing, two to three years following the identified episode. Finding consistent results in both performance measures mitigates concerns about spurious results, but more importantly, suggests that the identified collusion episodes distinguish industry-years that have predictable poor performance.

4. Analysis of determinants of collusion episodes

In this section, we report the analysis of the determinants of the collusion episodes identified in Section 3. We estimate the following logit model:[21]

$$\begin{aligned} COLLUSION_{iy} = \alpha_0 + \delta_1 PUBLIC_i + \delta_2 HETERO_{iy} + \delta_3 TAILRISK_i \\ + \delta_4 \%CRSP_{iy} + \delta_5 KS\_LIT_i + \delta_6 CONC_i + \epsilon_{iy} \end{aligned} \quad (1)$$

The dependent variable equals one for the 18 (11) industry-year episodes identified using the FULL (1SEG) sample and equals zero for the remaining non-missing industry-year observations. An industry-year is considered missing if data were not available to estimate the FOG model. As described previously, an industry-year is also considered missing for an industry *j* in years *y+1*

[21] We also estimate the model using a probit specification with consistent results.

through *y+3* if it was defined as a colluding industry in year *y*. This exclusion reduces noise in the "non-colluding" observations, since an industry that colludes in year *y* may continue to hide the news. We use a three year cutoff consistent with the findings in the subsequent performance tests.

4.1 Explanatory variables

Section 2.3 outlined our predictions about industry characteristics that affect the sustainability of a collusive equilibrium. In this section, we describe our proxies for these industry characteristics. Most of the explanatory variables relate to multiple predictions, and some predictions are represented by multiple proxies. At the end of this section, we summarize the mapping of our predictions into the proxy variables. Table 6 presents univariate comparisons of the explanatory variables across collusion and non-collusion industry-year observations.

PUBLIC: Availability of public information. Firms only have incentives to collude if the news is not already public and it is not likely to quickly become public exogenously. As stated in Prediction 1c, the longer the expected duration that the news is withheld, the greater is the expected value of having an artificially elevated stock price, the greater are the economic incentives to withhold, and the more likely it is that all firms will share these beliefs. Our proxy is intended to capture the availability of public information about firms in an industry, which decreases the sustainability of a collusive equilibrium.

We create an industry-level measure of the availability of public information (see Appendix A for details). For each of the FF49 industries, we separately estimate a standard single factor market model and a factor model that includes seven macro-economic risk factors: short-term interest rates, default spreads, term spreads, a foreign exchange factor, a producer price index, and the Fama-French SMB and HML factors. The seven factors represent observable public signals. The incremental power of these observable factors to explain returns for firms in the industry provides a proxy for the degree to which news about the industry is likely to be public. The variable *PUBLIC* is the incremental adjusted-R^2 from adding the observable risk factors to the single factor market model.[22]

[22] Appendix B (final column) reports the *PUBLIC* variable by FF49 industry. Eight industries stand out as having substantially higher incremental R^2s after adding the observable macro-economic factors: coal, precious metals, tobacco products, defense, shipbuilding/railroad equipment, petroleum and natural gas, utilities, and non-metallic/industrial metal mining. Eight industries with minimal incremental R^2s from adding the observable macro-economic factors are:

Table 6 shows that *PUBLIC* is significantly higher for the non-colluder industry-years. Using either sample to define collusion episodes, the difference in incremental R^2s is approximately 1% (4% vs. 3%). Industries that collude are characterized by a lower availability of observable public news to explain returns, consistent with the prediction of a longer expected withholding period.

HETERO: Intra-industry heterogeneity. Our industry-year measure of intra-industry heterogeneity is the annual average of the standard deviation of residuals from monthly within-industry estimations of a standard market model (see Appendix A). This proxy captures the average idiosyncratic component of returns relative to the industry-wide component. According to Prediction 1a, a withholding equilibrium is more sustainable when the raw magnitude of the industry-wide component of the news is significant. Industries with greater cross-sectional variation in idiosyncratic news are less likely to sustain a large common industry-wide shock and this will be common knowledge. While this reasoning suggests that heterogeneity makes collusion less sustainable, heterogeneity also decreases the probability that traders will infer industry-wide news from the disclosure of any individual firm conditional on an industry-wide shock occurring, which makes a withholding equilibrium less sustainable (Prediction 4). In summary, our prediction about the association between heterogeneity in industry operations and the likelihood of collusion episodes is directionally ambiguous.

Table 6 shows that the average standard deviation of the residuals for the 18 collusion industry-year observations determined using the FULL sample (0.1921) is significantly higher than for the 502 non-collusion industry-year observations (0.1596). Higher *HETERO* for the colluders is consistent with the second explanation above: an increased idiosyncratic component of returns for an industry decreases the likelihood that investors would infer industry-wide news from the disclosure by one firm (Prediction 4). The differences are directionally consistent but not statistically significant between the samples when collusion episodes are defined using the 1SEG sample.

Panel B of Table 6 reports a negative correlation between *HETERO* and *PUBLIC* (-0.251 and -0.278 in the FULL and 1SEG samples, respectively). The negative correlation could indicate that *PUBLIC* is capturing intra-industry homogeneity. *PUBLIC* is an industry-level proxy estimated over the entire sample period, while *HETERO* is based on monthly observations and generates an

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wholesale, business services, medical equipment, consumer goods, personal services, entertainment, electrical equipment, and machinery.

industry-year measure. Nonetheless, because of the correlation, we estimate the logit model with these variables entered individually as a robustness test when interpreting the results.

TAILRISK: Industry tail risk. We create an industry-level indicator variable for the propensity of an industry to experience negative tail risk (positive skew). According to Prediction 1b, a withholding equilibrium is more likely to be sustainable when the ultimate realization of the negative shock is more uncertain. We identify industries with high tail risk based on expected return skew implied by option prices of firms in an industry. Appendix A describes the process used to identify industries with high tail risk. The industries with *TAILRISK* =1 are marked with an asterisk in Appendix B. There are 17 high tail risk industries, or 35% of the FF49 industries.

The proportion of colluding and non-colluding industries that are designated as high tail risk is approximately 38% and not significantly different when we define the episodes using the FULL sample. When we define the episodes using the 1SEG sample, the colluders have a significantly greater proportion of high tail risk industries. Of the 11 episodes, eight (72%) have *TAILRISK* = 1: Entertainment (7), Electrical equipment (22), Communication (32), Transportation (41), Retail (43), Insurance (46), and Trading (48). The three industries that experience collusion episodes but that are not designated as high tail risk industries are Wholesale (42), Rubber and plastic products (15), and Computer software (36).

%CRSP: Stock price incentives to collude. We predict a withholding equilibrium is more sustainable if managers believe that other managers also have (individually motivated) incentives to maintain an elevated stock price by withholding adverse news in their 10-K filings (Prediction 1e). Our proxy for the industry-year equity incentives to collude is the number of firms in the industry that have data on CRSP divided by the number of firms that have data on Compustat (*%CRSP*). For CRSP, we count the firms in each FF49 industry/year with greater than 200 non-missing daily return observations. For Compustat, we count the firms in each FF49 industry/year in the fundamentals annual file with an available SIC code and non-missing annual firm-level sales and total assets data. Firms in industries with a higher percent of firms on CRSP (or, alternatively, a lower percent of firms on Compustat) are more likely to conjecture that other firms in the industry will have similar equity incentives to withhold adverse news in their filings, making withholding a sustainable equilibrium.

The percentage of firms that have publicly traded equity on CRSP, but that are not on Compustat (*%CRSP*), is on average higher for the colluding observations than for the non-colluder industry-year observations, although the differences are not statistically significant (Table 6).[23]

KS_LIT: Litigation risk. We predict that a withholding equilibrium is less likely to be sustainable when litigation risk is high (Prediction 1d). When litigation risk is high, each manager is less likely to conjecture that other managers will withhold, making disclosure her best choice as well. We also predict that a withholding equilibrium is less likely to be sustainable when the costs of being the second-mover are high (Prediction 2), and one component of the second-mover costs is litigation risk. Our measure for litigation risk uses Model 3 from Table 7 of Kim and Skinner (KS, 2012), which is intended to measure the *ex ante* probability of litigation risk, but we use a different sample. We estimate *KS_LIT* at the firm level for fiscal years between 1996 and 2008 and create an industry-year average. (See Appendix A for a description of their model and our sample.) We expect this proxy variable represents the litigation costs of withholding that accrue to all firms (c^{WH}) more than the second-mover costs (c^{2M}). As seen in Table 6, the litigation risk for colluding episodes and non-collusion industry-years are not statistically different.

CONC: Concentration ratios. Industry concentration can capture several industry features that relate to our predictions about the likelihood of collusion episodes. Greater industry concentration suggests that there are a smaller number of larger and perhaps more individually influential firms in an industry. This structure can negatively affect the sustainability of a collusive equilibrium if it implies that investors are more likely to infer industry-wide news from the disclosure of firm-specific news by any one firm in the industry (Prediction 4). More influential firms will also negatively affect the sustainability of a collusive equilibrium if such firms are subject to greater litigation risk and second-mover costs (Predictions 1d and 2). However, a smaller number of influential firms may generate beliefs that the influential firms will cooperate, perhaps because of explicit collusion opportunities, or perhaps because it is more likely that a focal point is available (Prediction 4).[24]

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[23] Appendix B presents the average annual *%CRSP* measures by FF49 industry. Industries with low percentages are: Shipbuilding, Railroad Equipment (73%), Rubber and Plastic Products (76%), Utilities (76%), and Apparel (78%), ignoring the "Almost Nothing" industry. Industries with high percentages are: Banking (215%), Precious Metals (305%), and Trading (344%). The average (median) is 121.4% (103.8%) and the interquartile range is 88.9% to 125.8% (not reported).

[24] Ivaldi et al. (2003) note this explanation for a relation between industry concentration and collusion even in product markets, but claim that there is little evidence. Instead, concentration ratios are commonly predicted to be associated

Greater industry concentration can also be associated with high entry barriers, in which case firms have less incentive to disclose adverse news to deter new entrants. Thus, the payoffs to withholding are greater than the payoffs to disclosure, *ceteris paribus*, increasing the sustainability of a withholding equilibrium. Finally, greater industry concentration may be associated with firms' conjectures that other firms have received a similar signal and believe each firm has (and so on), and that the signal affects all other firms in the same direction, again increasing the likelihood of collusion (Prediction 1a). In summary, we do not make a directional prediction about the relation between *CONC* and collusion.

Our proxies for industry concentration are a revenue based herfindahl index (*HERF_S*) and concentration ratios based on sales (*C6_SALES*) and total assets (*C6_TA*) at the industry-year level. *C6_SALES* (*C6_TA*) is the sum of the market shares of revenues (total assets) for the top six firms in the industry, computed for each FF49 industry and year.[25] See Appendix A for details. The herfindahl index gives some weight to "medium-sized" firms. Table 6 Panel A reports no significant differences in concentration across the colluders and non-colluders using any of our proxies. All three measures of concentration are highly correlated with each other (Panel B). There is some evidence of a positive correlation between *CONC* and *%CRSP*, especially in the 1SEG sample, as well as some correlation with *TAILRISK* in the 1SEG sample.

Figure 2 maps our predictions about the factors that affect the sustainability of a collusive equilibrium from Section 2.3 into the explanatory variables.

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with tacit collusion in product markets because concentration determines the profitability of a collusive price (or quantity) strategy relative to the non-collusive strategy, and the short-term profits from deviating, both of which affect firms' equilibrium beliefs about whether the collusive product-market strategy is sustainable.

[25] We also compute the concentration of the top four or eight firms and based on the market shares of total assets rather than total sales. The results using *C4_SALES*, *C8_SALES*, *C4_TA*, or *C8_TA* are virtually identical.

Figure 2: Predictions and explanatory variables in eqn. (1)

Collusion is more likely when:	Variable	Predicted relation
PRED #1: $P_i^{WH} - P^D$ is high		
Amount of shock is greater		
More homogeneous industries	HETERO	-
More concentrated industries	CONC	+
Uncertainty of shock greater	TAILRISK	+
Expected duration of withholding is longer	PUBLIC	-
c^{WH} is lower	KS_LIT	-
Equity incentives are higher	%CRSP	+
PRED #2: Second-mover costs (c^{2M}) are low	KS_LIT	-
PRED #3: V_i^1, $XS\_PRICE_i^{WH}$, c^{WH}, P^D are common knowledge	HETERO CONC	- +
PRED #4: Traders can infer industry-wide news from individual firms' disclosures	CONC HETERO	- +

The net result is a negative prediction for the availability of public information which shortens the expected duration of the benefits of withholding (*PUBLIC*) and for litigation risk (*KS_LIT*); a positive prediction for industry tail risk (*TAILRISK*) and equity incentives (*%CRSP*); and an ambiguous prediction for industry heterogeneity (*HETERO*) and concentration (*CONC*).

4.2 Results

Table 7 reports the logit model results. The model in Column (1) includes all explanatory variables and uses *C6_SALES* as the proxy for industry concentration.[26] Column (2) substitutes the revenue-based herfindahl index as a proxy for industry concentration. Column (3) excludes *HETERO* given its negative correlation with *PUBLIC*.

[26] Results (untabulated) are similar when we use *C6_TA* to measure industry concentration.

Panel A presents results when we define collusion episodes using the FULL sample. The first significant result is a negative association between the availability of observable information as measured by *PUBLIC* and collusion episodes. This result is consistent with the prediction that more public information shortens the expected duration of the surplus to withholding, thus decreasing the likelihood that firms believe a withholding equilibrium is sustainable.

The second significant result is that greater equity incentives as measured by the percentage of CRSP firms relative to Compustat firms in the industry (*%CRSP*) is consistently positively associated with the likelihood of a collusion episode. This result is consistent with our prediction that the probability of a withholding equilibrium is higher when the equity market incentives associated with financial reporting are higher. A second interpretation of the result, however, is that the positive association comes primarily from the low end of the *%CRSP* distribution. Low observations of *%CRSP* represent industries in which the number of Compustat firms relative to CRSP firms is high, which means that firms file annual reports despite not having publicly traded equity. Thus, the positive association between *%CRSP* and collusion episodes could imply that low-*%CRSP* industries are subject to significant mandated disclosure requirements, such that firms in these industries are less likely to believe that a withholding equilibrium is sustainable. Additional analysis in Section 4.3 attempts to disentangle these explanations.

Finally, in the analysis of the FULL sample collusion episodes, industry concentration (*CONC*) is positively associated with collusion episodes. Recall that the predicted relation is ambiguous. We predict a positive relation if greater concentration reflects a smaller number of influential firms such that firms are more likely to conjecture that other firms will have the same industry-wide news and that it is common knowledge. The relation is also expected to be positive if greater concentration is correlated with softer factors that can sustain a cooperative withholding equilibrium, such as whether there is a focal point, or if greater concentration implies more barriers to entry, which decrease firms' incentives to disclose adverse news (i.e., increase firms' incentives to withhold bad news). On the flip side, we predict a negative association if a smaller number of large influential firms makes it more likely that firms believe that the disclosure by any one firm will increase the likelihood that markets infer the arrival of industry-wide news, which decreases the sustainability of a withholding equilibrium.

Panel B presents results when we define collusion episodes using the 1SEG sample. There are 10 collusion episodes and 96 non-collusion industry-year observations. One collusion episode (from 1995) is lost because the litigation risk proxy is available starting in 1996. The coefficient estimate on *PUBLIC* is directionally consistent with the estimates in Panel A, suggesting that more observable public information deters collusion, but the estimates are not significant at conventional levels in these tests with the smaller sample size. Higher equity incentives as measured by *%CRSP* also show a positive association with collusion episodes, consistent with the prediction that the payoffs to withholding are higher. Again, the effect is directionally consistent with the estimates in Panel A, but the estimates are not significant at conventional levels in Panel B.

In Panel B, the association between *TAILRISK* and collusion episodes is positive, as predicted, and significant in all three model specifications. In industries we identify as having high negative expected return skewness, which we suggest reflects high uncertainty about large adverse events, firms are more likely to collude.

Litigation risk is positively associated with collusion episodes when we define them based on the 1SEG sample (Panel B) and in model (3) of the FULL sample (Panel A). The positive association contradicts the prediction that greater litigation costs provide incentives for firms to preemptively disclose bad news (Skinner, 1994). However, it is important to recall that the litigation cost (c^{WH}) that firms will incur is conditional on a suit being brought, which depends on the probability of subsequent realization of the shock. Our proxy for litigation costs (*KS_LIT*) is based on a model of suits that were brought against firms in an industry. It should reflect the degree to which an industry is subject to greater losses that will generate suits. As such, *KS_LIT* could reflect greater uncertainty about the news. Firms are more likely to believe that future valuation signals may increase (i.e., the bad news will improve or never materialize), and that this is common knowledge, which increases the sustainability of a withholding equilibrium.

The final determinant of collusion episodes that we investigate is within industry heterogeneity in operations, but we find no association with collusion episodes. The predicted association was ambiguous. On one hand, industry news is less likely to affect all firms in heterogeneous industries in the same direction and have the effect be common knowledge, which makes collusion less sustainable. On the other hand, intra-industry heterogeneity makes it less likely that investors will infer industry-wide news from the disclosure by one firm, decreasing the costs to withholding and making a collusive equilibrium more sustainable.

4.3 Additional analyses

Our first additional analysis is meant to determine whether correlated fundamentals explain our findings. A possible explanation for our findings is that the explanatory variables in the logit regression are correlated with changes in industry fundamentals that are omitted from the FOG model. However, if correlated fundamentals explain our findings, we expect the explanatory variables in the logit model to explain industry-wide significant *decreases* in FOG as well the industry-wide *increases* in FOG that we use as an indication of collusion. We conduct a falsification test to determine if the explanatory variables explain significant industry-wide decreases in FOG, which we call transparency episodes, expecting that they should not. The FULL (1SEG) sample have 12 (4) significant industry-year decreases or transparency episodes and 291 (63) non-transparency industry-year observations. Table 8 reports the results of estimating model 2 from Table 7 on the transparency episodes. The results for the collusion episodes are reported in column (1) for convenient comparison.

In the FULL sample (Panel A), the association between transparency episodes and the availability of public information (*PUBLIC*) switches sign relative to the collusion episodes and is significant at the 10% level. The associations between equity incentives (*%CRSP*) and industry concentration (*CONC*) that were observed for collusion episodes are not significant determinants of the transparency episodes. Litigation risk remains insignificant. Industry heterogeneity, however, is significantly and positively associated with the transparency episodes in contrast to its insignificant relation with collusion episodes. In the 1SEG sample (Panel B), we observe a similar lack of explanatory power for the transparency episodes relative to the collusion episodes.

Our second additional analysis is meant to distinguish two explanations for the positive association between *%CRSP* and collusion episodes reported in Table 7. As noted previously, firms in industries with high *%CRSP* are *more* likely to collude to hide adverse news because of greater equity incentives, consistent with a positive association. A second explanation, however, that is also consistent with a positive association, is that firms in industries with low *%CRSP* are *less* likely to collude because of existing mandatory disclosure regulation. We analyze whether the positive relation between *%CRSP* and collusion episodes comes from the low end or the high end of *%CRSP* (or both).

Table 9 reports the results. We use several model specifications given empirical problems associated with using interaction terms in logit models combined with our small sample sizes. In the first test specification, we expand model 2 in Table 7 to include an interaction term that distinguishes the low end of the *%CRSP* distribution. We create an indicator variable (*LOW*) equal to 1 for industry-year observations that are less than or equal to the median industry-year level of

%CRSP. Industry years with *LOW* = 1 (*LOW* = 0) are considered to have low (high) equity incentives.[27] The interaction term equals *%CRSP*LOW*.

Column (1) of Table 9 Panel A reports the results for the FULL sample. It is worth noting that the marginal probabilities for the other explanatory variables in the analysis (*PUBLIC*, *HETERO*, *TAILRISK, KS_LIT*, and *HERF_S*) are similar to those for model 2 in Table 7 in terms of sign, magnitude, and significance. The marginal effect of *%CRSP* declines but remains positive and significant. The marginal effect of the interaction term (*%CRSP*LOW*) is not significant.

Because the interpretation of interaction terms in logit models is unreliable (Ai and Norton, 2003), we also estimate model 2 from Table 7 for separate samples of industries with low equity incentives (*LOW* = 1) and high equity incentives (*LOW* = 0). For industry years with high equity incentives (column (3)), again the marginal effects of the explanatory variables other than *%CRSP* are similar to those for the full sample in terms of sign and significance. The marginal effect of *%CRSP* loses significance (p-value = 15%) in the subsample of firms with high equity incentives. For industry-years with low equity incentives (column (2)), the significance of the explanatory variables other than *%CRSP* to explain collusion episodes disappears, and *%CRSP* is not associated with collusion episodes. Thus, the explanatory power for the collusion episodes appears to derive from the high end of the *%CRSP* distribution, which represents managers who are more likely to benefit from keeping the adverse news hidden from equity markets, and who believe other firms in the industry share those benefits and beliefs.

Finally, in an effort to mitigate problems with reduced sample sizes in the separate estimations in columns (2) and (3), we divide the sample into terciles of the *%CRSP* distribution and estimate model 2 from Table 7 with separate *%CRSP* variables for the lower and upper terciles.[28] Again, the significance of the relation between *%CRSP* and collusion episodes appears strongest in the high end of the distribution, favoring the explanation that greater equity incentives are associated with withholding, although the estimate is not significant at conventional levels (p-value = 18%).

Panel B presents the results for the 1SEG sample. As in Panel A, the marginal effects for the explanatory variables other than *%CRSP* in the model in column (1) that includes the interaction term are generally consistent with the model 2 results in Table 7 although *TAILRISK* loses significance (p-value declines from 6% to 11%) as does *KS_LIT* (p-value declines from 10% to 20%). The marginal effect of *%CRSP* remains insignificant, and the interaction term is positive and

[27] The results are the same if we define low (high) equity incentives as *%CRSP*<1 (*%CRSP*≥1).

[28] In both samples, the number of industries in the middle tercile that collude is too small to estimate the model with a separate regressor for this group.

significant. We can only estimate the model for the subsample of firms with low equity incentives to investigate this finding. The model for the high *%CRSP* industry-year observations does not converge. The marginal effect of *%CRSP* is not significant. When we estimate the model with all observations but estimating a separate effect for terciles of *%CRSP*, the results are similar to those reported in Panel A. The significance of the relation between *%CRSP* and collusion episodes appears strongest in the high end of the distribution, favoring the explanation that greater equity incentives are associated with withholding, although the estimate is not significant at conventional levels (p-value = 19%).

5. Conclusion

This study empirically examines whether managers withhold bad news when it is industry-wide. Whether withholding industry-wide adverse news is a sustainable equilibrium depends on firms' conjectures about other firms' disclosure decisions. If firms conjecture that it is in the best interest of all firms in the industry to withhold, withholding is sustainable. If not, at least one firm is expected to deviate and disclose the news, imposing costs on the non-disclosers (i.e., the second movers). In this scenario, no firm will be willing to withhold the industry-wide news, fearing that other firms will disclose it first. Ultimately, because the sustainability of a withholding equilibrium depends on the relative magnitudes of the costs and benefits for each firm and conjectures about these costs and benefits for all firms in the industry, whether we expect to see collective withholding of industry-wide bad news in practice is an empirical question.

We first document a relatively small number of "collusion episodes." Of 735 possible industry-year combinations, we find between 11 and 18 collusion episodes (depending on the sample) in which an increase in intra-industry disclosure opacity suggests tacit collusion. Two years after the identified collusion episodes, we see evidence of lower sales growth, greater write-offs and more negative special items, and we observe greater write-offs and goodwill impairments in the third year. Also, adjusted returns are significantly more negative for the colluders than for the non-colluders starting in the second or third year following the collusion episode (depending on whether we include multi-segment firms). These results are consistent with the identified increases in opacity representing episodes in which the firms were indeed hiding bad news.

The collusion episodes are less likely in industries in which the news is likely to be public or become public quickly. Collusion is more likely in industries with a greater proportion of firms having equity incentives, in which the benefits of maintaining an elevated stock price are more likely to exceed the costs of withholding, and this would be common knowledge. Finally, withholding is more likely in concentrated industries and industries with greater litigation risk. We do not find evidence suggesting that within industry heterogeneity in operations is associated with collusion episodes, potentially because of offsetting predicted associations.

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Table 1: Summary of FOG prediction model estimates

Averages of select parameter estimates and model statistics for industry-year estimates of the FOG model from 1994-2008 for the FULL sample (Panel A) and the sample of single business segment firms (Panel B). The control variables included in the model are *MVE, MTB, Special items, Return volatility, Non-missing items, Firm age, Delaware, GEO Segments, and BUS Segments*. See Appendix A for variable definitions. Significance is determined by a 2-tailed p-value<0.10.

		Panel A: FULL sample				Panel B: 1SEG sample			
FF49 industry		Annual # regs	Avg annual "N"	Avg IND intercept	% sig intercepts	Annual # regs	Avg annual "N"	Avg IND intercept	% sig intercepts
1	Agric	8	10	18.80	75.00%	-	3	.	.
2	Food	15	38	14.85	26.67%	5	12	16.63	20.00%
3	Soda	-	4	.	.	-	2	.	.
4	Beer	1	8	36.25	100.00%	-	3	.	.
5	Smoke	-	4	.	.	-	1	.	.
6	Toys	14	28	17.07	35.71%	4	9	27.15	50.00%
7	Fun	15	47	20.00	6.67%	7	18	16.54	42.86%
8	Books	14	25	24.49	7.14%	3	6	24.99	100.00%
9	Hshld	15	59	19.08	13.33%	8	18	14.05	25.00%
10	Clths	14	40	20.04	14.29%	4	13	20.04	25.00%
11	Hlth	15	63	19.96	13.33%	6	19	23.17	33.33%
12	Medeq	15	105	23.21	0.00%	12	29	22.76	41.67%
13	Drugs	15	202	21.22	0.00%	15	49	21.14	13.33%
14	Chems	15	59	20.12	0.00%	5	13	21.70	0.00%
15	Rubbr	15	30	20.77	26.67%	4	8	16.16	50.00%
16	Txtls	5	10	18.28	80.00%	3	5	19.92	100.00%
17	Bldmt	15	60	20.04	13.33%	4	10	22.40	25.00%
18	Constr	15	42	17.68	6.67%	7	11	19.74	14.29%
19	Steel	15	46	19.67	0.00%	5	12	17.05	20.00%
20	Fabpr	8	13	14.01	25.00%	2	7	21.20	50.00%
21	Mach	15	109	17.63	6.67%	10	27	6.34	20.00%
22	Elceq	15	44	17.51	6.67%	4	7	16.55	50.00%
23	Autos	15	45	18.32	0.00%	4	11	23.82	25.00%
24	Aero	13	13	16.63	69.23%	-	4	.	.
25	Ships	-	6	.	.	-	2	.	.
26	Guns	-	6	.	.	-	1	.	.
27	Gold	6	10	19.43	16.67%	3	6	15.97	33.33%
28	Mines	6	10	20.21	50.00%	-	3	.	.
29	Coal	-	4	.	.	-	2	.	.
30	Oil	15	114	18.70	0.00%	15	36	17.70	26.67%
31	Util	15	91	18.74	0.00%	6	16	20.73	33.33%
32	Telcm	15	90	20.36	0.00%	13	23	21.31	46.15%
33	Persv	14	36	17.98	14.29%	3	8	22.20	0.00%
34	Bussv	15	172	21.68	0.00%	15	44	22.84	13.33%
35	Hardw	15	86	20.67	0.00%	12	28	2.11	41.67%
36	Softw	15	270	19.02	0.00%	15	64	20.54	0.00%
37	Chips	15	180	21.13	0.00%	12	42	12.72	41.67%
38	Labeq	15	71	16.40	13.33%	4	16	18.83	0.00%
39	Paper	15	37	21.82	13.33%	4	8	32.28	25.00%
40	Boxes	8	10	0.93	50.00%	-	3	.	.
41	Trans	15	81	19.46	0.00%	8	20	19.92	25.00%
42	Whlsl	15	121	19.08	0.00%	8	31	25.11	12.50%
43	Rtail	15	115	19.41	0.00%	15	33	22.21	40.00%
44	Meals	14	36	21.95	0.00%	5	10	14.30	40.00%
45	Banks	15	39	21.85	6.67%	4	11	9.51	50.00%
46	Insur	15	139	20.93	0.00%	15	35	26.04	6.67%
47	Rlest	14	24	22.16	7.14%	3	7	13.66	33.33%
48	Fin	15	200	21.06	0.00%	14	53	21.70	7.14%
49	Other	14	27	18.50	28.57%	3	12	16.38	0.00%
Grand avg		12	63	19.48	16.51%	6	17	19.16	30.29%

Table 2: Summary of collusion episodes

This table details the industry-years labeled as colluders for the full sample as well as the single segment sample. Panel A provides details for the full sample of firms with available data and Panel B provides details for the sample of single business segment firms.

Panel A: Full Sample

Year	# of models with data	Collusion episodes	Collusion Industries (Fama-French 49)
1994	23	0	
1995	29	0	
1996	41	0	
1997	42	0	
1998	41	2	7 (entertainment), 22 (electrical equipment)
1999	41	0	
2000	41	0	
2001	37	1	8 (printing and publishing)
2002	32	7	24 (aircraft), 32 (communication), 34 (business services), 35 (computers), 36 (computer software), 37 (electronic equipment), 46 (insurance)
2003	24	7	11 (healthcare), 13 (pharmaceutical products), 14 (chemicals), 20 (fabricated products), 38 (measuring and control equipment), 42 (wholesale), 48 (trading)
2004	23	1	9 (consumer goods)
2005	26	0	
2006	29	0	
2007	36	0	
2008	37	0	
Total	502	18	

Panel B: Single Segment Sample

1994	8	0	
1995	20	1	42 (wholesale)
1996	35	1	41 (transportation)
1997	37	0	
1998	27	3	7 (entertainment), 15 (rubber and plastic products), 22 (electrical equipment)
1999	14	0	
2000	14	0	
2001	11	0	
2002	7	2	32 (communication), 36 (computer software)
2003	6	1	46 (insurance)
2004	7	1	43 (retail)
2005	6	0	
2006	8	0	
2007	7	1	48 (trading)
2008	7	1	43 (retail)
Total	214	11	

Table 3: Summary of delisting frequencies

Panel A provides descriptive statistics on the percent of firms that delist in the sample of single segment firms (1SEG) used to compute subsequent accounting performance in each lead year after the measurement date of the collusion episode for colluders and non-colluders. Performance-related delistings include CRSP delisting codes 400-500 and 520-584. Merger-related delistings include CRSP delisting codes between 200 and 400. Panel B provides descriptive statistics on the percent of firms that delist in the sample used to compute subsequent returns performance in each denoted time interval after the measurement date of the collusion episode for colluders and non-colluders (e.g., Month+1 is May of 1996 for the firms that were colluders in 1995 and filed financial statements by April 1996). *** or * indicates statistical significance at the 1% or 10% level based on a t-test of the difference between the colluders and non-colluders.

Panel A: Percent of firm-year observations delisted in accounting performance tests			
	Year +1	Year +2	Year +3
Performance-related delistings			
Colluders	3.50***	1.07	0.46
Non-colluders	1.40	1.03	0.67
	0.00	0.93	0.50
Merger-related delistings			
Colluders	7.00***	4.87	2.28
Non-colluders	4.77	4.16	3.20
	0.01	0.37	0.19

Panel B: Percent of firm-year observations delisted in returns tests						
	Month +					
	1	2 to 3	4, 5, 6	7 to 12	13 to 24	25 to 36
Performance-related delistings						
Colluders	0.47	0.70	1.17	1.87	3.04	2.81
Non-colluders	0.39	0.64	1.00	2.13	3.00	3.06
	0.81	0.88	0.74	0.70	0.96	0.77
Merger-related delistings						
Colluders	0.00***	3.04	4.68*	5.85	5.62	5.15
Non-colluders	0.77	2.14	2.74	4.38	6.45	5.66
	0.00	0.28	0.06	0.20	0.47	0.64

Table 4: Accounting performance following the collusion episodes

Panels A-D of this table provides the coefficient level and significance for the collusion indicator in regressions of lead accounting performance on the collusion indicator and control variables. The model used in Panels A-D regresses lead accounting performance on the collusion indicator, control variables, and indicators for industry and year. Accounting performance is as follows: Panel A is sales growth (change in sales / lag sales), Panel B is Special Items (special items / total assets), Panel C is Write-offs (total write-offs / total assets, set to zero if write-offs are missing), and Panel D is Goodwill Impairments (goodwill impairment / total assets, set to zero if goodwill impairments are missing). The accounting performance variable is set to one, two, and three years ahead in the first, second, and third column, respectively. The control variables are *MVE*, *MTB*, *Firm age*, and *Return volatility* (defined in Appendix A). The control variables and lead performance variables are each equal to the mean for firms in the industry-year. The model also includes the concurrent value of the respective accounting performance measure included as the dependent variable. The data included in the regression is at the Fama-French 49 industry-year level and includes all firm-years between 1994-2008 that all required data and have a single business segment (1SEG). The standard errors are clustered at the year level. *, **, and *** equal 10%, 5%, and 1% significance at the 2-tailed level.

	Lead 1	**Lead 2**	**Lead3**
# Industry-years	225	225	217
Panel A: Sales Growth			
Collusion indicator	-0.0688	-0.1116 *	-0.0204
t-statistic	-1.13	-2.14	-0.27
Panel B: Special items			
Collusion indicator	-0.0045	-0.0121 **	-0.011
t-statistic	-0.62	-2.41	-1.53
Panel C: Write-offs			
Collusion indicator	-0.0011 **	-0.0027 *	-0.0019 **
t-statistic	-2.20	-2.08	-2.97
Panel D: Goodwill Impairment			
Collusion indicator	-0.0006	0.0009	-0.0032 **
t-statistic	-0.54	0.72	-2.96

Table 5: Returns following the collusion episodes

This table presents average cumulative abnormal size-decile adjusted returns (CARs) in portfolios of firms in industries defined as colluders and non-colluders identified using the 1SEG sample. Returns are presented for months 1-, 3-, 6-, 12-, 24- and 36 after month four (April) of the collusion episode year *y*. Panel A presents average CARs for single business segment firms with data as of December of the collusion year y. Multi-segment firms are excluded. Panel B presents average CARs for both single and multi-segment firms. Panel C presents average CARs for single business segment firms and adjusts the missing CARs of firms with performance-related delistings to equal the 5th percentile return for that industry in that period. Panel D includes both single and multi-segment firms and adjusts missing performance-related delisting returns. *, **, and *** equal 10%, 5%, and 1% significance at the 2-tailed level.

		Ret1mon	Ret3mon	Ret6mon	Ret12mon	Ret24mon	Ret36mon
Panel A: Single segment firms, no adjustment for performance-related delistings							
Colluders:	Return	0.0269	0.0417	0.0434	0.0290	-0.0023	-0.1212
	N	424	408	383	350	312	270
Non-colluders:	Return	0.0097	-0.0038	-0.0060	-0.0105	0.0147	0.0253
	N	14,427	14,025	13,465	12,496	11,095	9,734
Difference in returns		**0.0172***	**0.0455*****	**0.0494****	**0.0395**	**-0.017**	**-0.1465*****
t-value		1.87	3.36	2.50	1.36	-0.33	-3.33
Panel B: Single+multi-segment firms, no adjustment for performance-related delistings							
Colluders:	Return	0.0083	0.0210	0.0397	0.0275	-0.0744	-0.1259
	N	3,224	3,167	3,104	2,631	2,409	2,039
Non-colluders:	Return	-0.0004	-0.0096	-0.0173	-0.0306	-0.0331	-0.0422
	N	54,962	54,004	52,604	49,710	44,416	38,742
Difference in returns		**0.0087*****	**0.0306*****	**0.0570*****	**0.0581*****	**-0.0413*****	**-0.0837*****
t-value		3.09	6.86	8.41	5.44	-3.28	-5.42
Panel C: Single segment firms, adjust missing values for performance-related delistings							
Colluders:	Return	0.0259	0.0372	0.0297	-0.0028	-0.0692	-0.2204
	N	426	413	393	368	343	312
Non-colluders:	Return	0.0086	-0.0084	-0.0185	-0.0430	-0.0605	-0.0982
	N	14,483	14,172	13,762	13,101	12.137	11,212
Difference in returns		**0.0173***	**0.0456*****	**0.0482****	**0.0402**	**-0.0087**	**-0.1222*****
t-value		1.88	3.35	2.44	1.41	-0.24	-3.00
Panel D: Single+multi-segment firms, adjust missing values for performance-related delistings							
Colluders:	Return	0.0076	0.0170	0.0299	-0.0039	-0.1328	-0.2124
	N	3,234	3,203	3,163	2,764	2,633	2,337
Non-colluders:	Return	-0.0013	-0.0135	-0.0282	-0.0583	-0.0995	-0.1463
	N	55,152	54,505	53,625	51,807	48,420	44,221
Difference in returns		**0.0089*****	**0.0305*****	**0.0581*****	**0.0544*****	**-0.0333*****	**-0.0661*****
t-value		3.13	6.84	8.58	5.19	-2.79	-4.69

Table 6: Descriptive statistics for industry-level characteristics
This table provides descriptive statistics on industry-level characteristics for the full sample firms with required data (FULL) and the sample of single business segment firms (1SEG). Panel A presents means of industry characteristics across the industry-year non-collusion episodes and the collusion episodes. Industry size is the average number of firms in the industry with non-missing sales and assets data on Compustat. *PUBLIC* proxies for the availability of public information and is the difference between the adjusted R^2 values from estimation of a standard market model and a factor model for each firm *i* within industry *j*, estimated using monthly return and factor data over the period 1994-2008. The factor model includes seven macro-economic factors described in Appendix A. *HETERO* is the average monthly standard deviation of the idiosyncratic component of returns within industry *j* in year *y*. *TAILRISK* is an indicator variable that equals one for industries that have high negative expected return skewness computed based on the methods in Van Buskirk (2011), and zero otherwise (see Appendix A). *%CRSP* is the average annual number of firms with CRSP data relative to the number of firms with Compustat data (see Appendix B). *C6_SALES* and *C6_TA* are the sales-based and asset-based concentration ratios, respectively, for the largest six firms in the industry based on market share. *HERF_S* is the herfindahl index for the 50 largest firms in the industry. ** and *** indicate that the percent of delisting episodes in the collusion sample is significantly greater than the non-collusion sample at a 5% and 1% significance level based on t-tests. Panel B presents the correlations between these variables, by sample. Statistical significance in the collusion matrices at 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

Panel A: Average annual measures

		FULL SAMPLE			1SEG SAMPLE		
		Non-Colluder	Colluder		Non-Colluder	Colluder	
Num obs.		(502)	(18)		(214)	(11)	
Industry size		149	187		227	220	
PUBLIC		0.0401	0.0305	**	0.0402	0.0279	**
HETERO		0.1596	0.1921	***	0.1714	0.1763	
TAILRISK		0.3745	0.3889		0.4112	0.7273	**
%CRSP		1.1255	1.2083		1.2047	1.5788	
KS_LIT		0.0471	0.0560		0.0483	0.0557	
CONC:	*C6_SALES*	0.5526	0.5742		0.5023	0.4945	
	C6_TA	0.5760	0.6028		0.5194	0.5378	
	HERF_S	0.1022	0.1268		0.0861	0.1168	

Panel B: Correlation tables

FULL SAMPLE

	C6_SALES	*C6_TA*	*HERF_S*	*PUBLIC*	*HETERO-GENEITY*	*TAILRISK*	*%CRSP*
C6_TA	0.940***						
HERF_S	0.828***	0.802***					
PUBLIC	0.210***	0.078*	0.204***				
HETERO	-0.018	-0.056	0.026	-0.251***			
TAILRISK	0.015	0.068	0.022	-0.250***	0.008		
%CRSP	0.119***	0.162***	0.102**	0.212***	-0.195***	0.116***	
KS_LIT	-0.057	-0.137***	-0.034	-0.085*	0.249***	0.090*	-0.085*

1SEG SAMPLE

	C6_SALES	*C6_TA*	*HERF_S*	*PUBLIC*	*HETERO-GENEITY*	*TAILRISK*	*%CRSP*
C6_TA	0.887***						
HERF_S	0.773***	0.739***					
PUBLIC	0.233***	-0.001	0.096				
HETERO	0.096	-0.009	0.195***	-0.278***			
TAILRISK	0.183***	0.230***	0.142**	-0.137**	-0.097		
%CRSP	0.268***	0.416***	0.221***	0.156**	-0.422***	0.199***	
KS_LIT	0.106	-0.088	0.152**	-0.065	0.498***	-0.022	-0.193***

Table 7: Characteristics of industries that have collusion episodes

This table presents estimated marginal effects from a logit regression model of collusion episode occurrence on industry and industry-year level correlates. The unit of observation is at the industry-year level. See Table 6 for definitions of the independent variables. Panel A and Panel B present results for collusion episodes identified using the full sample (FULL) and the 1SEG sample (1SEG), respectively. P-values of the χ^2 test for coefficient significance are presented in brackets. Statistical significance (two-sided) at the 10%, 5% and 1% level is denoted by *, **, and ***, respectively.

Panel A: Collusion episodes identified using FULL SAMPLE
(18 collusion episodes; 157 non-collusion industry-year observations)

	Predicted Sign	Model specification: (1)	(2)	(3)
Intercept		-0.3615***	-0.2882***	-0.2586***
		(0.00)	(0.00)	(0.00)
Availability of public info (*PUBLIC*)	-	-2.3234**	-2.3322**	-2.5011***
		(0.03)	(0.03)	(0.01)
HETERO	?	0.3069	0.1806	
		(0.47)	(0.69)	
TAILRISK	+	-0.0315	-0.0318	-0.0341
		(0.43)	(0.43)	(0.39)
Equity incentives (*%CRSP*)	+	0.0801**	0.0786**	0.0764**
		(0.02)	(0.02)	(0.02)
Litigation risk (*KS_LIT*)	-	1.0336	1.1604	1.3794*
		(0.25)	(0.21)	(0.06)
Industry concentration (*CONC*)	?			
C6_SALES		0.1839		
		(0.14)		
HERF_S			0.4391*	0.4489**
			(0.06)	(0.05)
Pseudo R^2		9.85%	10.98%	10.84%
Wald χ^2 test statistic		13.24	12.56	12.77
Wald χ^2 p-value		0.04	0.05	0.03

Panel B: Collusion episodes identified using 1SEG SAMPLE
(10 Collusion episodes; 96 Non-collusion industry-year observations)

	Predicted Sign	Model specification: (1)	(2)	(3)
Intercept		-0.2511**	-0.2563*	-0.2461**
		(0.08)	(0.08)	(0.04)
Availability of public info (*PUBLIC*)	-	-0.9520	-1.1080	-1.1419
		(0.39)	(0.32)	(0.29)
HETERO	?	0.2802	0.0567	
		(0.53)	(0.90)	
TAILRISK	+	0.0805**	0.0753*	0.0751*
		(0.05)	(0.06)	(0.06)
Equity incentives (*%CRSP*)	+	0.0396	0.0289	0.0274
		(0.13)	(0.22)	(0.17)
Litigation risk (*KS_LIT*)	-	1.3685	1.5215*	1.5496*
		(0.15)	(0.10)	(0.09)
Industry concentration (*CONC*)	?			
C6_SALES		-0.0929		
		(0.51)		
HERF_S			0.0999	0.1135
			(0.65)	(0.55)
Pseudo R^2		18.93%	18.53%	18.51%
Wald χ^2 test statistic		9.51	9.59	9.59
Wald χ^2 p-value		0.15	0.14	0.09

Table 8: Falsification test: Characteristics of industries that have transparency episodes

This table (column (2)) presents estimated marginal effects from a logit regression model of industry years that exhibit significant decreases (rather than increases) in FOG, which we call transparency episodes, and industry and industry-year level correlates. Column (1) reports results for model (2) from Table 7 for the collusion episodes for convenient comparison. The unit of observation is at the industry-year level. See Table 6 for definitions of the independent variables. Panel A provides details for the full sample of firms with available data (FULL) and Panel B provides details for the sample of single business segment firms (1SEG). P-values of the χ^2 test for coefficient significance are presented in brackets. Statistical significance (two-sided) at the 10%, 5% and 1% level is denoted by *, **, and ***, respectively.

Panel A: Collusion episodes identified using FULL SAMPLE
(12 transparency episodes; 291 non-transparency industry-year observations)

	Predicted Sign	Collusion episodes (1)	Transparency episodes (2)
Intercept		-0.2882***	-0.1624***
		(0.00)	(0.00)
Availability of public info (*PUBLIC*)	-	-2.3322**	0.4403*
		(0.03)	(0.10)
HETERO	?	0.1806	0.4645***
		(0.69)	(0.00)
TAILRISK	+	-0.0318	0.0223
		(0.43)	(0.16)
Equity incentives (*%CRSP*)	+	0.0786**	0.0054
		(0.02)	(0.76)
Litigation risk (*KS_LIT*)	-	1.1604	-0.4856
		(0.21)	(0.30)
Industry concentration (*HERF_S*)	?	0.4391*	-0.1280
		(0.06)	(0.29)
Pseudo R^2		10.98%	11.63%
Wald χ^2 test statistic		12.56	11.79
Wald χ^2 p-value		0.05	0.07

Panel B: Collusion episodes identified using 1SEG SAMPLE
(4 transparency episodes; 63 industry-year observations)

	Predicted Sign	Collusion episodes (1)	Transparency episodes (2)
Intercept		-0.2563*	-0.0595
		(0.08)	(0.58)
Availability of public info (*PUBLIC*)	-	-1.1080	0.2135
		(0.32)	(0.56)
HETERO	?	0.0567	0.3260
		(0.90)	(0.48)
TAILRISK	+	0.0753*	0.0167
		(0.06)	(0.51)
Equity incentives (*%CRSP*)	+	0.0289	0.0012
		(0.22)	(0.97)
Litigation risk (*KS_LIT*)	-	1.5215*	-0.3227
		(0.10)	(0.53)
Industry concentration (*HERF_S*)	?	0.0999	-0.4553
		(0.65)	(0.44)
Pseudo R^2		18.53%	30.90%
Wald χ^2 test statistic		9.59	0.80
Wald χ^2 p-value		0.14	0.99

Table 9: Further analysis of *%CRSP as a determinant of collusion episodes*

This table presents estimated marginal effects from logit regression models of collusion episode occurrence on industry and industry-year level correlates allowing for a separate relation between firms with low and high equity incentives as measured by *%CRSP*. The unit of observation is at the industry-year level. Column (1) reports results for model (2) from Table 7 with an additional interaction term of *%CRSP* and *LOW*, which is an indicator variable for industry-year observations that have a *%CRSP* less than or equal to the median industry-year level. Columns (2) and (3) report results for the subsample of observations with low equity incentives (LOW = 1) and high equity incentives (LOW = 0), respectively. Column (4) reports results with separate *%CRSP* variables for terciles of the *%CRSP* distribution. See Table 6 for definitions of the independent variables. Panel A provides details for the full sample of firms with available data (FULL) and Panel B provides details for the sample of single business segment firms (1SEG). P-values of the χ^2 test for coefficient significance are presented in brackets. Statistical significance (two-sided) at the 10%, 5% and 1% level is denoted by *, **, and ***, respectively.

Panel A: Collusion episodes identified using FULL SAMPLE
LOW (HIGH) %CRSP sample has 7 (11) collusion episodes; 92 (65) non-collusion industry-year observations

	Predicted Sign	(1)	(2) LOW %CRSP	(3) HIGH %CRSP	(4)
Intercept		-0.2545**	-0.4218**	-0.1601	-0.1908*
		(0.03)	(0.02)	(0.44)	(0.08)
Availability of public info (*PUBLIC*)	-	-2.3080**	0.0094	-4.9209***	-2.2861**
		(0.03)	(0.99)	(0.00)	(0.04)
HETERO	?	0.1783	0.2710	0.2657	-0.0184
		(0.69)	(0.57)	(0.73)	(0.97)
TAILRISK	+	-0.0287	0.0321	-0.1403**	-0.0265
		(0.47)	(0.42)	(0.05)	(0.50)
Litigation risk (*KS_LIT*)	-	1.1351	0.7310	1.4405	1.7150
		(0.21)	(0.46)	(0.26)	(0.11)
Industry concentration (*HERF_S*)	?	0.4227*	0.2907	1.0268**	0.3863*
		(0.07)	(0.28)	(0.04)	(0.08)
Equity incentives (*%CRSP*)	+	0.0652*	0.1902	0.0861	
		(0.09)	(0.25)	(0.15)	
*%CRSP*LOW*		-0.0357			
		(0.45)			
%CRSP (lower tercile firms)					-0.0570
					(0.31)
%CRSP (upper tercile firms)					0.0335
					(0.18)
Pseudo R^2		11.47%	10.46%	21.21%	11.55%
Wald χ^2 test statistic		13.26	6.13	10.33	14.08
Wald χ^2 p-value		0.07	0.41	0.11	0.05

Table 9, continued

Panel B: Collusion episodes identified using 1SEG SAMPLE
LOW (HIGH) %CRSP sample has 7 (3) collusion episodes; 50 (46) non-collusion industry-year observations

	Predicted Sign	(1)	(2) LOW %CRSP	(3) Does not converge	(4)
Intercept		-0.2496*	-0.2280		-0.2608*
		(0.10)	(0.53)		(0.07)
Availability of public info (*PUBLIC*)	-	-0.9863	-2.6316		-0.9705
		(0.26)	(0.31)		(0.41)
HETERO	?	0.0845	-1.3013		0.1039
		(0.80)	(0.20)		(0.82)
TAILRISK	+	0.0536	0.1012		0.0836**
		(0.11)	(0.16)		(0.04)
Litigation risk (*KS_LIT*)	-	0.9564	1.6990		1.4188
		(0.20)	(0.33)		(0.16)
Industry concentration (*HERF_S*)	?	0.0969	0.0650		0.0768
		(0.58)	(0.94)		(0.74)
Equity incentives (*%CRSP*)	+	0.0395	0.2522		
		(0.15)	(0.48)		
*%CRSP*LOW*		0.0771*			
		(0.10)			
%CRSP (lower tercile firms)					0.0401
					(0.43)
%CRSP (upper tercile firms)					0.0269
					(0.19)
Pseudo R^2		25.49%	14.80%		18.85%
Wald χ^2 test statistic		6.60	6.13		9.57
Wald χ^2 p-value		0.47	0.41		0.19

Appendix A: Variable definitions
This appendix provides detailed definitions of variables that are included in the analysis.

Control variables used in the FOG model

MVE:	Log of market value of equity (CSHO * PRCC_F), winsorized at 1%.
MTB:	Market to book ratio (*MVE* + LT / AT), winsorized at 1%.
Special items:	Special items scaled by total assets (SPI/AT), winsorized at 1%
Return volatility:	Standard deviation of monthly stock returns for the 12 months ending in the third month of the current year (i.e., from current month three back to lag nine), winsorized at 1%. This variable is set to missing if there are less than 11 months of return data.
Non-missing items:	Number of non-missing items in Compustat, winsorized at 1%, to proxy for complexity. The total number of items possible is based on all data items for the balance sheet, income statement, and cash flow statement per the WRDS Compustat web interface.
Firm age:	Number of years a firm has been listed on CRSP.
Delaware:	An indicator = 1 if the firm was incorporated in Delaware (INCORP= "DE"), 0 otherwise.
GEO Segments:	Log of the number of geographic segments plus one, obtained from the Compustat Segment file. The variable is set to one if missing in the datafile.
BUS Segments:	Log of the number of business segments plus one, obtained from the Compustat Segment file. The variable is set to one if missing in the datafile.

Explanatory variables used in the logit model of the collusion episodes

<u>*PUBLIC*: Availability of public information</u>
For each of the FF49 industries, we estimate a standard market model (A1) and a factor model (A2) using monthly data between January 1994 and December 2008:

$$r_{im} = \alpha_j + \beta_j^{mkt} r_{mkt,m} + \varepsilon_{im} \tag{A1}$$

$$r_{im} = \alpha_j + \beta_j^{mkt} r_{mkt,m} + \sum_{z=1}^{7} \delta_j^z FACTOR_m^z + \xi_{im} \tag{A2}$$

where r_{im} is the monthly return for firm *i* in industry *j* (*j* = 1 to 49); $r_{mkt,m}$ is the monthly return on the CRSP equally-weighted market index; and $FACTOR_m^z$ is the monthly value of one of seven macro-economic risk factors identified in prior research and described below. *PUBLIC* is the difference between the adjusted R^2 values of the two models for each FF49 industry. The greater the difference in adjusted R^2s for an industry, the greater is the availability of public information.

The seven factors in Equation (A2) are:

(1) Short-term interest rates = the 3-month treasury bill rate from CRSP.
(2) Default premium = Moody's Seasoned Baa Corporate Bond Yield (available from http://www.federalreserve.gov/releases/h15/data.htm) minus the 10-year constant maturity government bond yield (from http://research.stlouisfed.org/fred2/).

(3) Term premium = the yield on 10-year constant maturity government bonds minus the yield on one-year constant maturity government bonds (from http://research.stlouisfed.org/fred2/).
(4) Foreign Exchange Rates = weighted average of the foreign exchange value of the U.S. dollar against a subset of the broad index currencies that circulate widely outside the country of issue (from http://research.stlouisfed.org/fred2/).
(5) Producer price index = the total finished goods producer price index from the Bureau of Labor Statistics.
(6) Small minus Big (SMB) = the Fama-French monthly benchmark factor for the performance of small stocks relative to big stocks.
(7) High – low book-to-market (HML) = the Fama-French monthly benchmark factor for the performance of value stocks relative to growth stocks.

SMB and HML are from Kenneth French's website:
(http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html).

HETERO: Intra-industry heterogeneity
We estimate a single factor market model using monthly returns for each FF49 industry for each calendar month between January 1994 and December 2008. *HETERO* represents the industry-year average of the standard deviations across all months in each calendar year.

TAILRISK: Industry tail risk
Using option data from OptionMetrics, we calculate the Van Buskirk (2011) measure of expected volatility skew before each I/B/E/S earnings announcement that meets his data requirements. This measure is calculated as the average implied volatility for out-of-the-money puts less the average implied volatility for at-the-money calls. We compute skew for 85,293 firm-quarters between 1995 and 2010. We calculate the firm average skew by year to convert this measure to firm-year observations so that firms with more populated data do not influence the measurement. The average (and median) firm-year have volatility skew of approximately 0.03 (higher values indicate more negative skewness).

On average, the industries have 32.8 firms in any given year (median = 20.2). The minimum (maximum) is 1.7 (131.2) per year for industry 1 (industry 36). The inter-quartile range is 5.9 to 50.9. For each industry, we obtained the median and maximum measure of skew over the 13 years for which we have observations. Industries with a maximum greater than the median industry maximum (0.061) and with a median greater than the global industry median of 0.03 are designated as having high skew (*TAILRISK* = 1).

An industry level proxy is preferable to an industry-year level proxy when using a market-based measure to estimate the extent to which an industry is subject to large but uncertain shocks. While this construct may vary over time for an industry, using an annual measure would require assumptions about when the market anticipates the yet unrealized shock, which managers are potentially attempting to hide.

%CRSP: Stock price incentives to collude
For each FF49 industry and year, we count the firms with greater than 200 non-missing daily return observations on CRSP and divide by the number of firms in the Compustat fundamentals annual file with an available SIC code and non-missing annual firm-level sales and total assets data.

KS_LIT: Litigation risk

Our proxy for litigation risk is based on Model 3 from Table 7 of Kim and Skinner (KS, 2012). This model is intended to capture the factors that make a firm more vulnerable to securities litigation prior to the revelation of "triggering events" like major price declines. In this sense, the model provides an *ex ante* probability of litigation risk. Specifically, we estimate the following model:

$$\begin{aligned} SUED = b_0 &+ b_1(FPS_t) + b_2(LNASSETS_{t-1}) + b_3(SALES\_GROWTH_{t-1}) + b_4(RETURN_{t-1}) \\ &+ b_5(RETURN\_SKEWNESS_{t-1}) + b_6(RETURN\_STD\_DEV_{t-1}) \\ &+ b_7(TURNOVER_{t-1}) + e \end{aligned}$$

We estimate *KS_LIT* for fiscal years between 1996 and 2008 for a sample of 48,844 firm-years (including 2,667 lawsuits). The firm-year observations are averaged to create the industry-year observations. Our sample of sued firms is generated from a database provided by Woodruff-Sawyer, a San Francisco-based insurance brokerage firm. As explained in Rogers and VanBuskirk (2009), Woodruff-Sawyer aggregates data from a number of sources including Securities Class Action Clearinghouse (SCAC), which is the source of the sued firm sample in KS. Estimating the model on this sample allows us to relax some of KS's sample requirements that are not necessary for our study (e.g., they require return data to be available for three years prior to the beginning of each fiscal year). As expected, the sign and significance of each of the independent variables is similar to that reported by KS (see KS for specific variable definitions).

CONC: Industry concentration ($C_n\ SALES, C_n\ TA$, $HERF\ S$ and $HERF\ TA$)

We compute industry-year level concentration ratios as the sum of the market shares of sales or total assets for the *n* largest firms in an industry:

$$C_n = \sum_{i=1}^{n}(Market\ Share)_i \qquad \text{(A3)}$$

We compute the concentration ratio for the top four, six and eight firms. Concentration ratios are commonly used in the cartel literature, but there is no consensus on the appropriate *n* to include.

We also compute a revenue-based herfindahl index for each FF49 industry for each year as:

$$HERF\_S_{jy} = \left(\sum_{i=1}^{m} \left(Sales_i \Big/ \sum_{i=1}^{m} Sales_i \right)^2 \right)$$

where $Sales_i$ is revenues for each firm *i* in industry *j* in year *y* and *m* equals 50 if there are at least 50 firms in the industry and equals the number of firms in industry *j* in year *y* if the number is less than 50. We similarly compute a herfindahl index based on market share of total assets ($HERF\_TA_{jy}$).

Appendix B: Summary of explanatory variables across the Fama-French 49 industries

An asterisk after the industry name indicates that the industry is designated as having high tail risk (TAILRISK=1). The following columns report the average annual number of firms in each FF49 industry over the period 1994-2008 with available data on Compustat and CRSP, the average annual percent of firms on CRSP relative to Compustat (*%CRSP*), and the *PUBLIC* score for each industry.

				Average annual # of firms		Average	
		Industry		Compustat	CRSP	%CRSP	PUBLIC
1	Agric	Agriculture		18	17	94.44%	0.064
2	Food	Food Products		90	82	91.11%	0.027
3	Soda	Candy & Soda		11	20	181.82%	0.068
4	Beer	Beer & Liquor		13	24	184.62%	0.052
5	Smoke	Tobacco Products		6	8	133.33%	0.174
6	Toys	Recreation	*	53	53	100.00%	0.021
7	Fun	Entertainment	*	102	87	85.29%	0.017
8	Books	Printing and Publishing		46	59	128.26%	0.025
9	Hshld	Consumer Goods	*	111	96	86.49%	0.016
10	Clths	Apparel	*	79	62	78.48%	0.036
11	Hlth	Healthcare		112	120	107.14%	0.020
12	Medeq	Medical Equipment		187	187	100.00%	0.015
13	Drugs	Pharmaceutical Products	*	317	324	102.21%	0.050
14	Chems	Chemicals		92	103	111.96%	0.033
15	Rubbr	Rubber and Plastic Products		58	44	75.86%	0.032
16	Txtls	Textiles		33	29	87.88%	0.058
17	Bldmt	Construction Materials		103	88	85.44%	0.022
18	Constr	Construction	*	70	71	101.43%	0.023
19	Steel	Steel Works Etc.		76	82	107.89%	0.055
20	Fabpr	Fabricated Products		22	20	90.91%	0.052
21	Mach	Machinery		184	177	96.20%	0.019
22	Elceq	Electrical Equipment	*	80	136	170.00%	0.019
23	Autos	Automobiles and Trucks	*	80	90	112.50%	0.047
24	Aero	Aircraft		22	26	118.18%	0.068
25	Ships	Shipbuilding, Railroad Equip.		11	8	72.73%	0.138
26	Guns	Defense	*	10	9	90.00%	0.153
27	Gold	Precious Metals		21	64	304.76%	0.186
28	Mines	Non-Metallic/Industrial Metal Mining		18	29	161.11%	0.093
29	Coal	Coal	*	7	12	171.43%	0.254
30	Oil	Petroleum and Natural Gas		199	243	122.11%	0.126
31	Util	Utilities		224	171	76.34%	0.097
32	Telcm	Communication	*	193	238	123.32%	0.025
33	Persv	Personal Services	*	69	68	98.55%	0.017
34	Bussv	Business Services		334	387	115.87%	0.007
35	Hardw	Computers		163	137	84.05%	0.027
36	Softw	Computer Software		524	447	85.31%	0.031
37	Chips	Electronic Equipment		338	350	103.55%	0.037
38	Labeq	Measuring & Control Equip.		124	122	98.39%	0.022
39	Paper	Business Supplies		66	67	101.52%	0.035
40	Boxes	Shipping Containers		16	17	106.25%	0.066
41	Trans	Transportation	*	139	152	109.35%	0.027
42	Whlsl	Wholesale		234	251	107.26%	0.007
43	Rtail	Retail	*	313	288	92.01%	0.028
44	Meals	Restaurants, Hotels, Motels	*	115	131	113.91%	0.036
45	Banks	Banking		337	723	214.54%	0.069
46	Insur	Insurance	*	207	200	96.62%	0.060
47	Rlest	Real Estate		56	49	87.50%	0.045
48	Fin	Trading	*	315	1085	344.44%	0.035
49	Other	Almost Nothing		79	25	31.65%	0.047