Preemptive Disclosure

Matthew J. Bloomfield*[1], Irina Luneva[1], and Mirko Heinle[1]

[1]The Wharton School of the University of Pennsylvania

March 28, 2024

Abstract

We analyze voluntary disclosure practices in the presence of a leak risk. In a standard model of voluntary disclosure, managers are less forthcoming when negative information may be leaked by external sources. However, if managers prefer to be the bearer of their firm's bad news, potential leaks motivate managers to disclose negative information, preemptively. Empirically, we document that when the probability of a leak is higher, firms offer earnings guidance more frequently and generate systematically lower returns on their voluntary disclosure dates, but subsequently perform better at the time of the potential leak. Poor disclosure-day returns are explained by potential imminent leaks, but not leaks that may have recently occurred. These patterns are consistent with our model of leak preemption; when facing a potential leak, managers become more forthcoming in order to preempt the leaks.

JEL Classifications: D22; D80; J33; L13; M48

Keywords: Voluntary Disclosure; Leaks; Relative Performance Evaluation

*Corresponding author. Mailing address: 1325 Steinberg-Dietrich Hall 3620 Locust Walk Philadelphia, PA 19104-6365. Phone number: (607) 351-3042. E-mail address: mjbloom@wharton.upenn.edu.

1 Introduction

A large body of prior research demonstrates that managers are not always able to control the flow of their firms' negative information. While managers can and do voluntarily disclose their own bad news (e.g. Skinner, 1994), this information can also be disclosed by other parties who wish to harm the company, such as whistle-blowers, short-sellers, and/or managers at other companies. In what follows, we use the term "leak" to refer to the disclosure/dissemination of a company's non-public negative information, for which the disclosure/dissemination choice is outside of the manager's control.

We examine how managers react to the possibility of a leak, vis-á-vis their own voluntary disclosure practices. In particular, we address the following questions. When leaks are more likely, do managers become more or less forthcoming about their bad news? We further assess whether managers try to preempt potential leaks, or take a "wait-and-see" approach, potentially offering their own disclosures in response to leaks that have already occurred. Finally, we investigate whether preemptive disclosure is effective in mitigating the negative impact of a leak.

To guide our empirical investigation, we first develop a simple model of voluntary disclosure in the face of a potential leak, in the style of a Dye (1985) and Jung and Kwon (1988) voluntary disclosure game. We find that the testable implications hinge on whether or not the manager has a preference for being the initial bearer of bad news.

If managers do not care whether bad news comes first from themselves or from leakers, the possibility of a leak prompts managers to adopt a less forthcoming disclosure policy, whereby more bad news is withheld by the manager. That is, the threshold for voluntary disclosure increases with the probability of the leak. This happens because investors understand that complete silence (no firm disclosure and no leak) suggests that there was no negative information to leak, which is a reassuring sign regarding firm value, thereby raising the equilibrium non-disclosure stock price.

Empirically, we would expect greater leak risk to be associated with fewer disclosures, and higher disclosure-day returns, on average.

In contrast, if managers have a sufficiently strong preference for being the first to reveal their firms' bad news, the testable implications flip. The possibility of a leak prompts managers to adopt a *more* forthcoming disclosure policy, whereby more bad news is disclosed by the manager. That is, the threshold for voluntary disclosure *decreases* as leak risk rises. Empirically, we would expect greater leak risk to be associated with more frequent disclosures, and lower average disclosure-day returns. Conditional on disclosure occurring, we would further expect that disclosures occur earlier (to facilitate preemption)—especially for disclosures that convey bad news.

It is not ex ante obvious which set of predictions is more likely to be descriptive. From one viewpoint, it is not clear that a manager would be concerned about who delivers information to the market. If the leaker and the manager both have access to the same signal (and must be truthful, if they choose to disclose it), then one could easily take the perspective that it should not matter which player discloses the information first. Even if signals can be distorted, it is not clear that a manager would care about being the *first* to disclose; they could take a wait-and-see approach, and offer corrective disclosures, as needed, to address any misleading leaks. In general, Bayesian updating is sequence-independent. As such, under most setups, rational capital market participants would reach the same posterior beliefs about firm value, irrespective of the order of signals received.

However, there are also reasons to expect that managers might prefer being the first to disclose. For one, there could be litigation concerns that arise when it becomes clear that a manager was withholding information. Alternatively, if disclosed signals are not fully revealing about the state of the world and/or managerial "type," then the act of voluntarily disclosing before any leaks occur could function credibly as a beneficial signal that is incrementally informative to the content of the disclosure itself—benefits that would likely be forfeit by disclosing after a leak occurs. Or perhaps, in a non-Bayesian framework (e.g., with boundedly-rational capital market participants), there may simply be value in "controlling the narrative" around bad news, and being first to disclose may be helpful in this regard.

We take our competing sets of predictions to the data. As is common in prior literature, we focus on earnings guidance as our primary proxy for voluntary disclosure.[1] Prior literature has not reached a consensus on a standard measure for leak risk. Based on recent work by Bloomfield, Heinle, and Timmermans (2023), we measure a firm's leak risk based on the number of companies that include the firm of interest as a "price-peer" for a stock price-based RPE grant in their CEO's pay plan.[2] Pay plans that compensate CEOs on the basis of relative total shareholder return ("rTSR") implicitly reward those CEOs for revealing negative information about their price-peers. Bloomfield et al. (2023) show that rTSR-using companies routinely leak novel negative information about their price-peers, in order to surpass them in relative stock performance. As such, being included as a price-peer in another company's CEO pay plan exposes that firm to additional leak risk—the more rTSR peer groups the firm is included in, the greater the risk.

Overall, we find strong support for the predictions that arise from assuming that the manager prefers to be the first to disclose their firm's bad news to the market. When facing heightened leak risk, firms become more forthcoming about their bad news. Consistent with a lowered disclosure threshold, they issue voluntary earnings guidance more frequently, and generate lower disclosure-day returns, on average. Moreover, conditional on disclosures occurring, firms facing heightened leak risk also *accelerate* their disclosures—especially their bad news disclosures—by a few days, in

[1] See, e.g., Penman (1980); Skinner (1994, 1997); Hutton, Miller, and Skinner (2003); Anilowski, Feng, and Skinner (2007); Francis, Nanda, and Olsson (2008); Kato, Skinner, and Kunimura (2009); Rogers, Skinner, and Van Buskirk (2009); Kim and Shi (2011); Gong, Li, and Zhou (2013); Ciftci and Salama (2018); Hope, Kang, and Kim (2013); Balakrishnan, Billings, Kelly, and Ljungqvist (2014); Heinle and Verrecchia (2016); Bourveau and Schoenfeld (2017); Cho, Kim, and Zang (2020); Allee, Christensen, Graden, and Merkley (2021).

[2] As defined by Bloomfield et al. (2023), a "price-peer" is an RPE peer used in stock price-based RPE grant.

a manner consistent with deliberate preemption.

Further supporting a preemption interpretation, we also document that managers' forthcoming disclosure practices appear to be related to impending potential leaks, but are not related to any leaks that may have recently occurred. Moreover, when we re-examine the sabotage patterns documented in Bloomfield et al. (2023), we find that instances of harm towards price-peers are significantly lessened for price-peers that had already issued their own preemptive disclosures.

Collectively, the empirical evidence in our study is consistent with disclosure preemption. When facing heightened leak risks, managers become more forthcoming about their own bad news, and appear to accelerate their disclosures so as to preempt leaks. Preemption is effective in the sense that it prevents leaks from occurring and/or lessens the capital market effects of future leaks (e.g., because the information is already impounded into prices). The evidence in our study does not speak to the effect of managers' disclosure practices on long-run valuation outcomes—we cannot say whether preempting leaks results in an overall higher valuation, or if preemption simply accelerates the valuation effects to the firm's (earlier) disclosure date.

Combined with our model, the empirical evidence suggests that managers care about the channel through which their bad news reaches the market. Based on our model, the patterns we document would not be expected to arise unless managers have sufficiently strong preferences for being the bearers of their firms' bad news. There are many potential reasons why managers might wish to preempt leaks: litigation concerns, controlling the narrative, costly signaling, etc. Ascertaining *why* managers care about being the initial bearer of their firms' negative information lies beyond the intended scope of this study, but we hope future work is able to shed light on the matter.

Our study makes several contributions. First and foremost, our study contributes to the literature on voluntary disclosure as one of the first to examine how managers' voluntary disclosure practices change in response to the possibility of bad news leaks. On the analytical front, we model voluntary disclosure in the face of a leak and demonstrate that the manager's response to leak risk hinges on their desire to be the bearer of their own bad news. We show that if managers are not concerned about the source of bad news, the possibility of a leak makes them less forthcoming with their own bad news, while the opposite is true if managers prefer to be the first to disclose. Related work also considers the possibility of leak risk influencing voluntary disclosure: Ebert, Schäfer, and Schneider (2022) shows that ex ante responses to leak risk depend on the managers' ability to respond ex post, while Heinle, Kim, and Verrecchia (2020) shows that, under certain conditions, leak risk can give rise to a two-threshold disclosure policy.

On the empirical front, we provide initial evidence regarding how managers alter their own voluntary disclosure practices, in anticipation of a potential leak. We document that managers become more forthcoming with their own company's bad news when the likelihood of a leak is greater. Moreover, managers appear to accelerate their own disclosures, moving them forward by several days, seemingly to preempt potential leaks. Prior literature has examined related topics such as the effect of whistle-blowing risk on corporate fraud (e.g., Berger and Lee, 2022), and the effect of activist short sellers' "short attack" campaigns on targeted firms' subsequent disclosures practices (e.g., Brendel and Ryans, 2021). However, no prior work has documented the effect of leak risk on *preemptive* voluntary disclosure practices.

Our study also contributes to the growing literature on RPE and sabotage[3] by documenting how potential sabotage targets react in anticipation of being harmed by the companies using them as RPE peers. Building on Bloomfield et al. (2023), who show that rTSR-using companies regularly disclose non-public negative information about their RPE price-peers, we document that these price-peers appear to anticipate being targeted, and in many instances will preempt the rTSR-using firms' harmful disclosures with voluntary disclosures of their own. This evidence suggests

[3]See, e.g., Dye (1984); Lazear (1989); Gibbons and Murphy (1990); Bloomfield, Guay, and Timmermans (2022); Bloomfield, Marvao, and Spagnolo (2023); Feichter, Moers, and Timmermans (2022); Bloomfield et al. (2023); Bloomfield (2023); Bloomfield, Friedman, and Kim (2024)

that RPE peers are aware of their inclusion in these RPE peer groups and understand the potential ramifications of their inclusion status, and anticipatorily change their own behavior in response.

Lastly, our study contributes more broadly to the accounting literature on information spillovers and cross-firm strategic interdependencies. Ample prior literature shows that firms' disclosures can be informative about other companies, influencing their stock price valuations, cost of capital, and investment decisions (e.g., Firth, 1976; Foster, 1981; Badertscher, Shroff, and White, 2013; Shroff, Verdi, and Yost, 2017; Roychowdhury, Shroff, and Verdi, 2019). Moreover, prior work demonstrates how one company's accounting policies (e.g., reporting, disclosure and/or incentive design practices) can affect *other* firms' strategic behavior. For example, prior studies on disclosure interdependencies document that one firm's disclosure decisions can influence other firms' voluntary disclosure decisions (e.g. Breuer, Hombach, and Müller, 2022; Dye and Sridhar, 1995). Another stream of work shows how firms' accounting practices (e.g., compensation incentives and/or disclosure policies) can be used to soften competition from current or potential product market rivals (e.g., Fershtman and Judd, 1987; Aggarwal and Samwick, 1999; Li, 2010; Vrettos, 2013; Bloomfield and Tuijn, 2019; Glaeser and Landsman, 2021; Bourveau, She, and Žaldokas, 2020; Bloomfield, 2021). Our study contributes to these strands of literature by showing that firms' voluntary disclosure practices can be influenced by other company's compensation practices. Specifically, we find that firms become more forthcoming in their voluntary disclosure practices when they are included as price-peers in other companies' rTSR peer groups.

The remainder of this paper is organized as follows. In Section 2, we develop the models of voluntary disclosure from which our empirical predictions are derived; in Section 3, we detail our data sources, sample selection and variable construction procedures; in Section 4, we present our empirical analyses and findings; and in Section 5, we conclude. In Appendix A, we provide proofs for all propositions stated in Section 2.

2 A Model of Voluntary Disclosure with Possible Leaks

In this section, we present a model of voluntary disclosure when the manager's information can be leaked. We start by adding the possibility of a leak to the classic framework of Dye (1985) and Jung and Kwon (1988). Next, we introduce incentives for the manager to be the initial messenger of bad news and show how this effect changes the manager's voluntary disclosure choices. Proofs for all propositions can be found in Appendix A.

2.1 Voluntary Disclosure and Leak Risk

A manager governs a firm, which is traded in a perfectly competitive market, and aims to maximize the firm's price. The firm's terminal value is $\pi = x, \tilde{x} \sim U[-1, 1]$. With a probability $0 < k < 1$, the manager privately observes x and can credibly disclose her information to investors.

This setup resembles one in Dye (1985) and Jung and Kwon (1988), except for one element: the presence of an adversarial 'leaker.' When $x \in [-1, 0)$, if the manager has not disclosed her information, there is a $0 < l < 1$ probability that x will be credibly leaked by an external party.

The timing of events is as follows. At time $t = 0$, the manager (if she is informed) can disclose her information x to investors. At time $t = 1$, if no disclosure occurred at time $t = 0$, x can be leaked. At time $t = 2$, the market prices the firm at its expected value given all the information received: $P = E[\tilde{x}|\Omega_2]$. Ω_2 denotes investors' information at time $t = 2$, which can be the information disclosed, either by the manager or by the leaker, or the fact that no disclosure and/or no leak occurred. We conjecture that in equilibrium, an informed manager will voluntarily disclose iff x is above a certain threshold, $x \geq \bar{t}$.

The firm's price is formed as follows. If the manager discloses her information, the price will simply equal to the disclosure: $P(D) = x$. If the manager does not disclose but the information gets leaked, the price is the same: $P(ND, L) = x$. Finally, if the manager does not disclose and no leak occurs, the market forms a rational expectation by considering the following three possibilities:

Possibility	**Description**	**Probability**	**Expected value**
A	Manager uninformed & there is no leak & $x < 0$	$\frac{1}{2}(1-k)(1-l)$	$-\frac{1}{2}$
B	Manager uninformed & there is no leak & $x \geq 0$	$\frac{1}{2}(1-k)$	$\frac{1}{2}$
C	Manager informed & does not disclose & there is no leak	$k(1-l)\frac{t+1}{2}$	$\frac{t-1}{2}$

To find the equilibrium disclosure strategy, one needs to find the value of x at which an informed manager is indifferent between disclosing and staying silent:

$$t = l \times t + (1-l) \times \frac{\frac{1-k}{4} - \frac{(1-k)(1-l)}{4} + \frac{(t^2-1)k(1-l)}{4}}{\frac{1-k}{2} + \frac{(1-k)(1-l)}{2} + \frac{(t+1)k(1-l)}{2}}, \tag{1}$$

which can be re-written as

$$t = \frac{\frac{1-k}{4} - \frac{(1-k)(1-l)}{4} + \frac{(t^2-1)k(1-l)}{4}}{\frac{1-k}{2} + \frac{(1-k)(1-l)}{2} + \frac{(t+1)k(1-l)}{2}}, \tag{2}$$

a condition that is very similar to Dye (1985) and Jung and Kwon (1988) except for the fact that investors update their beliefs about the firm's value based on the fact that no leak occurred. Since a leak is only possible for firms with negative values $x < 0$, upon no leak investors put a higher weight on the chance the firm has a positive value.

The lemma below describes the equilibrium.

Lemma 1. *In the classic voluntary disclosure model with a probability of a leak, there exists a unique threshold,* $\bar{t} \in (-1, 1)$ *solving (2), such that a manager with information* $x \geq \bar{t}$ *(*$x < \bar{t}$*) will (will not) disclose.*

We next characterize how the equilibrium disclosure threshold (and thus equilibrium disclosure policy) varies with l. We find that, the higher the probability of a leak, the higher the firm's price given no disclosure and no leak, and thus the higher the disclosure threshold. We formalize this result in the proposition below.

Proposition 1. *In the classic voluntary disclosure model with a probability of a leak, the optimal disclosure threshold is increasing in the probability of a leak, i.e., the manager is less likely to disclose when a leak is more likely:*

$$\frac{\partial \bar{t}}{\partial l} > 0.$$

Intuitively, when investors price the firm the same irrespective of who provided the information, the possibility of a leak makes a rational manager withhold more information, in equilibrium. The reasoning is the following. Because leaks reveal negative information, the non-disclosure/non-leak price is higher than the non-disclosure price absent leak risk. A manager with bad news ($x < 0$) therefore is more inclined to withhold the information, either the truth is revealed through a leak (making the manager no worse off than disclosing the news) or the manager receives the (higher) non-disclosure price.

Proposition 1 yields multiple testable predictions. Specifically, a higher probability of a leak will be associated with: (1) less frequent voluntary disclosures; and (2) more positive disclosure-day returns, on average.

2.2 Voluntary Disclosure and Leak Risk when Leaks are Costly

In the previous section, we considered a model where the source of information is irrelevant—managers are indifferent to disclosing information versus having the information leaked. We next consider the possibility that the source of the information matters, such that leaks are costly to the manager. In practice, leaks are likely to be costly for many reasons. For example, leaks may increase litigation risk, or prevent managers from "controlling the narrative." Alternatively, the manager may use the disclosure decision itself as a signaling mechanism—an opportunity that is taken away by a leak. In this section, we introduce the cost of a leak and study how it shapes the manager's disclosure choice.

We operationalize costly leaks by assuming that in the event of a leak, the firm's terminal value is reduced by $c > 0$ from x to $x - c$. The cost of a leak c can be interpreted, for example, as investors pricing in the damage to the firm from the leak (e.g., litigation cost). Alternatively, with a slight change to our analysis, one could also consider c to represent a direct cost of a leak to the manager, that need not be valuation relevant.[4] With this assumption, the equilibrium condition changes to:

$$t = l \times (t - c) + (1 - l) \times \frac{\frac{1-k}{4} - \frac{(1-k)(1-l)}{4} + \frac{(t^2-1)k(1-l)}{4}}{\frac{1-k}{2} + \frac{(1-k)(1-l)}{2} + \frac{(t+1)k(1-l)}{2}}. \tag{3}$$

The lemma below describes the equilibrium.

Lemma 2. *In the voluntary disclosure model with a probability of a leak and a cost of the leak, there exists a single threshold, solving (3), $-1 \leq \bar{\bar{t}} < 0$ such that a manager with information $x \geq \bar{\bar{t}}$ ($x < \bar{\bar{t}}$) will (will not) disclose.*[5]

When a leak of information is costly for the manager, her equilibrium disclosure threshold is determined by two countervailing forces. On the one hand, as shown in Section 2.1, when the leak is more likely, the firm's price given no disclosure and no leak increases (the term $\frac{\frac{1-k}{4} - \frac{(1-k)(1-l)}{4} + \frac{(t^2-1)k(1-l)}{4}}{\frac{1-k}{2} + \frac{(1-k)(1-l)}{2} + \frac{(t+1)k(1-l)}{2}}$ on the right-hand side of (3) increases in l[6]). On the other hand, when the leak is more likely, the probability of bearing the cost of the leak increases (term $(t - c)$ on the right-hand side of (3)). When the cost of the leak c is sufficiently high, the latter force dominates the former: the manager will disclose more information in equilibrium as the probability of a leak increases. The proposition below formally states the result.

[4]Such an alteration does not qualitatively affect the analysis.

[5]When the probability of a leak is large enough, $l \geq \frac{5+4c-\sqrt{(4c+1)^2+8}}{2+4c}$, an informed manager will always disclose their information (i.e., the disclosure threshold, $\bar{\bar{t}}$, reaches -1, such that no realizations of x are below the threshold).

[6]The derivative of the term $\frac{\frac{1-k}{4} - \frac{(1-k)(1-l)}{4} + \frac{(t^2-1)k(1-l)}{4}}{\frac{1-k}{2} + \frac{(1-k)(1-l)}{2} + \frac{(t+1)k(1-l)}{2}}$ with respect to l is $\frac{1}{8}\left(2 + k^2(-1+t)t + k(-2+t-t^2)\right)$, which is positive for any $k \in [0, 1)$.

Proposition 2. *In the voluntary disclosure model with a probability of a leak and a cost of the leak, there exists a unique value of the leak cost $\hat{c} > 0$, such that:*

$$\frac{\partial \bar{\bar{t}}}{\partial l} < 0 \quad \textit{for } c > \hat{c}$$

Proposition 2 yields multiple testable predictions. Specifically, if leaks are sufficiently costly, a higher probability of a leak will be associated with: (1) more frequent voluntary disclosures; and (2) lower disclosure-day returns, on average. Notably, these predictions are precisely the opposite of those from Section 2.1; the manager's reactions to the probability of a leak switches sign depending on how strongly she cares about preempting a possible leak. In what follows, we take these predictions to the data to ascertain which version of the model appears to be more descriptive of firms' voluntary disclosure practices.

3 Data, Sample and Variables

3.1 Data and Sample

The data for this study come from the CRSP daily returns data set, the Compustat annual fundamentals data set, the I/B/E/S voluntary disclosure data set, and the Incentive Lab data set on executive incentives. Our sample is the entire intersection of CRSP and Compustat over the period of 2006 to 2019. We further combine this sample with voluntary disclosure data from I/B/E/S and RPE peer group inclusion data from Incentive Lab. Our sample contains 16,431,064 firm-date observations, representing 69,095 firm-year observations from 8,921 unique firms.

3.2 Variables

Below we describe the construction of the variables used in our main analyses. Summary statistics can be found in Table 1.

3.2.1 Voluntary Disclosure Dates

We measure disclosure dates using data on voluntary earnings (specifically, EPS) guidance from I/B/E/S. We construct the indicator variable $Disc.\ Date$ which equals one on the first trading date for which the firm's disclosure was available. For disclosures occurring prior to market close, we code the disclosure date as $Disc.\ Date_{i,t} = 1$; for disclosures occurring after market close, we code the next trading day as $Disc.\ Date_{i,t} = 1$. For some analyses, we further aggregate these variables, at the firm-year level, to create the variable $\#Discs_{i,t}$ which reflects the number of voluntary earnings forecasts issued throughout the year. Due to the skewness of this variable, we use its natural logarithm in our analysis. Given that many firms provide no guidance, we further add one before taking the logarithm. In robustness analyses, we construct alternative versions of these variables that include any type of voluntary forecast (e.g., Sales, CapEx, cash flow, etc.), and not just EPS forecasts.

3.2.2 RPE Peer Status

We construct two primary measures of firms' status as RPE peers: $\#Price$ and $\#Profit$. $\#Price_{i,t}$ is equal to the number of RPE-using companies that use firm i at time t as a peer for a price-based RPE grant, as reflected by Incentive Lab. Analogously, $\#Profit_{i,t}$ is equal to the number of RPE-using companies that use firm i at time t as a peer for a profit-based RPE grant, as reflected by Incentive Lab.[7]

[7] Incentive Lab only covers the largest ~1,000 publicly listed companies, so our measures only reflect inclusion in large companies RPE peer groups. To the extent that smaller companies also use RPE, our measure likely understates the number of companies using any given firm as an RPE peer.

$\#Price_{i,t}$ is our primary measure of leak risk; being included in more price-based RPE grants indicates that more companies have a vested interest in depressing the firm's stock price by leaking bad news (Bloomfield et al., 2023). $\#Profit_{i,t}$ is a placebo/control variable—it has similar determinants to $\#Price_{i,t}$ but does not pose leak risk in the same manner as $\#Price_{i,t}$. Due to the skewness of these measures, we use their natural logarithms in our analyses. Given that many firms are not included in any RPE peer groups (i.e., $\#Price_{i,t}$ and $\#Profit_{i,t}$ frequently equal zero), we add one before taking the natural logarithm. We also construct an extensive margin variant of $\#Price_{i,t}$, $Price\ Peer_{i,t}$, as an indicator variable equal to one if firm i is included as a price-peer in at least one price-based RPE grant at time t.

To refine $\#Price_{i,t}$ as a proxy for leak risk, we further construct measures that factor in the rTSR-using companies' disclosure dates. Specifically, we consider the set of all companies using firm i and an rTSR price-peer at time t, and construct the variables $\#Upcoming\ Discs_{i,t}$ and $\#Recent\ Discs_{i,t}$. $\#Upcoming\ Discs$ reflects the number of earnings forecasts by those rTSR-using companies, in the 10 trading days after date t, and $\#Recent\ Discs$ reflects the number of earnings forecasts by those rTSR-using companies, in the 10 trading days before date t. As such, $\#Recent\ Discs$ captures leaks that may have already occurred and thus cannot be preempted at time t, while $\#Upcoming\ Discs$ captures leaks that may occur in the near future, and thus can be preempted at time t. As with $\#Price$, we use the natural logarithm of these measures due to their skewness, and add one before taking the logarithm to account for the large fraction of observations for which the number of relevant disclosures is equal to zero.

3.2.3 Stock Performance

We measure stock performance using daily returns, as reflected in CRSP. $Return_{i,t}$ reflects the daily return for firm i at date t. To mitigate kurtosis, we winsorize this variable at 1% and 99%.

4 Empirical Analysis

4.1 Empirical Strategy

We seek to understand how firms' exposure to leak risk influences their disclosure policies. In particular, we focus on two primary aspects of disclosure policy: (1) disclosure frequency; and (2) disclosure-day performance. If firms become more forthcoming in the face of leak risk (i.e., the disclosure threshold decreases), then leak risk should lead to more frequent disclosures which convey, on average, worse news. If firms become less forthcoming in the face of leak risk (i.e., the disclosure threshold increases), then leak risk should lead to less frequent disclosures which convey, on average, better news. In supplemental tests, we further seek to understand the timing of firms' disclosures (i.e., whether they occur earlier or later than usual, as a function of leak risk) as well as whether or not preemptive disclosures are effective in reducing the harm from the leak.

As an empirical matter, leak risk is a difficult construct to measure. Based on prior work by Bloomfield et al. (2023), we measure leak risk as the number of other companies that include the firm as a price-peer in an rTSR grant. Bloomfield et al. (2023) show that rTSR-using companies routinely provide peer-harming disclosures that reveal legitimate, non-public negative information about their price-peers. As such, from a price-peers' perspective, being included in another company's rTSR peer group represents a source of leak risk. The more companies that use a given firm as a price-peer, the greater is the risk of a leak.

However, inclusion in an rTSR peer group is endogenous. While firms do not, themselves, choose whether or not to be included in other companies' RPE peer groups, their inclusions/exclusions are non-random, and thus present a potential source of selection bias. Any observed relation between $log(1 + \#Price)$ and disclosure practices need not reflect a strategic response to leak risk; associations could also arise from other economic factors that happen to be correlated with inclusion in RPE peer groups. Any confounding factors arising from persistent firm characteristics and/or sample-wide (or industry-wide) time trends can easily be controlled for with firm and time (or industry-time) fixed effects. However, time-varying firm-specific factors continue to be a potential concern. For example, changes in a firm's risk exposure may influence both its disclosure practices, as well as its inclusion in other companies' peer groups, leading to a spurious association.

The ideal experiment would be one in which firms' leak risk exposures were randomly varied, allowing for the identification of the causal effect of leak risk on disclosure. While we cannot perform such an experiment, we seek to approximate it in our empirical design by controlling for firms' inclusions in *profit*-based RPE peer groups. The key supposition underlying this empirical approach is the following. We posit that, while being chosen as an RPE peer is endogenous, being chosen as a price-peer versus as a profit-peer (conditional on being selected as a peer) is plausibly exogenous, and that the primary difference between the two, vis-á-vis disclosure practices, is the leak risk imposed by price-based RPE. Under this assumption, any spurious relations between peer group inclusion and disclosure practices will be picked up by the coefficient on $log(1+\#Profit)$. The incremental leak risk imposed specifically by price-based RPE will be identified by the coefficient on $log(1 + \#Price)$. That is, under this assumption, our design identifies the causal effect of $log(1 + \#Price)$ on disclosure practices, through the mechanism of leak risk.

This identifying assumption is inherently untestable (much like a parallel trends assumption). Intuitively, the assumption would be violated if RPE-using companies decided whether to include a particular firm as a price-peer versus as a profit-peer based on omitted time-varying firm-specific factors that are related to the firms' disclosure practices. We can think of no clear economic story that would constitute a violation, but we cannot rule out the possibility of such a violation. To shed some additional light on the plausibility of these assumptions, we examine the determinants of $\#Price_{i,t}$ and $\#Profit_{i,t}$, to ensure that they are similar. If the determinants are quite different, it would suggest that inclusion in price-based versus profit-based RPE peer groups reflects highly disparate economic circumstances, thus rendering our empirical strategy invalid. We regress each variable on a slate of fundamental factors, each lagged by one year: firm size (market value of equity), trading volume, book-to-market ratio, industry concentration (HHI), and financial performance (ROA and stock returns). We present the results in Table 2.

Supporting our suppositions, we find that $\#Price_{i,t}$ and $\#Profit_{i,t}$ have very similar determinants. Both are positively related to trading volume to a similar degree, negatively related to financial performance to a similar degree, and unrelated to book-to-market ratios and industry concentration. The only significant difference between the two, with respect to their determinants, is firm size. Both are highly positively related to firm size, but this association is significantly stronger for $\#Price_{i,t}$ than for $\#Profit_{i,t}$. In sum, it seems that $\#Price_{i,t}$ and $\#Profit_{i,t}$ are quite similar in terms of their determinants (with the slight exception of size), and thus $\#Profit_{i,t}$ makes a very good placebo and/or control variable for use in our empirical analyses. We reiterate that the results in Table 2, while reassuring, do not constitute proof of the validity of our empirical approach. It remains conceivable that some omitted factor could drive the choice to use a given firm as a price-peer, as opposed to as a profit peer, as well as the firm's disclosure practices. While we can think of no clear cause of concern, the results to follow should be interpreted cautiously, with this possibility in mind.

4.2 Voluntary Disclosure Frequency

We begin our empirical analysis by assessing whether being an RPE price-peer is associated with greater voluntary disclosure frequency. We first examine the relation graphically, with a plot of annual earnings forecast frequency split into groups based on the number rTSR peer groups they are included in. As shown in Figure 1, we document that firms disclose more frequently, on average, when they are included in more companies' rTSR peer groups. Firms that are not included in any rTSR peer groups offer earnings guidance less than once per year, on average, while firms included in 1-5 rTSR peer groups offer earnings guidance twice per year, on average, and firms included in 6 or more peer groups offer earnings guidance twice per year more than two-and-a-half times per year, on average.

These patterns are consistent with firms employing a more forthcoming disclosure policy when they are more exposed to leak risk (as predicted under the assumption that managers prefer to be first to disclose their bad news). However, we caution that these patterns reflect univariate comparisons; a firm's inclusion in rTSR peer groups could be related to the firm's disclosure practices for any number of reasons that need not be related to leak risk. To refine the analysis, we use variants on the following estimating equation:

$$\begin{aligned} log(1+\#Discs_{i,t}) =& \beta_1 log(1+\#Price_{i,t}) + \beta_2 log(1+\#Profit_{i,t}) \\ &+ \beta_3 log(Size_{i,t}) + \tau_t + \mu_i + \varepsilon_{i,t}. \end{aligned} \tag{4}$$

The coefficient of interest is β_1, which reflects the relation between inclusion in an rTSR peer group and earnings forecast frequency. We control for firm size, which is a major determinant of both peer group inclusion and disclosure practices. We also control for inclusion in profit-based RPE peer groups, as the determinants of inclusion in an rTSR peer group are virtually indistinguishable from the determinants of inclusion in a profit-based RPE peer group (see Table 2). This variable functions jointly as a control/placebo. If peer inclusion is related to voluntary disclosure practices due to selection considerations (and not the causal effect of inclusion in an RPE peer group), then we would expect $log(1+\#Price_{i,t})$ and $log(1+\#Profit_{i,t})$ to carry similar coefficients. In contrast, if disclosure practices are related to peer group inclusion for the reasons we model in

Section 2, we predict that the relation will manifest through $log(1 + \#Price_{i,t})$, but not through $log(1 + \#Profit_{i,t})$. Being a profit-based RPE peer does not expose a company to additional leak risk, so should not prompt any disclosure policy reaction from the peer.

We use varying fixed effect structures to control for different sources of potentially observable variation. In Specification (1), we include only year fixed effects; in Specification (2), we include year and industry (i.e., 4-digit primary SIC) fixed effects; in Specification (3), we include interacted industry-year fixed effects; in Specification (4), we include year and firm fixed effects; and in Specification (5), we include industry-year and firm fixed effects. Results are tabulated in Table 3. In Panel A, we present results for $log(1+\#Price_{i,t})$, on its own; in Panel B, we include the controls for $log(1+\#Profit_{i,t})$ and $log(Size_{i,t})$.

Across all specifications, we document that heightened leak risk is associated with greater voluntary disclosure frequency. This effect is most pronounced in the cross-section (i.e., Specifications 1 through 3), where a 10% increase in leak risk is associated with a ~1.5% increase in disclosure frequency. With firm fixed effects, we document a qualitatively similar pattern that remains statistically significant, although the within-firm economic magnitude is considerably smaller; a 10% increase in leak risk is associated with a ~0.3% increase in disclosure frequency.

This evidence is consistent with managers being more forthcoming when facing heightened leak risk. Further supporting this interpretation, we find that $log(1+\#Profit_{i,t})$ has no significant association with forecast frequency in any specification. If $log(1+\#Price_{i,t})$ were related to disclosure frequency simply due to factors that determine peer group inclusion, we would expect similar patterns to exist between $log(1+\#Profit_{i,t})$ and disclosure frequency. The stark differences across $log(1+\#Price_{i,t})$ and $log(1+\#Profit_{i,t})$ indicate that the relation is specific to inclusion in rTSR peer groups, and not RPE peer groups, in general. While we cannot say with certainty that the documented patterns are related to the forces we model, in the context of voluntary disclosure, leak risk is likely the most salient difference between price-based and profit-based RPE, from the perspective of an RPE peer.

4.3 Voluntary Disclosure Day Performance

The preceding analysis demonstrates that rTSR peers disclose more frequently. We next examine whether this effect appears to be driven by firms becoming more forthcoming about their *bad news*, in particular. To shed light on this, we test whether firms facing greater leak risk have worse stock performance on their disclosure dates.

As before, we first examine the relations graphically, with a plot of average disclosure day returns, split into groups based on the number of rTSR peer groups they are included in. As shown in Figure 2, we document that disclosure day returns are lower, on average, for companies that are included in more firms' rTSR peer groups. Companies that aren't included in any rTSR peer groups generate average disclosure-day returns of roughly 18 basis points. Companies included in 1-5 rTSR peer groups generate slightly lower disclosure-day returns of roughly 15 basis points, on average. For companies included in 6-10 rTSR peer groups, this figure drops even further to roughly 4 basis points, and companies included in 10 or more rTSR peer groups generate slightly *negative* average disclosure day returns of around -1 to -2 basis points.

We next evaluate the relation between leak risk and disclosure day returns using variants on the following regression specification:

$$\begin{aligned} Return_{i,t} = {} & \beta_1 log(1{+}\#Price_{i,t}) \times Disc.\ Day_{i,t} + \beta_2 log(1{+}\#Profit_{i,t}) \times Disc.\ Day_{i,t} \\ & + \beta_3 Disc.\ Day_{i,t} + \beta_4 log(1{+}\#Price_{i,t}) + \beta_5 log(1{+}\#Profit_{i,t}) + \beta_6 log(Size_{i,t}) + \tau_t + \mu_i + \varepsilon_{i,t}. \end{aligned} \tag{5}$$

Results are tabulated in Table 4. In Panel A, we examine $log(1{+}\#Price_{i,t})$ and its interaction with $Disc.\ Day$, without controlling for $\#Profit$ or $log(Size)$. In Panel B, we further include $log(1{+}\#Profit)$ as a control/placebo, and control for $log(Size)$. As in Table 3, we use a variety of fixed effect structures. In Specification (1), we include year-month and industry fixed effects; in Specification (2), we include interacted year-month-industry fixed effects; in Specification (3), we include date and industry fixed effects; in Specification (4), we include interacted date-industry fixed effects; in Specification (5), we include year-month and firm fixed effects; in Specification (6), we include interacted year-month-industry and firm fixed effects; in Specification (7), we include date and firm fixed effects; and in Specification (8), we include date-industry and firm fixed effects.

Across all specifications, we find that the coefficient on $log(1{+}\#Price_{i,t}) \times Disc.\ Day_{i,t}$ is negative and both statistically and economically significant. On average, disclosure day returns are more lower for firms that are included in more rTSR peer groups. In terms of the magnitude, the results suggest that a one standard deviation increase in leak risk is associated with about a 3 to 4.5 basis point reduction in disclosure day returns. In contrast, $log(1{+}\#Profit_{i,t}) \times Disc.\ Day_{i,t}$ carries a null coefficient; being included in a profit-based RPE peer group seems to have no bearing on a firm's disclosure day returns.

While these results do not rely on any type of exogenous variation in leak risk, it is not clear why inclusion in an rTSR peer group would be related to disclosure day returns, while being included in a profit-based RPE peer group would not, other than through our proposed mechanism (i.e., a reaction to heightened leak risk). In particular, being in an RPE peer group is not something that a firm chooses, nor has much influence over. For the most part, a firm is included or excluded solely based on whether or not they provide some useful risk-sharing to an RPE-using firm. Moreover, in our tightest specifications (including date-SIC and firm fixed effects), we subsume any industry-level time effects in RPE usage or returns, and identify results based on within-firm variation in RPE peer group inclusion. Without appealing to our proposed story, it is not clear why, for a given firm, their disclosure day returns should be lower in the years when they are in many rTSR peer groups, and higher when they are in fewer. It is especially unclear why this pattern would be specific to rTSR peer group inclusion, and absent in the case of profit-based RPE peer groups. If instead the relation between rTSR peer group inclusion and disclosure day returns were driven spuriously by some confounding factor related to RPE peer group inclusion, such a factor would likely manifest as a spurious relation in the case of profit-based RPE peer groups, as well (which we do not observe).

Our proposed mechanism (i.e., a response to heightened leak risk) neatly explains these patterns with a simple and parsimonious model. Moreover, these findings comport with those of Table 3—both sets of results indicate the same underlying behavior: when facing heightened leak risk, firms become more forthcoming in their disclosure policies, lowering the threshold for voluntary disclosure. This lowered threshold results in disclosures that are more frequent and more negative, on average.

To further shed light on this proposed mechanism, we probe deeper into our model's testable implications. If firms react to leak risk– as posited– and hence become more forthcoming, then this behavior should only be related to potential impending leaks, and not leaks that already happened (or already failed to materialize). Only a leak that is yet to occur can be meaningfully preempted; once the leak occurs, there is no reason to be more forthcoming. With this intuition in mind, we refine the proxy for imminent leak risk by considering the timing of when the rTSR-using firms issue their disclosures. We construct the variables $log(1 + \#Upcoming\ Discs.)$ and $log(1 + \#Recent\ Discs.)$, which reflect the number of disclosures that are soon to come, or recently occurred, from the rTSR-using firms. In these tests, $log(1 + \#Upcoming\ Discs.)$ reflects heightened leak risk to which a firm may be inclined to react (e.g., via leak preemption), while $log(1 + \#Recent\ Discs.)$ reflects leak opportunities that already occurred, and thus can no longer be preempted.[8]

We present the results from these tests in Table 5. As in prior analyses, we use a variety of fixed effect structures: in Specification (1), we include date and industry fixed effects; in Specification (2), we include date-industry interacted fixed effects; in Specification (3), we include date and firm fixed effects; and in Specification (4), we include date-industry and firm fixed effects. Across all specifications, we find that $log(1 + \#Upcoming\ Discs) \times Disc.\ Day$ carries a significantly negative coefficient, while $log(1+\#Recent\ Discs) \times Disc.\ Day$ carries an economically smaller and statistically insignificant coefficient. That is, firms appear to become more forthcoming in response to leaks that might happen in the near future, but not in response to leaks that already occurred or failed to materialize.[9]

Collectively, the evidence is consistent with the version of the model presented in Section 2.2. When facing heightened (imminent) leak risk, managers appear to lower the threshold for voluntary disclosure, and thus issue earnings guidance more frequently, and generate lower returns on average from doing so. These findings are consistent with managers becoming more forthcoming with their bad news in order to preempt leaks, and thus suggest that managers prefer to be the initial bearers of their firms' bad news.

4.3.1 Preemption Efficacy

The prior results document that, when firms face heightened leak risk, they change their disclosure policies to be more forthcoming: they increase the frequency of disclosure, and disclose more negative news on average. Consistent with an intent to preempt leaks, firms become especially

[8] An important assumption underlying these tests is the notion that firms have a rough idea when other companies are about to issue disclosures, and can therefore change their own policies in anticipation of other companies' impending disclosures.

[9] As a sidenote, consistent with findings by Bloomfield et al. (2023) regarding the legitimacy of peer-harming disclosures, these patterns suggest that harmed firms cannot simply reverse the negative information that has been leaked about them via their own countervailing disclosures.

forthcoming shortly *before* the companies that use them as price-peers (i.e., the potential leakers) issue their own disclosures.

We next examine whether preemption appears to be effective at preventing share price damaging leaks. We do so by replicating the baseline analyses from Bloomfield et al. (2023), and splitting the sample based on whether or not the peers have recently issued their own voluntary earnings guidance. The intuition underlying these tests is the following. If managers do not preempt potential leaks, then rTSR-using firms' peer-harming disclosure strategies should be effective, and the price-peers will underperform on the rTSR-using firms' dates. In contrast, if managers preempt potential leaks with their own disclosures, then the price-peers should not subsequently underperform on the rTSR-using firms' disclosure dates, because the unfavorable information has already been impounded into stock prices. The key supposition is that rTSR-using firms' peer-harming disclosures are more likely to have been preempted when price-peers issue their own voluntary earnings guidance shortly before the focal firms' disclosure date.

We present the results from these tests in Table 6. In odd-numbered specifications, the sample is focal firm disclosure dates for which the price-peer issued their own voluntary earnings guidance within the past 10 trading days. In even-numbered specifications, the sample is focal firm disclosure dates for which the price-peer did not issue their own voluntary earnings guidance within the past 10 trading days. Consistent with the above intuition, we find that price-peers' underperformance on rTSR-using focal firms' disclosure dates is significantly reduced among price-peers that recently issued their own voluntary disclosures. On average, non-preempting price-peers underperform by roughly 30 to 40 basis points, and preemption reduces this effect by 20-25 basis points.

These results suggest that preemption is an effective means of mitigating RPE-using firms' peer-harming tactics. However, it is worth emphasizing that these results only speak to the reduction of peer harm on the rTSR-using focal firms' disclosure dates; these results do not indicate that the overall valuation impact of the negative information is reduced. One possibility is that this is a pure acceleration effect: by preempting, price-peers accelerate the revelation/pricing to their disclosure date, but the pricing impact is just as significant as it would have been on the focal firm's disclosure date, absent preemption. It is also possible that the overall valuation impact is reduced by preemption, but the evidence in our study does not allow us to weigh in on the matter. Distinguishing between these possibilities is quite difficult as it requires a comparison between the observed return patterns, and the counterfactual return patterns that would have arisen under alternative disclosure policies. Naïvely comparing return patterns for preempted and non-preempted cases would not address the issue because the preemption decision is non-random; managers likely choose whether or not to preempt in part based on how advantageous it would be to preempt, in their idiosyncratic circumstances.

4.4 Supplemental and Robustness Analyses

In this section, we discuss and present results from several supplemental analyses and robustness tests. In Section 4.4.1, we evaluate whether firms facing heightened leak risk appear to accelerate their disclosure dates to aid in preemption; in Section 4.4.2, we assess whether the results are driven by intensive versus extensive margin variation in $\#Price$; in Section 4.4.3, we test whether the results are stronger when firms are more likely to be the target of peer-harming disclosures; in Section 4.4.4, we evaluate whether our inferences are sensitive to our measurement of firms' disclosure dates; and in Section 4.4.5, we benchmark our findings against a placebo price-peer inclusions, based on the artificial peer groups constructed using the Bloomfield et al. (2022) peer selection algorithm.

4.4.1 Disclosure Timing

The preceding analyses suggest that firms become more forthcoming with bad news when facing heightened leak risk. Specifically, firms appear to lower their voluntary disclosure threshold for earnings guidance, resulting in (1) more frequent disclosures; and (2) lower disclosure day returns, on average. These results are consistent with a model of leak preemption in which managers prefer to be the first to disclose their firm's bad news. While we do not explicitly model the choice of disclosure *timing*, our model suggests an additional natural implication: conditional on choosing to offer guidance, managers who care about preempting potential leaks should prefer to accelerate their disclosures, to facilitate preemption.

This implication is difficult to test directly, as we only get to observe the realized disclosure date, and not the counterfactual date on which the disclosure might otherwise have occurred, absent the leak risk. However, we are able to test more loosely whether heightened leak risk is associated with an earlier disclosure date. We do so using variants on the following regression specification:

$$\begin{aligned} Date_{i,t} =& \beta_1 log(1{+}\#Price_{i,t}) + \beta_2 log(1{+}\#Profit_{i,t}) \\ & + \beta_3 log(\#Disc._{i,t}) + \beta_4 log(Size_{i,t}) + \tau_t + \mu_i + \varepsilon_{i,t}, \end{aligned} \tag{6}$$

where *Date* reflects how many days into the year a given disclosure occurs. As in our prior analyses, we control for $log(1{+}\#Profit_{i,t})$ as the determinants of RPE peer group inclusion are very similar across price- versus profit-based RPE grants, but our predictions are specific to price-based grants. We further control for the number of forecasts issued in the year, since this could have a mechanical relation to the average disclosure date, and also firm size which could play a role in disclosure timing through leader/follower patterns.

We present six total specifications, split into two groups of three. In the first group of specifications (one through three), we present results using year and industry fixed effects; in the second group of specifications (four through six), we present results using year and firm fixed effects. Within each group of three, the first specification includes the full sample, and the latter two specifications include subsamples based on the sign of the contemporaneous return: positive versus negative. The results are tabulated in Table 7.

We find that disclosures occur a few days earlier, on average, for firms that are included in more rTSR peer groups. This is particularly true for negative news disclosures (which are more likely to be related to the preemption of potential leaks). In contrast to some of the prior tables, we do not document a null effect of $log(1{+}\#Profit)$. Instead, we document a significantly *positive* coefficient in three out of six specifications—opposite the negative sign on $log(1+\#Price)$. The forces we examine in our model do not offer any explanation for this effect. One possibility is that firms included in profit-based peer groups worry that their earnings guidance could be used adversely for target-beating purposes by the RPE-using firms that benchmark against them (e.g., Martin and Timmermans, 2021).

4.4.2 Intensive versus Extensive Margins

We next assess whether our results are driven by intensive versus extensive margin variation in $\#Price$. To do so, we replicate Table 4 Panel A, with alternative measures or samples. To isolate intensive margin variation, we use the same $\#Price$ measure as before, but winnow the sample to include only those observations for which $\#Price \geq 1$. To isolate extensive margin variation, we use the full sample but replace $\#Price$ with the indicator variable $Price\ Peer$, which is equal to one if $\#Price \geq 1$. We present the results in Table 8. Panel A presents the intensive margin tests; Panel B presents the extensive margin tests. We find that the results in Table 4 cannot be solely attributed to either intensive or extensive margin variation. Instead, we document significant effects along both intensive and extensive margins.

4.4.3 Performance Proximity

An important supposition underlying our interpretation of the results is the notion that variation in $\#Price$ reflects variation in the probability of a leak. This supposition has theoretical and empirical support with Bloomfield et al. (2023) showing that rTSR-using firms have incentives to disclose peer-harming information about their price-peers (i.e., leak their peers' bad news), and frequently act on these incentives by disclosing negative information about their price-peers. However, even if $\#Price$ is positively related to leak risk, as presumed, this does not guarantee that the relation between $\#Price$ and disclosure practices is a result of managers' reactions to leak risk. There could plausibly be uncontrolled determinants of $\#Price$ that are responsible for the documented disclosure behaviors. Many features of our design help mitigate this possibility (e.g., controlling for $\#Profit$, and/or including a tight fixed effect structure), but we cannot entirely rule it out.

To further test the leak risk interpretation, we assess whether the negative relation between $\#Price$ and disclosure day returns is stronger when firms are more likely to be targeted by the rTSR-using companies who benchmark against them. Specifically, we look to Bloomfield et al. (2023), who demonstrate both analytically and empirically that rTSR-using firms are more likely to target price-peers whose period-to-date TSR performance is more similar to their own. The intuition is straightforward. Almost all rTSR grants are constructed as rank-order tournaments, so the benefits of harming a peer's TSR performance are greater when the harm is more likely to be marginal in the final performance ranking. Harming a peer that is insurmountably ahead or far behind is unlikely to affect the final performance rankings, while harming a peer whose performance is quite similar is far more likely to be a marginal determining factor in the final performance rankings.

With this intuition in mind, we posit that $\#Price$ more closely reflects leak risk (and thus has a stronger effect on disclosure practices) for firms whose recent TSR performance is similar to that of the rTSR-using companies that use them as a price-peer. To test this possibility, we do the following. First, we winnow the sample to include only those firms that are used as a price-peer by at least one company (i.e., ($Price\ Peer_{i,t} = 1$); the notion of performance proximity is ill-defined for any firm that isn't being used as an RPE price-peer. Second, for each firm-date observation in this subsample, we calculate the average year-to-date TSR differential between that firm and the rTSR-using companies that use them as a price-peer. We then split the sample into quartiles based on these average year-to-date TSR differentials, and code observations in the bottom quartile as "high proximity" observations and those in the top quartile as "low proximity" observations. We then replicate the analysis in Table 4 on the high proximity and low proximity subsamples, and present the results in Table 9. Panel A presents the results for the high-proximity sample; Panel B presents the results for the low-proximity sample.

Under our "leak risk" interpretation of the results, we would expect to observe a stronger negative relation between $\#Price$ and disclosure-day returns in the high proximity sample. We find that this expectation is strongly supported in the data. In Panel A (i.e., the high proximity subsample), $\#Price \times Disc.\ Day$ carries a significantly negative coefficient, that is roughly twice as large in magnitude as the corresponding coefficients from Table 8 Panel A.[10] In Panel B (i.e., the low proximity sample), the coefficient on $\#Price \times Disc.\ Day$ is statistically insignificant in all cases, and much smaller in magnitude. These results corroborate our interpretation of the prior findings, lending additional credence to the notion that $\#Price$ is related to disclosure practices through its impact on leak risk. If $\#Price$ is related to disclosure practices through some other channel,

[10] Table 8 Panel A offers a more apt comparison than Table 4, because these are intensive margin results—every observation in the Table 9 is used as a price-peer by at least one rTSR-using company.

it is unclear why its impact would depend on year-to-date performance proximity to rTSR-using companies.

4.4.4 Alternative Measure of Disclosure

In our main analyses, we measure voluntary disclosure using earnings forecasts, as reflected in the I/B/E/S guidance data set. While this is a standard measurement approach found in prior literature, our theoretical predictions are not specific to earnings forecasts, *per se*. To assess whether our inferences are sensitive to this measurement choice, we construct alternative versions of our disclosure variables that codes *any* type of forecast (e.g., sales, CapEx, cash flow, etc.) as a voluntary disclosure.

Using these alternative measures for $\#Discs$ and $Disc.\ Day$, we replicate the analyses in Tables 3 and 4, and tabulate the results in Tables 10 and 11. We find that our inferences are qualitatively unaffected by this alteration. $\#Price$ continues to be positively associated with disclosure frequency and negatively related to disclosure day returns. If anything, the results are statistically somewhat *stronger* using these broader measures of voluntary disclosure.

4.4.5 Controlling for Artificial Peer Group Inclusion

We interpret our findings as being *caused* by firms' inclusions in rTSR peer groups. While our use of tight fixed effect structures allows us to identify within-firm variation in rTSR peer group inclusion, even this residual variation is endogenous, and thus our estimates may be biased. For example, one possibility is that poor disclosure-day returns is simply a characteristic of the type of firm that is likely to be included in many rTSR peer groups, and that the actual inclusions/exclusions play no causal role. While there is no clear economic story as to why this might be the case, our results cannot rule it out.

As a step to allay these concerns, we look to the Bloomfield et al. (2022) peer selection algorithm to measure a firm's likely peer group inclusion, based on stock return comovements. Specifically, we use the Bloomfield et al. (2022) peer selection algorithm to construct RPE peer groups for each company-year in the CRSP/Compustat universe over our sample period. For each firm-year in our sample, we count the number of companies whose algorithmically-constructed "artificial" peer group includes the firm of interest. We refer to this variable as $\#Price\ Artificial$, and it captures how many rTSR peer groups a firm is likely to be included in, based purely on the firm's risk-sharing usefulness to other companies. Of note, we find that this variable is highly predictive of a firm's actual peer group inclusion; $\#Price\ Artificial$ and $\#Price$ have a correlation of $\sim$0.45 ($p < 0.01$).

We replicate the disclosure-day return analyses, including controls for $log(1{+}\#Price\ Artificial)$ and $log(1+\#Price\ Artificial) \times Disc.\ Day$. We find that $log(1+\#Price\ Artificial) \times Disc.\ Day$ has no significant association with returns, while $log(1+\#Price) \times Disc.\ Day$ continues to carry a significantly negative coefficient. That is, the negative disclosure-day effect appears to be specific to *actual* rTSR peer group inclusion; how many rTSR peer groups a firm might plausibly have been included in, based on risk-sharing considerations, does not have any noticeable relation to disclosure-day returns. These results are tabulated in Table 12.

5 Conclusion

We examine how firms alter their voluntary disclosure practices when facing the possibility of a leak. Specifically, we consider the case of an adversarial leaker that wishes to reveal a firm's bad news (e.g., a whistleblower, short-seller, rTSR-using firm, etc.) and assess how firms react to changes in the probability of a leak.

To guide our empirical tests, we first develop a parsimonious model of voluntary disclosure with leak risk, and show analytically that the manager's disclosure response hinges critically on whether or not she cares about being the initial bearer of bad news. If the manager is indifferent to the source of bad news, she reacts to leak risk by becoming less forthcoming; if she cares about the source of her firm's bad news, the opposite is true: she reacts to leak risk by becoming more forthcoming, in order to preempt potential leaks.

We take these competing predictions to the data and find strong support for the predictions that arise in the case where the manager prefers to be the bearer of bad news. Firms appear to respond to leak risk (as proxied for by the number of rTSR-using companies that use the firm as a price-peer, and thus have incentives to leak bad news about the firm) by becoming more forthcoming about their own bad news, in order to preempt leaks. Firms provide more frequent guidance and generate lower disclosure-day returns on average. Moreover, when facing heightened leak risk, firms appear to accelerate their disclosures—especially their bad news disclosures—in order to aid in leak preemption.

Our results provide initial evidence of how firms react, anticipatorily, when facing the possibility of leaks: they issue their own disclosures to preempt leaks. While our results do not directly explain *why* firms tend to follow this policy, taken in conjunction with our model, these empirical patterns suggest that managers have strong preferences for being the bearers of their firm's bad news. That is, managers act as if it would be more damaging to let leakers disclose the news first, and so try to preempt the leaks to mitigate the damage. Perhaps doing so lets firms control the narrative more effectively, or shields them from litigation costs. Alternatively, it could be that the act of voluntarily disclosing the bad news acts as a partially offsetting credible signal. We hope that future work can shed more light on *why* managers care preempt leaks, as opposed to waiting to see if/when leak occur, and responding as necessary after the fact.

A Proofs

A.1 Proof of Lemma 1

In this section, we need to prove that the equation

$$t - \frac{\frac{1-k}{4} - \frac{(1-k)(1-l)}{4} + \frac{(t^2-1)k(1-l)}{4}}{\frac{1-k}{2} + \frac{(1-k)(1-l)}{2} + \frac{(t+1)k(1-l)}{2}} = 0 \tag{7}$$

has a single root $-1 < \bar{t} < 1$.

The equation above is quadratic and can be re-written as

$$f(t) \equiv \frac{1}{4}k(1-l)t^2 + \frac{1}{2}\left(1-k+1-l\right)t + \frac{1}{4}\left(1-l-(1-k)\right) = 0$$

The discriminant is

$$D = -\frac{1}{4}(1-k)\left(-l^2 + (4+k)l - 4\right)$$

The function $h(l) \equiv -l^2+(4+k)l-4$ is a quadratic expression of l, has its maximum at $l = \frac{4+k}{2} > 1$, and $h(1) = -1+k < 0$. Therefore, for any $0 < l < 1$, $h(l) < 0$, and $D > 0$. Thus, 7 has two roots.

The minimum of the quadratic expression 7 is

$$t_0 = -\frac{(1-k)+(1-l)}{k(1-l)} < -1,$$

Implying that one of the roots is less than -1. Moreover,

$$f(1) = \frac{1}{4}(1-l-(1-k)) + \frac{1}{2}(1-k+1-l) + \frac{1}{4}k(1-l) > 0$$

Therefore, $f(t)$ has a root between -1 and 1.

A.2 Proof of Proposition 1

Re-write the equilibrium condition

$$g(t,l) \equiv t\left(\frac{1-k}{2} + \frac{(1-k)(1-l)}{2} + \frac{(t+1)k(1-l)}{2}\right) - \left(\frac{1-k}{4} - \frac{(1-k)(1-l)}{4} + \frac{(t^2-1)k(1-l)}{4}\right) = 0$$

$$\frac{\partial g(t,l)}{\partial l} = -\frac{1}{4}(1-k) - \frac{1}{4}k(1-t^2) - t\left(\frac{1}{2}(1-k) + \frac{1}{2}k(1+t)\right) = -\frac{3}{4} - \frac{1}{4}kt^2 < 0$$

In addition, as shown in the Appendix A.1, for $-1 < t < 1$, the function $g(t,l)$ is increasing in t for any $0 < l < 1$.

Using the Implicit Function Theorem,

$$\frac{\partial t}{\partial l} = -\frac{\frac{\partial g(t,l)}{\partial l}}{\frac{\partial g(t,l)}{\partial t}} > 0.$$

A.3 Proof of Lemma 2

The equilibrium condition 3 can be re-written as a quadratic equation of t:

$$f(t) \equiv \frac{1}{4}k(1-l)^2t^2 + \frac{1}{2}(1-l)\left((1-l) + (1-k) + klc\right)t + \frac{1}{4}\left(l(1-k) + 2cl(1-l) + 2cl(1-k) + l^2 + k\right) = 0 \tag{8}$$

The function $f(t)$ has a minimum at $t_0 = -\frac{(1-l)+(1-k)+klc}{k(1-l)} < -1$. Furthermore, $f(0) = \frac{1}{4}\left(l(1-k) + 2cl(1-l) + 2cl(1-k) + l^2 + k\right) > 0$. Finally,

$$f(-1) = -\frac{1}{4}(1-k)\left(l^2(1+2c) - l(5+4c) + 4\right)$$

The quadratic expression of l, $l^2(1+2c)-l(5+4c)+4$, is positive for $l=0$, negative for $l=1$, and has its minimum at $l_0=\frac{5+4c}{2+4c}>1$. $f(-1)$ is negative when $l<\frac{5+4c-\sqrt{(4c+1)^2+8}}{2+4c}$.

The conclusions above imply that for $l<\frac{5+4c-\sqrt{(4c+1)^2+8}}{2+4c}$, there exists a single root of 8 between -1 and 0.

A.4 Proof of Proposition 2

Consider the equilibrium condition 3:

$$g(t,l)\equiv -(t-l(t-c))+(1-l)\times\frac{\frac{1-k}{4}-\frac{(1-k)(1-l)}{4}+\frac{(t^2-1)k(1-l)}{4}}{\frac{1-k}{2}+\frac{(1-k)(1-l)}{2}+\frac{(t+1)k(1-l)}{2}}=0$$

$$\frac{\partial g(t,l)}{\partial l}=t-c-\frac{N}{D}+(1-l)\frac{\left(\frac{1-k}{4}-\frac{k(t^2-1)}{4}\right)D-\left(-\frac{1-k}{2}-\frac{k(1+t)}{2}\right)N}{D^2},$$

where $N=\frac{1-k}{4}-\frac{(1-k)(1-l)}{4}+\frac{(t^2-1)k(1-l)}{4}$ and $D=\frac{1-k}{2}+\frac{(1-k)(1-l)}{2}+\frac{(t+1)k(1-l)}{2}$.

$$\frac{\partial g(t,l)}{\partial t}=-1+l+(1-l)\frac{\frac{k(1-l)t}{2}D-\frac{k(1-l)}{2}N}{D^2}$$

Use the equilibrium condition 3, substitute $N=\frac{(t-l(t-c))D}{1-l}$ into the equation above, and obtain

$$\frac{\partial g(t,l)}{\partial t}=-1+l-\frac{1}{2D}kl(1-l)c<0$$

Using the Implicit Function Theorem,

$$\frac{\partial t}{\partial l}=-\frac{\frac{\partial g(t,l)}{\partial l}}{\frac{\partial g(t,l)}{\partial t}}=-\frac{t-c-\frac{N}{D}+(1-l)\frac{\left(\frac{1-k}{4}-\frac{k(t^2-1)}{4}\right)D-\left(-\frac{1-k}{2}-\frac{k(1+t)}{2}\right)N}{D^2}}{-1+l-\frac{1}{2D}kl(1-l)c}$$

$\frac{\partial t}{\partial l}$ is a linear function of c which equals zero when $c = t - \frac{N}{D} + (1-l)\frac{\left(\frac{1-k}{4} - \frac{k(t^2-1)}{4}\right)D - \left(-\frac{1-k}{2} - \frac{k(1+t)}{2}\right)N}{D^2}$.

$$\frac{\partial\left(\frac{\partial t}{\partial l}\right)}{\partial c} = \frac{1}{-1 + l - \frac{1}{2D}kl(1-l)c} < 0.$$

Therefore, for any $c > \hat{c} = t - \frac{N}{D} + (1-l)\frac{\left(\frac{1-k}{4} - \frac{k(t^2-1)}{4}\right)D - \left(-\frac{1-k}{2} - \frac{k(1+t)}{2}\right)N}{D^2}$, $\frac{\partial t}{\partial l} < 0$.

References

Aggarwal, R. K. and A. A. Samwick (1999). Executive compensation, strategic competition, and relative performance evaluation: Theory and evidence. *The Journal of Finance 54*(6), 1999–2043.

Allee, K. D., T. E. Christensen, B. S. Graden, and K. J. Merkley (2021). The genesis of voluntary disclosure: An analysis of firms' first earnings guidance. *Management Science 67*(3), 1914–1938.

Anilowski, C., M. Feng, and D. J. Skinner (2007). Does earnings guidance affect market returns? the nature and information content of aggregate earnings guidance. *Journal of accounting and Economics 44*(1-2), 36–63.

Badertscher, B., N. Shroff, and H. D. White (2013). Externalities of public firm presence: Evidence from private firms' investment decisions. *Journal of Financial Economics 109*(3), 682–706.

Balakrishnan, K., M. B. Billings, B. Kelly, and A. Ljungqvist (2014). Shaping liquidity: On the causal effects of voluntary disclosure. *the Journal of Finance 69*(5), 2237–2278.

Berger, P. G. and H. Lee (2022). Did the dodd–frank whistleblower provision deter accounting fraud? *Journal of Accounting Research 60*(4), 1337–1378.

Bloomfield, M. J. (2021). Compensation disclosures and strategic commitment: Evidence from revenue-based pay. *Journal of Financial Economics 141*(2), 620–643.

Bloomfield, M. J. (2023). Performance pay plans, power and product prices. *Available at SSRN 4419019*.

Bloomfield, M. J., H. L. Friedman, and H. Y. Kim (2024). Common ownership, executive compensation, and product market competition. *The Accounting Review 99*(2), 31–56.

Bloomfield, M. J., W. R. Guay, and O. Timmermans (2022). Relative performance evaluation and the peer group opportunity set. *Available at SSRN 3853940*.

Bloomfield, M. J., M. S. Heinle, and O. Timmermans (2023). Relative performance evaluation and strategic peer-harming disclosures. *Available at SSRN 3936860*.

Bloomfield, M. J., C. Marvao, and G. Spagnolo (2023). Relative performance evaluation, sabotage and collusion. *Journal of Accounting and Economics*, 101608.

Bloomfield, M. J. and M. Tuijn (2019). Do firms strategically announce capacity expansions in response to heightened entry threats? *Available at SSRN 3195932*.

Bourveau, T. and J. Schoenfeld (2017). Shareholder activism and voluntary disclosure. *Review of Accounting Studies 22*, 1307–1339.

Bourveau, T., G. She, and A. Žaldokas (2020). Corporate disclosure as a tacit coordination mechanism: Evidence from cartel enforcement regulations. *Journal of Accounting Research 58*(2), 295–332.

Brendel, J. and J. Ryans (2021). Responding to activist short sellers: Allegations, firm responses, and outcomes. *Journal of Accounting Research 59*(2), 487–528.

Breuer, M., K. Hombach, and M. A. Müller (2022). When you talk, i remain silent: Spillover effects of peers' mandatory disclosures on firms' voluntary disclosures. *The Accounting Review 97*(4), 155–186.

Cho, Y. J., Y. Kim, and Y. Zang (2020). Information externalities and voluntary disclosure: Evidence from a major customer's earnings announcement. *The Accounting Review 95*(6), 73–96.

Ciftci, M. and F. M. Salama (2018). Stickiness in costs and voluntary disclosures: Evidence from management earnings forecasts. *Journal of Management Accounting Research 30*(3), 211–234.

Dye, R. A. (1984). The trouble with tournaments. *Economic Inquiry 22*(1), 147.

Dye, R. A. (1985). Disclosure of nonproprietary information. *Journal of Accounting Research*, 123–145.

Dye, R. A. and S. S. Sridhar (1995). Industry-wide disclosure dynamics. *Journal of Accounting Research 33*(1), 157–174.

Ebert, M., U. Schäfer, and G. T. Schneider (2022). Information leaks and voluntary disclosure.

Feichter, C., F. Moers, and O. Timmermans (2022). Relative performance evaluation and competitive aggressiveness. *Journal of Accounting Research 60*(5), 1859–1913.

Fershtman, C. and K. L. Judd (1987). Equilibrium incentives in oligopoly. *The American Economic Review*, 927–940.

Firth, M. (1976). The impact of earnings announcements on the share price behaviour of similar type firms. *The Economic Journal 86*(342), 296–306.

Foster, G. (1981). Intra-industry information transfers associated with earnings releases. *Journal of accounting and economics 3*(3), 201–232.

Francis, J., D. Nanda, and P. Olsson (2008). Voluntary disclosure, earnings quality, and cost of capital. *Journal of accounting research 46*(1), 53–99.

Gibbons, R. and K. J. Murphy (1990). Relative performance evaluation for chief executive officers. *ILR Review 43*(3), 30–S.

Glaeser, S. A. and W. R. Landsman (2021). Deterrent disclosure. *The Accounting Review 96*(5), 291–315.

Gong, G., L. Y. Li, and L. Zhou (2013). Earnings non-synchronicity and voluntary disclosure. *Contemporary Accounting Research 30*(4), 1560–1589.

Heinle, M., J. M. Kim, and R. E. Verrecchia (2020). Voluntary disclosure from multiple sources and analyst forecast accuracy. *Working Paper*.

Heinle, M. S. and R. E. Verrecchia (2016). Bias and the commitment to disclosure. *Management Science 62*(10), 2859–2870.

Hope, O.-K., T. Kang, and J. W. Kim (2013). Voluntary disclosure practices by foreign firms cross-listed in the united states. *Journal of Contemporary Accounting & Economics 9*(1), 50–66.

Hutton, A. P., G. S. Miller, and D. J. Skinner (2003). The role of supplementary statements with management earnings forecasts. *Journal of Accounting Research 41*(5), 867–890.

Jung, W.-O. and Y. K. Kwon (1988). Disclosure when the market is unsure of information endowment of managers. *Journal of Accounting research*, 146–153.

Kato, K., D. J. Skinner, and M. Kunimura (2009). Management forecasts in japan: An empirical study of forecasts that are effectively mandated. *The Accounting Review 84*(5), 1575–1606.

Kim, J. W. and Y. Shi (2011). Voluntary disclosure and the cost of equity capital: Evidence from management earnings forecasts. *Journal of Accounting and Public Policy 30*(4), 348–366.

Lazear, E. P. (1989). Pay equality and industrial politics. *Journal of Political Economy 97*(3), 561–580.

Li, X. (2010). The impacts of product market competition on the quantity and quality of voluntary disclosures. *Review of Accounting studies 15*, 663–711.

Martin, M. and O. Timmermans (2021). Managerial incentives, disclosure, and disclosure substitution: evidence from relative performance evaluation. *Disclosure, and Disclosure Substitution: Evidence from Relative Performance Evaluation (October 12, 2021)*.

Penman, S. H. (1980). An empirical investigation of the voluntary disclosure of corporate earnings forecasts. *Journal of Accounting Research*, 132–160.

Rogers, J. L., D. J. Skinner, and A. Van Buskirk (2009). Earnings guidance and market uncertainty. *Journal of Accounting and Economics 48*(1), 90–109.

Roychowdhury, S., N. Shroff, and R. S. Verdi (2019). The effects of financial reporting and disclosure on corporate investment: A review. *Journal of Accounting and Economics 68*(2-3), 101246.

Shroff, N., R. S. Verdi, and B. P. Yost (2017). When does the peer information environment matter? *Journal of Accounting and Economics 64*(2-3), 183–214.

Skinner, D. J. (1994). Why firms voluntarily disclose bad news. *Journal of Accounting Research 32*(1), 38–60.

Skinner, D. J. (1997). Earnings disclosures and stockholder lawsuits. *Journal of accounting and economics 23*(3), 249–282.

Vrettos, D. (2013). Are relative performance measures in ceo incentive contracts used for risk reduction and/or for strategic interaction? *The Accounting Review 88*(6), 2179–2212.

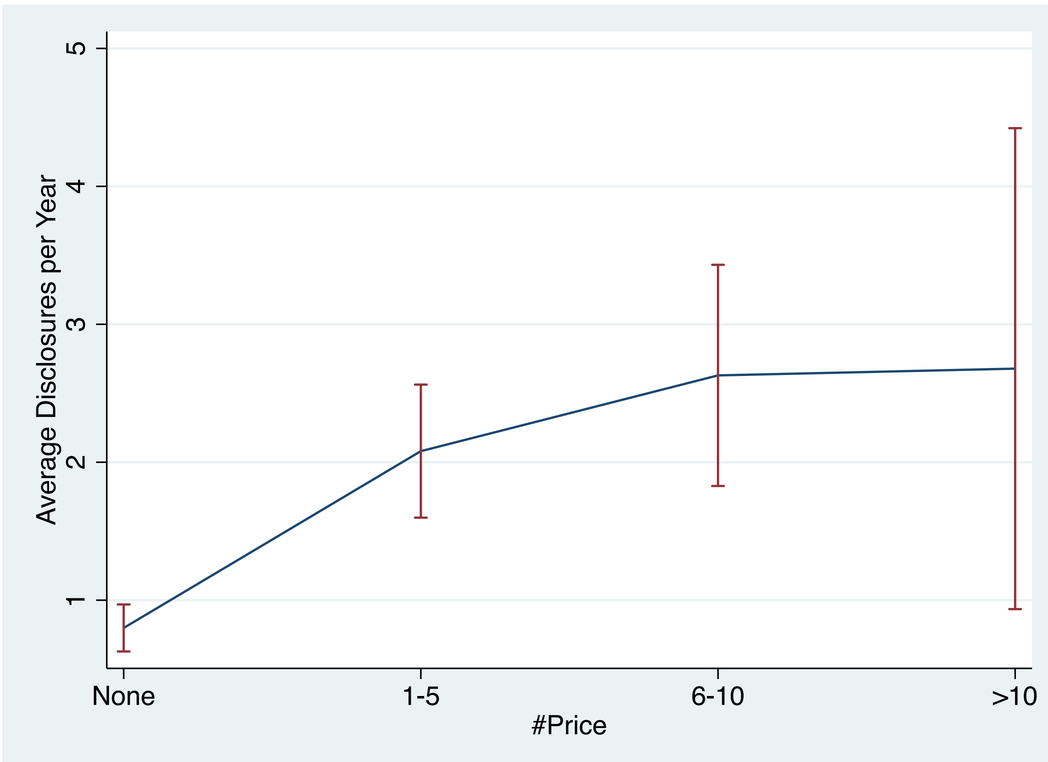

Figure 1. Voluntary Disclosure Frequency by Price-Peer Status

This figure plots the average number of voluntary earnings forecasts per year, along with a 95% confidence intervals, for four buckets of $\#Price$: $\#Price$=0; $1 \leq \#Price \leq 5$; $6 \leq \#Price \leq 10$; and $\#Price > 10$.

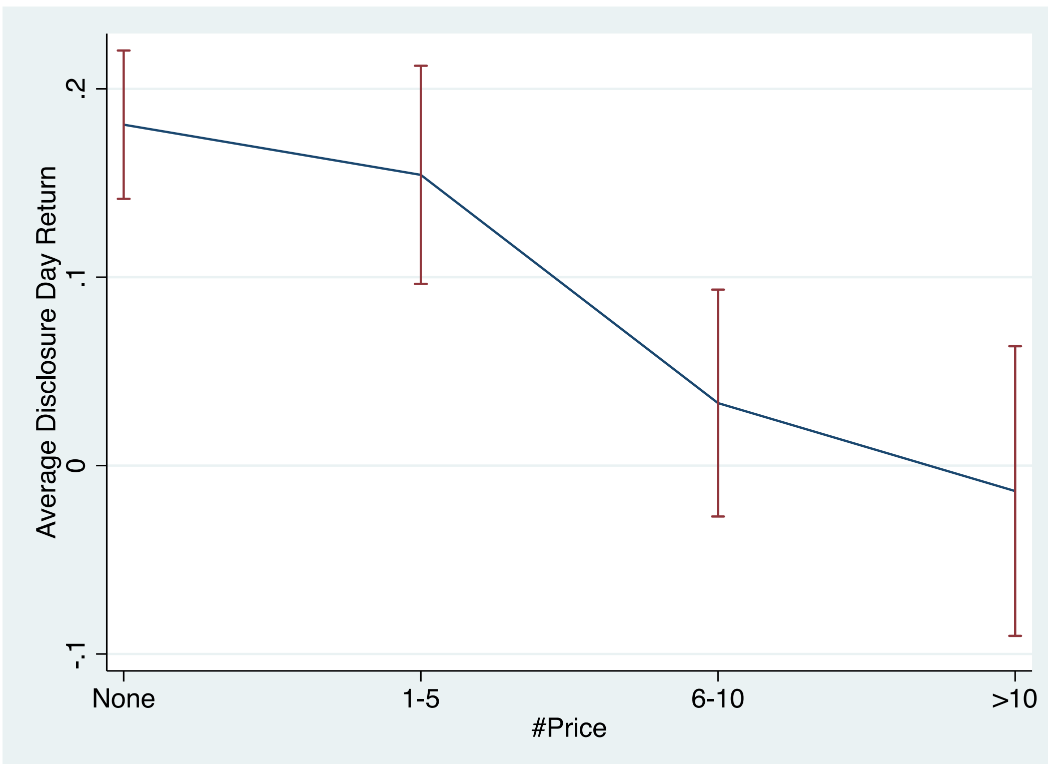

Figure 2. Voluntary Disclosure Day Returns by Price-Peer Status

This figure plots average disclosure day returns, along with a 95% confidence intervals, for four buckets of $\#Price$: $\#Price$=0; $1 \leq \#Price \leq 5$; $6 \leq \#Price \leq 10$; and $\#Price > 10$.

Table 1. Summary Statistics

This table presents summary statistics for all of the variables used in the main analyses.

Panel A: Firm-Year Level Observations

Variable	Num Obs.	Mean	SD	Q1	Med.	Q3
log(1+#Discs)	69,095	0.389	0.708	0.000	0.000	0.000
log(1+#Price)	69,095	0.226	0.546	0.000	0.000	0.000
log(1+#Profit)	69,095	0.119	0.421	0.000	0.000	0.000
log(Size)	69,095	13.202	2.069	11.720	13.160	14.612

Panel B: Firm-Date Level Observations

Variable	Num Obs.	Mean	SD	Q1	Med.	Q3
Return	16,431,064	0.021	2.955	-1.291	0.000	1.264
Disc. Day	16,431,064	0.004	0.066	0.000	0.000	0.000
log(1+#Price)	16,431,064	0.237	0.561	0.000	0.000	0.000
Price Peer	16,431,064	0.187	0.390	0.000	0.000	0.000
log(1+#Price) | Price Peer=1	3,070,913	1.271	0.610	0.693	1.099	1.609
log(1+#Upcoming Discs)	16,431,064	0.028	0.164	0.000	0.000	0.000
log(1+#Recent Discs)	16,431,064	0.028	0.164	0.000	0.000	0.000
log(1+#Profit)	16,431,064	0.125	0.436	0.000	0.000	0.000
log(Size)	16,431,064	13.237	2.084	11.750	13.204	14.657

Table 2. Determinants

This table presents evidence on the determinants of $log(1{+}\#Price)$ and $log(1{+}\#Profit)$. Below each coefficient, we report t-statistics using standard errors clustered by industry.

	(1) Outcome = log(1+#Price)	(2) Outcome = log(1+#Profit)	(3) Diff. btwn (1) and (2)
log(Size)	0.114***	0.054***	0.060***
	(11.048)	(9.069)	
log(Volume)	0.009**	0.027**	−0.018
	(2.041)	(2.315)	
log(BTM)	−0.002	0.004	0.006
	(−0.195)	(0.728)	
log(HHI)	−0.015	−0.014	−0.001
	(−0.920)	(−1.263)	
ROA	−0.083***	−0.081***	−0.002
	(−3.580)	(−3.763)	
Avg. Daily Return	−0.048***	−0.029***	−0.019
	(−2.912)	(−3.253)	
Fixed Effects	Year + SIC	Year + SIC	
Observations	56,439	56,439	
R-squared	0.398	0.227	

Table 3. Disclosure Frequency

This table presents evidence on the relation between $log(1{+}\#Price)$ and disclosure frequency. In all specifications, the dependent variable is $log(1+\#Discs)$, where $\#Discs$ is the number of voluntary earnings forecasts disclosed by the firm over during the year. Panel A, presents results without additional controls; Panel B, presents results with controls for $log(1{+}\#Profit)$ and $log(Size)$. Within each panel, specifications differ with respect to fixed effect structure: Specification (1) includes year fixed effects; Specification (2) includes year and industry fixed effects; Specification (3) includes year-industry fixed effects; Specification (4) includes firm and year fixed effects; Specification (5) includes firm and year-industry fixed effects. Below each coefficient, we report t-statistics using standard errors clustered by industry.

Panel A: No Controls

	Outcome = log(1+#Discs)				
	(1)	(2)	(3)	(4)	(5)
log(1+#Price)	0.335∗∗∗	0.323∗∗∗	0.352∗∗∗	0.039∗∗∗	0.036∗∗∗
	(5.304)	(6.723)	(6.499)	(3.565)	(3.773)
Fixed Effects	Year	Year + SIC	Year-SIC	Year + Firm	Year-SIC + Firm
Observations	69,095	69,095	69,095	69,095	69,095
R-squared	0.071	0.289	0.325	0.787	0.823

Panel B: Controlling for $log(1{+}\#Profit)$ and $log(Size)$

	Outcome = log(1+#Discs)				
	(1)	(2)	(3)	(4)	(5)
log(1+#Price)	0.141∗∗	0.143∗∗∗	0.158∗∗∗	0.028∗∗	0.029∗∗∗
	(2.079)	(3.453)	(3.391)	(2.504)	(2.937)
log(1+#Profit)	−0.032	−0.018	−0.026	0.014	0.003
	(−0.458)	(−0.427)	(−0.581)	(1.155)	(0.314)
log(Size)	0.117∗∗∗	0.117∗∗∗	0.116∗∗∗	0.064∗∗∗	0.060∗∗∗
	(14.359)	(11.657)	(11.380)	(7.755)	(7.442)
Fixed Effects	Year	Year + SIC	Year-SIC	Year + Firm	Year-SIC + Firm
Observations	69,095	69,095	69,095	69,095	69,095
R-squared	0.161	0.367	0.398	0.790	0.825

Table 4. Disclosure Day Returns

This table presents evidence on the relation between $log(1+\#Price)$ and disclosure versus non-disclosure day returns. In all specifications, the dependent variable is $Return$ Panel A, presents results without additional controls; Panel B, presents results with controls for $log(1+\#Profit)$ and $log(Size)$. Within each panel, specifications differ with respect to fixed effect structure: Specification (1) includes year-month and industry fixed effects; Specification (2) includes year-month-industry fixed effects; Specification (3) includes date and industry fixed effects; Specification (4) includes date-industry fixed effects. Specification (5) includes year-month and firm fixed effects; Specification (6) includes year-month-industry and firm fixed effects; Specification (7) includes date and firm fixed effects; Specification (8) includes date-industry and firm fixed effects. Below each coefficient, we report t-statistics using standard errors clustered by industry and date.

Panel A: No Controls

	Outcome = Return							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
log(1+#Price) x Disc. Day	−0.080***	−0.080***	−0.082***	−0.073***	−0.064***	−0.064***	−0.066***	−0.056***
	(−3.646)	(−3.701)	(−4.219)	(−3.939)	(−2.919)	(−3.001)	(−3.425)	(−3.049)
Disc. Day	0.169***	0.167***	0.196***	0.182***	0.141***	0.141***	0.169***	0.152***
	(4.750)	(4.663)	(7.896)	(7.823)	(3.962)	(3.932)	(6.827)	(6.637)
log(1+#Price)	0.015***	0.016***	0.014***	0.016***	−0.007*	−0.008**	−0.007	−0.008**
	(3.811)	(3.919)	(3.573)	(3.881)	(−1.835)	(−2.281)	(−1.537)	(−2.308)
Fixed Effects	YrMon + SIC	YrMon-SIC	Date + SIC	Date-SIC	YrMon + Firm	YrMon-SIC + Firm	Date +Firm	Date-SIC + Firm
Observations	16,431,064	16,431,064	16,431,064	16,431,064	16,431,064	16,431,064	16,431,064	16,431,064
R-squared	0.007	0.012	0.151	0.263	0.008	0.013	0.152	0.264

Panel B: Controlling for $log(1+\#Profit)$ and $log(Size)$

	Outcome = Return							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
log(1+#Price) x Disc. Day	−0.088***	−0.089***	−0.087***	−0.076***	−0.079***	−0.080***	−0.079***	−0.067***
	(−3.540)	(−3.641)	(−3.898)	(−3.627)	(−3.181)	(−3.243)	(−3.540)	(−3.188)
log(1+#Profit) x Disc. Day	0.034	0.035	0.029	0.026	0.033	0.033	0.028	0.024
	(1.005)	(1.044)	(0.962)	(0.835)	(0.976)	(0.968)	(0.922)	(0.761)
Disc. Day	0.159***	0.159***	0.183***	0.167***	0.147***	0.147***	0.173***	0.156***
	(4.564)	(4.551)	(7.698)	(7.585)	(4.202)	(4.174)	(7.169)	(7.036)
log(1+#Price)	0.005	0.009*	0.000	0.000	0.006	0.008*	0.003	0.003
	(1.065)	(1.909)	(0.072)	(0.083)	(1.169)	(1.939)	(0.500)	(0.699)
log(1+#Profit)	−0.001	−0.002	−0.003	−0.006*	−0.002	−0.002	−0.002	−0.002
	(−0.269)	(−0.619)	(−0.884)	(−1.773)	(−0.730)	(−0.494)	(−0.420)	(−0.566)
log(Size)	0.007**	0.005	0.010***	0.010***	−0.119***	−0.162***	−0.092***	−0.105***
	(2.119)	(1.481)	(3.083)	(3.204)	(−14.492)	(−15.587)	(−12.326)	(−14.337)
Fixed Effects	YrMon + SIC	YrMon-SIC	Date + SIC	Date-SIC	YrMon + Firm	YrMon-SIC + Firm	Date +Firm	Date-SIC + Firm
Observations	16,431,064	16,431,064	16,431,064	16,431,064	16,431,064	16,431,064	16,431,064	16,431,064
R-squared	0.007	0.012	0.152	0.263	0.008	0.014	0.153	0.264

Table 5. Preemption

This table presents evidence the relation between firms' voluntary disclosure returns, and the number of recent/upcoming disclosures by the rTSR-using companies using the firm as a price-peer. Specifications differ with respect to fixed effect structure: Specification (1) includes date and industry fixed effects; Specification (2) includes date-industry fixed effects. Specification (3) includes date and firm fixed effects; Specification (4) includes date-industry and firm fixed effects. Below each coefficient, we report t-statistics using standard errors clustered by industry and date.

	Outcome = Return			
	(1)	(2)	(3)	(4)
log(1 + #Upcoming Discs) x Disc. Day	−0.105∗∗∗	−0.098∗∗∗	−0.087∗∗	−0.080∗∗
	(−2.701)	(−2.910)	(−2.224)	(−2.328)
log(1 + #Recent Discs) x Disc. Day	−0.050	−0.040	−0.031	−0.022
	(−1.459)	(−1.339)	(−0.909)	(−0.722)
log(1 + #Upcoming Discs)	0.028∗∗∗	0.032∗∗∗	0.013	0.014∗∗
	(2.663)	(3.821)	(1.402)	(2.008)
log(1 + #Recent Discs)	0.021∗∗	0.031∗∗∗	0.006	0.014∗
	(2.158)	(3.538)	(0.601)	(1.811)
Disc. Day	0.176∗∗∗	0.164∗∗∗	0.151∗∗∗	0.138∗∗∗
	(7.740)	(7.749)	(6.703)	(6.599)
Fixed Effects	Date + SIC	Date-SIC	Date + Firm	Date-SIC + Firm
Observations	16,431,064	16,431,064	16,431,064	16,431,064
R-squared	0.151	0.263	0.152	0.264

Table 6. Preemption Effectiveness

This table presents evidence on preemption effectiveness. The empirical design exactly matches that of Bloomfield et al. (2023), Table 3; following Bloomfield et al. (2023), in these tests, "Firm" refers to the RPE-using companies, and "Peer" refers to the firms' RPE peers. The table is split into three specification pairs, that differ based on their cross-sectional fixed effect structure. Specifications (1) and (2) use industry and peer fixed effects; Specifications (3) and (4) use firm and peer fixed effects; Specifications (5) and (6) use firm-peer interacted fixed effects. Within each specification pair, the odd-numbered (even-numbered) specification uses the sample of firm voluntary disclosure dates for which the peer issued (did not issue) their own voluntary disclosure in the preceding 10 trading days. Below each specification pair, we present a test of the difference in coefficients on $rTSR$ across specifications. Below each coefficient, we report t-statistics using standard errors clustered by industry and date.

	Outcome = Peer Return					
	(1)	(2)	(3)	(4)	(5)	(6)
	Preempt	No Preempt	Preempt	No Preempt	Preempt	No Preempt
rTSR	−0.055	−0.276***	−0.140**	−0.352***	−0.155***	−0.400***
	(−0.866)	(−3.118)	(−2.202)	(−4.144)	(−2.956)	(−3.848)
Firm Return	0.168***	0.180***	0.170***	0.181***	0.173***	0.182***
	(8.290)	(12.451)	(8.449)	(12.459)	(8.147)	(12.038)
$\Delta\beta_1$	0.220***, t=3.543		0.212**, t=2.011		0.245**, t=2.045	
Fixed Effects	SIC + Peer + Year-Month		Firm + Peer + Year-Month		Firm-Peer + Year-Month	
Observations	19,236	62,744	19,236	62,744	19,236	62,744
R-Squared	0.158	0.228	0.167	0.239	0.200	0.294

Table 7. Disclosure Timing

This table presents evidence on the relation between $log(1+\#Price)$ and voluntary disclosure timing. In all specifications, the dependent variable is the number of days into the year a disclosure occurs. The panel is split into two groups of three specifications: Specifications (1) through (3) present results using year and industry fixed effects; Specifications (4) through (6) present results using year and firm fixed effects. Within each group of three, the first column presents results for the full sample; the second column presents results for the subsample of negative return days; the third column presents results for the subsample of positive return days. Below each coefficient, we report t-statistics using standard errors clustered by industry.

	Outcome = Disclosure Date					
	(1) Full Sample	(2) Neg. Ret.	(3) Pos. Ret.	(4) Full Sample	(5) Neg. Ret.	(6) Pos. Ret.
log(1+#Price)	−1.967***	−2.664**	−1.407	−1.212*	−3.582**	0.723
	(−2.787)	(−2.134)	(−1.366)	(−1.901)	(−2.571)	(0.585)
log(1+#Profit)	1.806***	1.486	2.188**	1.190*	2.020	0.552
	(2.882)	(1.389)	(2.049)	(1.804)	(1.521)	(0.446)
log(#Disc.)	0.669	2.723*	−1.308	0.069	1.591	−1.376
	(0.518)	(1.750)	(−0.755)	(0.043)	(0.816)	(−0.601)
log(Size)	0.278	0.842	−0.131	3.623***	4.538***	2.755*
	(0.718)	(1.610)	(−0.273)	(3.001)	(2.714)	(1.885)
$\Delta\beta_1$		-1.257, t=-0.709			-4.305*, t=-1.877	
$\Delta\beta_2$		-0.701, t=-0.401			1.468, t=0.675	
Fixed Effects	Year + SIC	Year + SIC	Year + SIC	Year + Firm	Year + Firm	Year + Firm
Observations	71,525	34,092	37,420	71,525	34,092	37,420
R-squared	0.995	0.995	0.995	0.995	0.995	0.995

Table 8. Disclosure Day Returns, Intensive versus Extensive Margins

This table presents a replication of Table 4, decomposing variation in *#Price* into intensive versus extensive margins. Panel A exploits intensive margin variation, restricting the sample to observations in which $\#Price \geq 1$. Panel B exploits extensive margin variation, using the full sample but replacing *#Price* with the indicator variable *Price Peer*, equal to one if $\#Price \geq 1$. The analysis is otherwise identical to that of Table 4 Panel A. Below each coefficient, we report t-statistics using standard errors clustered by industry and date.

Panel A: Intensive Margin

	Outcome = Return							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
log(1+#Price) x Disc. Day	−0.118***	−0.116***	−0.106***	−0.084**	−0.115***	−0.114***	−0.103***	−0.081**
	(−2.961)	(−2.946)	(−2.952)	(−2.325)	(−2.863)	(−2.873)	(−2.856)	(−2.221)
Disc. Day	0.226***	0.222***	0.219***	0.176***	0.220***	0.219***	0.214***	0.170**
	(3.142)	(3.061)	(3.529)	(2.677)	(3.049)	(2.998)	(3.432)	(2.583)
log(1+#Price)	0.002	0.002	0.002	0.002	−0.000	−0.007	−0.001	−0.007
	(0.602)	(0.531)	(0.453)	(0.450)	(−0.076)	(−1.337)	(−0.080)	(−1.405)
Fixed Effects	YrMon + SIC	YrMon-SIC	Date + SIC	Date-SIC	YrMon + Firm	YrMon-SIC + Firm	Date + Firm	Date-SIC + Firm
Observations	3,070,913	3,070,913	3,070,913	3,070,913	3,070,913	3,070,913	3,070,913	3,070,913
R-squared	0.010	0.023	0.324	0.582	0.010	0.024	0.325	0.582

Panel B: Extensive Margin

	Outcome = Return							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Price Peer x Disc. Day	−0.087**	−0.087**	−0.095***	−0.081**	−0.062*	−0.063*	−0.070**	−0.054*
	(−2.311)	(−2.331)	(−2.806)	(−2.505)	(−1.668)	(−1.725)	(−2.108)	(−1.732)
Disc. Day	0.161***	0.159***	0.190***	0.175***	0.132***	0.132***	0.162***	0.145***
	(4.444)	(4.374)	(7.509)	(7.432)	(3.650)	(3.638)	(6.447)	(6.254)
Price Peer	0.023***	0.024***	0.023***	0.024***	−0.010**	−0.011***	−0.010*	−0.011***
	(4.006)	(3.902)	(3.771)	(3.877)	(−2.476)	(−2.672)	(−1.897)	(−2.686)
Fixed Effects	YrMon + SIC	YrMon-SIC	Date + SIC	Date-SIC	YrMon + Firm	YrMon-SIC + Firm	Date +Firm	Date-SIC + Firm
Observations	16,431,064	16,431,064	16,431,064	16,431,064	16,431,064	16,431,064	16,431,064	16,431,064
R-squared	0.007	0.012	0.151	0.263	0.008	0.013	0.152	0.264

Table 9. Proximity and Disclosure Day Returns

This table presents a replication of Table 8 Panel A, splitting the sample based on average year-to-date proximity between each firm and the rTSR-using companies that use the firm as a price-peer. Panel A presents results for "high proximity" observations, defined as being in the bottom quartile of year-to-date TSR differentials; Panel B presents results for "low proximity" observations, defined as being in the top quartile of year-to-date TSR differentials. The analysis is otherwise identical to that of Table 8 Panel A. Below each coefficient, we report t-statistics using standard errors clustered by industry and date.

Panel A: High Proximity

	Outcome = Return							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
log(1+#Price) x Disc. Day	−0.231***	−0.219***	−0.198***	−0.150***	−0.234***	−0.220***	−0.199***	−0.149***
	(−3.662)	(−3.473)	(−3.397)	(−2.905)	(−3.678)	(−3.471)	(−3.380)	(−2.876)
Disc. Day	0.478***	0.469***	0.434***	0.322***	0.482***	0.473***	0.436***	0.320***
	(3.816)	(3.714)	(3.868)	(2.866)	(3.825)	(3.718)	(3.856)	(2.842)
log(1+#Price)	0.007	0.015**	0.006	0.005	0.004	0.012	0.005	−0.006
	(1.338)	(2.427)	(0.856)	(1.091)	(0.482)	(1.333)	(0.484)	(−0.689)
Fixed Effects	YrMon + SIC	YrMon-SIC	Date + SIC	Date-SIC	YrMon + Firm	YrMon-SIC + Firm	Date +Firm	Date-SIC + Firm
Observations	640,909	640,909	640,909	640,909	640,909	640,909	640,909	640,909
R-squared	0.012	0.050	0.354	0.654	0.016	0.054	0.357	0.658

Panel B: Low Proximity

	Outcome = Return							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
log(1+#Price) x Disc. Day	−0.075	−0.051	−0.099	−0.015	−0.070	−0.047	−0.093	−0.004
	(−0.616)	(−0.424)	(−0.924)	(−0.122)	(−0.575)	(−0.386)	(−0.874)	(−0.037)
Disc. Day	0.218	0.205	0.231*	0.115	0.212	0.203	0.224*	0.108
	(1.391)	(1.308)	(1.726)	(0.721)	(1.354)	(1.291)	(1.680)	(0.675)
log(1+#Price)	0.000	−0.014	−0.001	−0.007	−0.004	−0.028*	−0.009	−0.022
	(0.005)	(−1.496)	(−0.065)	(−0.659)	(−0.405)	(−1.733)	(−0.649)	(−1.349)
Fixed Effects	YrMon + SIC	YrMon-SIC	Date + SIC	Date-SIC	YrMon + Firm	YrMon-SIC + Firm	Date +Firm	Date-SIC + Firm
Observations	640,109	640,109	640,109	640,109	640,109	640,109	640,109	640,109
R-squared	0.012	0.035	0.324	0.610	0.014	0.037	0.326	0.612

Table 10. Disclosure Frequency, any forecast

This table replicates the analysis in Table 3, but uses an alternative dependent variable that considers all types of voluntary forecasts as disclosure dates (i.e., not just EPS forecasts, but also sales and/or CapEx forecasts). Below each coefficient, we report t-statistics using standard errors clustered by industry.

Panel A: No controls

	Outcome = log(1+#Discs)				
	(1)	(2)	(3)	(4)	(5)
log(1+#Price)	0.548***	0.503***	0.546***	0.097***	0.090***
	(20.199)	(21.382)	(18.944)	(7.425)	(6.729)
Fixed Effects	Year	Year + SIC	Year-SIC	Year + Firm	Year-SIC + Firm
Observations	69,095	69,095	69,095	69,095	69,095
R-squared	0.134	0.342	0.380	0.775	0.809

Panel B: Controlling for $log(1+\#Profit)$ and $log(Size)$

	Outcome = log(1+#Discs)				
	(1)	(2)	(3)	(4)	(5)
log(1+#Price)	0.292***	0.263***	0.289***	0.082***	0.079***
	(5.962)	(11.820)	(11.209)	(6.519)	(6.703)
log(1+#Profit)	−0.102	−0.064	−0.078*	0.008	−0.000
	(−1.049)	(−1.589)	(−1.827)	(0.507)	(−0.021)
log(Size)	0.168***	0.165***	0.165***	0.114***	0.110***
	(21.384)	(15.882)	(15.305)	(12.120)	(10.916)
Fixed Effects	Year	Year + SIC	Year-SIC	Year + Firm	Year-SIC + Firm
Observations	69,095	69,095	69,095	69,095	69,095
R-squared	0.260	0.448	0.480	0.780	0.813

Table 11. Disclosure Day Returns, any forecast

This table replicates the analysis from Table 4, but uses an alternative coding of *Disc. Day* that considers all types of voluntary forecasts as disclosure dates (i.e., not just EPS forecasts, but also sales and/or CapEx forecasts). Below each coefficient, we report t-statistics using standard errors clustered by industry and date.

Panel A: No Controls

	Outcome = Return							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
log(1+#Price) x Disc. Day	−0.087***	−0.086***	−0.093***	−0.091***	−0.074***	−0.074***	−0.079***	−0.077***
	(−5.195)	(−5.159)	(−6.206)	(−6.558)	(−4.405)	(−4.421)	(−5.329)	(−5.648)
Disc. Day	0.176***	0.175***	0.210***	0.196***	0.153***	0.152***	0.186***	0.171***
	(5.407)	(5.356)	(10.504)	(10.272)	(4.653)	(4.643)	(9.363)	(9.024)
log(1+#Price)	0.015***	0.016***	0.015***	0.016***	−0.007*	−0.007**	−0.007	−0.007**
	(3.908)	(4.002)	(3.666)	(3.978)	(−1.676)	(−2.096)	(−1.398)	(−2.102)
Fixed Effects	YrMon + SIC	YrMon-SIC	Date + SIC	Date-SIC	YrMon + Firm	YrMon-SIC + Firm	Date +Firm	Date-SIC + Firm
Observations	16,431,064	16,431,064	16,431,064	16,431,064	16,431,064	16,431,064	16,431,064	16,431,064
R-squared	0.007	0.012	0.151	0.263	0.008	0.013	0.152	0.264

Panel B: Controlling for $log(1{+}\#Profit)$ and $log(Size)$

	Outcome = Return							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
log(1+#Price) x Disc. Day	−0.094***	−0.094***	−0.096***	−0.090***	−0.086***	−0.086***	−0.089***	−0.083***
	(−5.090)	(−5.132)	(−5.828)	(−5.919)	(−4.658)	(−4.674)	(−5.420)	(−5.476)
log(1+#Profit) x Disc. Day	0.030	0.031	0.025	0.015	0.028	0.028	0.023	0.012
	(1.276)	(1.304)	(1.152)	(0.669)	(1.170)	(1.162)	(1.034)	(0.530)
Disc. Day	0.168***	0.169***	0.199***	0.185***	0.159***	0.159***	0.190***	0.176***
	(5.199)	(5.212)	(10.179)	(9.913)	(4.871)	(4.874)	(9.605)	(9.338)
log(1+#Price)	0.005	0.009**	0.001	0.001	0.006	0.009**	0.003	0.003
	(1.212)	(2.044)	(0.210)	(0.219)	(1.311)	(2.100)	(0.637)	(0.895)
log(1+#Profit)	−0.001	−0.002	−0.003	−0.006*	−0.003	−0.002	−0.002	−0.002
	(−0.346)	(−0.680)	(−0.931)	(−1.778)	(−0.788)	(−0.552)	(−0.453)	(−0.576)
log(Size)	0.006**	0.005	0.009***	0.010***	−0.119***	−0.163***	−0.093***	−0.105***
	(2.076)	(1.438)	(3.033)	(3.158)	(−14.495)	(−15.589)	(−12.334)	(−14.346)
Fixed Effects	YrMon + SIC	YrMon-SIC	Date + SIC	Date-SIC	YrMon + Firm	YrMon-SIC + Firm	Date +Firm	Date-SIC + Firm
Observations	16,431,064	16,431,064	16,431,064	16,431,064	16,431,064	16,431,064	16,431,064	16,431,064
R-squared	0.007	0.012	0.152	0.263	0.008	0.014	0.153	0.264

Table 12. Disclosure Day Returns, Controlling for Artificial Peer Group Inclusion

This table presents a replication of Table 4 Panel B, with additional controls for $log(1 + \#Price\ Artifcial) \times Disc.\ Day$ and $log(1 + \#Price\ Artifcial)$. The analysis is otherwise identical to that of Table 4 Panel B. Below each coefficient, we report t-statistics using standard errors clustered by industry and date.

	Outcome = Return							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
log(1+#Price) x Disc. Day	−0.075***	−0.076***	−0.075***	−0.065***	−0.070***	−0.070***	−0.070***	−0.060***
	(−3.002)	(−3.082)	(−3.319)	(−3.108)	(−2.793)	(−2.829)	(−3.115)	(−2.829)
log(1+#Price Artificial) x Disc. Day	−0.026	−0.027	−0.024	−0.021	−0.018	−0.019	−0.017	−0.014
	(−1.587)	(−1.607)	(−1.631)	(−1.514)	(−1.113)	(−1.163)	(−1.150)	(−1.016)
log(1+#Profit) x Disc. Day	0.045	0.047	0.040	0.035	0.040	0.041	0.035	0.030
	(1.281)	(1.315)	(1.255)	(1.097)	(1.147)	(1.145)	(1.111)	(0.922)
Disc. Day	0.183***	0.183***	0.206***	0.187***	0.164***	0.165***	0.189***	0.170***
	(4.527)	(4.518)	(7.109)	(6.695)	(4.055)	(4.046)	(6.492)	(6.065)
log(1+#Price)	0.002	0.005	−0.001	−0.001	0.005	0.007	0.002	0.002
	(0.359)	(0.977)	(−0.313)	(−0.309)	(0.992)	(1.618)	(0.398)	(0.522)
log(1+#Price Artificial)	0.009***	0.012***	0.006***	0.005***	0.013***	0.019***	0.008**	0.009***
	(4.359)	(5.857)	(2.612)	(2.740)	(4.129)	(5.142)	(2.549)	(2.875)
log(1+#Profit)	−0.004*	−0.006**	−0.004*	−0.007**	−0.003	−0.002	−0.002	−0.002
	(−1.740)	(−2.064)	(−1.678)	(−2.470)	(−0.893)	(−0.658)	(−0.503)	(−0.658)
log(Size)	0.004	0.002	0.008***	0.009***	−0.121***	−0.164***	−0.093***	−0.106***
	(1.453)	(0.664)	(2.690)	(2.847)	(−14.295)	(−15.461)	(−12.157)	(−14.161)
Fixed Effects	YrMon + SIC	YrMon-SIC	Date + SIC	Date-SIC	YrMon + Firm	YrMon-SIC + Firm	Date +Firm	Date-SIC + Firm
Observations	16,431,064	16,431,064	16,431,064	16,431,064	16,431,064	16,431,064	16,431,064	16,431,064
R-squared	0.007	0.012	0.152	0.263	0.008	0.014	0.153	0.264