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Relative Performance Evaluation and Strategic Peer-Harming Disclosures

MATTHEW J. BLOOMFIELD,* MIRKO S. HEINLE,* AND OSCAR TIMMERMANS†

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ABSTRACT

Many firms use relative stock performance to evaluate and incentivize their CEOs. We document that such firms routinely disclose information that harms their peers' stock prices, and sometimes explicitly mention the harmed peers, by name, in these disclosures. Consistent with deliberate sabotage, peer-harming disclosures appear to be aimed at peers whose stock price depressions are most likely to benefit the disclosing firms' CEOs. The pricing effect of these disclosures does not reverse, suggesting that the disclosures contain legitimate information regarding peers' prospects. In sum, our results suggest that relative performance evaluation in CEO pay motivates CEOs to internalize the externalities of their disclosures, and strategically disclose information that harms peers' stock prices, in order to improve their firms' relative standing within their peer group.

JEL codes: G12, G14, J33, M41

Keywords: relative performance evaluation; peer-harming actions; voluntary disclosure; sabotage; stock returns; capital markets

*The Wharton School of the University of Pennsylvania; †London School of Economics

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1. Introduction

Relative performance evaluation (hereafter "RPE") has become a common and important feature in CEO compensation plans. Although such pay plans are excellent tools for risk-sharing, an extensive theoretical literature demonstrates that RPE incentives explicitly reward, and therefore implicitly encourage, peer-harming tactics as a potentially costly side effect (e.g., Dye [1984], Lazear [1989], Aggarwal and Samwick [1999], Chen [2003], Chowdhury and Gürtler [2015]). Recent empirical work indirectly suggests that RPE-motivated sabotage occurs. For example, Bloomfield, Marvão, and Spagnolo [2023] document that the potential for sabotage appears to be an important determinant in boards' decisions regarding whether and how to use RPE in CEO pay plans. However, as yet, no empirical work documents the incidence of peer-harming actions as a consequence of using RPE in CEO pay plans. To bridge this gap, we provide evidence that firms strategically disclose information that harms their peers' stock prices in a manner consistent with deliberate sabotage.[1]

We posit that the benefits of peer-harming disclosures vary based on observable factors, and examine whether peer-harm is more prevalent when the benefits are likely to be greater. Specifically, we leverage heterogeneity in CEOs' peer-harming incentives based on the metric(s) used to assess relative performance (e.g., stock price vs. profits) and relative standings (e.g., whether the peer is performing better or worse than the focal firm). Across a battery of tests, we exploit different sources of variation in CEOs' incentives to harm some peers more than others and document a consistent pattern: the more the focal firm's CEO is likely to benefit from harming a particular peer's stock price, the worse the peer's stock performs on the focal firm's voluntary disclosure days.

In our first set of tests, we exploit variation based on the metric used to evaluate relative performance. Most RPE grants use relative total shareholder return ("rTSR") to evaluate performance on the basis of stock price; we refer to the peers used in such RPE grants as "price-peers". A sizeable minority of RPE grants evaluate performance based on accounting performance; we refer to the peers used in such RPE grants as "profit-peers". We posit that disclosure-based peer-harming tactics are more likely to be levied against price-peers than profit-peers; disclosures regularly affect other firms' stock prices, but only affect peers' profits if the disclosed information substantially alters peers' operations (e.g., changing peers' behavior, or causing issues with customers and/or suppliers). With this intuition in mind, we examine whether price-peers underperform relative to profit-peers on focal firms' voluntary disclosure days.

As our baseline empirical specification, we use a difference-in-differences design to estimate price-peers' stock return underperformance on focal

[1] Throughout, we refer to RPE-using firms as "firms" or "focal firms," and their self-selected RPE peers as "peers".

firms' voluntary earnings guidance dates. On these disclosure days, firms voluntarily provide a lot of information to an attentive capital market audience, thereby making these dates prime opportunities for peer-harming disclosures.[2] In these baseline tests, price-peers form the treated group, profit-peers form the control group, and focal firms' voluntary disclosure days constitute the treatment events. Profit-peers represent a natural control group for our study because both price-peers and profit-peers are chosen by focal firms due to their shared exposure to common sources of risk (and are therefore similarly susceptible to incidental spillovers). However, CEOs likely do not have strong incentives to use their disclosures to harm profit-peers' stock prices, as doing so confers no benefit vis-à-vis boosting incentive compensation.

We document that price-peers significantly underperform on focal firms' voluntary disclosure days. Compared to profit-peers, price-peers' daily stock returns are about 20–30 basis points lower, on average, on focal firms' disclosure days. On nondisclosure days, there is no appreciable difference between profit-peers' and price-peers' daily stock returns. These patterns remain stable under a variety of cross-sectional fixed effect structures: SIC + peer, firm + peer and firm × peer. As such, the return patterns we document are unlikely to be attributable to alternative explanations related to time-invariant firm, peer or firm-peer pair characteristics. In all analyses, we include year-month fixed effects to control for aggregate trends in disclosure, RPE usage, and stock returns. These patterns extend across industry and sector boundaries; price-peers' underperformance is evident to a similar degree among peers inside and outside the focal firm's industries/sectors. The breadth of price-peers' underperformance is difficult to reconcile with unintentional mechanisms such as incidental disclosure spillover (e.g., Firth [1976], Foster [1981]).

We next provide some descriptive evidence regarding how this strategy appears to be implemented. There are many possible disclosure strategies that focal firms could use that would give rise to the return patterns we document. One possibility is that firms selectively disclose/withhold information that is relevant to their own performance, disclosing bad news when it is worse news for their price-peers and withholding good news when it is better news for their price-peers. Such tactics would be very subtle, and nearly impossible to identify in broad sample textual analyses—documenting this behavior would require a great deal of contextual understanding regarding how various pieces of disclosed (and withheld) news differentially affect focal firms' and peers' valuations.

An alternative (but nonmutually exclusive) possibility is that firms take a more direct approach, and explicitly disclose negative information about their RPE price-peers. We assess whether and to what extent firms use this

[2] Of note, we are not interested in the earnings forecasts, themselves. Rather, it is the surrounding contextual and narrative content that we hope to capture with these guidance dates. Most of the disclosure days in our sample correspond to earnings call dates.

tactic by examining whether focal firms mention their RPE peers, by name, in their peer-harming disclosures. We analyze the textual content of any earnings calls that coincide with firms' disclosure dates, and observe that focal firms explicitly mention their peers, by name, during their earnings calls with some regularity, and mention price-peers twice as often as profit-peers. Moreover, peer-mentions are negatively associated with price-peers' contemporaneous returns, but are unassociated with profit-peers' returns. These results suggest that a significant amount of peer-harm is achieved through explicit peer-mentioning disclosures. However, as noted above, we caveat that much of the peer-harming disclosure behavior we document likely occurs through more subtle tactics, that do not involve explicit peer mentions, which our text-based analyses are unable to capture.

We further examine firm and peer trading volumes to quantify the amount of information contained in firms' disclosures vis-à-vis own firm and peer firm valuations. The evidence suggests that focal firms change the information content in their disclosures to make them more informative about harmed peers, and less informative about themselves. On focal firms' voluntary disclosure dates, price-peers get an extra boost in trading volume compared to profit-peers. These results are particularly prominent among peers with more negative returns on the disclosure date (i.e., among peers that are more likely to have been targeted). Furthermore, focal firms that rely more heavily on rTSR get less of a trading volume boost from their own disclosures. This evidence suggests that peer-harming disclosure tactics are costly to the focal firm; in order to harm their price-peers, focal firms seem to forgo some of the capital market benefits of disclosure.

Although this descriptive evidence is consistent with deliberate peer-harm, and difficult to reconcile with incidental spillover, we recognize that much of the identifying variation utilized in these baseline tests comes from endogenous focal firm choices. Firms choose whether and how to use RPE, including whether to make a particular peer a price-peer or a profit-peer, allowing for the possibility of selection bias in our estimation. If some omitted factor simultaneously affects (1) the propensity for a peer to be chosen as a price-peer (as opposed to a profit-peer); and (2) the propensity for a peer to underperform on focal firms' disclosure dates, then the results from our estimation may be biased, and would not necessarily reflect the effects of deliberate peer-harming tactics. We observe no systematic differences between price- and profit-peers to support this possibility, and our use of firm × peer fixed effects further mitigates the concern, but we nonetheless cannot rule it out.

To better attribute our results to deliberate peer-harm, we dispatch with profit-peers as a control group, and restrict our focus to price-peers. Rather than comparing price-peers to some other reference group, we exploit variation *among* a firm's price-peers with respect to the benefits of engaging in sabotage. To do so, we leverage the fact that almost all rTSR grants yield payouts based on percentile rankings, rather than performance relative to the peer group mean. As such, there can be variation among price-peers with respect to the benefits of sabotage—it may be worthwhile to harm one peer, while fruitless to harm another. Whether or not harming a peer is beneficial to the focal firm's CEO is a function of how likely it is that the harm done ends up being a marginal determining factor in the final performance rankings. With this in mind, we develop a parsimonious model of disclosure-based sabotage in the context of a TSR tournament, and use it to generate predictions regarding which price-peers are more versus less likely to be harmed, given the current firm-peer-time-specific circumstances (namely, period-to-date rTSR standings).

In our model, a focal firm and its price-peer(s) have TSRs that start at zero, and evolve stochastically over the course of the performance period. At some point during the period, the manager has the option to disclose or withhold negative private information regarding its price-peer(s). The information will eventually make its way into prices, but the manager's disclosure choice affects when the price impact occurs. If the manager discloses (withholds), the information will be impounded into price in the current (subsequent) performance period. Disclosure confers both benefits and costs to the manager. Disclosing accelerates the negative information into prices in the current performance period, increasing the probability of outperforming the harmed peer(s) in the current period. However, disclosing also removes the negative price impact from the subsequent period (when the information would have otherwise come to light), decreasing the probability of outperforming the peer(s) in the subsequent period. Disclosing can also have adverse capital market consequences for the firm, and may impose some personal cost on the manager. From our model, we generate several comparative statics regarding when the net benefits of peer-harming disclosure are larger versus smaller, based on period-to-date TSR differentials. These comparative statics yield multiple empirical implications, vis-à-vis managers' peer targeting incentives.

Our model suggests that the choice to sabotage a peer is driven by three primary aspects of period-to-date performance: (1) performance proximity; (2) relative ranking; and (3) density. Regarding (1), it is typically more beneficial to target peers whose period-to-date performance is more similar to that of the focal firm. Regarding (2), it is typically more beneficial to target a better-performing peer than a worse-performing peer. Regarding (3), to the extent that sabotage is costly, it is less beneficial to engage in sabotage if other peers have period-to-date performance that is very similar to that of the focal firm, as doing so is likely to help those other peers outperform the focal firm, rendering the act of sabotage counterproductive.

We take these empirical implications to the data and find strong support for all of them. On focal firms' disclosure dates, peer harm is greatest among price-peers whose period-to-date performance is similar to and/or better than the focal firm's. The strength of these effects increases significantly toward the end of the performance period. We further document that peer harm is lower when multiple peers have period-to-date performance very similar to the focal firm. Collectively, we find robust evidence consistent with the model's predictions: peers' stock prices are more likely to be harmed when doing so is more likely to be marginal in the final performance rankings (and thus more beneficial to the disclosing firm's CEO, vis-à-vis their RPE incentives). Of note, the variation we rely on in these tests comes from realized stock-performance outcomes. Stock performance is not a choice variable, nor easily predictable ex ante, making it unlikely that our inferences are confounded by some correlated omitted factor associated with the selection of price-peers.

In sum, evidence suggests that focal firms harm their price-peers' stock prices by disclosing negative information about them—sometimes explicitly—and that doing so increases harmed peers' trading volumes, but comes at the expense of the focal firm's own trading volume. Moreover, focal firms harm their price-peers in a highly targeted manner, harming those peers whose stock price depression would most benefit the disclosing firm's CEO, vis-à-vis their monetary RPE incentives. These incentives can be quite substantial—a typical rTSR grant in our sample pays out several million dollars (accounting for roughly 40% of compensation) and in many cases, ascending a single rank within the RPE peer group can increase compensation by more than a million dollars.

Collectively, these patterns are consistent with deliberate sabotage, whereby RPE-incentivized CEOs strategically *internalize* their firms' disclosure spillovers and alter their firms' disclosures to depress their price-peers' stock prices. We use a battery of robustness, placebo and sensitivity analyses to further support this interpretation. Although none of the evidence in our study can speak directly to managerial intent, the collective body of evidence is not well-explained by incidental disclosure spillover, and better explained by deliberate harm. In particular, under incidental disclosure spillover, it is unclear why price-peers should systematically underperform relative to profit-peers (even within a firm-peer pair) and it is especially unclear why the specific price-peers whose underperformance would most benefit the disclosing firm's CEO, based on year-to-date TSR, would be most affected. Deliberate peer-harming disclosures readily explain these patterns.

This study contributes to the growing literature on the strategic implications of using RPE in CEO pay plans.[3] Related prior work sheds light on the *determinants* of RPE, and investigates whether boards appear to make decisions regarding executive pay plans *as if* RPE is likely to motivate sabotage. For example, Bloomfield, Marvão, and Spagnolo [2023] show that firms are more likely to use RPE (and use it more effectively) when the possibility of engaging in costly sabotage is reduced by an illegal cartel arrangement. This existing evidence suggests that sabotage is a potential side

[3] See, for example, Aggarwal and Samwick [1999], Vrettos [2013], Feichter, Moers, and Timmermans [2022], Bloomfield, Marvão, and Spagnolo [2023], Bloomfield [2023], Bloomfield, Friedman, and Kim [2024].

effect of RPE—and that boards are concerned about it—but it does demonstrate any peer-harming actions being taken by RPE-using firms. Our study contributes by being the first to document the incidence of peer-harming actions as a *consequence* of using RPE in a CEO pay plan.

In so doing, our study complements recent work on the strategic consequences of using RPE. In particular, Feichter, Moers, and Timmermans [2022] and Bloomfield [2023] provide evidence that RPE in CEO pay plans prompts firms to compete more aggressively in the product market. Bloomfield [2023] uses NielsenIQ's product pricing data to show that accounting-based RPE is associated with lower product prices, while Feichter, Moers, and Timmermans [2022] uses a structured content analysis of RavenPack News Analytics data to show that peer group overlap is associated with a greater volume and complexity of competitive actions. However, neither of these studies document any peer-harm as a result of these aggressive actions, while ours shows firms' disclosure choices damage price-peers' stock performance. As such, our study also contributes more broadly to the large literature on RPE-motivated sabotage, beyond the CEO pay context. Existing research documents that RPE leads to peer-harm in a variety of other contexts, including athletic competitions (Del Corral, Prieto-Rodriguez, and Simmons [2010]), corporate promotions (Chen [2003], Harbring et al. [2007]), higher education (Royal and Guskey [2014]), and electoral politics (White [1994], Lau and Pomper [2001a], [2001b], [2002], [2004]). Our findings lend additional credence to the notion that the theory of RPE-motivated sabotage is also relevant in the context of CEO pay.

Our study also contributes to the literature on disclosure spillovers. Extant literature documents information spillovers from public firms' disclosures.[4] Recent work even suggests that disclosing firms might *internalize* these spillovers when making their disclosure choices. For example, Park et al. [2019] provide evidence that firms use voluntary disclosures to improve the liquidity of commonly owned companies, while Cao, Fang, and Lei [2021] provide evidence that firms disclose negative information on social media about their product-market rivals to signal their own strength (à la Spence [1973]).[5] More similar to our study, Kim, Verdi, and Yost [2020]

[4] See, for example, Firth [1976], Foster [1981], Shroff, Verdi, and Yost [2017], Breuer, Hombach, and Müller [2022].

[5] Like our study, Cao, Fang, and Lei [2021] document that firms disseminate bad news about other companies. However, our study departs from Cao, Fang, and Lei [2021] along three important dimensions. First, Cao, Fang, and Lei [2021] do not consider any role for compensation incentives (such as RPE), while we examine whether firms intentionally harm their price-peers, because of CEOs' RPE incentives. Of note, Cao, Fang, and Lei [2021] do not use the term "peer" in the same sense that we do. We use this term to refer to focal firms' RPE peers, while Cao, Fang, and Lei [2021] use the term to refer to product market rivals, the overwhelming majority of which are *not* RPE peers. Second, in terms of sample and context, Cao, Fang, and Lei [2021] study corporate Tweets, while we examine earnings forecasts and their corresponding conference calls. Third, the "negative peer disclosures" in Cao, Fang,

provide evidence to suggest that firms use strategic disclosures to depress the stock prices of acquisition targets, as a cost-saving technique. Our study offers an additional reason for firms to internalize the spillover effects from their own disclosures: a desire to improve *relative* stock performance, stemming from rTSR incentives in CEOs' compensation plans.

More broadly, our study contributes to the growing literature on strategic disclosure, which examines reasons for corporate disclosure beyond its capital market benefits.[6] More specifically, our study connects to the literature on strategic value-reducing disclosure. Ample prior evidence suggests managers and investors make public disclosure decisions intended to be harmful. In some cases, managers will intentionally depress their own firm's stock price, temporarily, for example, to boost their future option compensation (e.g., Aboody and Kasznik [2000]). In other cases, managers will disclose information to weaken competitors during union negotiations (e.g., Aobdia and Cheng [2018]) or to depress acquisition targets' prices (e.g., Kim, Verdi, and Yost [2020]). Similarly, in the short activism literature, there is significant evidence demonstrating that interested parties will publicly disclose information to harm a target's price, in order to improve the profitability—and reduce the risk—of their short positions (e.g., Liu [2015], Bliss, Molk, and Partnoy [2019], Brendel and Ryans [2021], Mitts [2020], Stice-Lawrence, Wong, and Zhao [2022]). Our study complements these existing bodies of literature by highlighting another reason managers might alter their disclosures to harm another firm's stock price: stock price-based RPE incentives.

The remainder of this manuscript is organized as follows. In section 2, we develop and state our testable predictions for the price- versus profit-peer analysis; in section 3, we describe our primary data sources, sample selection criteria and variable construction procedures; in section 4, we present our price- versus profit-peer analyses; in section 5, we present our model-motivated within-price-peer analyses; and in section 6, we conclude. Appendix A presents the model of disclosure-based sabotage from which our within-price-peer targeting predictions derive. The online appendix provides additional discussion and tabulated supplemental/robustness analyses.

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and Lei [2021] are not novel information releases to the market, but instead reminders of old news (e.g., re-Tweeting an article from several month ago) that have no negative impact on the peers' stock returns. In contrast, we document that focal firms' disclosures have a systematically negative impact on their price-peers' valuations, suggesting that *novel* negative information is being brought to light.

[6] See, for example, Darrough and Stoughton [1990], Darrough [1993], Li [2010], Bertomeu and Liang [2015], Burks et al. [2018], Bloomfield and Tuijn [2019], Bourveau, She, and Žaldokas [2020], Bloomfield [2021], Bertomeu et al. [2021], Kepler [2021].

2. *Hypothesis Development*

Firms often provide their executives with RPE-based compensation awards to filter out systematic risk/mitigate "pay for luck" and thereby facilitate efficient risk-sharing between shareholders and executives (e.g., Holmström [1982], Gong, Li, and Shin [2011], Ma, Shin, and Wang [2021], Bizjak et al. [2022], Bloomfield, Guay, and Timmermans [2022]). Although RPE can be an effective governance tool, it also has a potentially undesirable consequence: by benchmarking an agent's performance against the performance of a peer group, RPE gives the agent incentives to harm the peer group's performance (e.g., Dye [1984], Lazear [1989], Gibbons and Murphy [1990]). This can be especially harmful within firms when, for example, coworkers harm each other because they are all vying for the same bonus and/or promotion (Chen [2003], Harbring et al. [2007]).

In the context of CEO compensation, the RPE "peers" are not fellow employees within the same organization, but rather other companies. Given that CEOs do not directly interact with other companies in the same way that coworkers within an organization interact with each other, it may seem unlikely that RPE incentives in a CEO pay plan could lead to peer harm. However, there are several mechanisms through which a CEO can impact the performance of an RPE peer. For example, if a firm and its peers are product market competitors—which is common—aggressive price-cutting or overproduction could be used as viable tactics to harm peers' profits (e.g., Aggarwal and Samwick [1999], Feichter, Moers, and Timmermans [2022], Bloomfield [2023], Bloomfield, Marvão, and Spagnolo [2023]).

If relative performance is evaluated on the basis of stock price performance, we posit that firms may try to sabotage their peers via peer-harming disclosures. Ample prior literature demonstrates that firm disclosures can contain value-relevant information regarding *other* public companies (e.g., Firth [1976], Foster [1981], Freeman and Tse [1992], Shroff, Verdi, and Yost [2017], Breuer, Hombach, and Müller [2022]). Related work even suggests that firms will strategically *internalize* these effects when doing so is advantageous, for example using disclosures to depress the stock price of an acquisition target (e.g., Kim, Verdi, and Yost [2020]). In the context of price-based RPE, it seems likely that managers would similarly internalize the effects of their disclosures on their price-peers' stock prices, and use strategic peer-harming disclosures to reduce price-peers' stock prices, and thereby secure a better relative performance outcome (and thus greater compensation).

This tactic has several appealing properties. First, disclosure-based tactics do not require firms to distort their operations and sacrifice their own profitability; the disclosures need only affect investors' *beliefs* about peers' future cash flows. In contrast, operations-based tactics, such as price-cutting and overproduction, require the focal firm to deploy investment and/or product market strategies that are not value-maximizing—something shareholders are quite averse to (Bloomfield, Marvão, and Spagnolo [2023]).

Second, disclosure-based tactics are not constrained by product market boundaries. Price-cutting and/or overproduction can be effective methods of peer-harm against product market rivals, but will not be effective against peers outside of their product markets. In practice, firms often populate their RPE peer groups with several peers from outside their industry (e.g., De Angelis and Grinstein [2020], Bloomfield, Guay, and Timmermans [2022]), and disclosure is likely the most effective way to harm their stock prices. Third, a disclosure-based tactic could be executed with precision to harm specific price-peers. In contrast, operational approaches to sabotage such as excess product market aggression are likely to be blunt instruments that indiscriminately harm all players in the product market (potentially including many that are not even in the peer group).

We posit that disclosure-based sabotage would be less prevalent in the context of profit-based RPE. In the case of relative profits, effective peer-harming disclosures must significantly affect peers' operations, which is likely more difficult than simply influencing investors' beliefs about future cash flows.[7] Moreover, even when a focal firm can influence a profit-peer's operations/profits through its disclosures, the relative performance benefits from such a tactic would likely take a long time to realize (potentially many months, or even years), as the impact on operations would manifest in future earnings, gradually, over a long period of time. Even if ultimately effective, the delayed nature of the benefits likely diminishes the incentives to harm the peer in the first place—there is no guarantee the peer would remain a profit-peer in subsequent periods, so some of the benefit may never be realized at all.[8] In contrast, a disclosure that reveals negative information about a price-peer's future value-generation capabilities will have *immediate* valuation consequences, and thus confer an immediate benefit to the focal firm's CEO. Hence, we predict that on average, price-peers underperform compared to profit-peers on focal firms' voluntary disclosure dates.

One might reasonably be concerned that this prediction is at odds with an efficient market. Indeed, if all CEOs' relative performance incentives are fully known and understood by investors, then in a rational capital market, price-peers should not systematically underperform. The harm from disclosed damaging news would be perfectly counterbalanced by the rational inference from the absence of damaging news, in expecta-

[7] We acknowledge that firm disclosures can influence peers' operational performance outcomes, and thus be a viable strategy for sabotaging profit-peers, as well. For example, a disclosure that reveals a product flaw or affects a commodity spot price could harm a peer's operations by dampening customer demand or increasing production costs. Alternatively, in a game with oligopolistic interdependencies, a disclosure that reveals the focal firm's strategic intent could influence peers' strategic actions (e.g., pricing/production), affecting profits. As such, there may be some incentive for focal firms to harm their profit-peers via voluntary disclosure. However, the prospect of successful disclosure-based sabotage seems to be much greater in the case of price-based RPE than profit-based RPE.

[8] If the peer's operations were substantively harmed, boards might not view it as a viable peer for future periods.

tion, such that there would be no impact on average returns. In this sense, our prediction involves a joint hypothesis: (1) rTSR-incentivized managers internalize the externalities of their disclosures, and alter their disclosure strategies to harm their price-peers; and (2) this behavior is not perfectly anticipated by the capital market. As we discuss below, there are several compelling reasons to expect (2) to hold in our setting.

First, prior empirical evidence demonstrates that the capital market systematically underreacts to nondisclosure. For example, Zhou and Zhou [2020] document that a firm's choice *not* to provide earnings guidance reliably predicts large price drops at the subsequent earnings release, suggesting that investors do not rationally infer information from silence.[9] Second, setting aside the possibility of investor irrationality, there are many information frictions that could prevent peer-harming disclosures from being fully anticipated and impounded into prices, within a rational expectations framework (à la Grossman and Stiglitz [1980]). In particular, investors are not costlessly endowed with knowledge/understanding of CEOs' incentives to harm other companies.

Anticipating the amount of harm a particular company is likely to experience—and drawing appropriate inferences from the absence of any such harm—requires a great deal of costly information acquisition and processing. One must ascertain whether the company is used by any focal firms as an RPE peer, and if so, which metric(s) the focal firm(s) use(s) to measure relative performance (e.g., stock price vs. profit). This requires detailed knowledge regarding *all* of the CEO pay plans for the entire universe of firms that might plausibly use the company as a price-peer. This task is made even more onerous for investors by the fact that firms' public disclosures regarding their RPE usage (e.g., metrics and peer identities) are not provided until several months *after* the performance period concludes, through firms' DEF 14A filings. At the time of the voluntary disclosure dates we examine, investors will likely not be certain of which companies are used as price-peers by which focal firms. Although investors can gather/process available information to inform their beliefs (e.g., by looking at firms' prior compensation practices), this is a costly procedure and will likely not yield perfectly accurate beliefs regarding managers' disclosure incentives.

In light of these information acquisition/processing costs (and the nonpublic nature of CEOs' current pay plans), it seems unlikely that capital market participants—even if rational—would perfectly anticipate the disclosure-day harm to rTSR-using firms' price-peers, and fully impound

[9] In our setting, underreaction to nondisclosure may be even more rampant, because the rational inference from silence in a spillover context is subtler. Here, the burden on investors is to infer information about a given company's market value from a potentially large set of *other companies'* nondisclosure choices. If investors cannot reliably understand the valuation implications of silence for the silent firm itself (e.g., Zhou and Zhou [2020]), it seems unlikely that they would understand the valuation implications of one firm's silence for the entire set of its RPE peers.

the implications of firms' silence into price-peers' valuations. It is worth emphasizing that the return patterns we predict would not have been arbitragable based on information publicly available at the time. We are not proposing a viable trading strategy, but are instead positing that information available ex post to us as researchers (e.g., subsequent disclosures regarding RPE incentives and peer identities) can be used to explain historical return patterns.

Strategically altering disclosures to harm price-peers' stock prices is likely a costly action for the disclosing firm. We typically presume that firms' voluntary disclosure policies are optimized for the goal of maximizing their own stock prices (e.g., Verrecchia [1983], Dye [1985]). One major channel through which disclosures provide value is by reducing information asymmetry, thereby increasing stock liquidity (e.g., Diamond and Verrecchia [1991], Leuz and Verrecchia [2000], Bushee and Leuz [2005], Balakrishnan et al. [2014], Leuz and Wysocki [2016]). In this way of thinking, sabotage represents a potentially competing objective. It is unlikely that a voluntary disclosure policy, optimized to reduce information asymmetry (and thereby increase liquidity), could be adjusted to incorporate strategic peer-harm without any sacrifice in the informational quality of the disclosures. To effectuate peer-harm, focal firms likely change the nature of their voluntary disclosures to provide more (negative) information about peers, perhaps coming at the expense of information about themselves. Hence, we predict that rTSR-incentivized managers' voluntary disclosures are more positively associated with their peers' trading volume and less positively associated with their own firms' trading volume.

An important supposition underlying our predictions is that RPE incentives are strong enough to influence CEOs' behavior. If managerial behavior is driven entirely by equity incentives (e.g., "delta"), then there is no reason to expect CEOs to strategically harm their rivals to boost relative performance. There are several reasons to expect that managers alter their behavior based on their RPE incentives. First, the mere existence of such incentives suggests their importance; it is not clear why boards would devote any time/energy to constructing these complex performance-based pay plans if they have no impact on managerial conduct. Second, RPE incentives tend to be quite substantial, monetarily. On average, firms that use RPE base 49% of their CEOs' performance-based pay on RPE (De Angelis and Grinstein [2020]). In our sample, the average rTSR grant pays over $3 million for the target level of achievement (accounting for 38% of total compensation), and awards almost $6 million for being ranked top in the peer group. RPE awards can often jump by upward of $1 million, simply by increasing a single rank within the RPE peer group. As such, managers have considerable monetary incentives encouraging them to outperform their RPE peers.[10]

[10] See appendix B for an example rTSR grant.

Also worth discussing is whether these peer-harming disclosure tactics, if utilized, allow CEOs to garner excess compensation. On the one hand, it may seem obvious that such disclosures, if effective at boosting relative performance, must also be effective at securing excess compensation. Although this is certainly conceivable, this line of reasoning relies on two important assumptions that need not hold true (in general). First, this line of reasoning presumes that boards view peer-harming disclosures as an unproductive behavior, whereby the CEO "games" the compensation plan by taking actions that improve *measured* performance without similarly improving the underlying performance construct that the measure was intended to reflect (e.g., Campbell [1979], Goodhart [1984], Feltham and Xie [1994]). An alternative possibility is that (some) boards welcome this behavior, and are happy to reward it with additional compensation. Second, even if boards consider peer-harming disclosures to be an example of unproductive compensation "gaming," it would only result in excess compensation (on average) if boards failed to anticipate it. If boards understand CEOs' disclosure incentives, they could avoid awarding excess compensation by adjusting the relative performance targets and associated compensation awards with the possibility of peer-harming disclosures in mind. Existing empirical evidence suggests that boards are aware of the possibility of RPE-motivated sabotage, and make adjustments to pay plans, in response (Bloomfield, Marvão, and Spagnolo [2023]).[11] Ascertaining boards' views on peer-harming disclosures lies beyond the scope of our study.

3. *Data, Sample Selection, and Variable Construction*

In this section, we describe the data sources used in our study, the sample selection criteria, and variable construction details.[12] Summary statistics are presented in table 1.

3.1 DATA AND SAMPLE

The data for this study come from the intersection of CRSP, Compustat, I/B/E/S, and Incentive Lab. We restrict the sample to focal firms that use RPE with a self-selected peer group (*performancetype* contains "Rel" and *relativebenchmark*=="Peer Group"). Using Incentive Lab data on RPE peer groups, we construct a network data set for all focal firms in our sample covering the period of 2006–2016. The unit of observation is the firm-peer-day. Our sample includes 7,471,746 firm-peer-day observations coming from 477 unique focal firms, with 2,236 unique peers forming 9,428

[11] Of note, proper foresight by boards would not eliminate CEOs' incentives to issue peer-harming disclosures. Even if boards perfectly anticipate the behavior, it is still in the manager's best interest to engage in it, as their relative performance will be similarly discounted whether or not they do (e.g., Stein [1989]).

[12] In section OA1 of the online appendix, we explore the sensitivity of our inferences to alternative sample selection criteria and/or variable construction approaches, along with sundry alternative research design choices.

TABLE 1
Summary Statistics

Variables	Num Obs.	Mean	SD	Q1	Med.	Q3
CEO incentives						
$rTSR_{i,j,t}$	7,471,746	0.765	0.396	0.500	1.000	1.000
Firm disclosure						
$Disc.\ Day_{i,t}$	7,471,746	0.011	0.104	0.000	0.000	0.000
Stock market returns						
$Peer\ Return_{j,t}$	7,471,746	0.046	2.024	−0.897	0.052	0.999
$Firm\ Return_{i,t}$	7,471,746	0.052	1.959	−0.872	0.061	0.984
$Peer\ Group\ Performance_{i,k,t}$	3,356	−0.015	0.720	−0.330	−0.017	0.300
Textual characteristics						
$Mentions_{i,k,t}$	3,356	0.360	1.400	0.000	0.000	0.000
$Readability_{i,k,t}$	3,356	0.004	0.970	−0.590	0.083	0.680
$ln(Length_{i,k,t})$	3,356	8.153	0.530	7.840	8.104	8.390
$\%Competition_{i,k,t}$	3,356	0.973	1.280	0.000	0.604	1.470
$Sentiment_{i,k,t}$	3,356	2.492	1.680	1.310	2.075	3.140
Information content						
$ln(Peer\ Volume_{i,j,t})$	7,471,746	14.014	1.519	13.081	14.082	15.004
$ln(Volume_{i,t})$	7,471,746	14.161	1.307	13.341	14.158	14.998
Year-to-date standings						
$Proximity_{i,j,t}$	6,035,916	0.653	0.224	0.500	0.692	0.840
$Peer\ Above_{i,j,t}$	6,035,916	0.479	0.500	0.000	0.000	1.000
$Density_{i,j,t}$	6,035,916	0.298	0.219	0.000	0.316	0.447

This table presents summary statistics for all the variables used in our regressions. The sample is made up of firm-peer-day observations from the intersection of CRSP, Computstat, I/B/E/S and Incentive Lab, over the period of 2006–2016. Only firms with active RPE grants in their CEOs' pay packages are included in the sample.

unique firm-peer pairs. For guidance dates that coincide with firms' earnings calls, we further obtain call transcripts from S&P Capital IQ, yielding a sample of 3,356 earnings calls.

3.2 VARIABLE CONSTRUCTION

3.2.1. RPE Type. We measure RPE type using grant level compensation data from the Incentive Lab data set. We code each RPE grant as price-based if metrictype=="Stock Price" and as profit-based if metric type=="Accounting". We then match each grant to its focal firm and selected peers, and for each firm-peer pair, we construct the variable $rTSR$, equal to 1 for a focal firm's price-peers and 0 for a focal firm's profit-peers.[13]

In some analyses, we collapse the sample from firm-peer-date to firm-date. For these analyses, we replace $rTSR$ with $\overline{rTSR}$, which is the firm-date

[13] In rare instances, a focal firm will use a given peer as both a price-peer and a profit-peer, simultaneously; we code such observations as $rTSR = \frac{1}{2}$. These observations represent a small minority of our sample (<10%), and our results are not sensitive to our treatment of these observations. In robustness tests, we show that we can exclude them from our sample, yielding a binary $rTSR$ variable, without affecting our inferences. See section OA1.1 of the online appendix.

average of $rTSR$. A firm that only has price-peers has an $\overline{rTSR}$ of 1; a firm that only has profit-peers has an $\overline{rTSR}$ of 0; and a firm with both price-peers and profit-peers has an $\overline{rTSR} \in (0, 1)$, reflecting the proportion of price-peers.

3.2.2. Disclosure Dates. We measure disclosure dates using data on voluntary earnings guidance from I/B/E/S. We construct the indicator variable $Disc.\ Date$, which equals 1 on the first trading date for which a focal firm's disclosure was available. For disclosures occurring prior to market close, we use the same day as the disclosure date. For disclosures occurring after market close, we use the following trading day as the disclosure date.

Of note, we are not interested in managers' earnings forecasts, *per se*—it is not our position that managers alter the characteristics of their forecasts in order to sabotage their RPE peers (although it is conceivable that they do). Instead, we rely on earnings guidance dates as proxies for the much more general construct: "days when the firm chooses to reveal information to the market". Most guidance coincides with large (voluntary) information release, where firms provide a lot of contextual/narrative information. It is this package of information, in toto, that is released on these dates that may be used strategically to harm price-peers.[14]

3.2.3. Stock Performance. We measure stock performance using daily stock market returns data from CRSP. We construct two primary measures: $Firm\ Return$, equal to the focal firm's daily return; and $Peer\ Return$, equal to the peer's daily return. Both return variables are measured in percentage points, and winsorized at 1% and 99%. In some analyses, we benchmark overall peer group portfolio returns against contemporaneous returns from a counterfactual peer group portfolio. Specifically, $Peer\ Group\ Performance$ is the equal-weighted average return of the actual RPE peer group minus the equal-weighted average return of the counterfactual "artificial" peer group, constructed using the Bloomfield, Guay, and Timmermans [2022] peer selection algorithm.[15]

3.2.4. Textual Features. As is standard, before starting our textual analysis, we clean up the earnings call transcript text data by removing unnecessary whitespace and stopwords, and break the text into individual "tokens". This tokenization creates, for each individual transcript, a vector with one-token-per-element whereby each token is a single word.

The primary linguistic feature we are interested in is the number of explicit RPE peer mentions. To measure this construct, we first obtain the full company names of firms' peers from Compustat. We then "stem" each

[14] A large fraction of the guidance dates in our sample correspond to earnings calls. We examine the textual content of these calls in subsection 4.2.

[15] For details on the construction of these counterfactual peer groups, see appendix C.

peer's company name by reducing the name to its base.[16] Next, to reduce the potential for false positives, we drop company names whose single-word stem is part of the English dictionary, as such a mention might not refer to the RPE peer.[17] Finally, we count the number of times a focal firm mentions its peers in the conference call, using the single-word stem. We label this variable *Mentions*. Due to right-skewness, we use the natural logarithm in our analyses.

For each earnings call transcript, we further create four additional variables to analyze its textual content: disclosure length, readability score, proportion of competition-related words, and sentiment. *Length* is the natural logarithm of the number of words in the presentation component of the firm's conference call, which we use to control for differences in the length of firms' earnings call transcripts. *Readability* is the first principal component of the following measures of readability related to the presentation component of the firm's conference call: Flesch-Kincaid, RIX, Gunning Fog, ARI, and SMOG (e.g., Guay, Samuels, and Taylor [2016], Bushee, Gow, and Taylor [2018]).[18] *%Comp* is the number of occurrences of "competition," "competitor," "competitive," "compete," "competing," including those words with an "s" appended, and excluding any case where "not," "less," "few," or "limited" precedes the word by three or fewer words, scaled by the total number of words and multiplied by 1,000 (Li, Lundholm, and Minnis [2013]). *Sentiment* is the number of words with positive sentiment divided by the number of words with negative sentiment in the presentation component of the firm's conference call, whereby the classification of sentiment follows the Loughran–McDonald sentiment lexicon (Loughran and McDonald [2011]).

3.2.5. Information Content. We measure the information content of firm disclosures using daily trading volume data from CRSP. We construct two primary measures: *Volume*, equal to the focal firm's daily trading volume; and *Peer Volume*, equal to the peer's daily trading volume. Due to the right-skewness of trading volume, we use the natural logarithm in our analyses.

3.2.6. Year-to-Date Relative Standing. For each trading day in our sample, we measure focal firms' and price-peers' year-to-date performance as TSR starting from the first day of the focal firm's fiscal year, until the close of the prior trading day. (Our measure of year-to-date performance does not include the current trading day's returns.) We use these year-to-date performance measures to construct three variables: *Proximity*, *Peer Above* and *Density*.

[16] This step reduces, for example, "The Coca-Cola Company" and "The Boeing Company" to the single-word stem "coca-cola" and "boeing," respectively. We do so because (1) it is unlikely that a manager mentions a peer by its full legal name; and (2) this one-word structure matches the one-token-per-element structure of the text data.

[17] Our inferences do not depend on this design choice.

[18] In our sample, this factor explains 94% of the variation in all five measures.

Proximity is a continuous variable equal to 1 minus the absolute value of the difference between a focal firm's year-to-date TSR percentile rank and the peer's year-to-date TSR percentile rank, within the peer group. *Proximity* ranges between 0 and 1, taking a value of 1 (0) if the firm and peer are tied in (at opposite ends of) the year-to-date TSR rankings. *Peer Above* is an indicator variable equal to 1 if the peer's year-to-date performance is superior to that of the focal firm. *Density* is the proportion of other peers that are within a bandwidth of the focal firms' year-to-date TSR. For our main specifications, we use a bandwidth of 2 percentage points. Due to the right-skewness of this measure, we use a square-root transform to make it more symmetric.[19]

4. Empirical Analysis

We first examine, graphically, how rTSR-using focal firms' and their price-peers' returns behave around focal firms' voluntary disclosure dates. In figure 1, panel A, we plot rTSR-using focal firms' returns, in event time around their disclosure dates, along with the contemporaneous average return of their price-peers. We find that, in the five days preceding the disclosure date, and the five days following the disclosure date, the two groups' returns move together in tight lockstep ($\rho \approx 0.9$), and have similar average levels. However, on the disclosure date, the two diverge sharply with focal firms earning unusually high returns, while their price-peers earn unusually low returns. To assess whether the differential returns patterns on the disclosure date are significant, in panel B we plot the differences between price-peers' and focal firms' daily returns, in event time around focal firms' disclosure dates, along with 90% confidence intervals for the differences. For the five days preceding the disclosure date and the five days following the disclosure date, the return differences hover around zero. However, on the disclosure date, price-peers' returns drop by 11–12 basis points relative to focal firms' returns—an effect that is statistically significant at conventional inference levels ($p \approx 0.01$).

We caution that these graphical results are highly descriptive in nature. They are univariate patterns without any controls, nor benchmark for comparison. The actual extent of sabotage could be far larger or smaller than what these plots suggest. In our ensuing empirical analysis, we attempt to control for the endogeneity of peer selection and other relevant factors, and introduce a variety of benchmarks for comparison to better quantify the extent of peer harm. We first consider profit-peers as a control group in subsection 4.1, and then exploit within-price-peer variation in section 5.

[19] In robustness tests, we use alternative bandwidths for the *Density* measure (1 and 3 percentage points) and alternative *Proximity* measures based on raw absolute TSR differentials. We find that our results are robust to these alternative measurement approaches. See section OA1.2 of the online appendix for details.

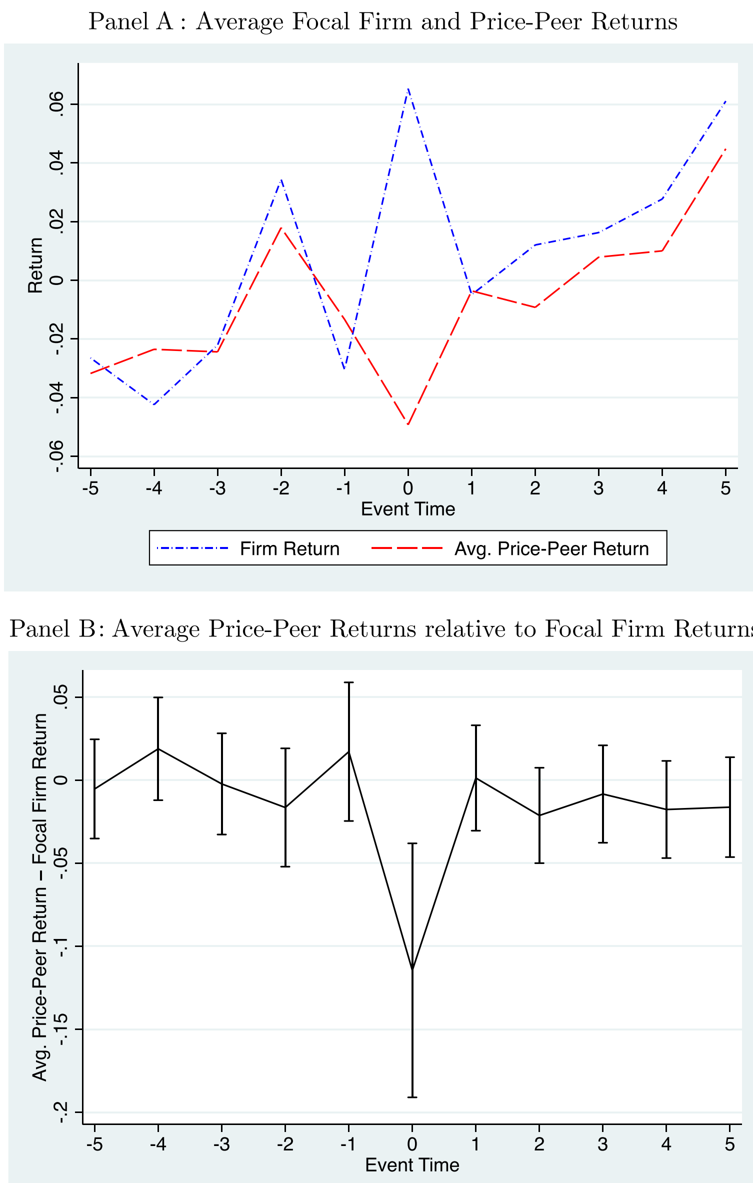

Fig 1.—Focal firm and peer group returns around disclosure days. This figure plots return patterns for focal firms and their price-peers around focal firms' disclosure dates. Panel A plots focal firm returns, and average price-peers returns, in event time, around the focal firms' disclosure dates. Panel B plots the difference between the average price-peers' return and the focal firms' returns, in event time, around the focal firms' disclosure dates, along with 90% confidence intervals. In both panels, Event Time represents trading days, with negative values reflecting days preceding the disclosure date, positive values reflecting days following the disclosure date, and 0 reflecting the disclosure date itself.

4.1 BASELINE ANALYSIS: PRICE-PEER VERSUS PROFIT-PEER RETURNS

Our baseline empirical strategy is to compare price-peers' stock returns to profit-peers' stock returns on focal firms' voluntary disclosure dates. The empirical specification is a difference-in-differences design: price-peers form our treatment group; profit-peers form our control group; and focal firms' voluntary disclosures are the treatment events. To identify disclosure-based peer-harming strategies, we examine whether price-peers respond more negatively than profit-peers to focal firms' voluntary disclosures. Profit-peers form a natural control group for our study; much like price-peers, profit-peers are self-selected by the focal firms due to their shared exposure to common sources of performance uncertainty.

To evaluate the validity of this empirical strategy, we first assess the comparability of price-peers and profit-peers with respect to a battery of market and relationship characteristics: size (market value of equity); relative size (ratio between peer's size and firm's size); liquidity (daily trading volume); stock return synchronicity (daily return correlation with the focal firm); industry overlap (indicator for same two-digit SIC); rivalry (indicator for disclosed competitors, per FactSet Revere); supply-chain partnership (indicator for disclosed customers and/or suppliers, per FactSet Revere); and RPE peer mutuality (indicator for two-way RPE peer relationships). We further present descriptives for a few grant features: rank-based (indicator for grants with rank-based awards); cash award (indicator for grants with a cash award); and equity award (indicator for grants with an equity award). We present these descriptive statistics in table 2. Panel A presents distributions of these characteristic for all firm-peer-year observations in the sample. Panel B presents averages, split by RPE type (i.e., price-peers vs. profit-peers).

We find that profit-peers seem to be almost indistinguishable from price-peers with respect to these characteristics. On average, price-peers and profit-peers are insignificantly different in terms of their size, relative size, stock return synchronicity, and stock liquidity. Moreover, they have similar degrees of industry-overlap, rivalry and supply-chain partnerships. The only difference with respect to these peer characteristics regards the mutual nature of the RPE peer relationship. Mutual peer relationships are fairly rare (9.7%) overall, but price-peer relationships are somewhat more likely to be mutual (10.6% vs. 6.0%, $p < 0.1$). We do observe substantial differences with respect to the nature of the performance awards. Price-based RPE grants are significantly more likely to use a rank-order tournament structure (97.2% vs. 90.3%), and are much more likely to compensate managers with equity as opposed to cash. In sum, the evidence in table 2 reassures that price-peers and profit-peers are very similar in terms of their capital market characteristics and their relationships to the focal firms, suggesting that profit-peers are a reasonable control group for our analysis.

TABLE 2
RPE Descriptives

Panel A: Peer characteristics

Variables	Mean	SD	Q1	Med.	Q3
Capital market characteristics					
Synchronicity	0.571	0.178	0.450	0.591	0.709
Size	15.562	1.486	14.619	15.504	16.548
Relative Size	−0.108	1.279	−0.864	−0.099	0.703
Liquidity	14.010	1.447	13.126	14.084	14.959
Firm–peer relationship					
Industry Overlap	0.689	0.463	0.000	1.000	1.000
Rivalry	0.321	0.467	0.000	0.000	1.000
Supply Chain	0.065	0.247	0.000	0.000	0.000
Mutual Peer	0.097	0.294	0.000	0.000	0.000
Grant features					
Rank-Based	0.959	0.198	1.000	1.000	1.000
Cash Award	0.234	0.423	0.000	0.000	0.000
Equity Award	0.795	0.404	1.000	1.000	1.000

Panel B: Means by RPE type

Variables	Price-Peers	Profit-Peers	Dif.
Capital market characteristics			
Synchronicity	0.566	0.592	−0.026
Size	15.578	15.493	0.085
Relative Size	−0.111	−0.095	−0.016
Liquidity	13.983	14.123	−0.140
Firm–peer relationship			
Industry Overlap	0.680	0.726	−0.046
Rivalry	0.322	0.317	0.005
Supply Chain	0.069	0.048	0.021
Mutual Peer	0.106	0.060	0.046*
Grant features			
Rank-Based	0.972	0.903	0.069***
Cash Award	0.167	0.521	−0.354***
Equity Award	0.846	0.574	0.272***

This table presents descriptive statistics regarding firms' use of RPE. We present descriptives on: *Synchronicity* (daily return correlation with the focal firm); *Size* (peer's market value of equity); *Relative Size* (ratio between peer's market value of equity and firm's market value of equity); *Liquidity* (peer's natural logarithm of daily trading volume); *Industry Overlap* (indicator for firms and peers in the same primary two-digit SIC); *Rivalry* (indicator for firms and peers that have named each other as competitors in their public disclosures); *Supply Chain* (indicator for firms and peers that have a disclosed supply chain partnership); *Mutual Peer* (indicator for firms and peers that have a mutual peer relationship, whereby each uses the other as an RPE peer); *Rank-Based* (indicator for RPE awards based on rank, or percentile, within the peer group); *Cash Award* (indicator for RPE grants with a cash award); and *Equity Award* (indicator for RPE grants with an equity award). Panel A presents distributions of these characteristic for all firm-peer-year observations in the sample. Panel B presents averages, split by RPE type. *, ** and *** indicate two-tailed statistical significance at the 10%, 5% and 1% levels, respectively.

We estimate the extent to which price-peers underperform relative to profit-peers on focal firms' voluntary disclosure dates using variants on the following regression specification:

$$Peer\ Return_{j,t} = \beta_1 rTSR_{i,j,t} + \beta_2 Firm\ Return_{i,t} + \tau_t + \theta_{i,j} + \varepsilon_{i,j,t}, \qquad (1)$$

TABLE 3
Baseline Analysis: Peer Returns on Firms' Disclosure and Nondisclosure Days

	Outcome = $Peer\ Return_{j,t}$					
	(1) Disc.	(2) Nondisc.	(3) Disc.	(4) Nondisc.	(5) Disc.	(6) Nondisc.
$rTSR_{i,j,t}$	-0.212^{***}	0.001	-0.279^{***}	0.001	-0.304^{***}	-0.001
	(-2.716)	(0.402)	(-4.350)	(0.125)	(-4.220)	(-0.187)
$Firm\ Return_{i,t}$	0.177^{***}	0.609^{***}	0.178^{***}	0.609^{***}	0.179^{***}	0.609^{***}
	(11.903)	(34.881)	(11.967)	(34.896)	(11.729)	(34.889)
Fixed effects	SIC + Peer + Year-Month		Firm + Peer + Year-Month		Firm-Peer + Year-Month	
$\Delta\beta_1$	-0.212^{***}, $t = -2.740$		-0.279^{***}, $t = -4.227$		-0.303^{***}, $t = -4.053$	
Observations	82,187	7,389,559	82,187	7,389,559	82,187	7,389,559
R^2	0.158	0.347	0.166	0.348	0.192	0.348

This table presents evidence on the relation between $rTSR$ and $Peer\ Return$ on focal firms' disclosure versus nondisclosure days. In odd-numbered (even-numbered) specifications, the sample consists of focal firms' disclosure (nondisclosure) days. Below each specification pair, we present a test of the difference in coefficients on $rTSR$ across disclosure and nondisclosure days. Specification pairs differ with respect to cross-sectional fixed effect structure. Specifications 1 and 2 include industry and peer fixed effects; specifications 3 and 4 include firm and peer fixed effects; specifications 5 and 6 include pairwise firm-peer fixed effects. All specifications include year-month fixed effects. Below each coefficient is a t-statistic, in parentheses, calculated using standard errors clustered by industry and date. *, ** and *** indicate two-tailed statistical significance at the 10%, 5% and 1% levels, respectively.

where i indexes firms, j indexes peers and t indexes dates. The variable of interest, $rTSR$, reflects what type of peer j is to firm i at time t. The coefficient of interest is β_1, which reflects the average degree of over- or underperformance for price-peers relative to profit-peers, on focal firms' voluntary disclosures dates. As a benchmark for comparison, we also run this regression on nondisclosure dates, and evaluate the differences between β_1's across disclosure and nondisclosure dates.

To control for unobservable variation, we use a variety of cross-sectional fixed effect structures, θ: industry (i.e., the firm's four-digit SIC) + peer; firm + peer; and firm × peer. In all analyses, we use time fixed effects, τ, for each year-month combination.[20] For each fixed effect structure, we present two specifications. In odd-numbered (even-numbered) specifications, we run the regression for focal firms' disclosure (nondisclosure) days. Below each specification pair, we present a statistical test of the difference in coefficients on $rTSR$—the difference-in-differences estimate.[21] Results are tabulated in table 3.

We find that $rTSR$ carries a significantly negative coefficient for all of the disclosure day specifications, and a nearly zero coefficient for all of

[20] In robustness tests, we replace the firm and year-month fixed effects with firm-date fixed effects, to identify coefficients from within-firm-time variation in peer returns and the benefits of peer-harm. See section OA1.3 of the online appendix.

[21] The identifying assumption is the following: after controlling for contemporaneous firm returns and accounting for cross-sectional and time-series fixed effects, price-peers and profit-peers would not be systematically different in their stock return behavior on focal firms' disclosure dates, but for focal firms' incentives to harm their price-peers.

the nondisclosure day specifications. The difference in coefficients between disclosure and nondisclosure days is significant at the 1% level in all cases. This pattern indicates that, relative to nondisclosure days, price-peers significantly underperform on focal firms' disclosure dates. Notably, these results extend even to the tightest specifications, in which we use firm × peer fixed effects. These specifications rely on identifying variation arising from evolving peer relations; a firm uses a peer as a profit-peer at some points in time, and as a price-peer at other points in time. Our results indicate that, even within a firm-peer pair, the peer's underperformance during the firm's disclosure days is greater when being used as a price-peer, instead of as a profit-peer. In terms of economic magnitudes, our results suggest that price-peers underperform by an average of 20–30 basis points on focal firms' voluntary disclosure dates.[22,23]

Although these results are consistent with deliberate sabotage, an alternative interpretation is that the results simply reflect incidental disclosure spillovers. As noted above, there is substantial endogeneity with regard to focal firms' choices over RPE type and peer selection. As such, our treatment group (price-peers) and control group (profit-peers) may be systematically different from one another along relevant dimensions. Perhaps firms tend to select price-peers that are (for reasons unrelated to sabotage) disproportionately sensitive to negative disclosure spillovers. Although the descriptive statistics in table 2 give no indication of any discrepancies worth noting, there could be unobserved differences that bias our estimation. To assess the matter, we augment the baseline analysis in three ways.

First, as our simplest approach, we examine whether the average magnitude of spillovers is different for price- versus profit-peers. To do so, we augment regression equation (1) with an $rTSR \times Firm\ Return$ interaction term. This interaction term allows for the average return-relation (i.e., the extent and direction of the spillover) between the focal firm and its peers to differ across price-peers and profit-peers. We find no evidence of a difference. Across all three fixed effect structures, the loading on $rTSR \times Firm\ Return$ is economically minuscule and statistically insignificant; there does not appear to be any systematic difference in the extent of disclosure spillovers across the two types of peers.[24]

[22] In supplemental tests, we explore when these patterns are stronger versus weaker. First, we assess whether these effects are stronger when RPE plays a more prominent role in the CEO pay plan, and find that they are (section OA2.1 of the online appendix). We also assess whether these effects vary based on the sign of firms' or peers' return (section OA2.2 of the online appendix). As discussed in section 2, the return patterns suggest that investors do not fully anticipate firms' disclosure practices. With this in mind, we examine whether these effects are stronger (weaker) in cases where investors are less (more) likely to anticipate firms' peer-harming disclosures, and find this to be the case (section OA2.3 of the online appendix).

[23] This 20–30 basis point figure represents the average effect, and as such, co-mingles the frequency of peer-harm with the magnitude of harm, conditional on occurrence. Our analysis does not allow us to separate the two.

[24] See section OA2.4 of the online appendix for details and tabulated results.

Second, we examine whether price-peers' underperformance is generic to companies that are economically similar to the focal firm (and thus might be most sensitive to disclosure spillovers). We do so from two perspectives: (1) industry and/or sector overlap; and (2) common risk exposure. Regarding (1), we assess whether price-peers' underperformance is constrained by industry and/or sector boundaries, and find that it is not. Price-peers systematically underperform on focal firms' disclosure dates, to a similar degree, irrespective of whether or not they share an industry and/or sector with the focal firm. Regarding (2), we use the Bloomfield, Guay, and Timmermans [2022] peer selection algorithm to construct "artificial" peer groups for each firm-year in our sample, based on common risk exposure. We then test whether underperformance extends to these *potential* peers that could have been included as price-peers, but were not. If so, it would suggest that our findings reflect incidental spillover onto peers with similar risk exposures. We do not observe this; consistent with deliberate peer-harm, disclosure-day underperformance manifests only for the actual price-peers (whose underperformance benefits the CEO, vis-à-vis their contractual incentives).[25]

Third, as a placebo analysis, we "flip" the baseline specification and assess whether price-peers' and profit-peers' disclosures have systematically different effects on the focal firms' returns. In so doing, we leverage the fact that most RPE relationships are nonmutual; in our sample, less than 10% of RPE peers use the focal firm as an RPE peer for themselves. As such, there is no RPE-motivated reason to observe focal firm underperformance on their peers' disclosure dates (for either price- or profit-peers), so there should be no sabotage-related effect of $rTSR$ on the focal firms' returns. However, if the significant coefficients on $rTSR$ in table 3 are not a result of strategic sabotage, but instead due to uncontrolled differences in firm-peer spillovers that happen to vary by RPE type, then focal firms would likely underperform on their price-peers' voluntary disclosure days, too. Reassuringly, we find that focal firm returns are no different on price-peers' versus profit-peers' voluntary disclosure dates; the estimated effects are statistically insignificant and economically small. These results are tabulated in table 4.

In sum, price-peers' systematic underperformance, as documented in table 3, does not appear to be attributable to incidental disclosure spillovers. Price-peers are no different from profit-peers, on average, with respect to disclosure-day spillovers. Moreover, price-peers' underperformance is explained by their inclusion in the rTSR peer group, and not by their economic similarity to the focal firm, in terms of industry/sector affiliations and/or common risk exposure. Finally, price-peers' disclosures have no similar effect on focal firms' returns, further making incidental spillover an unlikely explanation for our findings. In the remainder of our

[25] See section OA2.5 of the online appendix for details and tabulated results.

TABLE 4

Placebo Analysis: Firm Returns on Peers' Disclosure and Nondisclosure Days

	Outcome = $Firm\ Return_{i,t}$					
	(1) Disc.	(2) Nondisc.	(3) Disc.	(4) Nondisc.	(5) Disc.	(6) Nondisc.
$rTSR_{i,j,t}$	0.003	0.005	0.010	0.005	0.032	0.010
	(0.058)	(0.423)	(0.194)	(0.453)	(0.425)	(0.834)
$Peer\ Return_{j,t}$	0.169***	0.568***	0.171***	0.568***	0.171***	0.568***
	(12.750)	(31.753)	(12.891)	(31.772)	(12.548)	(31.774)
$\Delta\beta_1$	−0.002, $t = -0.036$		0.005, $t = 0.100$		0.022, $t = 0.308$	
Fixed effects	SIC + Firm + Year-Month		Firm + Peer + Year-Month		Firm-Peer + Year-Month	
Observations	74,974	7,396,772	74,974	7,396,772	74,974	7,396,772
R^2	0.128	0.347	0.152	0.347	0.181	0.347

This table presents a flipped specification from table 3, examining the relation between $rTSR$ and $Firm\ Return$ on peers' disclosure versus nondisclosure days. In odd-numbered (even-numbered) specifications, the sample consists of peers' disclosure (nondisclosure) days. Below each specification pair, we present a test of the difference in coefficients on $rTSR$ across disclosure and nondisclosure days. Specification pairs differ with respect to cross-sectional fixed effect structure. Specifications 1 and 2 include industry and peer fixed effects; specifications 3 and 4 include firm and peer fixed effects; specifications 5 and 6 include pairwise firm-peer fixed effects. All specifications include year-month fixed effects. Below each coefficient is a t-statistic, in parentheses, calculated using standard errors clustered by industry and date. *, ** and *** indicate two-tailed statistical significance at the 10%, 5% and 1% levels, respectively.

analysis, we present evidence regarding how these disclosure tactics appear to be implemented, and examine whether firms strategically harm the specific price-peers whose underperformance would be most likely to benefit the focal firms' CEOs, vis-à-vis their relative performance incentives.

4.2 TEXTUAL CONTENT

The preceding evidence suggests that firms strategically alter their disclosures in order to lower their price-peers' stock prices. There are a variety of disclosure tactics that firms could use to achieve this end. One possibility is that peer-harming information is communicated implicitly, whereby focal firms do not directly discuss their peers, but instead strategically withhold or disclose information about their *own* performance based on how that information is likely to affect their peers' valuations. Another (nonmutually exclusive) possibility is that firms take a more direct approach, and explicitly disclose negative information about their RPE price-peers (e.g., leaking peer-harming news). Although both approaches are fully consistent with our story, for the purposes of better understanding firms' disclosure strategies, it is worthwhile to distinguish between these two possibilities. To do so, we assess whether, and to what extent, firms explicitly mention their RPE peers in their peer-harming disclosures. Specifically, we analyze the subset of guidance dates that coincide with earnings calls, and examine how frequently firms explicitly mention their RPE peers, by name, during these calls and assess whether these "peer mentions" relate to peers' contemporaneous stock performance. In appendix D, we provide some anecdotal evidence of unfavorable (and plausibly strategic) explicit mentions of price-peers.

We begin by summarizing the textual characteristics of firms' earnings calls, split by RPE type (i.e., price-based vs. profit-based), in table 5 panel A. We observe that, overall, price-peer mentions occur twice as frequently as profit-peer mentions, with focal firms mentioning their price-peers (profit-peers) an average of 0.416 (0.198) times per call ($p < 0.01$).[26]

We next examine the link between peer mentions and peer group returns, by RPE type. To do so, we collapse our main specification to the firm-call level and use the following empirical specification:

$$\begin{aligned} \textit{Peer Group Performance}_{i,k,t} &= \beta_1 rTSR_{i,k,t} \times ln(1 + \textit{Mentions}_{i,k,t}) + \beta_2 rTSR_{i,k,t} \\ &+ \beta_3 ln(1 + \textit{Mentions}_{i,k,t}) + \beta_4 \textit{Length}_{i,k,t} + \tau_t + \theta_i + \varepsilon_{i,k,t}, \end{aligned} \quad (2)$$

where k indexes conference call, and $\textit{Peer Group Performance}_{i,k,t}$ is the average return of the actual RPE peer group minus the average return of the plausible counterfactual "artificial" peer group constructed using the Bloomfield, Guay, and Timmermans [2022] peer selection algorithm. Lower values for this variable indicate greater underperformance of the *actual* RPE peer group, compared to the artificial peer group.[27]

We tabulate results from this analysis in table 5, panel B. Consistent with the notion that *price*-peer mentions—but not *profit*-peer mentions—relate to peers' contemporaneous stock performance, the coefficient on $rTSR \times ln(1 + Mentions)$ is negative and statistically significant, whereas the main effect of $ln(1 + Mentions)$ is statistically insignificant. Combined, these patterns suggests that when focal firms explicitly mention their price-peers during a conference call, these peers' daily stock returns are, on average, lower than a plausible counterfactual peer group. In economic terms, this

[26] Tabulated stats are at the call level, and as such the rate of peer-mentions can be affected by peer group size. For example, a firm that has many peers might have more overall peer mentions, even if each individual peer is less likely to be discussed. This is relevant because rTSR peer groups tend to be larger than profit-based RPE peer groups. In untabulated tests, we change the unit of observation to be at the call-peer level (as opposed to call level), and examine whether any given peer is more or less likely to be mentioned, as a function of peer type. On average, each individual price-peer has a 1.3% probability of being mentioned at least once, and conditional on being mentioned, gets mentioned roughly 3.5 times. In contrast, each individual profit-peer has a 0.9% chance of being mentioned, and conditional on being mentioned, gets mentioned roughly 1.9 times. That is, compared to a profit-peer, a price-peer is ~45% more likely to be mentioned ($p < 0.05$), and gets mentioned ~185% as many times, conditional on being mentioned at all ($p < 0.01$).

[27] We benchmark the return of firms' actual peer groups against these plausible counterfactuals to accurately assess the impact of *explicit RPE-peer* mentions—and to avoid picking up systematic stock return patterns. For example, if managers were to mention peers by name only when they expect a general downturn in the economy, then actual peers' simple returns need not capture the strategic peer-harm we are interested in. If managers *explicitly* mention peers to harm particular peers' stock prices, then we expect actual peer groups' returns to deviate from plausible counterfactual peer groups' returns.

TABLE 5

Textual Analysis: Peer Mentions and Peer Returns

Panel A: Textual characteristic means by RPE type

Variables	(1) Price RPE	(2) Profit RPE	(3) Dif.
$Mentions_{i,k,t}$	0.416	0.198	0.218***
$Readability_{i,k,t}$	0.005	0.003	0.001
$Length_{i,k,t}$	8.169	8.105	0.064***
$\%Competition_{i,k,t}$	0.926	1.106	−0.180***
$Sentiment_{i,k,t}$	2.453	2.604	−0.151**

Panel B: Relation between peer mentions and peer returns

	Outcome = $Peer\ Group\ Performance_{i,k,t}$								
	(1) Full	(2) Split: Call Time	(3)	(4) Split: Readability	(5)	(6) Split: % Comp.	(7)	(8) Split: Sentiment	(9)
		Final	Nonfinal	Easy	Difficult	High	Low	Negative	Positive
$rTSR_{i,k,t} \times ln(1 + Mentions_{i,k,t})$	−0.261**	−0.514**	−0.108	−0.466***	0.098	−0.371***	0.244	−0.407***	−0.184
	(−2.291)	(−2.000)	(−0.857)	(−3.778)	(0.677)	(3.095)	(0.810)	(−2.648)	(−1.259)
$ln(1 + Mentions_{i,k,t})$	0.145	0.330	0.034	0.301**	−0.154	0.230**	−0.344	0.324**	0.058
	(1.386)	(1.313)	(0.310)	(2.547)	(−1.309)	(2.294)	(−1.206)	(2.245)	(0.464)
$rTSR_{i,k,t}$	0.092	0.219*	0.055	0.193**	0.077	0.103	0.085	0.131	0.191*
	(1.263)	(1.746)	(0.566)	(2.367)	(0.539)	(0.862)	(0.869)	(1.125)	(1.746)
$ln(Length_{i,k,t})$	0.026	−0.051	0.052	0.059*	−0.082	0.064*	−0.093*	−0.0140	0.0120
	(0.743)	(−0.876)	(1.300)	(1.729)	(−1.225)	(1.761)	(−1.844)	(−0.237)	(0.268)
Fixed effects	Firm + Year	Firm + Year		Firm + Year		Firm + Year		Firm + Year	
$\Delta\beta_1$		−0.405, t=−1.414		−0.563***, t=−2.967		−0.616*, t=−1.898		−0.223, t=−1.048	
Observations	3,356	991	2,188	1,678	1,678	1,678	1,678	1,678	1,678
R^2	0.036	0.105	0.060	0.013	0.054	0.052	0.030	0.048	0.059

(*Continued*)

TABLE 5—*(Continued)*

This table presents a textual analysis of the disclosure content in focal firms' earnings calls. Panel A presents means of each disclosure characteristic, by RPE type (price-based vs. profit-based). Panel B presents results from a regression of *Peer Group Performance* on $Mentions$ interacted with $rTSR$. Specification (1) presents results for the full sample. The remaining eight specifications are composed of four specification pairs, which split the sample based on call characteristics: specifications 2 and 3 split the sample based on whether the earnings call is the last one to occur during the year; specifications 4 and 5 split the sample based on whether the language of the call is above- versus below-median readability; specifications 6 and 7 split the sample based on whether there is an above- versus below-median incidence of competition-related words; specifications 8 and 9 split the sample based on whether the tone of the call was below- versus above-median in terms of positive sentiment. Below each specification pair, we present a test of the difference in coefficients on $rTSR \times ln(1 + Mentions)$ across subsamples. Below each coefficient is a *t*-statistic, in parentheses, calculated using standard errors clustered by firm. *, ** and *** indicate two-tailed statistical significance at the 10%, 5% and 1% levels, respectively.

underperformance closely matches the 20 to 30 basis points documented in the preceding empirical analysis. In contrast—and importantly—profit-peers' stock returns are not associated with the extent of profit-peer mentions.

Results in the remaining columns of table 5 show how this pattern varies with particular features of the conference call. In particular, our evidence suggests this pattern is especially pronounced for calls that: (1) occur toward the end of the fiscal year; (2) contain easier language; (3) mention more competition-related words; and (4) are more negative in their sentiment. We note, however, that the differences between coefficients for the subsamples split by call time and sentiment are not statistically significant at conventional inference levels.

Collectively, the results from our textual analysis complement our preceding capital market findings, and suggest that price-peers' underperformance on focal firms' disclosure days relates, in part, to explicit mentions during focal firms' earnings conference calls. Overall, these results provide corroborating evidence that supports our central prediction that disclosure-based sabotage tactics are useful in the context of price-based RPE. That being said, we caveat that these text-based results are likely the tip of the iceberg as there is probably significant peer-harm inflicted that does not involve explicit peer-naming, but relies on more subtle/implicit peer-harming disclosure tactics. Our textual analysis is blind to any of these alternative, more subtle approaches that firms might use.

4.3 TRADING VOLUME

Harming peers is likely a costly action for the disclosing firm. We typically presume that firms' voluntary disclosure policies are optimized for the goal of maximizing their own stock prices (e.g., Verrecchia [1983], Dye [1985]). One major channel through which disclosures provide value is by reducing information asymmetry, thereby increasing stock liquidity (e.g., Diamond and Verrecchia [1991], Leuz and Verrecchia [2000], Bushee and Leuz [2005], Balakrishnan et al. [2014], Leuz and Wysocki [2016]). In this way of thinking, sabotage represents a potentially competing objective. For example, it is unlikely that a voluntary disclosure policy, optimized to reduce information asymmetry and increase liquidity, could be adjusted to incorporate strategic harm without any sacrifice in the informational quality of the disclosures. If a firm deviates from a value-maximizing policy in order to improve relative performance (e.g., by engaging in peer-harming tactics), this presumably comes at the expense of some of these capital market benefits. For example, to effectuate peer-harm, focal firms might change the nature of their voluntary disclosures to provide more (negative) information about peers, coming at the expense of information about themselves.

We assess this possibility by examining the information content in focal firms' disclosures, as reflected by trading volumes (e.g., Blume, Easley, and O'Hara [1994], Gervais, Kaniel, and Mingelgrin [2002]). If rTSR-using focal firms alter their disclosure policies to be more informative about their peers (and potentially less informative about themselves), their voluntary disclosures would likely be more positively associated with their peers' trading volumes and less positively associated with their own trading volumes. We begin by estimating the impact of a focal firm's disclosure on its own trading volume, at the firm-date level, using the following regression specification:

$$\begin{aligned} ln(Volume_{i,t}) = {} & \beta_1 Disc.\ Day_{i,t} \times \overline{rTSR}_{i,t} \\ & + \beta_2 Disc.\ Day_{i,t} + \beta_3 \overline{rTSR}_{i,t} + \tau_t + \theta_i + \varepsilon_{i,t}, \end{aligned} \tag{3}$$

where $ln(Volume)$ is the focal firm's trading volume and $\overline{rTSR}$ reflects the *average* of $rTSR$. As this specification is estimated at the firm-date level, and not the firm-peer-date level, we collapse all of the focal firm's peer relationships into a single scalar. The results of this estimation are presented in table 6, specification 1.

We find that focal firms' trading volumes increase substantially on focal firms' disclosure days, but this effect is significantly muted among focal firms that use more rTSR (i.e., have more price-peers). In terms of economic magnitudes, our coefficients suggest that a firm that uses no rTSR (i.e., $\overline{rTSR} = 0$) experiences a ~130% increase in trading volume on its voluntary disclosure days. A firm that relies heavily on rTSR (i.e., $\overline{rTSR} = 1$) only experiences an ~80% increase in trading volume on its voluntary disclosure days.[28] These results are consistent with firms' voluntary disclosures providing value-relevant information about themselves, but to a lesser extent when they are more reliant on rTSR (and therefore may wish to use disclosures to harm peers, rather than purely to inform their investors about themselves).

We next estimate the impact of a focal firm's disclosures on its peers' trading volumes, at the firm-peer-date level, using variants on the following

[28] Calculations are as follows: $\exp(0.825) - 1 \approx 130\%$; $\exp(0.825 - 0.227) - 1 \approx 80\%$.

TABLE 6

Information Content: Firm and Peer Trading Volumes

	Outcome = $ln(Volume_{i,t})$	Outcome = $ln(Peer\ Volume_{i,j,t})$			
	(1)	(2) Full	(3) Peer Ret.<0%	(4) Peer Ret.<−1%	(5) Peer Ret.<−2%
$Disc.\ Day_{i,t} \times \overline{rTSR}_{i,t}$	−0.227***				
	(−3.606)				
$Disc.\ Day_{i,t} \times rTSR_{i,j,t}$		0.027*	0.035**	0.040**	0.054**
		(1.711)	(2.239)	(2.253)	(2.289)
$Disc.\ Day_{i,t}$	0.825***	0.555***	0.552***	0.491***	0.513***
	(17.773)	(5.135)	(5.024)	(4.220)	(3.583)
$\overline{rTSR}_{i,t}$	0.057				
	(0.861)				
$rTSR_{i,j,t}$		−0.084**	−0.081**	−0.083*	−0.087*
		(−2.059)	(−2.019)	(−1.953)	(−1.784)
$ln(Volume_{i,t})$		0.274***	0.275***	0.261***	0.238***
		(18.955)	(19.281)	(17.939)	(15.624)
$Disc.\ Day_{i,t} \times ln(Volume_{i,t})$		−0.045***	−0.045***	−0.040***	−0.041***
		(−5.895)	(−5.792)	(−5.028)	(−4.312)
Fixed effects	Firm + Year-Month	Firm + Peer + Year-Month	Firm + Peer + Year-Month	Firm + Peer + Year-Month	Firm + Peer + Year-Month
Observations	537,395	7,471,746	3,562,220	1,725,004	828,134
R^2	0.850	0.876	0.877	0.873	0.869

This table presents evidence on the relation between focal firm disclosures and trading volumes. In specification 1, we collapse the sample to the firm-date level and the dependent variable is $ln(Volume)$. In specifications 2–5, the unit of analysis is at the firm-peer-date level and the dependent variable is $ln(Peer\ Volume)$. Specifications 3–5 gradually restrict the sample to focus on more negative peer return days. Below each coefficient is a t-statistic, in parentheses, calculated using standard errors clustered by industry and date. *, ** and *** indicate two-tailed statistical significance at the 10%, 5% and 1% levels, respectively.

regression specification:

$$ln(Peer\ Volume_{i,j,t}) = \beta_1 Disc.\ Day_{i,t} \times rTSR_{i,j,t} + \beta_2 Disc.\ Day_{i,t} + \beta_3 ln(Volume_{i,t}) + \beta_4 Disc.\ Day_{i,t} \times ln(Volume_{i,t}) + \tau_t + \theta_{i,j} + \varepsilon_{i,j,t}, \quad (4)$$

where $ln(Peer\ Volume)$ is the peer's trading volume and $ln(Volume)$ is the focal firm's trading volume. The results of this estimation are presented in table 6, specifications 2–5. Specifications differ with respect to the sample. In specification 2, we use the full sample; in specifications 3–5, we winnow the sample to focus on peers with more negative returns.

We find that focal firms' voluntary disclosures confer a trading volume boost to peers. As a baseline, the effect is about half as strong for peers as for the disclosing focal firm, with profit-peers experiencing a roughly 75% increase in trading volume on focal firms' voluntary disclosure days. However, the effect is significantly stronger for price-peers, on average. Moreover, we find that the incremental volume boost for price-peers is more pronounced among peers with more negative returns on the disclosure date—those that are more likely to have been targeted.

Collectively, these results suggest that rTSR-using firms, on average, provide voluntary disclosures with a different information profile than non-rTSR-using firms. In particular, rTSR-using firms appear to issue voluntary disclosures that are less informative about their own performance/value, and more informative about their peers' performance/value. This is consistent with the notion that rTSR-using firms engage in disclosure-based peer-harming strategies, in which they disseminate negative information about their peers, seemingly in lieu of information about themselves. These findings thereby imply that disclosure-based peer-harming tactics can be costly to the firms that use them; firms appear to sacrifice some of the capital market benefits typically associated with voluntary disclosure in order to perform better in comparison to their RPE peers.

4.4 INFORMATION LEGITIMACY AND SHORT-TERM REVERSALS

The preceding empirical findings suggest that focal firms strategically disclose negative information regarding their peers to the market. However, it is conceivable that focal firms do not provide legitimate negative information, but instead propagate false narratives regarding their peers to depress their stock prices, and the market is temporarily fooled. In the latter case, we would expect to see countervailing forces in the subsequent days/weeks leading to stock price reversals (e.g., harmed peers issuing their own corrective statements to set the record straight).

To assess this possibility, we examine peer returns over the days subsequent to focal firms' disclosures, to ascertain whether there are short-term reversals. To do so, we use the following empirical specification:

$$Peer\ Return_{j,t+1\text{-}k} = \beta_1 rTSR_{i,j,t} \times Peer\ Return_{j,t} + \beta_2 rTSR_{i,j,t} + \beta_3 Peer\ Return_{j,t} + \beta_4 Firm\ Return_{i,t+1\text{-}k} + \tau_t + \theta_{i,j} + \varepsilon_{i,j,t}, \quad (5)$$

where $Peer\ Return_{j,t+1\text{-}k}$ ($Firm\ Return_{i,t+1\text{-}k}$) is the peer's (firm's) buy-and-hold return over the k trading days, starting the day after the firm's disclosure date. We test for reversals within 3 trading days and within 30 trading days and tabulate the results in table 7.

We find no evidence that price-peers' disclosure-day returns reverse any differently from profit-peers'. The lack of reversals suggests that peer-harming disclosures contain legitimate information content regarding price-peers' fair market values, and that the market is not misled by them.[29]

5. *Within-Price-Peer Analyses*

Taken together, the patterns documented in section 4 strongly comport with the theory of RPE-motivated sabotage. We observe that price-peers underperform (relative to profit-peers) on focal firms' disclosure dates in a manner that is difficult to explain via innocuous explanations such as incidental spillover. Moreover, these patterns can be partially explained by explicit peer mentions in firms' earnings calls. However, we acknowledge that peer selection is highly endogenous; firms choose who their RPE peers will be, and what performance metric(s) will be used to assess relative performance. As such, the prior results cannot rule out the possibility that the *choice* to use a given peer as a price-peer, instead of as a profit-peer, biases our results.

To side-step concerns related to the endogeneity of peer selection, we next restrict our focus to price-peers, and examine within-price-peer variation in peer-harm. As rTSR is almost always implemented as a rank-order tournament, peer-harm is only helpful to the focal firm's manager insofar as it is marginal in determining the final rankings. Sabotaging a price-peer in vain, failing to surpass it despite harming it, confers no benefit. Similarly, harming a peer unnecessarily, surpassing it without need of harming it, also confers no benefit. Moreover, harming a peer to the detriment of the focal firm's own performance, allowing other peers to surpass the focal firm, confers no benefit. Therefore, in most instances, the benefits of harming a price-peer will vary substantially across price-peers, and focal firms' managers are likely to direct their peer-harming efforts toward the targets that are most beneficial to sabotage.

It is not ex ante obvious which acts of peer harm are more versus less likely to be marginal in the final rankings. To shed light on the matter,

[29] We caveat that this result does not imply that peer-harming disclosures are *truthful*—merely that they are *informative* and not systematically misunderstood by the market, as a whole. One possible explanation is that firms provide truthful peer-harming disclosures, and market participants interpret them as truthful revelations, in their valuations. Alternatively, firms may systematically bias their peer-harming disclosures (e.g., through exaggeration and/or "cherry-picking"), but rational capital market participants unwind these biases, à la Stein [1989], such that assets are not systematically mispriced. Distinguishing between these possibilities lies beyond the scope of our study.

TABLE 7

Information Legitimacy: Short-Term Return Reversals

	Outcome = $Peer\ Return_{j,t+1\text{-}3}$			Outcome = $Peer\ Return_{j,t+1\text{-}30}$		
	(1)	(2)	(3)	(4)	(5)	(6)
$rTSR_{i,j,t} \times Peer\ Return_{j,t}$	−0.010	−0.012	−0.010	−0.031	−0.030	−0.023
	(−0.301)	(−0.364)	(−0.307)	(−0.369)	(−0.359)	(−0.266)
$rTSR_{i,j,t}$	0.093	0.105	0.235	0.035	0.189	0.234
	(1.057)	(0.658)	(1.413)	(0.139)	(0.389)	(0.439)
$Peer\ Return_{j,t}$	−0.037	−0.035	−0.035	−0.039	−0.041	−0.051
	(−1.303)	(−1.239)	(−1.204)	(−0.520)	(−0.537)	(−0.640)
$Firm\ Return_{i,t+1\text{-}3}$	0.446***	0.445***	0.445***			
	(20.375)	(20.155)	(19.696)			
$Firm\ Return_{i,t+1\text{-}30}$				0.428***	0.434***	0.434***
				(19.044)	(19.157)	(18.568)
Fixed effects	SIC + Peer + Year-Month	Firm + Peer + Year-Month	Firm-Peer + Year-Month	SIC + Peer + Year-Month	Firm + Peer + Year-Month	Firm-Peer + Year-Month
Observations	82,155	82,155	82,155	81,167	81,167	81,167
R^2	0.274	0.278	0.302	0.365	0.368	0.390

This table presents analyses regarding short-term stock return reversals. Specifications differ with respect to cross-sectional fixed effect structure and dependent variables. Specifications 1 and 4 include industry and peer fixed effects; specifications 2 and 5 include firm and peer fixed effects; specifications 3 and 6 include pairwise firm-peer fixed effects. All specifications include year-month fixed effects. In the first (latter) three specifications, the dependent variable is $Peer\ Return_{j,t+1\text{-}3}$ ($Peer\ Return_{j,t+1\text{-}30}$), the RPE peers' buy-and-hold return for the period lasting three (thirty) trading days, starting the day after the focal firm's disclosure date. Below each coefficient is a t-statistic, in parentheses, calculated using standard errors clustered by industry and date. *, ** and *** indicate two-tailed statistical significance at the 10%, 5% and 1% levels, respectively.

we develop a parsimonious model of strategic sabotage in the context of a rank-order TSR tournament. In our model, the manager of a focal firm has some negative information regarding each RPE price-peer, and can choose whether or not to disclose each piece of information to the market. We assume the information will naturally come to light (and thus be priced) in the next performance period, so the manager's disclosure decision is, in effect, whether or not to *accelerate* the revelation into the current period. This transfers the negative TSR impact from the future period into the current period, thereby making it *more* likely that the firm will outperform a harmed peer in the current period, but *less* likely that the firm will outperform a harmed peer in the subsequent period (when the revelation and price impact would otherwise have occurred). Moreover, disclosing the harmful information can have adverse capital market implications for the focal firm, and/or impose personal costs on the manager.

Before making a disclosure decision, the manager observes period-to-date TSRs for their own firm as well as for their peer(s). These period-to-date TSRs inform the manager of the probability of outperforming each peer with and without sabotage, and thus allow the manager to evaluate the net benefits of disclosure. From this simple setup, we generate comparative statics to investigate when the net benefits of disclosing the information (as opposed to withholding the information) are larger versus smaller, which yield several testable implications (see appendix A for details).

First, proximity matters. On average, harming a peer whose period-to-date performance is more similar to the focal firm's is more likely to be beneficial, vis-à-vis the final rankings. Intuitively, if a peer is already insurmountably ahead or far behind the focal firm, the harm from sabotage is unlikely to be a marginal factor in the rankings for the current period. We refer to this preference for harming similarly performing peers as the "proximity" effect.

Second, relative position matters. On average, it tends to be more beneficial to harm a peer that is currently ranked ahead of the focal firm, rather than behind the focal firm. When the peer is behind, the probability of winning is already higher than 50%, so there is less scope for harmful disclosure to improve the probability of ranking above the peer. However, when the peer is ahead, the probability of winning is less than 50%, providing more scope for a harmful disclosure to raise the probability of ranking above the peer (i.e., increasing expected compensation). We refer to this preference for harming better-performing peers as the "aim-up" effect.[30]

Third, the proximity and aim-up effects interact. The proximity effect is stronger when the peer has worse period-to-date performance than the

[30] In addition, the model shows that for a given absolute TSR differential it is *strictly* more beneficial to harm a better-performing peer than a worse-performing peer. This indicates that the aim-up effect should be observable with or without controlling for proximity.

focal firm. The intuition is the following. Among peers with worse period-to-date performance than the focal firm, the benefits of peer-harm strictly increase in proximity. Greater proximity implies that the peer is more likely to surpass the firm, and is thus more beneficial to harm, for the sake of securing the lead. Among peers with better period-to-date performance than the focal firm, the benefits of peer-harm are nonmonotonic in proximity. The benefits are greatest at intermediate degrees of proximity, where surpassing the peer is likely, with harm, but unlikely without harm. Deviations from this intermediate degree of proximity lower the benefits of peer-harm. A decrease in proximity implies that the peer is further ahead and thus less likely to be surpassed, even with peer-harm (lowering the benefits of peer-harm). An increase in proximity implies that the peer is less far ahead, and thus more likely to be surpassed, even without peer-harm (lowering the benefits of peer-harm). These countervailing forces among better-performing peers mute the proximity effect.

Fourth, it is less beneficial to harm a peer if there are other peers whose period-to-date performance is very similar to that of the focal firm. Intuitively, engaging in sabotage entails a capital market cost to the focal firm (e.g., because it requires deviating from the value-maximizing disclosure policy). By incurring this capital market cost, a focal firm loses ground to *all* of the other price-peers. If there are no other price-peers with similar period-to-date performance to the focal firm, this effect may be fairly negligible; if there are several other peers with similar performance, this capital market cost may be substantial enough to act as a deterrent to disclosure-based sabotage. We refer to this as the "density" effect.

We test these predictions as follows. First, we calculate year-to-date performance for each firm-date observation. Second, we calculate year-to-date performance for each price-peer (based on the focal firm's fiscal year, which is not necessarily the same as the peer's fiscal year). We then use these year-to-date performances to construct measures of performance proximity, relative ordering and the density of peers around the focal firm's performance, and examine whether these measures explain variation in price-peers' underperformance on focal firms' disclosure days.[31]

In these tests, we focus only on price-peers. As such, we do not rely on endogenous variation in RPE type to identify our results. Instead, we look within rTSR firm-peer pairs, and exploit variation in year-to-date performance.[32] This variation is not a choice variable for the focal firm, nor the peer, and is difficult to forecast at the time of contracting. However, this

[31] In appendix A.3, we discuss several hypothetical model extensions that yield additional testable implications that we do not formally derive. In section OA3 of the online appendix, we offer further discussion and tabulated results relating to these model-inspired (but not formally derived) predictions.

[32] This is similar to Chevalier and Ellison [1997], who use mutual funds' midyear relative performance to predict forward-looking risk taking.

variation is easy to observe, ex post, and can therefore affect disclosure choices during the performance period.

5.1 PROXIMITY AND AIM-UP EFFECTS

We first test our predictions regarding the proximity and aim-up effects with variants on the following regression specification:

$$\begin{aligned} \textit{Peer Return}_{j,t} = \beta_1 \textit{Proximity}_{i,j,t} + \beta_2 \textit{Peer Above}_{i,j,t} \\ + \beta_3 \textit{Firm Return}_{i,t} + \tau_t + \theta_{i,j} + \varepsilon_{i,j,t}. \end{aligned} \tag{6}$$

As in the baseline analyses, we use three cross-sectional fixed effect structures, θ: industry + peer; firm + peer; and firm × peer. In all analyses, we use time fixed effects, τ, for each year-month combination. For each fixed effect structure, we present two specifications. In odd-numbered (even-numbered) specifications, we run the regression for firms' disclosure (nondisclosure) days. Below each specification pair, we present a statistical test of differences in coefficients on *Proximity* and/or *Peer Above*.[33] Results are tabulated in table 8. In panel A (panel B), we examine *Proximity* (*Peer Above*) on its own. In panel C, we examine *Proximity* and *Peer Above*, jointly.

In panel A, we find that *Proximity* is a significant explanator of peers' underperformance on focal firms' disclosure dates. Peers that are more similar in ranking to the focal firm, at the time of the disclosure, experience significantly worse returns on the disclosure date. On nondisclosure days, *Proximity* appears to have no bearing on peer returns. The difference between disclosure and nondisclosure days is significant at the 1% level in all three specification pairs. In panel B, we find that *Peer Above* is significantly negatively associated with peer returns, on both disclosure days and nondisclosure days. However, the magnitude of the effect is roughly three to four times larger on disclosure days (~10–12 basis points vs. ~2–4 basis points). This difference is significant at the 1% level across all three specification pairs. In panel C, we find that the results from panels A and B are robust to considering the two variables, jointly. In sum, the evidence in table 8 is consistent with deliberate peer-harm, comporting closely with our model's predictions; disclosure day underperformance is most concentrated among those peers whose stock price depressions would be most likely to result in increased compensation for the disclosing firm's CEO.

5.2 TARGETING UP VERSUS DOWN

A more nuanced testable implication of our model is that the proximity and aim-up effects interact. That is, our model suggests that the proximity

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[33] The identifying assumption underlying these tests is the following: after controlling for contemporaneous firm returns and subsuming cross-sectional and time-series fixed effects, price-peers would not be systematically different in their stock return behavior on focal firms' disclosure dates, as a function of their period-to-date TSR relative to the focal firm, but for focal firms' incentives to target particular price-peers.

TABLE 8

Strategic Targeting: Proximity and Aim-Up Effects

Panel A: Proximity

	Outcome = $Peer\ Return_{j,t}$					
	(1) Disc.	(2) Nondisc.	(3) Disc.	(4) Nondisc.	(5) Disc.	(6) Nondisc.
$Proximity_{i,t,j}$	-0.124^{***}	0.003	-0.128^{***}	0.003	-0.147^{***}	0.002
	(−3.226)	(0.529)	(−3.314)	(0.735)	(−3.246)	(0.339)
$Firm\ Return_{i,t}$	0.180^{***}	0.595^{***}	0.182^{***}	0.595^{***}	0.183^{***}	0.596^{***}
	(10.688)	(32.450)	(10.734)	(32.458)	(10.529)	(32.452)
$\Delta\beta_1$	-0.126^{***}, $t = -3.289$		-0.131^{***}, $t = -3.395$		-0.149^{***}, $t = -3.340$	
Fixed effects	SIC + Peer + Year-Month		Firm + Peer + Year-Month		Firm-Peer + Year-Month	
Observations	69,610	5,966,306	69,610	5,966,306	69,610	5,966,306
R^2	0.168	0.337	0.172	0.337	0.196	0.338

Panel B: Peer above

	Outcome = $Peer\ Return_{j,t}$					
	(1) Disc.	(2) Nondisc.	(3) Disc.	(4) Nondisc.	(5) Disc.	(6) Nondisc.
$Peer\ Above_{i,j,t}$	-0.098^{***}	-0.023^{***}	-0.102^{***}	-0.029^{***}	-0.121^{***}	-0.043^{***}
	(−3.717)	(−4.478)	(−3.845)	(−5.396)	(−4.010)	(−7.622)
$Firm\ Return_{i,t}$	0.180^{***}	0.595^{***}	0.182^{***}	0.596^{***}	0.183^{***}	0.596^{***}
	(10.690)	(32.449)	(10.739)	(32.457)	(10.537)	(32.453)
$\Delta\beta_1$	-0.074^{***}, t=-2.988		-0.074^{***}, t=-2.956		-0.078^{***}, t=-2.892	
Fixed effects	SIC + Peer + Year-Month		Firm + Peer + Year-Month		Firm-Peer + Year-Month	
Observations	69,610	5,966,306	69,610	5,966,306	69,610	5,966,306
R^2	0.168	0.337	0.172	0.337	0.197	0.338

Panel C: Proximity and peer above, jointly

	Outcome = $Peer\ Return_{j,t}$					
	(1) Disc.	(2) Nondisc.	(3) Disc.	(4) Nondisc.	(5) Disc.	(6) Nondisc.
$Proximity_{i,j,t}$	-0.124^{***}	0.003	-0.128^{***}	0.004	-0.147^{***}	0.002
	(−3.227)	(0.547)	(−3.317)	(0.758)	(−3.264)	(0.397)
$Peer\ Above_{i,j,t}$	-0.098^{***}	-0.023^{***}	-0.103^{***}	-0.029^{***}	-0.121^{***}	-0.043^{***}
	(−3.714)	(−4.480)	(−3.845)	(−5.398)	(−4.005)	(−7.624)
$Firm\ Return_{i,t}$	0.180^{***}	0.595^{***}	0.182^{***}	0.596^{***}	0.183^{***}	0.596^{***}
	(10.691)	(32.448)	(10.741)	(32.457)	(10.539)	(32.453)
Fixed effects	SIC + Peer + Year-Month		Firm + Peer + Year-Month		Firm-Peer + Year-Month	
$\Delta\beta_1$	-0.127^{***}, t=-3.292		-0.131^{***}, t=-3.400		-0.149^{***}, t=-3.353	
$\Delta\beta_2$	-0.074^{***}, t=-2.987		-0.074^{***}, t=-2.959		-0.078^{***}, t=-2.889	
Observations	69,610	5,966,306	69,610	5,966,306	69,610	5,966,306
R^2	0.168	0.337	0.172	0.337	0.197	0.338

(*Continued*)

TABLE 8—*(Continued)*

This table presents results on the relation between year-to-date TSR performance and *Peer Returns*. The primary independent variables are *Proximity* and *Peer Above*. In panel A, we examine the effect of *Proximity*, on its own. In panel B, we examine the effect of *Peer Above*, on its own. In panel C, we examine the effects of *Proximity* and *Peer Above*, jointly. In all panels, the sample is restricted to price-peers. In odd-numbered (even-numbered) specifications, the sample consists of focal firms' disclosure (nondisclosure) days. Below each specification pair, we present a test of the difference in coefficients on *Proximity* and/or *Peer Above* across disclosure and nondisclosure days. Specification pairs differ with respect to cross-sectional fixed effect structure. Specifications 1 and 2 include industry and peer fixed effects; specifications 3 and 4 include firm and peer fixed effects; specifications 5 and 6 include pairwise firm-peer fixed effects. All specifications include year-month fixed effects. Below each coefficient is a *t*-statistic, in parentheses, calculated using standard errors clustered by industry and date. *, ** and *** indicate two-tailed statistical significance at the 10%, 5% and 1% levels, respectively.

TABLE 9
Strategic Targeting: Above Versus Below

	Outcome = $Peer\ Return_{j,t}$					
	(1) Below	(2) Above	(3) Below	(4) Above	(5) Below	(6) Above
$Proximity_{i,j,t}$	-0.355^{***}	0.106	-0.365^{***}	0.107	-0.470^{***}	0.142
	(−4.735)	(1.532)	(−4.954)	(1.502)	(−5.028)	(1.645)
$Firm\ Return_{i,j,t}$	0.177^{***}	0.191^{***}	0.178^{***}	0.193^{***}	0.180^{***}	0.195^{***}
	(9.773)	(10.410)	(9.752)	(10.456)	(9.411)	(9.948)
$\Delta\beta_1$	-0.461^{***}, t=−3.917		-0.472^{***}, t=−3.959		-0.612^{***}, t=−3.996	
Fixed effects	SIC + Peer + Year-Month		Firm + Peer + Year-Month		Firm-Peer + Year-Month	
Observations	36,424	33,186	36,424	33,186	36,424	33,186
R^2	0.187	0.208	0.192	0.217	0.230	0.258

This table presents evidence on the asymmetric relation between year-to-date TSR performance and *Peer Return*, split by whether the peer's to-date TSR is above versus below the focal firm's. We restrict the sample to only include price-peers on focal firms' disclosure dates. The primary independent variable is *Proximity*. In odd-numbered (even-numbered) specifications, we present regression results estimated observations in which *Peer Above* = 0 (*Peer Above* = 1). Below each specification pair, we present a test of the difference in coefficients on *Proximity*. Specification pairs differ with respect to cross-sectional fixed effect structure. Specifications 1 and 2 include industry and peer fixed effects; specifications 3 and 4 include firm and peer fixed effects; specifications 5 and 6 include pairwise firm-peer fixed effects. All specifications include year-month fixed effects. Below each coefficient is a *t*-statistic, in parentheses, calculated using standard errors clustered by industry and date. *, ** and *** indicate two-tailed statistical significance at the 10%, 5% and 1% levels, respectively.

effect is stronger when *Peer Above* = 0. The intuition is the following. Managers benefit most from harming a peer whose performance is somewhat above the focal firm in period-to-date performance. For peers with inferior period-to-date performance (i.e., *Peer Above* = 0), greater proximity implies that the peer is closer to the optimal targeting point, so it is more likely to be targeted (and thus have poor returns). In contrast, for peers with superior period-to-date performance (i.e., *Peer Above* = 1), greater proximity could result in the peer being closer to, or further from, the optimal targeting point—the effect of proximity is ambiguous, muting the proximity effect.

We examine this implication by estimating the relation between *Proximity* and *Peer Return* separately for cases with *Peer Above* = 0 and *Peer Above* = 1. We report these results in table 9. In odd-numbered

TABLE 10
Strategic Targeting: Peer Density

Panel A: Density

	Outcome = $Peer\ Return_{j,t}$					
	(1) Disc.	(2) Nondisc.	(3) Disc.	(4) Nondisc.	(5) Disc.	(6) Nondisc.
$Density_{i,j,t}$	0.232**	−0.005	0.239**	−0.006	0.245**	−0.007
	(2.032)	(−0.543)	(2.060)	(−0.592)	(2.015)	(−0.685)
$Firm\ Return_{i,t}$	0.180***	0.595***	0.182***	0.595***	0.182***	0.596***
	(10.683)	(32.446)	(10.724)	(32.454)	(10.520)	(32.448)
$\Delta\beta_1$	0.237**, t=2.112		0.245**, t=2.150		0.252**, t=2.148	
Fixed effects	SIC + Peer + Year-Month		Firm + Peer + Year-Month		Firm-Peer + Year-Month	
Observations	69,610	5,966,306	69,610	5,966,306	69,610	5,966,306
R^2	0.168	0.337	0.172	0.337	0.196	0.338

Panel B: Proximity, peer above and density, jointly

	Outcome = $Peer\ Return_{j,t}$					
	(1) Disc.	(2) Nondisc.	(3) Disc.	(4) Nondisc.	(5) Disc.	(6) Nondisc.
$Proximity_{i,j,t}$	−0.137***	0.003	−0.139***	0.004	−0.161***	0.002
	(−3.619)	(0.579)	(−3.690)	(0.787)	(−3.643)	(0.433)
$Peer\ Above_{i,j,t}$	−0.100***	−0.023***	−0.104***	−0.029***	−0.123***	−0.043***
	(−3.689)	(−4.464)	(−3.829)	(−5.384)	(−3.987)	(−7.609)
$Density_{i,j,t}$	0.248**	−0.004	0.253**	−0.004	0.263**	−0.005
	(2.178)	(−0.408)	(2.198)	(−0.461)	(2.174)	(−0.489)
$Firm\ Return_{i,j,t}$	0.180***	0.595***	0.182***	0.595***	0.183***	0.596***
	(10.688)	(32.444)	(10.735)	(32.453)	(10.533)	(32.449)
$\Delta\beta_1$	−0.139***, t=−3.685		−0.143***, t=−3.774		−0.163***, t=−3.740	
$\Delta\beta_2$	−0.076***, t=−2.979		−0.075***, t=−2.960		−0.080***, t=−2.889	
$\Delta\beta_3$	0.251**, t=2.250		0.258**, t=2.279		0.268**, t=2.296	
Fixed effects	SIC + Peer + Year-Month		Firm + Peer + Year-Month		Firm-Peer + Year-Month	
Observations	69,610	5,966,306	69,610	5,966,306	69,610	5,966,306
R^2	0.169	0.337	0.173	0.337	0.197	0.338

This table presents results on the relation between year-to-date TSR performances, *Peer Return*. The empirical specification exactly matches that of table 8, but adds *Density* as an independent variable. In panel A, we examine the effect of *Density*, on its own. In panel B, we examine the effects of *Proximity*, *Peer Above* and *Density*, jointly. Below each coefficient is a t-statistic, in parentheses, calculated using standard errors clustered by industry and date. *, ** and *** indicate two-tailed statistical significance at the 10%, 5% and 1% levels, respectively.

(even-numbered) specifications, the sample is all observations for which $Peer\ Above = 0$ ($Peer\ Above = 1$). Consistent with our model's predictions, the relation between *Proximity* and *Peer Return* is significantly more negative when $Peer\ Above = 0$. In odd-numbered specifications, *Proximity* and *Peer Return* have a negative relation that is roughly three times stronger than in table 8. In even-numbered specifications, the point estimates switch sign (becoming slightly positive) and are indistinguishable from zero at conventional inference levels. Across all three specification pairs, the difference across specifications is significant at the 1% level.

5.3 PEER DENSITY

The prior analyses are based on our single-peer model (appendix A.1). Our extended model considers multiple peers, and offers an additional testable implication: focal firms are likely to withhold their peer-harming information when *other* peers have period-to-date TSRs similar to the focal firm (appendix A.2). The intuition for this prediction is the following. If peer-harming disclosure carries a capital market cost to the focal firm (e.g., there is some negative effect on liquidity/cost of capital, or some blowback valuation damage from the harming disclosure), then issuing a peer-harming disclosure raises the probability that *other* peers outperform the focal firm. This effect can be quite negligible if there are no other peers with similar performance to the focal firm, but it could also be quite substantial if there are many other peers whose performance is neck-and-neck with the focal firm. In these instances, harming one peer might allow multiple other peers to outperform the focal firm, making the act of peer-harm counterproductive, vis-à-vis the eventual ranking outcome.

We test this prediction by modifying equation (6) to include *Density* and examining whether *Density* is positively related to price-peers' returns. We tabulate these results in table 10. In panel A, we consider *Density* on its own. In panel B, we consider *Proximity*, *Peer Above*, and *Density*, jointly. The results align with our predictions. On disclosure days, *Density* has a significantly positive coefficient; on nondisclosure days, *Density* has an economically minuscule and statistically insignificant coefficient. This difference between coefficients is significant at the 5% level, in all cases. Moreover, adding *Density* does not qualitatively impact the *Proximity* or *Peer Above* results. In sum, the results appear to comport with our model's predictions: *Proximity* and *Peer Above* are associated with lower peer returns, on disclosure days, while greater levels of peer density appear to mitigate peer harm.

6. *Conclusion*

We provide evidence to suggest that rTSR-using firms routinely engage in disclosure-based peer-harming tactics against their RPE peers. The peer-harming information in firms' disclosures appears to be legitimate (i.e., not misinformation), and in many cases appears to be communicated explicitly, with peers being mentioned by name in the harmful disclosures. Although our analysis cannot directly establish managerial intent, collectively the evidence is consistent with the theory of RPE-induced sabotage. Our study provides the first direct evidence of RPE-motivated peer-harm in the context of CEO compensation.

We see our study as providing some initial evidence regarding managers' strategic internalization of disclosure spillovers as a result of their RPE incentives. There is still much to learn about these disclosure strategies, and whether/how other agents anticipate and/or react to them. With respect to firms' RPE peers, future research could shed light on important related questions, such as: what exactly do firms disclose to harm their peers? Do RPE price-peers change their disclosure behavior, ex ante, in anticipation of being harmed (e.g., by issuing preemptive disclosures of their own)? Do harmed price-peers change their disclosure behavior, ex post, either by engaging in "damage control" or by retaliating against the firms that harmed them? With respect to investors, an important question to address is: to what extent do investors anticipate firms' harmful disclosures, and draw valuation inferences from their absence? We hope that future theoretical work can build on our findings to develop a more comprehensive theory of disclosure in the presence of internalized spillovers. Such a framework could be useful for understanding how disclosure practices are influenced by rTSR incentives as well as other considerations, such as M&A activity, etc.

As a caveat, we examine only a single peer-harming channel (i.e., disclosure) out of a large set of potential approaches to peer-harming. Although disclosure-based peer-harming tactics provide many advantages over operations-based tactics, we consider it quite likely that firms also utilize alternative tactics. As such, we view our study as setting the lower bound for the overall prevalence of RPE-induced inter-firm peer-harm. We hope future work is able to shed light on whether, and to what extent, firms use other sabotage strategies, such as aggressive price cutting, excess production, market-stealing advertising campaigns, product harmonization and/or labor talent poaching.

APPENDIX A: MODEL OF DISCLOSURE SABOTAGE IN A TSR TOURNAMENT

In this appendix, we develop a parsimonious model of disclosure-based sabotage. We first consider a focal firm with a single price-peer, and examine the conditions under which sabotage is more versus less advantageous to the manager of the focal firm (section A.1). In an extension, we allow for the firm to have multiple peers, show that our findings from the single peer case remain intact, and derive an additional testable prediction (section A.2). We further discuss potential modifications such as: incorporating Bayesian inference from nondisclosure; allowing peers to issue reactive disclosures; endogenizing disclosure timing with an option to wait; and accommodating convex compensation schemes (section A.3).

A.1 SINGLE-PEER CASE

A focal firm, $i = 0$, competes against a peer, $i = 1$, in a rank-order TSR tournament. Over the course of each period $\tau \in \{1, 2\}$, each firm's TSR, $x_\tau^i(t)$ for time $t \in [0, 1]$, naturally follows an independent Brownian process with drift μ and volatility σ^2 such that:

$$x_\tau^i(t) \sim N\left[\mu t, t^2\sigma^2\right]. \tag{A.1}$$

At the end of each period, the manager of the focal firm receives a bonus, b, if the firm's ending TSR value, $v_\tau^0 \equiv x_\tau^0(1)$ exceeds that of the peer's, $v_\tau^1 \equiv x_\tau^1(1)$.[34]

We consider the case in which the focal firm's manager is endowed with some damaging private information regarding its peer that would, if revealed to the market, immediately reduce the peer's TSR by $h > 0$.[35] For example, this could be some information about a risk factor that is relevant to the peer's stock price, or it could be the economic harm of some real actions the focal firm has taken (or plans to take), that are not yet known by the market. We assume that the manager's private information will naturally become known by the market during the next performance period, but at time $t \in [0, 1)$ during period 1, the manager has the option to disclose this information to the market, accelerating its valuation effect into the current period.[36]

This implies that disclosing harmful information is costly to the focal firm's manager. When the focal firm discloses negative information at $t \in [0, 1)$, it lowers the peer's TSR for $\tau = 1$ by h, but also raises the peer's TSR for $\tau = 2$ by h. This is because the disclosure choice accelerates the harm that would naturally have been priced during period 2 into period 1. In short, by revealing harmful information the focal firm increases the chance of outperforming the peer in the current period, but decreases its chance of outperforming the peer in the subsequent period. To the extent that the manager cares about relative ranking in the next period, this represents a cost of sabotage.

In addition, we allow for two other costs of sabotage. First, we allow for the possibility that sabotaging the peer has a blowback effect to the focal firm. We model this in reduced form by assuming that the focal firm's TSR

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[34] For ease of exposition, we consider uncorrelated Brownian processes. Our analysis extends without loss of generality to allow for any arbitrary correlation, where σ^2 represents the variance of the idiosyncratic portions of the firms' TSRs.

[35] In practice, there is likely to be considerable heterogeneity in a manager's endowment of damaging information, and some signals will be far more damaging than others. However, as we cannot empirically assess a manager's endowment of information, nor the harmfulness of their private signals, we abstract from these complications, and simply assume that h is an exogenous constant parameter. We further assume that the capital market is unaware of the manager's endowment of this information; in a rational capital market, investors who become aware that the manager has this bad news would react in the same way whether or not the manager chooses to disclose it, rendering the disclosure decision irrelevant.

[36] As a caveat, we do not consider a rational-expectations equilibrium whereby investors' rationally anticipate peer-harming disclosures, and draw inferences from nondisclosure. Specifically, we assume that the market does not infer anything about the focal firm, nor the peer, from silence. Incorporating rational expectations and learning from nondisclosure would complicate our analysis, likely making it intractable. However, provided there is sufficient friction to prevent perfect revelation (e.g., an exogenous disclosure cost, uncertainty about information endowment, uncertainty about managers' RPE incentives, etc.), we do not expect that allowing the market to update based on nondisclosure would qualitatively affect our model's predictions. We discuss the matter in more detail in section A.3.1.

in period 1 reduces by γh when it harms the peer by an amount of h, with $\gamma \in [0, 1)$. This reflects the fact that, absent RPE incentives, the firm would presumably use a value-maximizing disclosure strategy. Sabotage represents a competing disclosure incentive, which likely requires some compromises, causing the firm to forgo some of the usual capital-market benefits of disclosure. For example, sabotage disclosures may involve information that is somewhat harmful to the focal firm's own value (but more harmful to the peer). Alternatively, the harmful information itself could be irrelevant to the focal firm's value, but disclosing it comes at the expense of providing other valuation relevant information, which could have adverse liquidity effects for the focal firm. We do not explicitly model the microstructure of how the sabotage strategy harms the focal firm's TSR.

Second, we also allow for an exogenous personal cost of sabotage, c. This could capture a manager's personal disutility from engaging in sabotage, or perhaps the manager's concern about litigation arising from this behavior. We do not place any restrictions on c. In principle, it could be zero or even negative, if a manager gets personal enjoyment from engaging in sabotage. Large magnitudes of c yield trivial solutions; as $c \to \infty$ ($c \to -\infty$), the manager will never (always) engage in sabotage, irrespective of the other circumstances.

Before making the disclosure decision, the manager of the focal firm observes a noisy signal of end-of-period standings in the form of the period-to-date TSRs, as of time t: $x_1^0(t)$ and $x_1^1(t)$. Absent disclosure, the eventual TSR outcomes for period $\tau = 1$ are given by:

$$v_1^i = x_1^i(t) + (1-t)\mu + \epsilon_i, \tag{A.2}$$

with ϵ's drawn independently from $N[0, (1-t)^2\sigma^2]$. Conditional on the period-to-date TSRs, the ending TSR differential, $\Delta_1 \equiv v_1^1 - v_1^0$ is drawn from $N[\delta, 2(1-t)^2\sigma^2]$, where $\delta \equiv x_1^1(t) - x_1^0(t)$ is the current TSR differential between the peer and the focal firm, and $2(1-t)^2\sigma^2$ represents the remaining uncertainty regarding the firms' natural end-of-period TSR differential.[37]

At time t, without disclosing harmful peer information, the probabilities of being ranked first in periods 1 and 2, respectively, are:

$$\Pr\left(v_1^0 > v_1^1|\delta\right) = \int_{-\infty}^{\infty}\left(\int_{-\infty}^{\epsilon_0-\delta} f_\epsilon(\epsilon_1)\,d\epsilon_1\right) f_\epsilon(\epsilon_0)\,d\epsilon_0 = \Phi\left(\frac{-\delta}{\sqrt{2}(1-t)\sigma}\right) \text{ and} \tag{A.3}$$

$$\Pr\left(v_2^0 > v_2^1|\delta\right) = \int_{-\infty}^{\infty}\left(\int_{-\infty}^{v_2^0} f_v\left(v_2^1\right)dv_{1,2}\right) f_v\left(v_2^0\right)dv_2^0 = \Phi(0), \tag{A.4}$$

[37] Note that assuming correlated ϵ's merely changes the conditional variance of Δ_1 and, thus, does not qualitatively affect our analysis.

where f_ϵ and f_v are the PDFs for ϵ and v, respectively, and where $\Phi(\cdot)$ is the CDF for the standard normal distribution. Assuming that the manager receives a bonus b for ranking first in any given period, the expected utility of the focal firm's manager is given by

$$U(\delta) = b\Phi\left(\frac{-\delta}{\sqrt{2}(1-t)\sigma}\right) + b\Phi(0). \tag{A.5}$$

At time t, if the manager discloses harmful peer information, the probability of being ranked first in periods 1 and 2 is:

$$\Pr\left(v_1^0 > v_1^1|\delta\right) = \int_{-\infty}^{\infty}\left(\int_{-\infty}^{\epsilon_0-\delta-\gamma h} f_\epsilon(\epsilon_1+h)\,d\epsilon_1\right) f_\epsilon(\epsilon_0)\,d\epsilon_0 = \Phi\left(\frac{-\delta+(1-\gamma)h}{\sqrt{2}(1-t)\sigma}\right) \text{ and} \tag{A.6}$$

$$\Pr\left(v_2^0 > v_2^1|\delta\right) = \int_{-\infty}^{\infty}\left(\int_{-\infty}^{v_2^0-h} f_v\left(v_2^1\right) dv_2^1\right) f_v\left(v_2^0\right) dv_2^0 = \Phi\left(\frac{-h}{\sqrt{2}\sigma}\right), \tag{A.7}$$

with the expected utility given by

$$U^h(\delta) = b\Phi\left(\frac{-\delta+(1-\gamma)h}{\sqrt{2}(1-t)\sigma}\right) + b\Phi\left(\frac{-h}{\sqrt{2}\sigma}\right) - c. \tag{A.8}$$

If the manager discloses harmful information, the peer's period-1 TSR reduces by h and the focal firm's period-1 TSR reduces by γh, which increases the range of remaining returns, ϵ_0 and ϵ_1, where $v_1^0 > v_1^1$. However, the harm reverses in the second period, making it less likely that $v_2^0 > v_2^1$. In addition, the manager faces cost c from disclosing harmful information. The following Proposition compares equations (A.5) and (A.8) to characterize the manager's disclosure decision.

Proposition A.1. *The focal firm's manager will disclose harmful peer information when*

$$B(\delta) - C > 0 \tag{A.9}$$

with $B(\delta) = \Phi\left(\frac{-\delta+(1-\gamma)h}{\sqrt{2}(1-t)\sigma}\right) - \Phi\left(\frac{-\delta}{\sqrt{2}(1-t)\sigma}\right)$ *and* $C = \frac{c}{b} + \Phi(0) - \Phi\left(\frac{-h}{\sqrt{2}\sigma}\right)$.

The equilibrium condition in equation (A.9) has three components. First, the difference in the probability of a period-1 bonus when the manager chooses to disclose harmful information, $\Phi\left(\frac{-\delta+(1-\gamma)h}{\sqrt{2}(1-t)\sigma}\right) - \Phi\left(\frac{-\delta}{\sqrt{2}(1-t)\sigma}\right)$. Second, the difference in the probability of a period-2 bonus when the manager chooses to disclose harmful information, $\Phi\left(\frac{-h}{\sqrt{2}\sigma}\right) - \Phi(0)$. Third, the personal cost of disclosure, c, relative to the potential bonus, b. Note that Proposition A.1 allows a variety of solutions. For sufficiently small (large) values of C, the manager will always (never) choose to disclose harmful peer information, independent of the period-to-date rTSR. For intermediate values of C, the equilibrium admits a strategy, where the manager discloses harmful information for some realizations of δ but not others.

The following Proposition characterizes the equilibrium condition from equation (A.9).

Proposition A.2. *In the equilibrium condition (A.9),*

(1) The term $C = \frac{c}{b} + \Phi(0) - \Phi\left(\frac{-h}{\sqrt{2}\sigma}\right)$ *is independent of the period-to-date rTSR,* δ*;*

(2) The term $B(\delta) = \Phi\left(\frac{-\delta+(1-\gamma)h}{\sqrt{2}(1-t)\sigma}\right) - \Phi\left(\frac{-\delta}{\sqrt{2}(1-t)\sigma}\right)$ *is single-peaked at* $\frac{(1-\gamma)h}{2}$ *and is symmetric in* δ *around* $\frac{(1-\gamma)h}{2} > 0$.

Proof. Clearly, $\frac{d}{d\delta}\left(\frac{c}{b} + \Phi(0) - \Phi\left(\frac{-h}{\sqrt{2}\sigma}\right)\right) = 0$ and $\frac{dB(\delta)}{d\delta} = \frac{1}{\sqrt{2}(1-t)\sigma}\left(\phi\left(\frac{-\delta}{\sqrt{2}(1-t)\sigma}\right) - \phi\left(\frac{-\delta+(1-\gamma)h}{\sqrt{2}(1-t)\sigma}\right)\right)$, where $\phi(\cdot)$ is the PDF of a standard normal distribution. Note that both $\phi(\cdot)$ are PDFs of normal distributions with the same variance but different means. Thus, for $(1-\gamma)h > 0$, there exists a unique solution for $\phi\left(\frac{-\delta}{\sqrt{2}(1-t)\sigma}\right) - \phi\left(\frac{-\delta+(1-\gamma)h}{\sqrt{2}(1-t)\sigma}\right) = 0$. This solution is given by $\left(\frac{-\delta}{\sqrt{2}(1-t)\sigma}\right)^2 = \left(\frac{-\delta+(1-\gamma)h}{\sqrt{2}(1-t)\sigma}\right)^2$ or $\delta = \frac{(1-\gamma)h}{2}$. Because $\frac{d}{d\delta}\left(\frac{dB(\delta)}{d\delta}\right)|_{\delta=\frac{(1-\gamma)h}{2}} < 0$, $B(\delta)$ is single-peaked at $\frac{(1-\gamma)h}{2}$.

Finally, note that $B(\delta)$ is symmetric around $\frac{(1-\gamma)h}{2}$, because for any $\omega \in \mathbb{R}$:

$$\phi\left(\frac{-\left(\frac{(1-\gamma)h}{2}+\omega\right)}{\sqrt{2}(1-t)\sigma}\right) - \phi\left(\frac{-\left(\frac{(1-\gamma)h}{2}+\omega\right)+(1-\gamma)h}{\sqrt{2}(1-t)\sigma}\right) = -\left(\phi\left(\frac{-\left(\frac{(1-\gamma)h}{2}-\omega\right)}{\sqrt{2}(1-t)\sigma}\right) - \phi\left(\frac{-\left(\frac{(1-\gamma)h}{2}-\omega\right)+(1-\gamma)h}{\sqrt{2}(1-t)\sigma}\right)\right).$$

□

The proposition shows that C is independent of the observed period-to-date TSR differential. This term captures the impact of disclosing harmful peer information on the period-2 expected bonus payment, as well as the personal cost of disclosure. Because each firm's TSR is set to zero at the beginning of the second period, the observed period-to-date TSR values in period 1 have no information content for the ranking in the following period.

Further, note that for $h > 0$, $\Phi\left(\frac{-\delta+(1-\gamma)h}{\sqrt{2}(1-t)\sigma}\right) - \Phi\left(\frac{-\delta}{\sqrt{2}(1-t)\sigma}\right) > 0$, which shows that for $C \leq 0$, the manager will always choose to disclose harmful peer information. This can only happen when the manager receives a sufficiently large personal benefit from disclosing harmful information ($c \ll 0$) or in the knife-edged case where there are no personal costs and where $\sigma \to \infty$. However, as the difference in probabilities is bounded above by 1, the manager will never disclose if $\frac{c}{b} > 1$. Clearly, when the personal cost of disclosure is larger than the bonus payment, no disclosure is optimal. Proposition A.2 further indicates that when C is positive but not too large, the manager of the focal firm is most likely to disclose harmful peer information when $\delta = \frac{(1-\gamma)h}{2}$. That is, when the peer firm is ahead of the focal firm, but not too far, disclosing harmful information has the largest impact on the probability of achieving the bonus in period 1. For intermediate values of C, such that disclosure happens for some, but not all, values of δ, the disclosure region is characterized by two thresholds: $\frac{(1-\gamma)h}{2} \pm \theta$ where θ is a function of all the exogenous parameters, which cannot be expressed in closed-form. That is, disclosure happens for values of $\delta \in [\frac{(1-\gamma)h}{2} - \theta, \frac{(1-\gamma)h}{2} + \theta]$ and the manager chooses not to disclose for δ outside of this region.

The following corollaries further characterize the impact of δ on the effect of harmful disclosure on the probability of receiving a bonus in period 1, $B(\delta)$. In all of the ensuing analysis, we consider a possible disclosure choice, at a fixed point in time, t.

Corollary A.1. *For a given value* $\delta > 0$, $B(\delta) > B(-\delta)$.

Proof. Because $B(\delta)$ is single-peaked at $\frac{(1-\gamma)h}{2} > 0$ and symmetric around $\frac{(1-\gamma)h}{2}$, $B(\delta) > B(-\delta)$ holds for any $\delta > 0$. □

Corollary A.2. *On average, the utility gain of peer-harming disclosure is greater when* $\delta > 0$.

Proof. Following Corollary 1, for any point $\delta > 0$, $B(\delta) > B(-\delta)$. Because the distribution of δ is symmetric about zero, the density of δ is the same as that of $-\delta$. Therefore, for any $\delta > 0$, $B(\delta)\, f_\delta(\delta)$ pointwise dominates $B(-\delta)\, f_\delta(-\delta)$, thus implying that $E[B(\delta)|t, \delta > 0] > E[B(\delta)|t, \delta < 0]$. □

Corollaries A.1 and A.2 show that focal firms are more likely to benefit from harmful disclosure when the peer firm is ahead, with or without controlling for the period-to-date TSR differential, δ.

To provide further empirical implications, the next proposition and the following corollaries investigate effects of period-to-date TSR proximity (i.e., smaller values of $|\delta|$) between peer and focal firm on the expected $B(\delta)$, the change in period-1 winning probability due to a peer-harming disclosure. Note, in what follows, we condition on the sign of δ because proximity increases in δ for $\delta < 0$, but decreases in δ for $\delta > 0$. Thus, the marginal effect of proximity on the benefit of peer-harm is captured by $\frac{dB(\delta)}{d\delta}$ when δ is negative and $-\frac{dB(\delta)}{d\delta}$ when δ is positive.

Proposition A.3. *The expected marginal effects of proximity on the benefit of peer-harm when the peer is ahead (i.e.,* $\delta > 0$*) and when the peer is behind (i.e.,* $\delta < 0$*) are given by, respectively:*

$$E\left[\frac{dB(\delta)}{d\delta}|\delta > 0, t\right] = \alpha_1 \times \left(1 - 2\exp\left(-\alpha_2\right) \times \Phi\left(\alpha_3\right)\right) \text{ and} \tag{A.10}$$

$$E\left[\frac{dB(\delta)}{d\delta}|\delta < 0, t\right] = \alpha_1 \times \left(1 - 2\exp\left(-\alpha_2\right) \times \left(1 - \Phi\left(\alpha_3\right)\right)\right), \tag{A.11}$$

with $\alpha_1 = \frac{1}{2\sigma\sqrt{\pi\left((1-t)^2+t^2\right)}} > 0$, $\alpha_2 = \frac{1}{4\sigma^2}\frac{(1-\gamma)^2 h^2}{(1-t)^2+t^2} > 0$, *and* $\alpha_3 = \frac{t}{1-t}\frac{(1-\gamma)h}{\sigma\sqrt{2((1-t)^2+t^2)}} > 0$.

Proof.

$$
\begin{aligned}
E\left[\frac{dB(\delta)}{d\delta}|\delta > 0, t\right] &= 2\int_0^\infty \left(\frac{dB(\delta)}{d\delta}\phi\left(\frac{\delta}{\sqrt{2}t\sigma}\right)\right)d\delta \\
&= 2\frac{1}{\sqrt{2}(1-t)\sigma}\left(\int_0^\infty \frac{e^{-\frac{\delta^2}{2\left(\frac{\sqrt{2}(1-t)t\sigma}{\sqrt{(1-t)^2+t^2}}\right)^2}}}{2\sqrt{2}\pi t\sigma}d\delta - \int_0^\infty \frac{e^{-\left(\frac{(\delta-(1-\gamma)h)^2}{2\left(\sqrt{2}(1-t)\sigma\right)^2}+\frac{\delta^2}{2\left(\sqrt{2}t\sigma\right)^2}\right)}}{2\sqrt{2}\pi t\sigma}d\delta\right) \\
&= \alpha_1 - 2\frac{1}{\sqrt{2}(1-t)\sigma}\int_0^\infty \frac{e^{-\left(\alpha_2+\frac{\left(\delta-\frac{(1-\gamma)ht^2}{(1-t^2)+t^2}\right)^2}{2\left(\frac{2(1-t)t\sigma}{\sqrt{2((1-t)^2+t^2)}}\right)^2}\right)}}{2\sqrt{2}\pi t\sigma}d\delta \\
&= \alpha_1 - 2\frac{1}{\sqrt{2}(1-t)\sigma}\exp(-\alpha_2)\int_0^\infty \frac{e^{-\frac{\left(\delta-\frac{(1-\gamma)ht^2}{(1-t^2)+t^2}\right)^2}{2\left(\frac{2(1-t)t\sigma}{\sqrt{2((1-t)^2+t^2)}}\right)^2}}}{2\sqrt{2}\pi t\sigma}d\delta \\
&= \alpha_1 \times \left(1 - 2\exp(-\alpha_2) \times \Phi(\alpha_3)\right) \qquad \text{(A.12)}
\end{aligned}
$$

A similar approach yields the solution for $E\left[\frac{dB(\delta)}{d\delta}|\delta < 0, t\right]$. □

The following two corollaries investigate, first, the expected effect of proximity over the entire domain of δ and, second, the differential expected effects of proximity for the positive versus negative values of δ.

Corollary A.3. *On average, the marginal effect of proximity on the benefit of peer-harm is positive, that is:* $E\left[\frac{dB(\delta)}{d\delta}|\delta < 0, t\right] - E\left[\frac{dB(\delta)}{d\delta}|\delta > 0, t\right] > 0$.

Proof. Taking the difference of equations (A.11) and (A.12) yields $2\alpha_1 \exp(-\alpha_2) \times (2\Phi(\alpha_3) - 1) > 0$, which holds because $\alpha_1 > 0$, and $\alpha_3 > 0$. □

Corollary A.4. *On average, the marginal effect of proximity on the benefit of peer-harm is greater when the peer firm has a higher TSR than when the peer firm has a lower TSR, that is:*
$E\left[\frac{dB(\delta)}{d\delta}|\delta < 0, t\right] > -E\left[\frac{dB(\delta)}{d\delta}|\delta > 0, t\right]$.

Proof. We can rearrange the condition to $E\left[\frac{dB(\delta)}{d\delta}|\delta < 0, t\right] + E\left[\frac{dB(\delta)}{d\delta}|\delta > 0, t\right] > 0$. Taking the sum of equations (A.11) and (A.12) yields $2\alpha_1 \times \left(1 - \exp(-\alpha_2)\right) > 0$, which holds because $\alpha_1 > 0$ and $\alpha_2 > 0$. □

Combined, Corollaries A.3 and A.4 yield the following two testable implications. First, period-to-date TSR proximity should be positively associated with the likelihood of being harmed, on average. Second, this effect should be more pronounced when the peer's period-to-date TSR is below that of the focal firm.

A.2 EXTENSION TO MULTIPLE PEERS

In this section, we extend our baseline model to a setting where the focal firm $i = 0$ has N peers, $i = 1, \ldots, N$. As before, each $x^i_\tau(t)$ naturally follows an independent Brownian process with drift μ and volatility σ^2 such that:

$$x^i_\tau(t) \sim N\left[\mu t, t^2\sigma^2\right]. \tag{A.13}$$

At the end of each period, the manager of the focal firm receives a bonus, b, for each peer that the focal firm outperforms. Being ranked first yields a bonus of Nb; second yields a bonus of $(N-1)b$; and so forth. Being ranked last, yields a bonus of 0.

The manager of the focal firm has negative private information about each peer that, if disclosed, would immediately lower that peer's TSR by an amount of h, while lowering the focal firm's TSR by γh. As before, this private information would naturally come to light in the subsequent period, so disclosing *accelerates* the price impact from period 2 into period 1. At time t, the manager has the option to harm any set of peers.

Before making this disclosure choice, the manager observes the period-to-date standing for all firms: $x^i_1(t)$. From these period-to-date TSRs, we can characterize the relative standings in terms of the following set of period-to-date differentials:

$$\delta_i \equiv x^i_1(t) - x^0_1(t), \quad \forall i \in \{1, \ldots, N\}. \tag{A.14}$$

We now consider the trade-offs involved in harming peer j, as a function of δ_j and all δ_k's, with $k \neq j$, and arrive at the following proposition:

Proposition A.4. *The cost/benefit trade-off related to the choice to harm peer j is given by:*

$$B(\delta_j) - C(\delta_{-j}) \tag{A.15}$$

with

$$B(\delta_j) = \Phi\left(\frac{-\delta_j + (1-\gamma)h}{\sqrt{2}(1-t)\sigma}\right) - \Phi\left(\frac{-\delta_j}{\sqrt{2}(1-t)\sigma}\right), \tag{A.16}$$

$$C(\delta_{-j}) = \frac{c}{b} + \Phi(0) - \Phi\left(\frac{-h}{\sqrt{2}\sigma}\right) + \sum_{k\neq j}\left(\Phi\left(\frac{-\delta_k}{\sqrt{2}(1-t)\sigma}\right) - \Phi\left(\frac{-\delta_k - \gamma h}{\sqrt{2}(1-t)\sigma}\right)\right). \tag{A.17}$$

Proof. Using the same solution approach as in the single-peer case, one can easily establish that when a focal firm harms peer j, the probability of outperforming peer j in period 1 increases from: $\Phi\left(\frac{-\delta_j}{\sqrt{2}(1-t)\sigma}\right)$ to

$\Phi\left(\frac{-\delta_j+(1-\gamma)h}{\sqrt{2}(1-t)\sigma}\right)$, and (for $\gamma > 0$), the probability of outperforming $k \neq j$ in period 1 decreases from $\Phi\left(\frac{-\delta_k}{\sqrt{2}(1-t)\sigma}\right)$ to $\Phi\left(\frac{-\delta_k-\gamma h}{\sqrt{2}(1-t)\sigma}\right)$. Moreover, the probability of outperforming peer j in period 2 decreases from $\Phi(0) = \frac{1}{2}$ to $\Phi\left(\frac{-h}{\sqrt{2}\sigma}\right)$, while the probabilities of outperforming the other peers $k \neq j$ in period 2 are unaffected. Given the assumed linearity of the compensation contract, these changes in probabilities are all equally weighted in the manager's optimization. □

As can be seen, $B(\delta_j)$ is equivalent to $B(\cdot)$ from the single-peer case. Moreover, $C(\cdot)$ remains independent of δ_j, as in the single-peer case. Thus, Proposition A.3 and Corollaries A.2 through A.4 continue to hold in the multi-peer setting. That is, all of the testable implications derived in section A.1 apply to the multi-peer setting.

In the multi-peer setting, $C(\cdot)$ differs from that of the single-peer setting in that it includes the additional term $\sum_{k\neq j}\left(\Phi\left(\frac{-\delta_k}{\sqrt{2}(1-t)\sigma}\right) - \Phi\left(\frac{-\delta_k-\gamma h}{\sqrt{2}(1-t)\sigma}\right)\right)$. The term reflects an additional cost of disclosure in the multi-peer setting. When $\gamma > 0$, harming peer j reduces the focal firm's probability of outperforming each peer $k \neq j$.

Corollary A.5. *On average, the cost of harming peer j increases in the proximity of peer k to the focal firm,* $E\left[\frac{dC(\delta_k)}{d\delta_k}|\delta_k < 0, t\right] - E\left[\frac{dC(\delta_k)}{d\delta_k}|\delta_k > 0, t\right] > 0$.

Proof. Following an analogous approach as for Corollary A.3

$$E\left[\frac{dC(\delta_k)}{d\delta_k}|\delta_k < 0, t\right] = -\alpha_1 \times \left(1 - 2\exp(-\alpha_4)\Phi(\alpha_5)\right) \text{ and} \quad \text{(A.18)}$$

$$E\left[\frac{dC(\delta_k)}{d\delta_k}|\delta_k > 0, t\right] - \alpha_1 \times \left(1 - 2\exp(-\alpha_4)(1 - \Phi(\alpha_5))\right), \quad \text{(A.19)}$$

with $\alpha_1 = \frac{1}{2\sigma\sqrt{\pi\left((1-t)^2+t^2\right)}} > 0$, $\alpha_4 = \frac{1}{4\sigma^2}\frac{\gamma^2 h^2}{(1-t)^2+t^2} > 0$, and $\alpha_5 = \frac{t}{1-t}\frac{\gamma h}{\sigma\sqrt{2((1-t)^2+t^2)}} > 0$.

Taking the difference of equations (A.18) and (A.19) yields $2\alpha_1\exp(-\alpha_4) \times (2\Phi(\alpha_5) - 1) > 0$, which holds because $\alpha_1 > 0$, $\alpha_4 > 0$, and $\alpha_5 > 0$. □

The above Corollary provides an additional testable implication. The cost of sabotaging a peer is greater when other peers have similar period-to-date TSR to the focal firm. That is, firms should be less likely to harm a peer when there are more other peers whose period-to-date TSRs are in close proximity to the focal firm.

Proposition A.4 does not fully characterize the focal firm's optimal disclosure policy. It characterizes the trade-off associated with each act of sabotage. Holding fixed the sabotage choice pertaining to peer j, the firm should harm peer i iff $B(\delta_i) - C(\delta_j) > 0$. In the case with N peers, the full optimization problem provides 2^N options to the manager. Characterizing the optimal disclosure policy cannot be done in closed form. The costs and benefits of each peer-harming disclosure are interdependent; harming one peer changes all of the δ_i's, and therefore affects the cost/benefit trade-off for every other possible act of peer harm. However, the optimal disclosure strategy will simultaneously satisfy all of the propositions and corollaries derived above.

A.3 FURTHER DISCUSSION OF MODEL ASSUMPTIONS

In this subsection, we discuss some of our modeling assumptions and offer some intuition as to how our predictions might be affected by potential modeling extensions. In particular we consider: (1) Bayesian inference from nondisclosure; (2) reactive disclosures from harmed peers; (3) endogenous disclosure timing; and (4) nonlinear payout functions. We provide additional discussion and some tabulated empirical tests in section OA3 of the online appendix.

A.3.1 Inference from Nondisclosure. As noted above, we do not derive a rational expectations equilibrium in which investors anticipate the manager's disclosure choices and draw inferences from the manager's silence. For simplicity, we assume that investors are unaware that the manager has any damaging private information, and thus do not anticipate any such disclosure, nor infer anything from the managers' silence. Incorporating Bayesian updating from nondisclosure in the context of a rank-order tournament would likely be intractable. However, in principle one could imagine extending our model to derive a rational expectations equilibrium in which investors are aware of the possibility of peer-harming disclosures and draw inferences from silence, as well as from disclosures. Although such an extension would surely affect the manager's disclosure policy, given sufficient information frictions to prevent unraveling, we expect that managers' targeting incentives would be qualitatively similar to those derived in sections A.1 and A.2. The reasoning is the following.

Suppose, before considering the inference from nondisclosure, peer j is a prime target for sabotage (i.e., harming the peer is likely to be marginal in the final rankings). In such a case, rational investors (who understand that the manager has strong incentives to sabotage peer j) would infer good news regarding the peer from silence (e.g., "the manager must not have anything sufficiently damaging to say about peer j"). This inference from nondisclosure bolsters the manager's incentive to harm peer j. In contrast, suppose before considering the inference from nondisclosure, peer j' is an poor target for sabotage (i.e., harming the peer is unlikely to be marginal in the final rankings). In such a case, rational investors (who understand that the manager has no incentives to sabotage peer j') would infer nothing from silence; silence would be the anticipated behavior, irrespective of the managers' private information. Such an inference from nondisclosure has no impact on the manager's incentive to harm peer j'. In sum, Bayesian inference from nondisclosure would likely serve as a reinforcing mechanism;

it strengthens the manager's incentive to harm a peer that they already benefit from harming, while having no effect on the manager's incentives to harm a peer that they otherwise would not benefit from harming, keeping our predictions qualitatively intact.

A.3.2 Reactive Peer Disclosures. We model the manager of the focal firm as an active decision maker and treat the peers as passive benchmarks, that have no goals and take no actions. In practice, of course, the peers are decision makers in their own rights. It is quite plausible that a manager's choices would affect that peers' subsequent disclosure behavior, potentially influencing the managers' targeting behavior, ex ante.

One possibility of note is that of retaliation, whereby harmed peers might respond in kind. If so, focal firm managers would surely be more wary of engaging in sabotage. However, we do not expect this to be particularly common. Absent behavioral biases such as spite and/or a preference for retribution, in most cases, it is not clear why having been harmed would change a peers' disclosure practices, vis-à-vis harming the focal firm. If there was no incentive to harm the firm initially, it is not clear why having been harmed would affect this trade-off. One notable exception to this logic is the case of mutual (i.e., two-way) peer relationships, whereby each firm uses the other as an RPE price-peer. In mutual peer relationships, both firms care about their TSR standing relative to the other. Peer-harming disclosures affect these relative standings, and thus can influence the other's cost/benefit trade-off, vis-à-vis issuing a peer-harming disclosure of their own.

We do not model this bi-directional game, but one implication seems intuitively clear: the tendancy to target better-performing peers would likely be muted in the case of mutual peer relationships. The reasoning is the following. When a focal firm sabotages a peer slightly above them, the focal firm becomes the peer slightly above the harmed peer—they have put themselves right in the peer's cross-hairs as a tempting target. As such, the benefits of aiming up are likely to be reduced. Moreover, the benefits of aiming down increase, because the peer just below has the focal firm in its sights as a tempting target—harming a peer just below can give enough distance that the focal firm is no longer in the most temping region, at the greatest risk of harm. As such, the benefits of targeting better-performing peers decreases while the benefits of targeting worse-performing peers increases, thereby reducing managers' tendencies to target better-performing peers.[38]

Although we do not expect retaliation to be a common reaction, it is highly plausible that harmed peers might react in other ways, such as offering their own clarifying/corrective disclosures, and/or otherwise engaging in "damage control". Even if these damage control reactions do not impose costs on the focal firms (i.e., they are entirely nonretaliatory), to the extent that they affect the peers' TSR process, they can still alter the tournament rankings, and may thus be decision-relevant to the focal firm's

[38] See section OA3.1 of the online appendix for additional discussion.

manager, ex ante. Such a possibility is straightforward to incorporate into our existing analysis. If the manager is able to harm the peer by an amount of h, but the peer is quickly able to reverse some fraction, $\lambda \in [0, 1)$, of this harm through damage control efforts, then the manager should simply make their targeting decision as if the harm they can inflict is equal to the net harm done: $(1 - \lambda)h$. Such an extension leaves our predictions qualitatively unchanged.

A.3.3 Endogenous Disclosure Timing. In our analysis, we assume that at some exogenous time t the manager has the option to disclose or withhold some peer-harming information; the manager cannot later choose to disclose information that was withheld at time t. However, one could imagine extending our model to allow the manager to choose whether or not to disclose their information at various different points in time (perhaps even allowing continuous time control over the disclosure choice). Under such an extension, the choice to withhold would retain some option value: the manager can later choose to disclose information that was previously withheld, but cannot later choose to withhold information that was previously disclosed. As such, allowing the option to wait, managers would likely choose to exercise it, delaying their disclosures in order to make the most informed peer-harming choice.

In the extreme, allowing continuous time control and excluding any cost of waiting, managers would optimally wait until the last possible moment, so as to make fully informed peer-harming choices. However, in practice there are likely to be disciplinary forces preventing this edge case (e.g., their peer-harming disclosures are bundled with other information, such as earnings guidance, for which there is a demand for timely disclosure). Given that firms are likely to delay their peer-harming disclosures until later in the performance period, we expect that our model's predictions regarding optimal peer targeting would likely get stronger toward the end of the period.[39]

A.3.4 Convex Reward Functions. In our multi-peer framework, we assume that the bonus from beating each peer is a constant, such that the total bonus payout is linearly proportional to the number of peers beaten. In practice, many RPE grants are convex whereby the benefits from ascending by one rank are greater at the top of the peer group than at the bottom. For example, there might be no award at all for being below the median (whether just barely below, or all the way at the bottom), and the award might jump substantially going from third place to second, and by even more going from second place to first. Given a convex pay plan, our predictions regarding peer-harm would likely be more descriptive among

[39] See section OA3.2 of the online appendix for further discussion. Note that this is not a claim about comparative statics on t from our model. The economic force behind this intuition is the option value from waiting to become more informed—a force that is absent from our model.

firms in the top portion of the rankings than in the bottom portion of the rankings.[40]

APPENDIX B: EXAMPLE RTSR GRANT

The following text is an excerpt from the DEF 14A filing of Chevron Corporation ("Chevron") (2020, p. 45), where the firm describes its Long-Term Incentive Plan ("LTIP"), of which 50% is dependent on the rTSR performance/grant.[41]

B.1 LTIP TARGET COMPENSATION

The table below summarizes the 2019 target compensation opportunities the Board and the Management Compensation Committee ("MCC") approved for the Named Executive Officers ("NEOs").

Name	LTIP Target Value
Michael K. Wirth (CEO)	$15,000,000
Pierre R. Breber (CFO)	$3,963,120
James W. Johnson (COO)	$5,148,000
Joseph C. Geagea (COO)	$3,963,120
Mark A. Nelson (COO)	$3,963,120

The LTIP program comprises the following three equity vehicles:

- Performance shares (50%), which are completely dependent on the rTSR grant (see below)
- Restricted Stock Units ("RSUs") (25%)
- Stock Options (25%)

B.2 PERFORMANCE SHARES (50%) ("RTSR GRANT")

For this grant, payouts are dependent on Chevron's TSR over a three-year period, compared with our LTIP Performance Share Peer Group TSR. For the 2019 grant, the peer group is: ExxonMobil, BP, Shell, Total, and the S&P 500 Total Return Index.

Relative TSR ranking	1	2	3	4	5	6
2019 grant payout as a % of target	200%	160%	120%	80%	40%	0%

The proxy statement further details that: (1) performance shares accrue dividend equivalents that are reinvested as additional shares, to be paid

[40] See section OA3.3 of the online appendix for further discussion.
[41] See https://www.sec.gov/Archives/edgar/data/0000093410/000119312520100407/d838093ddef14a.htm

at the end of the performance period and are subject to the same three-year cliff vesting schedule and performance modifier; (2) the MCC can exercise negative discretion to reduce the payout; (3) actual number of shares granted is determined by dividing the proportionate value of the NEO's LTIP award by Chevron's closing common stock price on the grant date; and (4) payment is made in cash. Of their choice to use rTSR to evaluate and reward the CEO's performance, Chevron notes:

> "The MCC continues to believe that TSR is the best overall pay-for-performance measure to align our CEO's and other NEOs' performance with stockholder interests. TSR is the standard metric for stockholders to use in measuring the Company's performance because it easily allows for meaningful comparisons of our performance relative to other companies within our same industry, and also allows for easy comparison with our stockholders' other investment alternatives. It is objectively determined by third-party market participants independent of the Company's judgment. The MCC believes that Company performance on other measures'operational and financial, over the short-term and long-term'is ultimately reflected in TSR results."

APPENDIX C: PEER SELECTION ALGORITHM

The Bloomfield, Guay, and Timmermans ("BGT") peer selection algorithm works as follows:

(1) For each firm-year observation, define a universe of potential peers from which to select RPE peers. Following BGT, we use two-digit SIC.
(2) Calculate the pair-wise correlation in daily stock returns between the focal firm and each potential peer over the preceding three years.
(3) Rank potential peers based on these pair-wise correlations and then form equal-weighted portfolios consisting of the n potential peers with the highest correlations to the focal firm, for $n = 1, 2, 3 \ldots$ up to 50 or the number of potential peers in the universe, whichever is smaller.
(4) Calculate the correlation between each n-peer portfolio and the focal firm, over the preceding three years, and determine the n, n^*, for which this correlation is maximized. Consider this n^*-peer portfolio to be the "artificial" peer group, representing the plausible counterfactual peer group the firm might have constructed, for the purposes of risk-sharing.

For a more thorough discussion of the algorithm, and the properties of these artificial peer groups, see Bloomfield, Guay, and Timmermans [2022].

APPENDIX D: EXAMPLES OF POSSIBLY PEER-HARMING DISCLOSURES

In this appendix, we provide several examples of potentially peer-harming disclosures from earnings calls and/or management forecasts. Many, but not all, of these examples involve explicit mentions of price-peers. These examples appear to be plausible candidates for the type of peer-harming behavior we predict: unfavorable statements about price-peers that coincide with substantial price declines for those price-peers. However, we caveat that we cannot definitively assess the intent behind, nor the valuation effect of, the particular quotes we identify.

One plausible approach to peer-harm could be to make unfavorable remarks about peers' product market offerings. One example of such a remark can be found in Wendy's 2011 earnings call, in which they announced: "In a national taste test this year, 56% of consumers chose Wendy's fries over McDonald's." Coincident with this disclosure, McDonald's (one of Wendy's price-peers) underperformed Wendy's other price-peers by 3 percentage points.

An alternative approach might be to call attention to peers' potential operational difficulties. For example, in some earning guidance, Goodrich offered the following caveat: "Important factors that could cause actual results to differ from expected performance include… delays or cancellations associated with the Boeing 787 Dreamliner." Boeing was one of Goodrich's RPE price-peers at the time, and had been experiencing operational difficulties that were not yet widely known. Coincident with this disclosure, Boeing underperformed the rest of the peer group by ~4 percentage points.

We also observe instances of firms publicly announcing successful poaching of labor talent from their price-peers. For example, in a 2015 earnings call, Target announced: "[W]e've convinced Kathy Smith to join our leadership team. She served as CFO at…Walmart." Walmart was one of Target's RPE price-peers at the time, and coincident with this announcement, underperformed the rest of Target's RPE peer group by roughly 50 basis points.

We also observe examples of plausibly peer-harming disclosures that do not involve explicit peer mentions. For example, during an earnings call in 2011, Hershey's announced their intent to offer a "broader portfolio" of products, by adding "nutritional beverages that might focus on dairy and proteins". At the time, Dean Foods—the largest dairy firm in the United States—was one of Hershey's RPE price-peers. Hershey's announcement was effectively a public threat of potential product market encroachment, and coincident with the announcement, Dean Food's stock price declined by roughly 4 percentage points.

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