Relative Performance Evaluation and Sabotage: Evidence from Voluntary Earnings Guidance

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FIRST DRAFT

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Abstract

Many firms use relative stock performance to evaluate and incentivize their CEOs. We provide evidence that such firms routinely disclose information that harms peers' stock prices. Consistent with deliberate and strategic sabotage, peer-harming disclosures appear targeted at the peers whose stock price performances are more likely to have an impact on the CEO's compensation, especially towards the end of a fiscal year. This strategy also carries a cost for the disclosing firms; these disclosures appear to be more informative about the peers, and less informative about the disclosing firms, having larger effects on the peers' trading volumes and smaller effects on the disclosing firms' trading volumes. That is, firms seem to sacrifice some of the capital market benefits typically associated with voluntary disclosure in order to boost their relative standing amongst their RPE peers through sabotage.

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1 Introduction

In recent years, relative performance evaluation (hereafter "RPE") has become a common and important feature in executive compensation plans (e.g., Gong et al. 2011; Bettis et al., 2014; Ma et al., 2019). While RPE can facilitate efficient risk-sharing by filtering out common shocks (e.g., Lazear and Rosen, 1981; Holmström, 1982; Nalebuff and Stiglitz, 1983; Prendergast, 1999), it also carries a potentially unintended consequence: RPE gives agents an incentive to engage in costly sabotage. By damaging the performance of the benchmark against which they are compared, agents can improve their relative standing, even at significant cost to their absolute performance (e.g., Lazear, 1989; Gibbons and Murphy, 1990; Chowdhury and Gürtler, 2015; Bloomfield et al., 2020). We provide evidence that stock priced based RPE (i.e., relative total shareholder return or "rTSR") encourages firms to sabotage peers' stock prices with voluntary disclosures.

Existing research documents that relative performance drives sabotage behavior in a variety of contexts, including sports (e.g., Del Corral, Prieto-Rodriguez, and Simmons, 2010), corporate promotions (e.g., Chen, 2003; Harbring, Irlenbusch, Kräkel, and Selten, 2007) and higher education (e.g., Royal and Guskey, 2014). However, to our knowledge, there is no evidence that establishes that RPE in CEO pay plans drives firms to engage in inter-firm sabotage. Prior literature describes the possibility of RPE-induced inter-firm sabotage as "unlikely" because "CEOs tend to have limited interaction with CEOs in rival firms" (Gibbons and Murphy, 1990, page 31). We depart from this view, and consider the possibility that firms can sabotage each other. For example, in concentrated product markets, one firm's strategic choices can have a substantial impact on rivals' profitability. In such cases, firms would be able to improve their relative profitability by unilaterally implementing more aggressive product market strategies, such as lowering product prices, increasing production volumes, and/or running adversarial market-stealing advertising campaigns (e.g., Aggarwal and Samwick, 1999).

When relative stock performance matters (such as in the case of compensation based on rTSR), firms have another sabotage tool in their arsenal: strategic peer-harming disclosures. By disclosing negative information about their peers, firms can hurt their peers' stock values to improve their own relative standing. This strategy has a number of appealing properties. First and foremost, it can be deployed separately from any product market sabotage strategies. Thus, firms can use this strategy along with, or instead of, more costly strategies such as cutting prices on product market offerings. For firms that pay their CEO based on non-TSR RPE (e.g., relative profits), disclosure strategies are unlikely to be as effective unless the disclosures substantively effect peers' operations (e.g., by creating issues with customers or suppliers). In what follows, we refer to a firm's RPE peers as "price-peers" ("profit-peers") if they are used as peers for a price-based (profit-based) RPE grant. Our primary prediction is the following: focal firms systematically sabotage their price-peers through their voluntary disclosures, resulting in underperformance by price-peers on focal firms' voluntary disclosure dates.

To test our prediction, we use several variants on a difference-in-differences design in which we compare price-peers' stock returns to profit-peers' stock returns around focal firms' disclosure dates. In our baseline specifications, price-peers form the 'treatment group'; profit-peers form the 'control group'; and focal firms' voluntary disclosures are the 'treatment.' While a peer's assignment as a price-peer or profit-peer is endogenous, profit-peers form a natural control group for our study. Both price-peers and profit-peers are self-selected by the focal firms due to their shared exposure to common sources of performance uncertainty.

We provide evidence that rTSR in CEO pay packages pushes firms to enact voluntary disclosure policies designed to harm their price-peers' stock performance. Relative to profit-peers, a firm's price-peers' stock performance is significantly lower on the firm's voluntary disclosure days. The average extent of underperformance is about 20 to 30 basis points per disclosure day. However, as noted above, we recognize the endogenous nature of a peer's status as a price-peer or profit-peer and acknowledge that the estimated treatment effect need not reflect an unbiased estimate of the average extent of sabotage. We take a number of steps to rule out plausible alternative explanations for our findings, which we describe below.

First, we use a variety of cross-sectional fixed effect structures, to estimate treatment effects from a multitude of different sources of identifying variation. We document very similar results with SIC + peer, firm + peer and interacted firm-peer fixed effects. In our tightest specifications (with interacted peer-firm fixed effects), the treatment effect is identified from within-firm-peer pair variation in each peer's status as a price-peer versus profit-peer. As such, this design rules out any alternative explanation related to time-invariant firm, peer or firm-peer pair characteristics.

Second, we augment our treatment and control samples in several ways: (1) we use the artificial price-peer selection algorithm developed by Bloomfield, Guay and Timmermans (2021) to construct two alternative treatment groups and a placebo treatment group; and (2) we replicate our analyses for price- and profit-peers which were in use as RPE peers at some point over our sample window, but not currently, and document null results.

Third, we leverage institutional details related to the RPE plans commonly found in practice to exploit plausibly exogenous variation in a price-peer's likelihood of being targeted for sabotage. The vast majority of price-based RPE grants yield payouts based on percentile ranks, rather than performance relative to a peer group average. As such, a focal firm stands to benefit far more by sabotaging peers whose performance is more similar to their own (and therefore more likely to be marginal in determining the final percentile ranking), and far less by sabotaging peers whose performance is more dissimilar from their own. With this intuition in mind, we examine whether year-to-date performance proximity can explain price-peers' returns around focal firms' voluntary disclosure dates.

Consistent with deliberate, strategic and targeted sabotage, the underperformance effect is most prevalent among those price-peers whose year-to-date performance is most similar to the focal firm's, especially at the end of the focal firm's fiscal year. In these cases, peer-harming disclosures are likely to be most successful at increasing the CEO's compensation. Of note, this design identifies an effect based on plausibly exogenous variation, and is therefore less susceptible to bias. Rather than coding peers as treatment or control based on their status as price- or profit-peers (an endogenous firm choice), we measure treatment continuously using year-to-date performance proximity to the focal firm. Year-to-date performance proximity is not a choice variable, nor easily predictable *ex ante*, making it unlikely that our inferences are confounded by some correlated omitted factor.

In addition to establishing the average prevalence of a disclosure-based sabotage strategy, we further examine cross-sectional variation in its employ. We find that this strategy is used broadly. Large and small firms appear to engage in these tactics with similar frequency, and they appear to target peers larger than themselves, and smaller than themselves in roughly equal proportion. Moreover, the strategy does not appear to be constrained by industry boundaries. Firms appear to sabotage price-peers within their own primary 1-digit SIC industry, as well as price-peers that reside outside of their own primary 1-digit SIC industry.

Lastly, we explore the capital market consequences such a strategy may carry for the disclosing firms. While this strategy is likely less costly to the firm than other approaches to sabotage (e.g., aggressive price cutting), it is not costless. Typically firms' voluntary disclosure policies are designed with the goal of maximizing their own share price (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., Balakrishnan et al., 2014). If a firm deviates from a value-maximizing policy in order to improve relative performance (i.e., engage in sabotage), it will likely come at the expense of these capital market benefits. Consistent with this prediction, we document that rTSR-using focal firms suffer from lower trading volume on their voluntary disclosure dates. This could be the case when, instead of discussing their own performance, firms highlight information that is more relevant to the evaluation of their peers. Consistent with interpretation, we find that price-peers receiving a greater-than-average trading volume boost on focal firms' voluntary disclosure dates. Collectively, our evidence suggests that firms sacrifice some of the capital market benefits of disclosure in exchange for outperformance relative to price-peers.

Our study contributes to several strands of literature. First, our study contributes to the literature on executive incentives, and in particular the economic consequences of using RPE. We provide the first evidence on RPE-induced sabotage in an inter-firm corporate context. Prior evidence on RPE-induced sabotage comes predominantly from intra-firm contexts (e.g., competition for promotions) or non-corporate contexts (e.g., course grades and athletic tournaments). Prior studies provide suggestive evidence that firms avoid using RPE due to concerns that it may induce cost sabotage (e.g., Aggarwal and Samwick, 1999; Bloomfield et al., 2020; Bloomfield et al., 2021). Finally, Martin and Timmermans (2020) suggests that earnings-based RPE in CEO contracts reduces firms' willingness to provide earnings forecasts because they can provide a target for other firms to beat.

Further, our study connects to the emerging literature on disclosure spillovers. Extant literature documents information spillovers from disclosures (e.g., Schroff et al., 2017 and Breuer et al., 2018). More recent work further suggests that disclosing firms *internalize* these spillovers when making their disclosure choices. For example, in the common-ownership setting, Park et al., (2019) provide evidence to suggest that firms use voluntary disclosures to improve the liquidity of co-owned peers. Relatedly, for the setting of mergers and acquisitions, Kim et al., (2020) provides evidence to suggest that firms use disclosures to depress the stock prices of acquisition targets. Specifically, Kim et al., (2020) find that during acquisition negotiations, acquiring firms tend to disclose more good (bad) news about themselves when the target firms stock returns are more negatively (positively) correlated with the acquiring firm's. This pattern suggests that acquiring firms strategically reveal information about themselves, if they expect the information will have negative spillover consequences for the target firm's price. However, their study does not test whether target firms' stock prices actually *respond* to firm disclosures in the posited manner. Cao, Fang, and Lei (2020) provides evidence that firms disclose negative information on social media about their product-market rivals. Firms that provide such negative peer disclosure experience both positive short-term abnormal returns and product-market success in the year following the disclosure. Our study contributes to this literature as the first to show targeted disclosure-based sabotage strategies.

The remainder of this manuscript is organized as follows. In Section 2, we develop and state our testable predictions; in Section 3, we describe our primary data sources, sample selection criteria and variable construction procedures; in Section 4, we detail our empirical methodology and discuss our findings; and in Section 5, we conclude.

2 Hypothesis Development

Firms often provide their executives with RPE-based compensation awards to filter out systematic risk, to mitigate "pay for luck", and to facilitate efficient risk-sharing between shareholders and executives (e.g., Holmström, 1982; Gong et al., 2011; Ma, Shin, and Wang, 2019). While RPE can be an effective governance tool, it also encourages 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. This can be particularly harmful within firms when, e.g., team-members sabotage each other. In the context of corporate disclosure, this sabotage may manifest as firms issuing voluntary disclosures intended to harm peers' performance.

For an executive who has compensation tied to relative accounting profits, effective sabotage disclosures have to reduce the peers' reported costs or revenues for the reporting period. In the case of price RPE, however, the disclosures only have to affect investors' beliefs about peers' future cash flows. As a result, sabotage via voluntary disclosure is likely most effective when the CEO is evaluated on the basis of relative stock performance, as opposed to relative profit performance. This leads to our first prediction: on average, price-peers, relative to profit-peers, underperform on focal firms' voluntary disclosure dates.

Almost all rTSR grants link performance payouts to relative rankings, rather than performance relative to the mean of peer performance. Because sabotaging the entire peer group, without harming the own stock price is likely very difficult, we predict that firms will take a more targeted approach. Specifically, there is substantial heterogeneity in the strategic upside associated with sabotaging a peer. Consider a peer whose performance is expected to be far better or worse than the focal firm—a marginal change to this peer's price likely has zero impact on the focal firm's final ranking, and thus zero impact on compensation awarded to the CEO of the focal firm. Sabotaging such a firm confers little-to-no benefit to the focal firms' executives. In contrast, consider a peer whose performance is expected to be very similar to the focal firm—a marginal change to this peer's performance could very well be a deciding factor in the focal firm's final ranking, and thus could have a significant impact on compensation awarded to the CEO of the focal firm. Sabotaging such a firm can confer substantial benefit to the focal firms' executives. In line with this intuition, we predict that (i) a firm will only target a small number of peers; (ii) price-peers' underperformance is greater for peers whose year-to-date performance is more similar to the focal firms'; and (iii) this holds especially towards the end of the focal firm's fiscal year.

Sabotage is likely a costly action for the disclosing firm. In particular, if a typical firm's voluntary disclosure objective is to mitigate information asymmetry (thereby improving liquidity and cost of equity capital), then sabotage represents a competing goal. It is unlikely that a voluntary disclosure policy, optimized to minimize information asymmetry and maximize liquidity, could be adjusted to incorporate strategic sabotage without any sacrifice in the informational quality of the disclosures. In service of sabotage, focal firms will 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-users' voluntary disclosures will be more positively associated with their peers' trading volume and less positively associated with their own trading volume.

3 Data, Sample Selection and Variable Construction

In this section, we describe the data used in our data, the sample selection criteria, and variable construction details. 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==“Rel” or “AbsRel” and relativebenchmark==“Peer Group”).

Using Incentive Lab data on RPE peer groups, we construct a network dataset for all focal firms in our sample covering the period of 2006 to 2016. The unit of observation is at the firm-peer-day level. Our sample includes 7,769,934 firm-peer-day observations coming from 488 unique focal firms, with 2,296 unique peers forming 9,812 unique firm-peer pairs.

3.2 Variable Construction

3.2.1 RPE Type

We measure RPE type using grant-level compensation data from the Incentive Lab dataset. We code each RPE grant as price-based if metrictype==“Stock Price” and as profit-based otherwise. We then match each grant to its focal firm and selected peers, and for each firm-peer pair, we construct the the variable, $rTSR$ such that:

$$rTSR_{i,j,t} = \begin{cases} 0, & \text{if firm } i \text{ uses peer } j \text{ as a profit-peer, but not a price-peer, in fiscal year } t, \\ 1, & \text{if firm } i \text{ uses peer } j \text{ as a price-peer, but not a profit-peer, in fiscal year } t, \\ \frac{1}{2}, & \text{if firm } i \text{ uses peer } j \text{ as both a price-peer and a profit-peer, in fiscal year } t. \end{cases} \tag{1}$$

In untabulated tests we assess the sensitivity of our results to this coding of the $rTSR$ measure and in particular our treatment of peers that are used as both price- and profit-peers. We find that our results are not particularly sensitive to our treatment of these observations. These observations represent a small minority of our sample (<10%), and we can exclude them from our sample, yielding a binary $rTSR$ variable, without materially affecting our inferences. We further construct the variable *Any* $rTSR$ as a firm-date indicator variable equal to one for focal firms with at least one price-peer at that date.

3.2.2 Disclosure Date

We measure disclosure dates using data on voluntary earnings guidance from I/B/E/S. We construct the indicator variable *Disc. Date* which equals one 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.

3.2.3 Stock Performance

We measure stock performance using daily stock market returns data from CRSP. We construct two primary measures: *Own Return*, equal to the focal firm's daily return; and *Peer Return*, equal to the peer's daily return.

We construct several variants on the *Peer Return* variable to reflect the distribution of peer returns at the focal firm-date-level: $Peer\ Ret_{min}$, $Peer\ Ret_{10}$, $Peer\ Ret_{med}$, $Peer\ Ret_{90}$ and $Peer\ Ret_{max}$ which reflect the minimum, 10th percentile, median, 90th percentile and maximum peer returns for each firm-date observation.

All return variables are measured in percentage points, and winsorized at 1% and 99%.

3.2.4 Year-to-Date Performance Proximity

For each trading day in our sample, we measure focal firms' and 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 then calculate the year-to-date TSR percentile ranks for the firm and all of the price-peers based on year-to-date TSR performance.

Based on focal firms' and peers' year-to-date performance, we construct the variable *Proximity* equal to one 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. A *Promixity* of zero reflects that the focal firm and peer are at opposite ends of the year-to-date TSR performance ranking; a *Proximity* of one reflects that the firm and peer are exactly tied. Intermediate values of *Proximity* reflect intermediate degrees of year-to-date TSR performance similarity.

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: $OwnVolume$, 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 its natural logarithm in our analyses.

4 Empirical Analysis

In this section, we describe our empirical approach and present our findings.

4.1 Empirical Strategy

Our primarily empirical strategy is to compare price-peers' stock returns to profit-peers' stock returns on focal firms' voluntary disclosure dates. Our baseline empirical specification is a difference-in-differences design: price-peers form our treatment group; relative profit-peers form our control group; and focal firms' voluntary disclosures are the treatment. To identify disclosure-based sabotage 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.

However, we acknowledge that executive incentives are highly endogenous. Firms choose whether or not to use price-based or profit-based RPE (or both, or neither). Conditional on their choice of RPE, firms further choose which peers to include in the peer group against which relative performance is evaluated. As such, our treatment and control groups are not randomly assigned, thereby making it difficult to ascertain the causal effects of executive incentives on outcomes variables of interest. To best identify the effects of sabotage, we rely on two important research design strategies. First, within our difference-in-differences design, we use a variety of cross-sectional fixed effect structures, to estimate the coefficients of interest from multiple sources of identifying variation.[1] Second, we use a variety of alternative treatment/control splits, to better triangulate the effects. In what follows, we first discuss the variation in fixed effect structures and then discuss the variation in treatment/control splits.

In our loosest specifications, we include industry (4-digit SIC) and peer fixed effects. This specification allows for the coefficients of interest to be estimated, at least in part, from cross-sectional variation in focal firms' disclosure policies and use of RPE. With this fixed effect structure, with compare stock price reactions for peers' of rTSR-using focal firms to stock reactions for peers' of relative-profit-using focal firms. If rTSR-using firms are systematically different from relative-profit using firms, this specification can produce biased coefficients.

To reduce the potential for bias, we estimate the same effects in tighter specifications, where we replace industry fixed effects with firm fixed effects. This fixed effect structure subsumes cross-sectional variation in focal firms' disclosure policies and use of RPE; focal firms only contribute to the estimated coefficients if they use both price-based RPE and profit-based RPE at some point in our sample window. With this fixed effect structure, we compare the stock-price reactions of a given focal firm's price-peers to the stock price

[1] In all of our analyses, we include year-month fixed effects.

reactions of that same focal firm's profit-peers. This specification ensures that our inferences are impervious to time-invariant differences across focal firms and their peers. However, if focal firms' price-peers are systematically different from their profit-peers, this specification can produce biased coefficient estimates.

In our tightest specifications, we replace firm and peer fixed effects with pairwise firm-peer fixed effects. This fixed effect structure subsumes all time-invariant characteristics at the firm-peer pair level. As such, firm-peer pairs only contribute to the estimated coefficients if the focal firm uses the peer as both a price-peer and a profit-peer, at some point over our sample window. With this fixed effect structure, the estimated coefficients are identified from within firm-peer variation; we compare the stock price reactions of a given focal firm's given peer when that peer is a price-peer to the focal firm versus when that same peer is a profit-peer to the same focal firm. This fixed effect structure ensures that our inferences are impervious to time-invariant differences across firm-peer pairs.

Even with our tightest fixed effect structure, our inferences are still potentially susceptible to bias. In particular, focal firms likely do not switch a given peer from being a price-peer to a profit peer (or vice versa) at random. Such switches may be a response to time-varying characteristics, at the firm-peer level, that affect the optimal compensation plan. If these time-varying characteristics also effect peers' stock price reactions to focal firms' disclosures—a possibility we cannot entirely rule out—then this specification can produce biased coefficient estimates. For this reason, we augment our baseline analysis to identify results using different treatment/control splits, which we discuss below.

For our first treatment/control group modification, we form alternative treatment samples, using the algorithmic approach to peer group construction developed by Bloomfield, Guay, and Timmermans (2021). For each firm-year observation in our sample, we construct an artificial peer group using an algorithm designed to maximize the correlation between the equal-weighted peer group's portfolio returns and the focal firm's returns.

Bloomfield et al., (2021) shows that this approach yields peer groups that are very similar in nature to the peer groups actually constructed by rTSR-using firms. The artificial peer groups are approximately as effective, on average, at filtering noise (50.4% R^2 for the artificial peer group versus 54.1% R^2 for the actual peer group). Moreover the effectiveness of the artificial peer group and the actual peer group is highly correlated ($\rho = 0.788$), and the artificial peer group and actual peer group contain substantial overlap in chosen peers—on average 41.5% of peers chosen by the algorithm are also chosen for the actual peer group.

Using this algorithm, we construct three modified treatment groups. First, we exclude all price-peers that are included in the artificial peer group, yielding a treatment group of price-peers that are included as actual price-peers, but *not* as artificial price-peers. Second, we exclude all price-peers that are *not* included in the artificial peer group, yielding a treatment group of peers that are included as actual price-peers, *and* as artificial price-peers. Third, we exclude all actual price-peers, and replace them with the remaining artificial price-peers, yielding a placebo treatment group of peers that are included as artificial price-peers but *not* as actual price-peers.

For our second treatment/control group modification, we form alternative control samples by augmenting the sample to include all RPE peers that were ever used by each focal firm, at some point over the sample window. We refer to a peer as 'active' if they are currently in use as an RPE peer, and 'inactive' if they are not, but were active at some point in the past or future. We estimate difference-and-differences coefficients for the inactive and active samples, and the difference-in-differences coefficients across the two samples to form a triple-differences design. In this triple-differences design, inactive peers (both price-peers and profit-peers) create an extra layer of control, with the inactive peers forming a placebo sample against which to benchmark the estimated treatment effect.

For our third treatment/control group modification, we leverage one more institutional feature to aid in causal attribution, and provide further evidence of the disclosing firms' strategic intents. The vast majority of price RPE assesses performance based on percentile ranks, *not* performance relative to the peer group average. This institutional fact means that CEO's don't benefit by sabotaging price peers indiscriminately; sabotaging a peer that is already far behind or insurmountably ahead is likely to have no impact on the firm's percentile ranking. To maximize the benefits of the disclosure-based sabotage strategy, firms will choose to target peers whose year-to-date performance is close to their own—especially towards the end of the fiscal year.

With this intuition in mind, we modify our analysis restricting the sample to rTSR peers. We then construct a continuous measure of treatment, by exploiting variation in year-to-date TSR proximity between each focal firm and its rTSR peers. Worth noting, this source of variation is plausibly exogenous, as firms are likely not able to choose (nor even predict), which peers will have the most similar performance to themselves by the end of the fiscal year. We then use this variation to examine whether year-to-date TSR proximity explains peers' returns on focal firms' disclosure dates, and whether this relation varies over the course of the focal firms' fiscal year.

4.2 Baseline Analysis

We first examine whether price-peers underperform on focal firm's voluntary disclosure dates. To do so, we use variants on the following regression specification:

$$Peer\ Ret_{j,t} = \beta_1 rTSR_{i,j,t} + \beta_2 Firm\ Ret_{i,t} + \beta_3 rTSR_{i,j,t} \times Firm\ Ret_{i,t} + \tau_t + \theta_{i,j} + \varepsilon_{j,t}, \quad (2)$$

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; $rTSR$ takes a value of one (zero) if peer j is a price- (profit-)peer of firm i. In some instances, a firm will use a given peer as both a price-peer and a profit-peer, simultaneously. In these cases, $rTSR$ is set to 0.5.

The coefficient of interest is β_1, which reflects the average *level shift* for price-peers' returns, after controlling for focal-firm returns. We further include an interaction term, $rTSR_{i,j,t} \times Firm\ Ret_{i,t}$, to accommodate the possibility that price-peers might have a different comovement pattern to their respective focal firms (e.g., perhaps price-peers have more correlated price movements). If uncontrolled for, such differences could result in apparent differences in peer return levels, due to an information spillover channel, rather than a sabotage channel.[2]

To control for unobservable variation, we use a variety of cross-sectional fixed effect structures, θ: industry + peer; firm + peer; and firm-peer pair. 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

[2] We recognize that the inclusion of the interaction term can complicate the interpretation of the main effect. In our case, this seems not to be an issue; the coefficients on $rTSR_{i,j,t} \times Firm\ Ret_{i,t}$ tend to be economically small and statistically insignificant. Moreover, we can mean-center $Firm\ Ret$ variable, or drop the interaction term from the analysis without qualitatively affecting any of our primary inferences.

regression for firms' disclosure (non-disclosure) days. After each specification pair, we present a statistical test of differences in coefficients on $rTSR$. Results are tabulated in Table 2.

We find that $rTSR$ carries a significantly negative coefficient for all of the disclosure day specifications. Moreover, this coefficient is significantly different from the corresponding non-disclosure day coefficient, in all cases. This indicates that, relative to non-disclosure days, price-peers significantly underperform on firm's disclosure dates. Notably, these results extend even to the tightest specifications, in which we use firm-peer fixed effects. These specifications rely on evolving peer relations; sometimes a firm uses a peer as a profit-peer and other times as a price-peer. Our results imply that, even within a firm-peer combination, a peer's underperformance during the firm's disclosure days is greater when being used as a price-peer. In terms of economic magnitudes, our results suggest that price-peers underperform by an average of 20 to 30 basis points on focal firms' voluntary disclosure dates.

As noted above, there is substantial endogeneity with regards 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. If these differences are correlated with stock price reactions to focal firms' voluntary disclosures, we may spuriously interpret price-peers' underperformance as evidence of sabotage. In what follows, we attempt to address this concern by using alternative treatment and control samples.

4.2.1 Alternative Treatment Groups

We augment our baseline analysis by constructing alternate treatment groups. In the baseline analysis, our treatment observations (price-peers) could differ from our control observations (profit-peers) in two ways: (1) they could differ with respect to their stock price comovement relations to the focal firms; or (2) they could differ for reasons unrelated to their stock price comovement relations to the focal firms. Either source of divergence between treatment and control samples could be potentially problematic for our analysis. We use the algorithmic approach to peer group construction, developed by Bloomfield et al., (2021) to disentangle these two sources of divergence.

For each focal firm-year, we construct an optimal rTSR peer group from the standpoint of filtering common stock price risk, based purely on historical stock return comovements. We refer to this peer group as the 'artificial' peer group. Using these artificial peer groups, we form three non-overlapping treatment groups: (1) actual price-peers that are not artificial peers; (2) actual price-peers than are also artificial peers; and (3) artificial peers that are not actual price-peers.

For the first two alternative treatment groups, we expect the difference-in-differences results to be similar to the baseline results. If not, it would suggest that some endogenous aspect of price-peer selection drives our findings, rather than strategic sabotage. The third treatment group is a placebo treatment sample, and we do not expect to observe a significant difference between the treatment and control groups. If a difference does exist, it would suggest that some latent characteristics of an effective price-peer drives our inferences, rather than strategic sabotage. We present our results in Table 3.

In Panel A, we present results for the treatment sample comprised of actual price-peers that are not artificial peers. We find that the treatment sample reacts more negatively than the control sample on focal firms' voluntary disclosure dates. This suggests that the baseline results cannot be attributed to the fact that focal firms endogenously select price-peers based on return comovement patterns.

In Panel B, we present results for the treatment sample comprised of actual price-peers that are also artificial peers. Similar to Panel A, we find that the treatment sample reacts more negatively than the control sample on focal firms' voluntary disclosure dates. This suggests that the baseline results cannot be attributed to the fact that focal firms endogenously select price-peers based on some criteria other than return comovement patterns.

In Panel C, we present results for the placebo treatment sample comprised of artificial peers that are not actual price-peers. We find no evidence that the placebo treatment sample reacts differentially on focal firms' voluntary disclosure dates.

4.2.2 Alternative Control Group

In the analysis to this point, we use profit-peers as a control group. In this section, we instead use inactive price-peers as an additional control group, where an "inactive" peer is a price- or profit-peer that a focal firm uses as a peer at some point over the sample window, but does not use currently. We replicate the baseline difference-in-differences analyses for the active and inactive peer samples, and tabulate the results in Table 4. Panel A presents results using industry and peer fixed effects; Panel B presents results using firm and peer fixed effects; and Panel C presents results using pairwise firm-peer fixed effects.

Within each panel, specifications (1) and (2), as well as their difference, exactly replicate the difference-in-differences analyses tabulated in Table 2. Specifications (3) and (4), as well as their difference, form a placebo difference-in-differences analysis using the sample of inactive peers. In the last column of each panel, we compare the difference-in-differences estimates across active and inactive samples to generate a triple-differences estimate.

Across all three panels, we find that the difference-in-differences estimate is significantly negative for the active peers sample, but not for the inactive peers sample. Moreover, the difference across the two samples (the triple-differences estimate) is negative and statistically significant across all three panels. That is, on average, price-peers react more negatively than profit-peers to focal firms' voluntary disclosures, but only when currently active as price-peers. Past price-peers and future price-peers, whose performance is not explicitly considered in CEO compensation awards, do not appear to suffer any ill effects when focal firms issue voluntary disclosures. This is particularly notable since focal firms' economic relations to past price-peers and future price-peers are likely quite comparable to their economic relations to current price-peers.

In terms of economic magnitudes, the triple-differences results are comparable to the baseline analyses (Table 2). On focal firms' voluntary disclosure dates, active price-peers underperform by 20 to 46 basis points, on average.

4.3 Specificity

We next examine the specificity of the price-peers' negative stock price reactions to firm disclosures. In particular, we seek to determine whether there is a moderate downward reaction across all price-peers versus a large downward reaction among a concentrated subset of price-peers, and little-to-no negative reaction among the rest. To do so, we re-use the regression specification defined by eq. (3) with a few adjustments, which we detail below.

First, we collapse the dataset such that the unit of observation is the firm-day, as opposed to firm-peer-day. Second, we replace the outcome variable (*Peer Ret* in eq. (3)) with summary statistics to describe the distribution of *Peer Ret* at the firm-day level. In particular, we extract the following five summary statistics: minimum; 10th percentile; median; 90th percentile; and maximum. Third, we add a control for the number of peers, $log(Num.\ Peer)$, to address the mechanical relation between sample size and extrema.

With these adjustments, we replicate the regression analysis for all of the disclosure and non-disclosure days in our sample. We run five regressions, one for each of the summary statistics (i.e., minimum; 10th percentile; median; 90th percentile; and maximum.) The results are tabulated in Table 5. Panel A presents regression results for the disclosure day sample; Panel B presents regression results for the non-disclosure day sample.

We find that, on focal firms' voluntary disclosure dates, price-peers' underperformance is driven by extremely negative performance at the left tail. We observe no significant differences between price-peers and profit-peers at the median, 90th percentile or maximum. However, there is a marginally significant disparity between price-peers and profit-peers at the 10th percentile ($\sim$20 basis points) and a dramatic disparity at the minimum ($\sim$40 basis points). We find no evidence that price-peers and profit peers differ from each other, vis-á-vis their daily return distributions, on non-disclosure days.

4.4 Targeting

Having established that the negative price effect is highly specific to a particular peer, we next test predictions regarding *which* peer is most likely to be targeted. We posit that focal firms choose to sabotage the peer whose performance is most likely to be marginal in determining the executive's compensation. As rTSR is typically implemented as a rank-order tournament, this implies that firms should choose to sabotage the peers whose performance (in lieu of sabotage) is expected to be most similar to their own.

With this intuition in mind, we test our prediction 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 examine whether year-to-date TSR proximity explains the peers' underperformance on disclosure days.

In these tests, we discard all firm-peer pairs that use non-price RPE. 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.[3] This variation is plausibly exogenous in the sense that it is not a choice variable for either the firm, nor the peer, and is difficult to forecast at the time of contracting. However, this variation is easy to observe, ex post, and can therefore affect disclosure choices.

We test our predictions with variants on the following regression specification:

$$Peer\ Ret_{j,t} = \beta_1 Proximity_{i,j,t} + \beta_2 Firm\ Ret_{i,t} + \beta_3 Proximity_{i,j,t} \times Firm\ Ret_{i,t} + \tau_t + \theta_{i,j} + \varepsilon_{j,t}. \tag{3}$$

As in the baseline analyses, we use three cross-sectional fixed effect structures, θ: firm's 4-digit SIC + peer; firm + peer; and firm-peer pair. 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 (non-disclosure) days. After each specification pair, we present a statistical

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

test of differences in coefficients on *Proximity*. Results are tabulated in Table 6.

We find that *Proximity* is a highly significant explanator of peers' underperformance on disclosure dates. On average, moving from the extreme low end of the *Proximity* distribution to the extreme high end of the *Proximity* distribution is associated with an 11–13 basis point reduction in a peer's underperformance on focal firms' disclosure days, as shown in Table 6 Panel A.

In Table 6 Panel B, we further examine whether this relation evolves over the course of a focal firm's fiscal year. Toward the end of the fiscal year, a CEO is better able to assess which firms are likely to marginal vis-á-vis year-end ranking (and therefore compensation). We augment the regression specification to include the focal firm's fiscal month, *F. Month* (a variable that goes from 1-12), along with its interaction with proximity, *Proximity*×*F. Month*. Consistent with our predictions, we find that the relation between *Proximity* and *Peer Ret* strengthens over the course of the focal firm's fiscal year.

4.5 Cross-Sectional Variation

We next examine which types of firms are most likely to use this strategy, and which types of price-peers are most likely to be affected. In particular, we examine whether these strategies are employed predominately by big or small firms, as well as whether firms tend to target relatively larger or smaller firms. To do so, we replicate the baseline analyses, splitting the sample into two groups based on size (or relative size), where we measure size as the market value of equity. Results for these tests are tabulated in Table 7.

In Panel A, we split observations at the median into 'Small Firms' and 'Big Firms,' based on focal firm size, and estimate the disclosure date coefficient on $rTSR$ separately for the two size groups.[4] For parsimony, we do not examine non-disclosure dates in these analyses. We then compare across the two groups to form difference-in-differences estimates.

In Panel B, we split observations based on the relative sizes of focal firms and their peers. We estimate the disclosure date coefficient on $rTSR$ separately for peers that are bigger than the focal firm, and peers than are smaller than the focal firm. As in Panel A, we do not examine non-disclosure dates in these analyses. We then compare across the two groups to form difference-in-differences estimates.

We do not observe any significant cross-sectional patterns, vis-á-vis firms' reliance on disclosure-based sabotage tactics. We find that firms' use of disclosure sabotage strategies does not vary much with size, nor relative peer size. That is, this strategy is used by large and small firms, alike. And firms are similarly likely to use this strategy against larger or smaller firms.

We further examine whether firms use these sabotage tactics differentially against same-industry peers versus different industry peers. We split observations based on whether a peer resides within the same primary 1-digit SIC as the focal firm, and replicate the analyses for these two groups separately. These results are tabulated in Table 8.

We find that this strategy is not constrained by industry boundaries. Firms use the strategy against peers inside and outside their own 1-digit SIC, with similar prevalence. This last result is particularly notable, since it helps to preclude the possibility that focal firms' disclosures affect price-peers incidentally (e.g., due to information spillovers arising from inherent similarities between the focal firm and its peers). Such incidental spillovers

[4]The samples are different sizes because larger firms issue voluntary disclosures more frequently.

would almost certainly be more prevalent within industries than across industries, which is not what we observe.

4.6 Capital Market Costs

Finally, we analyze the potential costs this strategy may impose on the firms that use it. Specifically, we examine whether rTSR-using firms' disclosures differ in the information they provide to help investors value the focal firm. If rTSR-using firms are, in fact, using voluntary disclosures to provide negative information about peers, this likely comes at the expense of information about themselves. If so, we would expect rTSR firms' voluntary disclosures to be relatively more informative about peer value, and relatively less informative about their own value. We test this prediction by exploiting trading volume as a measure of information flow.

We test for the impact of voluntary disclosure on peer trading volume using variants on the following regression specification:

$$\begin{aligned} log(Peer\ Volume_{j,t}) = {} & \beta_1 Disc.\ Day_{i,t} \times rTSR_{i,j,t} + \beta_2 Disc.\ Day_{i,t} + \beta_3 log(Volume_{i,t}) \\ & + \beta_4 Disc.\ Day_{i,t} \times log(Volume_{i,t}) + \tau_t + \theta_{i,j} + \varepsilon_{j,t}, \end{aligned} \tag{4}$$

where $log(Peer\ Volume)$ is the peer's trading volume and $log(Volume)$ is the focal firm's trading volume.

Similarly, we test for the for the impact of voluntary disclosure on the focal firm's trading volume using variants on the following regression specification:

$$\begin{aligned} log(Volume_{i,t}) = \beta_1 Disc.\ Day_{i,t} \times Any\ rTSR_{i,t} \\ + \beta_2 Disc.\ Day_{i,t} + \beta_2 Any\ rTSR_{i,t} + \tau_t + \theta_i + \varepsilon_{i,t}, \end{aligned} \tag{5}$$

where $Any\ rTSR$ is an indicator equal to one if a focal firm uses any rTSR. Results from these analyses are presented in Table 9. In specifications (1) through (3), we present regression results regarding peers' trading volumes; In specifications (4) and (5), we present regression results regarding focal firms' trading volumes. For these latter specifications, we collapse the dataset such that the unit of observation is the firm-day, as opposed to firm-peer-day.

We find that disclosure dates are associated with greater trading volume for both the focal firm, and its peers. In cross-sectional specifications (i.e., in which we use industry fixed effects, but not firm fixed effects), we do not find any significant moderating effect based on the type of RPE used. However, in tighter specifications (i.e., with firm and/or firm-peer fixed effects), we find that the focal firm's reliance on rTSR significant moderates the relation between disclosure and trading volume. In specifications (2) and (3), we document that price-peers experience a greater increase in trading volume than profit-peers on focal firms' voluntary disclosure dates. In specification (5), we document that focal firms experience less of a increase in trading volume on their voluntary disclosure dates, when they use rTSR.

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 sabotage strategies, in which they disseminate negative information about their peers, seemingly lieu of information about themselves. These findings thereby imply that disclosure-based sabotage strategies 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.

5 Conclusion

We provide evidence that rTSR-using firms routinely engage in disclosure-based sabotage tactics against their RPE peers. While RPE-induced sabotage is a well-established result in the broader economics literature, our results provide the first clear evidence that these sabotage strategies extend to an inter-firm corporate context.

We examine only a single sabotage channel (i.e., voluntary disclosure) out of a large set of potential approaches to peer-harming behavior. As such, we view our study as setting the lower bar for the overall prevalence of RPE-induced inter-firm sabotage. Future work can examine other channels, such as aggressive price cutting, market-stealing advertising campaign, product harmonization and excess production.

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Table 1: Summary Statistics

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 to 2016. Only firms with active RPE grants in their CEOs' pay packages are included in the sample. Panel A presents descriptive statistics at the firm-peer-date-level. Panel B presents descriptive statistics at the firm-date-level.

Panel A: Descriptive Statistics at the Firm-Peer-Day-level

Variables	Num Obs.	Mean	SD	D1	Q1	Med.	Q3	D9
rTSR	7,769,934	0.766	0.394	0.000	0.500	1.000	1.000	1.000
Disc. Day	7,769,934	0.011	0.104	0.000	0.000	0.000	0.000	0.000
Firm Ret	7,769,934	0.052	2.008	−2.093	−0.874	0.061	0.985	2.158
Peer Return	7,769,934	0.047	2.069	−2.158	−0.899	0.052	1.000	2.208
Proximity	6,300,794	0.654	0.223	0.326	0.500	0.692	0.840	0.917
log(Volume)	7,769,934	14.187	1.269	12.603	13.374	14.168	14.996	15.813
log(Peer Volume)	7,769,037	14.016	1.511	12.163	13.086	14.080	15.002	15.831

Panel B: Descriptive Statistics at the Firm-Day-level

Variables	Num Obs.	Mean	SD	D1	Q1	Med.	Q3	D9
Any rTSR	556,421	0.763	0.425	0.000	1.000	1.000	1.000	1.000
Disc. Day	556,421	0.010	0.098	0.000	0.000	0.000	0.000	0.000
Peer Return$_{min}$	556,421	−1.957	2.239	−5.321	−3.025	−1.506	−0.513	0.285
Peer Return$_{10}$	556,421	−1.364	1.970	−3.885	−2.188	−1.030	−0.210	0.539
Peer Return$_{50}$	556,421	0.029	1.716	−1.772	−0.718	0.057	0.803	1.755
Peer Return$_{90}$	556,421	1.488	2.035	−0.526	0.317	1.196	2.349	4.033
Peer Return$_{max}$	556,421	2.132	2.339	−0.254	0.641	1.707	3.249	5.618
log(Volume)	550,944	14.328	1.312	12.685	13.470	14.341	15.204	15.993

Table 2: Baseline Analysis

This table presents the baseline analyses. The primary dependent variable is $rTSR$ which takes a value of one (zero) [one-half] for an RPE peer that the focal firm uses only as a price-peer (only as a profit-peer) [both a price-peer and a profit-peer]. In odd-numbered (even-numbered) specifications, we present regression results estimated from focal firms' disclosure (non-disclosure) days. After each specification pair, we present a test of the difference in coefficients on $rTSR$ across disclosure and non-disclosure 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. In all specifications, the dependent variable is *Peer Return*, the RPE peers' daily return. Below each coefficient is a t-statistic, in parentheses, calculated using standard errors clustered by industry and date.

	Outcome = Peer Return								
	(1)	(2)	(1)-(2)	(3)	(4)	(3)-(4)	(5)	(6)	(5)-(6)
rTSR	−0.194**	0.005**	−0.199**	−0.261***	0.006	−0.266***	−0.301***	0.003	−0.304***
	(−2.523)	(2.256)	(−2.606)	(−3.861)	(1.066)	(−3.835)	(−4.348)	(0.504)	(−4.235)
Firm Ret	0.159***	0.668***		0.161***	0.668***		0.162***	0.668***	
	(7.477)	(25.682)		(7.738)	(25.688)		(7.492)	(25.681)	
rTSR x Firm Ret	0.018	−0.080***		0.016	−0.080***		0.016	−0.080***	
	(0.643)	(−2.843)		(0.570)	(−2.842)		(0.564)	(−2.840)	
Sample	Disc.	No Disc.		Disc.	No Disc.		Disc.	No Disc.	
Fixed Effects	Year-Month, SIC, Peer	Year-Month, SIC, Peer		Year-Month, Firm, Peer	Year-Month, Firm, Peer		Year-Month, Firm-Peer	Year-Month, Firm-Peer	
Observations	85,492	7,684,377		85,492	7,684,377		85,492	7,684,377	
R-Squared	0.155	0.349		0.164	0.349		0.189	0.349	

Table 3: Actual versus Artificial Peers

This table presents an modification of the baseline analyses using alternative 'treatment' groups, based on the artificial peer groups developed by Bloomfield et al., (2021). In Panels A and B, we partition the baseline sample of price-peers based on inclusion in the artificial peer group: Panel A excludes price-peers if they are included in the artificial peer group; Panel B excludes price-peers if they are *not* included in the artificial peer group. In Panel C, we construct a placebo sample of price-peers, comprised of artificial peers that are *not* used as actual price-peers. Within each panel, the analysis exactly mirrors that of Table 2. Below each coefficient is a t-statistic, in parentheses, calculated using standard errors clustered by industry and date.

Panel A: Actual peers not included in artificial peer group

	Outcome = Peer Return								
	(1)	(2)	(1)-(2)	(3)	(4)	(3)-(4)	(5)	(6)	(5)-(6)
rTSR	−0.173**	0.005**	−0.178**	−0.210***	0.005	−0.215***	−0.244***	0.005	−0.249***
	(−2.457)	(2.469)	(−2.552)	(−3.660)	(1.257)	(−3.720)	(−4.252)	(0.942)	(−4.287)
Firm Ret	0.169***	0.669***		0.172***	0.669***		0.173***	0.669***	
	(7.999)	(26.964)		(8.281)	(26.972)		(8.021)	(26.965)	
rTSR x Firm Ret	0.003	−0.093***		0.001	−0.093***		0.002	−0.093***	
	(0.096)	(−3.463)		(0.036)	(−3.461)		(0.055)	(−3.459)	
Sample	Disc.	No Disc.		Disc.	No Disc.		Disc.	No Disc.	
Fixed Effects	Year-Month, SIC, Peer	Year-Month, SIC, Peer		Year-Month, Firm, Peer	Year-Month, Firm, Peer		Year-Month, Firm-Peer	Year-Month, Firm-Peer	
Observations	78,213	7,142,068		78,213	7,142,068		78,213	7,142,068	
R-Squared	0.155	0.342		0.164	0.342		0.191	0.342	

Panel B: Actual peers also included in artificial peer group

	Outcome = Peer Return								
	(1)	(2)	(1)-(2)	(3)	(4)	(3)-(4)	(5)	(6)	(5)-(6)
rTSR	−0.283***	−0.002	−0.281***	−0.311***	−0.004	−0.307***	−0.414***	−0.011*	−0.403***
	(−4.578)	(−0.389)	(−4.578)	(−4.788)	(−1.130)	(−4.786)	(−4.829)	(−1.903)	(−4.910)
Firm Ret	0.169***	0.660***		0.170***	0.660***		0.170***	0.660***	
	(8.668)	(28.459)		(8.823)	(28.461)		(8.569)	(28.450)	
rTSR x Firm Ret	0.107***	0.032		0.103***	0.032		0.103***	0.032	
	(3.233)	(1.253)		(3.141)	(1.251)		(3.015)	(1.252)	
Sample	Disc.	No Disc.		Disc.	No Disc.		Disc.	No Disc.	
Fixed Effects	Year-Month, SIC, Peer	Year-Month, SIC, Peer		Year-Month, Firm, Peer	Year-Month, Firm, Peer		Year-Month, Firm-Peer	Year-Month, Firm-Peer	
Observations	27,154	2,760,926		27,154	2,760,926		27,154	2,760,926	
R-Squared	0.205	0.412		0.216	0.412		0.234	0.412	

Panel C: Artificial peers not included in actual peer group

	Outcome = Peer Return								
	(1)	(2)	(1)-(2)	(3)	(4)	(3)-(4)	(5)	(6)	(5)-(6)
rTSR	−0.075	0.003	−0.078	−0.026	0.005	−0.031	−0.016	0.004	−0.020
	(−1.140)	(0.784)	(−1.178)	(−0.410)	(1.572)	(−0.491)	(−0.137)	(0.776)	(−0.180)
Firm Ret	0.167***	0.662***		0.169***	0.662***		0.170***	0.662***	
	(9.256)	(28.191)		(9.549)	(28.198)		(9.117)	(28.181)	
rTSR x Firm Ret	0.004	−0.110***		0.005	−0.110***		0.005	−0.110***	
	(0.166)	(−5.673)		(0.248)	(−5.671)		(0.227)	(−5.661)	
Sample	Disc.	No Disc.		Disc.	No Disc.		Disc.	No Disc.	
Fixed Effects	Year-Month, SIC, Peer	Year-Month, SIC, Peer		Year-Month, Firm, Peer	Year-Month, Firm, Peer		Year-Month, Firm-Peer	Year-Month, Firm-Peer	
Observations	54,360	5,647,522		54,360	5,647,522		54,360	5,647,522	
R-Squared	0.167	0.339		0.180	0.339		0.220	0.339	

Table 4: Active Status

This table presents a triple-differences modification of the baseline analyses, using inactive firm-peer relationships to form an extra layer of control. Within each panel, we present two tests of difference-in-difference tests. Specifications (1) and (2) use a sample of active peers, and therefore exactly replicate tests found in Table 2. Specifications (3) and (4) use a sample of inactive peers, and therefore form both a placebo sample, and a control group. In odd-numbered (even-numbered) specifications, we present regression results estimated from focal firms' disclosure (non-disclosure) days. After each specification pair, we present a test of the difference in coefficients on $rTSR$ across disclosure and non-disclosure days. The final column in each panel presents a statistical test of the differences across the difference-in-difference estimates from the active and inactive peer samples (i.e., a triple-differences estimate). Panels differ with respect to fixed effect structure. Panel A specifications include industry and peer fixed effects; Panel B specifications include firm and peer fixed effects; Panel C specifications include pairwise firm-peer fixed effects. Across all panels, 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.

Panel A: Industry and peer fixed effects

	Outcome = Peer Return						
	(1)	(2)	(1)-(2)	(3)	(4)	(3)-(4)	((1)-(2))-((3)-(4))
	Active Peers			Inactive Peers			
rTSR	−0.194**	0.005**	−0.199**	0.012	0.002	0.010	−0.209**
	(−2.523)	(2.256)	(−2.606)	(0.188)	(0.375)	(0.160)	(−2.155)
Firm Ret	0.159***	0.668***		0.135***	0.605***		
	(7.477)	(25.682)		(4.303)	(22.998)		
rTSR x Firm Ret	0.018	−0.080***		0.062**	−0.020		
	(0.643)	(−2.843)		(2.066)	(−0.803)		
Sample	Disc.	No Disc.		Disc.	No Disc.		
Fixed Effects	Year-Month, SIC, Peer	Year-Month, SIC, Peer		Year-Month, SIC, Peer	Year-Month, SIC, Peer		
Observations	85,492	7,684,377		64,384	5,305,577		
R-Squared	0.155	0.349		0.176	0.303		

Panel B: Firm and peer fixed effects

	Outcome = Peer Return						
	(1)	(2)	(1)-(2)	(3)	(4)	(3)-(4)	((1)-(2))-((3)-(4))
	Active Peers			Inactive Peers			
rTSR	−0.261***	0.006	−0.266***	0.024	−0.006*	0.030	−0.296**
	(−3.861)	(1.066)	(−3.835)	(0.379)	(−1.700)	(0.471)	(−2.448)
Firm Ret	0.161***	0.668***		0.134***	0.606***		
	(7.738)	(25.688)		(4.237)	(23.004)		
rTSR x Firm Ret	0.016	−0.080***		0.062**	−0.020		
	(0.570)	(−2.842)		(2.061)	(−0.805)		
Sample	Disc.	No Disc.		Disc.	No Disc.		
	Active Peers			Inactive Peers			
Fixed Effects	Year-Month, Firm, Peer	Year-Month, Firm, Peer		Year-Month, Firm, Peer	Year-Month, Firm, Peer		
Observations	85,492	7,684,377		64,384	5,305,577		
R-Squared	0.164	0.349		0.177	0.303		

Panel C: Firm-peer pair fixed effects

	Outcome = Peer Return						
	(1)	(2)	(1)-(2)	(3)	(4)	(3)-(4)	((1)-(2))-((3)-(4))
rTSR	−0.301***	0.003	−0.304***	0.152	−0.003	0.156	−0.460***
	(−4.348)	(0.504)	(−4.234)	(1.249)	(−0.498)	(1.282)	(−2.628)
Firm Ret	0.162***	0.668***		0.132***	0.606***		
	(7.492)	(25.681)		(4.066)	(22.995)		
rTSR x Firm Ret	0.016	−0.080***		0.065**	−0.020		
	(0.564)	(−2.840)		(2.077)	(−0.803)		
Sample	Disc.	No Disc.		Disc.	No Disc.		
Fixed Effects	Year-Month, Firm-Peer	Year-Month, Firm-Peer		Year-Month, Firm-Peer	Year-Month, Firm-Peer		
Observations	85,492	7,684,377		64,384	5,305,577		
R-Squared	0.189	0.349		0.193	0.304		

Table 5: Specificity

This table presents firm-date results on the relation between $rTSR$ and the distribution of peer returns. The dependent variable changes across specifications, reflecting five different summary statistics of the return distribution: In specification (1), the dependent variable is $Peer\ Ret_{min}$; In specification (2), the dependent variable is $Peer\ Ret_{10}$; In specification (3), the dependent variable is $Peer\ Ret_{med}$; In specification (4), the dependent variable is $Peer\ Ret_{90}$; In specification (5), the dependent variable is $Peer\ Ret_{max}$; Panel A presents results for focal firm disclosure days; Panel B presents results for focal firm non-disclosure days. All specifications include year-month and firm fixed effects. Below each coefficient is a t-statistic, in parentheses, calculated using standard errors clustered by industry and date.

Panel A: Disclosure Dates

VARIABLES	Peer Ret$_{min}$ (1)	Peer Ret$_{10}$ (2)	Peer Ret$_{med}$ (3)	Peer Ret$_{90}$ (4)	Peer Ret$_{max}$ (5)
rTSR	−0.381**	−0.191*	−0.126	−0.042	0.022
	(−2.364)	(−1.665)	(−1.405)	(−0.286)	(0.136)
Firm Ret	0.185***	0.195***	0.202***	0.198***	0.191***
	(5.529)	(6.449)	(6.468)	(7.020)	(6.089)
rTSR x Firm Ret	−0.003	−0.004	−0.007	0.006	0.007
	(−0.074)	(−0.103)	(−0.198)	(0.176)	(0.179)
log(Num. Peer)	−0.894***	−0.086	−0.081	0.068	0.833***
	(−4.236)	(−0.678)	(−1.083)	(0.587)	(5.799)
Fixed Effects	Year-Month, Firm	Year-Month, Firm	Year-Month, Firm	Year-Month, Firm	Year-Month, Firm
Observations	5,375	5,375	5,375	5,375	5,375
R-squared	0.364	0.336	0.299	0.329	0.343

Panel B: Non-Disclosure Dates

VARIABLES	Peer Ret$_{min}$ (1)	Peer Ret$_{10}$ (2)	Peer Ret$_{med}$ (3)	Peer Ret$_{90}$ (4)	Peer Ret$_{max}$ (5)
rTSR	−0.032	−0.027	0.006	0.045	0.040
	(−0.360)	(−0.353)	(1.043)	(0.631)	(0.469)
Firm Ret	0.638***	0.660***	0.695***	0.688***	0.673***
	(33.345)	(32.494)	(28.340)	(32.765)	(33.959)
rTSR x Firm Ret	−0.077***	−0.070***	−0.093***	−0.076***	−0.081***
	(−3.534)	(−3.221)	(−3.745)	(−3.399)	(−3.618)
log(Num. Peer)	−0.894***	−0.333***	−0.003	0.342***	0.949***
	(−13.526)	(−4.213)	(−0.528)	(4.283)	(14.389)
Fixed Effects	Year-Month, Firm	Year-Month, Firm	Year-Month, Firm	Year-Month, Firm	Year-Month, Firm
Observations	556,421	556,421	556,421	556,421	556,421
R-squared	0.480	0.521	0.593	0.506	0.471

Table 6: Strategic Targeting

This table presents results on the relation between year-to-date TSR proximity, and disclosure day underperformance. We restrict the sample to only include price-peers. The primary dependent variable is *Proximity* which is the absolute value of the difference in year-to-date performance (excluding the current date) between the focal firm and peer, starting from the first day of the focal firm's fiscal year. In odd-numbered (even-numbered) specifications, we present regression results estimated from focal firms' disclosure (non-disclosure) days. After each specification pair, we present a test of the difference in coefficients on $rTSR$ across disclosure and non-disclosure 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. In all specifications, the dependent variable is *Peer Return*, the RPE peers' daily return. Panel A presents a static estimation of the relation between *Proximity* and *Peer Return*; Panel B presents a dynamic estimation of the relation between *Proximity* and *Peer Return*, by allowing the relation to evolve linearly over the course of the focal firm's fiscal year. Below each coefficient is a t-statistic, in parentheses, calculated using standard errors clustered by industry and date.

Panel A: Main Effect of Proximity

	Outcome = Peer Return								
	(1)	(2)	(1)-(2)	(3)	(4)	(3)-(4)	(5)	(6)	(5)-(6)
Proximity	−0.114***	−0.005	−0.108***	−0.120***	−0.005	−0.115***	−0.133***	−0.006	−0.126***
	(−2.990)	(−0.950)	(−2.844)	(−3.141)	(−0.875)	(−3.015)	(−2.937)	(−1.034)	(−2.876)
Firm Ret	0.166***	0.524***		0.168***	0.524***		0.166***	0.525***	
	(9.395)	(20.963)		(9.440)	(21.001)		(9.019)	(21.032)	
Proximity x Firm Ret	0.014	0.111***		0.015	0.111***		0.019	0.111***	
	(0.834)	(5.908)		(0.860)	(5.920)		(1.036)	(5.941)	
Sample	Disc.	No Disc.		Disc.	No Disc.		Disc.	No Disc.	
Fixed Effects	Year-Month, SIC, Peer	Year-Month, SIC, Peer		Year-Month, Firm, Peer	Year-Month, Firm, Peer		Year-Month, Firm-Peer	Year-Month, Firm-Peer	
Observations	72,543	6,221,764		72,543	6,221,764		72,543	6,221,764	
R-Squared	0.164	0.340		0.168	0.340		0.192	0.340	0.339

Panel B: Interaction with *F. Month*

	Outcome = Peer Return								
	(1)	(2)	(1)-(2)	(3)	(4)	(3)-(4)	(5)	(6)	(5)-(6)
Proximity x F. Month	−0.028**	0.001	−0.029***	−0.028**	0.000	−0.028***	−0.029***	0.000	−0.030***
	(−2.603)	(0.825)	(−2.677)	(−2.551)	(0.713)	(−2.623)	(−2.640)	(0.675)	(−2.757)
Proximity	0.058	−0.009		0.049	−0.008		0.044	−0.009	
	(0.742)	(−1.218)		(0.625)	(−1.094)		(0.516)	(−1.182)	
Firm Ret	0.167***	0.524***		0.168***	0.524***		0.167***	0.525***	
	(9.360)	(20.963)		(9.404)	(21.001)		(8.987)	(21.031)	
Proximity x Firm Ret	0.014	0.111***		0.014	0.111***		0.018	0.111***	
	(0.793)	(5.909)		(0.816)	(5.920)		(0.989)	(5.941)	
Sample	Disc.	No Disc.		Disc.	No Disc.		Disc.	No Disc.	
Fixed Effects	Year-Month, SIC, Peer	Year-Month, SIC, Peer		Year-Month, Firm, Peer	Year-Month, Firm, Peer		Year-Month, Firm-Peer	Year-Month, Firm-Peer	
Observations	72,543	6,221,764		72,543	6,221,764		72,543	6,221,764	
R-Squared	0.165	0.340		0.169	0.340		0.193	0.340	0.338

Table 7: Cross-Sectional Tests: Firm and Peer Sizes

This table presents cross-sectional splits on the relation between $rTSR$ and *Peer Return* on focal firms' disclosure days. Panels differ with respect to the cross-sectional splitting criteria. Panel A partitions the sample at the median based on focal firms' market capitalization; In Panel A's odd-numbered (even-numbered) specifications, we present regression results estimated for smaller-than-median (bigger-than-median) focal firms. Panel B partitions the sample based on whether a peer is bigger or smaller than the focal firm; In Panel B's odd-numbered (even-numbered) specifications, we present regression results estimated for peers that are larger (smaller) than the focal firm. After each specification pair, we present a test of the difference in coefficients on $rTSR$ across the two samples. Within each panel, 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. In all specifications, the dependent variable is *Peer Return*, the RPE peers' daily return. Below each coefficient is a t-statistic, in parentheses, calculated using standard errors clustered by industry and date.

Panel A: Firm Size

	Outcome = Peer Return								
	(1)	(2)	(1)-(2)	(3)	(4)	(3)-(4)	(5)	(6)	(5)-(6)
	Small Firms	Big Firms		Small Firms	Big Firms		Small Firms	Big Firms	
rTSR	−0.275∗∗∗	−0.192∗∗	−0.083	−0.386∗∗∗	−0.236∗∗∗	−0.150	−0.405∗∗∗	−0.263∗∗∗	−0.142
	(−2.996)	(−2.055)	(−0.705)	(−3.501)	(−3.049)	(−1.074)	(−3.223)	(−3.189)	(−0.896)
Firm Ret	0.189∗∗∗	0.140∗∗∗		0.191∗∗∗	0.143∗∗∗		0.192∗∗∗	0.144∗∗∗	
	(5.059)	(6.550)		(5.189)	(6.696)		(5.069)	(6.484)	
rTSR x Firm Ret	0.064	0.014		0.063	0.012		0.062	0.012	
	(1.398)	(0.532)		(1.407)	(0.441)		(1.370)	(0.437)	
Fixed Effects	Year-Month, SIC, Peer	Year-Month, SIC, Peer		Year-Month, Firm, Peer	Year-Month, Firm, Peer		Year-Month, Firm-Peer	Year-Month, Firm-Peer	
Observations	24,053	61,409		24,053	61,409		24,053	61,409	
R-Squared	0.215	0.149		0.232	0.154		0.244	0.179	

Panel B: Relative Peer Size

	Outcome = Peer Return								
	(1)	(2)	(1)-(2)	(3)	(4)	(3)-(4)	(5)	(6)	(5)-(6)
	Bigger Peer	Smaller Peer		Bigger Peer	Smaller Peer		Bigger Peer	Smaller Peer	
rTSR	−0.198**	−0.279*	0.081	−0.268***	−0.280	0.013	−0.305***	−0.326***	0.021
	(−2.562)	(−1.837)	(0.568)	(−4.583)	(−1.491)	(0.071)	(−3.767)	(−2.786)	(0.165)
Firm Ret	0.160***	0.150***		0.162***	0.152***		0.162***	0.156***	
	(7.112)	(4.596)		(7.321)	(4.623)		(7.100)	(4.633)	
rTSR x Firm Ret	0.032	−0.011		0.030	−0.012		0.030	−0.014	
	(1.015)	(−0.334)		(0.972)	(−0.375)		(0.942)	(−0.417)	
Fixed Effects	Year-Month, SIC, Peer	Year-Month, SIC, Peer		Year-Month, Firm, Peer	Year-Month, Firm, Peer		Year-Month, Firm-Peer	Year-Month, Firm-Peer	
Observations	68,743	16,721		68,743	16,721		68,743	16,721	
R-Squared	0.168	0.171		0.178	0.174		0.198	0.193	

Table 8: Cross-Sectional Tests: Industry Relations

This table presents cross-sectional splits on the relation between *rTSR* and *Peer Return* on focal firms' disclosure days. In odd-numbered (even-numbered) specifications, we present regression results estimated for peers that are (are not) members of the focal firm's 1-digit SIC. After each specification pair, we present a test of the difference in coefficients on *rTSR* across the two samples. 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. In all specifications, the dependent variable is *Peer Return*, the RPE peers' daily return. Below each coefficient is a t-statistic, in parentheses, calculated using standard errors clustered by industry and date.

	Outcome = Peer Return								
	(1)	(2)	(1)-(2)	(3)	(4)	(3)-(4)	(5)	(6)	(5)-(6)
Sample	Same SIC1	Dif. SIC1		Same SIC1	Dif. SIC1		Same SIC1	Dif. SIC1	
rTSR	−0.215**	−0.186***	−0.029	−0.326***	−0.213***	−0.113	−0.283***	−0.295***	0.012
	(−2.278)	(−2.677)	(−0.415)	(−3.966)	(−3.471)	(−1.410)	(−2.687)	(−3.410)	(0.128)
Firm Ret	0.176***	0.140***		0.176***	0.145***		0.178***	0.146***	
	(6.475)	(6.842)		(6.592)	(7.064)		(6.405)	(6.877)	
rTSR x Firm Ret	0.023	0.016		0.023	0.013		0.023	0.013	
	(0.697)	(0.561)		(0.678)	(0.464)		(0.655)	(0.442)	
Fixed Effects	Year-Month, SIC, Peer	Year-Month, SIC, Peer		Year-Month, Firm, Peer	Year-Month, Firm, Peer		Year-Month, Firm-Peer	Year-Month, Firm-Peer	
Observations	46,123	39,272		46,123	39,272		46,123	39,272	
R-Squared	0.166	0.171		0.173	0.184		0.196	0.210	

Table 9: Capital Market Cost

This table presents evidence on the relation between focal firm disclosures and trading volumes. In specifications (1) through (3), we use the entire sample and the dependent variable is $log(Peer\ Volume)$; in specifications (4) and (5), collapse the sample to the firm-date-level, and the dependent variable is $log(Volume)$. Specifications further differ with respect to fixed effect structure. Specification (1) includes industry and peer fixed effects; Specification (2) includes firm and peer fixed effects; Specification (3) includes pairwise firm-peer fixed effects; Specification (4) includes industry fixed effects; Specification (5) includes firm 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.

	Outcome = log(Peer Volume)			Outcome = log(Volume)	
VARIABLES	(1)	(2)	(3)	(4)	(5)
Disc. Day x rTSR	−0.006	0.029*	0.031**		
	(−0.409)	(1.870)	(2.027)		
Disc. Day x Any rTSR				−0.121	−0.225***
				(−1.359)	(−3.741)
Disc. Day	0.373***	0.574***	0.557***	0.728***	0.819***
	(4.318)	(5.093)	(5.059)	(9.425)	(17.754)
rTSR	−0.038	−0.065*	−0.077**		
	(−1.539)	(−1.965)	(−2.221)		
Any rTSR				0.138	0.031
				(1.150)	(0.464)
log(Volume)	0.121***	0.280***	0.280***		
	(11.022)	(17.195)	(18.082)		
Disc. Day x log(Volume)	−0.024***	−0.046***	−0.045***		
	(−4.051)	(−5.822)	(−5.864)		
Fixed Effects	Year-Month, SIC, Peer	Year-Month, Firm, Peer	Year-Month, Firm-Peer	Year-Month, SIC	Year-Month, Firm
Observations	7,769,257	7,769,257	7,769,257	550,005	550,005
R-squared	0.868	0.875	0.888	0.420	0.846