# Executive equity-based compensation and tournament incentives

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## Abstract

We find that the losers in CEO promotion tournaments sell their equity holdings profitably to mitigate the reductions in the promotion-based component of their contracts. They avoid selling before losing the contest to maximize their promotion probabilities. Those who are more likely to compete in the tournament and to face a greater forgone tournament prize trade more aggressively. Our results suggest that tournament losers consider their trading opportunities as outside options to compensate themselves *ex-post.* This strategy weakens the relationship between tournament incentives and firm performance and highlights new implications for tournament incentives models, compensation committees, and insider trading regulations.

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## 1. Introduction

Firms hold promotion tournaments to select the most capable senior executives for the CEO positions and reward them with generous compensations, perks, and privileges. According to tournament incentive models, non-CEO managers are willing to accept lower compensation contracts in exchange for the expected value of future promotional prospects[1]. The resulting significant pay gaps among different ranks, referred to as the tournament incentives, encourages middle managers to exert efforts for the board to identify the most suitable senior manager for the CEO position. The increase in the winners' compensation package is possibly the largest in their lifetime[2]. Empirically, firms substitute the weaker promotion-based incentives with higher bonus-based incentives for the job positions with fewer promotion opportunities (Ederhof, 2011), and there is a positive causal relationship between the promotion-based incentives pay gap and firm performance (Kale, Reis, and Venkateswaran, 2009). However, the losers of the promotion contest who remain in their firm are under-compensated for their efforts due to the drastic decline in the expected value of their future promotion and firms do not compensate them for missing the promotion (Chan et al, 2022). The question remains as to whether they will sustain their efforts or recur to equity-based compensation in the form of equity sale to mitigate this opportunity loss.

We hypothesize that non-promoted executives are unlikely to buy equity to make money because they would not mimic the noisy purchases of the newly-appointed CEOs who buy to signal their commitment to improve the firm's performance, but such trades are not informed on average, and can cause overvaluation of the firm, leading to low ensuing long-term abnormal returns

[1] See, Lazear and Rosen (1981); Gibbons and Murphy (1992); Bognanno (2001); DeVaro (2006). Under these models, senior executives endure pay below the optimal market rates because they incorporate the implicit value of future promotion opportunity, which depends on the promotion subjective probability and the subsequent increases in compensation if they eventually win the contest. See Core, Guay and Larcker (2003) for a survey.

[2] For example, Adamson, Canavan and Ziemba (2020) report that CFOs make one-third of CEO pay and have relatively lower compensation increases and a smaller proportion in the form of stocks and LTIPs.

(Armstrong, Blackburne, and Quinn, 2021). Instead, we expect the losers in CEO promotion tournaments to leverage their private information and sell opportunistically their vested equity for personal monetary gain to reflect their discontent (Du et al., 2022), and future decreases in their productivity after being passed over for promotion (Karachiwalla and Park, 2016)[3].

We use a sample of 165,705 insider transactions undertaken by 21,723 US non-CEO executives between 1996 and 2019 to test our hypotheses. We find that 68% of non-promoted managers remain in their firm at least two years after losing the tournament. Their sale of equity when they lose the tournament represents 92% of their total trades, significantly higher than the overall proportion of sell trades unconditional on our event window of 65%. We find that they sell only after losing the contest because their winning probabilities would be adversely affected if they sell on their private negative information before the tournament. We show that a one opportunistic post-tournament sell trade lowers their unconditional probability of winning the CEO tournament by 5.2%. These results suggest that tournament losers consider the trading opportunity as an outside option to compensate themselves for missing the CEO promotion. Their semi-annual average dollar abnormal profit accounts for 20% of their salary and is significantly more profitable than that of their peers who left the firm. Their loss-averting sell trades in both exogenous and involuntary CEO turnover events persist two years after losing their CEO promotion. We find that the losing tournament effect is stronger for firms without a formal CEO succession plan (Cvijanovic, Gantchev and Li, 2022), as these firms are more likely to hold open CEO tournament.

[3] Insiders buy to seek profit but sell for several other reasons (Lakonishok and Lee, 2001; Cohen, Malloy, and Pomorski, 2012). Their purchases would be profitable if they signal their confidence in the new leadership and their firm's prospects, their willingness to focus on future promotion opportunities and to sustain their efforts to strengthen their case for promotion (Campbell 2008, Chan 2018), and if the disincentive from promotion rejection will not fully dilute the benefit of learning from ex-ante promotion incentives (Campbell 2008; Du et al., 2022). In behavior literature, there is evidence that people respond negatively to the success of others, supporting the argument that tournament losers will trade to compensate themselves for their loss (see Bikmetova, Davidson, and Pirinsky (2023) for a survey and evidence of profitable trades when insiders' peers are on the Forbes 400 list of wealthiest Americans).

We further relax the assumption in the existing tournament incentives literature that all highest paid directors are equally likely to compete in a CEO tournament and investigate which group of tournament losers are more likely to make informed transactions after CEO promotion by exploring the heterogeneities amongst the directors. We select tournament competitors based on their age, tenure, career horizon and subjective probability of being promoted to test for the positive relationship between the likelihood of insiders competing in the CEO tournament, and their probability of selling equity on negative information more aggressively for personal gains as their forgone CEO promotion is more valuable. We show that the trading profitability is higher for younger insiders because they have a higher expected value on the promotion-based components in their remuneration contracts as their career horizons are longer. We report that insiders who have long tenure but never won a CEO tournament trade with lower aggressiveness because they are unlikely to win any future CEO tournament, same as short career investment horizon insiders because they have shorter career prospects (Akbas, Jiang and Koch, 2020). However, non-promoted insiders with a higher probability of becoming CEO *ex-ante* trade aggressively on their private information *ex-post* because the expected value of their implicit promotion-based incentives is high. The results for their purchase transactions are, on average, insignificant implying that they do not buy to compensate themselves for the forgone promotion opportunity.

We recognize that executives may have several opportunities to extract private benefits. We first rule out four alternative hypotheses: trading on the change in stock price informativeness, staying with the firm and competing for other positions, trading on the higher stock volatility, and trading on the poorer CEO skills. We then argue that if the tournament losers truly trade to compensate themselves, then those with profitable trades are less likely to leave the firm, consistent with our story that insider trading allows executives to realign their compensation level

closer to their optimal effort level. We find that tournament losers with larger promotion-based incentives pay gap, trade more aggressively because of the higher opportunity loss, while those who receive a larger retention bonus after losing the tournament trade on their private information less aggressively as their forgone incentives is lowered, in line with Armstrong et al. (2021). We also consider that tournament losers may stay after CEO turnover to target a job at the same level of seniority in another larger firm if outside job prospects are imperfect substitutes for their CEO forgone promotion opportunities. We find that tournament losers trade with greater aggressiveness if the industry tournament incentives are lower or the firm discloses a non-compete agreement.

We then investigate the informational content behind these trades. We show that unobservable stock and market movement do not randomly drive their trading profits. Their firms strategically release more discretionary news in the month they sell shares, and this news enhances the stock return predictability. They trade on the future decreases in both return on asset and investor sentiment, and their firm's future underperformance, but this is not the case for their relatively rare purchases. Our results suggest that they exert less effort and trade on the worsening in future firm performance after losing the tournament for personal gains. We also find that the historical average insider trading profitability and the board conservatism can predict the scale of post-tournament turnover among non-promoted insiders, implying that they assess the profits they can generate from their future informed trading before deciding to leave or stay in the firm.

We examine the alternative hypothesis that tournament losers revert to their normal trading profitability after losing the promotion which may be more informed than their trades prior to knowing the tournament outcome. We employ a two-stage least square (2SLS) estimator to generalize the results and assess whether the increase in their equity sales profitability is significantly higher than their unconditional return predictabilities. We show that the increase in

the return predictability embedded in trades after the CEO turnover persists even two years after losing the CEO promotion opportunity. Their sell trades are more profitable when the newly appointed CEOs increase their holdings. We question why their buy trades, which involve relatively lower litigation risk (Dai, Fu, Kang, and Lee, 2016), are rare. Inspired by the findings of Armstrong et al. (2021) that newly appointed CEOs are noisy traders on average as they trade to prolong their contract not necessarily to generate profits. We find that non-promoted insiders sell against the purchases of newly appointed CEO, which result in short-term inflated stock prices but lower long-term returns. They also dissimulate their private information by making sequential equity sales and randomly mixing with uninformative vesting of equity to thwart outsiders and market regulators, strengthening the losing CEO competition effect.

One main concern in the insider trading literature is endogeneity, as the true motivations behind insiders' trades are not directly observable, leading to random post-trades' returns. Although we do not claim a causality, we base our results on three approaches to mitigate this problem. First, we specify a stacked diff-in-diff regression based on matched sample to isolate the losing CEO tournament effect. We follow Angrist and Pischke (2009), Cengiz et al. (2019), and Baker, Larcker and Wang (2021) to conduct an event-study type diff-in-diff regression to show the parallel trend assumption. Our results are stronger if we only use exogenous CEO turnover that cannot be predicted. Second, we use the age of former CEO as instrumental variable (IV) to further generalize the finding outside our event window and assess the profitability that is solely attributed to CEO tournament. The former CEO's age is a publicly available information, not correlated with the firm's future fundamental that insiders are exploiting because former CEO has left the firm on average six years ago, but it empirically embeds predictive power for the future CEO turnover. We test the exclusion restriction of our IV by showing that former CEO's age contains little predictive

power for non-CEO trading profitability outside the tournament event, further stressing the exclusion restriction plausibility.

Third, we acknowledge that the above two methods are imperfect to tackle the underlying endogeneity. If our story is not driven by an unobservable endogeneity, the positive causal effect between the tournament incentives and firm performance documented by Kale et al. (2009) is overestimated since insiders have outside options to trade on their private information to mitigate their forgone compensation incentives. We replicate their results to find persistence in the positive causal relationship between tournament incentives and firm performance in our sample period. However, when we follow Kim and Lu (2011) and use the sum of the maximum marginal federal and state long-term capital gain tax rates as our IV for the total non-promoted insider transactions, to mitigates endogeneity, the causal relationship becomes weaker when non-CEO insiders execute more trades, confirming our hypothesis that non-promoted insiders sell equity to signal their discontent after losing the promotion, and our previous results are not driven by endogeneity.

Our results are robust when we do not use diff-in-diff specification with matched sample, and when we use different return proxies and holding horizons, control for performance-induced CEO turnover, include additional control variables, and 10b5-1 trades, and exclude tournament competitors that are not the top two highest paid non-CEO managers in the firm or older than 60, firms that retain former CEOs, firms that promote outsider as CEO, firms with a COO prior to the tournament, and CFO trades. We construct pseudo-CEO turnovers to test for robustness of our diff-in-diff regressions and conduct 1,000 placebo tests for diff-in-diff and 2SLS regressions separately to rule out the possibility that our significant results are due to luck.

We contribute to the literature by focusing on equity-based compensation and tournament incentives, which established that firms hold open CEO promotion tournaments by making top

employees compete for an increase in compensation, a single senior position promotion-based prize (DeVaro, 2006; Kale et al, 2009)[4]. However, since firms do not alter their contracts because of adjustment costs (Chan et al, 2022), the non-promoted executives suffer a drastic decrease in the value of their contracts as the value of their implicit promotion-based incentives drops, they exploit aggressively their private information without attracting regulators' attention (Ali and Hirshleifer, 2017).[5] This opportunity to sell equity weakens the positive effect of tournament incentives on firm performance documented by Kale et al. (2009). Finally, we document a novel information-based profitable insiders' trading strategy that result in post-trade return predictabilities (Lakonishok and Lee, 2001; Cohen, et al., 2012; Biggerstaff, Cicero and Wintoki, 2020). We find that insiders adjust their equity sales not for rebalancing objectives, liquidity needs, uncertainty over market outlook, and their firm reaching a relative stability period (Cohen, et al., 2012), but to suit their career concerns and their forgone pay rise after promotion outcome, an unexplored area in previous literature, in addition to the previously documented complementing explicit forms of compensation (Roulstone, 2003) or missing performance-based bonuses (Gao, 2019). Since our insiders do not seem to be trading illegally around news releases and not likely to be subject to insider trading regulation and enforcement which hamper price informativeness (Kacperczyk and Pagnotta, 2024), our results suggest that their trades increase price discovery.

The remainder of the paper proceeds as follows. Section 2 describes the data and the methodology. Section 3 presents the empirical results. Section 4 presents the IV results, robustness, and the placebo tests. The conclusions are in Section 5.

[4] Cvijanovic *et al.* (2022) show that 83.6% of S&P 1500 firms do not have a formal CEO succession plan and hold open CEO tournaments for competition.

[5] In conventional insider trading models, informed agents' trading aggressiveness $\alpha$ is increasing in their risk tolerance (Cespa, 2008), which becomes higher as their overall compensation decreases and the expected loss of losing their job is lower if regulators prosecute them for illegal insider trading. Thus, we hypothesize that non-promoted managers will tolerate higher litigation risk and trade on their private information more aggressively.

## 2. Sample and Variable Construction

We follow prior literature (Kale et al., 2009; Kini and Williams, 2012) to identify CEO turnover event and collect manager's compensation data from Execucomp, which covers S&P 1500 firms from 1996 to 2019, with the first CEO turnover event occurring in 1997. Our initial sample consists of 269,456 manager-year observations with 4,838 CEO turnover events. Our event window is (-2, 1) relative to CEO turnover year 0, as we assume that the tournament begins in year -2, and the losing tournament effect will gradually decay outside our event window. We also restrict that there is only one CEO turnover in the event window to remove confounding event.

We define tournament competitors as those covered by Execucomp but are not CEOs in their firms, as in Kale et al. (2009) and Kini and Williams (2012). These filters select tournament competitors relatively properly because Execucomp mainly covers the top five highest-paid managers in a firm; their only promotion destination is the CEO position. We reckon the total compensation package that managers receive better measures their seniority within the firm than their job title. We exclude insiders who are not covered by Execucomp in years (-2, -1) but gained coverage in years (0, 1) from the tournament competitor category, as they are either new joiner or low-rank non-participating insiders in the CEO tournament. We also omit ex-CEOs and founders who remain in the firm after stepping down from their position, like Microsoft's Bill Gates, but have lower probability and fewer incentives to become the next CEO. The median (mean) number of tournament contestants is 4 (3.8). We use CRSP to extract stock prices and holding period returns Compustat for accounting and financial data. We exclude non-common shares (*shrcd* not 10 or 11) and stocks under $2 at the start of a calendar year.

We compile all U.S. insider transactions from January 1996 to August 2019 from Thomson Insider Filling (TR). We keep all insiders' open market transactions in Form 4. We exclude

problematic trades with cleanse code A or S, and trades with less than 100 shares, in line with previous studies (Lakonishok and Lee, 2001; Cohen et al., 2012), and any 10b5-1 pre-scheduled trades, as their information content is likely to be trivial, but include them in robustness tests since Larcker et al. (2021), Franco and Urcan (2022) and Fich, Parrino and Tran (2023) find that insiders exploit them. We aggregate insider transactions at the insider-day level. We measure the direction of the trades by computing the net purchasing value (*NPV*) as the dollar value of the buy trades minus that of the sell trades over the total dollar value[6].

We match Execucomp's unique manager identifier *execid* to TR's unique legal entity identifier *personid*. We use BoardEx to crosscheck the validity of our *execid-personid* match. We match 43,952 of the 48,429 distinct *execid* in Execucomp (90.8%) with 44,187 *personid.* We match 42,358 of 46,720 (90.7%) distinct *execid* for non-CEO managers. We discard the unmatched *execid* from our sample, as they have not reported any transactions on Form 4. After removing 29% cases with confounding events, we construct a sample of 3,428 firms with CEO turnover events, out of which 2,636 (77%) had internal promotions, close to the 72% reported by Cziraki and Jenter (2022). We find 1,259 (37%) firms did not report any insider trades in year 0, leaving 2,169 firms in our final sample. Our insider trading sample starts with 269,456 events. After various filters, our final sample includes 13,062 (94%) sell and 832 (6%) buy trades during the event window (0, 1), where year 0 is when the tournament occurred [7].

[6] We find similar results using net number of shares (Lakonishok and Lee, 2001). We exclude 0.3% cases with 0 NPV.
[7] We report in Internet Appendix S1 the matching method, in Internet Appendices S2 and S3 the details of our samples and the annual distribution of CEO turnover, and in Internet Appendices S4 the summary statistics of CEO and other directors who are not defined as tournament contenders. Our results are robust when the event window is extended to (-3,3), narrowed to (-1,1), restricted to cases with only one turnover in (-4,2), includes all confounding events and the three types of non-CEO managers we excluded, or we only keep the top two highest paid non-CEO executives. We do not restrict other event years than the turnover year in the event window of other CEO turnover events as this effectively implies one turnover in ten years. In unreported results, we employ the trades reported by insiders not covered by Execucomp, we cannot observe the same increase in trading profitability for both the buy and sell samples.

We compute the buy-and-hold (BHAR) abnormal return for trade *i* in period *t* as follows:

$$BHAR_{s,t} = \prod_{s=1}^{t}(1 + \text{return}_{s,t+i}) - \prod_{s=1}^{t}(1 + \text{mkt}_{t+i}) \quad (1)$$

where $\text{return}_{s,t+i}$ is the stock's *s* holding period return, and $\text{mkt}_{t+i}$ is the value weighted CRSP index. We measure BHAR one day after insider trade date to 180-calendar day holding period as "short-swing profit" rule in Section 16(b) of the 1934 Security Act prohibits insiders from profiting from short-term price movements. All our results remain robust using the 365 trading days holding period. Our sample size varies depending on the availability of the *execid-personid* link table and our control variables. The comparative analysis of subsequent insider trading profitability across these two samples can better disentangle the incremental change solely attributable to the loss of CEO turnover within our event window. We estimate the following diff-in-diff regression to test whether the return predictability of insider buy (sell) trades remains the same or increases (decreases) in and/or after the CEO events by focusing on our event window only:

$$\text{BHAR\_m\_180}_{i,t} = \alpha + \beta_1 \text{Post}_{i,t} + \beta_2 \text{Treat}_{i,t} + \beta_3 \text{Post}\times\text{Treat}_{i,t} + \beta_4 \text{CEO\_IT}_{j,t} + X_{i,t} + \gamma + \rho + u_{i,t} \quad (2)$$

where $Treat_{i,t}$, is equal to one for insider transaction *i* in our treated firms, and $Post_t$, is equal to one for two years (0, 1) post-CEO tournament outcome, depending on the specific focus period. These variables are not subsumed in our regression specification as no control firms had never changed CEO. We expect $\beta_3$ to be positive if the vesting of equity is profitable and negative if the sale of equity is loss-avoiding. We include $X_{it}$ for controls, $\gamma$ for month and $\rho$ for firm fixed effects.

To control for insider characteristics, we include CEO trading direction, promotion of an outsider CEO, succession planning prior to CEO turnover, tournament incentives following Coles,

Daniel and Naveen (2006) detailed in Internet Appendix S1, and high incentive managers[8]. At firm-level, we control for firm's recent and long-term stock price momentum, growth, profitability, size, innovation level using last year research and development cost, the Amihud (2002) illiquidity measure, financial analyst coverage, firm's financial health and Core and Guay (2002) and Coles *et al.* (2006) Delta and Vega. Appendix A reports the details of our variables.

We cluster our standard errors at firm-month level as insiders group their trades with colleagues (Alldredge and Blank, 2019). We match the time dimension of the control variables on the insider trade date instead of CEO turnover event.[9] We match our test firms with control firms with no CEO turnover in (-2, 2) and shortest Mahalanobis distance on the average insider buy/sell profitability, logarithm of the total asset, and the book-to-market ratio in the year *t-1*. We match each treated firm with one control firm to mitigate the biasedness. We matched 192 out of 547 (35%) firm-year observations with at least one insider buy, and 1,331 of 1,775 (75%) with at least one insider sell trade in the tournament year [10]. We provide in Internet Appendix S5 detailed results that indicate that our matching strategy is successful, supporting the parallel trend assumption between control and treated firms. The BHARs for sell trades, reported in Panel B, decrease from positive to -0.01 and -0.06 in year 0 and +1, respectively, indicating tournament losers make loss-avoiding sell transactions for personal gains. We display the graphs of parallel trend in Figure S15.

Table 1 shows the summary statistics of insiders and firm characteristics across their buy (Panel A) and their sell (Panel B) trades. As expected, their net equity sales of 13,062 trades

[8] In some rare cases, non-CEO managers are paid more than CEO, such as Bill Gates (*execid*: 00635) relative to Steven Ballmer. We restrict the difference in total compensation to zero. Our results are robust with or without those outliers.
[9] Our results are robust if we match the time dimensions of these control variables by using the end of last month figure in the last fiscal year, and if we include both the one-fiscal year and one-month lagged control variables.
[10] Many firms do not report insider purchase transactions in years (-2, -1). We find that matching on year -1 insider trading profitability yields the most suitable results. Our results are robust if we do not use diff-in-diff specification with a matched sample.

represent 94 per cent of their total 13,984 net trades, and 73 percent and 85 per cent of the number and value of shares traded, respectively. They tend to sell in larger firms and those with high pay gap, total compensation, ROA, and sell-side analyst coverage, and in more liquid firms, but a lower proportion of outside directors. They are more likely to be contrarians as they sell when the long-term and short-term momentum stock returns, as proxied by *mom* or *ret30*, are higher and book to market is lower, in line with previous evidence (Lakonishok and Lee, 2001; Cohen et al., 2012). Internet Appendix S6 reports the full set of summary statistics for buy and sell samples.

[Insert Table 1 here]

## 3. Empirical Results

### *3.1 Insider trading propensity around CEO tournament*

For insider trading to be considered as an outside option to tournament incentives, there must be a cost to exercise the option earlier prior to the tournament outcome. Otherwise, corporate insiders will unconditionally maximize their trading profitability by making informed transactions before the tournament. We first investigate why tournament losers do not maximize their trading profitability unconditionally and must wait for the tournament outcome. Cziraki et al. (2021) and Suk and Wang (2021) show that the insider purchase (sell) activity is a credible positive (negative) signal to future firm performance, and the board and outside investors will examine their trading activities to gauge their confidence in improving their future firm performance. Consequently, we conjucture that if they execute a large number of opportunistic sell (buy) trades, their probability of winning the CEO promotion is lower (higher).

We follow Cohen et al. (2012) to classify insiders into opportunistic or routine traders before the CEO turnover. We focus on CEO turnover in year (0, 0) and estimate a logit model and

a linear probability model with firm and year fixed effects at insider-firm level. The dependent variable is a dummy variable equal to one for newly promoted CEO, and zero for other non-promoted insiders. The explanatory variables of interest are the number of buy (*#_buy)* and sell (*#_sell*) trades in column (1) and (2) and the number of opportunistic buy and sell trades in column (3) and (4). The regression results in Table 2 Panel A show that insiders with more buy (sell) trades are more (less) likely to win the CEO competition, these results become stronger if we only consider opportunistic transactions. If we include all transactions in year -1 and year -2, the results for sell trades remain robust, but there is no significant signaling effect for the opportunistic buy trades in column (4). The results in column (3) show that every opportunistic sell transaction is associated with 1.3% lower CEO promotion probability, 5.2% lower than the 25% unconditional probability of becoming CEO.

Next, we assess whether non-promoted insiders are more likely to execute opportunistic transactions after losing the CEO tournament. We re-estimate Panel A using firms with CEO turnover event. The dependent variable $opp\_D_{i,t}$ is a dummy equal one for opportunistic trades *i* and zero for routine ones. Columns (1) to (4), Panel B, show that the coefficient of the $Post_{i,t}$ is positive and statistically significant, suggesting that non-promoted insiders are more likely to make opportunistic purchase and sell transactions after losing the tournament. In contrast, the coefficients of $CEO\_IT_{j,t}$ in columns (1) to (2) for buy trades are positive and significant, in columns (3) to (4) for the sell trades are negative and significant. This suggests that non-promoted executives decrease (increase) their propensity to buy (sell) opportunistically in year (0, 1), and they do so if the newly appointed CEO is selling (buying) more. The coefficients of the control variables, not report, are consistent with the existing literature (e.g., Lakonishok and Lee, 2001).

Overall, our results suggest that insiders are more likely to make opportunistic sell transactions after losing the CEO competition, which are more informative than an average sell trade suggested by Cohen et al. (2012). In unreported logit regressions, we find that they are more likely to sell, not buy, opportunistically after losing their promotion, suggesting that they incorporate private information into their sell trades to compensate themselves for losing the CEO competition. These results indicate also that non-promoted insiders strategically time their transactions based on the trading activity of the newly appointed CEO.

[Insert Table 2 here]

*3.2 Diff-in-Diff regression results*

Table 3, Panel A, shows the diff-in-diff results using both opportunistic and routine transactions. The interaction term (Treat×Post) is statistically significant, implying that insiders' buy trades after losing their CEO tournament yield 8.5% higher profits that those generated without CEO turnover, *ceteris paribus*. The negative and significant $(\text{Treat}\times\text{Post})_{i,t}$ in Column (5) to (6) indicate that the sell trades in treated firms are systematically more loss averting of between 2.0% in years (0, 0) and 3.9% in year (1, 1), than those of the control firms. Using the average sell value in year 0 and year 1, insiders' sell trades with 180 holding period would yield $18,864 ($37,030) more profit if their trades are made in the year 0 (year 1) than other non-CEO managers, accounting for about 6.5% of their average salary in year 0 and 1, higher than the $12,000 reported by Cziraki and Gider (2021) using 365-day holding period between 1986 and 2013. Since, on average, each executes 3 sell trades per year, their yearly total abnormal profit accounts for 20% of their salary in these two years, and 33% if they release their capital gains in one calendar year. They also mainly sell to compensate themselves because the losing tournament effect persists until year +1 in the insider sell sample, while for the insider purchase sample, the effect solely exists in the year

of CEO turnover. The significance of our control variables is consistent with previous insider trading studies (e.g., Cohen et al., 2012).

We recognize that not all firms may hold CEO promotion tournaments. We follow Cvijanović et al. (2022) to search all proxy statements to identify firms with CEO succession plan prior to CEO turnover. We exclude 20.8% sample that report a formal CEO succession plan, comparable with the 16.4% reported in Cvijanović et al. (2022). Panel B shows stronger loss-averting sell trades of 2.6% and 5.5% in years (0, 0) and (1, 1), respectively.

We further consider the possibility that the motives for the CEO turnover will affect the informed insider trading behavior. We follow Gentry et al. (2021) and split the treated sample and its nearest-neighbor control firm into exogenous and involuntary CEO turnover events. We define exogenous CEO turnover events are those in which CEO was replaced because of sudden death, illness and personal behavioral issue. CEO was dismissed immediately in these events and did not have an opportunity to resign (Gentry et al. 2021). In these exogenous CEO events, we can rule out anticipated endogenous trading behavior in the pre-period. We follow the classification of Gentry et al. (2021) to identify involuntary CEO turnover. We estimate the diff-in-diff regressions. Table 3, Panel C and Panel D, indicate that informed equity sales are common in both exogenous and involuntary CEO turnover events, and the sell transaction profitability behind exogenous CEO turnover is much lower than our baseline regression, further supporting our conclusion that non-promoted directors are more likely to sell on their private negative information for personal gains.

[Insert Table 3 here]

### *3.3 Likelihood of participating in tournaments and insider trading profitability*

The tournament incentives literature assumes that all high-rank non-CEO directors are equally likely to participate in the CEO tournament (Kale et al., 2009; Kini and Williams, 2012).

However, Chan et al. (2022) show that the losers in the first tournament are unlikely to win any future tournament in the same firm, suggesting that some are less likely to be tournament contenders. While there is no established method to identify non-competing insiders, we use insider characteristics as proxies for their likelihood of competing. We hypothesize that those who are more likely to compete in the tournament *ex-ante*, will trade on their private information with greater aggressiveness *ex-post,* and yield higher post-transaction returns. We triple-interact $(\text{Post}\times\text{Treat})_{i,t}$ with our moderator variable and include all main levels. For brevity, we only report the coefficients of $(\text{Post}\times\text{Treat})_{i,t}$ and the triple-interaction term in Table 4.

Gibbons and Murphy (1992) and Chan et al. (2022) show that managers close to their retirement age have lower *ex-post* expectation discrepancy and will place less importance on the promotion-based incentives. Consequently, we hypothesize that older managers are less likely to compete for the CEO position, i.e., the subsequent changes in their profitability will be less dramatic than that of younger managers. To test this hypothesis, we employ the natural logarithm of the current age of managers as the moderator variable. Table 4, Panel A shows that the coefficient of $(Post\times Treat\times lnage)_{i,t}$ is negative and significant in buy sample, and positive and significant in sell sample, in line with our previous findings that older managers will trade on their private information to show their discontent for the forgone promotion-based incentives with higher aggressiveness. They did not place much implicit value on their future promotion opportunities because their career horizons are shorter. In the same vein, Chan et al. (2022) show that executives who stay longer in a firm are less likely to be competing in subsequent CEO tournaments because more competent non-CEO executives are more likely to leave the firm after losing the first tournament. We employ the natural logarithm of the current tenure of managers as the moderator variable. Panel B shows that the coefficients of the $(Post\times Treat\times lntenure)_{i,t}$ are all

statistically significant and negative for the buy sample, but positive and significant in sell sample. These results are in line with our hypothesis that the effect is stronger for non-promoted executives who are more likely to compete in the tournament.

We then employ insiders' personal investment horizons to proxy for their career horizons. Akbas et al. (2020) show that short horizon (SH) insider sellers frequently reverse their previous buy positions to avoid overconcentration of their personal portfolios in their firms. These insiders have shorter career horizons in their firms and are less likely to compete in the CEO tournament. We modify the investment horizon measure proposed by Akbas et al. (2020) to identify SH sellers, as detailed in Internet Appendix S1. We find only 2.3% (9.2%) of our buy (sell) trades were short-horizon insider sellers, suggesting that SH sellers are less likely to trade after they have lost the tournament. We create short-horizon dummy variable $SHD_{p,t}$ equals to one for SH insiders, and zero otherwise. Panel C shows that the coefficient of $(Post \times Treat \times SHD)_{i,t}$ is significantly positive in sell sample, suggesting that insiders with shorter career horizon will trade on their private information with lower aggressiveness.

Lastly, we compute the subjective probability of insiders becoming CEO, $Probability_{p,t-1}$, by estimating a cross-section regression using only firms that had a CEO turnover in the year *t* and employ the probability in year *t-1* as the moderator, as detailed in Internet Appendix S1. We assess that tournament contenders in our focal firms will only use public information to compare themselves with other tournament winners to compute the subject probability of winning CEO promotion in their own firms, in line with Kale et al. (2009). We intentionally use public information only to estimate these coefficients because tournament contenders in our focal firms will not have access to the private information that the board of directors in the other CEO turnover firms possessed at the time of CEO turnover. We include firm level variables to estimate the

probability of becoming CEO because the attractiveness of the CEO position also depends on the operating condition of the firm. The results in Panel D imply that non-promoted executives with higher subjective probability of becoming CEO *ex ante* exploit their private negative information more aggressively in their sell trades *ex post*. There is no significant effect for insider purchase transactions. Overall, these results suggest that non-CEO directors who are more likely to contest the CEO position will trade on their private negative information to compensate themselves for the forgone promotion incentives.

[Insert Table 4 here]

### *3.4 Motivations behind more informed insider transactions*

We recognize that CEO turnover is an information-intensive corporate event, and insider trading is expected to be more intensive and more informed during the period. The previous diff-in-diff setting is insufficient to show that they are trading on their forgone promotion opportunity rather than other unobservable factors. In this section, we assess whether insiders intentionally trade to compensate themselves for the forgone CEO promotion by exploring their heterogeneities.

We first employ the tournament prize. We expect a stronger increase (decrease) in trading profits by insiders whose tournament prizes are larger and who stand to lose more from missing the promotion. We re-estimate our diff-in-diff regression with a triple interaction term $(Post \times Treat \times Pay\_rank)_{i,t}$, which we expect to be positive for insiders' equity sell, if managers with high tournament prizes show a higher discontent because of their high *ex-post* expectation discrepancy, after missing the promotion by trading with greater intensity than other insiders. In Table 5, Panel A, $(Post \times Treat \times Pay\_rank)_{i,t}$ is statistically insignificant in the buy trade sample, but positive and significant in the sell trade sample. This suggests that non-promoted insiders who have forgone larger promotion prizes are more dissatisfied after being passed over for promotion;

they sell on negative private information more aggressively. Next, we consider the possibility that the board will retain executives by awarding them a large retention bonus to compensate them for their forgone incentives (Armstrong et al., 2021), making them trade less aggressively on their private information. We create the dummy variable $BA_{p,t-1}$ equals to one if the change in a manager *p*'s bonus is higher than the sample median among all managers in the same firms in the same year, otherwise zero. Panel B shows that non-promoted executives with larger bonus increases exploit their private negative information less aggressively in their sell trades. Panel C shows that non-promoted directors are less likely to trade on their private negative information when the board is conservative, using Khan and Watts (2009) *C_quintj,t*, the quintile number board conservatism, for all firms in the same industry in each year.

In addition to these intra-firm heterogeneities, we focus on outside job opportunities for insiders, which we expect to play a significant role in determining their post-transaction trading profitability if they trade to compensate for the forgone promotion. We hypothesize that those in industries with greater industry tournament incentives, and, thus, lower decreases in the implicit component in their compensation contracts, given alternative job opportunities, will trade on their private information with less aggressiveness than those with limited outside career options. As in Coles et al. (2018), we construct $ind\_incern_{j,t}$, as outlined in Internet Appendix S1. The results in Panel D show that the coefficients of the interaction term are significantly positive for insider sell trades, indicating a better outside career option will suppress informed insider trading activities, further reaffirming that the forgone CEO promotion opportunity motivates insiders to trade. Lastly, since tournament losers from firms with non-compete agreement have limited outside job opportunity, they will trade on their private negative information more aggressively because their

forgone CEO promotion incentives is larger[11]. We follow Mueller (2023) to scrape 10-K and 10-Q files from EDGAR. We then create a dummy variable $NoncomD_{j,t}$ equals to one if a firm mentioned non-compete agreement in the year *t*, zero otherwise. The results in Panel D show that the coefficients of the $(Post \times Treat \times NoncomD)_{i,t}$ are all negative and significant in sell sample, in line with our hypothesis. If insiders are trading on unobservable factors other than losing CEO promotion, their trading profitability should not vary with these moderating effects[12].

[Insert Table 5 here]

### *3.5 Ruling out alternative explanations*

Although we have documented that the main motivation behind these more informed sell transactions is forgone CEO incentives, we rule out four alternative hypotheses by exploring firm-level heterogeneity[13]. The first alternative hypothesis is that the increase in insider sell transactions is due to the change in stock informativeness. We use the Tucker and Zarowin's (2006) Future Earnings Response Coefficient (FERC) and Piotroski and Roulstone's (2004) return synchronicity to proxy for stock informativeness. In Internet Appendix S8, Panel A and Panel B, we find that insiders' trading profitability does not depend on the future earnings information and the level of firm-specific information, ruling out the alternative explanations that tournament losers are trading on the change in information environments for personal gains.

[11] In un-tabulated results, we find the presence of non-compete agreements at the firm level will reduce the propensity of tournament losers existing the firm after a tournament.

[12] More detailed results are in Internet Appendix S7 where we also include litigation risk industries as moderator variable.

[13] We report the remaining results in this section in Internet Appendix S8 and the details of the construction of the variables $C\_quint_{j,t}$, $riskD_{j,t}$, $FERC_{i,t}$, and $Synch_{j,t}$, used below, in Internet Appendix S1. In unreported results, we did not find the implementation of the Sarbanes–Oxley Act in 2002 nor the implementation of Say-on-Pay law in 2011 to have a significant impact on the non-promoted executives' profits.

We recognize that the non-promoted managers may stay with the firm after losing the CEO competition because they target other higher-ranking positions within the firm with an attractive increase in the salary, which mitigates their incentives to respond negatively to a promotion pass-over by offloading their holdings using their private information. This possibility is trivial because Execucomp mainly reports the top four highest-paid managers whose career path is already at the top of the corporate hierarchy, and any increase in their compensation package will not be as significant as the CEO promotion reward. To rule out this possibility, we focus on isolated CEO promotion from years 0 to 7. We estimate new regressions using one or two-years change in the natural logarithm value of the total compensation as the dependent variable, and insider, firm, and year fixed effects, and the previously stated control variables. We focus on a dummy variable that equals to one for year (0, 4) and zero otherwise. We find, but not report, no significant change in the total compensation of non-promoted executives in both first and second year after losing the CEO promotion, in line with Kale et al. (2009) and Chan et al. (2022). These results suggest that the non-promoted executives are not compensated for the dimmer career prospects.

Third, Kotter and Larkin (2024) demonstrate that the presence of non-CEO directors on the board allows for more effective identification of CEOs with superior managerial talent, indicating that these directors possess an informational advantage over outside directors regarding CEO capabilities. Therefore, the increase in insider sell transaction profitability is due to treated firms appointing new CEOs with skills below the control firms, and, therefore, non-promoted directors are more likely to yield higher loss-averting sell transactions. We employ the CEO skill measure of Daniel et al. (2020) which we include as a moderator. The results in Internet Appendix S7, Panel C show that although $skill_{j,t}$ is, as expected positive and significant for the sell sample, the inclusion of the proxy does not eliminate the significantly negative coefficients of

$(Treat \times Post)_{i,t.}$, suggesting that new CEO's skill is unlikely to explain the increase in the profitability of loss-averting sell trades.

Fourth, Cziraki and Groen-Xu (2020) show that firms systematically have higher return volatility after changing CEOs. Insider trades may be more profitable and more frequent because volatility is temporarily higher, increasing insiders' informational advantage. We further rule out the alternative story by solely focusing on firms that have low volatility after CEO changes. We select firms with the lower-than-median return volatility in its 2-digit SIC industry in event year 0 and re-estimate the diff-in-diff regression by focusing on these firms and their nearest neighbors. In Panel D, we show that our baseline results remain robust and significant, indicating that the return volatility is unlikely to be the main driver for informed trading behavior. Overall, these results suggest that the increase in insider sell transactions is not due to changes in stock informativeness, but, more likely, to compensate themselves for the forgone CEO promotion.

### *3.6 Informational content embedded in insider transactions*

We examine the information content of the non-promoted executives trading strategy after losing the CEO competitions to confirm that the unobservable firm characteristics do not drive their more informed transactions. The loss of a promotion opportunity will lower their total compensation packages to a suboptimal level for their efforts. Although they will trade to compensate themselves, these more informed transactions cannot fully adjust their packages to the optimal level, otherwise they would not have enough incentives to compete in the tournament *ex-ante*. Therefore, they will exert less effort and their sell trades will predict a worsening in their firm's future performance. We focus on three non-mutually exclusive possibilities: insiders may

trade on the future changes in the operating performance, and in the cost of capital, and/or exploit the change in investor sentiments[14].

We compute changes in ROA from year *t* to *t+2*, ΔROA, with year *t*, the insider transaction year, to estimate future operating performance changes[15]. To measure the changes in investor sentiment, ΔSentiment, we compute the market-to-book ratio decomposition of Rhodes–Kropf, Robinson and Viswanathan (2005). Cziraki, Lyandres and Michaely (2021) argue that the method can separate the firm-specific from the industry-level sentiment. It is appealing to insider trading studies because insiders are more likely to possess private information on the former than on the latter (Wang, 2019). We follow Cziraki et al. (2021) to measure changes in sentiment from *t-1* to *t+1*, $\Delta\text{Sentiment}_{t-1,t+1}$. To measure the change of cost of capital, $\Delta r_{t,t+2}$, we follow Cziraki et al. (2021) and estimate a modified Fama and French (1993) three-factor model.

[Insert Table 6 here]

Table 6 shows the diff-in-diff results using these three proxies as dependent variables. The coefficients of $(\text{Post}\times\text{Treat})_{i,t}$ are mainly significant for the sell transactions in columns (3) and (4). Panel A reports that insider sell transaction can significantly predict a 1.7%, and 1% decrease in $\Delta ROA_{t,t+2}$ in year 0 and 1, respectively. Similarly, in Panel B, insider sell trades predict 4.7% and 6.3% in years 0 and +1, respectively, future decrease in investor sentiment, $\Delta Sentiment_{t-1,t+1}$, while insider purchases increase investment sentiment by 19.3% and 57.1%. Moreover, in Panel C insider purchases do not predict the future decreases in the cost of capital, $\Delta r_{t,t+2}$, in year 0, but

[14] In unreported results, we confirm that the parallel trend assumption is satisfied using the same control firm sample.
[15] The details of the constructions of our variables are in Appendix A and Internet Appendix S1. Our results remain robust if we use the change in ROA from year of trading *t* to *t+1*.

their sell trades predict 0.1% in its increases in both year 0 and 1, as $(Post \times Treat)_{i,t}$ is significant at the 95%, and 90% in column 3 and 4, respectively.

Although we have shown that insiders sell when they are informed, their trading strategy remains unclear. We consider one possibility that they strategically release more news to better time the market. We follow Edmans et al. (2018) and use *Key Development* to identify discretionary corporate announcements. We include their monthly number as the moderator variable. Panel D reports that more discretionary news released in the insider sell transaction month lead to higher ROA decreases and investor sentiment predictability in the next two years. However, this is not the case for the cost of capital, as the coefficients of $(Treat \times Post \times News)_{i,t}$ are not statistically significant in both (0,0) and (1,1) models. These results suggest that the higher return predictability embedded in insiders' sell trades is not random, as insiders appear to time their significant loss-averting sell, but not to profit from their buy, trades.

Chan et al. (2022) show that more competent managers are more likely to leave the firm because a higher explicit compensation contract does not compensate the permanent loss in their implicit promotion-based incentives. If non-promoted insiders are trading on talent losses rather than their private information, their sell transactions should contain little future performance predictability. We split our sample depending on whether a non-promoted insider leaves the firm in the next year, and we repeat the regressions in Table 6. Our results, reported in Internet Appendix S9, remain overall robust, meaning that insiders are trading on their private information regarding the firm's future performance rather than the simple talent loss. We also consider that high industry-level incentives will motivate non-promoted insiders to change their job to the same level of seniority but at a larger firm. We identify firms in the top quantile of the large industry tournament incentives each year. In unreported results, we find that our previous results remain

robust, but $\Delta r_{t,t+2}$ becomes insignificant for firms with high industry tournament incentives, indicating that industry tournament incentives will not mitigate the adverse effect. Overall, these results suggest that tournament losers will exert lower level of effort to improve the firm performance as their total compensation packages declined in value.

### *3.7 Insider trading activities of existing managers*

We expect non-promoted executives who increase their opportunistic trading to stay with the firm, as they will view the overall level of compensation to be sufficient to maintain employment. To test this hypothesis, we first use the same diff-in-diff regression. Internet Appendix S10 reports the results. In column (1) and (3), the dependent variable is $ExitD_{p,t}$, a dummy variable equals to one for exiting executives who are leaving the firm in the year (0, 2), and zero otherwise. We include the same set of control variables. The results indicate that the coefficients of $(Post \times Treat)_{i,t}$ for both samples do not explain executives' exiting probability, suggesting that exiting insiders do not abnormally purchase or offload their positions in their firms before they leave. In columns (2) and (4), we compare the post-transaction return between exiting and staying insiders by interacting the moderator $LastD_{p,t}$, a dummy equal to one if an insider *p* is staying in the firm for the last year, and zero otherwise. While there is no significant difference between the two samples for the buy trades, the interaction variable is positive and significant for the sell sample suggesting that the trades of the exiting managers are not more profitable as those of their staying counterparts, and thus, they are more likely to leave the firm. On the other hand, higher trading profitability compensates managers for their forgone CEO promotion incentives and aligns their compensation closer to the optimal level, making them less likely to leave.

Panel B reports the results based on insider matched sample. For each exiting insider who is leaving in year (0, 2), we select a control insider in year *t-1*, which is one year before CEO

turnover, by matching on their total compensation, average insider trading profitability and total shares traded. We require that there is no CEO turnover event that occurred for our control sample within years (-3, 3). The coefficient of $(\text{Post}\times\text{Treat})_{i,t}$ is negative (positive) and statistically significant for purchase (sell) sample, but columns (2) and (4) show that the post-trade profitability is not significant. These results suggest that the exiting insiders systematically make less non-informative buy and sell trades.[16]

### *3.8 Firm level characteristics for high turnover firms*

We further assess whether the effect is more persistent for firms that have many non-promoted insiders leave in year (0, 1). The sample median for the proportion of exiting directors is 0.4. We define a dummy variable $High\_TurnoverD_{j,t}$ equal to one if for firm $j$ more than 40% of their tournament contenders left the firm in the next two years, and zero otherwise. We compute for firm $j$ with and without CEO turnover in the years (-3, -2) the average *BHAR_m_180*, which we multiply by -1 for sell transaction to correct for the direction. We control for firm level characteristics, firm risk taking incentives as the first years of new CEOs see an increase in stock return volatility leading to insider trades to be more profitable and more frequent because volatility is temporarily higher, increasing insiders' informational advantage (Pan, Wang, and Weisbach, 2015; Cziraki and Groen-Xu, 2020), corporate governance, and analyst talent, which lowers insider trading profitability (Dang et al., 2021). Appendix A details our variables. We estimate both logit and fixed effect regressions by including year dummy variables. We use robust standard errors for logit and cluster standard errors at year-industry level in our fixed effect regressions.

[16] Under SEC rule 16a-2(b) executives need to file their trades for six months after they have left their firms.

The results in Internet Appendix S11 show that tournament losers are more likely to leave firms with higher book-to-market value, analyst coverage, research and development costs, stock returns and cash flow volatilities. They are also smaller and have more independent managers on the board. The coefficients of historical average insider profitability remain negative and significant, indicating that non-promoted insiders are less likely to leave in the future firms where insider trading profitability was higher in the past. Moreover, the coefficient of $C\_score_{j,t-1}$ is positive and significant, implying that tournament rejectees are more likely to leave firms with more conservative board, suggesting that a higher non-promoted director turnover rate among firms that have more rigorous insider trading rules.

### *3.9 Insider trading and the effect of the tournament incentives*

We have established that tournament contenders do not trade on their private negative information opportunistically before the tournament because the insider trading behavior will lower their winning probability. However, once the loss of the tournament has been revealed, tournament rejectees engage in informed trading. We acknowledge that the diff-in-diff regression specification is imperfect to eliminate the underlying endogeneity. We argue that if our previous results are not driven by unobservable endogeneity, we expect that the presence of insider trading opportunity weakens the well-established positive casual effect of tournament incentives on firm performance because part of the tournament prize can be compensated after losing the tournament by the tournament losers. In this section, we revisit the empirical finding in Kale et al. (2009) by considering insider trading opportunity as a substitute for CEO promotion opportunity. We use the following refined firm-year fixed effect regression version of Kale et al. (2009) using Tobin's Q and ROA to proxy for the firm performance.

$$\text{firm\_performance}_{j,t} = \alpha + \beta_1 \text{pay\_gap}_{j,t} + \beta_2 \text{rd}_{j,t} + \beta_3 \text{sale}_{j,t} + \beta_4 \text{sale}^2_{j,t} + \beta_5 \text{capital-to-sale}_{j,t} + \beta_6 \text{advertising-to-sale}_{j,t} + \beta_7 \text{dividend-yield}_{j,t} + \beta_8 \text{leverage}_{j,t} + \beta_9 \text{lnage}_{j,t} + \beta_{10} \text{skt\_ret\_volatility}_{j,t} + \rho + \delta + \varepsilon_i \quad (3)$$

where *pay_gap*$_{j,t}$ proxies for tournament incentives as previously specified. $\rho$ is firm fixed effect, and $\delta$ is year fixed effect. We cluster the standard error at the firm level. Appendix A defines the remaining variables. We expect $\beta_1$ to be statistically significant and positive, because, as in Kale et al. (2009), the higher tournament incentives, the better the firm performance. We estimate a 2SLS regression with two first-stage regressions. We follow Kale et al. (2009) to use the median value of tournament incentives for firms in the same sales quintiles and the same two-digit SIC industry as the firm as instrumental variable because it is a significant determinant of the amount of each firm's tournament incentives. Our second stage regression is as follows:

$$\text{firm\_performance}_{j,t} = \alpha + \beta_1 \widehat{\text{pay\_gap}}_{j,t} + \beta_2 (\widehat{\text{pay\_gap} \times \text{all\_IT}})_{j,t} + \beta_3 \text{all\_IT}_{j,t} + X_{i,t} + \varepsilon_{j,t} \quad (4)$$

where *all_IT*$_{j,t}$ is the total number trades of non-CEO insiders in firm *j* in year *t*, to proxy for their trading intensity. We expect $\beta_2$ to be negative and significant if the trading activity of non-promoted insiders weakens the positive relationship between the tournament incentives and the firm performance. However, our regression specification implicitly assumes that the variable *all_IT*$_{j,t}$ is exogenous. The reverse causality may be one source of endogeneity, as insiders may purchase (sell) more in outperforming (underperforming) firms , knowing their firms' future valuation. Thus, simply using one IV for the tournament incentives is not sufficient to conclude the causal relations. We use an additional IV to proxy for *all_IT*$_{j,t}$ to relax this assumption. We follow Kim and Lu (2011) and use the sum of maximum state and federal marginal personal income tax rates (referred to hereafter as tax rate) as our second instrumental variable. Kim and

Lu (2011) argue that personal income taxes may affect the personal portfolio composition and the timing of stock transactions and option exercises as, *ceteris paribus*, managers in high tax states may prefer more tax-exempt securities to stocks, thus causing lower stock ownership. We expect tax changes to also lead to changes in share ownership as managers may sell (hold) more shares when they anticipate a tax increase (decrease). Moreover, the variation in state tax laws across states and years is exogenous to a firm's future performance. We collect the sum of the maximum state and federal marginal long-term capital gain tax rates from Feenberg and Coutts (1993), assuming a married representative taxpayer with joint filings and top tax bracket in her state. Insiders are subject to capital gains tax on any capital return from trading stocks, and high rates will reduce their propensity to trade.

Table 7 reports the results. For brevity, we omit the first-stage regression result and report only the first-stage F statistics. Columns (1) and (2) indicate that, in line with Kale et al. (2009), the coefficient of $pay\_gap_{j,t}$ is positive and significant, indicating that the higher the tournament incentives, the higher the firm performance. Our results suggest that the higher the pay disparity, the higher the motivation of the non-CEO executives to exert more effort to compete for the next CEO position, resulting in higher firm's performance. In columns (3) and (4), we employ the median industry tournament incentive as an IV and interact the insider trading intensity with the tournament incentive. As in columns (1) and (2), the coefficient of $\widehat{pay\_gap}_{j,t}$ is positive and statistically significant, further supporting the findings in Kale et al. (2009) of a causal relationship between tournament incentives and firm performance. However, the interaction term is negative and statistically significant, suggesting that the insider trading opportunities weaken the tournament incentives' positive effect on the firm performance. In columns (5) and (6), we employ the tax rate as the second IV to predict the number of insider trades $all\_IT_{j,t}$. In an unreported result,

we find that the coefficient of the tax rate, used as the only IV to explain $all\_IT_{j,t}$ in the first-stage regression, is negative and significant, suggesting that a higher tax rate is associated with fewer insider transactions. As in columns (3) and (4), the coefficient of $\widehat{pay\_gap}_{j,t}$ is positive and significant and that of the interaction term is negative and statistically significant with a magnitude of around a third of that of $\widehat{pay\_gap}_{j,t}$, suggesting that the tournament incentive's effect on firm performance will be overestimated by a third when executives trade on their private information to realize their implicit promotion-based compensation. The coefficient of $all\_IT_{j,t}$ is positive and statistically significant, suggesting that insider trading improves the firm's performance, mitigating any agency problems by aligning managers' and shareholders' interest.

[Insert Table 7 here]

## 4. Robustness Test

### *4.1 Reverse causality concern and generalization of results outside event windows*

We subject our results to various robustness checks. One drawback of our diff-in-diff estimator in this research setting is that we only compare the post-tournament insider trading profitability in year (0, 1) with pre-tournament insider trading profitability in year (-2, -1). These post-tournament transaction returns may not be significantly different from their transaction returns outside a CEO turnover event before tournament began. We employ a 2SLS estimator to generalize the results outside this event period, control for potential endogeneity, and compare the post-tournament insider trading profit with their unconditional ones. We include the same set of control variables and fixed effects as Equation 2. Additionally, we interact the endogenous non-promoted executive dummy $NPED_{i,t}$ with CEO trading measure $CEO\_IT_{i,t}$ to investigate whether tournament losers trade against new CEO.

We use the last fiscal year's former CEO age as our IV[17]. The former CEO age embeds predictive power for the CEO turnover to satisfy the relevance condition (Peters and Wagner, 2014; Cziraki and Jenter, 2022), because CEO who left the firm longer (shorter) time ago is likely to be older (younger) than the average former CEO[18]. The IV is not correlated with insiders' trading profitability, which proxy for their private information regarding the firm's future fundamental. Although the exclusion condition is not formally testable, it is less of a concern. The former CEO's age *per se* will not affect a firm's valuation as it bears no impact on its future cash flow and the average time distance between year *t* and the year that the former CEO left the firm of six years is relatively long to affect the firm's future value and corporate policies (Bhagat and Bolton, 2013). We collect our instrumental variable, the last fiscal year former CEO's age, from Execucomp, or, if the data is missing, manually from BoardEx or Factiva.

Table 8, Panel A and B report that the coefficients of $age\_ceo_{j,t-1}$ in our first-stage regressions are all statistically significant with the expected signs, indicating that it is an appropriate instrumental variable for CEO turnover event. It is positive and statistically significant for periods (0, 0), suggesting that the older the former CEO, the higher the likelihood of a CEO turnover in the next fiscal year, in line with our hypothesis. For periods (1, 1), it becomes negative and statistically significant, suggesting that the recently left CEO is younger than the average former CEO age among all firms covered by Execucomp. The first stage F statistics, computed without

[17] For example, Allan Ray Landon became the CEO of the Bank of Hawaii Corporation in 2005, a year we designate as year 0 in our study. For insider transactions that occurred in 2005 and 2006, we use the age of the former CEO, Michael E. O'Neill, which was 58 and 59 in these two years, respectively, as instrumental variables. For insider transactions in 2004, a year we refer to as year -1, when Michael E. O'Neill was the acting CEO, we use the age of the previous CEO, Lawrence M. Johnson, who was 64, as the instrumental variable.

[18] The use of former CEO age reduced our sample size as it discards all observations in our entire sample before the first CEO turnover. The correlation between the current and former CEO is 0.25. We recognize that the former measure is more exogenous than the current CEO age. The correlation between former CEO age and current CEO tenure is 0.39. We use current CEO tenure in our 2SLS in robustness test, all coefficients remain robust but weaker.

the interaction term $\widehat{NPED \times CEO\_IT}_{i,t}$ reported at the bottom of Panel C, Table 8, are all above 10, which is the minimum value to alleviate the weak instrument concern, providing significant support for the relevance condition. The Anderson-Rubin F-statistic rejects the null hypothesis and indicates that the endogenous $NPED_{i,t}$ is statistically significant. The Difference-in-Sargan C-statistic rejects the null hypothesis that $NPED_{i,t}$ is exogenous.

Table 8, Panel C, reports the second-stage regression results. For insider purchase sample, the coefficients of both $\widehat{NPED}_{i,t}$ and $\widehat{NPED \times CEO\_IT}_{i,t}$ are insignificant, suggesting that when non-promoted managers make purchase transactions, they do not consider the current CEO trading activity, and their informed purchase transactions do not generate higher return predictability outside the event window. The endogeneity problem is likely to be more severe in insiders' sell than buy trades, because many insiders do not sell to seek profit (Cohen et al., 2012). The coefficients of $\widehat{NPED}_{i,t}$ are negative and statistically significant for insider sell sample, suggesting that insiders incorporate more private negative information into their sell trades to compensate themselves for the forgone promotion-based incentives. The interaction term's coefficient is positive and statistically significant in both year 0 and +1, indicating that their sell trades are systematically loss averting when the newly appointed CEO increases her holding, suggesting that managers strategically time their sell trades against the current CEO's noisy transactions. $\widehat{(NPED \times CEO\_IT)}_{i,t}$ is larger in year 0, implying that the CEO trading direction plays a more prominent role in insiders' trading decision-making process in year 0 and 1.

The asymmetry effect of CEO trading activity proxied by $CEO\_IT_{j,t}$ in the insider sell samples is due to the noisy buy trades of the newly-appointed CEOs to prolong their contracts, not necessarily to make profits, as suggested by Armstrong et al. (2021). CEO purchase transactions embed a strong signaling effect for the stock undervaluation and the outside investors will adjust

the stock price upward even if the signal is false (Wu, 2019). The short term buying pressure from these uninformed investors will temporarily boost the stock price, setting up a premise for the non-promoted executives to sell their shares at an inflated price. The price will be gradually corrected in the long term making their sell trades loss-avoiding. Moreover, non-promoted executives will not benefit from trading against CEO's sell trades to cover their buy trades as new CEOs rarely sell shares in the first year of their appointment as evident by the insignificant interaction term for insider purchase sample. To confirm the noisy trading behaviour of the newly appointed CEO, we investigate whether the return profitability of CEO purchase transaction will decrease to negative in the long term as suggested by Wu (2019). In Internet Appendix S12, we estimate a fixed effect regression using the same set of control variables of equation (2). We find no signficant change in CEO buy trades in year 0 return profitabilities in 30-day holding period, but the return predictability is 11.1%, significantly lower than their average buy trades in 365-day holding period. The return reversal is clearer in year 1. CEO purchases generate a statistically significant 2.2% higher abnormal profit in 30-day period, indicating that their buy trades boosted stock prices. However, these buy trades yield significant 10.4% lower profits confirming that these CEO buy trades are nosiy, and the market corrects the inflated prices to a lower level. Our results confirm that non-CEO managers adopt contrarian strategies by trading profitably against their CEO.

Overall, the 2SLS results confirm that insiders incorporate more negative private information into their sell transactions in all post-event years, consistent with our diff-in-diff regression results. Additionally, we apply the 2SLS estimator with the same IV based on the matched insider sell sample. Internet Appendix S13 reports the results. The signs and levels of significance of $\widehat{NPED}_{i,t}$ are consistent with the 2SLS estimates based on the universal sample.

[Insert Table 8 here]

*4.2 Additional robustness tests*

Another drawback of our diff-in-diff speciation is that we discard firms that cannot be matched with a control firm. Table 9 Panel A shows that our results are not dependent on the diff-in-diff specification by re-estimating the Table 8 Panel C regression specification using OLS estimator. Overall, $NPED_{i,t}$, the dummy variable equals to one for insider trades made in year *t* after CEO turnover event remains positive for insider purchase transactions in year *0* only, and negative and statistically significant for insider sell transactions in both year *0* and *1*. These results suggest that our previous findings remain robust using an unconditional insider trading sample.

One main assumption behind our results is that our IV, the last year former CEO's age, and the private information that non-CEO managers are exploiting are not correlated. We recognize the possibility that former CEOs may affect their firm's future valuation through an adoption of corporate decisions with long-lasting effects. Although there is no reason to believe that the preference for a long-lasting policy is systematically related to the manager's age, this possible violation of exclusion restriction will lead to an inconsistent estimate. We alleviate this potential concern by including a set of proxy variables for corporate performance in our 2SLS regressions.

In the first robustness test, we add to 2SLS regression fourteen additional control variables that embed predictive power for the firm's future fundamental and are possibly determined by the personal preferences of CEOs in different age groups to better demonstrate the validity of the exclusion restriction and the robustness of our results. Internet Appendix S14 Panel A reports the result. For insider sell samples, the sign and significance of $\widehat{NPED}_{i,t}$ and $\widehat{NPED \times CEO\_IT}_{i,t}$ are consistent with our previous results. As the second robustness test, we consider that the former CEO's age will only affect non-CEO's trading profitability through CEO turnover. Therefore, if we regress the *BHAR_m_180* on former CEO's age by using years other than years 0 and 1, the

coefficient of CEO's age should be statistically insignificant if the exclusion restriction holds. In un-tabulated results, we find that its coefficient is statistically insignificant for buy and sell samples, strengthening the plausibility of exclusion restrictions further.

In the third robustness test, we only keep the top two highest paid managers who are younger than 60 in each year for each firm. We consider that their likelihood of competing in a CEO turnover is the highest. We re-estimate the diff-in-diff regression and 2SLS regression. Table 9, Panel B, shows that our conclusions remain robust despite losing more than half of our sample. The results show that our conclusions do not hinge on the assumption that all top 4 highest paid non-CEO managers are tournament contenders. In the fourth robustness test, we only keep opportunistic transactions. Panel C shows that our conclusions remain robust, suggesting that insiders will better time their opportunistic transactions after losing the CEO turnover. In the fifth robustness test, Panel D shows that our conclusions remain robust when we exclude all CFO trades as CFOs are less likely to become CEOs. In unreported results, we find that our findings are robust when we include 10b5-1 transactions, and we exclude firms that retain their previous CEOs.

We further test the validity of our diff-in-diff regression results over a $(-2, +1)$ period around pseudo-CEO turnovers, which are arbitrarily set as three years before or after the actual CEO turnover. We find, but not report for brevity, that the coefficient of the interaction term $Post \times Treat_{i,t}$ in Equation (2) remains insignificant for both purchase and sell samples, supporting the validity of the parallel trend assumption and the credibility of our diff-in-diff design[19].

[19] We additionally conduct 1,000 placebo tests for both the difference-in-difference regression and 2SLS regression. We discuss the detailed process of picking random firms in the Internet Appendix S1. Overall, these results indicate that if we use a randomly selected sample of firms without CEO turnover events, we cannot replicate our main findings obtained from both diff-in-diff regression and 2SLS. We also follow Biggerstaff et al. (2020) to adjust the trading profitability to account for their dissimulation trading strategies. We explain the process in the Interest Appendix S1. Overall, our results become stronger after adjusting for their dissimulation strategies. Results for placebo tests, dissimulation strategy tests, and pseudo-CEO turnovers are omitted for brevity. They are available upon request.

We also employ alternative holding periods and Fama-French Four-Factor model (Fama and French, 1993) to compute alpha over 30-, 180- and 360- calendar holding periods, as alternative measures of abnormal returns using Kenneth French's Data Library, as follows:

$$\text{return}_{j,t} - \text{rf}_t = \alpha + \beta_1(\text{MKT}_t - \text{rf}_m) + \beta_2\text{SMB}_t + \beta_3\text{HML}_t + \beta_4\text{MOM}_t + \epsilon_t \quad (5)$$

where α, the risk-adjusted excess return, is estimated from one day after the transaction date over the next 30/180/365 calendar days. $return_{j,t}$ is the daily return adjusted for dividend, $rf_m$ is the risk-free rate proxied by the one-month T-bill rate. $MKT_t$ is the CRSP value-weighted market index. We time the daily α by 22, 126, and 252 for these 3 holding periods. Additionally, we report the raw cumulative return $ret_{t+1,t+i}$ and the NYSE value-weighted size-decile adjusted return $BHAR\_size\_j$. Table 9 Panel E displays only the coefficients of $Post \times Treat_{i,t}$ from re-estimating Table 4. Internet Appendix S14 Panel B reports only the coefficient of $\widehat{NPED}_{i,t}$ for brevity from re-estimating Table 8. For the buy trades, it is mainly insignificant, while for the sell trades, it is mainly negative and significant, suggesting that these trades are loss avoiding for the 180 and 365 holding periods. The remaining results did not change.

[Insert Table 9 here]

## 4 Conclusion

We investigate the causal relationship between losing the promotion opportunity and the trading behavior of non-promoted executives. We eliminate the endogeneity by using a matched sample to specify a diff-in-diff regression. We find that they systematically avoid trading on their private negative information when competing for the CEO position in year (-2, -1), to avoid affecting adversely their winning probabilities, but after losing the tournament context, they predominantly sell, not buy, profitably against the nosiy buy trades of the newly promoted CEO.

Their trading profitability reflects their *ex-post* expectation discrepancy of their forgone promotion opportunity, investors' sentiments, and the decrease in their effort, which results in their firm's future declining performance. Our results hold after accounting for the different levels of firm-level price information informativeness. Finally, we show that the trading opportunity of non-promoted insiders weakens the positive relationship between the tournament incentives and firm performance as insiders use their transactions to realize the tournament incentives prior to the tournament. Our results are robust to various econometric and estimations specifications.

Our results may be affected by other factors we have not considered in our analysis. Non-promoted executives could be trading just before material news is announced or for other non-identifiable reasons. While data on news announcements is not available in machine readable form, we tried to control indirectly for the other non-directly observable motives. We have used non-CEO executives' personal and company characteristics as controls, but we could not find enough observations for an exogenous shock, such as sudden death of current CEO, that will affect their personal career horizon only. The extent to which these factors will better eliminate endogeneity and alter or confirm our results is the subject of further research.

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**Table 1: Summary Statistics**

This table reports the summary statistics of the non-CEO insider buy and sell trades in year 0 when CEO turnover tournament occurred. *pay_gap_firm* is the natural logarithm of the difference between the adjusted CEO total compensation (*tdc1*) and the median adjusted total compensation of non-CEO insiders, deflated to 2010 CPI. $OutsiderD_{j,t}$ is a dummy equal to one if the promoted CEO is an outsider. $COOD_{j,t}$ is a dummy equal to one if the CEO succession was planned in $(-2,-1)$. *ret30* and *Mom* are days -30 to -1 and -364 to -31 stock price momentum. *bm*, *ROA*, *rd*, and *marketcap* proxy for growth, profitability, research and development cost, and size of the firm, respectively. $illiq_{j,m-1}$ is the Amihud (2002) illiquidity measure. $numest_{j,m-1}$ is financial analyst coverage. $Delta_{p,t-1}$ ($Vega_{p,t-1}$) is dollar change in manager *p*'s wealth associated with a 1% change in the firm's stock price in $000 (standard deviation of the firm's returns). $Rating_{j,t-1}$ is the yearly industry average S&P long-term rating from Compustat. $CEO\_IT\_Net\_Value_{j,t}$ is the net insider trading value of the current CEO. $high\_incentiveD_{p,t-1}$ is equal to one for high (in the top three) incentive managers and zero otherwise. Appendix A details all the variables. $^{***}$, $^{**}$, $^{*}$ ($^{a}$, $^{b}$, $^{c}$) indicate the sample mean (differences in means and medians) between the two samples is statistically different at the 99%, 95% and 90% confidence levels, respectively. All variables except insider purchase size and shares are winsorised at the top 99% and the bottom 1% level.

| Variable | **Mean** | **Median** | **N** | **Mean** | **Median** | **N** |
|---|---|---|---|---|---|---|
| | Panel A: Buy Trades | | | Panel B: Sell Trades | | |
| pay_gap_firm ($000s) | 2,079 | 674 | 832 | 3,340$^{***}$ | 2,147$^{a}$ | 13,019 |
| Non-CEO comp ($000s) | 1,070 | 681 | 832 | 2,143$^{***}$ | 1,346$^{a}$ | 13,062 |
| illiq (000s) | 0.576 | 0.087 | 832 | 0.032$^{***}$ | 0.005$^{a}$ | 13,062 |
| marketcap ($million) | 1,765 | 545 | 832 | 14,112$^{***}$ | 3,361$^{a}$ | 13,062 |
| Mom | 0.000 | 0.042 | 831 | 0.288$^{***}$ | 0.240$^{a}$ | 13,059 |
| ret30 | -0.021 | -0.029 | 709 | 0.056$^{***}$ | 0.048$^{a}$ | 11,048 |
| bm | 0.883 | 0.752 | 832 | 0.418$^{***}$ | 0.337$^{a}$ | 13,062 |
| numest | 5.905 | 5.000 | 832 | 12.492$^{***}$ | 11.000 | 13,062 |
| ROA | -0.009 | 0.005 | 832 | 0.061$^{***}$ | 0.060 | 13,062 |
| rd | 0.034 | 0.001 | 832 | 0.078$^{***}$ | 0.005$^{a}$ | 13,062 |
| delta (in $000) | 25 | 11 | 767 | 154$^{***}$ | 57$^{a}$ | 12,345 |
| vega (in $000) | 11 | 5 | 760 | 48$^{***}$ | 16$^{a}$ | 12,342 |
| $OutsiderD_{jt}$ | 0.369 | 0.000 | 832 | 0.295$^{***}$ | 0.000$^{a}$ | 13,062 |
| $COOD_{jt}$ | 0.133 | 0.000 | 832 | 0.186$^{***}$ | 0.000$^{a}$ | 13,062 |
| $high\_incentiveD_{p,t-1}$ | 0.453 | 0.000 | 832 | 0.562$^{***}$ | 1.000$^{a}$ | 13,062 |
| $rating_{j,t-1}$ | 1.319 | 1.366 | 821 | 1.392 | 1.439 | 12,645 |
| $CEO\_IT\_Net\_Value_{i,t}$ ($000s) | 300 | -42 | 832 | -2,581$^{***}$ | 0,000$^{a}$ | 13,062 |
| Mean No Shares traded | 10,176 | 2,000 | 832 | 27,781$^{***}$ | 10,000$^{a}$ | 13,062 |
| Mean trade value ($000s) | 163 | 19 | 832 | 944$^{***}$ | 327$^{a}$ | 13,062 |
| Average No of Observations | 416 | | | 6,531 | | |

**Table 2: Insider Trading Propensity after Losing the CEO Competition**

Panel A reports the logit and linear probability models estimating the likelihood of a manager *p* becoming CEO in year *t.* The dependent variable is one for CEO, and zero otherwise, using all tournament competitors and for CEO turnover year *t* only. The sample is at manager-firm level. The variables $\#\_buy_{p,t}$ and $\#\_sell_{p,t}$ represent the number of opportunistic insider purchase and sell transactions made by insiders *p* in year *t*, following Cohen et al. (2012). Other independent variables included but not reported are $ret30_{j,t-1,(d-1,d-30)}$, $mom_{j,\ t-1,(d-31,d\ -364)}$, $bm_{j,t-1}$,$illiq_{j,t-1}$,total $asset_{j,t-1}$, $roa_{j,t-1}$,tobin's $Q_{j,t-1}$,$leverage_{j,t-1}$. Panel B reports the logit and linear probability regression outputs. The dependent variable is $opp\_D_{i,t}$ equal to one for insider transactions executed by opportunistic traders, and zero otherwise. We only include firms with CEO turnover event. Standard errors reported in parentheses in Panel A and B are computed based on robust standard errors for logit model and clustered at the firm level for linear probability model. Appendix A defines all our variables. ***, **, and * denote significance at the 99%, 95% and 90% confidence level, respectively. All variables are winsorised at the top 99% and the bottom 1% level.

| Panel A: Opportunistic insider trading and the probability of winning CEO promotion | | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | Routine and Opportunistic trades | | Opportunistic trades only | |
| | $CEOD_{i,t}$ | $CEOD_{i,t}$ | $CEOD_{i,t}$ | $CEOD_{i,t}$ |
| $age_{p,t-1}$ | -0.005*** | -0.005** | -0.003 | -0.003 |
| | (0.002) | (0.002) | (0.002) | (0.002) |
| $tenure_{p,t-1}$ | 0.005 | 0.009*** | 0.006 | 0.005 |
| | (0.03) | (0.003) | (0.004) | (0.004) |
| $COOD_{j,t-1}$ | 0.461*** | 0.453*** | 0.400*** | 0.400*** |
| | (0.029) | (0.309) | (0.034) | (0.035) |
| $\#\_buy_{p,t-1}$ | 0.029* | | 0.046* | |
| | (0.017) | | (0.027) | |
| $\#\_sell_{p,t-1}$ | -0.007** | | -0.013*** | |
| | (0.004) | | (0.005) | |
| $\#\_buy_{p,(t-2,t-1)}$ | | 0.052** | | 0.022 |
| | | (0.023) | | (0.015) |
| $\#\_sell_{p,(t-2,t-1)}$ | | -0.006* | | -0.006** |
| | | (0.004) | | (0.003) |
| $delta_{p,t-1}(\times 0.01)$ | 0.019** | 0.024*** | 0.005 | 0.006 |

| | | | | |
|---|---|---|---|---|
| | (0.008) | (0.007) | (0.005) | (0.005) |
| $vega_{p,t-1}(\times 0.01)$ | 0.016 | 0.022 | 0.106*** | 0.103*** |
| | (0.014) | (0.017) | (0.033) | (0.033) |
| $lncompen_{p,t-1}(\times 0.01)$ | 0.004*** | 0.003*** | 0.006*** | 0.006*** |
| | (0.001) | (0.001) | (0.001) | (0.001) |
| Other Control | Yes | Yes | Yes | Yes |
| Fixed Effect | Firm, Year | Firm, Year | Firm, Year | Firm, Year |
| Sample | 1,471 | 1,375 | 1,364 | 1,364 |
| With-in $R^2$ | 0.44 | 0.44 | 0.43 | 0.42 |

Panel B: Opportunistic Insider trading propensity after losing the CEO competition

| | Insider Purchase Transactions | | Insider Sell Transactions | |
|---|---|---|---|---|
| | Logit | Linear Probability | Logit | Linear Probability |
| | $OppD_{i,t}$ | $OppD_{i,t}$ | $OppD_{i,t}$ | $OppD_{i,t}$ |
| $Post_{i,t}$ | 1.797*** | 0.049*** | 0.171*** | 0.020** |
| | (0.626) | (0.021) | (0.051) | (0.009) |
| $CEO\_IT_{j,t}$ | 0.414** | 0.016** | -0.069*** | -0.009*** |
| | (0.200) | (0.008) | (0.015) | (0.003) |
| Constant | 1.545 | 0.889*** | 0.024 | 0.676*** |
| | (3.080) | (0.119) | (0.343) | (0.070) |
| *Control Variables* | Yes | Yes | Yes | Yes |
| Sample | 521 | 558 | 18,299 | 18,261 |
| Within $R^2$ | 0.26 | 0.123 | 0.029 | 0.021 |
| Fixed Effect | | Firm, Year | | Firm, Year |

**Table 3: Difference-in-difference regression output**

The table reports the regression results where the dependent variable is BHAR_m_180. The explanatory variable of interest is $(\text{Post}\times\text{Treat})_{i,t}$, a dummy variable equals to one for firms that have a CEO turnover in year *t*, and zero otherwise. Other variables are described in Appendix A. In Panel A, we only include sample in pre-CEO turnover period (-2, -1) and post-CEO turnover period (t, t+i). In Panel B, we exclude pre-planned CEO turnover identified by following Cvijanovic et al. (2022). In Panel C and Panel D, we split the entire treated sample with its nearest neighbor control firm into exogenous and involuntary CEO turnover event according to Gentry et al. (2021). Standard errors in parentheses are based on robust standard errors clustered at the firm-month level. $^{***}$, $^{**}$, and $^{*}$ denote significance at the 99%, 95% and 90% confidence level, respectively. All variables are winsorised at the top 99% and the bottom 1% level. All regressions include control variables and firm and month fixed effects.

| | **Insider Purchase** | | | **Insider Sell** | | |
|---|---|---|---|---|---|---|
| Year *t* | (0,1) | (0,0) | (1,1) | (0,1) | (0,0) | (1,1) |
| Panel A: Baseline Regressions | | | | | | |
| $\text{Post}_{i,t}$ | 0.016 | 0.016 | -0.034 | 0.013$^{**}$ | 0.008 | 0.022$^{***}$ |
| | (0.027) | (0.032) | (0.051) | (0.005) | (0.006) | (0.007) |
| $\text{Treat}_{i,t}$ | -0.119$^{**}$ | -0.148$^{**}$ | -0.081 | 0.012$^{*}$ | 0.019$^{**}$ | 0.012$^{*}$ |
| | (0.051) | (0.058) | (0.078) | (0.007) | (0.008) | (0.007) |
| $(\text{Treat}\times\text{Post})_{i,t}$ | 0.085$^{**}$ | 0.092$^{*}$ | 0.029 | -0.027$^{***}$ | -0.020$^{**}$ | -0.039$^{***}$ |
| | (0.043) | (0.053) | (0.087) | (0.008) | (0.010) | (0.011) |
| $\text{CEO\_IT}_{j,t}$ | 0.009 | 0.006 | 0.046$^{**}$ | 0.012$^{***}$ | 0.010$^{***}$ | 0.013$^{***}$ |
| | (0.013) | (0.015) | (0.018) | (0.002) | (0.002) | (0.002) |
| $\text{COOD}_{jt}$ | -0.041 | -0.032$^{***}$ | -0.020 | 0.038$^{***}$ | 0.043$^{***}$ | 0.037$^{**}$ |
| | (0.094) | (0.116) | (0.118) | (0.013) | (0.019) | (0.015) |
| Sample | 2,092 | 1,754 | 1,316 | 45,515 | 36,231 | 34,231 |
| Within $R^2$ | 0.21 | 0.21 | 0.24 | 0.08 | 0.15 | 0.08 |
| Panel B: Exclude pre-planned CEO Turnover | | | | | | |
| $\text{Post}_{i,t}$ | 0.090 | -0.028 | -0.096 | 0.012$^{**}$ | 0.008 | 0.020$^{**}$ |
| | (0.055) | (0.041) | (0.062) | (0.006) | (0.007) | (0.009) |
| $\text{Treat}_{i,t}$ | -0.061 | -0.177$^{**}$ | 0.001 | 0.014 | 0.023$^{**}$ | 0.012 |
| | (0.068) | (0.072) | (0.069) | (0.009) | (0.011) | (0.009) |
| $(\text{Treat}\times\text{Post})_{i,t}$ | 0.058 | 0.132$^{**}$ | 0.058 | -0.035$^{***}$ | -0.026$^{**}$ | -0.055$^{***}$ |
| | (0.078) | (0.061) | (0.103) | (0.010) | (0.012) | (0.014) |
| Sample | 1,839 | 1,506 | 1,116 | 35;864 | 28,537 | 26,521 |
| Within $R^2$ | 0.29 | 0.24 | 0.27 | 0.09 | 0.09 | 0.09 |
| Panel C: Exogenous CEO Turnover Event | | | | | | |
| $\text{Post}_{i,t}$ | -0.004 | -0.126 | 0.759$^{*}$ | 0.052$^{**}$ | 0.055$^{*}$ | 0.082$^{**}$ |
| | (0.107) | (0.093) | (0.427) | (0.026) | (0.031) | (0.038) |
| $\text{Treat}_{i,t}$ | -0.247 | -0.269 | 0.743$^{*}$ | -0.062 | -0.054 | -0.014 |
| | (0.220) | (0.228) | (0.436) | (0.048) | (0.059) | (0.056) |
| $(\text{Treat}\times\text{Post})_{i,t}$ | 0.132 | 0.437$^{***}$ | -1.920$^{***}$ | -0.189$^{***}$ | -0.143$^{**}$ | -0.212$^{***}$ |
| | (0.162) | (0.134) | (0.633) | (0.048) | (0.067) | (0.065) |
| Sample | 422 | 339 | 263 | 1,573 | 1,250 | 1,184 |
| Within $R^2$ | 0.41 | 0.67 | 0.61 | 0.27 | 0.26 | 0.24 |
| Panel D: Involuntary CEO Turnover Event | | | | | | |
| $\text{Post}_{i,t}$ | 0.081$^{*}$ | 0.020 | -0.052 | 0.025$^{***}$ | 0.017$^{*}$ | 0.036$^{***}$ |
| | (0.047) | (0.031) | (0.051) | (0.009) | (0.009) | (0.012) |
| $\text{Treat}_{i,t}$ | -0.163$^{*}$ | -0.158$^{**}$ | -0.159$^{**}$ | 0.004 | 0.040 | -0.009 |
| | (0.083) | (0.067) | (0.073) | (0.021) | (0.030) | (0.022) |
| $(\text{Treat}\times\text{Post})_{i,t}$ | -0.001 | 0.057 | 0.063 | -0.043$^{**}$ | -0.037$^{*}$ | -0.061$^{**}$ |
| | (0.083) | (0.058) | (0.126) | (0.021) | (0.022) | (0.030) |
| Sample | 1,686 | 1,395 | 979 | 18,319 | 14,489 | 13,187 |
| Within $R^2$ | 0.30 | 0.24 | 0.26 | 0.09 | 0.09 | 0.08 |

**Table 4: Insider heterogeneity and participation in tournament**

The table reports the fixed effect diff-in-diff regression results where BHAR_m_180 is the dependent variable. We match each treated firm with CEO turnover event in year *t* with one control firm using Mahalanobis distance on the average insider purchase/sell profitability, logarithm of the total asset and the book-to-market ratio in the fiscal year *t-1*. We restrict that the control firm sample does not have any CEO turnover in (-2, 2). In Panel A, we use $Lnage_{p,t}$, the natural logarithm of the age of the insider *p* in year *t*. In Panel B, we use $Lntenure_{p,t}$, the natural logarithm of the tenure of the insider *p* in year *t* in firm *j*. In Panel C, we employ $SHD_{p,t}$, a dummy variable equals to one for short-horizon insiders identified by following Akbas et al (2020), and zero otherwise. In Panel D, the moderator variable is $Probability_{p,t-1}$, the estimated subjective probability of insiders becoming CEO based on their personal characteristics. We include firm and month fixed effects. We describe all control variables in Table 1 and Appendix A. Internet Appendix S1 details the moderators. Robust standard errors in parentheses are at the firm-month level. $^{***}$, $^{**}$, and $^{*}$ denote significance at the 99%, 95% and 90% confidence level, respectively. All variables are winsorised at the top 99% and the bottom 1% level.

| | **Insider Purchase** | | **Insider Sell** | |
|---|---|---|---|---|
| Year t | (0,0) | (1,1) | (0,0) | (1,1) |
| | Panel A: Age Effect | | | |
| $(Treat\times Post)_{i,t}$ | $2.018^{**}$ | $5.678^{***}$ | $-0.539^{**}$ | $-1.179^{***}$ |
| | (1.016) | (1.683) | (0.252) | (0.279) |
| $(Post\times Treat\times lnage)_{i,t}$ | $-0.478^{*}$ | $-1.407^{***}$ | $0.137^{**}$ | $0.293^{***}$ |
| | (0.256) | (0.432) | (0.064) | (0.070) |
| Control and main levels | Yes | Yes | Yes | Yes |
| Sample | 1,369 | 1,025 | 32,111 | 29,481 |
| | Panel B: Tenure Effect | | | |
| $(Treat\times Post)_{i,t}$ | $0.229^{*}$ | $0.677^{***}$ | $-0.057^{**}$ | $-0.122^{***}$ |
| | (0.121) | (0.192) | (0.024) | (0.033) |
| $(Post\times Treat\times lntenure)_{i,t}$ | $-0.151^{*}$ | $-0.469^{***}$ | $0.032^{***}$ | $0.054^{***}$ |
| | (0.051) | (0.119) | (0.012) | (0.016) |
| Control and main levels | Yes | Yes | Yes | Yes |
| Sample | 1,791 | 1,282 | 36,746 | 33,527 |
| | Panel C: Investment Horizon | | | |
| $(Treat\times Post)_{i,t}$ | 0.075 | -0.001 | $-0.017^{*}$ | $-0.028^{**}$ |
| | (0.052) | (0.079) | (0.010) | (0.012) |
| $(Post\times Treat\times SHD)_{i,t}$ | -0.104 | 0.084 | $0.050^{**}$ | $0.051^{*}$ |
| | (0.149) | (0.219) | (0.023) | (0.029) |
| Control and main levels | Yes | Yes | Yes | Yes |
| Sample | 1,791 | 1,282 | 35,873 | 33,519 |
| | Panel D: Predicted probability of becoming CEO | | | |
| $(Treat\times Post)_{i,t}$ | $-0.304^{***}$ | -0.026 | $0.037^{***}$ | $0.045^{***}$ |
| | (0.109) | (0.176) | (0.014) | (0.017) |
| $(Post\times Treat\times Probability)_{i,t}$ | $1.137^{***}$ | 0.303 | $-0.064^{*}$ | $-0.125^{***}$ |
| | (0.413) | (0.425) | (0.039) | (0.048) |
| Control and main levels | Yes | Yes | Yes | Yes |
| Sample | 662 | 557 | 24,648 | 24,285 |

**Table 5: Insider heterogeneity and their trading motivation**

This table reports the fixed effect diff-in-diff regression results with BHAR_m_180 as the dependent variable. In Panel A, the moderator variable is $Pay\_rank_{p,t}$, the rank of non-promoted manager sorted by their total compensation in year -1 among all tournament competitors. In Panel B, we use $BA_{p,t-1}$, the bonus award in t-1 for insider $p$. In Panel C, the moderate variable is $ind\_incen_{j,t}$, the natural logarithm of the industry tournament incentives, as outlined in Coles et al. (2018). In Panel D, we use $NoncomD_{p,t}$, a dummy variable equals to one if firm $j$ disclosed a non-compete agreement in its 10-K or 10-Q in year $t$, zero otherwise. We include firm and month fixed effects. We describe all control variables in Table 1 and Appendix A. Internet Appendix S1 details the moderators. Robust standard errors in parentheses are at the firm-month level. ***, **, and * denote significance at the 99%, 95% and 90% confidence level, respectively. All variables are winsorised at the top 99% and the bottom 1% level.

| | **Insider Purchase** | | **Insider Sell** | |
|---|---|---|---|---|
| Year t | (0,0) | (1,1) | (0,0) | (1,1) |
| | Panel A: Tournament Prize | | | |
| $(Treat\times Post)_{i,t}$ | 0.098 | -0.074 | -0.076*** | -0.058*** |
| | (0.108) | (0.203) | (0.022) | (0.019) |
| $(Post\times Treat\times Pay\_rank)_{i,t}$ | 0.021 | -0.006 | 0.018*** | 0.011** |
| | (0.030) | (0.047) | (0.006) | (0.005) |
| Control and main levels | Yes | Yes | Yes | Yes |
| Sample | 1,551 | 1,056 | 34,808 | 31,603 |
| | Panel B: Bonus award effect | | | |
| $(Treat\times Post)_{i,t}$ | 0.652*** | 0.074 | -0.018* | -0.031** |
| | (0.200) | (0.536) | (0.010) | (0.012) |
| $(Treat\times Post\times BA)_{i,t}$ | -0.150** | -0.113 | 0.045** | 0.043** |
| | (0.065) | (0.140) | (0.020) | (0.022) |
| Control and main levels | Yes | Yes | Yes | Yes |
| Sample | 1,554 | 1,059 | 35,190 | 31,841 |
| | Panel C: Board Conservatism | | | |
| $(Treat\times Post)_{i,t}$ | 0.299** | -0.387 | -0.039** | -0.072*** |
| | (0.137) | (0.248) | (0.020) | (0.022) |
| $(Treat\times Post\times C\_quint)_{i,t}$ | -0.101** | 0.097 | 0.016** | 0.018** |
| | (0.045) | (0.070) | (0.008) | (0.008) |
| Control and main levels | Yes | Yes | Yes | Yes |
| Sample | 1,791 | 1,282 | 36,748 | 33,527 |
| | Panel D: Industry Tournament Incentives | | | |
| $(Treat\times Post)_{i,t}$ | 0.329 | -0.781** | -0.097** | -0.162** |
| | (0.337) | (0.381) | (0.038) | (0.074) |
| $(Treat\times Post\times ind\_incen)_{i,t}$ | -0.037 | 0.117** | 0.014** | 0.022** |
| | (0.049) | (0.057) | (0.005) | (0.010) |
| Control and main levels | Yes | Yes | Yes | Yes |
| Sample | 1,471 | 1,030 | 27,751 | 25,065 |
| | Panel E: Non-compete agreements | | | |
| $(Treat\times Post)_{i,t}$ | -0.016 | 0.057 | 0.013** | -0.013 |
| | (0.050) | (0.082) | (0.006) | (0.012) |
| $(Post\times Treat\times NoncomD)_{i,t}$ | 0.265 | -0.225 | -0.039*** | -0.055* |
| | (0.222) | (0.167) | (0.015) | (0.030) |
| Control and main levels | Yes | Yes | Yes | Yes |
| Sample | 1,791 | 1,282 | 36,944 | 33,521 |

**Table 6: Post CEO turnover insider trading and changes in firm and investor features**

This table reports the fixed effect regression output based on matched sample in Table 4. In Panel A, the dependent variable is the change in return on asset between year t and year t+2. In Panel B, the dependent variable is the change in investor sentiment measured as firm-specific component from the market-to-book decomposition of Rhodes–Kropf et al. (2005). The change in investor sentiment $\Delta\text{Sentiment}_{-1,1}$ is measured between year t-1 to year t+1. In Panel C, we obtain the $\Delta r_{t,t+2}$ by following Cziraki et al. (2021) to estimate a modified Fama and French (1993) Three-Factor model. In Panel D, we additionally include the moderate variable $\text{News}_{j,m}$, that is the number of discretionary news released by the company in the insider trading month *m* for firm *j*, defined by following Edmans et al. (2018). We include, but not report for brevity, the control variables in Equation (2). Standard errors reported in parentheses are computed based on robust standard errors clustered at the firm-month level. $^{***}$, $^{**}$, and $^{*}$ denote significance at the 99%, 95% and 90% confidence level, respectively. All variables are winsorised at the top 99% and the bottom 1% level. All regressions include control variables and firm and month fixed effects.

| | Insider Purchase | | Insider Sell | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Year *t* | (0,0) | (1,1) | (0,0) | (1,1) |
| | Panel A: Future Firm Performance | | | |
| *Dependent Variable* | $\Delta\text{ROA}_{t,t+2}$ | $\Delta\text{ROA}_{t,t+2}$ | $\Delta\text{ROA}_{t,t+2}$ | $\Delta\text{ROA}_{t,t+2}$ |
| $\text{Post}_{i,t}$ | 0.006 | 0.008 | -0.001 | -0.003 |
| | (0.012) | (0.012) | (0.003) | (0.003) |
| $\text{Treat}_{i,t}$ | -0.097*** | -0.039*** | 0.012*** | 0.016*** |
| | (0.027) | (0.019) | (0.003) | (0.004) |
| $(\text{Post}\times\text{Treat})_{i,t}$ | 0.011 | -0.015 | -0.017*** | -0.010** |
| | (0.015) | (0.024) | (0.004) | (0.005) |
| Within R-square | 0.11 | 0.15 | 0.06 | 0.05 |
| Sample | 1,688 | 1,222 | 35,580 | 32,628 |
| | Panel B: Investor Sentiment | | | |
| *Dependent Variable* | $\Delta\text{Sentiment}_{t-1,t+1}$ | $\Delta\text{Sentiment}_{t-1,t+1}$ | $\Delta\text{Sentiment}_{t-1,t+1}$ | $\Delta\text{Sentiment}_{t-1,t+1}$ |
| $\text{Post}_{i,t}$ | -0.080 | -0.243** | -0.005 | 0.036** |
| | (0.066) | (0.106) | (0.013) | (0.016) |
| $\text{Treat}_{i,t}$ | -0.245* | -0.126 | 0.041** | 0.049*** |
| | (0.128) | (0.143) | (0.016) | (0.017) |

| | | | | |
|---|---|---|---|---|
| $(Post \times Treat)_{i,t}$ | 0.193* | 0.571*** | -0.047** | -0.063** |
| | (0.116) | (0.211) | (0.022) | (0.025) |
| Within R-square | 0.11 | 0.17 | 0.08 | 0.08 |
| Sample | 1,691 | 1,244 | 35,892 | 32,872 |
| | | Panel C: Change in Cost of Capital | | |
| *Dependent Variable* | $\Delta r_{t,t+2}$ | $\Delta r_{t,t+2}$ | $\Delta r_{t,t+2}$ | $\Delta r_{t,t+2}$ |
| $Post_{i,t}$ | -0.002** | 0.003 | -0.001** | -0.000 |
| | (0.001) | (0.002) | (0.000) | (0.000) |
| $Treat_{i,t}$ | 0.003 | 0.005 | 0.000 | -0.000 |
| | (0.003) | (0.003) | (0.000) | (0.000) |
| $(Post \times Treat)_{i,t}$ | 0.001 | -0.001 | 0.001** | 0.001* |
| | (0.002) | (0.003) | (0.000) | (0.001) |
| Within R-square | 0.10 | 0.12 | 0.09 | 0.09 |
| Sample | 1,796 | 1,286 | 36,999 | 33,727 |

| | | | | | | |
|---|---|---|---|---|---|---|
| | | | Panel D: Discretionary News Release and Sell Transaction Predictability | | | |
| | (0,0) | (1,1) | (0,0) | (1,1) | (0,0) | (1,1) |
| | $\Delta ROA_{t,t+2}$ | $\Delta ROA_{t,t+2}$ | $\Delta Sentiment_{t-1,t+1}$ | $\Delta Sentiment_{t-1,t+1}$ | $\Delta r_{t,t+2}$ | $\Delta r_{t,t+2}$ |
| $(Treat \times Post)_{i,t}$ | -0.012** | 0.003 | -0.013 | -0.019 | 0.001** | 0.000 |
| | (0.005) | (0.004) | (0.028) | (0.033) | (0.000) | (0.001) |
| $(Treat \times Post \times News)_{i,t}$ | -0.002** | -0.001* | -0.010** | -0.011** | -0.000 | -0.000 |
| | (0.001) | (0.01) | (0.004) | (0.005) | (0.000) | (0.000) |
| $News_{j,m}$ | -0.000 | -0.000 | -0.002 | -0.002 | 0.000 | 0.000 |
| | (0.000) | (0.000) | (0.001) | (0.002) | (0.000) | (0.000) |
| Control and main levels | Yes | Yes | Yes | Yes | Yes | Yes |
| Within R-square | 0.05 | 0.07 | 0.08 | 0.08 | 0.09 | 0.10 |
| Sample | 33,538 | 32,521 | 35,892 | 32,872 | 36,999 | 33,726 |

**Table 7: Insider trading and tournament incentives**

The table reports the results of a shorter version of Kale et al. (2009) regression specification. In column (1) and (2), we regress Tobin's Q and ROA on all control variables with firm and year fixed effects, respectively. In column (3) to (6), we conduct a 2SLS regression with two first-stage regressions. Our endogenous variables are $pay\_gap_{j,t}$ and the interaction term between $pay\_gap_{j,t}$ and our insider trading intensity measure which is $all\_IT_{j,t}$. In the first stage regression, we employ the median $pay\_gap_{j,t}$ in the same sales quintiles and the interaction term between the $all\_IT_{j,t}$ and $pay\_gap_{j,t}$ as our two IVs in column (3) and (4). In column (5) and (6), we use the sum of the maximum federal and state long-term capital gain tax rates as the IV for $all\_IT_{j,t}$, and use the product between the tax rate and median $pay\_gap_{j,t}$ as the IV for the endogenous interaction term. In the second stage, we regress the Tobin's Q and ROA on all control variables with predicted $\widehat{pay\_gap}_{j,t}$, $\widehat{all\_IT}_{j,t}$ and predicted interaction term. The control variables in all six columns are $rd_{j,t}$, $sales_{j,t}$, $sales^2_{j,t}$, $capital\text{-}to\text{-}sales_{j,t}$, $advertising\text{-}to\text{-}sales_{j,t}$, $dividend\text{-}yield_{j,t}$, $leverage_{jt}$, $lnage_{j,t}$ and $skt\_ret\_voilatility_{j,t}$. Appendix A defines all the variables. We cluster standard errors, reported in parentheses at firm level. $^{***}$, $^{**}$, and $^{*}$ denote significance at the 99%, 95% and 90% confidence level, respectively. We winsorised all variables at the top 99% and bottom 1% level. All columns include firm and year fixed effects.

| | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** |
|---|---|---|---|---|---|---|
| | Fixed Effect | | 2SLS-Second Stage | | | |
| | | | One IV | | Two IVs | |
| *Dependent Variable* | Tobin's $Q_{j,t}$ | $ROA_{j,t}$ | Tobin's $Q_{j,}$ | $ROA_{j,y}$ | Tobin's $Q_{j,t}$ | $ROA_{j,y}$ |
| $pay\_gap_{j,t}$ | 0.014*** | 0.001*** | | | | |
| | (0.005) | (0.000) | | | | |
| $\widehat{pay\_gap}_{j,t}$ | | | 0.084*** | 0.002* | 0.168** | 0.015** |
| | | | (0.016) | (0.001) | (0.086) | (0.007) |
| $pay\_gap\widehat{\times all\_IT}_{j,t}$ | | | -0.008*** | -0.003*** | -0.037* | -0.005** |
| | | | (0.002) | (0.000) | (0.022) | (0.002) |
| $all\_IT_{j,t}$ | 0.021*** | 0.002*** | 0.088*** | 0.004*** | | |
| | (0.002) | (0.001) | (0.014) | (0.001) | | |
| $\widehat{all\_IT}_{j,t}$ | | | | | 0.383** | 0.029* |
| | | | | | (0.179) | (0.015) |
| *Other Control Variable* | Yes | Yes | Yes | Yes | Yes | Yes |
| First-Stage F-$\widehat{pay\_gap}_{j,t}$ only | | | 334.37*** | 345.28*** | 209.57*** | 209.60*** |
| Sanderson-Windmeijer F-$\widehat{pay\_gap}_{j,t}$ | | | | | 11.04*** | 11.14*** |
| Sanderson-Windmeijer F-Interaction | | | | | 10.37*** | 10.46*** |
| Sanderson-Windmeijer F -$\widehat{all\_IT}_{j,t}$ | | | | | 9.06*** | 9.11*** |
| Sample | 35,806 | 35,822 | 35,806 | 35,822 | 34,258 | 34,274 |

**Table 8: 2SLS regression result for purchase and sell transactions**

The table reports the results of the first and second stage 2SLS regressions. For the first stage regressions, the dependent variable in Panel A is $NPED_{i,t}$, a dummy variable equal to one for the non-promoted managers' buy/sell trades in the tournament year (0,0) and (1,1), zero for years outside the event window (-2, -1), and, in Panel B, the interaction term $NPED \times CEO\_IT_{i,t}$. We exclude transactions in year +2 to remove confounding events and transactions made by CEO and non-CEO competitors. The instrumental variable is the last fiscal year's previous CEO age, and the interaction term between previous CEO age and $CEO\_IT_{j,t}$. Panel C reports the results of the second stage regressions. In the second stage of IV regressions, we include *ret30, mom, bm, numest, illiq* and *size*, calculate at the end of the last month of the insider transaction date. We use the same control variables as in Table 3. $^{***}$, $^{**}$, and $^{*}$ denote significance at the 99%, 95% and 90% confidence level, respectively. All regressions include firm-month fixed effects. We do not report the coefficients of the mostly insignificant control variables $numest_{j,m-1}$, $pay\_gap_{j,t-1}$, $illiq_{j,m-1}$, $rd_{j,t-1}$, $bm_{j,m-1}$, $rating_{j,t-1}$, $delta_{p,t-1}$, and $roa_{j,t-1}$. All variables are detailed in Appendix and winsorised at the top 99% and the bottom 1%.

| | **Insider Purchase** | | **Insider Sell** | |
|---|---|---|---|---|
| Year *t* | (0,0) | (1,1) | (0,0) | (1,1) |
| Panel A First Stage - *Endogenous Variable is* $NPED_{i,t}$ | | | | |
| $Previous\_ceo\_age_{j,t-1}$ | $0.015^{***}$ | $-0.025^{***}$ | $0.010^{***}$ | $-0.018^{***}$ |
| | (0.004) | (0.004) | (0.001) | (0.001) |
| $Previous\_ceo\_age_{j,t-1} * CEO\_IT_{j,t}$ | 0.002 | $0.004^{***}$ | $-0.0004^{***}$ | $0.0003^{***}$ |
| | (0.001) | (0.001) | (0.0001) | (0.0001) |
| Other control and main covariates | Yes | Yes | Yes | Yes |
| Panel B First Stage - *Endogenous Variable is the interaction term* $NPED \times CEO\_IT_{i,t}$ | | | | |
| $Previous\_ceo\_age_{j,t-1}$ | $0.017^{***}$ | $-0.030^{***}$ | $0.019^{***}$ | $-0.058^{***}$ |
| | (0.004) | (0.005) | (0.002) | (0.003) |
| $Previous\_ceo\_age_{j,t-1} * CEO\_IT_{j,t}$ | $0.003^{*}$ | 0.003 | $-0.0006^{**}$ | $0.002^{***}$ |
| | (0.001) | (0.002) | (0.0002) | (0.0002) |
| Other control and covariates | Yes | Yes | Yes | Yes |
| Panel C Second Stage - *Dep Variable is BHAR_m_180, Endogenous Variables are* $(NPED)_{i,t}$ *and* $(NPED \times CEO\_IT)_{i,t}$ | | | | |
| $\widehat{NPED}_{i,t}$ | 5.161 | -0.648 | $-2.239^{**}$ | $-0.339^{***}$ |
| | (5.322) | (0.914) | (1.071) | (0.170) |
| $\widehat{NPED \times CEO\_IT}_{i,t}$ | -4.053 | 0.219 | $1.163^{**}$ | $0.107^{**}$ |
| | (4.329) | (0.718) | (0.561) | (0.054) |
| $CEO\_IT_{j,t}$ | 0.111 | $0.051^{***}$ | -0.026 | 0.001 |
| | (0.077) | (0.034) | (0.019) | (0.006) |
| $OutsiderD_{j,t}$ | -0.353 | -0.036 | $0.950^{**}$ | $0.159^{**}$ |
| | (0.227) | (0.117) | (0.450) | (0.065) |
| $COOD_{j,t}$ | 0.042 | -0.049 | 0.003 | $-0.002^{***}$ |
| | (0.058) | (0.042) | (0.008) | (0.025) |
| Sample | 2,441 | 2,628 | 37,757 | 40,785 |
| Control | Yes | Yes | Yes | Yes |
| Difference in Sargan C ($\chi^2$) | 2.16 | 2.65 | $7.219^{***}$ | $2.96^{*}$ |
| First-Stage F-$\widehat{NPED}_{i,t}$ | $26.63^{***}$ | $11.92^{***}$ | $98.85^{***}$ | $471.47^{***}$ |
| Anderson-Rubin Wald Test, F statistic | 2.84 | 2.51 | $18.97^{***}$ | 2.25 |

**Table 9: Robustness test**

The table reports the results obtained from running various robustness checks regressions. Panel A reports the regression results with the same control variable and fixed effect as in Table 8 Panel C but with OLS estimator only. In Panel B, we only keep two top highest paid non-CEO, and younger than 60, executives for each firm in each year. In Panel C, we only keep opportunistic transactions defined by following Cohen et al. (2012). In Panel D, we remove CFO transactions from our sample and replicate our previous results. In Panel E, we report the coefficients of $(\text{Post}\times\text{Treat})_{i,t}$ using alternative holding returns measures including raw cumulative return $ret_{t+1,t+i}$ and the 4-factor α multiplied by the median number of trading days of 22, 126, 252 in the three holding periods, respectively, calculated by running regression $r_{jt} - rf_t = \alpha_{it} + \beta_1(r_{crsp,t} - rf_t) + \beta_2 SMB_t + \beta_3 HML_t + \beta_4 UMD_t + \varepsilon_t$ from the day after insider transaction day to 3/6/12 month. $r_{crsp,t}$ is CRSP value-weighted market index and $UMD_t$ is up-minus-down factor (momentum). BHAR_size_m is the NYSE value-weighted size-decile adjusted return. In all four panels, we replicate our previous diff-in-diff regression in Table 3 and 2SLS regression in Table 8. We use the same control variables and fixed effects, but only report the coefficients of the variables of interest. $^{***}$, $^{**}$, and $^{*}$ denote significance at the 99%, 95% and 90% confidence level, respectively. All variables are winsorised at the top 99% and the bottom 1% level.

| | **Insider Purchase** | | **Insider Sell** | |
|---|---|---|---|---|
| | (0,0) | (1,1) | (0,0) | (1,1) |
| Panel A: Regression result without matching and difference-in-difference specification | | | | |
| $NPED_{i,t}$ | 0.080** | 0.071 | -0.027** | -0.039*** |
| | (0.040) | (0.052) | (0.013) | (0.015) |
| Control | Yes | Yes | Yes | Yes |
| Sample | 5502 | 5111 | 78,822 | 78,290 |
| Panel B: Alternative tournament contenders (two non-CEO executives, younger than 60) | | | | |
| *Diff-in-Diff regression* | | | | |
| $(\text{Post}\times\text{Treat})_{i,t}$ | 0.193* | 0.601 | -0.028* | -0.039*** |
| | (0.098) | (0.195) | (0.016) | (0.015) |
| Sample | 494 | 359 | 13,901 | 13,206 |
| *2SLS* | | | | |
| $\widehat{NPED}_{i,t}$ | 0.326 | 0.364 | -3.227** | -0.454* |
| | (0.754) | (1.594) | (1.613) | (0.264) |
| Sample | 723 | 793 | 14,639 | 16,001 |
| Panel C: Opportunistic transactions only | | | | |
| *Diff-in-Diff regression* | | | | |
| $(\text{Post}\times\text{Treat})_{i,t}$ | 0.090 | 0.190 | -0.024** | -0.035*** |
| | (0.071) | (0.143) | (0.011) | (0.012) |
| Sample | 907 | 654 | 24,841 | 23,871 |
| *2SLS* | | | | |
| $\widehat{NPED}_{i,t}$ | 0.026 | -0.500 | -1.773* | -0.258** |
| | (0.359) | (0.759) | (0.795) | (0.130) |
| Sample | 1,154 | 1,238 | 28,011 | 28,893 |
| Panel D: No CFO trades | | | | |
| *Diff-in-Diff regression* | | | | |
| $(\text{Post}\times\text{Treat})_{i,t}$ | 0.099* | 0.007 | -0.023** | -0.027** |
| | (0.060) | (0.098) | (0.011) | (0.013) |
| Sample | 1,509 | 1,129 | 31,374 | 20,532 |
| *2SLS* | | | | |

| | | | | |
|---|---|---|---|---|
| $\widehat{NPED}_{i,t}$ | 1.201* | 1.184 | -3.202* | -0.416*** |
| | (.0711) | (2.924) | (1.678) | (0.107) |
| Sample | 1,791 | 859 | 33,464 | 34,622 |
| Panel E: The coefficient of $(Post \times Treat)_{i,t}$ using alternative return measure | | | | |
| BHAR_m_30 | -0.014 | -0.021 | 0.001 | -0.005 |
| | (0.026) | (0.049) | (0.004) | (0.004) |
| BHAR_m_365 | 0.216*** | 0.325** | -0.047*** | -0.071*** |
| | (0.072) | (0.136) | (0.014) | (0.016) |
| $\alpha_{t+1,t+30}(\times 22)$ | -0.068* | -0.062 | 0.001 | -0.002 |
| | (0.037) | (0.076) | (0.005) | (0.005) |
| $\alpha_{t+1,t+180}(\times 126)$ | 0.081 | 0.079 | -0.022** | -0.023** |
| | (0.051) | (0.087) | (0.010) | (0.011) |
| $\alpha_{t+1,t+365}(\times 252)$ | 0.203*** | 0.209* | -0.037*** | -0.036** |
| | (0.060) | (0.113) | (0.014) | (0.015) |
| $ret_{t+1,t+30}$ | -0.024 | -0.028 | 0.001 | -0.009* |
| | (0.028) | (0.059) | (0.004) | (0.005) |
| $ret_{t+1,t+180}$ | 0.072 | 0.013 | -0.021** | -0.042*** |
| | (0.067) | (0.109) | (0.010) | (0.012) |
| $ret_{t+1,t+365}$ | 0.265*** | 0.093 | -0.048*** | -0.072*** |
| | (0.098) | (0.275) | (0.015) | (0.016) |
| BHAR_size_30 | -0.031 | -0.034 | 0.001 | -0.010** |
| | (0.028) | (0.056) | (0.004) | (0.005) |
| BHAR_size_180 | 0.066 | -0.004 | -0.022** | -0.046*** |
| | (0.065) | (0.014) | (0.010) | (0.012) |
| BHAR_size_365 | 0.288*** | 0.076 | -0.046*** | -0.070*** |
| | (0.096) | (0.278) | (0.015) | (0.016) |

**Appendix A: Definition of Variables**

| Variable Notation | Data Source | Definition |
|---|---|---|
| $\alpha_{t+1,t+i}$ | CRSP, French Data Library | The regression intercept: $r_{i,t} - rf_t = \alpha_{i,t} - \beta_1(r_{crsp,t} - rf_t) + \beta_2 SMB_t + \beta_3 HML_t + \beta_4 UMD_t + \varepsilon_t$ from the day after insider transaction day to 30/180/365 calendar days. $rf_t$ is the risk-free rate, $r_{crsp,t}$ is CRSP value-weighted market index, $SMB_t$ is small-minus-big factor (size), $HML_t$ is high-minus-low factor (value), and $UMD_t$ is up-minus-down factor (momentum). |
| advertising-to-sale$_{j,t-1}$ | Compustat | Advertising expenditure to sales (Compustat: xad to sale), zero otherwise. |
| age_ceo$_{j,\ t-1}$ | Execucomp | In fiscal year *t-1,* we identify the former CEO of firm *j*. The variable is her age in year *t-1*. If Execucomp does not report the age of manager any year, we use the age of the same manager in other years. |
| all_IT$_{j,t}$ | Insider Filling | The total number of non-CEO insider transaction for firm *j* in year *t*, zero otherwise. |
| analyst_talent$_{j,t-1}$ | I/B/E/S | The average talent of financial analysts that cover firm *j* in the last fiscal year. It is the innate ability of sell-side analysts measured by the analyst fixed effect from the regression on analysts' forecast accuracy. Calculated according to Dang et al. (2021) |
| BHAR_m_180$_{(d+1,\ d+180)}$ | CRSP | 180-calendar day Buy-N-Hold return adjusted by using the CRSP value-weighted market index, computed as: $BHAR_{mn} = \prod_{t=1}^{d}[1 + R_{jt}] - \prod_{t=1}^{d}[1 + R_{mt}]$ |
| bm$_{j,m-1}$ | CRSP, Compustat | The book-to-market ratio calculated as the ratio of last fiscal year's book value over the market capitalization in the last trading day in December. Book value is equal to stockholder equity + deferred taxes and investment tax credit (Compustat: txditc, zero if missing) - preferred stock value. Stockholder equity is parent stockholder equity (Compustat: seq), or total common equity (Compustat: ceq) plus total preferred stock capital (Compustat: pstk) or the difference between the total asset (Compustat: at) and total liability (Compustat: lt), in that order, as available. Preferred stock value is preferred stock redemption value (Compustat: pstkrv), or preferred stock liquidation value (Compustat: pstkl), or total preferred stock capital (Compustat: pstk), or zero, in that order as available. Negative bm ratio is restricted to zero. The ratio is calculated for firm *j* at the end of the last month. |
| capital_intensity$_{j,t-1}$ | Compustat | Capital expenditure over total asset (Compustat: capx over at ) |
| capital-to-sale$_{j,t-1}$ | Compustat | Net fixed asset to sales (Compustat: ppent to sale). |

| | | |
|---|---|---|
| $cash\_flow\_vol_{j,t-1}$ | Compustat-Quarterly | It is the seasonally adjusted standard deviation of cash flows over assets defined as EBITDA over total asset (Compustat: saleq- cogsq- xsgaq over atq) for a five-year window (*t, t+4*). We require there are at least a three-year data to compute this variable. For each of the four quarters in the year, we compute the mean values across the five-year window and then subtract these quarterly mean values to obtain the seasonally adjusted cash flows (Kini and Williams, 2012). |
| $CEO\_IT_{j,t}$ | Execucomp, Insider Filling | The number of quintiles of the net CEO selling value for firm *j* in year *t.* Net CEO selling value is the total value of selling transaction minus the total value of buying transaction executed by CEO in year *t* for firm *j.* If there is no CEO insider transaction in year *t*, the number is set to be 0. |
| $COOD_{j,t}$ | Execucomp | Dummy that is equal to one for firms that had a COO during the years $(-2, -1)$, and zero otherwise. We define COO as the manager who is younger than the incumbent CEO and whose job title (*titleann*) on Execucomp contains *chief operating office* or *chief operation officer* or *chief operations officer* or *chf operations officer* or *chf operation officer* or *che operating officer* or *coo* or *president* or/and *pres* |
| $delta_{p,t-1}$ | Execucomp | Dollar changes in wealth associated with a 1% change in the firm's stock price (in $000) for manager *p*. Calculated according to Coles et al. (2013). |
| $dividend\text{-}yield_{j,t-1}$ | Compustat | Dividends per share by ex-date divided by the close price for the fiscal year (Compustat: dvpsx_f over prcc_f). |
| $FERC_{j,t}$ | CRSP, Compustat | Dummy that is equal to one for firms in the top quantile of future earnings response coefficient calculated according to Tucker and Zarowin (2006), and zero for other firms. |
| $firm\_focus_{j,t-1}$ | Compustat-Segment | Dummy that is equal to one if the firm operates only in one segment and decreases as the firm diversifies (Kini and Williams, 2012), using Compustat segment sales according to their four-digit SIC code. |
| $high\_incentiveD_{p,t-1}$ | Execucomp | A dummy variable that is equal to one for high incentive managers, defined as managers *p* whose compensation differences between their CEOs and themselves are the largest three in firm *j* in year *t-1*. |
| $illiq_{j,m-1}$ | CRSP | Amihud's (2002) measure of illiquidity for firm *j* at the end of the last month. The measure is calculated as the monthly average of the daily ratio of absolute stock return to dollar volume. |
| $independent\_manager_{j,t-1}$ | Boardex | Percentage of independent managers on the company board. |
| $independent\_committee_{j,t-1}$ | Boardex | Percentage of independent managers on the company compensation committee. |

| | | |
|---|---|---|
| $institution\_ownership_{j,q-1}$ | Thomson Reuter 13F Holding | Percentage of shares owned by institution investors over total shares outstanding in the last quarter. |
| $leverage_{j,t}$ | Compustat | Long term debt plus debt in current liability) over the total assets (Compustat: (dltt+dlc)/at)) |
| $lnage_{p,t}$ | Execucomp | The natural logarithm of the current age of the manager *p* in year *t.* |
| $lncompen_{p,t-1}$ | Execucomp | The natural logarithm of *tdc1* adjusted by following Coles et al. (2014) and Brockman et al. (2016). |
| $mom_{j,(d-31,d-364)}$ | CRSP | The cumulative raw return from (d-395, d-31), insider transaction occurs in day d. If there are less than 243 trading days in the event window, the variable is set to be missing. |
| $News_{j,m}$ | Key Development | The number of discretionary news (Edmans et al., 2018), released in insider trading month *m* for firm *j*. |
| $NPED_{i,t}$ | Execucomp | Dummy variable equals one for non-promoted insider buy or sell trades in the event year *t* zero for years other than *t*. *t* takes the value of 0, 1 in the study. |
| $NPV_{p,d}$ | Insider Filling | Net purchasing value in day *d* executed by insider *p*, calculated as the ratio of net dollar amount of insider trades over the total dollar amount of insider transactions. |
| $numest_{j,m-1}$ | I/B/E/S | Analyst coverage defined as the number of analysts that report a forecast for the next 1-fiscal year earnings per share for firm *j* at the end of the last month. For no earning forecast, the variable is set to zero. |
| $opp\_D_{i,t}$ | Insider Filling | Dummy variable equal to one for opportunistic insider transactions, defined, following Cohen et al. (2012), as a trade executed by insiders who had made at least one transaction in the same calendar year in the past three consecutive years. We reclassify insiders at the beginning of each calendar year. |
| $OutsiderD_{j,t}$ | Execucomp | Dummy that is equal to one for insider transactions for firms that appointed outside CEO who did work in the company in the years (-5, -2) during the year $(0, 1)$, and zero otherwise. |
| $pay\_gap_{j,t-1}$ | Execucomp | The natural logarithm of the difference between the CEO total compensation (*tdc1*) and the median total compensation of other non-CEO managers covered by Execucomp in firm *j* in the last fiscal year. We follow Kini and Williams (2012) and remove former CEO who remains in the firm as an executive and gets excessive perks (Cohan, 2022) when identifying the median non-CEO manager pay. |
| $pay\_rank_{p,t-i}$ | Execucomp | The rank of non-promoted manager sorted by their total compensation in year -1 among all tournament competitors in the same firm. |

| | | |
|---|---|---|
| $rating_{j,\ t-1}$ | Compustat | The average monthly S&P long-term issuer credit rating of firms in the same Fama-French 48 industry in the last fiscal year. Following Peters and Wagner (2014), we assign AAA a value 2 to CC a value of 23, then scale them by 9, so that a unit increase in the scaled rating corresponds to a change in rating from AAA to BBB or BBB to CCC. |
| $rd_{j,t-1}$ | Compustat | Research and development expense calculated as the research and development expense (Compustat: xrd) over sales (Compustat: sale) for firm *j* at the end of the last fiscal year. It is zero if missing in Compustat. |
| $ret30_{j,(d-1,d-30)}$ | CRSP | The cumulative raw return from (d-30, d-1), insider transaction occurs in day d. If there are less than 20 trading days in the event window, the variable is set to be missing. |
| $roa_{j,t-1}$ | Compustat | Return on asset calculated as the net income (Compustat: ni) after taking out preferred dividend (Compustat: dvp), over the total asset (Compustat: at) for firm *j* at the end of the last fiscal year. |
| $sale_{j,t-1}$ | Compustat | The natural logarithm of the sale (Compustat: sale). |
| $size_{j,m-1}$ | CRSP | The logarithm of market capitalization defined as adjusted stock price times adjusted shares outstanding for firm *j* at the end of the last month. The number is reported in a million. |
| $skt\_ret\_volatility_{j,t-1}$ | CRSP | Variance of 60 monthly returns preceding the sample year *t-1* |
| $Synch_{j,t}$ | CRSP | Dummy that is equal to one for firms in the top quantile of return synchronicity calculated according to Piotroski and Roulstone (2004), and zero for other firms. |
| tobin's $Q_{j,t-1}$ | Compustat | Market value of equity plus book value of debt-deferred tax over book value of total assets (Compustat: (at+csho×prcc_f-ceq-txdb)/at)) |
| total $asset_{j,t-1}$ | Compustat | Logarithm of the total asset (Compustat: at) in the last fiscal year, used to match our treated firms. |
| $vega_{p,t-1}$ | Execucomp | Dollar changes in wealth associated with a 0.01 change in the standard deviation of the firm's returns (in $000) for manager *p*. Calculated according to Coles et al. (2013). |

# Internet Appendix

## Executive equity-based compensation and tournament incentives

Additional Supporting Information is in the online version of this article at the publisher's website.

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**Internet Appendix S1: Data cleaning process details and variable construction**

*S1.1 Estimation of the value of executive options*

Coles, Daniel and Naveen (2014) point out that Execucomp's total compensation figure is not comparable before and after 2006 because of the passage of Financial Accounting Standards Board (FASB) 123R revision to the stock and options accounting and an expanded compensation disclosure requirement regarding the manager compensation disclosure. We follow Coles et al. (2014), Kini and Williams (2012) and Brockman, Lee and Salas (2016) to correct our pre- and post-2006 total compensation item *tdc1*[20]. Specifically, for the pre-2006 period, the stock option is based on the Black-Scholes formula. However, in the post-2006 period, firms report its fair value. A small number of firms still report their proxy statements in the old reporting format in 2006. We use the reporting flag (*old_datafmt_flag*) to identify these firms. Then, we correct the post-2006 period option value using the same set of the following Black-Scholes assumption that Execucomp used for the pre-2006 period:

1. The strike price per share (*expric*) is specified in the proxy statement.
2. The market price per share (*mktprice*) is equal to the strike price per share, unless if it is specified in the proxy statement.
3. The option is granted on July 1st of each particular year for which data was reported. The option's nominal term is the period between July 1st of the year of grant and the expiration date (*exdate*) reported in the proxy statements. We round the nominal term to the nearest year figure. However, the option's term was reduced to 70% of its nominal term as managers rarely hold their stock options until their expiration year. Since the expiration

[20] Our results remain robust if we do not correct for the FSBA change and use raw figures reported by Execucomp.

date is not available on Execucomp for the post-2006 reporting format, we follow Kini and Williams (2012) to assume all options have seven years until expiration.

4. The risk-free rate corresponding to the option's maturity is the historical annual series of treasury constant maturity with 7-year term downloaded from the Federal Reserve[21].
5. The individual stock price volatility is the annualized volatility calculated using the last 60 months, winsorised at the top and bottom 5%. To calculate the volatility, Execucomp requires at least 12-month return data. For stocks that are traded less than 12 months, the stock volatility is the average volatility value for the firms in the S&P 1500 index.
6. Execucomp uses the average dividend yield in the last three years to calculate the estimated future dividend yield. It is then winsorised at the top and bottom 5%.

We find that the correlation between our Black-Sholes value and the Black-Sholes value calculated by Execucomp is 95.9%[22]. We further recalculate all option awards for both the pre- and post-2006 periods by using the same set of Black-Sholes assumptions to ensure consistency.

We then follow Brockman et al. (2016) to value the ex-ante value of stock awards. We multiply the number of performance shares granted to the CEO (*shrtarg*) by the firm's fiscal year-end stock price (Compustat *prcc_f*). We recalculate total compensation, *tdc1*, for all firm-year observations that reported in the pre-2006 old format (item *old_datafmt_flag=1*) by adding salary (*salary*), bonus (*bonous*), other annual compensation (*othann*), restricted stock grant (*rstkgrnt*), all other total (*allothtot*), the fair value of stock awards (*shrtarg*×*prcc_f*) and the Black-Scholes value of option grant (*option_awards_blk_value*). For firms that reported in the post-2006 new format (item *old_datafmt_flag=0*), *tdc1* is the sum of salary (*salary*), bonus (*bonous*), non-equity

[21] https://www.federalreserve.gov/datadownload/Choose.aspx?rel=H15
[22] Kini and Williams (2012) report a correlation of 96.8% for 2005. The difference is possibly due to different risk-free rate sources, which they do not report.

incentive plan compensation (*noneq_incent*), fair value of stock awards (*stock_awards_fv*), all other compensations (*othcomp*), deferred earnings (*defer_rpt_as_comp_tot*) and the Black-Scholes value of option grant.

To build a link table between Execucomp and Insider Filling, we first obtain all historical *cusip* codes using the CRSP/Compustat link table. Second, for a given manager in Execucomp, we match the manager with all the managers who have traded the security with the same *cusip*. Third, we calculate the Damerau-Levenshtein (DL) distance and vectoral decomposition (VD) of texts with single gram and root weighting scheme between the name of the manager provided by Execucomp and reported by Insider Filling. We sort these matches by DL distance and VD score to manually verify each pair of *execid-personid* match.

*S1.2. Estimation of short and long investment horizons of insiders*

To identify short horizon sellers, we modify the investment horizon measure proposed by Akbas, Jiang and Koch (2020). Firstly, we define HOR as:

$$\mathrm{HOR}_{\mathrm{p,j,t}} = \frac{\sum_{\mathrm{Year-8}}^{\mathrm{Year-1}} \mathrm{NPV_t}}{\mathrm{N}}$$

That is, for each year, we compute the annual NPV for each insider *p* in firm *j* in year *t* in the last eight calendar years. Then, we compute the average NPV by summing the annual NPV and divide by the number of calendar years that an insider has traded in the last eight calendar years. HOR can only take a value between -1 and +1, which are the bounds of the NPV. If an insider only sold (bought) in the last eight years, then each of its NPV is -1 (1), and therefore, the average will be -1 (1) as well. We define SH sellers as those with negative, but larger than the median $\mathrm{HOR}_{\mathrm{p,j,t}}$, after excluding the $\mathrm{HOR}_{\mathrm{p,j,t}}$ of -1 which account for more than 50% of the insider sell sample. We restrict SH sellers must have traded at least in three different years in the past eight years.

*S1.3. Estimation of insiders' probability of becoming a CEO*

We run the following cross-section regression model to estimate the probability of becoming a CEO:

$$\begin{aligned} CEOD_{p,t} = \alpha + \beta_0 lncompen_{p,t-1} + \beta_1 age_{p,t-1} + \beta_2 tenure_{p,t-1} + \beta_3 exp_{p,t-1} + \beta_4 maleD_p \\ + \beta_5 COO_{p,t-1} + \beta_6 COO\_firm_{j,t-1} + \beta_7 bm_{j,t-1} + \beta_8 momentum_{j,t-1} + \beta_9 roa_{j,t-1} \\ + \beta_{10} outsider_{j,t} + e_t \end{aligned}$$

The dependent variable is a dummy variable one for the manager who became CEO in the year, and zero for insiders who failed to become CEO. In each year *t,* we obtain a list of firms that have a CEO turnover event and keep all insiders except the former founders/co-founders, former CEOs and new joiners. $lncompen_{p,t-1}$ is a one-year lagged adjusted total compensation. $tenure_{p,t-1}$ is the lagged number of years the manager worked for the firm. $exp_{p,t-1}$ is the lagged number of years the manager has worked for any firm in the entire Execucomp. $maleD_p$ is a dummy variable equal to one for male, and zero for female. $COO_{p,t-1}$ is a dummy variable equal to one for COO as identified using manager's title, and zero otherwise. $COO\_firm_{j,t-1}$ is a dummy variable equal to one if the firm had a COO before the turnover, and zero otherwise. $outsider_{j,t}$ is a dummy equal to one if the firm hired an external CEO in year *t*, and zero otherwise. We use the estimated coefficient to calculate the estimated probability $Probability_{p,t}$ of a given insider *p* to become the CEO in the year *t*, and re-estimate the cross-section every year using only the firm that had a CEO turnover in year *t*. We intentionally use public information only to estimate these coefficients because we assess that tournament contenders in other firms will not have access to the private information that the board of directors in the CEO turnover firms possessed at the time of CEO turnover. These tournament contenders will estimate their subjective probabilities of winning the CEO competition

in their firms using the latest CEO promotion winner's characteristics from other firms, in line with Kale et al. (2009). We include many firm level variables to estimate the probability of becoming CEO because the attractiveness of the CEO position also depends on the operating condition of the firm, as, if the firm is financially distressed, only fewer incumbent executives are willing to take the CEO position.

*S1.4. Estimation of Future Earnings Response Coefficient (FERC)*

We follow Tucker and Zarowin (2006) and Wang (2019) to construct the FERC by first estimating the following equation:

$$R_{j,t} = \alpha + \beta_0 X_{j,t-1} + \beta_2 X_{j,t} + \beta_3 \left(X_{j,t+1} + X_{j,t+2} + X_{j,t+3}\right) + \beta_3 R_{j,t+3} + \varepsilon_{j,t}$$

where $X_{j,t}$ is the basic annual earnings per share excluding extraordinary items (*epspx*), adjusted for stock splits and stock dividends and deflated by the stock price at the beginning of the fiscal year *t*. $R_{j,t}$ is the firm's annual return beginning at the fiscal year *t*. $R_{j,t+3}$ is a three-year future return for the firm from fiscal year *t+1* to *t+3*. The coefficient of the sum of the future three-year earnings per shares $\beta_3$ is the FERC. We truncate all variables at the top and bottom 1%. A higher $\beta_3$ means the current stock return impounds more future earnings information and is more informative for future earnings and *vice versa.* We follow Wang (2019) to estimate a rolling panel regression using the trailing 36 months across each two-digit SIC industry. We restrict that there are at least 8 (24) months in $R_{j,t}$ ($R_{j,t+3}$) for a stock to be included in the regression and create binary variable FERC that is one for the top quintile of the $\beta_3$ and zero otherwise.

*S1.5. Estimation of stock return synchronicity*

We follow Piotroski and Roulstone (2004) and estimate the stock return synchronicity from the following equation:

$$\mathrm{FirmRET_{j,t}} = \alpha + \beta_1 \mathrm{MktRET_{j,t}} + \beta_2 \mathrm{MktRET_{j,t-1}} + \beta_3 \mathrm{IndRET_{k,t}} + \beta_4 \mathrm{IndRET_{k,t-1}} + \varepsilon_{i,t}$$

where $\mathrm{MktRET_{j,t}}$ is the market return proxied by the CRSP value-weighted buy-and-hold market return in year t. $\mathrm{IndRET_{k,t}}$ is the value-weighted average industry buy-and-hold return identified using the two-digit SIC code in year *t*. We estimate the regression for each firm-year observation with weekly returns data and restrict a minimum of 45 weekly observations each year. The synchronicity is measured as $\ln\left(\frac{R^2}{1-R^2}\right)$, where $R^2$ is the R square of the above regression. A higher $Synch_{i,t}$ indicates the current firm returns comove strongly with the current and lagged market and industry returns, which further indicates the stock price contains less firm-specific information.

*S1.6. Estimation of the changes in investor sentiment*

To measure the changes in investor sentiment denoted as $\Delta\mathrm{Sentiment}$, we compute the market-to-book ratio decomposition of Rhodes–Kropf, Robinson and Viswanathan (2005) defined as the residual from the following regression

$$\ln(\mathrm{market\_value})_{j,t} = \alpha + \beta_{1z,t}\ln(\mathrm{book\_value})_{j,t} + \beta_{2k,t}\ln(\mathrm{net\_income})^{+}_{j,t} + \beta_{3k,t} I_{(<0>)}\ln(\mathrm{net\_income})^{+}_{j,t}$$

$$+\beta_{4k,t}\mathrm{leverage}_{j,t} + \varepsilon_i$$

where subscript *k* indexes for Fama-French 12 industries, *j* for firms and *t* for year. We estimate the regression for each industry-year. $I_{(<0>)}$ is a dummy variable equal to one for loss-making firms, and zero otherwise. The firm-specific residual obtained from the regression is the part of the firm's market value not explained by fundamentals or by changes in the market valuation common across firms in the same industry. We follow Cziraki et al. (2021) to measure the change in sentiment between $(\mathrm{t}-1, \mathrm{t}+1)$ with year *t* as insider trading year.

*S1.7. Estimation of the changes in the cost of capital*

We estimate the following modified Fama and French (1993) three-factor model, following Czirakiet al. (2021), to measure the changes in the cost of capital:

$$r_{j,t}-r_{f,t}=\alpha_{-j}+\alpha_{\Delta j}D_t+b_{-j}(r_{m,t}-r_{f,t})+b_{\Delta j}D_t(r_{m,t}-r_{f,t})+s_jSMB_t+s_{\Delta j}D_tSMB_t+h_{-j}HML_t+h_{\Delta j}D_tHML_t+e_t$$

where $r_{j,t}$ is the monthly stock return, $r_{f,t}$is the return on 1-month U.S Treasury bill, $r_{m,t}$ is the CRSP value-weight market index, $SMB_t$ and $HML_t$ are the returns on the size and book-to-market ratio portfolios. $D_t$ is a dummy variable that equals one if the year is in $(0,1)$, and zero for years in $(-3,-1)$. We use years $(-3,2)$ to estimate the cost of capital prior and after the CEO turnover. The expected change of cost of capital is obtained using the estimated coefficient of $\hat{\alpha}_{\Delta i}$ plus the product between $\hat{b}_{\Delta j}$, $\hat{s}_{\Delta j}$, $\hat{h}_{\Delta j}$ and the corresponding average factor premium estimated using all firms in CRSP database between 1993 and 2019[23].

$$\Delta r_{t,t+2} = \hat{\alpha}_{\Delta j}+ \hat{b}_{\Delta j}\overline{(r_{m,t}-r_{f,t})}+ \hat{s}_{\Delta j}\overline{SMB}_t+\hat{h}_{\Delta j}\overline{HML}_t$$

*S1.8. Estimation of the board conservatism*

To measure the board conservatism, we follow the Khan and Watts (2009) to compute the C_Score, which is based on Basu (1997). We first estimate the annual cross-sectional regression model as follows:

$$\begin{aligned} X_i = {} & \beta_1 + \beta_2 D_j + R_j(\mu_1 + \mu_2 Size_j + \mu_3 MB_j + \mu_4 Lev_j) \\ & + D_j R_j(\lambda_1 + \lambda_2 Size_j + \lambda_3 MB_j + \lambda_4 Lev_j) \\ & + (\delta_1 Size_j + \delta_2 MB_j + \delta_3 Lev_j + \delta_4 D_j Size_j + \delta_5 D_j MB_j + \delta_6 D_j Lev_j) + \varepsilon_j \end{aligned}$$

[23] The average factor premium in our sample is 0.007 for $\overline{(r_{m,t}-r_{f,t})}$, 0006 for $\overline{SMB_t}$ and 0.002 for $\overline{HML_t}$.

where $X_i$ is the income before extraordinary items (*ib*) scaled by the lagged market value of equity (*csho*prcc_f*). $D_j$ is a dummy variable equals one for firm-years with negative cumulative returns, zero otherwise. $R_j$ is the 12-month cumulative abnormal return for the firm in the same fiscal year. $Size_j$ is the natural log of market value of equity. $MB_j$ is the ratio of market value of equity to book value of equity (*ceq*) at the end of the year. $Lev_j$ is the leverage, defined as long-term debt (*dltt*) plus short-term debt (*dlc*) over the market value of equity. After estimating the regression, we calculate the C_Score as following:

$$\text{C\_Score} = \hat{\lambda}_1 + \hat{\lambda}_2 \text{Size}_j + \hat{\lambda}_3 \text{MB}_j + \hat{\lambda}_4 \text{Lev}_j$$

*S1.9. Estimation of insiders' dissimulation strategies*

Huddart, Hughes and Levine (2001) argue that insiders dissimulate their private information by randomly trading in a manner inconsistent with their informational agent role. If their private information is long-lived,[24] they will intentionally make noisy transactions to thwart outsiders who intend to follow them. Biggerstaff et al. (2020) report that insiders incorporate their private negative information into multiple/sequential sell trades, executed at most 30 days apart, to minimize the price impact, and the return of the last transaction in a sequence is more negative than isolated sell trades. The dissimulation strategy is only effective to disguise the negative private information embedded in sell trades, not the positive one in buy trades. We follow Biggerstaff et al. (2020) to adjust BHAR.

In un-tabulated univariate statistics, we find that 48.9% of all sell trades are sequential sells, which typically last for 23 days with an average of eight transactions, and only 7% contain both buy and sell trades. This is due to the short-swing rule which prevents insiders

[24] Insiders with short-lived information, which is revealed quickly to the market, cannot adopt this strategy.

from realizing profit from two offsetting transactions in the first six months after the first trade. All our results are robust if we remove buy trades and solely focus on sequential sell trades. We re-estimate 2SLS regression with the adjusted BHAR_m_180 based on all sequential and isolated sell transactions. Our overall results remain unchanged, but the coefficients of $\widehat{NPED}$ become more negative in all two post-event years for sells, implying that the losing tournament effect is stronger after controlling for insider dissimulation strategy. We further test for robustness by substituting the BHAR_m_180 from the last trade in a sequence for all sequential trades in the same sequence. We also extend the holding period for sequential sells from 1 day after the first trade to 180 days after the last trade. Since the holding horizon varies depending on the sequence length, we multiply the daily average BHAR_m_180 by 126, the median number of trading days. The coefficients of $\widehat{NPED}$, not reported for brevity, remain negative and statistically significant.

*S1.10. Placebo tests*

To conduct placebo tests for 2SLS, we randomly select 400 firm-year observations with at least one insider purchase transaction and 1,600 firm-year observations with at least one insider sell transaction considered as CEO turnover year for insider purchase and sell sample, respectively, the nearest hundreds for the actual numbers, of 386 and 1,601 in year 0, of distinct CEO turnover firm-year observations. We remove firm-year observations with actual CEO turnover event and the following two years from our sample pool. For each firm-year observation, we match the insider trading transactions in the given year and set $NPED_{i,t}$ to be one for all insider transactions in the year.

To conduct placebo tests for diff-in-diff regression, we first randomly select 1,000 firm-year observations without CEO turnover and not in any CEO turnover window. We then match these treated firms with one control firm with placement in the same year *t* using the same matching algorithm. We assume year *t* to be the event year. We estimate a diff-in-diff regression by using the observations of matched sample for years$_{t-2,\ t}$. We conduct placebo tests for insider purchase and sell samples separately. We restrict the treated firm cannot match to itself in the last year.

**Internet Appendix S2: Sample size across different database**

| | Unique *execid* | Unique *personid* | Sample Size |
|---|---|---|---|
| Raw Execucomp Sample | 48,429 | | 269,456 |
| Match with *execid-personid* link table | 43,952 | 44,187 | 277,113 |
| Match with CRSP both insider purchase and sale, including CEO | 26,570 | 26,617 | 257,033 |
| Match with CRSP both insider purchase and sale, excluding CEO | 24,275 | 24,310 | 188,960 |
| Remove new joiner, previous CEO, co-founders/founders | 21,723 | 21,764 | 165,295 |
| Remove trades without CEO turnover or firms with no control firms or outside CEO tournament event window (0,1) | 3,976 | 3,985 | 13,894 |
| Valid insider purchase sample for Non-Promoted Manager in (0,1) with matched firm | 348 | 350 | 832 |
| Valid insider sell sample for Non-Promoted Manager in (0,1) with matched firm | 3,690 | 3,696 | 13,062 |

**Internet Appendix S3: CEO turnover summary**

The table shows a summary of the time series distribution of CEO turnover events and insider transactions. We use Execucomp historical annual CEO flag (*ceoann*) to identify CEO turnover events. In columns (2) and (4), we report the number of internal promotions and CEO turnovers after removing the confounding events. We define an external CEO promotion if the incoming CEO has not worked for the firm within the event window of (-5, -2). In columns (5) to (8), we exclude all CEO trades and report the unconditional non-CEO insider transactions with matched firms. In columns (7) and (8) we aggregate insider buy and sell trades at the daily frequency by using the closing price at the transaction day times the number of shares bought/sold to compute the individual transaction value and report the yearly average insider transaction value.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| **Fiscal Year** | No. Isolated CEO Turnover | No. Isolated internal Promotions | Isolated Non-CEO Manager Trade | Isolated CEO Turnover with Insider Trading | Non-CEO Insider Purchase Sample with control firm | Non-CEO Insider Sell Sample with control firm | Average non-CEO Insider Purchase Value ($000) | Average non-CEO Insider Sell Value ($000) |
| 1996 | | | 10,045 | | 711 | 4,011 | 138.23 | 1,408.52 |
| 1997 | 136 | 65 | 10,184 | 65 | 840 | 5,468 | 156.54 | 910.07 |
| 1998 | 146 | 31 | 10,586 | 95 | 1,170 | 5,277 | 113.10 | 964.49 |
| 1999 | 122 | 23 | 9,951 | 87 | 1,188 | 5,061 | 109.77 | 1,322.45 |
| 2000 | 160 | 34 | 9,269 | 104 | 988 | 6,297 | 181.07 | 1,517.59 |
| 2001 | 179 | 33 | 9,250 | 112 | 559 | 6,786 | 94.05 | 867.65 |
| 2002 | 113 | 23 | 9,451 | 73 | 708 | 5,700 | 75.42 | 686.37 |
| 2003 | 137 | 25 | 9,677 | 87 | 503 | 7,922 | 93.61 | 910.97 |
| 2004 | 131 | 24 | 8,766 | 82 | 327 | 8,923 | 150.71 | 960.54 |
| 2005 | 147 | 29 | 7,281 | 97 | 294 | 7,603 | 345.33 | 1,043.40 |
| 2006 | 132 | 33 | 8,765 | 88 | 329 | 9,267 | 278.93 | 987.41 |
| 2007 | 170 | 46 | 10,488 | 119 | 646 | 9,960 | 221.14 | 923.73 |
| 2008 | 197 | 54 | 10,046 | 122 | 1,001 | 6,287 | 161.35 | 825.85 |
| 2009 | 153 | 29 | 9,506 | 93 | 588 | 5,811 | 63.87 | 608.25 |
| 2010 | 123 | 32 | 9,289 | 77 | 298 | 7,125 | 123.84 | 736.35 |
| 2011 | 150 | 24 | 9,132 | 89 | 566 | 8,035 | 238.71 | 792.32 |
| 2012 | 164 | 32 | 9,006 | 110 | 485 | 8,672 | 81.88 | 876.73 |
| 2013 | 160 | 45 | 8,918 | 107 | 248 | 9,644 | 531.51 | 966.48 |
| 2014 | 152 | 47 | 8,805 | 107 | 296 | 7,208 | 171.67 | 1,068.98 |
| 2015 | 150 | 40 | 8,448 | 104 | 399 | 5,129 | 301.97 | 1,087.62 |
| 2016 | 162 | 31 | 8,052 | 110 | 282 | 3,889 | 176.48 | 1,005.09 |
| 2017 | 144 | 40 | 7,588 | 96 | 214 | 4,125 | 254.86 | 1,057.52 |
| 2018 | 142 | 18 | 7,311 | 53 | 72 | 1,328 | 175.32 | 1,232.57 |
| 2019 | 158 | 34 | 6,550 | 92 | 310 | 2,745 | 259.11 | 1,204.34 |
| All | 3,428 | 2,636 | 216,364 | 2,169 | 13,022 | 152,273 | 162.88 | 969.29 |

**Internet Appendix S4: Post-transaction returns of other directors**

This table reports the BHAR_m_180 for CEO and Others insider transaction sample. For each treated firm, we collect the CEO transactions and all other directors' transactions excluding tournament competitors. We compute and report the post-transaction return proxied by BHAR_m_180. We winsorize the BHAR_m_180 at the top 99% and the bottom 1% level.

| | Purchase Sample | | | | Sell Sample | | | |
|---|---|---|---|---|---|---|---|---|
| **Event Year** | -2 | -1 | 0 | 1 | -2 | -1 | 0 | 1 |
| **CEO** | 0.069 | 0.079 | 0.254 | 0.028 | 0.091 | 0.041 | 0.034 | 0.042 |
| **No.** | 128 | 202 | 281 | 84 | 3,963 | 4,515 | 1,139 | 1,222 |
| **Others** | 0.142 | 0.233 | 0.106 | 0.120 | 0.067 | 0.044 | 0.054 | 0.054 |
| **No.** | 585 | 1,153 | 1,049 | 762 | 4,919 | 8,483 | 8,238 | 5,897 |

**Internet Appendix S5: Test of the parallel trend assumption**

Panel A reports the summary statistics at firm level for both the treated firms and control firms in the pre-CEO turnover period (-2, -1) and Panel B shows summary statistics of BHAR in event window (-2, +1). Firms that have CEO turnover event in year t are matched with firms on the average insider purchase/sell profitability, logarithm of the total asset and the book-to-market ratio in the fiscal year t-1 using Mahalanobis distance. Columns (3) and (6) report the t-test results by assuming unequal variance between treated and control firms for insider purchase and sell transaction, respectively. In Panel C, we follow Angrist and Pischke (2009) and Cengiz et al. (2019) to conduct an event-study type diff-in-diff regression and formally test on the parallel trend assumption. Variable $Pre_t$ equals to 1 for treated firms in year *t,* and zero otherwise. Year *t* refers to the year in our event window with year *0* as the CEO turnover occurred. Variable $Post_t$ is defined with the same logic. The coefficients of $Pre_{-1}$ should be statistically insignificant for the parallel trend assumption to hold. We drop one pre-treated period to avoid perfect multicollinearity. Columns (1) and (2) focus on insider purchase and sell transactions, respectively. We control for firm, year, and cohort fixed effects. Standard errors are clustered at the firm-month level. $^{***}$, $^{**}$, and $^{*}$ denote significance at the 99%, 95% and 90% confidence level, respectively. All variables are winsorised at the top 99% and the bottom 1% level.

**Panel A: Summary Statistics in Pre-Treatment Period (-2, -1) at firm level**

| | Insider Purchase Transactions | | | Insider Sell Transactions | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Treated Firms | Control Firms | Difference (1)-(2) | Treated Firms | Control Firms | Difference (4)-(5) |
| $\Delta$BHAR_m_180$_{(-2,-1)}$ | -0.033 | -0.009 | 0.024 | -0.004 | -0.004 | -0.000 |
| | (0.013) | (0.012) | (0.018) | (0.002) | (0.002) | (0.001) |
| total asset$_{j,t-1}$ | 7.322 | 7.238 | 0.083 | 8.000 | 7.943 | 0.056 |
| | (0.085) | (0.081) | (0.118) | (0.029) | (0.028) | (0.040) |
| mom$_{j,\ t,(d-31,d\ -364)}$ | 0.148 | 0.184 | -0.036 | 0.176 | 0.192 | -0.015 |
| | (0.025) | (0.020) | (0.033) | (0.007) | (0.007) | (0.010) |
| bm$_{j,m-1}$ | 0.634 | 0.634 | 0.000 | 0.492 | 0.488 | 0.003 |
| | (0.019) | (0.022) | (0.029) | (0.007) | (0.007) | (0.010) |
| roa$_{j,t-1}$ | 0.027 | 0.033 | -0.006 | 0.053 | 0.055 | -0.002 |
| | (0.001) | (0.000) | (0.007) | (0.002) | (0.002) | (0.003) |
| Non-CEO total comp ($000s) | 1,231 | 1,325 | -94.04 | 2,115 | 1,971 | 144$^{***}$ |
| | (59.62) | (92.52) | (110.06) | (20.24) | (17.69) | (26.89) |
| Transaction Value | 156,920 | 89,887 | 67,032$^{***}$ | 1,004,076 | 1,039,358 | 35,285 |
| | (16,169) | (19,477) | (25,314) | (18,873) | (20,050) | (27,535) |
| N Matched Firm-Year | 192 | 192 | | 1331 | 1331 | |
| N Transactions. | 834 | 889 | | 17,153 | 17,804 | |

| Panel B: Summary Statistics of BHAR in pre- and post-event period | | | | | | |
|---|---|---|---|---|---|---|
| $BHAR\_m\_180_{(t = -2)}$ | 0.045 | 0.015 | 0.030 | 0.031 | 0.029 | -0.002 |
| | (0.022) | (0.017) | (0.028) | (0.003) | (0.003) | (0.002) |
| $BHAR\_m\_180_{(t = -1)}$ | 0.033 | 0.061 | -0.028 | 0.016 | 0.017 | 0.001 |
| | (0.016) | (0.013) | (0.020) | (0.003) | (0.002) | (0.004) |
| $BHAR\_m\_180_{(t = 0)}$ | 0.205 | 0.094 | 0.111*** | -0.006 | 0.015 | -0.022*** |
| | (0.017) | (0.015) | (0.023) | (0.002) | (0.003) | (0.003) |
| $BHAR\_m\_180_{(t = +1)}$ | 0.098 | 0.196 | -0.098*** | -0.013 | 0.021 | -0.034*** |
| | (0.026) | (0.030) | (0.040) | (0.002) | (0.003) | (0.004) |
| **Panel C. event-study type diff-in-diff regression** | | | | | | |
| | (1) | (2) | (3) | (4) | | |
| | BHAR_m_180 | BHAR_m_180 | BHAR_m_180 | BHAR_m_180 | | |
| $Pre_{-2}$ | -0.018 | 0.023 | 0.007 | 0.012 | | |
| | (0.060) | (0.046) | (0.006) | (0.008) | | |
| $Pre_{-1}$ | | | | | | |
| $Post_0$ | 0.131*** | 0.111*** | -0.038*** | -0.026*** | | |
| | (0.042) | (0.039) | (0.006) | (0.009) | | |
| $Post_1$ | 0.106 | 0.047 | -0.024*** | -0.019* | | |
| | (0.066) | (0.054) | (0.006) | (0.010) | | |
| Control | No | Yes | No | Yes | | |
| Sample | 3,318 | 2,283 | 47,094 | 42,523 | | |
| Within $R^2$ | 0.01 | 0.21 | 0.30 | 0.12 | | |

**Internet Appendix S6: Summary statistics for insider purchase and sell samples**

| | Insider Purchase Sample | | | | | Insider Sell Sample | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Mean | Median | S.D | 25 Per | 75 Per | Mean | Median | S.D | 25 Per | 75 Per |
| $BHAR\_m\_180_{i,t}$ | 0.090 | 0.030 | 0.371 | -0.151 | 0.249 | 0.014 | -0.001 | 0.231 | -0.122 | 0.127 |
| $Pay\_gap_{j,t-1}$ | 6.635 | 6.705 | 1.446 | 6.054 | 7.510 | 7.326 | 7.699 | 1.944 | 6.815 | 8.449 |
| $Ret30_{j,t}$ | -0.058 | -0.048 | 0.163 | -0.153 | 0.032 | 0.060 | 0.053 | 0.103 | 0.002 | 0.110 |
| $Mom_{j,t}$ | 0.081 | 0.069 | 0.362 | -0.121 | 0.236 | 0.263 | 0.227 | 0.373 | 0.050 | 0.430 |
| $Bm_{j,m-1}$ | 0.730 | 0.676 | 0.424 | 0.397 | 1.001 | 0.461 | 0.379 | 0.344 | 0.222 | 0.607 |
| $Numest_{j,m-1}$ | 6.748 | 6.000 | 5.260 | 3.000 | 9.000 | 12.988 | 12.000 | 8.065 | 6.000 | 18.000 |
| $Illiq_{j,m-1}$ | 0.572 | 0.069 | 1.730 | 0.014 | 0.345 | 0.030 | 0.005 | 0.122 | 0.002 | 0.018 |
| $Size_{j,m-1}$ | 6.462 | 6.478 | 1.392 | 5.407 | 7.399 | 8.164 | 8.018 | 1.428 | 7.092 | 9.135 |
| $Roa_{j,t-1}$ | 0.018 | 0.019 | 0.079 | 0.001 | 0.059 | 0.064 | 0.060 | 0.078 | 0.026 | 0.101 |
| $Delta_{p,t-1}$ | 0.365 | 0.150 | 0.822 | 0.058 | 0.329 | 2.026 | 0.730 | 5.737 | 0.286 | 1.863 |
| $Vega_{p-.t-1}$ | 0.140 | 0.053 | 0.291 | 0.007 | 0.134 | 0.571 | 0.212 | 1.085 | 0.053 | 0.594 |
| $Rd_{j,t-1}$ | 0.021 | 0.000 | 0.067 | 0.000 | 0.012 | 0.050 | 0.000 | 0.091 | 0.000 | 0.071 |
| $Lncompen_{j,t,-1}$ | 6.703 | 6.526 | 0.834 | 6.158 | 7.141 | 7.279 | 7.252 | 0.956 | 6.674 | 7.852 |
| $Rating_{j,t-1}$ | 1.315 | 1.351 | 0.187 | 1.175 | 1.471 | 1.391 | 1.443 | 0.172 | 1.275 | 1.500 |
| $CEO\_IT_{j,t}$ | 3.179 | 4.000 | 1.330 | 2.000 | 4.000 | 2.965 | 3.000 | 1.547 | 1.000 | 4.000 |
| $\Delta Roa_{t,t+2}$ | 0.006 | 0.001 | 0.116 | -0.023 | 0.032 | -0.014 | -0.002 | 0.102 | -0.029 | 0.016 |
| $\Delta Sentiment_{t-1,t+1}$ | 0.004 | -0.016 | 0.641 | -0.355 | 0.378 | -0.028 | -0.005 | 0.513 | -0.272 | 0.232 |
| $\Delta r_{t,t+2}$ | 0.004 | 0.002 | 0.032 | -0.007 | 0.012 | -0.003 | -0.001 | 0.013 | -0.006 | 0.003 |
| No. Shares (net) | 5,777 | 2,000 | 10,626 | 600 | 5,272 | -28,237 | -10,452 | 47,790 | -30,000 | -4,000 |
| No. Shares (total) | 5,875 | 2,000 | 10,696 | 625 | 5,498 | -28,255 | -10,460 | 47,844 | -30,000 | -4,000 |
| $ Shares (net) | 63,838 | 22,000 | 98,735 | 8,500 | 73,302 | -1,006,818 | -377,357 | 1,843,089 | -1,039,352 | -125,303 |
| $ Shares (total) | 88,531 | 22,218 | 395,242 | 8,565 | 74,700 | -1,008,340 | -377,500 | 1,852,591 | -1,039,456 | -125,548 |

**Internet Appendix S7: Insider trading and price informativeness around CEO turnover**

This table reports the fixed effects regression output based on the matched sample. The dependent variable is BHAR_m_180 in year (0,0) in columns (1) and (3) and (1,1) in columns (2) and (4). We match each treated firm with CEO turnover event in year *t* with one control firm using Mahalanobis distance on the average insider purchase/sell profitability, logarithm of the total asset and the book-to-market ratio in the fiscal year *t-1*. We restrict that the control firm sample does not have any CEO turnover in (-2, 2). In Panel A, the moderator variable is $Pay\_rank_{p,t}$, the rank of non-promoted manager sorted by their total compensation in year -1 among all tournament competitors. In Panel B, we use $BA_{p,t-1}$, the bonus award in t-1 for insider *p*. In Panel C, the moderator variable is $\mathrm{C\_quint}_{\mathrm{j,t}}$, the quintile number based on the board conservatism, following Khan and Watts (2009), for all firms in the same industry in each year. In Panel D, the moderate variable is $ind\_incen_{j,t}$, the natural logarithm of the industry tournament incentives, as outlined in Coles et al. (2018). In Panel E, we use $NoncomD_{p,t}$, a dummy variable equals to one if firm *j* disclosed a non-compete agreement in its 10-K or 10-Q in year *t*, zero otherwise. In Panel F, the moderate variable is $riskD_j$, a dummy variable equal to one if the firm is in high illegal insider trading industry, as outlined in Kacperczyk and Pagnotta (2024), and zero otherwise. In Panel G, we use $Lntenure_{p,t}$, the natural logarithm of the tenure of the insider *p* in year *t* in firm *j.* In Panel H, we use $Lnage_{p,t}$, the natural logarithm of the age of the insider *p* in year *t*. In Panel I, we employ $SHD_{p,t}$, a dummy variable equals to one for short-horizon insiders identified by following Akbas et al (2020), and zero otherwise. In Panel J, the moderator variable is $Probability_{p,t-1}$, the estimated subjective probability of insiders becoming CEO based on their personal characteristics. Appendix A and Internet Appendix 1 define all variables in the table. We include the same set of control variables as in Equation (2). The robust standard errors clustered at the firm-month level are in parentheses. $^{***}$, $^{**}$, and $^{*}$ denote significance at the 99%, 95% and 90% confidence level, respectively. All variables are winsorized at the top 99% and the bottom 1% level.

| | **Insider Purchase** | | **Insider Sell** | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | **Panel A: Tournament Prize** | | | |
| $\mathrm{Post}_{\mathrm{i,t}}$ | 0.023 | 0.037 | 0.012 | 0.046*** |
| | (0.080) | (0.093) | (0.011) | (0.012) |
| $\mathrm{Treat}_{\mathrm{i,t}}$ | -0.061 | -0.037 | 0.026** | 0.027** |
| | (0.097) | (0.115) | (0.011) | (0.011) |
| $(\mathrm{Treat\times Post})_{\mathrm{i,t}}$ | 0.098 | -0.074 | -0.028* | -0.058*** |
| | (0.108) | (0.203) | (0.016) | (0.019) |
| $(\mathrm{Post\times Treat\times Pay\_rank})_{\mathrm{i,t}}$ | 0.021 | -0.006 | 0.010** | 0.011** |
| | (0.030) | (0.047) | (0.002) | (0.005) |
| $(\mathrm{Treat\times Pay\_rank})_{\mathrm{i,t}}$ | -0.010 | -0.000 | -0.007** | -0.006** |
| | (0.027) | (0.017) | (0.003) | (0.003) |
| $(\mathrm{Post\times Pay\_rank})_{\mathrm{i,t}}$ | -0.017 | -0.022 | -0.004 | -0.008** |
| | (0.026) | (0.020) | (0.003) | (0.004) |
| $\mathrm{Pay\_rank}_{\boldsymbol{p},\mathrm{t}}$ | -0.000 | 0.004 | 0.001 | -0.000 |
| | (0.025) | (0.017) | (0.003) | (0.003) |
| Sample | 1,551 | 1,056 | 34,808 | 31,603 |
| | **Panel B: Bonus award effect** | | | |
| $\mathrm{Post}_{\mathrm{i,t}}$ | 0.017 | 0.039 | 0.010 | 0.023*** |
| | (0.034) | (0.042) | (0.006) | (0.008) |
| $\mathrm{Treat}_{\mathrm{i,t}}$ | -0.093 | -0.015 | 0.006 | 0.010 |
| | (0.059) | (0.078) | (0.007) | (0.007) |
| $(\mathrm{Treat\times Post})_{\mathrm{i,t}}$ | 0.072 | -0.083 | -0.018* | -0.031** |
| | (0.056) | (0.089) | (0.010) | (0.012) |
| $(\mathrm{Treat\times Post\times BA})_{\mathrm{i,t}}$ | 0.053 | 0.174* | 0.045** | 0.043** |
| | (0.069) | (0.094) | (0.020) | (0.022) |
| $(\mathrm{Treat\times BA})_{\mathrm{i,t}}$ | -0.053 | -0.084** | 0.009 | 0.003 |

| | | | | |
|---|---|---|---|---|
| | (0.053) | (0.040) | (0.011) | (0.011) |
| $(Post \times BA)_{i,t}$ | -0.016 | -0.040 | -0.034*** | -0.014 |
| | (0.046) | (0.063) | (0.012) | (0.014) |
| $BA_{p,t}$ | -0.002 | 0.011 | -0.001 | -0.007 |
| | (0.040) | (0.026) | (0.008) | (0.008) |
| Sample | 1,554 | 1,059 | 35,190 | 31,841 |
| | **Panel C: Board Conservatism** | | | |
| $Post_{i,t}$ | -0.191** | 0.104 | 0.032** | 0.028** |
| | (0.095) | (0.135) | (0.013) | (0.014) |
| $Treat_{i,t}$ | -0.326*** | -0.309*** | 0.024* | 0.018 |
| | (0.101) | (0.111) | (0.012) | (0.012) |
| $(Treat \times Post)_{i,t}$ | 0.299** | -0.387 | -0.039** | -0.072*** |
| | (0.137) | (0.248) | (0.020) | (0.022) |
| $(Treat \times Post \times C\_quint)_{i,t}$ | -0.101** | 0.097 | 0.016** | 0.018** |
| | (0.045) | (0.070) | (0.008) | (0.008) |
| $(Treat \times C\_quint)_{i,t}$ | 0.083*** | 0.087*** | -0.008* | -0.003 |
| | (0.030) | (0.032) | (0.005) | (0.005) |
| $(Post \times C\_quint)_{i,t}$ | 0.093*** | -0.000 | -0.013** | -0.003 |
| | (0.035) | (0.043) | (0.005) | (0.005) |
| $C\_quint_{j,t}$ | -0.071** | -0.078*** | -0.003 | -0.009** |
| | (0.030) | (0.030) | (0.004) | (0.004) |
| Sample | 1,791 | 1,282 | 36,748 | 33,527 |
| | **Panel D: Industry Tournament Incentives** | | | |
| $Post_{i,t}$ | -0.025 | -0.041 | 0.136*** | 0.071 |
| | (0.193) | (0.236) | (0.023) | (0.050) |
| $Treat_{i,t}$ | -0.238 | -0.449* | 0.100*** | 0.016 |
| | (0.233) | (0.233) | (0.027) | (0.046) |
| $(Treat \times Post)_{i,t}$ | 0.329 | -0.781** | -0.097** | -0.162** |
| | (0.337) | (0.381) | (0.038) | (0.074) |
| $(Treat \times Post \times ind\_incen)_{i,t}$ | -0.037 | 0.117** | 0.014** | 0.022** |
| | (0.049) | (0.057) | (0.005) | (0.010) |
| $(Treat \times ind\_incen)_{i,t}$ | 0.018 | 0.062 | -0.014*** | -0.002 |
| | (0.033) | (0.041) | (0.004) | (0.006) |
| $(Post \times ind\_incen)_{i,t}$ | 0.012 | 0.015 | -0.017*** | -0.009 |
| | (0.030) | (0.038) | (0.003) | (0.007) |
| $ind\_incen_{j,t}$ | 0.001 | -0.006 | 0.011*** | 0.005 |
| | (0.023) | (0.028) | (0.003) | (0.005) |
| Sample | 1,471 | 1,030 | 27,751 | 25,065 |
| | **Panel E: Non-compete agreements** | | | |
| $Post_{i,t}$ | 0.030 | -0.005 | 0.007* | 0.019** |
| | (0.032) | (0.046) | (0.004) | (0.008) |
| $Treat_{i,t}$ | -0.130** | -0.089** | 0.011** | 0.010 |
| | (0.066) | (0.094) | (0.005) | (0.008) |
| $(Treat \times Post)_{i,t}$ | -0.016 | 0.057 | 0.013** | -0.013 |
| | (0.050) | (0.082) | (0.006) | (0.012) |
| $(Post \times Treat \times NoncomD)_{i,t}$ | 0.265 | -0.225 | -0.039*** | -0.055* |
| | (0.222) | (0.167) | (0.015) | (0.030) |
| $(Treat \times NoncomD)_{i,t}$ | 0.136 | 0.183** | 0.040*** | 0.021 |
| | (0.116) | (0.116) | (0.010) | (0.013) |
| $(Post \times NoncomD)_{i,t}$ | -0.191 | 0.105 | -0.004 | 0.015 |
| | (0.166) | (0.131) | (0.010) | (0.018) |
| $NoncomD_{j,t}$ | -0.041 | -0.127 | -0.039*** | -0.032*** |
| | (0.086) | (0.091) | (0.007) | (0.010) |

| | | | | |
|---|---|---|---|---|
| Sample | 1,791 | 1,282 | 36,944 | 33,521 |
| | **Panel F: High illegal insider trading industry** | | | |
| $Post_{i,t}$ | 0.029 | -0.001 | -0.008 | 0.017 |
| | (0.039) | (0.055) | (0.009) | (0.012) |
| $Treat_{i,t}$ | -0.145* | -0.195 | 0.004 | 0.007 |
| | (0.085) | (0.130) | (0.008) | (0.008) |
| $(Treat \times Post)_{i,t}$ | -0.054 | -0.011 | 0.008 | -0.043*** |
| | (0.060) | (0.103) | (0.013) | (0.017) |
| $(Treat \times Post \times riskD)_{i,t}$ | 0.161* | -0.011 | -0.009 | 0.064*** |
| | (0.094) | (0.141) | (0.009) | (0.023) |
| $(Treat \times riskD)_{i,t}$ | 0.057 | 0.275 | -0.006 | -0.012 |
| | (0.127) | (0.168) | (0.012) | (0.013) |
| $(Post \times riskD)_{i,t}$ | -0.047 | 0.050 | 0.006 | -0.029 |
| | (0.067) | (0.096) | (0.013) | (0.016) |
| Sample | 1,791 | 1,282 | 36,775 | 34,431 |
| | **Panel G: Tenure Effect** | | | |
| $Post_{i,t}$ | 0.027 | -0.236** | 0.018 | 0.057** |
| | (0.083) | (0.115) | (0.016) | (0.022) |
| $Treat_{i,t}$ | -0.191* | -0.135* | 0.029** | 0.033*** |
| | (0.100) | (0.080) | (0.013) | (0.012) |
| $(Treat \times Post)_{i,t}$ | 0.229* | 0.677*** | -0.057** | -0.122*** |
| | (0.121) | (0.192) | (0.024) | (0.033) |
| $(Post \times Treat \times lntenure)_{i,t}$ | -0.151* | -0.469*** | 0.032*** | 0.054*** |
| | (0.051) | (0.119) | (0.012) | (0.016) |
| $(Treat \times lntenure)_{i,t}$ | 0.036 | 0.065 | -0.014*** | -0.014** |
| | (0.066) | (0.044) | (0.006) | (0.006) |
| $(Post \times lntenure)_{i,t}$ | 0.029 | 0.157** | -0.009 | -0.021* |
| | (0.051) | (0.069) | (0.008) | (0.011) |
| $lntenure_{p,t}$ | -0.013 | -0.017 | 0.013*** | 0.013*** |
| | (0.053) | (0.036) | (0.005) | (0.005) |
| Sample | 1,791 | 1,282 | 36,746 | 33,527 |
| | **Panel H: Age Effect** | | | |
| $Post_{i,t}$ | -0.564 | -2.013** | 0.410** | 0.803*** |
| | (0.614) | (0.874) | (0.180) | (0.197) |
| $Treat_{i,t}$ | -1.156* | -1.467** | 0.296* | 0.481*** |
| | (0.692) | (0.579) | (0.160) | (0.154) |
| $(Treat \times Post)_{i,t}$ | 2.018** | 5.678*** | -0.539** | -1.179*** |
| | (1.016) | (1.683) | (0.252) | (0.279) |
| $(Post \times Treat \times lnage)_{i,t}$ | -0.478* | -1.407*** | 0.137** | 0.293*** |
| | (0.256) | (0.432) | (0.064) | (0.070) |
| $(Treat \times lnage)_{i,t}$ | 0.255 | 0.353** | -0.074* | -0.121*** |
| | (0.174) | (0.145) | (0.040) | (0.039) |
| $(Post \times lnage)_{i,t}$ | 0.140 | 0.519** | -0.103** | -0.198*** |
| | (0.155) | (0.219) | (0.046) | (0.050) |
| $Lnage_{p,t}$ | -0.216* | -0.189* | 0.053 | 0.093*** |
| | (0.128) | (0.109) | (0.034) | (0.032) |
| Sample | 1,369 | 1,025 | 32,111 | 29,481 |
| | **Panel J: Investment Horizon** | | | |
| $Post_{i,t}$ | 0.007 | -0.004 | 0.009 | 0.020** |
| | (0.034) | (0.046) | (0.006) | (0.008) |
| $Treat_{i,t}$ | -0.146*** | -0.050 | 0.012* | 0.012 |
| | (0.053) | (0.068) | (0.007) | (0.008) |
| $(Treat \times Post)_{i,t}$ | 0.075 | -0.001 | -0.017* | -0.028** |

| | | | | |
|---|---|---|---|---|
| | (0.052) | (0.079) | (0.010) | (0.012) |
| $(Post \times Treat \times SHD)_{i,t}$ | -0.104 | 0.084 | 0.050** | 0.051* |
| | (0.149) | (0.219) | (0.023) | (0.029) |
| $(Treat \times SHD)_{i,t}$ | -0.054 | -0.130 | -0.001 | 0.000 |
| | (0.126) | (0.165) | (0.015) | (0.015) |
| $(Post \times SHD)_{i,t}$ | 0.029 | -0.062 | -0.020 | 0.010 |
| | (0.121) | (0.181) | (0.016) | (0.022) |
| $SHD_{p,t}$ | 0.043 | 0.149 | 0.002 | -0.005 |
| | (0.121) | (0.162) | (0.012) | (0.012) |
| Sample | 1,791 | 1,282 | 35,873 | 33,519 |
| | **Panel I: Predicted probability of becoming CEO** | | | |
| $Post_{i,t}$ | 1.197*** | 0.065 | -0.010 | 0.004 |
| | (0.065) | (0.055) | (0.011) | (0.014) |
| $Treat_{i,t}$ | -0.170** | -0.079 | -0.005 | -0.001 |
| | (0.086) | (0.090) | (0.010) | (0.011) |
| $(Treat \times Post)_{i,t}$ | -0.304*** | -0.026 | 0.037*** | 0.045*** |
| | (0.109) | (0.176) | (0.014) | (0.017) |
| $(Post \times Treat \times Probability)_{i,t}$ | 1.137*** | 0.303 | -0.064* | -0.125*** |
| | (0.413) | (0.425) | (0.039) | (0.048) |
| $(Treat \times Probability)_{i,t}$ | 0.019 | -0.012 | 0.044* | 0.057** |
| | (0.102) | (0.086) | (0.025) | (0.027) |
| $(Post \times Probability)_{i,t}$ | -0.681*** | 0.094 | 0.051* | 0.009 |
| | (0.220) | (0.090) | (0.030) | (0.036) |
| $Probability_{p,t}$ | -0.066 | -0.033 | -0.412 | -0.018 |
| | (0.065) | (0.048) | (0.024) | (0.024) |
| Sample | 662 | 557 | 24,648 | 24,285 |

**Internet Appendix S8: Alternative explanations**

This table reports the results of fixed effect regressions. We match firms with CEO turnover event in year *t* with firms with no CEO turnover in (-2, 2) using the average insider purchase/sell profitability, logarithm of the total asset and the book-to-market ratio in the fiscal year *t-1* with shortest Mahalanobis distance. The dependent variable is BHAR_m_180. In Panel A, the moderator variable is future earnings response coefficient (*FERC*), a dummy variable equals to one for firms in the top quantile of $FERC_{j,t}$ in year *t,* and zero otherwise, following Tucker and Zarowin (2006). In Panel B, the moderator variable is the return synchronicity (*Synch*), a dummy variable equals to one for firms in the top quantile of $Synch_{j,t}$ in year *t* in the same two-dig sic industry*,* and zero otherwise, following Piotroski and Roulstone (2004). In Panel C, the moderator variable is $skill_{j,t}$, the skills of the current CEO defined by Daniel *et al.* (2020). In Panel D, the sample is only based on treated firms that have lower-than-median stock return volatility during its CEO turnover year in its 2-digit SIC industry and their corresponding control firms. We include firm and month fixed effects and control variables described in Table 1 and the main level of moderators. Appendix A and Internet Appendix 1 define all variables in the table. Standard errors in parentheses are based on robust standard errors clustered at the firm-month level. $^{***}$, $^{**}$, and $^{*}$ denote significance at the 99%, 95% and 90% confidence level, respectively. All variables are winsorised at the top 99% and the bottom 1% level.

| | Insider Purchase | | Insider Sell | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Year *t* | (0,0) | (1,1) | (0,0) | (1,1) |
| **Panel A: Future Earnings Response Coefficient** | | | | |
| $Post_{i,t}$ | 0.087** | 0.055 | 0.032** | 0.019** |
| | (0.034) | (0.063) | (0.018) | (0.009) |
| $Treat_{i,t}$ | -0.121* | -0.094 | -0.004 | 0.006 |
| | (0.062) | (0.070) | (0.012) | (0.007) |
| $(Treat\times Post)_{i,t}$ | 0.001 | 0.115 | -0.029 | -0.027** |
| | (0.063) | (0.086) | (0.020) | (0.013) |
| $(Post\times Treat\times FERC)_{i,t}$ | 0.093 | -0.237 | -0.001 | -0.010 |
| | (0.118) | (0.145) | (0.047) | (0.029) |
| $(Treat\times FERC)_{i,t}$ | -0.047 | -0.124 | 0.028 | -0.007 |
| | (0.099) | (0.112) | (0.025) | (0.017) |
| $(Post\times FERC)_{i,t}$ | -0.145* | -0.049 | 0.020 | 0.039* |
| | (0.075) | (0.102) | (0.036) | (0.022) |
| $FERC_{j,t}$ | 0.054 | 0.137 | -0.019 | 0.010 |
| | (0.074) | (0.089) | (0.022) | (0.016) |
| Sample | 1,358 | 1,075 | 28,415 | 28,260 |
| **Panel B: Return Synchronicity** | | | | |
| $Post_{i,t}$ | -0.051 | 0.039 | 0.014 | 0.038*** |
| | (0.052) | (0.067) | (0.009) | (0.010) |
| $Treat_{i,t}$ | -0.203*** | -0.094 | -0.001 | 0.005 |
| | (0.070) | (0.083) | (0.009) | (0.009) |
| $(Treat\times Post)_{i,t}$ | 0.177** | -0.095 | -0.017 | -0.036** |
| | (0.075) | (0.093) | (0.013) | (0.016) |
| $(Post\times Treat\times Synch)_{i,t}$ | -0.241*** | 0.189 | 0.021 | 0.017 |
| | (0.103) | (0.145) | (0.019) | (0.020) |
| $(Treat\times Synch)_{i,t}$ | 0.127 | 0.095 | 0.016 | 0.010 |
| | (0.085) | (0.084) | (0.012) | (0.012) |
| $(Post\times Synch)_{i,t}$ | 0.120* | -0.031 | -0.021 | -0.033** |
| | (0.069) | (0.078) | (0.013) | (0.015) |
| $Synch_{j,t}$ | -0.028 | -0.099 | 0.002 | 0.008 |
| | (0.066) | (0.079) | (0.009) | (0.009) |
| Sample | 1,786 | 1,276 | 36,746 | 33,527 |

| | Panel C: Newly-Appointed CEO Ability | | | |
|---|---|---|---|---|
| *Dependent Variable* | BHAR_m_180 | BHAR_m_180 | BHAR_m_180 | BHAR_m_180 |
| $Post_{i,t}$ | 0.069* | 0.012 | 0.011 | 0.014 |
| | (0.038) | (0.060) | (0.006) | (0.008) |
| $Treat_{i,t}$ | -0.025 | -0.083 | 0.034** | 0.024*** |
| | (0.060) | (0.090) | (0.013) | (0.007) |
| $(Treat \times Post)_{i,t}$ | 0.019 | 0.019 | -0.071** | -0.037* |
| | (0.053) | (0.105) | (0.032) | (0.018) |
| $(Treat \times Post \times skill)_{j,t}$ | -0.263 | 1.022** | -1.401*** | -0.218** |
| | (0.383) | (0.510) | (0.274) | (0.110) |
| $(Treat \times skill)_{i,t}$ | 0.237 | -0.318 | 0.159*** | 0.149*** |
| | (0.201) | (0.250) | (0.044) | (0.035) |
| $(Post \times skill)_{i,t}$ | 0.216 | -0.507* | 0.064* | 0.207*** |
| | (0.211) | (0.305) | (0.034) | (0.029) |
| $skill_{j,t}$ | 0.093 | 0.312* | 0.216*** | 0.243*** |
| | (0.163) | (0.167) | (0.037) | (0.041) |
| Within R-square | 0.23 | 0.23 | 0.19 | 0.21 |
| Sample | 2,272 | 1,282 | 23,291 | 22,269 |
| | **Panel D: Low Volatility Firms Only** | | | |
| *Dependent Variable* | BHAR_m_180 | BHAR_m_180 | BHAR_m_180 | BHAR_m_180 |
| $Post_{i,t}$ | -0.033 | 0.010 | 0.011* | 0.046*** |
| | (0.054) | (0.043) | (0.005) | (0.010) |
| $Treat_{I,t}$ | -0.238 | 0.132 | 0.018** | 0.024*** |
| | (0.162) | (0.115) | (0.006) | (0.009) |
| $(Post \times Treat)_{i,t}$ | 0.159** | -0.220 | -0.014* | -0.048*** |
| | (0.068) | (0.147) | (0.008) | (0.013) |
| Within R-square | 0.32 | 0.24 | 0.12 | 0.14 |
| Sample | 741 | 574 | 22,922 | 21,120 |

**Internet Appendix S9: Insider trading informativeness based on exiting directors.**

This table reports the results of fixed effect regressions. We use a matched sample as in Table 4. In Panel A, the dependent variable is the change in return on asset between year t and year t+2. In Panel B, the dependent variable is the change in investor sentiment measured as firm-specific component from the market-to-book decomposition of Rhodes–Kropf et al. (2005). The change in investor sentiment $\Delta Sentiment_{-1,1}$ is measured between year t-1 to year t+1. In Panel C, we use change in the cost of capital, $\Delta r_{t,t+2}$ by following Cziraki et al. (2021) and estimating a modified Fama and French (1993) Three-Factor model. The results include the control variables in Equation (2) but omitted for brevity. We split the sample based on whether the firm has at least one non-CEO director that is leaving in the next year. Appendix A and Internet Appendix 1 define all variables in the table. The robust standard errors reported in parentheses are clustered at the firm-month level. $^{***}$, $^{**}$, and $^{*}$ denote significance at the 99%, 95% and 90% confidence level, respectively. All variables are winsorized at the top 99% and the bottom 1% level.

| | **Insider Sell (No Exiting)** | | **Insider Sell (With Exiting)** | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Year *t* | (0,0) | (1,1) | (0,0) | (1,1) |
| | **Panel A: Future Firm Performance** | | | |
| *Dependent Variable* | $\Delta ROA_{t,t+2}$ | $\Delta ROA_{t,t+2}$ | $\Delta ROA_{t,t+2}$ | $\Delta ROA_{t,t+2}$ |
| $Post_{i,t}$ | 0.003 | 0.000 | -0.005 | 0.004 |
| | (0.005) | (0.005) | (0.004) | (0.005) |
| $Treat_{i,t}$ | 0.008** | 0.019*** | 0.039*** | 0.040*** |
| | (0.004) | (0.004) | (0.011) | (0.010) |
| $(Post \times Treat)_{i,t}$ | -0.014** | -0.015* | -0.014** | -0.026*** |
| | (0.007) | (0.008) | (0.007) | (0.008) |
| *Other Control* | Yes | Yes | Yes | Yes |
| Within R-square | 0.11 | 0.08 | 0.07 | 0.10 |
| Fixed Effect | Firm, Month | Firm, Month | Firm, Month | Firm, Month |
| Sample | 18,424 | 15,647 | 17,023 | 16,426 |
| | **Panel B: Investor Sentiment** | | | |
| *Dependent Variable* | $\Delta Sentiment_{t-1}$ | $\Delta Sentiment_{t-1,t+1}$ | $\Delta Sentiment_{t-1,t+1}$ | $\Delta Sentiment_{t-1,t+1}$ |
| $Post_{i,t}$ | -0.028 | 0.104*** | 0.047** | 0.041* |
| | (0.022) | (0.024) | (0.018) | (0.024) |
| $Treat_{i,t}$ | 0.087*** | 0.077*** | 0.060** | 0.026 |
| | (0.025) | (0.026) | (0.026) | (0.027) |
| $(Post \times Treat)_{i,t}$ | -0.039 | -0.100** | -0.124*** | -0.064* |
| | (0.035) | (0.042) | (0.036) | (0.038) |
| *Other Control* | Yes | Yes | Yes | Yes |
| Within R-square | 0.11 | 0.11 | 0.15 | 0.17 |
| Fixed Effect | Firm, Month | Firm, Month | Firm, Month | Firm, Month |
| Sample | 18,612 | 15,766 | 16,765 | 16,172 |
| | **Panel C: Change in Cost of Capital** | | | |
| *Dependent Variable* | $\Delta r_{t,t+2}$ | $\Delta r_{t,t+2}$ | $\Delta r_{t,t+2}$ | $\Delta r_{t,t+2}$ |
| $Post_{i,t}$ | -0.001*** | -0.002** | 0.000 | 0.001*** |
| | (0.000) | (0.001) | (0.000) | (0.001) |
| $Treat_{i,t}$ | -0.001 | -0.001 | -0.001 | -0.001** |
| | (0.001) | (0.001) | (0.001) | (0.001) |
| $(Post \times Treat)_{i,t}$ | 0.002*** | 0.002** | 0.001** | 0.001 |
| | (0.001) | (0.001) | (0.000) | (0.001) |
| *Other Control* | Yes | Yes | Yes | Yes |
| Within R-square | 0.05 | 0.05 | 0.14 | 0.08 |
| Fixed Effect | Firm, Month | Firm, Month | Firm, Month | Firm, Month |
| Sample | 19,038 | 16,804 | 17,485 | 16,789 |

**Internet Appendix S10: Insider trading of exiting managers after CEO turnover**

The table reports the results of the fixed effect regressions. The dependent variable in columns (1) and (3) is $ExitD_{p,t}$, a dummy variable equal to one for managers who left the firm in the year (0, 2), and zero otherwise. The dependent variable in columns (2) and (4) is *BHAR_m_180*. The moderator variable in columns (2) and (4) is $LastD_{p,t}$,a dummy variable equal to one if a manager is staying in the firm for the last year, and zero otherwise. In Panel A, we employ the same matched sample as Table 4. In Panel B, we match each exiting managers using their total compensation, average insider trading profitability and total shares traded in year *t-1* with a manager from firms with no CEO turnover in year (-3,3) using the shortest Mahalanobis distance. We include the control variables in Equation (2), omitted for brevity. Appendix A defines all variables in the table. Our observations are between event year (-2, 1). Standard errors reported in parentheses are based on robust standard errors clustered at the firm-month level. ***, **, and * denote significance at the 99%, 95% and 90% confidence level, respectively. All variables are winsorised at the top 99% and the bottom 1% level.

| | **Insider Purchase** | | **Insider Sell** | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Year *t* | (0,1) | (0,1) | (0,1) | (0,1) |
| Panel A: Firm Matched Sample | | | | |
| *Dependent Variable* | $ExitD_{p,t}$ | BHAR_m_180 | $ExitD_{p,t}$ | BHAR_m_180 |
| $Post_{i,t}$ | 0.009*** | 0.115** | 0.038*** | 0.013** |
| | (0.032) | (0.057) | (0.007) | (0.006) |
| $Treat_{i,t}$ | -0.064 | -0.128** | 0.012 | 0.020** |
| | (0.075) | (0.064) | (0.009) | (0.008) |
| $(Post \times Treat)_{i,t}$ | 0.002 | -0.001 | 0.008 | -0.017* |
| | (0.052) | (0.078) | (0.013) | (0.010) |
| $LastD_{p,t}$ | | -0.008 | | -0.002 |
| | | (0.056) | | (0.007) |
| $(Post \times Treat \times LastD)_{i,t}$ | | 0.018 | | 0.024* |
| | | (0.104) | | (0.014) |
| *Control Variable* | Yes | Yes | Yes | Yes |
| Within R-square | 0.04 | 0.31 | 0.02 | 0.12 |
| Fixed Effect | Firm, Month | Firm, Month | Firm, Month | Firm, Month |
| Sample | 2,134 | 2,130 | 46,002 | 46,389 |
| Panel B: Insider Matched Sample | | | | |
| *Dependent Variable* | $ExitD_{p,t}$ | BHAR_m_180 | $ExitD_{p,t}$ | BHAR_m_180 |
| $Post_{i,t}$ | 0.228*** | 0.120* | -0.005 | 0.009 |
| | (0.041) | (0.070) | (0.003) | (0.007) |
| $Treat_{i,t}$ | 0.995 | -0.060 | 0.246*** | -0.016 |
| | (0.057) | (0.142) | (0.015) | (0.012) |
| $(Post \times Treat)_{i,t}$ | -0.316*** | 0.187 | 0.434*** | -0.005 |
| | (0.072) | (0.144) | (0.022) | (0.015) |
| Within R-square | 0.36 | 0.33 | 0.28 | 0.14 |
| Fixed Effect | Firm, Month | Firm, Month | Firm, Month | Firm, Month |
| Sample | 949 | 874 | 17,442 | 17,269 |

**Internet Appendix S11: Firm-level characteristics and the scale of non-CEO director turnover**

This table reports the results of firm level Logit and Linear Probability regressions. The dependent variable $High\_TurnoverD_{j,t}$ is a dummy equals to one if firm $j$ has more than 40% of their tournament contenders leave the firm in year (0, 1), and zero otherwise. We include in all regressions, year dummy and year fixed effect. We also include, but omit to report for brevity, the following lagged independent: Illiq, roa, tobin'sQ, dividend yield, leverage, capital intensity, institutional ownership, independent committee, and analyst talent. $(Mean\_BAHR\_with\_CEO)_{j,(t-3,t-2)}$ is the average BHAR for 180 holding period with CEO trades between year (-2, -1). We multiply the BHAR of sell trades by -1 to correct the direction. Standard errors reported in parentheses are computed based on robust standard errors for logit regression and clustered at the year-industry level for fixed effect regression. Appendix A and Internet Appendix 1 define all variables. ***, **, and * denote significance at the 99%, 95% and 90% confidence level, respectively. All variables are winsorised at the top 99% and the bottom 1% level.

| | **Logit** | | **Linear Probability Model** | |
|---|---|---|---|---|
| | $High\_TurnoverD_{j,t}$ | $High\_TurnoverD_{j,t}$ | $High\_TurnoverD_{j,t}$ | $High\_TurnoverD_{j,t}$ |
| $(Mean\_BHAR\_with\_CEO)_{j,(t-3,t-2)}$ | -1.169*** | | -0.253*** | |
| | (0.191) | | (0.046) | |
| $(Mean\_BHAR\_without\_CEO)_{j,(t-3,t-2)}$ | | -1.089*** | | -0.238*** |
| | | (0.205) | | (0.049) |
| $C\_score_{j,t-1}$ | 0.003*** | 0.003* | 0.001** | 0.001** |
| | (0.001) | (0.002) | (0.000) | (0.000) |
| $mom_{j,(d-364,d-31),t-1}$ | 0.354** | 0.274 | 0.074* | 0.058 |
| | (0.181) | (0.192) | (0.045) | (0.047) |
| $bm_{j,t-1}$ | 0.566*** | 0.687*** | 0.126*** | 0.152*** |
| | (0.169) | (0.183) | (0.040) | (0.043) |
| $numest_{j,t-1}$ | 0.023** | 0.027*** | 0.005** | 0.006** |
| | (0.010) | (0.010) | (0.002) | (0.003) |
| $size_{j,t-1}$ | -0.162*** | -0.173*** | -0.035** | -0.037*** |
| | (0.058) | (0.062) | (0.014) | (0.015) |
| $rd_{j,t-1}$ | 1.807** | 1.963** | 0.402** | 0.435** |
| | (0.782) | (0.889) | (0.178) | (0.192) |
| $skt\_ret\_volatility_{j,t-1}$ | 9.684** | 11.088** | 2.233** | 2.505** |
| | (4.661) | (4.984) | (1.083) | (1.195) |
| $cash\_flow\_vol_{j,t-1}$ | 10.999*** | 12.889*** | 2.524*** | 2.943*** |
| | (3.649) | (4.084) | (0.881) | (0.940) |
| $independent\_manager_{j,t-1}$ | 1.169*** | 1.416*** | 0.252*** | 0.300*** |
| | (0.394) | (0.423) | (0.096) | (0.097) |
| Sample | 1,953 | 1,764 | 2,016 | 1,814 |

**Internet Appendix S12: CEO purchase transaction trading profitability after CEO turnover**

The dependent variable is Buy-N-Hold abnormal return calculated for 30, 180 and 365-calenday holding periods, respectively. The variable of interest is $yearD_{i,t}$ is a dummy variable equals to one for focal year, and zero otherwise. We only include CEO purchase transaction in the table. Standard errors in parentheses are based on robust standard errors clustered at the firm-month level. ***, **, and * denote significance at the 99%, 95% and 90% confidence level, respectively. All variables are winsorized at the top 99% and the bottom 1% level. All regressions include control variables and firm and month fixed effects. Control variables that are omitted for brevity are $bm_{j,m-1}$, $roa_{j,t-1}$, $vega_{p,t-1}$, and $rd_{j,t-1}$. Appendix A defines all control variables in the table.

| | Year 0 | | | Year 1 | | |
|---|---|---|---|---|---|---|
| BHARs | 30 | 180 | 365 | 30 | 180 | 365 |
| $yearD_{i,t}$ | -0.014 | -0.029 | -0.111** | 0.022** | -0.011 | -0.104** |
| | (0.012) | (0.034) | (0.055) | (0.011) | (0.030) | (0.043) |
| $pay\_gap_{j,t-1}$ | 0.006** | 0.021*** | 0.016 | 0.006** | 0.022*** | 0.021* |
| | (0.003) | (0.007) | (0.012) | (0.003) | (0.007) | (0.012) |
| $ret30_{j,t-1,(d-1,d-30)}$ | -0.008 | 0.050 | 0.363** | -0.018 | 0.001 | 0.394** |
| | (0.040) | (0.106) | (0.175) | (0.035) | (0.099) | (0.178) |
| $mom_{j,\ t-1,(d-31,d\ -364)}$ | -0.028** | -0.048 | -0.112** | -0.029** | -0.051 | -0.114** |
| | (0.012) | (0.034) | (0.047) | (0.011) | (0.033) | (0.052) |
| $illiq_{j,m-1}$ | 0.012 | 0.234*** | 0.423*** | 0.011 | 0.233*** | 0.408*** |
| | (0.015) | (0.047) | (0.066) | (0.015) | (0.048) | (0.067) |
| $size_{j,m-1}$ | -0.042*** | -0.223*** | -0.392*** | -0.038*** | -0.231*** | -0.390*** |
| | (0.011) | (0.032) | (0.051) | (0.011) | (0.036) | (0.051) |
| $delta_{p,t-1}(\times 0.01)$ | 0.001* | 0.004*** | 0.008*** | 0.001** | 0.004*** | 0.008*** |
| | (0.000) | (0.001) | (0.002) | (0.001) | (0.001) | (0.002) |
| $lncompen_{p,t-1}(\times 0.01)$ | 0.017** | 0.064*** | 0.144*** | 0.017** | 0.066*** | 0.108*** |
| | (0.008) | (0.021) | (0.042) | (0.008) | (0.021) | (0.030) |
| $rating_{j,\ t-1}$ | 0.079 | 0.332* | 0.612** | 0.111 | 0.449** | 0.875*** |
| | (0.075) | (0.193) | (0.269) | (0.075) | (0.200) | (0.283) |
| Constant | 0.038 | 0.434 | 0.701 | -0.035 | 0.332 | 0.535 |
| | (0.114) | (0.307) | (0.481) | (0.112) | (0.311) | (0.462) |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Sample | 4193 | 5116 | 5061 | 4193 | 5116 | 5086 |
| Fixed Effect | Firm, Month | Firm, Month | Firm, Month | Firm, Month | Firm, Month | Firm, Month |
| Within $R^2$ | 0.029 | 0.114 | 0.174 | 0.027 | 0.158 | 0.160 |

**Internet Appendix S13: 2SLS regression result for matching sample**

The table reports the results of 2SLS regressions. The dependent variable in the first stage of the regression is $NPED_{i,t}$, a dummy variable equals to one for the purchase/sell transactions of promotion rejectees in $(0,0)$ or $(1,1)$ with year 0 the CEO turnover event, and zero for years outside the event window. We match firms with CEO turnover event in year *t* with firms on the average insider purchase/sell profitability, logarithm of the total asset and the book-to-market ratio in the fiscal year *t-1* to the nearest neighbor matching using Mahalanobis distance. We match each treated firm with one control firm. We restrict that the control firm sample does not have any CEO turnover in (-2, +2). Our instrumental variable is the previous CEO's age in the last fiscal year. We include the same set of control variables as in Equation (2). Appendix A and Internet Appendix 1 define all variables. We report the robust standard errors clustered at the firm-month level in parentheses. ***, **, and * denote significance at the 99%, 95% and 90% confidence level, respectively. All variables are winsorized at the top 99% and the bottom 1% level.

| | **Insider Sell** | |
|---|---|---|
| | (1) | (2) |
| | First Stage | |
| Year *t* | $(0,0)$ | $(1,1)$ |
| *Dependent Variable* | $NPED_{i,t}$ | $NPED_{i,t}$ |
| $age\_ceo_{j,t-1}$ | $0.019^{***}$ | $-0.026^{***}$ |
| | (0.002) | (0.002) |
| *Control Variable* | Yes | Yes |
| | Second Stage | |
| *Dependent Variable* | BHAR_m_180 | BHAR_m_180 |
| *Endogenous Variable* | | |
| $\widehat{NPED}_t$ | $-0.251^{**}$ | $-0.494^{**}$ |
| | (0.127) | (0.236) |
| $NPED \widehat{\times CEO\_IT}_{i,t}$ | $0.234^{***}$ | $0.175^{**}$ |
| | (0.083) | (0.083) |
| *Control Variables* | | |
| $CEO\_IT_{j,t}$ | 0.004 | -0.006 |
| | (0.005) | (0.009) |
| *Other Control Variable* | Yes | Yes |
| Sample | 18,342 | 19,262 |
| Fixed Effect | Firm, Month | Firm, Month |
| First-Stage F-$\widehat{NPED}_{i,t}$ | $149.39^{***}$ | $264.48^{***}$ |
| Anderson-Rubin Wald Test, F-Statistics | $19.18^{***}$ | $5.47^{*}$ |

**Internet Appendix S14: Other robustness tests**

In Panel A, we extend the control variables in Equation (5) and report the 2nd stage of 2SLS regressions. We do not report the coefficients of *tobin's Q* $_{j,t-1}$, *institution_ownership*$_{j,q-1}$, *cash_flow_vol*$_{j,t-1}$, *advertising-to-sale*$_{j,t-1}$, *dividend-yield*$_{j,t-1}$, *lnage*$_{i,t}$ and for buy trades, the interaction term $\widehat{NPED\times CEO\_IT}_{i,t}$ as they are insignificant. Panel B reports the coefficients of $\widehat{NPED}_{i,t}$ using alternative holding returns measures including raw cumulative return $ret_{t+1,t+i}$ and the 4-factor $\alpha$ multiplied by the median number of trading days of 22, 126, 252 in the three holding periods, respectively. We calculate the 4-factor $\alpha$ by running regression $r_{jt} - rf_t = \alpha_{it} + \beta_1(r_{crsp,t} - rf_t) + \beta_2 SMB_t + \beta_3 HML_t + \beta_4 UMD_t + \varepsilon_t$ from the day after insider transaction day to 3/6/12 months. $r_{crsp,t}$ is CRSP value-weighted market index and $UMD_t$ is up-minus-down factor (momentum). We report the cluster standard errors at the firm-month level parentheses. ***, **, and * denote significance at the 99%, 95% and 90% confidence level, respectively. All variables are winsorised at the top 99% and the bottom 1% level. All columns include control variables, defined in Appendix A and Internet Appendix 1, and firm and month fixed effects.

| Panel A: Extended Set of Control Variables | | | | |
|---|---|---|---|---|
| | Insider Purchase | | Insider Sell | |
| | (0,0) | (1,1) | (0,0) | (1,1) |
| 2nd Stage -*Dep Variable is BHAR_m_180, Endogenous Variables are (NPED)$_{i,t}$ and (NPED×CEO_IT)$_i$* | | | | |
| $\widehat{NPED}_{i,t}$ | -12.541 | -5.710 | -0.581** | -0.643** |
| | (22.256) | (4.825) | (0.230) | (0.269) |
| $\widehat{NPED\times CEO\_IT}_{i,t}$ | 11.817 | 2.360 | 0.381*** | 0.205** |
| | (19.882) | (2.564) | (0.145) | (0.084) |
| $CEO\_IT_{j,t}$ | 0.112 | 0.034 | 0.002 | -0.005 |
| | (0.123) | (0.073) | (0.005) | (0.008) |
| $capital\text{-}to\text{-}sale_{j,t-1}$ | -0.683 | -0.356* | -0.039** | -0.070*** |
| | (0.937) | (0.208) | (0.018) | (0.016) |
| $leverage_{j,t-1}$ | -0.114 | -1.079 | -0.187** | -0.146*** |
| | (1.033) | (1.059) | (0.050) | (0.037) |
| $skt\_ret\_volatility_{j,t-1}$ | 24.518 | 14.097 | 0.486 | -0.634 |
| | (26.006) | (14.754) | (0.572) | (0.669) |
| $capital\_intensity_{j,t-1}$ | -3.142 | 1.563 | -0.005 | -0.069 |
| | (10.994) | (2.266) | (0.178) | (0.151) |
| $firm\_focus_{j,t-1}$ | 0.056 | -0.384 | -0.025 | 0.031 |
| | (0.449) | (1.040) | (0.019) | (0.026) |
| $independent\_director_{j,t-1}$ | -1.063 | 0.221 | 0.029 | 0.071* |
| | (2.011) | (0.838) | (0.042) | (0.040) |
| $independent\_committee_{j,t-1}$ | 1.702 | -0.568 | 0.054 | -0.068 |
| | (1.995) | (0.945) | (0.041) | (0.042) |
| $analyst\_talent_{j,t-1}$ | -0.814 | 0.546 | -0.099*** | -0.083** |
| | (1.711) | (1.165) | (0.037) | (0.034) |
| Other variables | Yes | Yes | Yes | Yes |
| Sample | 1,067 | 1,119 | 24,009 | 25,578 |
| First-Stage F-$\widehat{NPED}_{j,t}$ | 30.09*** | 1.43 | 61.74*** | 320.35*** |
| Anderson-Rubin Wald F Test | 10.37*** | 11.29*** | 12.91*** | 8.48** |
| Panel B: The coefficient of $\widehat{NPED}_{i,t}$ using Alternative Return Measure | | | | |
| BHAR_m_30 | -0.054 | -0.041 | -0.236 | -0.060 |
| | (0.065) | (0.059) | (0.175) | (0.057) |
| BHAR_m_365 | 14.932 | -0.955 | -2.911** | -0.793*** |
| | (14.170) | (0.771) | (1.332) | (0.259) |
| $\alpha_{t+1,t+30}$(×22) | 0.041 | -0.147* | -0.293 | -0.035 |
| | (0.074) | (0.077) | (0.207) | (0.068) |
| $\alpha_{t+1,t+180}$(×126) | 0.066 | 0.016 | -1.812** | -0.124 |
| | (0.165) | (0.135) | (0.763) | (0.157) |

| | | | | |
|---|---|---|---|---|
| $\alpha_{t+1,t+365}(\times 252)$ | 0.088 | -0.045 | -1.765* | -0.466** |
| | (0.214) | (0.160) | (0.923) | (0.208) |
| $ret_{t+1,t+30}$ | -0.116 | -0.059 | -0.316 | -0.079 |
| | (0.096) | (0.083) | (0.218) | (0.069) |
| $ret_{t+1,t+180}$ | 0.269 | -0.199 | -2.929** | -0.374** |
| | (0.340) | (0.236) | (1.211) | (0.191) |
| $ret_{t+1,t+365}$ | 0.903 | -0.845 | -3.436** | -0.472* |
| | (0.815) | (0.557) | (1.740) | (0.278) |
| BHAR_size_30 | -0.016 | -0.092 | -0.335* | -0.072 |
| | (0.082) | (0.075) | (0.201) | (0.059) |
| BHAR_size_180 | 0.427 | -0.226 | -2.104** | -0.415** |
| | (0.324) | (0.228) | (0.923) | (0.174) |
| BHAR_size_365 | 0.952 | -0.840 | -2.647* | -0.744*** |
| | (0.781) | (0.557) | (1.373) | (0.257) |

**Figure S15: Event study difference-in-difference point estimate graphs**