European Financial Management

WILEY

EUROPEAN FINANCIAL MANAGEMENT

ORIGINAL ARTICLE OPEN ACCESS

Executive Equity-Based Compensation and Tournament Incentives

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Received: 19 June 2024 | **Revised:** 14 October 2024 | **Accepted:** 29 October 2024

Keywords: career outcome | executive compensation | insider trading | tournament incentives

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.

JEL Classification: G14, G11, G12, G40, G41

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 so that the board identifies 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, Evans, and Hong 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 consider that insiders will purchase equity in their own firm to seek profit, as documented in the previous literature (Lakonishok and Lee 2001; Cohen, Malloy, and Pomorski 2012), only if their purchases 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,

We thank John Doukas (the editor), two anonymous referees, Massimiliano Barbi, Lauren Cohen, Marc Goergen, Omrane Guedhami, Masatoshi Kurusu, Tse-Chun Lin, Ian Marsh, Lingfei Kong, Lilian Ng, Onur Tosun, Anh Tran, Patrick Verwijmeren, Ru Xie, and seminar participants at Bayes Business School, City St George's, University of London, University of Liverpool, University of Technology Sydney, University of Sussex Business School, University of Bath Business School, CICF 2022, FMA Annual 2021, FMA Europe 2022, EFMA 2022, AFS 2022, FMCG 2023, Corporate Finance Day Lille 2023 for their helpful comments. We are thankful to Professors Gentry, Harrison, Quigley and Boivie for providing the comprehensive CEO turnover data https://zenodo.org/record/5348198#.Y86W7nbP0uV, French for stock returns data to compute the alphas https://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html, Naveen for CEO skills data https://sites.temple.edu/lnaveen/data/, Feenberg for state-level tax data https://users.nber.org/~taxsim/state-rates/, and Zhichuan Li for sharing analyst talent data. Any errors remain our own responsibility.

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). However, in our context, 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 (Armstrong, Blackburne, and Quinn 2021). Instead, we expect them 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 2017).[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. Our primary focus is on the profitability of their trades, but we cover also their trading behaviour. We find that 68% of non-promoted managers remain in their firm at least 2 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. However, after losing the CEO promotion tournament, these executives are more likely to engage in opportunistic transactions with larger size. Their semiannual 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 2 years after losing their CEO promotion. We find that the losing tournament effect is stronger for the large proportion of firms without a formal CEO succession plan, as documented by Cvijanović, Gantchev, and Li (2023), because these firms are more likely to hold open CEO tournament.

We further relax the assumption in the existing tournament incentives literature that all top four highest paid non-CEO 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 among the directors. We, therefore, select tournament competitors based on their specific personal characteristics to assess further their behaviour and their trading probability. We show that their loss-avoiding sell transactions are 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 of their buy trades 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, Blackburne, and Quinn (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, which is our primary focus. 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 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 imply 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 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 before 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 2 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 et al. 2016), are relatively rare. Inspired by the findings of Armstrong, Blackburne, and Quinn (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 (2022) and 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 an 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 6 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, Reis, and Venkateswaran (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 before the tournament, and CFO trades. We construct pseudo-CEO turnovers to test for robustness of our diff-in-diff regressions and conduct 1000 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 on equity-based compensation and tournament incentives, which established that, more than 80% of firms are likely to hold open CEO promotion tournaments, as they do not disclose formal CEO succession plans (Cvijanović, Gantchev, and Li 2023), they make top employees compete for a promotion, a single senior position promotion-based prize (DeVaro 2006; Kale, Reis, and Venkateswaran 2009). The compensation of the non-promoted executives decreases drastically as the implicit promotion-based incentives drop, since their contracts are not altered because of adjustment costs (Chan, Evans, and Hong 2022). They reward themselves by exploiting aggressively their private information without attracting the regulators' attention (Ali and Hirshleifer 2017).[4] This opportunity to sell equity weakens the Kale, Reis and Venkateswaran (2009) positive causal effect of tournament incentives on firm performance. Finally, we document a novel information-based profitable insiders' trading strategy that result in post-trade return predictabilities (Lakonishok and Lee 2001; Cohen, Malloy, and Pomorski 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, Malloy, and Pomorski 2012), but to suit their career concerns and their forgone pay rise after promotion outcome, an unexplored area in previous literature. Since our insiders are not 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.

2 | Sample and Variable Construction

We follow prior literature (Kale, Reis, and Venkateswaran 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 4838 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 follow the only commonly-accepted definition of tournament competitors as the firms' top four highest-paid non-CEO managers, covered by Execucomp (Kale, Reis, and Venkateswaran 2009; Kini and Williams 2012; Gregory-Smith and Wright, 2019; Chan, Evans, and Hong 2022). Their only promotion destination is the CEO position. Tournament losers are defined as the tournament competitors who fail to become the CEO after the competition. We reckon that 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 2 years before CEO turnover event but gained coverage afterwards from the tournament competitor category, as they are either new joiner who joined the firm after the CEO tournament 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 and 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-Reuters 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, Malloy, and Pomorski 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.[5]

We match Execucomp's unique manager identifier *execid* to TR's unique legal entity identifier *personid*. We use BoardEx to cross-check 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 3428 firms with CEO turnover events, out of which 2636 (77%) had internal promotions, close to the 72% reported by Cziraki and Jenter (2022). We find 1259 (37%) firms did not report any insider trades in year 0, leaving 2169 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.[6] For insider transactions executed before the CEO turnover date in year 0, we exclude them from our posttreatment sample.

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 mkt_{t+i} is the value weighted CRSP index. We measure BHAR 1 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:

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

where $Treat_{i,t}$, is equal to one for insider transaction i in our treated firms, and $Post_t$, is equal to one for 2 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 β_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, γ for month and ρ for firm fixed effects.

To control for insider characteristics, we include CEO trading direction, promotion of an outsider CEO, succession planning before CEO turnover, tournament incentives following Coles, Daniel, and Naveen (2006) detailed in Supporting Information S1: Appendix S1, and high incentive managers.[7] 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, Daniel, and Naveen (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.[8] 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 1331 of 1775 (75%) with at least one insider sell trade in the tournament year.[9] 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 S20.

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 represent 94 per cent of their total 13,984 net trades, and 73 percent and 85% of the number and value of shares traded, respectively. They tend to sell in larger firms and those with high pay gap, total compensation,

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.

	Panel A: Buy trades			Panel B: Sell trades		
Variable	**Mean**	**Median**	***N***	**Mean**	**Median**	***N***
pay_gap_firm ($000 s)	2079	674	832	3340***	2147[a]	13,019
Non-CEO comp ($000 s)	1070	681	832	2143***	1346[a]	13,062
illiq (000 s)	0.576	0.087	832	0.032***	0.005[a]	13,062
marketcap ($million)	1765	545	832	14,112***	3361[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
$CEO\_IT\_Net\_Value_{i,t}$ ($000 s)	300	−42	832	−2,581***	0000[a]	13,062
Mean no. shares traded	10,176	2000	832	27,781***	10,000[a]	13,062
Mean trade value ($000 s)	163	19	832	944***	327[a]	13,062
Average no. of observations	416			6531		

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, Malloy, and Pomorski 2012). Supporting Information S1: Appendix S6 reports the full set of summary statistics for buy and sell samples.

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 before 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, Lyandres, and Michaely (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 conjecture 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, Malloy, and Pomorski (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 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 columns (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 remain robust. The results in column (3) show that every opportunistic sell transaction is associated with a 1.1% lower CEO promotion probability, 4.4% lower than the 25% unconditional probability of becoming CEO.

Next, we assess whether non-promoted insiders are more likely to execute opportunistic transactions and make larger transactions after losing the CEO tournament. We re-estimate Panel A using firms with CEO turnover event. The dependent variable $opp\_D_{i,t}$ in columns (1) and (3) is a dummy equal one for opportunistic trades i and zero for routine ones. The dependent variable $tran\_size_{i,d}$ in columns (2) and (4) is the ratio of total shares traded normalized by the total shares outstanding, which we multiply by one thousand for reporting purposes. Columns (1) and (3), 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 unreported results, we find that insiders are more likely to sell rather than to purchase shares after losing the CEO promotion opportunity in all transactions and in our opportunistic transactions sub-sample. The coefficient of $constrained_{j,t-1}$ in columns (3) for the sell trade is negative and significant. This suggests that non-promoted executives increase their propensity to sell opportunistically in year (0, 1) when their firms are more financially constrained (Phan, Simpson, and Nguyen 2017).[10] The coefficient of $CEO\_IT_{j,t}$ is positive and significant for buy trade in column (1), but negative and significant for the sell trades in column (3). 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. In column (4), the coefficient of $Post_{i,t}$ is positive and significant, indicating that tournament losers systematically sell more shares after losing the CEO promotion opportunity. 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, Malloy, and Pomorski (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.

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 than those generated without CEO turnover, ceteris paribus. The negative and significant $(\text{Treat} \times \text{Post})_{i,t}$ in Columns (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 each executes, on average, 3 sell trades per year, their annual abnormal profit accounts for 20% of their salary in these 2 years, and 33% if they release their capital gains in one calendar year. They mainly sell to compensate themselves because the losing tournament effect persists until year +1, 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, Malloy, and Pomorski 2012). Our results hold when we include firm-insider and year-fixed effects.

We recognize that not all firms may hold CEO promotion tournaments. We follow Cvijanović, Gantchev, and Li (2023) and search all proxy statements to identify firms with CEO succession plan before CEO turnover. We exclude 20.8% sample that report a formal CEO succession plan, comparable with the 16.4% reported in Cvijanović, Gantchev, and Li (2023). Panel B shows stronger loss-averting sell trades of 2.6% and 5.5% in years (0, 0) and (1, 1), respectively.

We then consider the possibility that the motives for the CEO turnover will affect the insider trading aggressiveness. We follow Gentry et al. (2021) and split the treated sample and its nearest-neighbour 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 behavioural 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 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.

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

TABLE 2 | Insider Trading Propensity after Losing the CEO Competition.

Panel A reports the linear probability model 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, Malloy and Pomorski (2012). Other independent variables included but not reported are $ret30_{j,t-1,(d-1,d-30)}$, $mom_{j,t-1,(d-31,d-364)}$, $bm_{j,t-1}$, $illiq_{j,t-1}$, $total\ asset_{j,t-1}$, $roa_{j,t-1}$, $tobin's\ Q_{j,t-1}$, $leverage_{j,t-1}$, $constrained_{j,t-1}$. Panel B reports the linear probability regression output. 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 clustered at the firm level for linear probability model. $tran\_size_{i,d}$ is the ratio between total number of shares traded by insiders in day *d* over total number of shares outstanding. $tran\_size_{i,d}$ is scaled by timing 1000. 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

	Routine and opportunistic trades		**Opportunistic trades only**	
	$CEOD_{i,t}$ (1)	**$CEOD_{i,t}$ (2)**	$CEOD_{i,t}$ (3)	**$CEOD_{i,t}$ (4)**
$age_{p,t-1}$	−0.006***	−0.005**	−0.005**	−0.005**
	(0.002)	(0.002)	(0.002)	(0.002)
$tenure_{p,t-1}$	0.006*	0.006*	0.011**	0.012***
	(0.004)	(0.004)	(0.004)	(0.005)
$COOD_{j,t-1}$	0.429***	0.426***	0.390***	0.390***
	(0.036)	(0.035)	(0.039)	(0.039)
$\#\_buy_{p,t-1}$	0.070**		0.063*	
	(0.029)		(0.033)	
$\#\_sell_{p,t-1}$	−0.007*		−0.011**	
	(0.004)		(0.005)	
$\#\_buy_{p,(t-2,t-1)}$		0.088***		0.056**
		(0.027)		(0.026)
$\#\_sell_{p,(t-2,t-1)}$		−0.013**		−0.006*
		(0.005)		(0.004)
$constrained_{j,t-1}$	−0.010	−0.012	0.015	0.009
	(0.020)	(0.020)	(0.028)	(0.028)
$delta_{p,t-1}(\times 0.01)$	0.045***	0.051***	0.018	0.019
	(0.013)	(0.012)	(0.013)	(0.013)
$vega_{p,t-1}(\times 0.01)$	−0.002	−0.005	-0.011	−0.014
	(0.013)	(0.014)	(0.037)	(0.037)
$lncompen_{p,t-1}(\times 0.01)$	0.006***	0.005***	0.006***	0.006***
	(0.002)	(0.000)	(0.002)	(0.002)
Other control	Yes	Yes	Yes	Yes
Fixed effect	Firm, Year	Firm, Year	Firm, Year	Firm, Year
Sample	1100	1104	881	883
With-in R^2	0.46	0.46	0.41	0.41

Panel B: Insider trading propensity after losing the CEO competition

	Insider purchase transactions		**Insider sell transactions**	
	Linear probability $OppD_{i,t}$	**Linear probability $tran\_size_{i,d}$**	**Linear probability $OppD_{i,t}$**	**Linear probability $tran\_size_{i,d}$**
$Post_{i,t}$	0.055**	−0.002	0.028**	0.023**

(Continues)

TABLE 2 | (Continued)

Panel B: Insider trading propensity after losing the CEO competition				
	Insider purchase transactions		**Insider sell transactions**	
	Linear probability $OppD_{i,t}$	**Linear probability $tran\_size_{i,d}$**	**Linear probability $OppD_{i,t}$**	**Linear probability $tran\_size_{i,d}$**
	(0.018)	(0.043)	(0.012)	(0.010)
$CEO\_IT_{j,t}$	0.014*	-0.002	−0.004**	0.001
	(0.008)	(0.010)	(0.002)	(0.006)
$constrained_{j,t-1}$	0.065*	−0.003	−0.020***	−0.011
	(0.035)	(0.032)	(0.004)	(0.013)
Constant	0.920***	−1.157	0.877***	1.080***
	(0.121)	(0.749)	(0.075)	(0.290)
Control Variables	Yes	Yes	Yes	Yes
Sample	542	456	13,404	17,240
Within R^2	0.17	0.17	0.05	0.04
Fixed Effect	Firm, Year	Firm, Year	Firm, Year	Firm, Year

tournament (Kale, Reis, and Venkateswaran 2009; Kini and Williams 2012). However, Chan, Evans, and Hong (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 $(Post \times Treat)_{i,t}$ with our moderator variable and include all main levels. For brevity, we only report the coefficients of $(Post \times Treat)_{i,t}$ and the triple-interaction term in Table 4.

Gibbons and Murphy (1992) and Chan, Evans, and Hong (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, that is, 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 coefficients of $(Post{\times}Treat{\times}lnage)_{i,t}$ are significant, but negative in buy sample and positive 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, as they did not place much implicit value on their future promotion opportunities because their career horizons are shorter. Similarly, Chan, Evans, and Hong (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 significant; negative for the buy sample, but positive in the 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.

Next, we use insiders' personal investment horizons to proxy for their career horizons. Akbas, Jiang, and Koch (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, Jiang, and Koch (2020) to identify SH sellers, as detailed in Supporting Information S1: Appendix S1. We find only 2.3% (9.2%) of our buy (sell) trades are SH sellers, suggesting that they are less likely to trade after losing 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 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 Supporting Information S1: Appendix S1. We assess whether 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 a CEO promotion in their own firms, in line with Kale, Reis, and Venkateswaran (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

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 $(Post \times Treat)_{i,t}$, 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 Cvijanović, Gantchev, and Li (2023). In Panel C and Panel D, we split the entire treated sample with its nearest neighbour 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						
$Post_{i,t}$	0.016	0.016	−0.034	0.013**	0.008	0.024***
	(0.027)	(0.032)	(0.051)	(0.005)	(0.006)	(0.007)
$Treat_{i,t}$	−0.119**	−0.148**	−0.081	0.012*	0.019**	0.012*
	(0.051)	(0.058)	(0.078)	(0.007)	(0.008)	(0.007)
$(Treat \times Post)_{i,t}$	0.085**	0.092*	0.029	−0.027***	−0.020**	−0.042***
	(0.043)	(0.053)	(0.087)	(0.008)	(0.010)	(0.011)
$CEO\_IT_{j,t}$	0.009	0.006	0.046**	0.012***	0.010***	0.012***
	(0.013)	(0.015)	(0.018)	(0.002)	(0.002)	(0.002)
$COOD_{jt}$	−0.041	−0.032***	−0.020	0.038***	0.043***	0.037**
	(0.094)	(0.116)	(0.118)	(0.013)	(0.019)	(0.014)
Sample	2092	1754	1316	45,515	36,231	33,705
Within R^2	0.21	0.21	0.24	0.08	0.15	0.08
Panel B: Exclude pre-planned CEO turnover						
$Post_{i,t}$	0.090	−0.028	−0.096	0.012**	0.008	0.020**
	(0.055)	(0.041)	(0.062)	(0.006)	(0.007)	(0.008)
$Treat_{i,t}$	−0.061	−0.177**	0.001	0.014	0.023**	0.013
	(0.068)	(0.072)	(0.069)	(0.009)	(0.011)	(0.009)
$(Treat \times Post)_{i,t}$	0.058	0.132**	0.058	−0.035***	−0.026**	−0.057***
	(0.078)	(0.061)	(0.103)	(0.010)	(0.012)	(0.014)
Sample	1839	1506	1116	35;864	28,537	26,079
Within R^2	0.29	0.24	0.27	0.09	0.09	0.09
Panel C: Exogenous CEO turnover event						
$Post_{i,t}$	−0.004	−0.126	0.759*	0.052**	0.055*	0.085**
	(0.107)	(0.093)	(0.427)	(0.026)	(0.031)	(0.039)
$Treat_{i,t}$	−0.247	−0.269	0.743*	−0.062	−0.054	−0.012
	(0.220)	(0.228)	(0.436)	(0.048)	(0.059)	(0.056)
$(Treat \times Post)_{i,t}$	0.132	0.437***	−1.920***	−0.189***	−0.143**	−0.210***
	(0.162)	(0.134)	(0.633)	(0.048)	(0.067)	(0.065)
Sample	422	339	263	1573	1250	1177
Within R^2	0.41	0.67	0.61	0.27	0.26	0.24
Panel D: Involuntary CEO turnover event						
$Post_{i,t}$	0.081*	0.020	−0.052	0.025***	0.017*	0.039***
	(0.047)	(0.031)	(0.051)	(0.009)	(0.009)	(0.012)
$Treat_{i,t}$	−0.163*	−0.158**	−0.159**	0.004	0.040	−0.008
	(0.083)	(0.067)	(0.073)	(0.021)	(0.030)	(0.022)

(Continues)

TABLE 3 | (Continued)

	Insider purchase			Insider sell		
Year *t*	**(0, 1)**	**(0, 0)**	**(1, 1)**	**(0, 1)**	**(0, 0)**	**(1, 1)**
$(\text{Treat} \times \text{Post})_{i,t}$	−0.001	0.057	0.063	−0.043**	−0.037*	−0.063**
	(0.083)	(0.058)	(0.126)	(0.021)	(0.022)	(0.030)
Sample	1686	1395	979	18,319	14,489	12,915
Within R^2	0.30	0.24	0.26	0.09	0.09	0.08

subjective probability of becoming CEO ex ante exploit their private negative information more aggressively in their sell trades ex post. The effect is not significant for insider purchases. Overall, our results suggest that executives who are more likely to contest the CEO position trade on their private information to compensate themselves for the forgone promotion incentives.

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 the non-promoted executives are trading on their forgone promotion opportunity rather than other unobservable factors. In this section, we assess whether they 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 are more discontented 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, Blackburne, and Quinn 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, Li, and Wang (2018) and Islam et al. (2022), we construct $ind\_incern_{j,t}$, as outlined in Supporting Information S1: 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]

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 Tucker and Zarowin's (2006) Future Earnings Response Coefficient (FERC) and Piotroski and Roulstone's (2005) return synchronicity to proxy for stock informativeness. In Supporting Information S1: Appendix S8, Panel A and Panel B, we find that insiders' trading profitability does not depend on the future earnings information and the

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, Jiang, and Koch (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. Supporting Information S1: 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		
$(\text{Treat} \times \text{Post})_{i,t}$	2.018**	5.678***	−0.539**	−1.179***
	(1.016)	(1.683)	(0.252)	(0.279)
$(\text{Post}\times\text{Treat} \times \text{lnage})_{i,t}$	−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	1369	1025	32,111	29,481
		Panel B: Tenure effect		
$(\text{Treat} \times \text{Post})_{i,t}$	0.229*	0.677***	−0.057**	−0.122***
	(0.121)	(0.192)	(0.024)	(0.033)
$(\text{Post}\times\text{Treat} \times \text{lntenure})_{i,t}$	−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	1791	1282	36,746	33,527
		Panel C: Investment Horizon		
$(\text{Treat} \times \text{Post})_{i,t}$	0.075	−0.001	−0.017*	−0.028**
	(0.052)	(0.079)	(0.010)	(0.012)
$(\text{Post}\times\text{Treat} \times \text{SHD})_{i,t}$	−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	1791	1282	35,873	33,519
		Panel D: Predicted probability of becoming CEO		
$(\text{Treat} \times \text{Post})_{i,t}$	−0.304***	−0.026	0.037***	0.045***
	(0.109)	(0.176)	(0.014)	(0.017)
$(\text{Post}\times\text{Treat} \times \text{Probability})_{i,t}$	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

level of firm-specific information, ruling out the alternative explanations that tournament losers are trading on the change in information environments for personal gains.

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

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, Li, and Wang (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		
$(\text{Treat} \times \text{Post})_{i,t}$	0.098	−0.074	−0.076***	−0.058***
	(0.108)	(0.203)	(0.022)	(0.019)
$(\text{Post} \times \text{Treat} \times \text{Pay\_rank})_{i,t}$	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	1551	1056	34,808	31,603
		Panel B: Bonus award effect		
$(\text{Treat} \times \text{Post})_{i,t}$	0.652***	0.074	−0.018*	−0.031**
	(0.200)	(0.536)	(0.010)	(0.012)
$(\text{Treat} \times \text{Post} \times \text{BA})_{i,t}$	−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	1554	1059	35,190	31,841
		Panel C: Board conservatism		
$(\text{Treat} \times \text{Post})_{i,t}$	0.299**	−0.387	−0.039**	−0.072***
	(0.137)	(0.248)	(0.020)	(0.022)
$(\text{Treat} \times \text{Post} \times \text{C\_quint})_{i,t}$	−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	1791	1282	36,748	33,527
		Panel D: Industry tournament incentives		
$(\text{Treat} \times \text{Post})_{i,t}$	0.329	−0.781**	−0.097**	−0.162**
	(0.337)	(0.381)	(0.038)	(0.074)
$(\text{Treat} \times \text{Post} \times \text{ind\_incen})_{i,t}$	−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	1471	1030	27,751	25,065
		Panel E: Non-compete agreements		
$(\text{Treat} \times \text{Post})_{i,t}$	−0.016	0.057	0.013**	-0.013
	(0.050)	(0.082)	(0.006)	(0.012)
$(\text{Post} \times \text{Treat} \times \text{NoncomD})_{i,t}$	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	1791	1282	36,944	33,521

using one or 2-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, Reis, and Venkateswaran (2009) and Chan, Evans, and Hong (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, leading the non-promoted directors to yield higher loss-averting sell transactions. We follow Daniel, Li, and Naveen (2020) in measuring the CEO skill, which we include as a moderator. The results in Supporting Information S1: 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 neighbours. 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 these post-transaction returns. 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 keep their total compensation below the suboptimal level for their efforts. Although they will trade to compensate themselves, these trading gains cannot fully adjust their compensation to its optimal level, to maintain their 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 trade on future changes in the operating performance, and in the cost of capital, and/or to exploit changes in investor sentiments. In unreported results, we confirm that the parallel trend assumption is satisfied using the same control firm sample. The constructions of our proxy variables are detailed in Appendix A and Supporting Information S1: Appendix S1. Our results remain robust if we use changes in ROA from year of trading t to $t+1$.

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. To measure the changes in investor sentiment, $\Delta Sentiment$, 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, Lyandres, and Michaely (2021) to measure changes in sentiment from $t-1$ to $t+1$, $\Delta Sentiment_{t-1,t+1}$. To measure the change of cost of capital, $\Delta r_{i,t+2}$, we follow Cziraki, Lyandres, and Michaely (2021) and estimate a modified Fama and French (1993) three-factor model.

Table 6 shows the diff-in-diff results using these three proxies as dependent variables. The coefficients of $(Post{\times}Treat)_{i,t}$ are mainly significant for the sell transactions in columns (3) and (4). Panel A reports that insider sell transactions can significantly predict in year 0 and 1, respectively, 1.7%, and 1% decrease in $\Delta ROA_{t,t+2}$, and in Panel B, 4.7% and 6.3% future decrease in investor sentiment, $\Delta Sentiment_{t-1,t+1}$, compared to the 19.3% and 57.1% increases when insiders buy. Moreover, in Panel C insider purchases do not predict the future decreases in the cost of capital, $\Delta r_{i,t+2}$, in year 0, but their sell trades predict 0.1% increases in both year 0 and 1, as $(\text{Post} \times \text{Treat})_{i,t}$ 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 result in higher ROA decreases and investor sentiment predictability in the next 2 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, Evans, and Hong (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

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, Robinson, and Viswanathan (2005). The change in investor sentiment $\Delta Sentiment_{-1,1}$ is measured between year $t-1$ to year $t+1$. In Panel C, we obtain the $\Delta r_{t,t+2}$ by following Cziraki, Lyandres, and Michaely (2021) to estimate a modified Fama and French (1993) Three-Factor model. In Panel D, we additionally include the moderate variable $News_{j,m}$, 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**	
Year *t*	**(1)** **(0, 0)**	**(2)** **(1, 1)**	**(3)** **(0, 0)**	**(4)** **(1, 1)**
Panel A: Future firm performance				
Dependent variable	$\Delta ROA_{t,t+2}$	$\Delta ROA_{t,t+2}$	$\Delta ROA_{t,t+2}$	$\Delta ROA_{t,t+2}$
$Post_{i,t}$	0.006	0.008	−0.001	−0.003
	(0.012)	(0.012)	(0.003)	(0.003)
$Treat_{i,t}$	−0.097***	−0.039***	0.012***	0.016***
	(0.027)	(0.019)	(0.003)	(0.004)
$(Post \times Treat)_{i,t}$	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	1688	1222	35,580	32,628
Panel B: Investor sentiment				
Dependent variable	$\Delta Sentiment_{t-1,t+1}$	$\Delta Sentiment_{t-1,t+1}$	$\Delta Sentiment_{t-1,t+1}$	$\Delta Sentiment_{t-1,t+1}$
$Post_{i,t}$	−0.080	−0.243**	−0.005	0.036**
	(0.066)	(0.106)	(0.013)	(0.016)
$Treat_{i,t}$	−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	1691	1244	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	1796	1286	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)
	$\Delta ROA_{t,t+2}$	$\Delta ROA_{t,t+2}$	$\Delta Sentiment_{t-1,t+1}$	$\Delta Sentiment_{t-1,t+1}$	$\Delta r_{t,t+2}$	$\Delta r_{t,t+2}$

(Continues)

TABLE 6 | (Continued)

	Insider purchase			Insider sell		
Year *t*	**(1) (0, 0)**		**(2) (1, 1)**	**(3) (0, 0)**		**(4) (1, 1)**
$(\text{Treat} \times \text{Post})_{i,t}$	−0.012**	0.003	−0.013	−0.019	0.001**	0.000
	(0.005)	(0.004)	(0.028)	(0.033)	(0.000)	(0.001)
$(\text{Treat} \times \text{Post} \times \text{News})_{i,t}$	−0.002**	−0.001*	−0.010**	−0.011**	−0.000	−0.000
	(0.001)	(0.01)	(0.004)	(0.005)	(0.000)	(0.000)
$\text{News}_{j,m}$	−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

regressions in Table 6. Our results, reported in Supporting Information S1: Appendix S9, remain overall robust, suggesting that insiders trade on their private information regarding their 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. Each year we identify firms in the top quantile of the large industry tournament incentives. In unreported results, we find that our previous results remain robust, but $\Delta r_{i,t+2}$ becomes insignificant for firms with high industry tournament incentives, indicating that industry tournament incentives do not mitigate the adverse effect. Overall, these results suggest that tournament losers 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

Corporate insiders are undiversified individuals because their personal wealth is concentrated in their firms. 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. On the other hand, exiting managers will make uninformed sell transactions because these trades will reduce their exposure to firm-specific risk. To test this hypothesis, we first estimate the same diff-in-diff regressions with different dependent variable. Supporting Information S1: Appendix S10 reports the results. In columns (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. We use insider trading transactions to predict the likelihood of managers leaving the company. 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 exiting managers' trades are less profitable than those of their staying counterparts, and thus, they are more likely to leave the firm. On the other hand, the 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 1 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 $(Post{\times}Treat)_{i,t}$ is negative and statistically significant for both purchase and sell samples in columns (1) and (3), suggesting that exiting managers are less likely to trade after losing the promotion opportunity. Columns (4) show that the post-trade profitability is positive and significant for sell sample, indicating that exiting managers do not trade on their private negative information, a result consistent with Panel A. These results suggest that the exiting insiders systematically make less non-informative buy and sell trades not mainly for compensating themselves but for reducing firm-specific risk.[14]

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 more than 40% of firm j tournament contenders left the firm in the next 2 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.

The results in Supporting Information S1: 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 with past high insider trading profitability. 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 these informed transactions 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 in eliminating 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 to weaken 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, Reis, and Venkateswaran (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, Reis and Venkateswaran (2009):

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

where firm performance is *Tobin's Q* and *ROA*, $pay\_gap_{j,t}$ proxies for tournament incentives as previously specified. ρ is firm fixed effect, and δ is year fixed effect. We cluster the standard error at the firm level. Appendix A defines the remaining variables. We expect β_1 to be statistically significant and positive, because, as in Kale, Reis, and Venkateswaran (2009), the higher tournament incentives, the better the firm performance. We estimate a 2SLS model with two first-stage regressions. We follow Kale, Reis, and Venkateswaran (2009) and use the median value of tournament incentives of control firms in the same sales quintiles and two-digit SIC industry as instrumental variable because it is a significant determinant of the amount of each firm's tournament incentives. Our second stage regression is:

$$\begin{aligned} \text{firm\_performance}_{j,t} = {} & \alpha + \beta_1 \text{pay\_}\hat{g}\text{ap}_{j,t} \\ & + \beta_2 (\text{pay\_gap} \hat{\times} \text{all\_IT})_{j,t} + \beta_3 \\ & \text{all\_IT}_{j,t} + X_{i,t} + \varepsilon_{j,t}, \end{aligned} \tag{4}$$

where $all\_IT_{j,t}$ is the non-CEOs total number of trades in firm j in year t, to proxy for their trading intensity. We expect β_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 lead to changes in share ownership as managers may sell (hold) more shares when they anticipate tax increases (decreases). The variation in state tax laws across states and years is also 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) show that, in line with Kale, Reis, and Venkateswaran (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 a 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 $pay\_\hat{g}ap_{j,t}$ is positive and statistically

TABLE 7 | Insider trading and tournament incentives.

The table reports the results of a shorter version of Kale, Reis, and Venkateswaran (2009) regression specification. In columns (1) and (2), we regress Tobin's *Q* and ROA on all control variables with firm and year-fixed effects, respectively. In columns (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 columns (3) and (4). In columns (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 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.

	Fixed effect		**2SLS-Second stage**			
			One IV		**Two IVs**	
Dependent variable	**Tobin's $Q_{j,t}$** **(1)**	**$ROA_{j,t}$** **(2)**	**Tobin's $Q_{j,t}$** **(3)**	**$ROA_{j,y}$** **(4)**	**Tobin's $Q_{j,t}$** **(5)**	**$ROA_{j,y}$** **(6)**
$pay\_gap_{j,t}$	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)
$\widehat{pay\_gap} \times all\_IT_{j,t}$			−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

significant, further supporting the findings in Kale, Reis, and Venkateswaran (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 unreported results, 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 $\widehat{pay\_gap}_{j,t}$. Our results suggest 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.

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 pretournament 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 the newly appointed CEO.

We use the last fiscal year's former CEO age as our IV.[15] 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.[16] 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 6 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 Panel B report that the coefficients of $age\_ceo_{j,t-1}$ in our first-stage regressions are all statistically significant with the expected signs, confirming 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 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.

Table 8, Panel C, reports the second-stage regression results. For insider purchase sample, the coefficients of both $\widehat{NPED}_{i,t}$ and $NPED \times \widehat{CEO\_IT}_{i,t}$ 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, Malloy, and Pomorski 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. $(NPED \times \widehat{CEO\_IT})_{i,t}$ 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, Blackburne, and Quinn (2021). CEO purchase transactions embed a strong signalling 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 Supporting Information S1: 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. Supporting Information S1: 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.

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 when we re-estimate the regression specification in Table 8, Panel C, using OLS estimator. Overall, $NPED_{i,t}$, the dummy variable equals 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.

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)
$NPED \times \widehat{CEO\_IT}_{i,t}$	-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	2441	2628	37,757	40,785
Control	Yes	Yes	Yes	Yes
Difference in Sargan C (χ^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

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 our 2SLS regression 14 additional control variables that embed predictive power for the firm's future fundamentals and are possibly determined by the personal preferences of CEOs in different age groups to better

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, Malloy and Pomorski (2012). In Panel D, we remove CFO transactions from our sample and replicate our previous results. In Panel E, we report the coefficients of $(Post \times Treat)_{i,t}$ 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				
$(Post \times Treat)_{i,t}$	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				
$(Post \times Treat)_{i,t}$	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	1154	1238	28,011	28,893
Panel D: No CFO trades				
Diff-in-Diff regression				
$(Post \times Treat)_{i,t}$	0.099*	0.007	−0.023**	−0.027**
	(0.060)	(0.098)	(0.011)	(0.013)
Sample	1509	1129	31,374	20,532
2SLS				
$\widehat{NPED}_{i,t}$	1.201*	1.184	−3.202*	−0.416***
	(0.0711)	(2.924)	(1.678)	(0.107)
Sample	1791	859	33,464	34,622

(Continues)

TABLE 9 | (Continued)

	Insider purchase		Insider sell	
	(0,0)	**(1,1)**	**(0,0)**	**(1,1)**
Panel E: The coefficient of (Post × 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)
$\text{ret}_{t+1,t+30}$	−0.024	−0.028	0.001	−0.009*
	(0.028)	(0.059)	(0.004)	(0.005)
$\text{ret}_{t+1,t+180}$	0.072	0.013	−0.021**	−0.042***
	(0.067)	(0.109)	(0.010)	(0.012)
$\text{ret}_{t+1,t+365}$	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)

demonstrate the validity of the exclusion restriction and the robustness of our results. Supporting Information S1: Appendix S14 Panel A reports the result. For insider sell samples, the sign and significance of $\widehat{NPED}_{i,t}$ and $NPED \times \widehat{CEO\_IT}_{i,t}$ are consistent with our previous results. As a 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 a 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 four 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 3 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.

We additionally conduct 1000 placebo tests for both the difference-in-difference regression and 2SLS regression by picking random firms, as detailed in the Supporting Information S1: 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, Cicero, and Wintoki (2020) to adjust the trading profitability to account for their dissimulation trading strategies. We explain the process in the Supporting Information S1: Appendix S1. Overall, our results are similar, and they become stronger after adjusting for their dissimulation strategies.[17]

Finally, we 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 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 1 day after the transaction date over the next 30/180/365 calendar days. $return_{j,t}$ is the daily return, rf_m is the risk-free 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. Supporting Information S1: Appendix S14 Panel B reports only the coefficient of $\hat{NPED}_{i,t}$ 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. Lastly, we remove all firms that have a CEO succession plan before CEO tournament and replicate Tables 2–6 using the remaining sample, Internet Appendix S15-19 display these results. Overall, our results become even stronger after removing the small proportion of firms that disclosed their CEO succession plan.

5 | Conclusion

We investigate the relationship between losing the promotion opportunity and the informed trading 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 before 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 illegally 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. Moreover, 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 and other factors will better eliminate endogeneity and alter or confirm our results is the subject of further research.

Data Availability Statement

Data subject to third-party restrictions—The main data that support the findings of this study are available from Compustat, CRSP and Refinitiv. Restrictions apply to the availability of these data, which were used under license for this study.

Endnotes

[1] See, 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.

[3] In behaviour 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 wealthiest list).

[4] In conventional insider trading models, informed agents' trading aggressiveness α 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.

[5] We find similar results using net number of shares (Lakonishok and Lee 2001). We exclude 0.3% cases with 0 NPV.

[6] We report in Supporting Information S1: Appendix S1 the matching method, in Supporting Information S1: Appendices S2 and S3 the details of our samples and the annual distribution of CEO turnover, and in Supporting Information S1: 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. 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 in the same year.

[7] 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.

[8] 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 1-month lagged control variables.

[9] Our results are robust if we do not use diff-in-diff specification with a matched sample.

[10] We follow Hadlock and Pierce (2010) to compute financial constraint index. Our results are robust to using the financial constraint index proposed by Whited and Wu (2006) and the log of total dollar value of shares traded.

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

[12] Detailed results are in Supporting Information S1: Appendix S7 where we also include litigation risk industries as moderator variable. We have also replicated the results in Tables 4 and 5 using NPV as the dependent variable. We find, but do not report for space reasons, that non-promoted executives are less likely to sell on their private information to compensate themselves for the forgone CEO promotion opportunity when their forgone promotion opportunity has lower value.

[13] We report the remaining results in this section in Supporting Information S1: 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 Supporting Information S1: 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.

[14] Under SEC rule 16a-2(b) executives need to file their trades for 6 months after they have left their firms.

[15] For example, Allan Ray Landon became the CEO of the Bank of Hawaii Corporation in 2005, our year 0. For insider trades that occurred in 2005 and 2006, we use the age of the former CEO, Michael E. O'Neill, who was 58 and 59 in these 2 years, respectively, as instrumental variables. For insider trades in 2004, our 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.

[16] 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.

[17] We do not report the placebo, dissimulation strategy, and the pseudo-CEO turnovers test results for brevity. They are available upon request.

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Supporting Information

Additional supporting information can be found online in the Supporting Information section.

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*.
$all\_IT_{j,t}$	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 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.
$bm_{j,m-1}$	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 (Compustat: txditc, zero if missing) - (Compustat: seq), or (Compustat: ceq) plus (Compustat: pstk) or the difference between (Compustat: at) and (Compustat: lt), in that order, as available. Preferred stock value is (Compustat: pstkrv), or (Compustat: pstkl), or (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 5-year window (*t, t + 4*). We require there are at least a 3-year data to compute this variable. For each of the four quarters in the year, we compute the mean values across the 5-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.
$Constrained_{j,t-1}$	Compustat	The yearly industry portfolio decile number for firms that had financial constraint index constructed by following Hadlock and Pierce (2010).
$COOD_{j,t}$	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).

(Continues)

Variable notation	Data source	Definition
$\text{dividend} - \text{yield}_{j,t-1}$	Compustat	Dividends per share by ex-date divided by the close price for the fiscal year (Compustat: dvpsx_f over prcc_f).
$\text{FERC}_{j,t}$	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.
$\text{firm\_focus}_{j,t-1}$	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.
$\text{high\_incentiveD}_{p,t-1}$	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*.
$\text{illiq}_{j,m-1}$	CRSP	Amihud, (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.
$\text{independent\_manager}_{j,t-1}$	Boardex	Percentage of independent managers on the company board.
$\text{independent\_committee}_{j,t-1}$	Boardex	Percentage of independent managers on the company compensation committee.
$\text{institution\_ownership}_{j,q-1}$	Thomson Reuter 13F Holding	Percentage of shares owned by institution investors over total shares outstanding in the last quarter.
$\text{leverage}_{j,t}$	Compustat	Long term debt plus debt in current liability) over the total assets (Compustat: (dltt+dlc)/at)).
$\text{lnage}_{p,t}$	Execucomp	The natural logarithm of the current age of the manager *p* in year *t*.
$\text{lncompen}_{p,t-1}$	Execucomp	The natural logarithm of *tdc1* adjusted by following Coles, Daniel, and Naveen (2014) and Brockman, Lee, and Salas (2016).
$\text{tran\_size}_{i,d}$	Thomson Reuter 13F Holding, CRSP	The ratio between total number of shares traded by insiders in day *d* over total number of shares outstanding. $\text{tran\_size}_{i,d}$ is scaled by timing 1000.
$\text{mom}_{j,(d-31,d-364)}$	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.
$\text{News}_{j,m}$	Key Development	The number of discretionary news (Edmans et al. 2018), released in insider trading month *m* for firm *j*.
$\text{NPED}_{i,t}$	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.
$\text{NPV}_{p,d}$	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.
$\text{numest}_{j,m-1}$	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.
$\text{opp\_D}_{i,t}$	Insider Filling	Dummy variable equal to one for opportunistic insider transactions, defined, following Cohen, Malloy, and Pomorski (2012).
$\text{OutsiderD}_{j,t}$	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.
$\text{pay\_gap}_{j,t-1}$	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.

(Continues)

Variable notation	Data source	Definition
$pay\_rank_{p,t-i}$	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 calculated as Peters and Wagner (2014).
$rd_{j,t-1}$	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 (2005), 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).