Tournament incentives and insider trading

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Abstract

I show that high-ranked non-CEO directors trade on their private negative information opportunistically and more aggressively after losing the CEO promotion opportunity to compensate themselves for the forgone pay rise associated with the CEO position. Consistent with the implication of the tournament incentive models, the non-promoted directors intentionally make more sell transactions. They trade profitably on investors' sentiments, and on their firm's future declining performance and increase in its cost of capital. Using instrumental variable to address the reverse causality concern, I find that this insider trading opportunity weakens the well-documented positive relationship between tournament incentives and firm performance.

Keywords: Insider Trading; Tournament Incentives; Director Compensation; Career Outcome
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I. Introduction

On 1st November 2016, The Toro Company (NYSE: TTC) internally promoted Richard M. Olson to be the next CEO, replacing the eleven-year incumbent Mr Michael J. Hoffman, with a subsequent increase in his total compensation package from $1.5 million to $4 million. The other three internal CEO candidates who missed the promotion and the remuneration awards stayed with the firm. The following year, they executed seven loss-avoiding sell trades with an average yearly abnormal buy-and-hold return of -13.78% and generated 40.43% (41.89%) lower yearly abnormal returns than their sell transactions executed one year (two years) before the CEO decision was made.

I investigate why such non-promoted directors' transactions become drastically more informative after losing the CEO promotion opportunity. I argue that the loss of future promotion opportunity and the forgone rise in compensation associated with the CEO position motivate them to exploit their informational advantage by trading on their private information more aggressively. I base my argument on the intersection between tournament incentives and insider trading literature.

The former has established that firms hold promotion tournaments by making several top employees compete for a single more senior position promotion-based prize, which is the increase in compensation (DeVaro, 2006; Kale, Reis and Venkateswaran, 2009). Cvijanovic, Gantchev and Li (2021) show that 83.6% of S&P 1500 firms do not have a formal CEO succession plan and hold open CEO tournaments for competition. Employees are willing to accept contracts that offer them explicit incentives such as annual salary and bonuses below the optimal levels for their effort, because they value the chance of future promotion; they incorporate the expected increase in the explicit incentives associated with the promotion into their contracts (Lazear and Rosen, 1981; Main, O'Reilly and Wade, 1993). At the highest level of the corporate hierarchy, the CEO position and pay are the only promotion destination and ultimate tournament prize that incentivise senior non-CEO directors to exert efforts to win. Kale, *et al.* (2009) find a positive relationship between the amounts of pay increase non-CEOs expect to receive if they successfully realise the promotion-based incentives and firm performance.

However, senior directors who lose the first CEO promotion tournament during their time in the firm see a significant reduction in their likelihood of winning the next round of CEO tournament in the same firm. Consequently, there is a drastic decline in the overall value of tournament losers' contracts because the value of their implicit promotion-based incentives is much lower, if not foregone completely. Since firms are restrained from adjusting their contracts to compensate them for the forgone compensation opportunity and restoring the explicit incentives to the optimal level (Chan, Evans and Hong, 2019), more competent directors leave the firm to participate in other firms' tournaments rather than face compensation contract below the optimal level. This contributes to the high turnover rate among senior directors observed empirically following the appointment of a new CEO (Chan, Evans and Hong, 2022; Gregory-Smith and Wright, 2019).

I hypothesise that non-promoted directors who choose to stay with the firm, and costly to layoff, will be motivated to compensate themselves for the forgone promotion opportunity by exploiting their private information more aggressively because their contracts are now worth less, and the explicit incentives are below the optimal level. One strategy is to trade on more price-sensitive private information to generate higher abnormal returns as corporate insiders are closely involved with the firm's daily operation and have superior access to price-sensitive information and trading on this price-sensitive information is profitable and rarely attracts the market regulator's attention (Ali and Hirshleifer, 2017).[2] Empirical evidence has unanimously documented that corporate insiders actively trade on their private information regarding their firms' future to generate excess returns, resulting in return predictabilities following both insider purchase and sell transactions (Lakonishok and Lee, 2001; Cohen, Malloy and Pomorski, 2012; Biggerstaff, Cicero and Wintoki, 2020). Their transactions become drastically more informative before some specific corporate events, such as the release of quarterly earnings announcement (Ali and Hirshleifer, 2017), around M&A rumour (Davis *et al.*, 2020), when there is a worsening in the industry level information environment (Contreras and Marcet, 2021), and if they narrowly miss their performance-based bonus (Gao, 2019). This evidence suggests that insiders will intentionally trade on their private information more aggressively when the expected gain is large enough to outweigh the associated litigation risk and to maximise their private benefits. I extend this evidence by assessing the extent to which the gains from their trades will compensate them for the foregone CEO promotion opportunity.

I use a sample of 165,705 US non-CEO director's insider transactions undertaken by 21,723 non-CEO insiders between 1996 and 2019 to assess whether non-promoted directors will trade on their private information with greater aggressiveness following the loss of CEO promotion opportunity. One main concern in the insider trading literature is endogeneity, as the true motivations behind insider transactions, including private information, personal liquidity need and portfolio diversification, are not directly observable, leading to random post-transaction returns, and the omitted variable bias will subsequently result in inconsistent estimates. I use two approaches to mitigate this problem. Firstly, I specify a stacked diff-in-diff regression based on matched sample to isolate the losing CEO tournament effect within the event year $(-2, 1)$. I match my test firms with a control group without CEO turnover by total assets, average insider trading profitability and book-to-market ratio one year before my test firms' CEO turnover. Second, I additionally apply two-stage least square (2SLS) estimator by using the age of former CEO who has left the firm on average six years ago, as instrumental variable (IV) to further generalise the finding outside my event window.

[2] In a traditional insider trading model, an informed agent's trading aggressiveness α is increasing in his risk tolerance (Cespa, 2008). Since there is a decrease in insider's overall compensation value, her risk tolerance should become higher because the expected loss of losing her job is lower if regulators prosecute them for illegal insider trading. Consequently, I hypothesise that insiders will bear higher litigation risk and trade on their private information more aggressively.

I find that non-promoted insiders execute more opportunistic sell, but not buy, transactions in the next two years after losing the CEO promotion. I document that the insider purchase (sell) transactions are systematically more profitable (loss avoiding) in the year that these non-promoted directors lose CEO competition, and their gains from their sell trades persist one year after the CEO turnover. I find that the buy trades executed by insiders in the year of losing their CEO competition yield on average an excess of 24.5% profit than these transactions would have generated without CEO turnover. The corresponding average treatment excess effect is 3.0% in year 0 and 4.8% in year one for their loss-avoiding sell trades. I report that for firms that planned a CEO successor prior to the tournament, the losing tournament effect becomes weaker, consistent with the hypothesis that assigning a CEO successor is a way of depressing the discontent among non-promoted directors.

I conduct additional tests to investigate the motivations behind these informed insider trades. I focus on two non-mutually exclusive hypotheses: (i) compensation for the forgone CEO promotion prize known as *forgone incentives hypothesis*, or (ii) exploiting the stock mispricing after a major corporate change referred to as *stock misevaluation hypothesis*. In the first case, I expect insiders with larger pay difference with their CEO before the tournament outcome to trade on their private information more aggressively because of the higher opportunity loss than insiders whose compensation is already close to the current CEO's. In the same logic, the increase in the return predictability should be higher for younger insiders than older ones because the former have a higher expected value on the promotion-based components in their remuneration contracts as their career horizons are longer. In contrast, the latter are closer to their retirement and should have placed less importance on the future promotion opportunity. Similarly, I conjecture that short investment horizon insider sellers also have shorter career horizons because they frequently reverse their previous buy positions to reduce their ownerships (Akbas, Jiang and Koch, 2020). Lastly, directors with higher probability of becoming CEO but failed to be promoted should trade on their private negative information more aggressively because they have higher expected value of implicit promotion-based incentives. Thus, they will trade with lower aggressiveness to compensate themselves for the forgone promotion opportunity. My results support these hypotheses for insiders' sell trades and suggest that insiders trade on negative insider information for personal gains and probably to undermine the performance of the newly promoted CEO.

To test for the firm-level informativeness, I follow Tucker and Zarowin (2006) and construct the future earnings response coefficient, and Piotroski and Roulstone (2004) to calculate the return synchronicity. I expect insiders' sell trades to be less profitable when the future earnings response coefficient is lower, and their buy trades do not vary with these two firm-level informativeness measures. I find no significant relationship between the return synchronicity and insider transaction profitability. I show that the change in insider trading profitability is robust to the inclusion of these two proxies, suggesting that the increase in profitability is attributed to insiders trading on the stock misevaluation, but also is a way of compensating themselves for the forgone CEO promotion opportunity. I investigate

the informational content behind these more informed insider transactions to show that unobservable stock and market movement do not randomly drive the higher abnormal profit. I find their sell trades systematically predict the future decreases in both return on asset and investor sentiment, and an increase in the future cost of capital, but this is not the case for their purchases.

Inspired by these results, I investigate the possibility that insiders will trade to realize their promotion awards before the announcement of the next CEO. If they can trade *ex-post* the tournament, there is nothing to prohibit them from trading *ex-ante*. Consequently, the positive causal effect between the tournament incentives and firm performance may not be as high as documented by Kale *et al.* (2009). To investigate this possibility, I first replicate the results of Kale *et al.* (2009). I show that the positive causal relationship between tournament incentives and firm performance persists in my sample period. Following Kim and Lu (2011), I further use the sum of the maximum marginal federal and state long-term capital gain tax rates as my IV for the total non-promoted insider trading transactions. I find a weaker causal relationship between the tournament incentives and firm performance when non-CEO insiders execute more transactions, further confirming my hypothesis that insiders trade to realise their tournament incentives *ex-ante* the release of the tournament outcome.

I consider that tournament competitors may avoid trading on their private negative information that adversely lowers their winning probabilities, and tournament losers are more likely to be those insiders who trade on their private negative information more aggressively. I employ two approaches to address this possible reverse causality. First, a 2SLS estimator to generalize the results outside the CEO turnover event window and investigate whether the increase in insider trading profitability is significantly higher than their unconditional return predictabilities. I use as an IV the former CEO's age in the last fiscal year, which is a publicly available information, not correlated with the firm's future fundamental that insiders are exploiting because former CEO left the firm six years ago on average, but it empirically embeds predictive power for the future CEO turnover. I show that the increase in the return predictability embedded in both insider purchase and sell trades following the CEO turnover persists when I take insider transactions outside the CEO turnover event window into consideration. The more negative abnormal return predictability embedded in insider sell transactions persists two years after losing the CEO promotion opportunity. Their sell, but not their buy, trades yield more negative abnormal returns when the newly appointed CEO increases her holdings, suggesting that they intentionally incorporate more negative private information into their transactions to trade against the current CEO, in line with Armstrong *et al.* (2020) who argue that that newly appointed CEO is likely to be noisy trader. Second, I show that insider transactions embed little predictive power for the CEO promotion outcome in my robustness tests. Furthermore, I consider that insiders will dissimulate their private negative information by making sequential sell transactions and randomly mixing with uninformative purchase transactions to thwart outsiders and market regulators. I show that the losing CEO competition effect becomes stronger after accounting for this insider trading strategy.

To test the appropriateness of my matching algorithm, I follow Angrist and Pischke (2009) and Cengiz *et al.* (2019) and conduct an event-study type diff-in-diff regression to show the parallel trend assumption. I further test the validity of the exclusion restriction of my IV by considering the possibility that former CEOs may have adapted long-lasting corporate policies, affecting a firm's future fundamentals. I additionally include another fourteen control variables that proxy for the possible channels in which the age of a former CEO can indirectly affect the firm's future value. I find robust results and provide evidence that the exclusion restriction of my IV is satisfied. Furthermore, I show that former CEO's age contains little predictive power for non-CEO insider trading profitability outside the CEO turnover event, further stressing the exclusion restriction plausibility. My results are robust when I use different return proxies, control for performance-induced CEO turnover, and when I remove firms with a COO prior to the tournament and CFO trades. I construct pseudo-CEO turnovers to show the robustness of my diff-in-diff regression and conduct 1,000 placebo tests for diff-in-diff and 2SLS regression separately to rule out the possibility that these significant results are due to luck.

I contribute to the literature from three aspects. First, I focus on two streams of literature, tournament incentives and insider trading, which although both study the directors' behaviours, the ongoing investigations in these two domains are largely parallel and do not intersect. To the best of my knowledge, this is the first empirical analysis to bridge these two streams of literature. I show that the realisation of their tournament incentives affects insiders' trading. Second, I contribute to the tournament incentives literature by documenting an unintended consequence of holding a CEO tournament that is causing more aggressive insider trading activities. I report that insider trading opportunity weakens the positive effect of tournament incentives on firm performance documented by Kale *et al.* (2009). My results imply that the compensation committee must consider the opportunity of trading on private information to set out the optimal level of tournament incentives because the tournament incentives are not as effective as the compensation committee reckons because tournament rejectees can compensate themselves *ex-post*. Unlike many tournament incentive studies, my paper uniquely focuses on these "rejectees", and I shed light on losing competitors' investment decisions to show that their career concern affects their trading decisions. Finally, I contribute to the insider trading literature by documenting one more corporate event in which insiders systematically incorporate private information into their trading decisions to seek higher abnormal returns. My results suggest that insiders adjust their trading strategies depending on their career concerns and the forgone pay rise, an unexplored area in insider trading literature.

The remainder of the paper proceeds as follows. In Section I, I review the relevant literature. Section II describes my sample and the constructions of variables, justifies the exclusion and relevance conditions of my IV and specifies my regression. Section III presents the empirical results and revisits the results of Kale *et al.* (2009) by accounting for the role of insider trading opportunity. Section IV presents the 2SLS estimation results, robustness, and placebo tests. The conclusions are in Section V.

II. Literature Review and Hypotheses development

A CEO promotion tournament involves a contest amongst senior executives to become the firm's next CEO. The winner will receive the corresponding promotion-based monetary rewards, such as remuneration, benefits, and other privileges. The increase in the winner's compensation package, referred to as the tournament incentives, is possibly the largest in her lifetime. The losers will either be laid off, but at a cost, stay in the same firm and wait for the next chance for advancement, or leave to participate in tournaments in other firms (Lazear and Rosen, 1981; Gibbs, 1995; DeVaro, 2006). Boards hold promotion tournaments to encourage agents to exert effort, identify the most suitable senior manager for the CEO position, and improve firm performance.

Theorists have supported the logic behind the tournament-type CEO succession. Lazear and Rosen (1981), Gibbons and Murphy (1992) and Main, O'Reilly and Wade (1993) developed model on tournament incentives where senior executives endure pay below the optimal market rates because they not only value the explicit incentives such as the regular increase in their salaries, stock options and annual bonuses but incorporate the implicit value of the future promotion opportunity. The implicit value of the future promotion opportunity depends on both the promotion subjective probability and the subsequent increases in their compensation packages if they eventually win it (Kale *et al*, 2009). Gibbons and Murphy (1992) show that an optimal incentive contract must optimise the combination of employee's career concern regarding future promotion opportunity and the current explicit incentives. Thus, if the employee is close to her retirement, the subjective probability of future promotion becomes lower, which attributes to the lower expected promotion-based incentives. Consequently, the director will place, largely, more importance on explicit incentives and not value the future promotion opportunity. In the same logic, Holmstrom and Milgrom (1994), and Baker, Gibbs and Holmstrom (1994) have documented the complementarity between explicit and implicit incentives components in designing the optimal remuneration contract. Ederhof (2011) studies the pay structure of a multinational firm in a single year and shows that firms adjust the pay structures of their mid-level managers with fewer promotion levels to reach in the corporate hierarchy by substituting the weaker promotion-based incentives with higher bonus-based incentives, a form of explicit incentives. In the same vein, Gibbs (1995) argues that the tournament prize must rise at an increasing rate when executives are moving up to the corporate hierarchy because principles need to maintain a large enough incentive for the senior executives who already receive relatively high compensations. This pronounces the most the pay disparity between the CEO and other non-CEO senior executives[3], reflecting the strongest implicit incentives at the top level of the hierarchy and justifying the largest compensation gap between the CEO and other senior directors observed in real life.

[3] 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.

I argue that an additional implication of these tournament incentives models is the behaviour of the promotion rejectees, as the loss of a CEO tournament lowers drastically the promotion-based component in their contract, resulting in a decrease in their overall value of their compensation plan, because of, at least, the following four reasons. First, the timing and the outcome of the next round of the tournament is uncertain (DeVaro, 2006). This is because the higher the hierarchical level of the non-promoted director, the fewer the promotion opportunities, as the only promotion destination is the CEO position, a long-tenure job[4]. Second, the negative image of a previous tournament loser will further lower the probability for the senior director's promotion to the CEO position in the next tournament, further lowering the expected value of promotion opportunity in their contracts, and, consequently, their contracts' overall value.[5] Third, there is a fundamental difference between implicit promotion-based and explicit performance-based awards, as the former is only possible to realise with the occurrence of a promotion, unlike the explicit incentives such as annual salary increases or bonuses which are recurring and relatively predictable incomes that directors will receive without promotion (DeVaro, 2006). Becoming the next CEO in the firm is the ultimate victory and is the only way to realise fully the CEO promotion prize. The uncertainty about the timing of the next promotion opportunity jointly with the lower probability of winning the next promotion leads to a lower value of promotion-based incentives. Finally, firms will not adjust the explicit incentives to compensate the non-promoted directors for losing the tournament because of high adjustment costs of restructuring the incentive plan at the end of a tournament. This causes a suboptimal equity ownership level in managers' incentive contract (Morck, Shleifer and Vishny, 1988) and leading firms to always have misaligned incentives because their transaction costs overweight the benefits of a properly aligned incentive (Core, Guay and Larcker, 2003).

Empirically, several studies show that firms do not adjust their incentive plans to compensate non-promoted directors because the high adjustment cost curbs firms to compensate the tournament losers *ex-post*, weakening the *ex-ante* tournament incentives (Chan et al., 2022). This lack of adjustment of compensation contracts leads also to a lower overall incentive plan and a gradual decline in tournament losers' performance rating (Gibbs, 1995). Bushman, Dai and Zhang (2016) show high adjustment costs associated with issuing equity constrain firms' abilities to restore the optimal pay-performance sensitivity. Kale *et al.* (2009) find that firms will systematically provide a higher-level tournament incentive proxied by the larger pay gap between the CEO and the executive team's median compensation following a new CEO's appointment. The uncertainty regarding the future CEO promotion lowers the non-promoted directors' subjective probabilities of successfully realising the implicit promotion-based incentives in the next tournament.

[4] My data shows an average of 9 years, close to 7.6 years in UK (Gregory-Smith and Wright, 2019).
[5] Chan *et al.* (2022) estimate a probit model to show the expected probability of winning a future CEO tournament significantly decreased from 27.4% to 9.4% after directors lose their first tournament while there is no significant increase in the number of competitors in the future tournament.

However, previous studies assume a rather passive role of the tournament losers, who either accept the loss and the subsequent decrease in their compensation contract's overall value or leave the firm to participate in tournaments in other firms. My data shows that 68% of them stay with the firm two years after the tournament. I argue that they have incentives to stay to exploit their informational advantage more assertively by conducting insider trading with greater aggressiveness. Since the promotion-based incentive represents an unrealised part of senior directors' remuneration contracts, they can materialise their private information regarding the firm's true future valuation to gradually make up the discrete losses in the valuation of their positions. The existing tournament incentives studies overlooked this strategy, but it is plausible because all CEO tournament competitors are high-ranked directors closely involved in their firms' daily operations, and they are privy to price-sensitive information, which they can trade on. Although the SEC prohibits corporate insiders from trading on any material private information, anecdotal evidence and empirical studies in insider trading literature have shown that corporate insiders trade profitably (Lakonishok and Lee, 2001; Cohen, *et al.*, 2012). Their trades are based on future earnings (Piotroski and Roulstone, 2005), future cash flows (Jiang and Zaman, 2010) or in the month before quarterly earnings announcements (Ali and Hirshleifer, 2017), violating the regulation as the expected monetary gain outweighs any litigation risk. The profitability embedded in insider trades persists from the 80s until today, even though insider trading regulation has tightened after the Sarbanes-Oxley act in 2002 implementation (Beneish and Markarian, 2019).[6]

Roulstone (2003) finds that firms set up internal policies to restrict insider trading activity and offer their directors a premium for their forgone insider trading opportunity, as directors, *de-facto*, consider their trading opportunities as a way of compensating themselves. Bourveau, Brochet, Ferri, and Sun (2021) report that the mandatory adoption of say-on-pay increases executives' incentives to engage in insider trading to offset the regulatory-induced increase in compensation risk. Gao (2019) uses a regression discontinuity to find that directors who marginally missed their relative performance goals and lost their performance-based bonuses trade more profitably than their counterparts who narrowly met the goals and received the bonuses, suggesting that they intentionally trade on their private information more aggressively to compensate themselves for the forgone bonuses. Overall, I expect promotion rejectees to trade more aggressively and profitably on inside information to make up for the decreases in the overall valuation of their positions, particularly as they are "under the shadow" relative to the CEO who is exposed to public visibility through the media, market regulators and investors scrutiny, the key determinants of insider trading profitability (Sabherwal and Uddin, 2019).

[6] Sarbanes-Oxley act came into force in 30 July 2002. It shortens the reporting deadline to SEC from 10 to 2 days after the end of the month in which insiders executed the transactions. At the same time, the SEC adopted Rule 10b5-1 to allow insiders to set up planned pre-announced trades, executed by their brokers, generally at a fixed time interval, without allegations of illegal insider trading. However, Larcker et al (2021) report opportunistic use of 10b5-1, particularly plans with a short cooling-off period, and those adopted just before that quarter's earnings announcement. Franco and Urcan (2021) find that insiders trade profitable by using equity deferrals to circumvent Rule 10b5 trading restrictions through the timing and content of corporate disclosures around these trades.

III. Sample and Variable Construction

I follow prior literature (Kale *et al.*, 2009; Kini and Williams, 2012) to identify CEO turnover event and collect director's compensation data from Execucomp, which covers S&P 1500 firms from 1996 to 2019, with the first CEO turnover event occurring in 1997. My initial sample consists of 269,456 director-year observations with 4,838 CEO turnover events. I use the annual CEO flag (*ceoann*) to identify the historical CEO changes. Throughout the study, my event window is $(-2, 1)$ relative to CEO turnover year 0, as I assume that the tournament begins in year -2, and the losing tournament effect will gradually decay outside my event window. I additionally restrict that there is only one CEO turnover in the window (-2, 2) to remove confounding event. I use CEO promotion and CEO turnover interchangeably to denote the change of CEO position and solely refer to non-CEO directors whenever I mention insiders, directors, or promotion rejectees unless specified otherwise.

I define tournament competitors as those covered by Execucomp but are not CEOs in their firms (Kale *et al.*, 2009; Kini and Williams, 2012). These filters select tournament competitors relatively properly because Execucomp mainly covers the top five highest-paid directors in a firm; their only promotion destination is the CEO position. I reckon the total compensation package that a director receives better measures her seniority within the firm than her job title. I exclude three groups of insiders from the tournament competitor category. The first are insiders not covered by Execucomp in years (-2, -1) but gained coverage in years (0, 1) as they are either new joiner or low-rank directors who did not participate in the CEO tournament but gained the coverage of Execucomp after the tournament. The second are, like Microsoft's Bill Gates, ex-CEOs in the firm and remain with the firm after stepping down from their position, but have both lower probability and fewer incentives to become the next CEO. Third, founder and co-founder of the company identified by using the job title (*titleann*). The median (mean) number of tournament competitor is 4 (3.8) in my final sample.[7]

To construct the tournament incentive measure, I first use the item total compensation (*tdc1*), adjusted to account for the regulatory change of Financial Accounting Standards Board (FASB) 123R revision, as detailed in Appendix S1, following Coles, Daniel and Naveen (2006) and Walker (2009). I then take the logarithm of the difference between the CEO's total compensation and the median total compensation of other non-CEO directors (Kini and Williams, 2012; Coles *et al.*, 2014). I follow Kini and Williams (2012) and remove former CEO who remain in the firm as an executive role when identifying the median non-CEO director pay. I collect my instrumental variable, the former CEO's age in the last fiscal year (*age*), from Execucomp, or BoardEx or searches on Factiva if data is missing. I extract accounting and financial data from Compustat, and stock prices and holding period returns data

[7] My results are robust if event window is extended to (-3, 3), narrowed to (-1, 1), restricted to cases with only one CEO turnover in $(-4, 2)$, or include all confounding events and the three types of non-CEO directors I exclude. I do not restrict other event years than CEO turnover year in the event window of other CEO turnover event because such restriction is effectively requiring that there is only one CEO turnover in ten years.

from CRSP, excluding non-common shares (share code (*shrcd*) 10/11) and stocks priced under $2 at the beginning of a calendar year. Appendix S2 shows the sample sizes across my databases.

I compiled all U.S. insider transactions from January 1996 to August 2019 from Smart Insider Ltd[8]. I keep all insider open market transactions in Form 4. I exclude transactions with less than 100 shares, in line with previous studies (Lakonishok and Lee, 2001; Cohen *et al.*, 2012), and any 10b5-1 pre-scheduled trades, as their information content is likely to be trivial, even though Larcker et al (2021) and Franco and Urcan (2021) find that insiders exploit it. I aggregate these insider trades at the insider-day level. To measure the direction of insider trades, I compute the net purchasing value (NPV) as the dollar value of the purchase transaction minus that of the sell transaction over the total dollar value[9]. If *NPV* is greater (less) than zero, the insider is net buying (selling) on a given day. I exclude the 0.3% of my matched insider trading sample with *NPV* equal to 0 from my final sample.

I match Execucomp's unique director identifier *execid* to Smart Insider's non-unique insider identifier *personid*. I use BoardEx to crosscheck the validity of my *execid-personid* match. For 48,429 distinct *execid* in Execucomp, I match 43,952 (90.8%) of them with 44,187 *personid.* I match 42,358 of 46,720 (90.7%) distinct *execid* for non-CEO directors. I discard the unmatched *execid* from my sample, as they have not reported any transactions on Form 4. After removing 29% cases with confounding events, I construct a sample of 3,428 CEO turnover events with 2,636 (77%) internal promotions, close to the 72% reported by Cziraki and Jenter (2020).[10] I find 1,259 (37%) firms did not report any insider transactions in year 0, leaving 2,169 events in my final sample. I find 152,273 matched sell trades but only 13,022 purchases, representing 8% of the total trades, significantly lower than the 37% reported by, say, Lakonishok and Lee (2001) or the 20% in my full database, indicating a higher propensity to sell by non-promoted insiders. The details are in Appendix S3.

I use the CRSP value-weighted market index return to adjust the holding period return and compute the buy-and-hold (BHAR) abnormal return for holding period *t* as follow:

$$BHAR_{j,it} = \prod_{i=1}^{t}(1 + return_{j,t+i}) - \prod_{i=1}^{t}(1 + mkt_{t+i}) \quad (1)$$

where $return_{j,t+i}$ is the stock's *j* holding period return, and mkt_{t+i} is the value weighted CRSP index. I measure BHAR one day after insider transaction date and a holding period of 365-calendar day as "short-swing profit" rule in Section 16(b) of the 1934 Security Act prohibits insiders from profiting from short-term price movement (Anginer, Hoberg and Seyhun, 2018). I require at least 243 trading days in the holding period as in Agrawal and Nasser (2012). Appendix S4 shows details of my variables.

[8] This database (https://www.smartinsider.com/) is the same as Thomson Reuters. It gathers data from Form 5, the annual statement of change in beneficial ownership and reports any exempt trades not reported on Form 4.
[9] Some studies use net purchasing ratio, NPR, the ratio of the number of shares bought over the total traded as an alternative measure of insider trading direction (Lakonishok and Lee, 2001). I find same results using NPR.
[10] My results are robust to the inclusion of the confounding events.

Endogeneity Concern and Identification Strategy

One major concern in insider trading literature is endogeneity because the true motivation behind their trading decisions is not observable. Insiders sell for reasons other than profit-seeking, such as personal liquidity needs and portfolio diversification to mitigate the excess idiosyncratic risk they undertake by over-concentrating their portfolios on their firms (Huddart and Ke, 2007). Conversely, they may acquire stocks because they believe the firm is undervalued, for controlling purposes, or even to signal fake firm undervaluation when there is an increase in short interests (Wu, 2019). The omitted variable bias will lead to an inconsistent OLS estimate for the losing tournament effect. I use an extensive set of explanatory variables to control for insider trading return and include firm and month fixed effects to proxy for time-invariant unobservable variables to eliminate potential endogeneity[11].

Nevertheless, I recognize that these approaches do not completely solve the endogeneity issue. I follow Cengiz *et al.* (2019) and Baker, Larcker and Wang (2021) and specify a stacked diff-in-diff regression based on a matched sample as my baseline regression to eliminate the concern that unobservable market anticipation will bias my results. I match my test firms with control firms with no CEO turnover in (-2, 2) and shortest Mahalanobis distance on the average insider purchase/sell profitability, logarithm of the total asset, and the book-to-market ratio in the year *t-1*, in line with Berger, Kick and Schaeck (2014). I match one treated firm with one control firm to minimize the biasedness. I successfully match 192 out of 547 (35%) firm-year observations with at least one insider purchase transaction in the CEO turnover event year, and 1,331 of 1,775 (75%) for firms with at least one insider sell transaction[12]. My sample size varies depending on the availability of the *execid-personid* link table and the different control variables included. The comparative analysis of the subsequent insider trading profitability across these two samples can better disentangle the incremental change solely attributable to the loss of CEO turnover within my event window. I estimate the following diff-in-diff regression to study whether the return predictability of insider purchase (sell) trades remains systematically the same or increases (decreases) in and/or after the CEO events by focusing on my event window only:

$$\text{BHAR\_m\_365}_{i,t} = \alpha + \beta_1 \text{Post}_{i,t} + \beta_2 \text{Treat}_{i,t} + \beta_3 \text{Post}\times\text{Treat}_{i,t} + \beta_4 CEO\_IT_{I,t} + \text{controls} + \gamma + \rho + u_i \quad (2)$$

where γ and ρ are firm and month fixed effect, respectively. I cluster my standard errors at the firm-month level as Alldredge and Blank (2019) show that insiders cluster their trades with their colleagues. Subscripts *t, d* and *m* are for fiscal year, trading day and month, respectively. I match the time dimension of the control variables on the insider transaction date instead of the CEO turnover event.[13]

[11] My results are robust when I replicate all diff-in-diff regressions with firm and year fixed effects.

[12] Many firms do not report insider purchase transactions in years (-2, -1). I tried various schemes to match on their past insider trading profitability, matching on year -1 yields the most suitable results.

[13] My results remain robust if I match the time dimensions of these control variables in my first stage regression to the CEO turnover event by using the end of last month figure in the last fiscal year. However, to better control for the firm characteristics that will affect insider trading profitability, I prefer to match the dimension with insider transactions. My results also remain unchanged if I include both the one-fiscal year lagged control variables and one-month lagged control variables in my first and second stage regression.

The treatment dummy, $treat_i$, is equal to one for my treated firms, and the post-treatment period dummy, $post_t$, is equal to one for two years from 0 to +1 post-CEO tournament outcome, depending on the specific focus period. I expect β_3 to be positive if the buy trades are profitable and negative if the sell trades are loss-avoiding, after losing the CEO tournament. I also include $CEO\_IT_{I,t}$ to proxy for the CEO trading direction and to capture the trading strategy that non-CEO insiders time their transactions based on the current CEO's trading activity. Armstrong *et al.* (2020) show that newly appointed CEO is systematically more likely to make noisy purchase transactions to signal their commitments to improve the firm's performance, not necessarily to seek a profit, but to prolong their tenure even if they underperform, yet the market reacts positively, overvaluing the firm. These buy trades systematically generate low long-term abnormal returns, leading non-promoted insiders to adopt contrarian strategies by selling overvalued shares and increasing their trading profitability.[14] To account for this strategy, I first compute the net insider trading value of a CEO in the year *t* as the difference between the aggregated value of insider sell and buy trades, which I then divide into annual quintiles to get $CEO\_IT_{I,t}$ as the quintile number. If the CEO is not trading in year t, the selling and buying values are zero, but the lower the $CEO\_IT_{I,t}$, the more shares the CEO has purchased in the year *t*.

I include various control variables in my regression to account for firm and insider personal characteristics (Lakonishok and Lee, 2001; Cohen *et al.*, 2012). I compute a dummy equals to one for firms that promoted an outsider CEO, and a dummy equals to one if the CEO succession was planned in $(-2, -1)$ to assess whether such appointment reduce insiders' intensity of exploiting their private information advantage. I measure the tournament incentive at the firm level by computing the natural logarithm of the difference between the adjusted CEO total compensation and the median adjusted total compensation of other insiders. At director level, I use a dummy equals to one for high incentive directors whose total difference in the adjusted total compensation between CEO and directors is in the top three in their companies, given that the median and mean ranks are three, and zero otherwise[15]. I control for the 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, and the financial analyst coverage that controls the firm's information environment. To control for some personal characteristics that can affect insiders' trading returns, I include personal wealth risk (Beneish and Markarian, 2019) by following Core and Guay (2002) to calculate the performance-based incentives as a dollar change in director *i*'s wealth associated with a 1% change in the firm's stock price (in $000). In line with Coles, Daniel and Naveen (2006), I calculate the risk-taking incentives as a dollar change

[14] Armstrong *et al.* (2020) show that the market reaction to the purchase transactions executed by CEO who successfully (failed to) prolonged her tenure in the next year is positive (negative). Since I removed all the confounding events in my sample, all the CEOs in my post-tournament period prolonged their tenures.

[15] This measure proxies for the potential increase in remuneration packages if promoted to be the next CEO. In some rare cases, some non-CEO directors have higher compensation than CEO, such as Bill Gates (*execid*: 00635) continued to be compensated significantly more than Steven Ballmer, who took over Gates' CEO position. I restrict the difference in total compensation to be zero and my result is robust with or without those outliers.

in director *i*'s wealth associated with a 0.01 change in the standard deviation of the firm's stock returns (in $000). Finally, I control for firm's financial health using the yearly industry average S&P long-term rating, which summarises industry risk and can predict forced CEO turnover. Following Peters and Wagner (2014), I assign AAA a value 2 to CC a value of 23, then scale them by 9, so that a unit increase in the scaled rating corresponds to a change in rating from AAA to BBB or BBB to CCC.

Table 1 shows the summary statistics of insiders and firm characteristics. Panel A reports that the profitability embedded in non-promoted insiders' buy trades before the CEO tournament (-2, -1) is 5.9%, rising significantly to 30.4% in the post- (0, +1) event window, suggesting that they trade on their private information to compensate themselves for the forgone promotion opportunity as their average *total_compensation* declines significantly from $1.5million to $1.07 million. The momentum, *mom*, a proxy for long term stock returns is significantly higher after the tournament, suggesting that they often buy to support the price when their stocks perform poorly. Panel B shows that their sell trades are not loss avoiding, even though the abnormal returns are significantly lower than the pre-event period. They are more likely to adopt contrarian strategies by buying (selling) when the long-term and short-term momentum stock return, as proxied by *mom*, and *ret30*, are lower (higher) and book to market higher (lower) in line with previous evidence (Lakonishok and Lee, 2001; Cohen et al., 2012). They tend also to buy (sell) in smaller firms and those with lower (high) *pay_gap_firm* and *total_compensation*, *ROA*, and sell-side analyst coverage, and in less (more) liquid firms. The BHARs, not reported, are more pronounced for non-promoted insiders and depend on whether the promoted CEO is an external, the CEO succession is planned, and the incentives are high. I account for these factors in my regressions.

[Insert Table 1 here]

One drawback of my diff-in-diff estimator in this research setting is that I only compare the post-tournament insider trading profitability in year $(0, 1)$ with pre-tournament insider trading profitability in year $(-2, -1)$. To generalise the results outside this event period, and to control for potential endogeneity, and compare the post-tournament insider trading profit with their unconditional ones outside the event window, I employ a 2SLS estimator. The IV should embed predictive power for the CEO turnover event one year after the event to satisfy the relevance condition, should not correlate with insiders' trades abnormal returns, which proxy for their private information regarding the firm's future fundamentals to meet the exclusion restriction (Ali and Hirshleifer, 2017; Cziraki, Lyandres and Michaely, 2021), and should account for insiders' ability to trade profitably from their economically-link industry peers' public incremental information (Alldredge and Cicero, 2015).

I select the former CEO age in the last fiscal year as a suitable IV in my setting. Peters and Wagner (2014), Cziraki and Jenter (2020) and Jenter and Lewellen (2021) show that the CEO's age embeds significant predictive power for CEO turnover in addition to the CEO tenure and firms' performance, as measured as average industry-adjusted monthly stock returns scaled by the standard

deviation of returns, and other firm-level characteristics. Inspired by these results, I hypothesise that the age of the former CEO also embeds predictive power for the CEO turnover because the younger (older) the former CEO, the more likely the incumbent CEO had been replaced the firm less (more) recently, decreasing (increasing) the likelihood of a future CEO turnover[16]. The former CEO's age in the last fiscal year also embeds predictability not only for the year of CEO turnover, but also for one year after the CEO turnover. I expect these recently left CEOs are systematically younger than other former CEOs. I formally test the relevance condition in Table 7.

Although the exclusion condition is not formally testable, it is less of a concern, as the average time distance between year *t* and the year that the former CEO left the firm of six years is relatively long to affect the firm's future value and corporate policies decision making[17]. Moreover, since former CEO's age is a public information, and insiders trade on the firm's future value that has not been fully incorporated into the current stock price (Lakonishok and Lee, 2001), my IV can satisfy the exclusion restriction. I employ the 2SLS estimator to study insider's trading propensity after losing the CEO turnover. I conduct additional tests to rule out the possible channels that my IV can influence the insiders' private information in the robustness test to further show the exclusion restriction's plausibility. I run two first-stage regressions to overcome endogeneity in my interaction variable:

$$\mathrm{NPED}_{i,t}=\alpha+\beta_1 \mathrm{age\_ceo}_{j,\,t-1}+\beta_2(\mathrm{age\_ceo}_{j,\,t-1}\times \mathrm{CEO\_IT}_{i,t})+\beta_3 \mathrm{CEO\_IT}_{i,t}+\mathrm{control}+u_i \quad (3)$$

$$(\mathrm{NPED}_{i,t}\times \mathrm{CEO\_IT}_{i,t}) = \alpha+\beta_1 \mathrm{age\_ceo}_{j,\,t-1}+\beta_2(\mathrm{age\_ceo}_{j,\,t-1}\times \mathrm{CEO\_IT}_{i,t})+\beta_3 \mathrm{CEO\_IT}_{i,t}+\mathrm{control}+z_i \quad (4)$$

where $NPED_{I,t}$ is a dummy equal to one for insider buy/sell trades executed in the post turnover year *t*, and zero for other years. $\mathrm{age\_ceo}_{j,\,t-1}$, the interaction term between my IV $\mathrm{age\_ceo}_{j,\,t-1}$, and the moderator variable $CEO\_IT_{I,t}$ are my first and second joint IV to predict the $NPED_{i,t}$ and $\left(NPED_{i,t} \times CEO\_IT_{I,t}\right)$.

In the second-stage regression, I replace the $NPED_{\mathrm{I,t}}$ and $(\mathrm{NPED_{I,t}}\times \mathrm{CEO\_IT_{I,t}})$ by the estimated $\widehat{NPED}_{\mathrm{I,t}}$, a continuous variable representing the predicted probability that a given insider purchase or sell transaction executed in the post-tournament year *t*, and $(\mathrm{NPED}\widehat{\times \mathrm{CEO}}\mathrm{\_IT})_{\mathrm{I,t}}$ as follows:

$$\mathrm{BHAR\_m\_365}_{i,(d+1,d+365)} = \beta_1 \widehat{\mathrm{NPED}}_{i,t}+\beta_2(\mathrm{NPED}\widehat{\times \mathrm{CEO}}\mathrm{\_IT})_{i,t}+\beta_3 \mathrm{CEO\_IT}_{i,t}+\mathrm{control}+\varepsilon_i \quad (5)$$

If directors exploit their informational advantage to compensate themselves for losing the CEO tournament, β_1 should be positive (negative) for buy (sell) trades. If they increase their selling activities when the CEO is increasing their holdings to prolong her tenure, I expect β_2 to be positive and significant for insider sell transactions. I include the same set of control variables and fixed effects.

[16] The use of last fiscal year's former CEO age discards all observations in my entire sample before the first CEO turnover, reducing my sample size. With the last fiscal year CEO's age, the sample size is larger. The correlation between the two measures is 0.39. With the current CEO tenure in my 2SLS in robustness test, all coefficients remain robust but weaker. I recognise that the former measure is more exogenous than the last fiscal year CEO.

[17] These decisions include governance changes (Nelson, 2005), firm's cash holding (Feng and Rao, 2018), total risk and idiosyncratic risk (Cen and Doukas, 2017), and performance (Palia, 2001; Bhagat and Bolton, 2013).

IV. Empirical results

A. Insider Trading Propensity around CEO tournament

Table 2 Panel A reports the results of matching my test firms with control firms with no CEO turnover in (-2, 1) with the shortest Mahalanobis distance on the average insider purchase or sell profitability, logarithm of the total asset and the book-to-market ratio in the year *t-1*, in line with Berger, Kick and Schaeck (2014). I first account for the pre-event period performance using changes in stock returns during the pre-event period, $\Delta\text{BHAR\_m\_365}_{(-2,-1)}$, as a proxy, as firms that replace their CEOs are more likely to be underperforming. I find no statistical significance in size, book to market, momentum and profitability, which I do not use in my matching, indicating my matching procedure is appropriate. However, the average purchase transaction for the treated firm is statistically larger than that of control firms, and the non-CEO directors from treated firms receive 7% higher total compensation than their counterparts from control firms for sell sample, but I do not expect these significant differences to affect my results as, economically, they are relatively small.

Panel B reports that the difference in BHAR_m_365 between test and control firms for both insider purchase and sell samples are statistically indifferent from zero in the years (-2, -1), indicating my matching strategy is successful, and rejecting the hypothesis that there is a parallel trend returns between control and treated firms. Furthermore, the test firms generate higher (lower) BHAR_m_365 in year 0 (1) than control firms in purchase sample and yield lower returns in year 0 and 1 in sell sample, further supporting my hypothesis. I conduct a formal parallel trend assumption test following Angrist and Pischke (2009) and Cengiz *et al.* (2019). The coefficient of Pre_{-1} is statistically insignificant in both purchase and sell samples. This means that the trend in (-2, -1) between control and treated firm is parallel after controlling for firm characteristics that can explain insider trading profitability suggesting that the post-tournament results are not driven by the matching algorithm's inappropriateness to obtain the control group and the use of the diff-in-diff estimator. Appendix S5 reports the full results.

I classify insider transactions into opportunistic and routine traders, in line with Cohen *et al.* (2012) to assess whether insiders are more likely to execute opportunistic sell transactions. The former trades are executed by insiders who regularly trade in a clear pattern, which I define as trades in the same calendar month in the past three years, and the latter are discretionary trades that embed higher return predictability and more private information on average. I re-classify each insider at the beginning of each calendar year based on her past three years' trading history. I exclude insiders who did not make any trades in the past three consecutive years. I follow Equation (2) and estimate the regression using the matched sample. Table 2 Panel C reports the results. The dependent variable is $opp\_D_{I,t}$ that equals one for opportunistic transactions and zero for routine transactions. Columns (1) to (2) show that the coefficient of the interaction term $(Treat \times Post)_{i,t}$ is insignificant, suggesting that there is no significant change in the propensity of executing opportunistic buy transactions in years 0 and +1.

In contrast, columns (3) to (4) indicate a clear pattern in the insider sell trades. The coefficients of $(Treat \times Post)_{I,t}$ and $CEO\_IT_{I,t}$ are positive and significant. This suggests an increase in the propensity of insiders to make opportunistic sell transactions in year $(0, 1)$, and that the newly appointed CEO's trading direction significantly determine the director's propensity to make opportunistic sell trades and that insiders are uniformly more likely to sell opportunistically if the newly appointed CEO is decreasing her holdings. I also find but not report that the coefficient of the momentum control variable is positive and statistically significant, suggesting that insiders adopt contrarian strategies by selling when the stock return are high. Similarly, the negative and significant coefficients of $bm_{j,m-1}$ and $size_{j,m-1}$ imply that opportunistic insider selling is more pervasive in small and growth stocks. The sign and significance of the remaining control variables are consistent with the existing literature (e.g., Lakonishok and Lee, 2001).

Overall, these results suggest that insiders are more likely to make opportunistic sell transactions in year (0,1) after losing the CEO competition, which are more informative than an average sell trades suggested by Cohen *et al.* (2012). In an unreported logit regression, I find that insiders are more likely to execute opportunistic sell, than buy, trades after they have lost the promotion, consistent with my hypothesis. These findings are consistent with my hypothesis that insiders mainly incorporate more private information into their sell transactions to compensate themselves for losing the CEO competition. Furthermore, these results provide preliminary evidence that non-promoted insiders strategically time their transactions based on the trading activity of the newly appointed CEO.

[Insert Table 2 here]

B. Diff-in-Diff regression results

Table 3 reports the diff-in-diff estimation result. In column (2), the coefficient of the interaction term $(treat \times post)_{(0,0)}$ is statistically significant, implying that the buy trades executed by insiders after losing a CEO turnover tournament yields a 24.5% higher BHAR_m_365 that those generated without CEO turnover, *ceteris paribus*. However, it not significant in the remaining buy trades columns. Column (5) to (6) indicate that, the sell trades in treated firm systematically generate more negative BHAR_m_365 of between 3.0% in years (0,0) and 4.8% in year (1,1), than those of the control firms, as the coefficients of the interaction term $(treat \times post)_{i,t}$ are negative and statistically significant. Using the average sell transaction value in year 0 and year 1, non-promoted insiders' sell transactions would yield \$28,209 (\$45,567) more profit if their transactions are made in the year 0 (year 1) than other non-CEO directors. The dollar profit is higher than the average profit of \$12,000 reported by Cziraki and Gider (2021) between 1986 and 2013. Additionally, the abnormal dollar profit accounts for 2.1% (3.3%) of the average non-CEO director total compensation in year 0 (year 1), higher than the average 1.2% reported by Cziraki and Gider (2021) for all non-CEO directors covered by Execucomp.

The losing tournament effect is weaker for insiders who stay with a firm that had a CEO successor prior to the tournament because the coefficients of $COOD_{I,j}$ are in the opposite signs to the coefficients of $(treat \times post)_{i,t}$ for both insider purchase and sell samples. This evidence shows that a pre-assigned successor will serve to depress the discontent among directors effectively. Thus, they will react to the loss of CEO tournament with less intensity because their sell transactions do not generate as negative returns as their counterparts from a firm that did not have a CEO successor. Moreover, insiders mainly make sell transactions to compensate themselves because the losing tournament effect persists until year +1 in the insider sell sample. In contrast, the effect solely exists in the year of CEO turnover in the insider purchase sample. The short-term and long-term momentum variables, $ret30_{j,t,(d-1,d-30)}$ and $mom_{j,t,(d-31,d-364)}$ are both negative and mostly statistically significant for insider sell sample, but $mom_{j,t,(d-31,d-364)}$ is negative and significant only in column (1) for buy trade sample, suggesting that worst performing firms generate higher subsequent returns. The coefficient of $size_{j,m-1}$ is constantly negative and significant, consistent with the well-documented size effect. Overall, the significance and signs of my control variables are consistent with other insider trading studies Cohen *et al.* (2012), Beneish and Markarian (2019) and Contreras and Marcet (2021).

[Insert Table 3 here]

C. Motivations behind more informed insider transactions

In this section, I assess whether insiders intentionally trade to compensate themselves for the forgone CEO promotion, referred as *forgone incentives hypothesis*, or to exploit the stock misevaluation after a major corporate change, referred as *stock misevaluation hypothesis*. In the former I expect a stronger increase (decrease) in the BHAR_m_365 of transactions submitted by insiders whose tournament prizes are larger. Although I control for the pay disparity in the last fiscal year using $high\_incentiveD_{I,t-1}$ as a proxy in my previous results, the historical pay disparity in year -1 is a more relevant measure for their tournament prizes if they had won the tournament. A larger tournament prize indicates a larger opportunity loss, and they should trade on their private information more aggressively.

I further re-specify my diff-in-diff regression with a triple interaction term $(Post \times Treat \times Pay\_rank)_{I,t}$, which I expect to be negative (positive) for insider purchase (sell) trades, if directors with high tournament prizes compensate themselves for the forgone promotion-based opportunity with greater intensity than other insiders. I also include $Pay\_rank_{I,t}$, $(Post \times Pay\_rank)_{I,t}$ and $\left(Treat_{I,t} \times Pay\_rank\right)_{I,t}$.I report the results in Table 4 Panel A. I include the same set of control variables but omit their coefficients for brevity. The coefficient of $(Post \times Treat \times Pay\_rank)_{I,t}$ is statistically insignificant in columns (1) and (2), indicating that incentive does not affect the profitability of their buy trades. However, it is positive and statistically significant in columns (3) and (4), suggesting

that non-promoted insiders with higher tournament incentives compensate themselves for the forgone promotion opportunity by exploiting negative private information with greater aggressiveness.

Another method to reaffirm the *forgone incentives hypothesis* is to check the age effect. Gibbons and Murphy (1992) show that directors close to their retirement age will place less importance on the promotion-based incentives. Consequently, I hypothesise that older directors will react to the loss of tournament with less intensity, i.e., the changes in the abnormal return of older directors will be less dramatic than younger directors. To test the hypothesis, I employ the natural logarithm of the current age of directors as the moderator variable. Table 4 Panel B shows that the coefficient of $(Post \times Treat \times lnage)_{I,t}$ is insignificant in column (1) and (2), but positive and significant in column (3) and (4), in line with my previous findings that older directors will trade on their private information to compensate themselves for the forgone promotion-based incentives with lower aggressiveness. They did not place much implicit value on their future promotion opportunities because their career horizons are shorter, in line with Gibbons and Murphy (1992).

Thirdly, I employ insider personal investment horizons to proxy for their career horizons to further confirm the *forgone incentives hypothesis*. Akbas, *et al.* (2020) show that short horizon (SH) insider sellers frequently reverse their previous buy positions to avoid overconcentration of their personal portfolios in their firms. Consequently, these insiders have shorter career horizon in their firms. I hypothesize that SH sellers will trade on their private information with lower aggressiveness if they are motivated by the forgone CEO promotion because a shorter career horizon indicates a lower expected value for the forgone CEO incentives. I modify the investment horizon measure proposed by Akbas, *et al.* (2020) to identify SH sellers, as detailed in Appendix S1 . I find 2.3% (9.2%) of my buy (sell) trades were by short-horizon insider sellers. The relatively small sample of insider purchases indicates that SH sellers are less likely to make purchase transactions after they have lost the tournament.

I create short-horizon dummy variable $SHD_{I,t}$ equals to one for SH insiders, and zero otherwise. I employ $SHD_{I,t}$ as the moderator and report the results in Table 4 Panel C. The coefficient of $(Post \times Treat \times SHD)_{I,t}$ is significantly positive in columns (3) and (4), suggesting that insiders who frequently unload their ownerships in their firms will trade on their private information with lower aggressiveness. The sign and overall significance of the $(Treat \times Post)_{I,t}$ is consistent in all three panels, suggesting that insiders incorporate more positive (negative) private information into their purchase (sell) transactions after controlling for their forgone incentives, pay rank and investment horizons[18].

[Insert Table 4 here]

[18] In unreported results, I also create dummy variable for sample after 2011, the year in which the unbinding Say-on-Pay law was passed. I did not find the implementation of Say-on-Pay law plays a significant result.

Lastly, I compute the probability of insiders becoming CEO denoted as $Probability_{i,t-1}$ by estimating a cross-section regression using only firms that had a CEO turnover in the year, and employ the probability in *t-1* as the moderator and report the results in Table 4 Panel D. I explain the estimation of the variable in Appendix S1. Consistent with the *forgone incentives hypothesis*, the insiders who has higher probability of becoming CEO but failed will exploit their private negative information more aggressively, there is no significant effect for insider purchase transactions. The result further reaffirms that the forgone CEO promotion opportunity motivates insiders to trade.

I investigate whether *stock misevaluation hypothesis* plays a role in the insider trading decision, I employ two proxies to measure the stock informativeness: the Future Earnings Response Coefficient (FERC) proposed by Tucker and Zarowin (2006) and the return synchronicity suggested by Piotroski and Roulstone (2004). I explain the constructions of these two proxies in details in Appendix S4. For FERC, I create binary variable $\text{FERC}_{i,t}$ equal to one for the top quintile of stocks whose current prices contain the most future earnings information and zero otherwise. As for return synchronicity, I create a binary variable $\text{Synch}_{\text{I,t}}$ that equals to one for the top quintile of stocks whose current prices contain less firm-specific information and co-move strongly with the current and lagged market and industry returns, and zero otherwise. I then employ $\text{FERC}_{i,t}$ and $\text{Synch}_{\text{I,t}}$ as the second moderator variables separately. I hypothesize that when the firm's share price is less (more) informative for the firm-specific information, insider trading returns will be high (lower). The significance and the sign of the coefficient of $(\text{Treat}\times\text{Post})_{\text{I,t}}$ should be robust to the inclusions of these two firm information environment measures because insiders' motivation to trade is not only to correct the mispricing but to compensate themselves for the forgone CEO promotion opportunity.

I find, but not report, that for the buy trades, the coefficient of $(\text{Post}\times\text{Treat}\times\text{FERC})_{\text{I,t}}$ is insignificant suggesting that insider purchase profitability after the CEO turnover is not affected by the level of stock price informativeness for future earnings. However, for the sell trades, it is positive and statistically significant. This implies that insiders' sell transaction generate returns that are more negative when the current stock price reflects lower future earnings information in year 0. I also employ $Synch_{i,t}$ as the moderator variable. Although the sign and significance of $(\text{Treat}\times\text{Post})_{\text{I,t}}$ remain consistent, the coefficient of $(\text{Post}\times\text{Treat}\times\text{Synch})_{\text{I,t}}$ is statistically insignificant in all columns, suggesting that insiders' trading profitability does not depend on the level of co-movement between current firm return and the current and lagged market and industry returns, i.e., when stock price contains firm-specific information. The results are in Appendix S6 .

In conclusions, the significant roles of age, historical pay rank and personal investment horizon further lend stronger support to the *forgone incentives hypothesis*. The motivation behind insider sell transactions in the year $(0,1)$ is not necessarily to trade on stock misevaluation but mainly to seek profit to compensate themselves for the loss of the CEO compensation.

D. Informational content embeds in insider transactions

I examine the information content of insider trading after losing the CEO competitions to confirm that the unobservable firm characteristics do not drive these more informed insider transactions. I focus on three non-mutually exclusive possibilities; insiders may trade on future operating performance changes, exploit the change in investor sentiments and base on the future change in the cost of capital. I compute the 2-year change in ROA from $(t, t+2)$ with year t being the insider transaction year to estimate the former, denoted as ΔROA[19]. I explain the constructions of the change in investor sentiments and change in the cost of capital in details in Appendix S4. To measure the change in investor sentiment denoted as $\Delta Sentiment$, I compute the market-to-book ratio decomposition of Rhodes–Kropf, Robinson and Viswanathan (2005). Cziraki *et al.* (2021) argue the method can separate the firm-specific sentiment from industry-level sentiment and is appealing to insider trading studies because insiders are more likely to possess private information on the former than on the latter (Wang, 2019). I follow Cziraki *et al.* (2021) to measure the change in sentiment $\Delta Sentiment_{t-1,t+1}$ between $(t-1, t+1)$ with year t as insider trading year. To measure the change of cost of capital $\Delta r_{t,t+2}$, I follow Cziraki, *et al.* (2021) and estimate following modified Fama and French (1993) three-factor model. I re-estimate the difference-in-difference regression by separately substituting $\Delta ROA_{t,t+2}$, $\Delta Sentiment_{t-1,t+1}$ and $\Delta r_{t,t+2}$ for the dependent variable BHAR_m_365. I control the same set of control variables and report the regression results in Table 5.

Panel A, where the dependent variable is $\Delta ROA_{t,t+2}$, shows that insider sell transaction can significantly predict a decrease in ROA in the next three years. Insider sell transactions predict a 2%, and 1.1% decrease in $\Delta ROA_{t,t+2}$ in year 0 and 1, respectively, unlike insider purchase transactions as column (1) and (2) show that $(\text{Post}\times\text{Treat})_{\text{I,t}}$ is not significant. Similarly, in Panel B, where the dependent variable is $\Delta Sentiment_{t-1,t+1}$, insider purchase transactions do not significantly predict future changes in investor sentiment in year 0, while insider sell transactions in years 0 and 1 predict a 5.4% and 6.2% additional decrease in the firm's market value that fundamentals do not explained. In Panel C, $\Delta r_{t,t+2}$ is the dependent variable. I can observe that insider purchase sample does not predict the future decrease in the cost of capital in year 0 whereas insider sells predict 0.1% increases in the cost of capital in both year 0 and 1. The coefficient of $(\text{Post}\times\text{Treat})_{\text{I,t}}$ is statistically significant at the 95%, and 90% in column 3 and 4, respectively. Overall, these results highlight that the higher return predictability embedded in the insider sell transactions is not random. Insiders exploit the worsening in future firm performance, the lower investor sentiment, and an increase in the future cost of capital to yield more negative return in sell transactions. However, there is no clear result for insider purchases.

[Insert Table 5 here]

[19] My results remain robust if I use the change in ROA from $(t, t+1)$ with insiders' trade in year t.

E. Insider trading and the effect of the tournament incentives

My previous results imply that directors consider their trading opportunities as a means to compensate themselves for the forgone promotion-based incentives (Roulstone, 2003). Nevertheless, if the tournament incentives truly play an important role in the insiders' information set, they can also trade on their private information prior to the tournament if the expected gain outweighs the associated litigation risk. One additional implication implied from my result is that the insider trading opportunity weakens the tournament's incentive effect because the tournament prize is not as high as it appears. After all, directors always have outside options to trade on their private information. In this section, I revisit the empirical finding in Kale *et al.* (2009) by considering insider trading activity as an additional factor to consider and investigate whether the presence of insider trading opportunity weakens the positive effect of tournament incentives on firm performance.

To measure the total non-CEO insider trading activity, I construct the variable $all\_IT_{j,t}$ which is the total number of insider transactions executed by non-CEO directors for firm *j* in year *t*. The higher $all\_IT_{j,t}$, the more prevailing the insider trading activity in firm *j*. Furthermore, I use the following refined fixed effect regression version of Kale *et al.* (2009) to proxy the firm performance using Tobin's Q and ROA.

$$\begin{aligned}\text{firm\_performance}_{j,t} = \alpha &+ \beta_1 \text{pay\_gap}_{j,t} + \beta_2 \text{rd}_{j,t} + \beta_3 \text{sale}_{j,t} + \beta_4 \text{sale}_{j,t}^2 + \beta_5 \text{capital-to-sale}_{j,t} + \\ &\beta_6 \text{advertising-to-sale}_{j,t} + \beta_7 \text{dividend-yield}_{j,t} + \beta_8 \text{leverage}_{i,t} + \\ &\beta_9 \text{lnage}_{j,t} + \rho + \delta + \varepsilon_i \end{aligned} \quad (6)$$

where $pay\_gap_{j,t}$ is the proxy for tournament incentives as previously specified. ρ represents firm fixed effect, and δ is year fixed effect. I cluster the standard error at the firm level. Appendix S4 defines the remaining variables. $Pay\_gap_{j,t}$ represents the tournament incentives, and β_1 should be statistically significant and positive according to Kale *et al.* (2009) because the higher tournament incentives, the better the firm performs. Remarkably, Kale *et al.* (2009) did not correct the CEO compensation figure for the FASB 123R revision. Therefore, my proxy for the tournament incentives is not constructed exactly as Kale *et al.* (2009).

To investigate the effect of insider trading activity on the tournament incentives, I follow Kale *et al.* (2009) to estimate a 2SLS regression with two first-stage regressions. Kale *et al.* (2009) applied the median value of tournament incentives for firms in the same sales quintiles and the same two-digit SIC industry as the firm as their instrumental variable because it is a significant determinant of the size of each firm's tournament incentives. In addition, the level and structure of managerial compensation vary by industry and firm's size, which I proxy by sales. Since the tournament incentives depend on the compensation structure within an organization, the median value of tournament incentives in the same size and industry is a natural choice for the IV.

My second stage regression is as follows:

$$firm\_performance_{j,t} = \alpha + \beta_1 \widehat{pay\_gap}_{j,t} + \beta_2 \widehat{pay\_gap \times all\_IT}_{j,t} + \beta_3 all\_IT_{j,t} + control + \varepsilon_i \quad (7)$$

If the presence of high insider trading activity weakens the positive relationship between the tournament incentives and the firm performance, β_2 will be negative and statistically significant. The above regression specification implicitly assumes $all\_IT_{j,t}$ is exogenous. One obvious source of endogeneity is reverse causality as expect insiders may purchase (sell) more in outperforming (underperforming) firms, as they understand their firms' future valuation. Thus, simply using one IV for the tournament incentives is not sufficient to conclude the causal relations.

I relax the assumption that $all\_IT_{j,t}$ is exogenous, by using an additional IV to proxy for $all\_IT_{j,t}$. I follow Kim and Lu (2011) and use the sum of maximum state and federal marginal personal income tax rates (hereafter called tax rate) as my 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 directors in a high tax state may prefer tax-exempt securities to stock more than directors located in a low tax state, ceteris paribus, thus causing lower stock ownership. In the same vein, the tax change may also lead to a change in share ownership as directors may sell (hold) more shares when they anticipate a tax increase (decrease). Moreover, the variation in state tax laws across states and years is exogenous to a firm's future performance. Kim and Lu (2011) also employ Tobin's Q to proxy for firm performance in the second stage of their 2SLS regression. I collect the sum of maximum state and federal marginal long-term capital gain tax rates from Feenberg and Coutts (1993)[20]. Taxpayers, including corporate insiders, are subject capital gain tax on any capital return from trading stocks. The tax rate, available from 1997 until 2019, assumes a married representative taxpayer with joint filing and in top tax bracket in her state. Kim and Lu (2011) show that a higher tax rate will cause the CEO to reduce their stock ownership holdings to lessen her expected capital gain. In the same vein, I hypothesize that a higher tax rate will lead to a lower insider trading activity as any capital gains directors obtain from their trades will be taxed more heavily, reducing their propensity to trade.

Table 6 reports the results. For brevity, I omit the first-stage regression result and report only the first-stage F statistics. In column (1) and (2), I replicate the finding in Kale *et al.* (2009). The coefficient of $pay\_gap_{j,t}$ is positive and statistically significant at the 99% confidence level in both columns, indicating that tournament incentives' positive effect on the firm performance persists in my sample period. In column (3) and (4), I employ the median industry tournament incentive as the IV and interact the insider trading intensity with the predicted tournament incentive. The coefficient of $\widehat{pay\_gap}_{j,t}$ is positive and statistically significant at the 99% and 90% confidence level in column (3) and (4),

[20] I thank Prof. Feenberg for updating these data regularly and making these data publicly available. https://users.nber.org/~taxsim/state-rates/

respectively. The result further highlights the finding in Kale *et al.* (2009) that there is a causal relationship between tournament incentives and firm performance. A higher pay disparity between the CEO and other directors will motivate them to exert higher effort to compete for the next CEO position and consequently improve the firm performance. More importantly, the interaction terms' coefficient is negative and statistically significant at the 99% confidence level in columns (3) and (4). The results are consistent with my previous findings that insider trading opportunity weakens tournament incentives' positive effect on the firm performance.

In column (5) and (6), I employ the tax rate as my IV to predict the number of insider transactions $all\_IT_{j,t}$. I omit the first-stage regression output for brevity. I report Sanderson-Windmeijer F statistics, which tests the null hypothesis of under-identification of each endogenous variables because I have three endogenous variables in the first stage regression. These test results show that all three endogenous variables are identified. The Sanderson-Windmeijer F-statistics is marginally below 10 for $all\_IT_{j,t}$. In an unreported result, I separately check the explanatory power of tax rate on insider trading transactions by including the tax rate as the only IV to explain the $all\_IT_{j,t}$ in the first-stage regression. The tax rate coefficient is negative and statistically significant at the 99% confidence level with 11.4 first-stage F statistics[21], meaning a higher tax rate is associated with fewer insider transactions. In column (5) and (6), the coefficient of $\widehat{pay\_gap}_{j,t}$ is positive and statistically significant at the 95% confidence level in both columns, in line with Kale *et al.* (2009). Moreover, the interaction term's coefficient is negative and statistically significant and its magnitude is around a third of the coefficient of $\widehat{pay\_gap}_{j,t}$, suggesting that the tournament incentive's effect on firm performance will be overestimated by a third if the possibility that directors can realize their implicit promotion-based compensations by trading on their private information is overlooked. The coefficient of $\widehat{all\_IT}_{j,t}$ is also positive and statistically significant, suggesting that more insider trading transactions improve firm's performance, mitigating he agency problem by aligning directors' interest with shareholders. Overall, I provide strong evidence that the insider trading opportunity weakens the positive effect of tournament incentive on firm performance. Moreover, these results reaffirm that insiders consider their unrealized promotion prize when they make transactions, consistent with my previous findings.

[Insert Table 6 here]

[21] Stock and Yogo (2005) weak identification test also support my conclusion that the tax rate can explain the variation in insider transaction number.

V. Robustness Test

A. Reverse causality concern

The results so far indicate a systematic increase in non-promoted directors' trading profitability after losing the CEO promotion, mainly because of their forgone tournament incentives. However, it is possible that tournament competitors systematically avoid trading on their private negative information when competing for the CEO position in year (-2, -1) because their trading decisions may adversely affect their winning probabilities because their sell trades would be seen as a lack of belief in their firm. In the same vein, insiders who frequently trade on their private information may have lower probability of promotion to the CEO position. The possible reverse causality will induce endogeneity and further questions my results. I argue that it is applaudable to assume the occurrence of the non-CEO director transactions will not affect the outcome of CEO turnover. Legal insider trading is pervasive in the stock market since 80s, and therefore firms widely accepted that insiders trade on their private information to complement their compensation packages (Roulstone, 2003).

To further reaffirm that my results are not affected by the potential endogeneity, are robust to the alternative estimation method, and do not hinge on the underlying matched sample, I estimate the 2SLS using the last fiscal year's former CEO age as my IV based on the universal sample to generalize my results outside the tournament period. I compare non-promoted directors' transaction profitability with their unconditional return to investigate whether their post-tournament transaction return is significantly different from their transaction returns outside a CEO turnover event when the CEO tournament has not begun. I focus on the isolated CEO turnover and exclude transactions in year +2 to have a cleaner sample with no confounding events to be consistent with diff-in-diff regression, but my results are robust to its inclusion. In the robustness test, I further conduct a test on the predictive power of insider trading on tournament outcome to alleviate further the reverse causality concern.

I find, but not report for brevity reasons that the coefficients of $age\_ceo_{I,t-1}$ in all first-stage regressions are statistically significant with the expected signs, indicating $age\_ceo_{I,t-1}$ 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 my hypothesis. For periods (1,1), the coefficients of $age\_ceo_{I,t-1}$ become negative and statistically significant, suggesting that the recently left CEO is younger than the average former CEO age among all firms covered by Execucomp. The first stage F statistics, computed without the interaction term $\widehat{\text{NPED}\times\text{CEO\_IT}}_{i,t}$ reported at the bottom of Panel A, are all above 10, which is the minimum value to alleviate the weak instrument concern[22], providing significant support for the relevance condition. The Anderson-Rubin F-statistic rejects the null hypothesis and indicates that the endogenous regressor $NPED_{I,t}$ is statistically significant. The result indicates that after losing the CEO

[22] The first stage F-statistics are all relatively large for my insider sell sample because of the large sample size and the two fixed effects and/or the high predictive power embedded in my IV for my endogenous variable. If my IV and endogenous variable are high predictable, then the amount of exogenous variation left for the second-stage regression will be small. To address this concern, I separately estimate all the first-stage regression and check the within R-squared whenever the first stage F-statistics is larger than 200. After using the firm and month fixed effects, the within R-squared in the first-stage regression is generally around 0.4, making my IV suitable.

competition, insiders indeed incorporate more private information embedded into their transactions. The Anderson-Rubin F-statistic is robust to the presence of weak instrumental variable (Andrews, Stock and Sun, 2019) and thus reaffirm my findings. In unreported result, I also check for a potential weak instrument using the Stock and Yogo (2005) test and the Shea Partial R-squared values. I find that my IV does not suffer from weak instrument problem throughout the study. The Difference-in-Sargan *C*-statistic rejects the null hypothesis that the $NPED_{i,t}$ is exogenous to insider transactions' profitability. Since I have only one endogenous variable and one instrumental variable, the *Difference-in-Sargan C-test* is equivalent to a *Hausman* test comparing 2SLS estimates with fixed effect (FE) estimates. The significant *C*-statistics confirm the necessity of applying 2SLS rather than the FE estimator.

Panel A reports the second-stage regression results. For insider purchase sample, I omit to report the coefficient of $NPED \times CEO\_IT_{I,t}$, which is insignificant, suggesting that when non-promoted directors make purchase transactions, they do not consider the current CEO trading activity. The coefficient of $\widehat{NPED}_{i,t}$ is positive and statistically significant in period (0,0). The results indicate that every 1% increase in the probability of the occurrence of CEO turnover event in year 0 leads to a 0.626% increase in the BHAR_m_365. The results are consistent with my diff-in-diff regression result that insider who lost the CEO competition incorporate more positive private information into their purchase transactions, but this is only in event year (0,0). The coefficients of $OutsiderD_{I,j}$ is negative and significant, suggesting that the trades executed by insiders from firms that hired an outsider CEO will trade on their private information with relatively lower aggressiveness.

The endogeneity problem is likely to be more severe in insiders' sell than buy trades, because many insiders do not sell to seek profit. The coefficients of $\widehat{NPED}_{i,t}$ are negative and statistically significant, suggesting that insiders incorporate more private negative information into their sell transactions 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, consistent with my hypothesis that directors strategically time their sell trades against the current CEO. For an otherwise-average insider sell transaction, a 1% increase in the predicted probability of the transaction in year 0 will cause the returns to decrease by 1.117% (= 2.911-1.794) in (0,0) and by 0.6% if the 1% increase is in year 0 and +1. $(NPED \times \widehat{CEO\_IT})_{I,t}$ is the largest in year 0, further highlights that the CEO trading direction plays a more prominent role in the director's decision-making process in year 0 and 1.

The asymmetry effect of CEO trading activity proxied by $CEO\_IT_{I,t}$ in the insider purchase and sell sample is due to the asymmetric litigation risk associated with insider trading based on private information. Insiders sell based on negative private information involve higher litigation risks than purchase based on positive private information. Skinner (1994) argues that the insider purchase transaction will only lead to an opportunity loss, which is more difficult to prevail before juries, than an out-of-pocket loss from the sell trades. Therefore, directors will intentionally sell more shares to exploit their negative private information when the current CEO purchases more shares to prolong their

tenures. These less informative CEO purchase transactions can distract outsiders' attention and cover directors' sell trades because CEOs have higher public visibility and are subject to stricter market scrutiny (Sabherwal and Uddin, 2019). As a result, director's sell transactions, which are on average uninformative, will greatly benefit from the trading opportunities to reduce the litigation risk and incorporate more negative private information into their sell transactions.

Contrary, insider purchase transactions are associated with lower litigation risk, and insiders can trade relatively freely on their positive private information to reap monetary gains. Consequently, there is stronger return predictability based on firm-specific private information embedded in their purchase transactions (Lakonishok and Lee, 2001). Insiders will not benefit greatly from trading against CEO's sell transactions to cover their purchase transactions. Thus, the interaction term is insignificant in unreported results for insider purchase sample. Moreover, the coefficient of $COOD_{I,j}$ is positive and statistically significant in year 1 for the insider sell sample, suggesting that non-promoted insiders from firms with a CEO successor prior to the tournament trade on their private negative information with less aggressiveness than their counterparts from firms that had not pre-assigned a CEO successor.

Overall, the diff-in-diff estimation results are in line with my hypothesis that non-promoted directors will make more informative purchase and sell transactions after losing the CEO promotion. The 2SLS results show insiders will incorporate more negative private information into their sell transactions in all post-event years, consistent with the diff-in-diff regression results. Additionally, I apply the 2SLS estimator with the same IV based on the matched insider sell sample. I display the regression result in Appendix 7. Like my previous finding, the last fiscal year's former CEO age remains a valid predictor for CEO turnover because the first stage F statistics are all above 10, highlighting that my IV's relevance condition is valid in the smaller sample. The signs and significance of the coefficient of $\widehat{NPED}_{I,t}$ are overall consistent with the 2SLS estimates obtained using the universal sample. Insiders incorporate more negative information into their sell transactions in all two post-event years. For the insider purchase sample, there are only 770 observations with a valid non-missing former CEO age. The coefficient is insignificant, and I omit the regression output.

Moreover, I focus on CEO turnover 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 directors who were competing in the turnover. The main variables with interests are the numbers of insider purchase and sell transactions in year -1 and year -2. If there is no reverse causality concern, the coefficients of the numbers of purchase and sells should be statistically insignificant. I control for director's age, tenure, total compensation, delta and vega and other firm-level characteristics all calculated at the end of year -1. If the director was either chief operating officer or president, the $COOD_{I,t-1}$ is equal to one, and zero otherwise. The results not reported but in Appendix S8 show that the coefficients of $no\_buy_{i,t-1}$, $no\_sell_{i,t-1}$, $no\_buy_{i,t-2}$, and

$no\_sell_{i,t-2}$ are all statistically insignificant, highlighting that insider transactions before CEO turnover year bear little predictive power for CEO promotion probability. I additionally control for director and year-industry fixed effects or estimate the regression at the insider-transaction level, all my results remain robust. These results rule out the possibility reverse causality concern

B. Insider sequential sell transactions around dissimulation strategy

Huddart, Hughes and Levine (2001) argue that the implementation of the U.S security law will increase the market scrutiny of insiders' transactions and reduce insider dealing profitability by strictly regulating corporate insiders to disclose their transactions two days after execution publicly. Despite a potential lessening of their returns by as much as a half because of the improved market efficiency, trading on private information remains a profitable strategy for insiders. Consequently, profit-maximizing insiders who actively materialize their private information have incentives to dissimulate their private information by randomly trading in a manner inconsistent with their informational agent role. If their private information is long-lived,[23] they will intentionally make noisy transactions to thwart outsiders who intend to follow them. In the same vein, Kose and Ranga (1997) model that insiders can maximise their expected gains by randomly mixing sell transactions with uninformative purchase transactions to dissimulate the private information they exploit. Biggerstaff *et al.* (2020) report that insiders incorporate their private negative information into multiple sell transactions to minimise the price impact. They define sequential sells as sell transactions executed at most 30 days apart and show that the return of the last transaction in a sequence is more negative than the isolated sell transaction on average. The dissimulation strategy is only effective to disguise the negative private information embedded in sell transactions, not the positive private information embedded in purchase transactions.

Inspired by these results, I test whether the losing tournament effect persists after accounting for the possibility that insiders intentionally split their private negative information into many sell transactions and randomly mix with purchase transactions. I define transactions are in the same sequence when they are executed within ten, fifteen, or thirty calendar days. When a sequence contains both purchase and sell transactions, I aggregate the trading value to compute the sequence's trading direction. If the total value is negative, I define all transactions in the sequence as sequential sells. Other sell transactions not in a sequence are isolated sells.

Furthermore, I adjust the BHAR_m_365 for all transactions in a sequence using the BHAR_m_365 from the last transaction in a sequence, or by extending the holding period from the beginning to the 365 calendar days after the last transaction. I implicitly assume insiders will close all her positions 365 days after the last transaction. In un-tabulated univariate statistics, 48.9% of all sell transactions are sequential sell transactions. A typical sell sequence will last for 23 days, consists of eight transactions on average. Out of these sequential sells, only 7% contains both purchase and sell

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

transactions. I expect the result because the short-swing rule prevents insiders from realizing profit from two offsetting transactions in the first six months after the first transaction. All my results are robust if I remove purchase transactions and solely focus on sequence consists of sell transactions. I re-estimate Equation (5) with the adjusted BHAR_m_365 based on all sequential and isolated sell transactions. In un-tabulated results, I substitute the BHAR_m_365 from the last transaction in a sequence for all sequential transactions in the same sequence. The coefficients of $\widehat{NPED}$ are negative and statistically significant, providing further robustness to my results. Furthermore, I extend the holding period for sequential sells from 1 day after the first transaction to 365 days after the last transaction. Since the holding horizon varies depending on the sequence length, I compute the daily average BHAR_m_365×252, the median number of trading days in a 365-calendar day holding period. I report the coefficients of $\widehat{NPED}$ in Table 7 Panel B. My overall results remain unchanged, but the coefficients of $\widehat{NPED}$ become more negative in all two post-event years for sells, implying the losing tournament effect is stronger after controlling for insider dissimulation strategy.

[Insert Table 7 here]

C. Additional tests for IV exclusion restriction

One of the main assumptions behind my results is that my IV, the last year former CEO's age, is not correlated with the private information that non-CEO directors are exploiting. The former CEO's age *per se* will not affect a firm's valuation as it bears no impact on its future cash flow, but I recognize the possibility that former CEOs may affect her firm's future valuation through the adaption of corporate decisions with long-lasting effect. Although there is no reason to believe that the preference for a long-last policy is systematically related to director age, this possible violation of exclusion restriction will lead to an inconsistent estimate and weakens my conclusions. I alleviate this potential concern by including a set of proxy variables for corporate performance in my 2SLS regression.

In the first robustness test, I add to Equation (5) fourteen additional control variables that embed predictive power for the firm's future fundamental and are possibly determined by the personal preferences of CEOs in different age groups to better demonstrate the validity of the exclusion restriction and the robustness of my results. Appendix S4 details the construction of my variables. I include $tobin's\ Q_{I,t-1}$, $capital\text{-}to\text{-}sale_{j,t-1}$, $advertising\text{-}to\text{-}sale_{j,t-1}$, $capital\_intensity_{I,t-1}$, $leverage_{I,t-1}$, $dividend\text{-}yield_{j,t-1}$ to control for firm level characteristics. I compute the segment sales-based Herfindahl index denoted as $firm\_focus_{I,t-1}$ to control for firm diversification. I include $cash\_flow\_vol_{I,t-1}$ and $skt\_ret\_volatility_{I,t-1}$ to control for firm risk taking incentives, and $institution\_ownership_{j,q-1}$, $independent\_director_{j,t-1}$ and $independent\_committee_{j,t-1}$ which is the proportion of independent directors on the compensation committee to control for corporate governance. I also control for the natural logarithm of the current age of non-CEO directors denoted as $lnage_{j,t}$.

Following Dang *et al.* (2021), I include $analyst\_talent_{j,t-1}$, which significantly lowers the insider trading profitability, to proxy for the average talent of sell-side analysts following the firm *j* in the last fiscal year and to control for industry-level informativeness[24]. Lastly, I include $CEO\_tenure_{j,t-1}$ to control for the tenure of CEO in the last fiscal year to show that my IV is not simply capturing the current CEO tenure. Table 8 Panel A reports the result without the interaction term $\widehat{NPED \times CEO\_IT}_{I,t}$ for the insider purchase sample which is insignificantly. In column (1), the coefficient of $\widehat{NPED}_{I,t}$ is 1.448 and significant at the 95% confidence level. I remove these additionally control variables one by one and the statistical significance of the coefficient of $\widehat{NPED}_{I,t}$ increases monotonically with my sample size while remaining positive. For insider sell samples, the sign and significance of $\widehat{NPED}_{I,t}$ and $\widehat{NPED \times CEO\_IT}_{i,t}$ are consistent with my previous results. I find, but do not report, similar results when firm characteristics above are at the end of the year that the former CEO left the company.

As the second robustness test, I consider that former CEO's age will only affect non-CEO's trading profitability through CEO turnover. Therefore, if I regress the BHAR_m_365 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, I re-estimate the regression in Table 8 by substituting the former CEO's age for the $\widehat{NPED}_{I,t}$with the same set of control variables. I find that the coefficient of the former CEO's age is statistically insignificant for both insider purchase and sell samples, strengthening the plausibility of exclusion restrictions further. I recognize that some firms retain their former CEOs on the board after they left their role. I argue that the possible retention does not affect the irrelevance condition because Evans, Nagarajan and Schloetzer (2010) show that the CEO retention does not affect firm's future stock return, and only 11.67% of my insider trading sample was made in a CEO retention year. Nevertheless, I replicate my 2SLS regression by excluding firms that retain the former CEO after the turnover. I lost 5% (2.6%) of insider purchase and 3.8% (2.6%) of insider sell in year 0 (year 1). In unreported results, all my conclusions remain robust.

D. Other robustness tests

In the third robustness test, I refine my year *0* sample into the transactions-day level. I have shown that directors are more likely to incorporate more positive (negative) private information into their purchase (sell) transactions in year 0. The conclusion hinges crucially on the assumption that I do not mis-specify the insider transactions prior to the tournament outcome as post-tournament transactions. I rely on Execucomp item *becomeceo* to identify the specific date for the CEO turnover. Jenter and Lewellen (2021) report that Execucomp reports wrong CEO turnover dates *becomeceo* for several CEO turnover events. I first manually check all the CEO turnover date reported by Execucomp. For the *becomeceo* date that is one calendar year apart from the fiscal year, I manually check and correct it by

[24] I are grateful to Dr Li for making the analyst talent data available.

crosschecking BoardEx. I reclassify the transactions before the succession of the new CEO as pre-tournament transactions and re-estimate Equation (5). In an un-tabulated result, the coefficients of $\widehat{NPED}_{I,t}$ are 0.733 and -3.078 and is statistically significant at the 90% and 95% confidence level for insider purchase and sell samples in year 0, respectively.

I also use alternative holding periods and the four-factor alpha over 30-, 180- and 360- calendar holding periods, as alternative measures of abnormal returns to check for robustness of my results. I use Kenneth French's Data Library[25] to gather the Size, Value, Momentum factors, risk-free rate to compute the alpha from Carhart (1997)'s Four-Factor model, which builds on the Fama-French Three-Factor model (Fama and French, 1993) as follows:

$$return_{it} - rf_t = \alpha + \beta_1(MKT_t - rf_t) + \beta_2 SMB_t + \beta_3 HML_t + \beta_4 MOM_t + \epsilon_t \quad (8)$$

α, the risk-adjusted return is estimated from one day after the transaction date over the next 30/180/365 calendar days. $return_{i,t}$ it is the daily return adjusted for dividend, rf_t is the risk-free rate proxied by the one-month T-bill rate. MKT_t is the CRSP value-weighted market index. *SMB/HML/MOM* denote the conventional size, book-to-market, and momentum factors. I time the daily α by the median number of trading days of 22, 126, and 252 in these 3 holding periods. Additionally, I report the raw cumulative return $ret_{t+1,t+i}$ and the NYSE value-weighted size-decile adjusted return *BHAR_size_i*. Table 8 Panel B reports the regression results of re-estimating Equation (5) but for only the coefficient of $\widehat{NPED}_{I,t}$ for brevity. For the buy trades, $\widehat{NPED}_{I,t}$ is mainly insignificant. In contrast, for the sell trades, it is mainly negative and significant, suggesting that these transactions are loss avoiding for the 180 and 365 holding periods. The remaining results are consistent with the previous results.

The fourth robustness test investigates the probability that performance-induced turnover or planned succession drives the increase in insider trading profit. To proxy for the former, I create underperforming dummy variable equals one for the bottom quintile of firms divided by the raw annual stock return in the last fiscal year in the same two-digit SIC industry among all S&P 1500 firms, zero otherwise. I follow the same specification in Equation (5) with the additional underperforming dummy as the moderator. In an un-tabulated result, the interaction term between the underperforming dummy variable and $\widehat{NPED}_{I,t}$ remains statistically insignificant in all post-event years for both buy and sell samples, suggesting that performance-induced turnover does not drive my results.

The fifth robustness tests the validity of my diff-in-diff regression results over a $(-2, +1)$ period around pseudo-CEO turnovers, which are arbitrarily set as three years before or after the actual CEO turnover. I use the same pair of treated and matched firms obtained early in the study but remove

[25] https://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html. I thank Professor French for making these data publicly available.

the firms that had a CEO turnover in the pseudo-event window. I re-estimate Equation (2). I find, but not report for brevity, that the coefficient of the interaction term $Post{\times}Treat_{I,t}$ remains statistically insignificant for both insider purchase and sell samples, supporting the validity of the parallel trends assumption and the credibility of my diff-in-diff design.

Finally, to confirm that unobservable market and firm conditions do not affect my findings, I re-estimate Equation (5) using 1,000 placebo tests for insider purchase and sell samples separately. Although the use of 2SLS estimator has greatly eliminated the probability that chance drives my results, I conduct the placebo tests to reaffirm the robustness of my results and my IV validity. Each test entails randomly selecting 400 firm-year observations with at least one insider purchase transaction and 1,600 firm-year observations with at least one insider sell transaction considered as CEO turnover year for insider purchase and sell sample, respectively. These two numbers are the nearest hundreds for the actual numbers of distinct CEO turnover firm-year observations, which are 386 and 1,601 in year 0 for purchase and sell samples, respectively. I remove the firm-year observations with actual CEO turnover event and the following two years from my sample. For each of the firm-year observations, I match the insider trading transactions in the given year and set $NPED_{I,t}$ to be one for all insider transactions in the year. I replicate Equation (5) without $OutsiderD_{I,t}$ and $COOD_{I,t}$ and report the coefficient of $\widehat{NPED}_{I,t}$ and the first-stage F statistics in Table 8 Panel C. If my results are due to chance or unobservable factors, a relatively large proportion of my placebo tests report will have a higher first-stage F statistics and the coefficients of $\widehat{NPED}_{I,t}$ will be statistically positive (negative) for insider purchase (sell) sample, respectively. Column (1) shows that, the mean coefficient for the insider buy sample is statistically indifferent from zero. The distribution of coefficient of $\widehat{NPED}_{I,t}$ is right-skewed. For the insider sell sample, the mean coefficient is positive and statistically insignificant with a right-skewed distribution. On the right-hand side of the panel, I report the percentage of the placebo sample that has both a positive (negative) significant coefficient of $\widehat{NPED}_{I,t}$ and a first-stage F-statistics larger than 10 for insider purchase (sell) sample. There is no single test for insider purchase samples with both a significant positive coefficient and a valid first stage F-statistics.

For insider sell sample, I find only 8 cases, out of 1,000 placebo tests, that report a significantly negative coefficient of $\widehat{NPED}_{I,t}$ and an F-statistics larger than 10. Relying on a one-sided binomial test-statistic, none of the proportions is statistically different from the corresponding theoretical levels of 1%, 5% and 10%. I also find 34 tests that report a first-stage F-statistics larger than 10 with a maximum of 19. In Panel A, my first-stage F is generally larger than 10, indicating my IV will not randomly be significant, and it does not contain predictive power outside CEO turnover event. I also conduct 1,000 placebo tests for my diff-in-diff regression. I first randomly select 1,000 firm-year observations without CEO turnover and not in any CEO turnover window. I then match these treated firms with one control firm with placement in the same year *t* based on the *t-1* average insider purchase/sell profitability,

logarithm of the total asset and the book-to-market ratio. I assume year *t* to be the event year. I estimate a diff-in-diff regression by using the observations of matched sample for year (t-2, t). I conduct placebo tests for insider purchase and sell samples separately. I restrict the treated firm cannot match to itself in the last year. I report the placebo test results in Table 8, Panel D. The average coefficient of the interaction term is negative (positive) for insider purchase (sell) sample. In column (5) to (7), I report the percentage of placebo tests with statistically significant and positive (negative) coefficient for purchase (sell) sample. As in Panel C, no proportion is statistically different at any significance level based on a one-sided binomial test-statistic.

Additionally, Goodman (2010) reports that Chief Finance Officers (CFOs) are less likely to become the next CEOs because these two roles required different skills. I find that only 5% of the new CEOs in my sample period previously served as CFOs in their companies. To test that CFO trading does not drive my results, I remove all CFO transactions in my pre-turnover window, which accounts for 9% of both the insider purchase and sell transactions sample. In unreported results, I re-estimate the results in Table 3 and the coefficient of $(Treat \times Post)_{i,t}$ for insider sell in year +1 becomes weakly significant at the 90% confidence level, and the sign and significance of all other results remain robust. I further drop 10% observations within year $(-2, 1)$ from firms with a COO prior to the CEO turnover and re-estimate both the diff-in-diff and 2SLS regression, all my results remain robust.

Overall, these results indicate that if I use a randomly selected sample of firms without CEO turnover events, I cannot replicate my main findings obtained from both diff-in-diff regression and 2SLS. The placebo tests further indicate that my IV is only relevant to explain years close to the CEO turnover, and it is extremity unlikely that I will obtain a significantly positive (negative) $\widehat{NPED}_{I,t}$ while satisfying my IV relevance condition. The profitability of an average insider transaction embedded in purchase (sell) transactions is unlikely to increase (decrease) without a CEO turnover.

[Insert Table 8 here]

E. Non-promoted director future promotion opportunity and sample selection

I recognize that the non-promoted directors 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 compensate themselves for the forgone CEO promotion. I recognize that the possibility is trivial because Execucomp mainly reports the top four highest-paid directors whose career path is already at the top of the corporate hierarchy in addition to the CEO. Therefore, any increase in their compensation package will not be as significant as the CEO promotion reward. To investigate this possibility, I focus on isolated CEO promotion events not followed by another CEO tournament window in the next six years, i.e., where there is only one CEO turnover from year 0 to 7. I use the same restriction to calculate the pay rise for directors' total compensation package with the absence of CEO turnover. I rank directors by their total compensation

package in their firms after excluding CEO and directors who are not competing for the CEO position. For example, if a director's pay rank is 1, her total compensation package is the highest among all CEO competitors. Then, I compare their pay rank and total compensation package between years -1 and 4.

I find, but not report, that non-promoted director's pay rank decreases by 1.4 from year -1 to 4, with year 0 as the CEO turnover year. The pay rank decrease is 0.6 in the same 5-year period without losing CEO turnover. The difference is statistically significant. I compute the difference in total compensation package between years -1 and 4 to understand further the dollar value of the faster promotion speed. I find non-promoted directors receive a $0.73 million pay rise in a 5-year time after losing the CEO turnover, compared to $0.57 million if they have not lost the CEO competition. The $0.16 million difference is statistically significant. Directors who were the 4th highest paid among all CEO candidates in year -1 have relatively more promotion opportunities than directors who were the highest-paid non-CEO directors. These directors receive a $0.73 million pay rise if they lose the CEO competition, $0.25 million higher than that $0.48 million they normally receive in a five-year time. Insiders who were the top three highest-paid directors before losing the CEO promotion do not receive any significant additional pay rise in the next 5-year period. Non-CEO director's total compensation package is $1.86 million in year -1, and the newly appointed CEO's average total compensation package in year 0 is $5 million. The additional $0.16 million pay rise in five years is unlikely to weaken their incentives to compensate themselves for the forgone CEO promotion opportunity.

I estimate a fixed effect regression with director, firm, and year fixed effects. The dependent variable is the change in the natural logarithm value of the total compensation in one or two-years' time. I focus on a dummy variable that equals to one for year (0,4) and zero otherwise. After controlling for director's age, tenure, delta and vega, firm's size, leverage, book-to-market ratio, ROA, and Tobin's Q, I find, but not report, no significant change in the total compensation of non-promoted director in both one- and two-years' time after they have lost the CEO promotion, in line with Kale, *et al.* (2009) and Chan *et al.* (2022) findings that losers are not compensated for the dimmer career prospects.

VI. Conclusion

Corporate directors' remuneration contracts consist of both the explicit payment component such as annual salary, bonus and the implicit promotion-based component that provides them with the promotion opportunity and the chance to receive a pay rise from their higher job position known as the tournament incentive. For the high-rank directors, their only promotion destination is the CEO position. An unsuccessful CEO promotion lowers drastically or forgoes completely the likelihood of winning future CEO competitions. Consequently, the overall value in her remuneration contract is lower because the expected value of their implicit promotion-based component has decreased. To compensate themselves for the overall decrease in her compensation contract, non-promoted directors may more

aggressively trade on her private information. I investigate the causal relationship between losing the CEO promotion opportunity and the director trading profitability.

I eliminate the endogeneity by using a matched sample to specify a diff-in-diff regression. I show that losing the CEO competition causes an increase (decrease) in the abnormal returns of the non-promoted directors' purchase (sell) transactions. The results indicate that directors indeed trade on their private information more aggressively and incorporate more positive (negative) private information into their purchase (sell) transactions. While the profitability of their sell transactions persists until one year after losing the tournament, that of their buy trades is limited to the year of losing CEO promotion competition.

Moreover, insiders with higher implicit promotion-based component incorporate more negative private information into their sell transactions, supporting the argument that insiders trade to compensate themselves for the forgone promotion opportunity. These changes in trading profitability are in addition to the profitability changes attributed to the different level of firm-level price information informativeness. My results remain the same if I use the last fiscal year former CEO's age as my IV and estimate a 2SLS regression to eliminate the endogeneity. Directors are more sophisticated when selling their shares than buying shares due to the asymmetric litigation risk embedded in these two types of transactions. They will incorporate more negative information into their sell transactions and execute more opportunistic sells when the newly appointed CEO increases their holdings. I do not find the same trading strategy when directors buy shares. Lastly, I revisit the findings in Kale *et al.* (2009) and show that the insider trading opportunity will weaken the positive relationship between the tournament incentives and firm performance because insiders use their transactions to realize the tournament incentives prior to the tournament.

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Table 1: Summary Statistics

This table reports the summary statistics for the main sample with matched firm. In Panel A (B), I report the sample averages for the non-CEO insider purchase (sell) trades around CEO turnover event. $OutsiderD_{i,j}$ is a dummy equal to one if the promoted CEO is an outsider; $COOD_{i,j}$ is a dummy equal to one if the CEO succession was planned in $(-2,-1)$; *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. $ret30_{j,(d-1,d-30)}$ and $mom_{j,(d-31,d-364)}$ are the long-term and short-term stock price momentum; $bm_{j,m-1}$, $roa_{j,t-1}$, $rd_{j,t-1}$ and $size_{j,m-1}$ 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 the financial analyst coverage; $delta_{i,t-1}$ is a dollar change in director *i*'s wealth associated with a 1% change in the firm's stock price (in $000); $vega_{i,t-1}$ is the dollar change in director *i*'s wealth associated with a 0.01 change in the standard deviation of the firm's returns (in $000); $rating_{i,t-1}$ is the yearly industry average S&P long-term rating from Compustat, where I assign AAA a value 2 to CC a value of 23, and then scaled by dividing by 9, so one unit in the increase in the scaled rating corresponding to an increase in rating from AAA to BBB and an increase in rating from BBB to CCC; $CEO\_IT\_Net\_Value_{i,t}$ is the net insider trading value of the current CEO. I also include the following dummy variables: $OutsiderD_{i,j}$ is equal to one for firms with outside CEO appointment during the year $(0,1)$; $COOD_{i,j}$ is equal to one for firms with COO; $high\_incentiveD_{i,t-1}$ is equal to one for high (in the top three) incentive directors and zero otherwise; Appendix S4 details the variables. ***, **, * (^{a}, ^{b}, ^{c}) indicate the sample mean (differences in means and medians) between the pre- (-2, -1) and post- (0, 1) event) is statistically different at the 99%, 95% and 90% confidence level, respectively. All variables except insider purchase size and shares are winsorised at the top 99% and the bottom 1% level.

	Event Window (-2, -1)			Event Window (0, 1)		
Variable	Mean	Median	Observation	Mean	Median	Observation
Panel A: Non-CEO Insider Purchase transactions around CEO Turnover Event						
BHAR_m_365	0.059**	-0.059	834	0.304*** a	0.119 a	818
pay_gap_firm (000s)	1,560.411***	696.403	742	2,079.033*** a	674.560	832
Non-CEO compensation (000s)	1,403.734***	893.773	834	1,070.692*** a	681.124 a	832
illiq (000s)	0.271***	0.042	831	0.576*** a	0.087 a	832
marketcap (million)	2,425.926***	834.245	834	1,765.036*** c	545.452 a	832
Mom	0.059***	0.050	801	0.000 b	0.042	831
ret30	-0.067***	-0.056	717	-0.021*** a	-0.029 a	709
bm	0.787***	0.597	833	0.883*** b	0.752 a	832
numest	7.753***	6.000	834	5.905*** a	5.000 a	832
ROA	0.029***	0.025	834	-0.009** a	0.005 a	832
rd	0.028***	0.000	834	0.034***	0.001 a	832
delta	174.156***	15.596	805	25.120*** a	11.540 a	767
vega	18.917***	5.929	803	11.119*** a	5.528	760
$OutsiderD_{i,j}$	0.000	0.000	834	0.369*** a	0.000 a	832
$COOD_{i,j}$	0.000	0.000	834	0.133*** a	0.000 a	832
$high\_incentiveD_{i,t-1}$	0.388***	0.000	834	0.453*** a	0.000 a	832
$rating_{i,t-1}$	1.325***	1.353	825	1.319***	1.366	821
$CEO\_IT\_Net\_Value_{i,t}$	-819,345***	0.000	834	300,034*** a	-42,188 a	832

Average Purchase No. Shares	12,255***	2,882	834	10,176***	2,000 [a]	832
Average Purchase Value	156,920***	38,743	834	163,246***	19,689 [a]	832
Average No of Observations	417			416		
Panel B: Non-CEO Insider Sell Transactions around CEO Turnover Event						
BHAR_m_365	0.057***	0.012	17,137	0.026*** [a]	-0.005 [a]	12,676
pay_gap_firm (000s)	3,507.651***	2,183.192	16,194	3,340.159*** [a]	2,147.950 [a]	13,019
Non-CEO compensation (000s)	2,308.358***	1,400.411	17,153	2,143.866*** [a]	1,346.983 [a]	13,062
illiq (000s)	0.049***	0.007	17,146	0.032*** [a]	0.005 [a]	13,062
market cap (million)	12,092.906***	2,751.448	17,153	14,112.585*** [a]	3,361.305 [a]	13,062
mom	0.320***	0.264	16,798	0.288*** [a]	0.240 [a]	13,059
ret30	0.059***	0.053	14,452	0.056*** [a]	0.048 [a]	11,048
bm	0.419***	0.334	17,143	0.418***	0.337 [a]	13,062
numest	12.497***	11.000	17,153	12.492***	11.000	13,062
ROA	0.064***	0.062	17,150	0.061*** [a]	0.060	13,062
rd	0.058***	0.000	17,153	0.078*** [a]	0.005 [a]	13,062
delta	229.445***	65.856	16,295	154.390*** [a]	57.420 [a]	12,345
vega	49.088***	18.484	16,293	48.193***	16.870 [a]	12,342
$OutsiderD_{i,j}$	0.000	0.000	17,153	0.295*** [a]	0.000 [a]	13,062
$COOD_{i,j}$	0.000	0.000	17,153	0.186*** [a]	0.000 [a]	13,062
$high\_incentiveD_{i,t-1}$	0.537***	1.000	17,153	0.562*** [a]	1.000 [a]	13,062
$rating_{i,t-1}$	1.380***	1.431	17,069	1.392*** [a]	1.439 [a]	12,645
$CEO\_IT\_Net\_Value_{i,t}$	-15,508,847***	-3,497,724	17,153	-2,581,300*** [a]	0,000.000 [a]	13,062
Average Sell No. Shares	33,382.895***	11,191	17,153	27,781*** [a]	10,000 [a]	13,062
Average Sell Value	1,039,358.5***	355,280	17,153	944,193*** [a]	327,369 [a]	13,062
Average Yearly No of Observations	8,576			6,531		

Table 2: Insider trading propensity after losing the CEO competition.

Table Panel A reports the summary statistics at firm level for both the treated firms and control firms in the pre-CEO turnover period (-2, -1) and Panel B shows summary statistics of BHAR in event window (-2, +1). Firms that have CEO turnover event in year *t* are matched with firms on the average insider purchase/sell profitability, logarithm of the total asset and the book-to-market ratio in the fiscal year *t-1* using Mahalanobis distance. Column (3) and (6) reports the t-test results by assuming unequal variance between treated and control firms for insider purchase and sell transaction, respectively. Panel C 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. I identify opportunistic traders by following Cohen *et al.* (2012). Appendix S4 defines all control variables in the table. ***, **, and * denote significance at the 99%, 95% and 90% confidence level, respectively. Standard errors reported in parentheses are computed based on robust standard errors clustered at the firm-month level. All variables are winsorised at the top 99% and the bottom 1% level.

Panel A: Summary Statistics in Pre-Treatment Period (-2, -1) at firm level						
	Insider Purchase Transactions			Insider Sell Transactions		
	(1)	(2)	(3)	(4)	(5)	(6)
	Treated Firms	Control Firms	Difference (1)-(2)	Treated Firms	Control Firms	Difference (4)-(5)
$\Delta BHAR\_m\_365_{(-2,-1)}$	0.124	0.111	0.013	-0.055	-0.058	0.003
	(0.030)	(0.033)	(0.045)	(0.005)	(0.005)	(0.008)
$total\ asset_{i,t-1}$	7.322	7.238	0.083	7.99	7.92	0.04
	(0.085)	(0.081)	(0.118)	(0.033)	(0.033)	(0.047)
$mom_{j,\ t,(d-31,d\ -364)}$	0.148	0.184	-0.036	0.176	0.192	-0.015
	(0.025)	(0.020)	(0.033)	(0.007)	(0.007)	(0.010)
$bm_{j,m-1}$	0.634	0.634	0.000	0.492	0.488	0.003
	(0.019)	(0.022)	(0.029)	(0.007)	(0.007)	(0.010)
$roa_{j,t-1}$	0.027	0.033	-0.006	0.053	0.055	-0.002
	(0.001)	(0.000)	(0.007)	(0.002)	(0.002)	(0.003)
Non-CEO total comp ($000s)	1,231	1,325	-94.04	2,115	1,971	144^{***}
	(59.62)	(92.52)	(110.06)	(20.24)	(17.69)	(26.89)
Transaction Value	156,920	89,887	$67,032^{***}$	1,004,076	1,039,358	35,285
	(16,169)	(19,477)	(25,314)	(18,873)	(20,050)	(27,535)
N Matched Firm-Year	192	192		1331	1331	
N Transactions.	834	889		17,153	17,804	
Panel B: Summary Statistics of BHAR in event window (-2, +1)						
$BHAR\_m\_365_{(t = -2)}$	-0.017	-0.002	-0.015	0.069^{***}	0.070^{***}	-0.001
	(0.029)	(0.022)	(0.037)	(0.004)	(0.004)	(0.006)
$BHAR\_m\_365_{(t = -1)}$	0.085	0.115	-0.030	0.047^{***}	0.040^{***}	0.007
	(0.029)	(0.021)	(0.036)	(0.004)	(0.004)	(0.006)
$BHAR\_m\_365_{(t = 0)}$	0.405	0.213	0.192^{***}	0.032^{***}	0.043^{***}	-0.011^{*}
	(0.032)	(0.026)	(0.041)	(0.004)	(0.006)	(0.007)
$BHAR\_m\_365_{(t = +1)}$	0.075	0.279	-0.204^{***}	0.014^{***}	0.038^{***}	-0.024^{***}
	(0.038)	(0.050)	(0.062)	(0.004)	(0.004)	(0.006)

Panel C: Opportunistic Insider trading propensity after losing the CEO competition				
	Insider Purchase Transactions		Insider Sell Transactions	
Year *t*	(0,0)	(1,1)	(0,0)	(1,1)
$Post_{i,t}$	-0.050**	-0.073	-0.025***	-0.066***
	(0.023)	(0.054)	(0.008)	(0.011)
$Treat_{i,t}$	-0.064**	-0.107**	-0.006	-0.015
	(0.027)	(0.044)	(0.010)	(0.010)
$(Treat \times Post)_{i,t}$	0.043	-0.024	0.025**	0.047***
	(0.029)	(0.084)	(0.012)	(0.016)
$CEO\_IT_{i,t}$	-0.025*	0.031**	0.008***	0.006**
	(0.013)	(0.015)	(0.003)	(0.003)
Constant	0.674	1.668*	1.295***	1.391***
	(0.614)	(0.942)	(0.100)	(0.111)
Control Variables	Yes	Yes	Yes	Yes
Sample	987	715	30,879	28,462
Within R^2	0.17	0.22	0.36	0.37
Fixed Effect	Firm, Month	Firm, Month	Firm, Month	Firm, Month

Table 3: Difference-in-difference regression output

The dependent variable is BHAR_m_365. $(Post \times Treat)_{I,t}$ is a dummy variable equals to one for firms that have a CEO turnover in year *t*, and zero otherwise. Other variables are described in Table 1 and Appendix S4. I only include sample in pre-CEO turnover period (-2, -1) and post-CEO turnover period (t, t+i); I exclude years in the post-CEO turnover period other than *t*. 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.

	Insider Purchase			**Insider Sell**		
Year *t*	(0,1)	(0,0)	(1,1)	(0,1)	(0,0)	(1,1)
	(1)	(2)	(3)	(4)	(5)	(6)
$Post_{i,t}$	0.105	-0.002	0.152	0.021***	0.007	0.042***
	(0.073)	(0.051)	(0.181)	(0.008)	(0.009)	(0.011)
$Treat_{i,t}$	-0.320***	-0.349***	-0.342**	0.017*	0.011	0.008
	(0.108)	(0.117)	(0.133)	(0.010)	(0.010)	(0.010)
$(Treat \times Post)_{i,t}$	0.082	0.245**	-0.177	-0.038***	-0.030**	-0.048***
	(0.110)	(0.101)	(0.256)	(0.013)	(0.015)	(0.017)
$CEO\_IT_{i,t}$	0.036	0.015	0.108**	0.010***	0.009***	0.013***
	(0.029)	(0.024)	(0.044)	(0.003)	(0.003)	(0.003)
$COOD_{i,j}$	-0.442***	-0.421***	-0.440*	0.060***	0.069***	0.054**
	(0.135)	(0.145)	(0.227)	(0.018)	(0.021)	(0.025)
$ret30_{j,t,(d-1,d-30)}$	-0.811**	-0.333**	-0.963**	-0.171***	-0.185***	-0.131***
	(0.317)	(0.152)	(0.447)	(0.032)	(0.032)	(0.036)
$mom_{j,\ t,(d-31,d\ -364)}$	-0.182***	-0.102	-0.105	-0.035***	-0.039***	-0.036**
	(0.070)	(0.079)	(0.100)	(0.012)	(0.012)	(0.014)
$size_{j,m-1}$	-0.909***	-0.766***	-0.764***	-0.275***	-0.263***	-0.276***
	(0.159)	(0.116)	(0.243)	(0.012)	(0.011)	(0.014)
$delta_{i,t-1}(\times 0.01)$	0.002***	0.135***	0.129**	0.002**	0.001*	0.002**
	(0.000)	(0.051)	(0.053)	(0.001)	(0.001)	(0.001)
$vega_{i,t-1}(\times 0.01)$	-0.257***	-0.240***	-0.201*	-0.015***	-0.007**	-0.009**
	(0.092)	(0.087)	(0.119)	(0.004)	(0.003)	(0.004)
$lncompen_{j,t-1}$	0.018	0.033	0.027	0.032***	0.026***	0.035***
	(0.035)	(0.029)	(0.035)	(0.007)	(0.006)	(0.007)
$rating_{i,t-1}$	3.996***	3.207***	3.963***	-0.100	0.011	-0.147*
	(0.950)	(0.596)	(1.375)	(0.076)	(0.078)	(0.084)
Constant	0.777	0.895	-0.802	2.120***	1.934***	2.153***
	(0.907)	(0.938)	(1.026)	(0.146)	(0.145)	(0.166)
Sample	2,126	1,833	1,328	45,776	36,829	33,658
Within R^2	0.38	0.37	0.39	0.15	0.15	0.14
Fixed Effect	Firm, Month	Firm, Month	Firm, Month	Firm, Month	Firm, Month	Firm, Month

Table 4: Insider heterogeneity and their trading intensity

This table reports the fixed effect regression output based on the matched sample. The dependent variable is BHAR_m_365. I match firms with CEO turnover event in year *t* with firms with no CEO turnover in (-2, 2) on the average insider purchase/sell profitability, logarithm of the total asset and the book-to-market ratio in the fiscal year *t-1* using Mahalanobis distance. In Panel A, I interact the treatment dummy and post-event dummy with $Pay\_rank_{i,t}$, the rank of non-promoted director sorted by their total compensation in year -1 among all tournament competitors. In Panel B, the moderator variable is $ln\text{age}_{I,t}$ which is the natural logarithm of the age of the insider *i* in year *t*. In Panel C, the moderator variable is $SHD_{I,t}$, a dummy variable equals to one for short-horizon insiders identified by following Akbas et al (2020), and zero otherwise. In Panel D, the moderator variable is $Probability_{i,t-1}$, the estimated probability of insider becoming the CEO estimated using his personal characteristics. I include firm and month levels and control variables described in Table 1 and detailed in Appendix S4. Standard errors in parentheses are based on robust standard errors clustered at the firm-month level. ***, **, and * denote significance at the 99%, 95% and 90% confidence level, respectively. All variables are winsorised at the top 99% and the bottom 1% level.

	Insider Buy Trades		Insider Sell Trades	
Year t	(0,0)	(1,1)	(0,0)	(1,1)
	Panel A: Tournament Prize			
$(Treat\times Post)_{i,t}$	0.248*	-0.072	-0.076***	-0.091***
	(0.150)	(0.363)	(0.022)	(0.027)
$Pay\_rank_{i,t}$	0.003	-0.003	-0.007**	0.006**
	(0.020)	(0.032)	(0.004)	(0.003)
$(Post\times Treat\times Pay\_rank)_{i,t}$	-0.007	-0.083	0.018***	0.019***
	(0.031)	(0.078)	(0.006)	(0.007)
Control variables and main levels	Yes	Yes	Yes	Yes
Sample	1,590	1,100	34,883	28,988
	Panel B: Age Effect			
$(Treat\times Post)_{i,t}$	-1.988	0.634	-0.743**	-1.032***
	(1.412)	(2.459)	(0.322)	(0.384)
$lnage_{i,t}$	0.312*	0.185	0.137***	0.152***
	(0.180)	(0.162)	(0.037)	(0.037)
$(Post\times Treat\times lnage)_{i,t}$	0.556	-0.133	0.183**	0.250***
	(0.356)	(0.631)	(0.081)	(0.096)
Control variables and main levels	Yes	Yes	Yes	Yes
Sample	1,415	1,074	32,158	29,552
	Panel C: Investment Horizon			
$(Treat\times Post)_{i,t}$	0.167***	0.177*	-0.034**	-0.053***
	(0.074)	(0.104)	(0.016)	(0.017)
$SHD_{i,t}$	0.061	0.227	0.032	0.038*
	(0.174)	(0.220)	(0.021)	(0.020)
$(Post\times Treat\times SHD)_{i,t}$	-0.177	0.090	0.070**	0.080*
	(0.252)	(0.541)	(0.035)	(0.044)
Control variables and main levels	Yes	Yes	Yes	Yes
Sample	1,833	1,328	36,829	33,658
	Panel D: Predicted probability of becoming CEO			
$(Treat\times Post)_{i,t}$	0.048	0.074	0.042*	0.028
	(0.149)	(0.512)	(0.024)	(0.025)
$Probability_{i,t}$	0.440**	0.527	-0.016	0.007
	(0.224)	(0.323)	(0.031)	(0.032)
$(Post\times Treat\times Probability)_{i,t}$	-0.400	1.702*	-0.163***	-0.190***
	(0.655)	(1.027)	(0.059)	(0.067)
Control variables and main levels	Yes	Yes	Yes	Yes
Sample	635	542	24,793	23,358

Table 5: Insider trading after CEO turnover and changes in firm and investor characteristics

This table reports the fixed effect regression output based on matched sample. In Panel A, the dependent variable is the change in return on asset between year t and year t+2. In Panel B, the dependent variable is the change in investor sentiment measured as firm-specific component from the market-to-book decomposition of Rhodes–Kropf, *et al.*, (2005). The change in investor sentiment $\Delta Sentiment_{-1,1}$ is measured between year t-1 to year t+1. In Panel C, I obtain the $\Delta r_{t,t+2}$ by following Cziraki *et al.* (2021) to estimate a modified Fama and French (1993) Three-Factor model. I include the control variables in Equation (2), omitted for brevity. 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.

	Insider Purchase		**Insider Sell**	
	(1)	(2)	(3)	(4)
Year *t*	(0,0)	(1,1)	(0,0)	(1,1)
		Panel A: Future Firm Performance		
Dependent Variable	$\Delta ROA_{t,t+2}$	$\Delta ROA_{t,t+2}$	$\Delta ROA_{t,t+2}$	$\Delta ROA_{t,t+2}$
$\text{Post}_{i,t}$	-0.001	0.015	-0.001	-0.003
	(0.012)	(0.012)	(0.003)	(0.003)
$\text{Treat}_{i,t}$	-0.087***	-0.069***	0.015***	0.019***
	(0.022)	(0.019)	(0.004)	(0.004)
$(\text{Post}\times\text{Treat})_{i,t}$	0.007	-0.018	-0.020***	-0.011**
	(0.015)	(0.025)	(0.005)	(0.005)
Other Control Variable	Yes		Yes	Yes
Within R-square	0.15	0.19	0.07	0.06
Fixed Effect	Firm, Month	Firm, Month	Firm, Month	Firm, Month
Sample	1,727	1,271	35,582	32,628
		Panel B: Investor Sentiment		
Dependent Variable	$\Delta Sentiment_{t-1,t}$	$\Delta Sentiment_{t-1,t+1}$	$\Delta Sentiment_{t-1,t+1}$	$\Delta Sentiment_{t-1,t+1}$
$\text{Post}_{i,t}$	-0.086	-0.284**	-0.003	0.037**
	(0.064)	(0.113)	(0.014)	(0.017)
$\text{Treat}_{i,t}$	0.038	0.104	0.034**	0.034**
	(0.134)	(0.137)	(0.016)	(0.017)
$(\text{Post}\times\text{Treat})_{i,t}$	0.046	0.038*	-0.054**	-0.062**
	(0.121)	(0.219)	(0.023)	(0.026)
Other Control Variable	Yes		Yes	Yes
Within R-square	0.07	0.18	0.07	0.10
Fixed Effect	Firm, Month	Firm, Month	Firm, Month	Firm, Month
Sample	1,728	1,288	35,894	31,232
		Panel C: Change in Cost of Capital		
Dependent Variable	$\Delta r_{t,t+2}$	$\Delta r_{t,t+2}$	$\Delta r_{t,t+2}$	$\Delta r_{t,t+2}$
$\text{Post}_{i,t}$	-0.000	0.007**	-0.000	-0.000
	(0.013)	(0.003)	(0.000)	(0.000)
$\text{Treat}_{i,t}$	-0.085***	0.008***	-0.001	-0.001
	(0.022)	(0.002)	(0.000)	(0.000)
$(\text{Post}\times\text{Treat})_{i,t}$	0.005	-0.004***	0.001**	0.001*
	(0.016)	(0.003)	(0.000)	(0.001)
Other Control Variable	Yes	Yes	Yes	Yes
Within R-square	0.14	0.21	0.05	0.05
Fixed Effect	Firm, Month	Firm, Month	Firm, Month	Firm, Month
Sample	1,727	1,334	37,001	33,727

Table 6: Insider trading and tournament incentives

The data covers all firm-year observations in Execucomp during 1996-2019. The control variables are $rd_{j,t}$, $sale_{j,t}$, capital-to-sale$_{j,t}$, advertising-to-sale$_{j,t}$, dividend-yield$_{j,t}$, lnage$_{j,t}$ and skt_ret_volatility$_{i,t}$ in all six columns. The regression specification is a shorter version of Kale *et al.* (2009). Appendix 4 defines all variables in the table. In column (1) and (2), I regress Tobin's Q and ROA on all control variables with firm and year fixed effects, respectively. In column (3) to (6), I conduct a 2SLS regression with two first-stage regressions. My endogenous variables are $pay\_gap_{j,t}$ and the interaction term between $pay\_gap_{j,t}$ and my insider trading intensity measure which is all_IT$_{i,t}$. In the first stage regression, I employ the median $pay\_gap_{j,t}$ in the same sales quintiles and the interaction term between the all_IT$_{i,t}$ and $pay\_gap_{j,t}$ as my two IVs in column (3) and (4). In column (5) and (6), I use the sum of the maximum federal and state long-term capital gain tax rates as the IV for all_IT$_{i,t}$, 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, I regress the Tobin's Q and ROA on all control variables with predicted $\widehat{pay\_gap}_{j,t}$, $\widehat{all\_IT}_{j,t}$ and predicted interaction term. I cluster my standard error at firm level and report it in the parentheses. ***, **, and * denote significance at the 99%, 95% and 90% confidence level, respectively. All variables are winsorised at the top 99% and the bottom 1% level.

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

Table 7: 2SLS regression result for purchase and sell transactions

Panel A reports the output of the 2SLS regression. The dependent variable in the first stage regression is $NPED_{it}$, a dummy variable equal to one for the non-promoted directors' buy/sell trades in the tournament year (0,0) and (1,1), zero for years outside the event window and (-2, -1). I exclude transactions in year +2 to remove confounding events and CEO observations and insider transactions conducted by non-competitors. Appendix S4 details the variables. The instrumental variable is the last fiscal year's previous CEO age. I calculate *ret30, mom, bm, numest, illiq* and *size* at the end of last month relates to the insider transaction date that will be used in the second stage of IV regression. Panel B extends the holding period for sequential sells from 1 day after the first transaction to 365 days after the last transaction, using the daily average BHAR_m_365×252, the median number of trading days in a 365-calendar day holding period. Standard errors reported in parentheses are based on robust standard errors clustered at the Firm-Month level. ***, **, and * denote significance at the 99%, 95% and 90% confidence level, respectively. I do not report the coefficients of the control variables $rating_{i,t-1}$, $delta_{i,t-1}$, and $roa_{j,t-1}$; they are all insignificant. All variables are winsorised at the top 99% and the bottom 1%.

	Insider Purchase Transactions		Insider Sell Transactions	
Year *t*	(0,0)	(1,1)	(0,0)	(1,1)
Panel A Second Stage - *Dep Variable is BHAR_m_365, Endogenous Variables are (NPED)$_{i,t}$ and (NPED×CEO_IT)$_{i,t}$*				
$\widehat{NPED}_{i,t}$	0.626*	-0.790	2.911**	-0.793***
	(0.369)	(0.538)	(1.332)	(0.259)
$\widehat{NPED \times CEO\_IT}_{i,t}$			1.794***	0.193**
			(0.695)	(0.079)
$CEO\_IT_{i,t}$	0.069***	0.080***	-0.038	-0.012
	(0.022)	(0.028)	(0.023)	(0.008)
$OutsiderD_{i,j}$	-0.244**	0.032	0.944*	0.367***
	(0.102)	(0.193)	(0.570)	(0.104)
$COOD_{i,j}$	0.017	-0.109	-0.008	0.110***
	(0.032)	(0.083)	(0.012)	(0.042)
$high\_incentiveD_{i,t-1}$	-0.011	0.024	-0.010	0.025***
	(0.028)	(0.053)	(0.012)	(0.004)
$pay\_gap_{j,t-}$	-0.001	-0.011	0.022**	0.003
	(0.022)	(0.030)	(0.010)	(0.003)
$ret30_{j,t,(d-1,d-30)}$	-0.470***	-1.110***	-0.171***	-0.151***
	(0.119)	(0.366)	(0.050)	(0.033)
$mom_{j,t,(d-31,d-364)}$	-0.156***	-0.485***	-0.006	-0.011
	(0.055)	(0.160)	(0.023)	(0.014)
$bm_{j,m-1}$	0.130	-0.146	0.060	0.047**
	(0.089)	(0.219)	(0.042)	(0.023)
$numest_{j,m-1}$	-0.010	-0.015	-0.001	0.002**
	(0.007)	(0.011)	(0.002)	(0.001)
$illiq_{j,m-1}$	0.044	0.112	-0.132**	-0.026
	(0.028)	(0.089)	(0.067)	(0.052)
$size_{j,m-1}$	-0.358***	-0.800***	-0.285***	-0.247***
	(0.060)	(0.186)	(0.025)	(0.012)
$vega_{i,t-1}$(×0.01)	-0.094**	-0.018	0.003	-0.011**
	(0.047)	(0.070)	(0.007)	(0.005)
$rd_{j,t-1}$	-1.459*	-2.839**	-0.323	0.090
	(0.777)	(1.352)	(0.380)	(0.185)
$lncompen_{j,t-1}$	0.070**	0.149**	0.034**	0.053***
	(0.035)	(0.062)	(0.015)	(0.008)
Sample	2,416	2,630	37,554	40,606
Fixed Effect	Firm, Month	Firm, Month	Firm, Month	Firm, Month
Difference in Sargan C (χ^2)	3.31*	2.067	58.08***	26.94***
First-Stage F-$\widehat{NPED}_{I,t}$	27.42***	25.20***	101.78***	508.45***
Anderson-Rubin Wald Test, F statistic	3.68*	2.27	29.93***	11.51***
Panel B: Dissimulation Strategy Results: t+1 after the first and t+365 after the last transaction				
$\widehat{NPED}_{i,t}$	0.623*	-0.428*	-2.945**	-0.979**
	(0.367)	(0.236)	(1.331)	(0.427)
Control Variables	Yes	Yes	Yes	Yes

Table 8: Robustness Test

This table reports three regression outputs for robustness tests. Appendix S4 the variables. Panel A reports the 2nd stage of 2SLS regression results with more control variables to partial out the potential channels that CEO age can affect future firm valuation. Panel B simulates for alternative measures for my dependent variable for different holding horizons, including the BHAR_m_30 and BHAR_m_180, and the 4-factor $\alpha_{t+1,t+30}$ calculated by running regression $r_{i,t} - rf_t = \alpha_{i,t} - \beta_1(r_{crsp,t} - rf_t) + \beta_2 SMB_t + \beta_3 HML_t + \beta_4 UMD_t + \varepsilon_t$ from the day after insider transaction day to 3/6/12 month. 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). I time the daily $\alpha_{t+1,t+30}$ by the median number of trading days of 22, 126, 252 in these three holding periods, respectively. I report the coefficient of $\widehat{\text{NPED}}_{I,t}$ by following the specification in Equation (5). I also report the raw cumulative return $ret_{t+1,t+i}$. For insider purchase sample, I do not include the interaction term $\text{NPED}\widehat{\times\text{CEO\_IT}}_{I,t}$ as it is insignificant. I report the cluster standard errors at the firm-month level parentheses. ***, **, and * denote significance at the 99%, 95% and 90% confidence level, respectively. All variables are winsorised at the top 99% and the bottom 1% level. In Panel C, I report the 1,000 placebo test results. I report the average coefficient of $\widehat{\text{NPED}}_{I,t}$, the standard error of the coefficient of $\widehat{\text{NPED}}_{I,t}$ and its skewness. In column (4) to (6), I report the percentage of my placebo test that has both a positive (negative) coefficient of $\widehat{\text{NPED}}_{I,t}$ for purchase (sell) sample and a first-stage F statistics larger than 10. In Column (7), I report the percentage of sample that has a first-stage F statistics larger than 10. In Panel D, I report the 1,000 placebo test results for the diff-in-diff regression with the average, median, standard deviation and skewness of the coefficient of the interaction term in column (1), (2), (3), (4), respectively. In column (5) to (7), I report the percentage of my placebo test that has a positive (negative) coefficient of the interaction term for purchase (sell) sample and is statistically significant at the 99%. 95% and 90% confidence level, respectively. Relying on a binomial one-sided test-statistic, none of the proportions are statistically different from the corresponding theoretical level in Panel C and D.

Panel A: Extended Set of Control Variables

	(1)	(2)	(3)	(4)
	Insider Purchase		Insider Sell	
	(0,0)	(1,1)	(0,0)	(1,1)
2nd Stage -*Dep Variable is BHAR_m_365, Endogenous Variables are (NPED)$_{i,t}$ and (NPED×CEO_IT)$_i$*				
$\widehat{\text{NPED}}_{i,t}$	1.448**	-7.027	-0.531*	-0.780*
	(0.574)	(7.323)	(0.316)	(0.473)
$\text{NPED}\widehat{\times\text{CEO\_IT}}_{i,t}$			0.324**	0.249**
			(0.146)	(0.119)
$\text{CEO\_IT}_{i,j}$	0.089*	0.148	-0.004	-0.012
	(0.046)	(0.113)	(0.007)	(0.012)
tobin's $Q_{i,t-1}$	-0.074	0.380	0.012	-0.009
	(0.103)	(0.510)	(0.010)	(0.013)
capital-to-sale$_{j,t}$-1	-0.410**	-0.607**	-0.019	-0.056***
	(0.201)	(0.301)	(0.022)	(0.020)
advertising-to-sale$_{j,t}$-1	20.013	-12.008	-0.372	0.129
	(13.213)	(36.062)	(0.616)	(0.838)
dividend-yield$_{j,t}$-1	0.655	1.667	-0.017	0.056
	(4.777)	(10.590)	(0.348)	(0.085)
$\text{lnage}_{j,t}$	-0.424	0.296	0.014	0.050
	(0.370)	(0.700)	(0.023)	(0.034)
$\text{leverage}_{i,t-1}$	-0.694	-0.047	-0.135**	-0.102*
	(0.490)	(1.456)	(0.062)	(0.053)
$\text{skt\_ret\_volatility}_{i,t-1}$	17.409*	16.884	-0.208	-0.848
	(9.555)	(21.345)	(0.643)	(0.694)
$\text{capital\_intensity}_{i,t-1}$	4.162*	-0.745	-0.003	-0.018
	(2.123)	(4.691)	(0.209)	(0.222)
$\text{firm\_focus}_{i,t-1}$	0.268	-1.504	-0.075***	-0.015

	(0.262)	(1.795)	(0.028)	(0.035)
$cash\_flow\_vol_{i,t-1}$	-1.695	-18.148	-0.585	-0.641
	(4.830)	(20.125)	(0.535)	(0.573)
$institution\_ownership_{j,q-1}$	0.648	0.007	-0.024	-0.001
	(0.451)	(0.956)	(0.048)	(0.051)
$independent\_director_{j,t-1}$	-0.880	-0.765	0.092*	0.164***
	(0.574)	(1.457)	(0.054)	(0.060)
$independent\_committee_{j,t-1}$	0.252	0.877	0.200***	0.145***
	(0.234)	(0.723)	(0.038)	(0.042)
$analyst\_talent_{j,t-1}$	0.492	2.288	-0.220***	-0.209***
	(0.690)	(2.652)	(0.052)	(0.050)
$CEO\_tenure_{j,t-1}$	0.116***	-0.291	0.015***	-0.001
	(0.044)	(0.352)	(0.003)	(0.011)
Other Control Variables	Yes	Yes	Yes	Yes
Sample	1,104	1,169	23,872	25,399
Fixed Effect	Firm, Month	Firm, Month	Firm, Month	Firm, Month
First-Stage F-$\widehat{NPED}_{I,t}$	34.31***	1.23	266.55***	34.54***
Anderson-Rubin Wald Test, F statistic	6.13***	5.60***	14.43***	3.19**
Panel B: The coefficient of $\widehat{NPED}_{I,t}$ using Alternative Return Measure				
BHAR_m_30	-0.054	-0.041	-0.236	-0.060
	(0.065)	(0.059)	(0.175)	(0.057)
BHAR_m_180	0.197	-0.079	-2.026**	-0.379**
	(0.213)	(0.145)	(0.881)	(0.171)
$\alpha_{t+1,t+30}(\times 22)$	0.041	-0.147*	-0.293	-0.035
	(0.074)	(0.077)	(0.207)	(0.068)
$\alpha_{t+1,t+180}(\times 126)$	0.066	0.016	-1.812**	-0.124
	(0.165)	(0.135)	(0.763)	(0.157)
$\alpha_{t+1,t+365}(\times 252)$	0.088	-0.045	-1.765*	-0.466**
	(0.214)	(0.160)	(0.923)	(0.208)
$ret_{t+1,t+30}$	-0.116	-0.059	-0.316	-0.079
	(0.096)	(0.083)	(0.218)	(0.069)
$ret_{t+1,t+180}$	0.269	-0.199	-2.929**	-0.374**
	(0.340)	(0.236)	(1.211)	(0.191)
$ret_{t+1,t+365}$	0.903	-0.845	-3.436**	-0.472*
	(0.815)	(0.557)	(1.740)	(0.278)
BHAR_size_30	-0.016	-0.092	-0.335*	-0.072
	(0.082)	(0.075)	(0.201)	(0.059)
BHAR_size_180	0.427	-0.226	-2.104**	-0.415**
	(0.324)	(0.228)	(0.923)	(0.174)
BHAR_size_365	0.952	-0.840	-2.647*	-0.744***
	(0.781)	(0.557)	(1.373)	(0.257)

Panel C: Placebo Test for 2SLS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				% significant coefficient with valid IV significanc first-stage F (>10)			
	Mean	Median	SD	Skewness	1%	5%	10%
$(Post\times Treat)_{i,t}$-Buy	6.007	158.87	28.904	0.00%	0.00%	0.00%	0.40%
$(Post\times Treat)_{i,t}$-Sell	2.174	135.57	11.848	0.20%	0.40%	0.80%	3.4%

Panel D: Placebo Test for Diff-in-Diff regression

				% significant positive (negative) for buy (sell)			
	Mean	Median	SD	Skewness	1%	5%	10%
$(Post\times Treat)_{i,t}$-Buy	-0.049	-0.038	0.218	-0.328	0.70	3.2%	5.8%
$(Post\times Treat)_{i,t}$-Sell	0.132	0.123	0.126	0.428	0.60	1.00	1.40

Tournament incentives and insider trading

Supporting Information

Additional Supporting Information may be found in the online version of this article at the publisher's website

- Appendix S1: Data cleaning process details
- Appendix S2: Sample size across different database
- Appendix S3: CEO Turnover Summary
- Appendix S4: Definition of Variables
- Appendix S5: Test on Parallel Trend Assumption
- Appendix S6: Insider trading and price informativeness around the CEO turnover
- Appendix S7: 2SLS regression result for matching sample
- Appendix S8: Insider trading and the probability of becoming CEO

Appendix S1: Data Cleaning Process Details

Walker (2009) and Coles, Daniel and Naveen (2014) point out that Execucomp's total compensation figure is not comparable before and after 2006 because of the passage of Financial Accounting Standards Board (FASB) 123R revision to the stock and options accounting and an expanded compensation disclosure requirement regarding the director compensation disclosure. I follow Coles *et al.* (2014), Kini and Williams (2012) and Brockman, Lee and Salas (2016) to correct my pre- and post-2006 total compensation item *tdc1*[26]. Specifically, the stock option was valued using the Black-Scholes formula for the pre-2006 period but reported its fair value for the post-2006 period. A small number of firms still report their proxy statements in the old reporting format in 2006, I use the reporting flag to identify (*old_datafmt_flag*) these firms. Then, I correct the post-2006 period option value using the same set of Black-Scholes assumption that Execucomp used for the pre-2006 period. The Black-Scholes assumption used are listed as follows:

1. Strike price per share is specified in its proxy statement. (*expric*)
2. Market price per share is assumed to be equal to the strike price per share unless specified in its proxy statement. (*mktprice*)
3. Option grant terms: Options are assumed to be granted on July 1st of the particular year for which data were reported. The option's nominal term is the period between July 1st of the year of grant and the expiration date (*exdate*) reported in its proxy statement. The nominal term is further rounded to the nearest year figure. However, the option's term was reduced to 70% of its nominal term as directors rarely hold its stock option until its expiration year. The expiration date is not available on Execucomp for post-2006 reporting format. Therefore, I follow Kini and Williams (2012) to assume all options have seven years until expiration.
4. Risk-free rate corresponding to the option's maturity is the historical annual series of treasury constant maturity with 7-year term downloaded from the Federal Reserve website[27].
5. Stock price volatility: Individual stock price volatility is the annualised volatility calculated using the last 60 months. The stock volatility of all companies is winsorised at the top and bottom 5%. To calculate the volatility, Execucomp requires at least 12-month return data. For stocks that are traded less than 12 months, Execucomp the average volatility value for the firms in the S&P 1500 index.
6. Future dividend yield. Execucomp uses the average dividend yield in the last three years to calculate the estimated future dividend yield. It is then winsorised at the top and bottom 5%.

[26] My results remain robust if I do not correct for the FSBA change and use raw figures reported by Execucomp.
[27] https://www.federalreserve.gov/datadownload/Choose.aspx?rel=H15

Using these assumptions, I replicate the Black-Sholes option value for 2005, and the correlation between my Black-Sholes value and the Black-Sholes value calculated by Execucomp is 95.9%[28]. I further recalculate all option awards for both pre- and post-2006 period by using the same set of Black-Sholes assumptions to ensure consistency. Secondly, I follow Brockman *et al.* (2016) to value the ex-ante value of stock awards. I multiply the number of performance shares granted to the CEO (*shrtarg*) by the firm's fiscal year-end stock price (Compustat *prcc_f*). Finally, I recalculate the *tdc1* for all firm-year observations that reported in the pre-2006 old format (item *old_datafmt_flag=1*) by summing salary (*salary*), to bonus (*bonous*), other annual compensation (*othann*), restricted stock grant (*rstkgrnt*), all other total (*allothtot*), the fair value of stock awards (*shrtarg*×*prcc_f*) and Black-Scholes value of option grant (*option_awards_blk_value*). For *tdc1* reported in post-2006 new format (item *old_datafmt_flag=0*), I sum salary (*salary*), bonus (*bonous*), non-equity incentive plan compensation (*noneq_incent*), fair value of stock awards (*stock_awards_fv*), all other compensations (*othcomp*), deferred earnings (*defer_rpt_as_comp_tot*) and Black-Scholes value of option grant.

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

To identify short horizon seller, I modify the investment horizon measure proposed by Akbas, *et al.* (2020). Firstly, I define HOR as:

$$HOR_{i,j,t} = \frac{\sum_{Year-8}^{Year-1} NPV_t}{N}$$

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

[28] Kini and Williams (2012) report a correlation of 96.8% for 2005. The difference is possibly due to different risk-free rate sources, which they do not report.

I estimate the probability of becoming CEO from a cross-section regression. In each year *t*, I obtain a list of firms that have a CEO turnover event and keep all insiders except the former founders/co-founders, former CEOs and new joiners. I then estimate the following cross-section regression:

$$\begin{aligned}\text{CEOD}_{\text{i,j,t}} = \alpha + \beta_0 lncompen_{i,t\text{-}1} + \beta_1 age_{i,t\text{-}1} + \beta_2 tenure_{i,j,t\text{-}1} + \beta_3 exp_{i,t\text{-}1} + \beta_4 maleD_{\text{i}} + \beta_5 COO_{i,j,t\text{-}1} \\ + \beta_6 \text{COO\_firm}_{j,t\text{-}1} + \beta_7 bm_{j,t\text{-}1} + \beta_8 momentum_{j,t\text{-}1} + \beta_9 roa_{j,t\text{-}1} + \beta_{10} outsider_{j,t} + e_t\end{aligned}$$

The dependent variable is a dummy variable one for the director who became CEO in the year, and zero for insiders who failed to become CEO. $lncompen_{j,t\text{-}1}$ is the adjusted total compensation in the year before, $tenure_{i,j,t\text{-}1}$ is the number of year the director worked for the firm. $exp_{i,t\text{-}1}$ is the number of year the director has worked for any firm in the entire Execucomp. $maleD_{\text{i}}$ is a dummy variable equal to one for male, and zero for female $COO_{i,j,t\text{-}1}$ is a dummy variable equal to one for COO as identified using director's title, and zero otherwise. $COO\_firm_{j,t\text{-}1}$ is a dummy variable equal to one if the firm had a COO before the turnover, and zero otherwise. Other variables are as defined before. $outsider_{j,t}$ is a dummy equal to one if the firm hired an external CEO in year *t*, and zero otherwise. I use the estimated coefficient to calculate the estimated probability $Probability_{i,t}$ of a given insider i in the same year t to become the CEO. I re-estimate the cross-section every year using only the firm that had a CEO turnover in the year.

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

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

where $X_{i,t}$ is the basic annual earnings per share excluding extraordinary items (*epspx*), adjusted for stock splits and stock dividends and deflated by the stock price at the beginning of the fiscal year *t*. $R_{i,t}$ is the firm's annual return beginning at the fiscal year *t* and $R_{i,t+3}$ is a three-year future return for the firm from fiscal year *t+1* to *t+3*. The coefficient of the sum of the future three-year earnings per shares β_3 is the FERC. I truncate all variables at the top and bottom 1%. A higher β_3 means the current stock return impounds more future earnings information and is more informative for future earnings and *vice versa*. I follow Wang (2019) to estimate a rolling panel regression using the trailing 36 months across each two-digit SIC industry. I restrict that there are at least 8 (24) months in $R_{i,t}$ ($R_{i,t+3}$) for a stock to be included in the regression and create binary variable FERC that is one for the top quintile of the β_3 and zero otherwise.

I use the stock return synchronicity used by Piotroski and Roulstone (2004) estimated from the following equation:

$$\text{FirmRET}_{\text{i,t}} = \alpha + \beta_1 \text{MktRET}_{\text{j,t}} + \beta_2 \text{MktRET}_{\text{j,t}-1} + \beta_3 \text{IndRET}_{\text{k,t}} + \beta_4 \text{IndRET}_{\text{k,t}-1} + \varepsilon_{\text{i,t}}$$

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

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

$$\ln(\text{market\_value})_{i,t}=\alpha+\beta_{1j,t}\ln(\text{book\_value})_{i,t}+\beta_{2j,t}\ln(\text{net\_income})^{+}_{i,t}+\beta_{3j,t}I_{(<0>)}\ln(\text{net\_income})^{+}_{i,t}+\beta_{4j,t}\text{leverage}_{i,t}+\varepsilon_i$$

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

To measure the change of cost of capital, I estimate the following modified Fama and French (1993) three-factor model by following Cziraki, *et al.* (2021)

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

where $r_{i,t}$ is the monthly stock return, $r_{f,t}$is the return on 1-month U.S Treasury bill, $r_{m,t}$ is the CRSP value-weight market index, SMB_t and HML_t are the returns on the size and book-to-market ratio portfolios. D_t is a dummy variable that equals one if the year is in $(0,1)$, and zero for years in $(-3,-1)$. I use years $(-3,2)$ to estimate the cost of capital prior and after the CEO turnover. The expected change of cost of capital is obtained using the estimated coefficient of $\hat{\alpha}_{\Delta i}$ plus the product between $\hat{b}_{\Delta i}$, $\hat{s}_{\Delta i}$, $\hat{h}_{\Delta i}$ and the corresponding average factor premium estimated using all firms in CRSP database between 1993 and 2019[29].

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

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

Appendix S2: Sample size across different database

	Unique *execid*	Unique *personid*	Sample Size
Raw Execucomp Sample	48,429		269,456
Match with *execid-personid* link table	43,952	44,187	277,113
Match with CRSP both insider purchase and sale, including CEO	26,570	26,617	257,033
Match with CRSP both insider purchase and sale, excluding CEO	24,275	24,310	188,960
Remove new joiner, previous CEO, co-founders/founders	21,723	21,764	165,705
Valid insider purchase sample for Non-Promoted Director in (0,0)	536	537	860
Valid insider purchase sample for Non-Promoted Director in (0,1)	844	845	1,492
Valid insider sell sample for Non-Promoted Director in (0,0)	3,107	3,110	7,935
Valid insider sell sample for Non-Promoted Director in (0,1)	4,527	4,532	15,443

Appendix S3: CEO Turnover Summary

The table shows a summary of CEO turnover event, insider transactions in different fiscal years. I use Execucomp historical annual CEO flag (*ceoann*) to identify CEO turnover events. In column (2), I report the number of internal promotions after removing the confounding events. I define an external CEO promotion if the incoming CEO has not worked for the firm within the event window of (-5, -2). In column (4), I report the number of CEO Turnover after removing confounding events. In column (5) to (8), I exclude all CEO transactions and transactions occurred in the confounding events. In column (7) and (8), I report the yearly average insider transaction value. I aggregate insider purchase and sell transactions at the daily frequency by using the closing price at the transaction day times the number of shares bought/sold to compute the individual transaction value.

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

Appendix S4: Definition of Variables

Variable Notation	Data Source	Definition
$BHAR\_m\_365_{(d+1,\ d+365)}$	CRSP	365-calendar day Buy-N-Hold return adjusted by using the CRSP value-weighted market index. Defined as the following: $BHAR_{m_n} = \prod_{t=1}^{d}[1 + R_{jt}] - \prod_{t=1}^{d}[1 + R_{mt}]$
$NPV_{i,d}$	Smart Insider	Net purchasing value for insider transactions in day *t* executed by insider *i*, calculated as the ratio of the net dollar amount of insider transactions over the total dollar amount of insider transactions. If *NPV_i* is greater (less) than 0, I recognise that the insider *i* is net buying (selling) on a given day *d*.
$opp\_D_{i,t}$	Smart Insider	Dummy variable equal to one for opportunistic insider transactions, and zero otherwise. I identify opportunistic transactions by following Cohen *et al.* (2012), that is the transaction executed by insiders who had made at least one transaction in the same calendar year in the past three consecutive years. Other insiders are routine traders. I reclassify each insider at the beginning of each calendar year.
$NPED_{i,t}$	Execucomp	Dummy variable equals one for insider purchase or sell transactions executed by non-promoted director in the event year *t* zero for years other than *t*. *t* takes the value of 0, 1 in the study.
$pay\_gap_{j,t-1}$	Execucomp	The natural logarithm of the difference between the CEO total compensation (*tdc1*) and the median total compensation of other non-CEO directors covered by Execucomp in firm *j* in the last fiscal year. *tdc1* is adjusted by following Coles *et al.* (2014) and Brockman *et al.* (2016).
$lncompen_{j,t-1}$	Execucomp	The natural logarithm of *tdc1* adjusted by following Coles *et al.* (2014) and Brockman *et al.* (2016).
$rating_{j,\ t-1}$	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.
$high\_incentiveD_{i,t-1}$	Execucomp	A dummy variable that is equal to one for high incentive directors, and zero otherwise. High incentive directors are defined as those directors *i* whose compensation differences between their CEOs and themselves are the largest three in firm *j* in year *t-1*.
$Pay\_rank_{i,t-i}$	Execucomp	The rank of non-promoted director sorted by their total compensation in year -1 among all tournament competitors in the same firm.
$mom_{j,(d-31,d\ -364)}$	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.
$ret30_{j,(d-1,d-30)}$	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.
$bm_{j,m-1}$	CRSP, Compustat	The book-to-market ratio calculated as the ratio of last fiscal year's book value over the market capitalisation in the last trading day in December. Book value is

		computed as the following. Book value is equal to stockholder equity + deferred taxes and investment tax credit (Compustat: txditc, zero if missing) −preferred stock value. Stockholder equity is parent stockholder equity (Compustat: seq), or total common equity (Compustat: ceq) plus total preferred stock capital (Compustat: pstk) or the difference between the total asset (Compustat: at) and total liability (Compustat: lt), in that order, as available. Preferred stock value is the preferred stock redemption value (Compustat: pstkrv), or preferred stock liquidation value (Compustat: pstkl), or total preferred stock capital (Compustat: pstk), or zero, in that order as available. Negative bm ratio is restricted to zero. The ratio is calculated for firm *j* at the end of the last month.
$leverage_{i,t}$	Compustat	Long term debt plus debt in current liability) over the total assets $\frac{(dltt + dlc)}{at}$
$illiq_{j,m-1}$	CRSP	Amihud's (2002) measure of illiquidity for firm *j* at the end of the last month. The measure is calculated as the monthly average of the daily ratio of absolute stock return to dollar volume.
$size_{j,m-1}$	CRSP	The logarithm of market capitalisation 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.
$roa_{j,t-1}$	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.
$age\_ceo_{j,\ t-1}$	Execucomp	In the fiscal year *t-1*, *I* identify the former CEO of firm *j*. The variable is her age in year *t-1*. If Execucomp does not report the age of director in a given year, I use the age of the same director in other years to complete the age of the director in the year.
$numest_{j,m-1}$	I/B/E/S	Analyst coverage is 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. If there is no earning forecast, the analyst coverage is set to be zero.
$rd_{j,t-1}$	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. If Compustat reports missing research and development expense, it is set to be zero.
$delta_{i,t-1}$	Execucomp	Dollar change in wealth associated with a 1% change in the firm's stock price (in $000) for director *i*. Calculated according to Coles *et al.* (2013).
$vega_{i,t-1}$	Execucomp	Dollar change in wealth associated with a 0.01 change in the standard deviation of the firm's returns (in $000) for director *i*. Calculated according to Coles *et al.* (2013).

$OutsiderD_{i,j}$	Execucomp	If the new CEO had not been working in the company in the (-5,-2) of the CEO turnover window, the CEO is defined as outsiders. The dummy takes the value of one for insider transactions for firms with outside CEO appointment during the year $(0,1)$, and zero otherwise.
$COOD_{i,j}$	Execucomp	If the firms had a COO and the COO is younger than the current CEO before the CEO tournament, the firm is defined as COO firm. The dummy takes the value of one for non-promoted insider transactions for COO firms during the year $(0,1)$, and zero otherwise. I define COO is the director who is younger than the incumbent CEO and whose job title (*titleann*) 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*
$CEO\_IT_{j,t}$	Execucomp, Smart Insider	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.
$lnage_{j,t}$	Execucomp	The natural logarithm of the current age of the director *i* in year *t*.
total $asset_{j,t-1}$	Compustat	Logarithm of the total asset (Compustat: at) in the last fiscal year. The variable is only used to conduct the matching only.
$FERC_{j,t}$	CRSP, Compustat	It is a dummy variable equal to one for firms in the top quantile of future earnings response coefficient calculated accorindg to Tucker and Zarowin (2006), and zero for other firms.
$Synch_{j,t}$	CRSP	It is a dummy variable equal to one for firms in the top quantile of return synchronicity calculated accorindg to Piotroski and Roulstone (2004), and zero for other firms.
tobin's $Q_{i,t-1}$	Compustat	Market value of equity plus book value of debt-deferred tax over book value of total assets. $$\frac{(at + csho \times prcc\_f - ceq - txdb)}{at}$$
capital-to-$sale_{j,t-1}$	Compustat	Net fixed asset (Compustat: ppent) to sales (Compustat: sale).
advertising-to-$sale_{j,t-1}$	Compustat	Advertising expenditure (Compustat: xad) to sales (Compustat: sale). It is assumed to be zero if firms do not report advertising expenditure.
dividend-$yield_{j,t-1}$	Compustat	The dividends per share by ex-date divided (Compustat: dvpsx_f) by the close price for the fiscal year (Compustat: prcc_f).
$all\_IT_{j,t}$	Smart Insider	The total number of non-CEO insider transaction for firm *j* in year *t*. If there is no non-CEO insider transaction in year *t*, the number is set to be 0.
$sale_{j,t-1}$	Compustat	The natural logarithm of the sale (Compustat: sale).

$skt\_ret\_volatility_{i,t-1}$	CRSP	Variance of 60 monthly returns preceding the sample year *t-1*
$capital\_intensity_{i,t-1}$	Compustat	Capital expenditure (Compustat: capx) over total asset (Compustat: sale)
$firm\_focus_{i,t-1}$	Compustat-Segment	Firm focus is computed as the segment sales-based Herfindahl index. I use Compustat segment file to identify a firm's segment sales according to their four-digit SIC code. Firm focus is equal to one if the firm operates only in one segment and decreases as the firm diversifies. (Kini and Williams, 2012)
$cash\_flow\_vol_{i,t-1}$	Compustat-Quarterly	It is the seasonally adjusted standard deviation of cash flows over assets for a five-year window (*t, t+4*). I require there are at least a three-year data to compute this variable. Quarterly cash flows over assets is defined as the EBITD (Compustat: saleq- cogsq-xsgaq) over total asset (Compustat: atq). For each of the four quarters in the year, I compute the mean values across the five-year window and then subtract these quarterly mean values to obtain the seasonally adjusted cash flows. I then compute the standard deviation of these adjusted cash flows over assets over the period *t* to *t+4.* (Kini and Williams, 2012)
$institution\_ownership_{j,q-1}$	Thomson Reuter 13F Holding	Percentage of shares owned by institution investors over total shares outstanding in the last quarter.
$independent\_director_{j,t-1}$	Boardex	Percentage of independent directors on the company board.
$independent\_committee_{j,t-1}$	Boardex	Percentage of independent directors on the company compensation committee.
$analyst\_talent_{j,t-1}$	I/B/E/S	The average talent of financial analysts that cover firm *j* in the last fiscal year. It is the innate ability of sell-side analysts measured by the analyst fixed effect from the regression on analysts' forecast accuracy. Calculated according to Dang *et al.*, (2021)
$\alpha_{t+1,t+i}$	CRSP, French Data Library	The intercept calculated by running regression $r_{i,t} - rf_t = \alpha_{i,t} - \beta_1(r_{crsp,t} - rf_t) + \beta_2 SMB_t + \beta_3 HML_t + \beta_4 UMD_t + \varepsilon_t$ from the day after insider transaction day to 30/180/365 calendar day. 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).
$CEO\_tenure_{j,t-1}$	Execucomp	Computed as the difference between year *t* and the year the director became CEO (Execucomp:*becameceo*). If the *becameceo* is missing, it is the number of yearly observations the director has become CEO.

Appendix S5: Test on Parallel Trend Assumption

I follow Angrist and Pischke (2009) and Cengiz *et al.* (2019) to conduct an event-study type diff-in-diff regression and formally test on the parallel trend assumption. Variable Pre_t equal to 1 for treated firms in year *t*, and zero otherwise. Year *t* refers to the year in my event window with year *0* as the CEO turnover occurred. Variable $Post_t$ is defined with the same logic. The coefficients of Pre_{-1} should be statistically insignificant for the parallel trend assumption to hold. Pre_{-2} is omitted to avoid perfect multicollinearity. Column (1) and (2) focuses on insider purchase and sell transactions, respectively. I control for firm, year, and event fixed effects. Standard errors are clustered at the firm-month level. ***, **, and * denote significance at the 99%, 95% and 90% confidence level, respectively. All variables are winsorised at the top 99% and the bottom 1% level.

	Purchase Transactions	**Sell Transactions**
	(1)	(2)
	BHAR_m_365	BHAR_m_365
Pre_{-1}	0.108	-0.030
	(0.080)	(0.019)
$Post_0$	0.211*	-0.061**
	(0.119)	(0.026)
$Post_1$	0.079	-0.082***
	(0.151)	(0.032)
$CEO\_IT_{i,t}$	0.031	0.010**
	(0.025)	(0.004)
$OutsiderD_{i,j}$	0.138	0.053*
	(0.125)	(0.032)
$COOD_{i,j}$	-0.169	0.055*
	(0.115)	(0.031)
$high\_incentiveD_{i,t-1}$	0.027	-0.001
	(0.021)	(0.005)
$pay\_gap_{j,t-1}$	-0.024	-0.006
	(0.016)	(0.005)
$ret30_{j,t,(d-1,d-30)}$	-0.418***	-0.249***
	(0.102)	(0.033)
$mom_{j,\ t,(d-31,d\ -364)}$	-0.058	0.031
	(0.059)	(0.022)
$bm_{j,m-1}$	-0.064	-0.028
	(0.056)	(0.045)
$numest_{j,m-1}$	-0.012	-0.003
	(0.011)	(0.003)
$illiq_{j,m-1}$	0.065**	0.291*
	(0.028)	(0.153)
$size_{j,m-1}$	-0.732***	-0.669***
	(0.097)	(0.037)
$roa_{j,t-1}$	-0.415	0.366**
	(0.425)	(0.171)
$delta_{i,t-1}(\times 0.01)$	0.129**	0.001
	(0.063)	(0.001)
$vega_{i,t-1}(\times 0.01)$	-0.230**	-0.015**
	(0.111)	(0.008)
$rd_{j,t-1}$	0.910	0.386
	(0.821)	(0.282)
$lncompen_{j,t-1}$	0.057**	0.024**
	(0.025)	(0.010)
$rating_{i,t-1}$	0.636	-0.345
	(1.120)	(0.217)
Constant	3.700**	5.854***
	(1.689)	(0.449)
Sample	2,309	47,094
Within R^2	0.38	0.30

Appendix S6: Insider trading and price informativeness around the CEO turnover

This table reports the fixed effects regression output based on the matched sample. Firms that have CEO turnover event in year *t* are matched with firms on the average insider purchase/sell profitability, logarithm of the total asset and the book-to-market ratio in the fiscal year *t-1*. The distance is calculated by using Mahalanobis distance. Each treated firm is matched with one control firm. I restrict that the control firm sample does not have any CEO turnover in (-2, 2). In Panel A, the moderator variable is the future earnings response coefficient (FERC) calculated according to Tucker and Zarowin (2006) and the $NPED_{i,t}$. $FERC_{i,t}$, is a dummy variable equal to one for firms in the top quantile of $FERC_{I,t}$ in year *t*, and zero otherwise. In Panel B, the moderator variable is the return synchronicity (Synch) calculated according to Piotroski and Roulstone (2004). $Synch_{i,t}$ is a dummy variable equal to one for firms in the top quantile of $Synch_{I,t}$ in year *t* in the same two-dig sic industry, and zero otherwise. Appendix S4 defines all variables in the table. For years in the post turnover period other than year *t*, it is not included in these regressions. I state the year t at the top of the table. I include the same set of control variables as 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.

	Insider Purchase		**Insider Sell**	
Panel A: Future Earnings Response Coefficient				
	(1)	(2)	(3)	(4)
Year *t*	(0,0)	(1,1)	(0,0)	(1,1)
Dependent Variable	BHAR_m_365	BHAR_m_365	BHAR_m_365	BHAR_m_365
$Post_{i,t}$	0.125	0.037	0.016	0.037***
	(0.055)	(0.085)	(0.011)	(0.013)
$Treat_{i,t}$	-0.337***	-0.196	0.002	-0.002
	(0.113)	(0.121)	(0.012)	(0.012)
$(Treat \times Post)_{i,t}$	0.163*	0.196	-0.036**	-0.034*
	(0.095)	(0.124)	(0.018)	(0.020)
$FERC_{i,t}$	-0.117	0.057	-0.029	-0.013
	(0.115)	(0.112)	(0.020)	(0.023)
$(Post \times Treat \times FERC)_{i,t}$	-0.011	-0.095	0.099***	0.029
	(0.186)	(0.179)	(0.036)	(0.047)
Other control variable and main levels	Yes	Yes	Yes	Yes
Sample	1,400	1,079	30,879	28,415
Panel B: Return Synchronicity				
$Post_{i,t}$	0.005	0.119	0.014*	0.031**
	(0.069)	(0.126)	(0.011)	(0.013)
$Treat_{i,t}$	-0.311***	-0.215*	0.016	0.012
	(0.114)	(0.116)	(0.013)	(0.012)
$(Treat \times Post)_{i,t}$	0.234**	0.011	-0.031**	-0.038**

	(0.103)	(0.170)	(0.019)	(0.019)
$Synch_{i,t}$	0.040	0.001	0.021	-0.013
	(0.084)	(0.080)	(0.013)	(0.017)
$(Post \times Treat \times Synch)_{i,t}$	-0.142	0.222	0.028	0.014
	(0.136)	(0.191)	(0.033)	(0.040)
Other control variable and main levels	Yes	Yes	Yes	Yes
Sample	1,828	1,323	31,131	28,542

Appendix S7: 2SLS regression result for matching sample

The table reports the regression output of 2SLS regression on sample obtained by nearest neighbour matching. The dependent variable in the first stage of the regression is $NPED_{,I,t}$. It is dummy variable that equals to one for the purchase/sell transactions of promotion rejectees in $(0,0)$ and $(1,1)$ with year 0 as the CEO turnover event depending on the year *t*. $NPED_{,I,t}$ is equal to zero for years outside the event window and $(-2,-1)$. For years in the post turnover period other than year *t*, it is not included in the regression. I state the year t at the top of the table. In all columns, the sample is obtained by the nearest neighbour matching. Firms that have CEO turnover event in year *t* are matched with firms on the average insider purchase/sell profitability, logarithm of the total asset and the book-to-market ratio in the fiscal year *t-1*. The distance is calculated by using Mahalanobis distance. Each treated firm is matched with one control firm. I restrict that the control firm sample does not have any CEO turnover in (-2, 2). My instrumental variable is the previous CEO's age in the last fiscal year. I include the same set of control variables as 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.

	Insider Sell	
	(1)	(2)
	First Stage	
Year *t*	(0,0)	(1,1)
Dependent Variable	$NPED_{i,t}$	$NPED_{i,t}$
$age\_ceo_{i,t-1}$	0.019^{***}	-0.023^{***}
	(0.002)	(0.002)
Control Variable	Yes	Yes
	Second Stage	
Dependent Variable	BHAR_m_365	BHAR_m_365
Endogenous Variable		
$\widehat{NPED}_t$	-0.543^{*}	-1.132^{**}
	(0.309)	(0.467)
$\widehat{NPED \times CEO\_IT}_{i,t}$	0.564^{***}	0.331^{**}
	(0.200)	(0.157)
Control Variables		
$CEO\_IT_{i,t}$	0.004	-0.024
	(0.011)	(0.016)
Other Control Variable	Yes	Yes
Sample	18,368	18,831
Fixed Effect	Firm, Month	Firm, Month
Difference in Sargan *C* (χ^2)	37.23^{***}	18.35^{***}
First-Stage F-$\widehat{NPED}_{l,t}$	163.75^{***}	225.09^{***}
Anderson-Rubin Wald Test, F-Statistics	20.82^{***}	8.71^{***}

Appendix S8: Insider trading and the probability of becoming CEO

This table reports linear probability models estimating the likelihood of a director *i* becomes CEO in year *t*. The dependent variable equals one for CEO, and zero otherwise. Regressions are estimated using all tournament competitors defined previously and for CEO turnover year *t* only. Sample is at director-firm level. Variables $no\_buy_{I,t-1}$ and $no\_sell_{I,t-1}$ represent the number of insider purchase and sell transactions made by insiders *i* in year *t-1*. $Age_{I,t-1}$,$tenure_{I,,t-1}$ represents the age and tenure of insiders *i* in year *t-1*, respectively. $COOD_{I,t-1}$ is a dummy variable equals to one if the director *i* is chief operating officer or president in year *t-1*, and otherwise zero. All other variables are defined in Appendix S4 and winsorised at the 1% level. I include firm and year fixed effects; standard errors are clustered by firm and reported within brackets below the corresponding coefficient estimate. ***, **, and * denote significance at the 99%, 95% and 90% confidence level, respectively.

	$CEOD_{i,t}$	$CEOD_{i,t}$
$age_{i,t-1}$	-0.005**	-0.004**
	(0.002)	(0.002)
$tenure_{i,,t-1}$	0.006*	0.006*
	(0.003)	(0.004)
$COOD_{i,t-1}$	0.435***	0.434***
	(0.032)	(0.032)
$no\_buy_{i,t-1}$	0.044	0.041
	(0.027)	(0.028)
$no\_sell_{i,t-1}$	-0.006	-0.005
	(0.004)	(0.005)
$no\_buy_{i,t-2}$		0.009
		(0.033)
$no\_sell_{i,t-2}$		-0.000
		(0.006)
$delta_{i,t-1}$(×0.01)	0.012**	0.012**
	(0.006)	(0.006)
$vega_{i,t-1}$(×0.01)	0.062**	0.061**
	(0.031)	(0.031)
$lncompen_{i,t-1}$	0.000***	0.000***
	(0.000)	(0.000)
$ret30_{j,t-1,(d-1,d-30)}$	0.522***	0.525***
	(0.167)	(0.167)
$mom_{j,\ t-1,(d-31,d\ -364)}$	0.036	0.036
	(0.054)	(0.054)
$bm_{j,t-1}$	0.132*	0.131*
	(0.075)	(0.075)
$illiq_{j,t-1}$	0.038	0.040
	(0.076)	(0.076)
total $asset_{j,t-1}$	-0.118***	-0.118**
	(0.046)	(0.046)
$roa_{j,t-1}$	-0.113	-0.113
	(0.213)	(0.212)
tobin's $Q_{j,t-1}$	0.017	0.017
	(0.020)	(0.021)
$leverage_{j,t-1}$	0.059	0.057
	(0.130)	(0.130)
Constant	0.880**	0.880**
	(0.401)	(0.404)
Sample	1,364	1,364
Fixed Effect	Firm, Year	Firm, Year
Within R^2	0.45	0.45