Get the Money Somehow: The Effect of Missing Performance Goals on Insider Trading

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Abstract:

This paper uses a regression discontinuity design to identify the effect of compensation shocks on insider trading. I find that CEOs who narrowly miss relative performance goals and hence suffer a loss in compensation subsequently earn higher abnormal profits from their insider trades than otherwise similar CEOs who narrowly beat such goals. CEOs who narrowly miss relative performance goals also become less likely to provide earnings and sales guidance. These results suggest that managers can use insider trading to make up for compensation losses due to missing relative performance goals, which could reduce the incentive effect of performance-based pay.

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I. Introduction

Tying executive pay to corporate performance has become increasingly important in creating incentives for corporate managers.[1] Theoretical models argue that compensation contracts that reward managers contingent on performance, especially performance relative to peer firms, can induce managerial effort (e.g., Holmstrom (1979), Holmstrom (1982)). However, since effort exertion is privately costly, self-interested managers may take actions to "undo" the incentive effect of performance-based pay (e.g., Murphy and Jensen (2011); Edmans, Gabaix, and Jenter (2017)). In this paper, I investigate one such action, i.e., insider trading.

Theories suggest that insider trading may be a suboptimal way to compensate management. For instance, Fischer (1992) argues that allowing insider trading can exacerbate existing agency problems, because it expands managers' unobservable strategy space. In the context of performance-based pay, the ability of managers to earn abnormal profits from insider trading ex post after observing their performance may weaken their ex ante incentive to improve performance. In other words, if managers are able to recoup much of the loss in compensation due to missing performance goals through abnormal profits from insider trading, their ex ante incentive to beat performance goals may be limited.

In this paper, I use a regression discontinuity design (RDD) to examine whether managers use insider trading to undo some of the incentive effects of performance-based pay. I exploit the discontinuity in compensation induced by missing relative performance goals in executive

[1] For example, based on data from Incentive Lab, the proportion of the largest firms in the U.S. market (roughly 1,400 firms each year) that have explicit performance-based incentives has increased from 81.9% in 2006 to 96.1% in 2016, and that with explicit relative performance incentives has increased from 19.3% to 50.5% over the same period.

incentive contracts. Managers whose performance is right around a relative performance goal presumably have strong incentives to improve performance because of the discontinuous jump in compensation arising from beating the goal. Yet, if managers are able to generate higher abnormal profits from insider trading when they miss the performance goal, they can make up for the loss in compensation and hence mitigate the incentive effect.[2] I hypothesize that missing a relative performance goal prompts managers to earn higher abnormal profits from insider trading to make up for the loss in performance-based compensation.

This hypothesis is grounded in the notions of reference points and loss aversion, which have been extensively validated by empirical studies in different settings.[3] Building on the common theme in these papers that economic agents base their decisions relative to an expected

[2]Prior studies using disclosed insider trades show that insiders earn abnormal returns on their trades (e.g., Jaffe (1974); Finnerty (1976); Seyhun (1986); Rozeff and Zaman (1988); Lakonishok and Lee (2001); Cohen, Malloy, and Pomorski (2012); Ali and Hirshleifer (2017)), suggesting that some of the trades are motivated by insiders' informational advantage over outside investors. Moreover, despite various trading restrictions, some insiders are able to trade strategically on their informational advantage. For example, Jagolinzer, Larcker, and Taylor (2011) find that restricted trade windows do not appear binding and that insider trades made within these windows are generally informed. Ali and Hirshleifer (2017) show that firms with blackout periods in place often allow trading during such periods on a by-request basis. Jagolinzer (2009) shows that insiders who trade through SEC rule 10b5-1 plans generate higher abnormal returns than those who do not participate in 10b5-1 plans within the same firm, suggesting that trades by 10b5-1 participants are not solely based on diversification purposes.

[3]For example, Coval and Shumway (2005) show that professional traders that lose money in the morning take on more risk in the afternoon. Rizzo and Zeckhauser (2003) find that physicians whose income falls short of their reference points take unappealing actions to boost their income. Camerer, Babcock, Loewenstein, and Thaler (1997) find that taxi drivers work longer hours on low-wage days and quit early on high-wage days, suggesting that they are averse to falling below a target income. Van der Stede, Wu, and Wu (2020) show that when workers' base salaries are

level of compensation, I posit that CEOs who narrowly miss their relative performance goals are more likely to perceive the compensation associated with meeting these goals as a reference point and exhibit loss aversion. As a result, these CEOs may derive a higher marginal utility from each additional dollar of insider trading profits than otherwise similar CEOs who narrowly beat their goals. I detail the development of this hypothesis in Section II of the paper.

Using a sample of relative performance grants for which the payout structure exhibits jumps around performance goals, I first show that there is no bunching on either side of the performance goals and the density function is smooth around the goals. This pattern is in contrast to that for grants based on absolute performance goals, which are subject to manipulation by managers (Bennett, Bettis, Gopalan, and Milbourn (2017)). Because relative performance goals are based on peer companies' performance, which is not observed until the performance period ends, it is difficult for managers to perfectly control whether their performance is above or below a relative goal in a narrow range around the goal. I also show that firms that just beat and those that just miss a relative performance goal have balanced observable covariates. These results allow me to use missing relative performance goals right around the cutoff to identify the effect of compensation shocks on insider trading.

The main results of the paper can be summarized as follows. First, relative to managers that narrowly beat a relative performance goal, those that narrowly miss one suffer a loss of about 12% to 13% of their total compensation. This result suggests that missing relative performance goals has a large negative effect on CEO compensation. Importantly, I find no evidence of ex post "settling up": boards do not appear to subsequently use discretionary bonuses or other

used as a reference point, penalties result in stronger disutility and incentives than bonuses, consistent with behaviors predicted by loss aversion.

compensation to offset this loss. This confirms that the observed compensation shock is real, which might prompt managers to engage in opportunistic insider trading.

Second, relative to managers that just beat a relative performance goal, those that just miss one subsequently earn higher abnormal profits from insider trading that amount to about 8% of their total compensation. Combined with the above estimate on the effect of missing relative performance goals on abnormal incentive pay, this estimate suggests that managers use insider trading to make up for over half of the loss in compensation due to missing performance goals.

Third, I find that relative to managers that just beat a relative performance goal, those that just miss one are less likely to provide earnings and sales guidance, suggesting that managers strategically withhold information to increase their informational advantage. The economic magnitude of this effect is large: For example, the difference in the likelihood of making voluntary guidance disclosures between firms that narrowly miss and those that narrowly beat relative performance goals is 25.0 percentage points, which is large considering that the mean likelihood of providing guidance disclosures is 67.3% in the full sample. These results provide suggestive evidence that managers that miss relative performance goals act strategically to increase the gains from insider trading.

Fourth, to mitigate the concern that the observed effect on insider trading profits is driven by CEOs who receive equity (as opposed to cash) awards and hence have a diversification motive in their insider trades (Ofek and Yermack (2000)), I repeat the regression on the subsamples of equity awards and cash awards separately. I find that the effect of missing relative performance goals on insider trading profits holds for both subsamples, suggesting that the diversification story is not a main driver of the observed results.

Last, I explore the mechanics of this behavior and find the results are driven by better

timed opportunistic trades rather than by increased trading volume. The abnormal profits are concentrated in the second and third quarters after compensation is set. These results suggest that goal-missing CEOs do not trade more, but rather trade more profitably through strategically timed, opportunistic trades.

This paper contributes to our understanding of the efficacy of performance-based compensation. The results that managers who miss a relative performance goal become more likely to engage in opportunistic insider trading to make up for the loss in compensation suggest that performance-based incentive awards may be limited in generating incentives for managers. Closer scrutiny of managerial actions, such as insider trading, may be necessary to improve the effectiveness of performance-based pay.

As the first to identify the effect of missing relative performance goals on managerial actions, this paper contributes to three strands of literature. The first is the literature on performance-based pay. A large number of studies examine the effects of performance goals in compensation contracts on managerial behavior.[4] Prior studies show that performance-based pay can lead to earnings and real activities management (e.g., Healy (1985); Holthausen, Larcker, and Sloan (1995); Murphy (2000); Murphy and Jensen (2011); Bizjak, Hayes, and Kalpathy (2015); Bennett et al. (2017)) and increased risk-taking incentives (e.g., Park and Vrettos (2015); Bettis, Bizjak, Coles, and Kalpathy (2018)). My paper contributes to this literature by focusing on a setting where performance goals are hard to manipulate and presenting causal evidence that managers use insider trading to make up for the loss in compensation due to missing relative performance goals. My findings also complement those in the literature on managers'

[4]See Murphy (2013) and Edmans et al. (2017) for comprehensive reviews of the literature.

compensation-related opportunistic behavior (e.g., Lambert, Lanen, and Larcker (1989); Yermack (1997); Aboody and Kasznik (2000); Lie (2005); Heron and Lie (2007), Heron and Lie (2009); Narayanan and Seyhun (2008); Daines, McQueen, and Schonlau (2018)).

The second strand of the literature this paper contributes to is that on insider trading. While considerable evidence shows that corporate managers make abnormal profits from opportunistic insider trading (e.g., Cohen et al. (2012); Agrawal and Cooper (2015); Ali and Hirshleifer (2017)), the motivations underlying such opportunistic behavior remain open to debate. For example, Bhattacharya and Marshall (2012) show that "richer" managers (i.e., those with higher compensation) are associated with a higher likelihood of carrying out illegal insider trading, which they interpret as suggesting that insider trading is not motivated by rational monetary considerations. In contrast, using detailed Swedish data, Kallunki, Kallunki, Nilsson, and Puhakka (2018) find that insiders with a lower level of wealth and income are more likely to engage in informed insider trading. My paper is the first to provide causal evidence that a negative shock to compensation induces more profitable insider trading, which is consistent with an economic motive for opportunistic insider trading (Becker (1968)).[5]

Last, my paper contributes to the literature on insider trading as a form of executive compensation. Theories suggest that insider trading profits can substitute for explicit compensation and provide incentives to managers to increase firm values (Manne (1966); Carlton

[5]A small subset of the literature on insider trading argues that insiders' intention to sell can provide strong incentives to manage earnings to beat analyst forecasts (e.g., McVay, Nagar, and Tang (2006); Shon and Veliotis (2013)). My paper differs from these studies by focusing on a setting where managers cannot precisely manipulate the performance outcome (i.e., missing vs. beating relative performance goals) and showing the effect of the performance outcome on CEOs' insider trading behavior.

and Fischel (1983); Baiman and Verrecchia (1995), Baiman and Verrecchia (1996)). Existing empirical studies test this idea by focusing on the relation between insider trading restrictions and executive compensation (e.g., Roulstone (2003); Henderson (2011); Denis and Xu (2013)). For example, Roulstone (2003) and Denis and Xu (2013) find that firm- and country-level insider trading restrictions, respectively, are associated with higher executive compensation. While a positive association between insider trading restrictions and the level of executive compensation is consistent with insider trading profits and explicit executive compensation being substitutes, it is also consistent with insider trading restrictions inducing firms to use more incentive pay to elicit managerial effort, which in turn increases the level of compensation. In other words, these results could be confounded by boards choosing a compensation structure that varies with insider trading restrictions to maintain optimal incentive levels. By focusing on insider trading behavior in a setting where compensation contracts are fixed, my paper abstracts from the incentive effect and provides cleaner evidence on the substitution effect. Also, since insider trading restrictions could be an endogenous response to executive compensation arrangements, the RD setting in my paper provides sharper identification of the effect of compensation shocks on insider trading.

The rest of the paper is organized as follows. Section II develops the hypothesis that a loss in compensation due to missing performance goals may lead managers to use insider trading to make up for the loss. Section III describes the data and main variables. Section IV discusses the identification strategy. Section V presents the empirical results, and Section VI concludes.

II. Hypothesis Development

Economic agents exhibit behaviors consistent with reference points and loss aversion, as proposed by Kahneman and Tversky (1979) and Tversky and Kahneman (1991), Tversky and Kahneman (1992). Using experimental evidence, they argue that economic agents make decisions based on the outcomes relative to a reference level, and the value function exhibits a steeper slope when losses occur relative to this reference level than when gains are made. In the context of corporate managers, Dittmann, Maug, and Spalt (2010) and Spalt (2013) incorporate these features into their models to explain patterns in executive option holdings and firms' option granting behaviors. Heath, Huddart, and Lang (1999) and Klein and Maug (2019) provide empirical evidence suggesting the presence of reference points in executives' option exercising behavior.

Therefore, CEOs whose actual performance is close to a relative performance goal may treat the compensation associated with meeting the goal as a reference point and exhibit loss aversion when they narrowly miss the goal. In other words, CEOs derive utility from gains and losses relative to an expected level of compensation, and the negative effect of losses in compensation on utility is larger in magnitude than the positive effect of gains. This suggests that CEOs incur greater utility losses from lower-than-expected compensation due to missing the goals than utility gains from receiving the expected level of compensation due to beating the goals. As such, CEOs who narrowly miss a performance goal and hence suffer a loss in compensation would derive a higher marginal utility from an additional dollar of insider trading profits than otherwise similar CEOs who narrowly beat the goal.[6]

[6]Another possible economic mechanism is the diminishing marginal utility of income. Specifically, due to the

When deciding whether to engage in opportunistic trades, CEOs weigh the benefits, such as the utility derived from insider trading profits, against the costs, including the disutility associated with pecuniary and non-pecuniary penalties. While penalties for insider trading upon conviction, including disgorgement, reputational harm, or even imprisonment, may substantially exceed the realized profits, structural estimates suggest that detection rates are low. For instance, Patel and Putniņš (2021) estimate that only about 15% of illegal insider trades tied to mergers and acquisitions or earnings events are detected. Given the salience of such events, this figure likely represents an upper bound. Legal scholars (e.g., Brudney (1979); Carlton and Fischel (1983)) similarly note the significant evidentiary challenges of proving that a trade was based on "material nonpublic information." Consistent with this, I retrieve all 1,058 SEC insider-trading enforcement actions from 2006–2024 and find that none involve CEOs in my sample. Thus, the expected penalties faced by CEOs who narrowly miss goals may remain small compared to the insider profits they earn.

Taken together, these considerations suggest that while the marginal cost remains low and

significant negative shock to their compensation, CEOs who narrowly miss a relative performance goal would, compared to those who narrowly beat one, derive a higher marginal utility from an additional dollar of abnormal profits from insider trading. However, there are at least two reasons why diminishing marginal utility of income may not fully explain the observed results. First, considering that the median CEO stock-based wealth in my sample is $31.8 million and the average compensation difference between beating and missing a goal is approximately $1 million, the difference in marginal utility might be negligible under the principle of diminishing marginal utility of income. Second, if diminishing marginal utility of income were the primary driver, one might expect the impact of missing the goal to be more pronounced among CEOs with lower personal wealth. However, in an untabulated test using CEO stock-based wealth as a proxy for personal wealth, I do not observe a stronger effect for CEOs with a below-median stock-based wealth.

roughly the same for CEOs who miss and those who beat the goal, the marginal benefit from opportunistic insider trading is higher for CEOs who miss a performance goal. I therefore hypothesize that missing relative performance goals induces CEOs to earn higher abnormal profits from insider trading.

One potential counterargument is that CEOs who narrowly miss performance goals may face heightened scrutiny from various stakeholders such as boards, investors, regulators, and the media. This increased attention could raise the perceived costs of opportunistic trading for these goal-missing CEOs, thereby reducing their incentive to recoup lost compensation through insider trades. This possibility, therefore, runs counter to my main prediction and would bias my tests against finding higher insider trading profits for these CEOs.

III. Data and Variable Construction

A. Data and Sample

I obtain data on relative performance grants from ISS Incentive Lab. The data provide detailed information on performance grants for all named executives, including metrics used, performance goals, performance periods, vesting schedules, performance benchmarks, benchmarking methods, award types, etc. Incentive Lab covers the largest 750 companies by market capitalization in each year and backfills the data for new firms that enter the database back to 1998. I focus on relative performance grants, which use the performance of peer companies or market indexes as the performance benchmark. Since the SEC requires firms to provide standardized disclosure of executive compensation starting from July 2006, I confine the sample

to relative performance grants held by CEOs with performance periods ending between June 2006 and December 2016.

To minimize potential errors in estimating the running variable, i.e., the difference between actual performance and the relative performance goal, I focus on two types of performance metrics: (1) stock return metrics, i.e., total shareholder return (TSR), and (2) standard accounting metrics that are available in Compustat, including the level and the growth rate of earnings per share (EPS),[7] EBIT, EBITDA, EBT, FFO, operating income, and sales. I exclude grants with performance metrics containing the word "adjusted" or "non-GAAP," because it is not straightforward to estimate the running variable for these grants. The initial sample contains 4,205 unique observations of relative performance grants held by CEOs who stay in office during the grant's performance period at 753 distinct firms.[8]

I match relative performance grants to CRSP to obtain stock returns data and Compustat to retrieve financial statement data.[9] I exclude grants with missing information on performance

[7]Compustat provides four EPS measures, i.e., EPSFX (diluted EPS excluding extraordinary items), EPSPX (basic EPS excluding extraordinary items), EPSFI (diluted EPS including extraordinary items), EPSPI (basic EPS including extraordinary items). I use the description of the metrics provided by Incentive Lab (the "metric other" field) to determine the appropriate EPS measure. I use EPSFX if the grant specifies diluted EPS and EPSPX if otherwise.

[8]I treat a grant based on a performance metric during a performance period as a unique observation.

[9]Relative performance grants using stock return metrics use either a group of peer companies or a market index as the benchmark. I use stock returns of peer companies from CRSP and index returns from the Compustat Index Prices file to calculate the performance goal. If the index returns are not available in the Compustat file, I manually collect the data from index providers' websites. For grants where the peer group is defined as the constituents of an index, I require that the data on the historical constituents of the index are available in the Compustat Index Constituents file. Relative performance grants using accounting metrics use peer firms as benchmarks. I use accounting data from the Compustat annual file to calculate the performance goal for these grants.

metrics, performance periods, performance goals, award types, award quantities, relative benchmarks, or peers. The remaining sample consists of 2,148 observations.

I then require that the payout structure exhibit discontinuous jumps around performance goals. A performance grant usually has a performance target, at or above which the executive receives a target payout. Below the target goal, another performance goal, i.e., threshold goal, may be included in the grant. Below the threshold goal, the actual payout is zero. Between the threshold goal and target goal, the actual payout can stay at the threshold payout level until the performance reaches the target goal or be interpolated between the threshold payout and the target payout. To focus on grants with discontinuous jumps in performance-based pay, I restrict the sample to three types of relative performance goals, i.e., threshold goals,[10] target goals only, and target goals without interpolation between the threshold and target goals.[11],[12] Given that my focus is on the discontinuity in compensation in a narrow range around performance goals, for grants with more than one goal (e.g., a target and a threshold goal) for the same performance metric, I

[10]I use the term "threshold goals" broadly to include both cases with an associated target goal and those without. This is because a threshold goal, by definition, introduces a discontinuous jump from zero in payout once performance reaches the threshold. Accordingly, I include all grants with threshold goals, regardless of whether they are accompanied by target goals.

[11]As will be discussed in Section V.E, I conduct placebo tests using grants with interpolation between the threshold and target goals, which do not exhibit discontinuity in payouts around the goals.

[12]Besides the threshold and target goals, some compensation grants specify a maximum goal, which could also be associated with a discontinuous jump in compensation. However, actual performance rarely falls around the maximum goal. In my baseline sample, only 11 observations have actual performance within $\pm 5\%$ around the maximum goal. I thus follow Bennett et al. (2017) to only focus on threshold and target goals.

follow Bennett et al. (2017) and keep only the goal closest to the actual performance. This procedure yields a sample of 1,438 relative performance grants.

I obtain data on CEO compensation from Incentive Lab and S&P ExecuComp. I rely mainly on Incentive Lab, and use ExecuComp database when the information is not available in Incentive Lab. The compensation data provide a breakdown of the components, such as salary, bonus, stock awards, non-equity incentive plan compensation, etc. After dropping observations with missing compensation information at the end of the performance period, my final sample contains 1,317 relative performance grants. Table IA-1 in the Internet Appendix shows the construction of the sample.

I obtain insider trading data from Thomson Reuters Insider Filing Data Feed, which contains detailed information on insider trades, including the name of the insider, ticker, CUSIP, name of the company, transaction date, transaction price, and transaction quantity. I match insider trading data to Incentive Lab data by CEO name and company identifier. I also match insider trading data with CRSP to retrieve data on stock returns. Following Lakonishok and Lee (2001) and Marin and Olivier (2008), I focus on CEOs' open market or private sales (transaction code "S") and open market or private purchases (transaction code "P") of non-derivative securities. To minimize data errors, I follow Lakonishok and Lee (2001) and Marin and Olivier (2008) by excluding transactions for which the transaction price is not within 20% of the CRSP closing price on the transaction day and those for which the number of shares traded is greater than 20% of shares outstanding.

B. Variable Construction

Performance-based grants are incentive awards of which the actual payouts are determined by one or a few metrics derived from the firm's stock market or financial performance. The firm's performance is measured over a pre-defined performance period, typically one or three years. At the end of the performance period, the firm's actual performance is compared to the performance goal to determine the actual payout of the grant. Missing a performance goal, even by a small margin, could result in a large loss in compensation. I use the discontinuity in compensation induced by missing relative performance goals as a source of plausibly exogenous variation in CEO pay.

The running variable is *Actual minus goal*, defined as the difference between a firm's actual performance and its corresponding relative performance goal. For relative performance grants based on stock return metrics, I calculate the running variable as the difference in the buy-and-hold total shareholder returns between the firm and the benchmark. For relative performance grants based on accounting metrics in the form of growth rates, I calculate the running variable as the difference in the growth rates between the firm and the benchmark. For accounting metrics in levels, the running variable is measured as the percentage difference between actual performance and the performance goal, i.e., the difference between the actual value of the performance metric of the firm under consideration and that of its benchmark divided by the benchmark.

Relative benchmarking can be broadly categorized into two forms, i.e., attaining a certain ranking among peer companies and attaining a certain level of relative performance. If a grant uses relative ranking, the running variable is calculated as the actual performance of the firm

minus the performance of the peer firm whose performance rank is the specified rank in the grant. For example, for a grant that sets a performance goal of achieving the 75th percentile of TSR among a group of 100 peer companies, the running variable is the TSR of the firm minus the TSR of the company ranks 75th among the peer companies in terms of TSR. If the grant uses the level of relative performance as the goal, the running variable is calculated as the actual performance of the firm minus the specified performance goals. For example, for a grant with a performance goal of delivering a TSR of 3% above the S&P 500 total return, the running variable is calculated as the TSR of the firm minus the sum of 3% and the S&P 500 total return.

I use the difference between actual incentive compensation earned and the pre-defined incentive compensation payout specified in the grant for meeting the performance goal to capture the shock to compensation induced by missing performance goals. For each performance-based grant, Incentive Lab provides information on pre-defined payouts for meeting each performance goal. However, it does not provide the actual amount of payout earned. I rely on data provided by Incentive Lab, using information in the *Summary Compensation* table, together with the *Outstanding Equity Awards*, *Option Exercises and Stock Vested*, and *Grants of Plan-Based Awards* tables extracted from DEF 14A filings to infer the actual payout. For equity awards, i.e., performance grants with a stock payout, I use the total number of shares vested minus the time-vested restricted stock in the grant's vesting year as a proxy for the actual payout of the grant.[13] I define abnormal incentive pay as the estimate for the actual payout of the grant minus

[13]For some grants, Incentive Lab does not clearly specify the grant date or the vesting date, which makes it hard for the above approach to obtain an accurate estimate of the actual payout. To obtain data on the actual payout from these grants, I check the DEF 14A filings and hand-collect information on the actual payout mentioned in the Compensation Discussion and Analysis section of the filings.

the pre-defined payout for meeting the performance goal, scaled by total compensation in the previous fiscal year. I obtain the total number of shares vested from the *Option Exercises and Stock Vested* table. The *Grants of Plan-Based Awards* table tracks annual grants of time-vesting restricted stock, while the *Outstanding Equity Awards* table reports the CEO's unvested non-performance-vested equity holdings. To proxy for the actual time-vesting shares vested in a given year, I subtract the year-over-year change in unvested non-performance-vested shares from the newly granted time-vesting stock awards. For cash awards, i.e., performance grants with a cash payout, I use the CEO's non-equity incentive plan compensation reported in the *Summary Compensation* table in the vesting year of the grant as a proxy for the actual cash payout of the grant.

When multiple performance grants vest in the same fiscal year, I aggregate their respective target (or threshold) amounts and use the sum as the expected payouts. While the lack of grant-level payout data necessitates aggregation and inevitably introduces measurement error, it is unlikely to bias the main estimates. Because my analysis focuses on a narrow range around a focal relative-performance goal, whether a CEO misses or beats the goal is unlikely to be systematically correlated with the vesting of other, non-focal grants in the same year. Therefore, any resulting noise is likely random, which may attenuate statistical significance but should not bias the regression estimates.[14]

[14]To ensure this aggregation does not bias the results, I estimate the baseline specification separately for CEOs with a single grant and those with multiple grants. The results, reported in Table IA-2 in the Internet Appendix, show that the main estimates are similar in magnitude across both groups. While the coefficient for the single-grant subsample is statistically insignificant, this reflects limited statistical power (i.e., only 28 observations fall within the $\pm 5\%$ bandwidth) rather than contradictory evidence. Overall, the stability of the point estimates supports the claim

I measure abnormal insider trading profits of a CEO as the total dollar amount of abnormal profits of all trades placed by the CEO during a 12-month period following the performance period end divided by total compensation in the previous fiscal year. I use lagged total compensation as the scaling factor for both abnormal incentive pay and abnormal insider trading profits to facilitate comparing the magnitude of the effects.[15]

For each trade, I calculate the firm's buy-and-hold raw stock return over the subsequent 126 trading days and subtract the buy-and-hold return on the CRSP value-weighted market index over the same period to obtain the market-adjusted return.[16] Abnormal insider trading profit of a trade is then calculated as the product of this buy-and-hold market-adjusted stock return and the signed dollar value of the trade (where purchases are positive and sales are negative). I calculate the total abnormal trading profits of a CEO by summing the abnormal profits of all trades placed by the CEO over the 12-month period. For relative performance grants with stock return metrics, the 12-month period starts immediately after the end of the performance period. For relative performance grants with accounting metrics, the 12-month period starts three months after the

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that aggregating multiple grants does not introduce systematic bias; rather, it introduces random noise that, if anything, would attenuate the main estimates.

[15]The measure for abnormal insider trading profits I construct here differs from percentage returns of insider trading used in the literature. Since the focus of my paper is on the economic value managers extract from insider trading and its relationship with the shock to compensation, dollar profits (scaled by lagged compensation) are more relevant than percentage returns.

[16]I choose a holding period of 126 trading days, because the short-swing rule (Section 16b of the Securities Exchange Act) requires that insiders disgorge any profits made by offsetting round-trip transactions within a six-month period. For robustness, I also compute the profits over three other windows, i.e., 21, 63, and 252 trading days. I report the results using these alternative holding periods in Section V.E.

end of the performance period. The three-month lag allows the relative performance outcome, which is based on the financial statements of the company under consideration and those of its peer companies, to be observed.

Table 1 reports the summary statistics of the main variables for the full sample (Panel A) as well as for the $\pm 5\%$ sample (Panel B). The $\pm 5\%$ sample consists of observations where *Actual minus goal* falls within a $\pm 5\%$ range around zero. *Actual minus goal* has a mean of 0.08 and a standard deviation of 0.31 in the full sample, and the corresponding numbers in the $\pm 5\%$ sample are 0.00 and 0.03. Abnormal incentive pay and abnormal insider trading profits as a fraction of total compensation have a mean of 5.35% and 0.56%, and a standard deviation of 21.60% and 6.70%, respectively, in the full sample. The modest mean profitability of insider trading is consistent with Cziraki and Gider (2021), who show that the dollar profits to insider trading are on average small.

Table 1 also reports the summary statistics of various firm characteristics, including total assets, market-to-book, leverage, return on assets, investments, analyst coverage, institutional ownership, prior market-adjusted returns, share repurchases, and discretionary accruals, as well as CEO characteristics, including CEO age, tenure, total compensation, fraction of stock awards, CEO ownership, and CEO stock-based wealth. All characteristics except prior stock returns, share repurchases, discretionary accruals, CEO age, and CEO tenure are measured in the fiscal year ending 12 months before the performance period end. Prior stock returns, share repurchases, discretionary accruals, CEO age, and CEO tenure are measured as of the end of the performance period. The full sample and the $\pm 5\%$ sample are generally very similar in these firm and CEO characteristics.

[Insert Table 1 approximately here]

IV. Empirical Strategy

A. Regression Discontinuity Approach

I use a fuzzy regression discontinuity design to identify the effect of compensation shocks induced by missing relative performance goals on insider trading.[17] The identification assumption is that, except for a discontinuous jump in incentive compensation, there is no other discontinuity in firm or CEO characteristics around the relative performance goals. In other words, beating or missing the performance goals cannot impact CEOs' insider trading behavior except through its effect on CEO compensation. Since relative performance goals are based on the performance of peer companies during the same performance period and become known only after the end of the performance period, it is plausible that corporate managers cannot precisely control their performance relative to their peers within a narrow band around the goals. Thus, whether a firm beats or misses its relative performance goals is "as good as randomized" around the cutoff of the performance goals (Lee and Lemieux (2010)).

In my baseline regressions, I use the $\pm 5\%$ sample, i.e., relative performance grants with *Actual minus goal* within 5% around zero. The choice of bandwidth around the cutoff involves a fundamental trade-off between bias and variance (Roberts and Whited (2013)). Restricting the

[17] OLS regressions of insider trading profits on *Actual minus goal* suffer from an omitted variable bias, because *Actual minus goal* could be correlated with a number of factors that can directly affect abnormal profits from insider trading. For example, a higher *Actual minus goal* could be due to a relatively low goal and hence may indicate weak corporate governance, which can positively impact insider trading profitability. Also, a more positive *Actual minus goal* could be correlated with a higher price for the firm's stock and hence a higher value of the CEO's equity holdings in the firm, which could reduce the incentive to engage in opportunistic insider trading. Because of these confounding factors, regressing abnormal insider trading profits on *Actual minus goal* would produce biased estimates.

analysis to a narrow window minimizes omitted variable bias by ensuring that the treated and control groups remain fundamentally comparable around the cutoff. Further away from the cutoff, confounding factors naturally arise and could bias the estimates, as CEOs who miss goals by a wide margin are likely systematically different from those who easily beat them in terms of unobservable managerial skill, firm fundamentals, or corporate governance. Conversely, although an even tighter bandwidth would further reduce bias, it would simultaneously decrease the sample size and reduce statistical power. While the 5% bandwidth strikes a reasonable balance, to ensure that the findings are not driven by the choice of this specific cutoff, I provide two sets of robustness checks. First, in Section V.E.3, I demonstrate that the main results remain stable across a wide range of alternative bandwidths from 3% to 15%. Second, in Section V.E.4, I show that the results remain robust to a nonparametric approach that relies on an optimal bandwidth selection algorithm rather than a pre-specified bandwidth.

I first test the effect of missing a relative performance goal on abnormal incentive compensation by running the following regression,

$$\begin{aligned} \textit{Abnormal incentive pay}_{i,t} &= c + \alpha_1 I^{\text{miss}}_{i,j,t} + \alpha_2 \textit{Actual minus goal}_{i,j,t} \\ &\quad + \alpha_3 I^{\text{miss}}_{i,j,t} \times \textit{Actual minus goal}_{i,j,t} [+\gamma \boldsymbol{X}_{i,t-1}] + \epsilon_{i,t}. \end{aligned} \tag{1}$$

where *Abnormal incentive pay*$_{i,t}$ is the abnormal incentive pay received by the CEO of firm i in year t, $I^{\text{miss}}_{i,j,t}$ is an indicator that equals one if firm i's actual performance on metric j at the end of the performance period t is below its corresponding performance goal and zero otherwise, *Actual minus goal*$_{i,j,t}$ is the difference between firm i's actual performance on metric j at the end of the performance period t and its corresponding performance goal, and $\boldsymbol{X}_{i,t-1}$ is a vector of

firm and CEO characteristics. I measure prior stock returns, share repurchases, discretionary accruals, CEO age, and CEO tenure as of the end of the performance period. All other characteristics are measured in year $t-1$, i.e., the fiscal year ending 12 months before the performance period end. Under the random assignment assumption of RDD, it is not necessary to control for the term in brackets, i.e., firm and manager characteristics. Including these controls, however, could help reduce the sampling variability in the estimator (Lee and Lemieux (2010)). I thus run the regression with and without the term in brackets. I cluster standard errors at the firm level. The coefficient of interest, α_1, measures the local average treatment effect of missing a relative performance goal on CEO compensation.

I then use a reduced-form model to test the impact of missing a relative performance goal on insider trading. I run the following regression,

$$\begin{aligned} \textit{Abnormal insider trading profit}_{i,t+1} &= c + \beta_1 I^{\text{miss}}_{i,j,t} + \beta_2 \textit{Actual minus goal}_{i,j,t} \\ &\quad + \beta_3 I^{\text{miss}}_{i,j,t} \times \textit{Actual minus goal}_{i,j,t} [+\theta \mathbf{X}_{i,t-1}] + \epsilon_{i,t+1}. \end{aligned} \tag{2}$$

where *Abnormal insider trading profit*$_{i,t+1}$ is the abnormal profits the CEO of firm i earns from insider trading in a 12-month period following the performance period end, i.e., year $t+1$, scaled by the CEO's total compensation in year $t-1$, and $I^{\text{miss}}_{i,j,t}$, *Actual minus goal*$_{i,j,t}$, and $\boldsymbol{X}_{i,t-1}$ are as defined above. I again cluster standard errors at the firm level. The coefficient of interest, β_1, measures the local average treatment effect of missing a relative performance goal on subsequent insider trading profitability.

B. Validity of the RDD

There are two primary concerns regarding the validity of the regression discontinuity approach (Imbens and Lemieux (2008)), the first of which is the manipulation of the running variable around the cutoff. If a firm can precisely manipulate its performance metric to beat the performance goal right around the cutoff, it violates the quasi-experimental assumption that the assignment of the treatment group should be "as good as randomized." I test whether actual performance exhibits bunching on one side of performance goals. The second concern is the presence of differences other than the treatment effect around the cutoff. I compare various firm and CEO characteristics between CEOs that narrowly miss and those that narrowly beat their relative performance goals.

1. Continuity of the Density of the Running Variable

The underlying assumption of RDD is that the running variable cannot be precisely manipulated to be just above or below the performance cutoff. The relative performance metric used in a CEO incentive grant is determined by other firms' performance during the same performance period. Because of the contemporaneous feature of this benchmark, it is plausible that managers cannot perfectly control whether their performance is above or below a relative goal in a narrow range around the goal. McCrary (2008) develops a test for continuity of the running variable density function to detect manipulation of the running variable around the cutoff point. I implement this density test and plot the estimated density function of *Actual minus goal* in a bandwidth of 10%. Figure 1 shows that there is no bunching on either side of the cutoff and the density function is smooth at the cutoff. The McCrary test statistic is -0.13, which fails to reject

the null hypothesis of continuity. This result is consistent with Bennett et al. (2017), who find no evidence of performance bunching around relative performance goals. In my subsequent main RD analyses (i.e., Tables 3 and 4, and Figures 2 and 3), I use the same running variable and estimation sample as in Figure 1. The McCrary density test result therefore supports the validity of these analyses as well.

[Insert Figure 1 approximately here]

2. Continuity of Other Covariates

One important implication of the RDD identification assumption is that there are no discontinuous characteristics directly affecting the outcome variable around the cutoff other than the treatment effect. If any discontinuity in covariates is detected, it would imply a failure of the underlying assumption of RDD. Although not all covariates are observable, I follow the standard analysis in the RDD framework and test the continuity in the distribution of observable covariates around the cutoff. I consider various firm characteristics, including total assets, market-to-book, leverage, return on assets, investments, analyst coverage, institutional ownership, prior market-adjusted returns, share repurchases, and discretionary accruals, as well as CEO characteristics, including CEO age, tenure, total compensation, fraction of stock awards, CEO ownership, and CEO stock-based wealth. I run a linear regression of each characteristic on the indicator for missing performance goals, the running variable, and an interaction term combining the two variables, with industry and year fixed effects using a 5% bandwidth around the cutoff. The first column of Table 2 shows that none of the coefficients on the indicator for missing relative performance goals are statistically significant, suggesting that the distribution is smooth around the cutoff. Notably, prior one- and three-year abnormal stock returns do not exhibit

discontinuity at the cutoff, mitigating the concern that missing a performance goal might induce negative wealth effects due to the loss in the value of CEOs' existing stock holdings in the firm. Also, there is no discontinuity in discretionary accruals or share repurchases around the cutoff, suggesting that CEOs that beat relative performance goals do not manage earnings or repurchase shares more aggressively than those that miss the goals. These results confirm that managers cannot precisely manipulate their performance. The results that there is no discontinuity in discretionary accruals or analyst coverage also mitigate the concern that insider trading profitability may vary with information asymmetry faced by firms (e.g., Frankel and Li (2004); Aboody, Hughes, and Liu (2005)).

I then compare these characteristics in a narrow range of 5% on each side of the performance cutoff. The second through fourth columns of Table 2 report the means of each characteristic for the two groups as well as p-values from t-tests of the difference between the means. The p-values show that none of the characteristics are significantly different between CEOs that just beat and those that just miss relative performance goals, suggesting that the two groups of CEOs have balanced observable covariates.

Thus, I fail to reject the assumption of randomization in the data. This allows me to use missing relative performance goals right around the cutoff to identify the effect of compensation shocks on insider trading.

[Insert Table 2 approximately here]

V. Empirical Results

A. The Effect of Missing Relative Performance Goals on CEO Compensation

I first present graphical evidence of discontinuity in the abnormal incentive pay. Figure 2 plots the mean of abnormal incentive pay for each bin of the running variable, i.e., *Actual minus goal*, together with fitted lines on both sides of the cutoff using grants for which the running variable is within narrow bands of 10% on both sides of the cutoff. Abnormal incentive pay displays a strong discontinuity at the cutoff. The jump in the abnormal incentive compensation at the cutoff is about 15% of total compensation, suggesting that missing performance goals has a large impact on abnormal incentive pay. This result echoes Murphy (2000)'s finding that managers with "externally" determined performance goals have more variable bonuses, because they do not have compensation-related reasons to manage earnings to beat performance goals.

[Insert Figure 2 approximately here]

I estimate Eq. (1) to formally test the effect of missing a relative performance goal on CEO compensation. Table 3 reports the results. I start from a specification that includes only the indicator for missing performance goals, the running variable, and their interaction term as regressors. Column 1 shows that the coefficient on the indicator for missing performance goals is -0.131, suggesting that abnormal incentive pay as a fraction of total compensation drops by 13.1% at the cutoff. This number is similar in magnitude to the estimate presented in the graphical analysis. I then include year fixed effects, contract type fixed effects, and firm and CEO characteristics as controls in the regression. Year fixed effects absorb variation in abnormal

incentive compensation over time. I sort compensation contracts into four types along two dimensions, i.e., whether a threshold or target goal is used as the performance benchmark and whether it is a stock or cash award. The inclusion of contract type fixed effects allows me to control for heterogeneity across different types of compensation contracts. Columns 2 and 3 show that the estimate of the coefficient on the miss indicator is largely unchanged, again confirming that missing a relative performance goal is independent of these observable characteristics.

[Insert Table 3 approximately here]

The economic magnitude of the effect of missing performance goals on abnormal incentive pay is large. For example, since the mean for total compensation in my sample is $9.7 million, a 13% loss amounts to a pay cut of $1.3 million. This number is about one third of the total dollar value of stock awards for the average CEO, which is around $3.8 million.[18] Thus, missing relative performance goals has a large negative impact on executive compensation, which may lead managers to take actions to make up for the loss in compensation.

B. The Effect of Missing Relative Performance Goals on Abnormal Insider Trading Profits

As discussed in Section II, there are at least two reasons why CEOs who narrowly miss relative performance goals might be induced to make abnormal insider trading profits, the first of which is diminishing marginal utility of income. CEOs who miss a relative performance goal and

[18] A CEO's compensation package commonly contains more than one incentive award and more than one performance metric. Also, some incentive awards have more than one goal for a given performance metric. In such cases, missing one performance goal makes the CEO lose a portion of the total incentive payout.

hence suffer a loss in compensation would derive a higher marginal utility for each additional dollar earned from insider trading than those who narrowly beat a relative performance goal. The second reason is reference point and loss aversion. CEOs might treat the compensation associated with meeting relative performance goals as a reference point and exhibit loss aversion when they narrowly miss the goals (Kahneman and Tversky (1979)). Moreover, the marginal cost of exploiting private information in insider trading is arguably the same between CEOs who narrowly miss a goal and those who narrowly beat one. Therefore, missing relative performance goals may induce CEOs to earn higher abnormal profits from insider trading.

I again first present graphical evidence of discontinuity in abnormal insider trading profits. Figure 3 plots the mean of abnormal insider trading profits for each bin of the running variable, i.e., *Actual minus goal*, together with fitted lines on both sides of the cutoff using grants for which the running variable is within narrow bands of 10% on both sides of the cutoff. The figure shows a strong discontinuity in abnormal insider trading profits at the cutoff. The jump in abnormal insider trading profits at the cutoff is about 8% of total compensation, suggesting that missing performance goals induces corporate managers to exploit insider information in their trades.[19]

[Insert Figure 3 approximately here]

I estimate Eq. (2) to formally test the effect of missing relative performance goals on the profitability of insider trading. Table 4 reports the regression results. I again start from a specification without any controls and add year and contract type fixed effects as well as firm and CEO characteristics in steps. The coefficient of the indicator for missing relative performance

[19]Figures 2 and 3 use the same running variable and estimation sample as Figure 1. As shown in Figure 1, the McCrary (2008) density test yields a statistic of -0.13 and fails to reject the null hypothesis of continuity at the cutoff. This result supports the validity of the RD design throughout the analysis.

goals ranges from 0.077 to 0.085, suggesting that CEOs who just miss a relative performance goal earn significantly higher abnormal profits from insider trading than otherwise similar CEOs who just beat one.[20]

[Insert Table 4 approximately here]

In the fuzzy RDD framework, the causal effect of compensation shocks on abnormal insider trading profits is the ratio of the discontinuity in abnormal insider trading profits at the cutoff obtained from the reduced-form regression to the discontinuity in abnormal incentive pay at the cutoff obtained from the first-stage regression. Using the estimates from the regressions with (without) fixed effects and controls, the causal effect of compensation shocks on abnormal insider trading profits is 0.691 (0.588). This suggests that CEOs use insider trading to make up for about 60% to 70% of the loss in compensation, which is economically substantial.[21]

[20]The effect of missing relative performance goals on insider trading profitability should increase with the size of the compensation shock. That is, the larger the prespecified award associated with beating performance goals as a fraction of total compensation, the greater is the negative impact of missing performance goals on CEOs' compensation, thereby providing CEOs a stronger incentive to carry out opportunistic insider trading to make up for the loss in compensation. I repeat the test on subsamples of grants with high and low ratios of prespecified award value to total compensation separately. Table IA-3 in the Internet Appendix shows suggestive evidence that is consistent with this prediction.

[21]To ensure the results are not driven by outliers but are present across the distribution, I estimate fixed-effects quantile regressions (Machado and Santos Silva (2019)) at the 10th, 25th, 50th, 75th, and 90th percentiles. Internet Appendix Table IA-4 shows that narrowly missing a relative-performance goal lowers Abnormal Incentive Pay across the distribution (Panel A) and increases Abnormal Insider Trading Profits at all percentiles (Panel B).

C. Cash vs. Equity Awards

In equity awards, CEOs who beat their performance goals will receive shares in their own firms and hence might have an increased incentive to trade for diversification purposes (Ofek and Yermack (2000)), which may explain the lower insider trading profits for these CEOs. In contrast, this diversification story would not play a role in cash-based incentive awards, because the payouts from these awards are cash and thus do not change the need for diversification. If the diversification channel drives the results on abnormal insider trading profits, the effect should be present only for equity awards, but not for cash awards. To test this prediction, I repeat the reduced-form regression on the subsamples of equity awards and cash awards separately. The results, reported in Table 5, show that the effect holds for both subsamples. The coefficient of interest for cash and stock awards in regressions with year and contract type fixed effects and the full set of controls is 0.108 (significant at the 10% level) and 0.080 (significant at the 5% level), respectively, suggesting that the diversification story is not a main driver of the observed results.

[Insert Table 5 approximately here]

D. The Effect of Missing Relative Performance Goals on Management Disclosure Choices

So far, the results suggest that missing relative performance goals induces CEOs to make higher abnormal profits from insider trading. In order to exploit their private information, corporate managers may withhold information from outside investors by reducing voluntary guidance disclosures. Since the release of earnings forecasts by management tends to decrease information asymmetry between managers and outside shareholders (e.g., Trueman (1986)),

managers who receive a negative compensation shock may strategically reduce the release of information to gain an informational advantage in trading their firms' stocks.

To test this, I retrieve data on earnings and sales guidance issued by management from I/B/E/S. I construct two measures for management earnings and sales guidance. The first is the logarithm of one plus the number of earnings and sales guidance disclosures issued during a 12-month period following a performance period end, and the second is an indicator for whether the firm issues earnings and sales guidance in the 12-month period. Table 6 shows that, relative to managers that just beat a relative performance goal, those that just miss one are less likely to provide earnings and sales guidance, suggesting that managers strategically withhold information to increase their informational advantage. The economic magnitude of this effect is large. For example, column 4 shows that the difference in the likelihood of making voluntary guidance disclosures between firms that narrowly miss and those that narrowly beat relative performance goals is 25.0 percentage points, which is large given that the mean likelihood of providing guidance disclosures is 67.3% in my sample. These results suggest that missing relative performance goals induces strategic behavior by managers. My preferred interpretation is that managers that miss performance goals act strategically to increase the gains from insider trading. However, I cannot rule out the possibility that missing a performance goal makes CEOs more cautious about communicating with the market.[22]

[Insert Table 6 approximately here]

[22] As will be discussed in Section V.F, I find no discontinuity in subsequent stock and operating performance, suggesting that the observed discontinuity in voluntary disclosure is not driven by expected future performance.

E. Robustness Checks

1. Pre-Existing Differences in Abnormal Insider Trading Profits

If, for some reason, CEOs with a greater tendency to exploit private information in their insider trades happen to have a higher likelihood of narrowly missing relative performance goals, the observed results on abnormal insider trading profits might be spurious. The insignificant differences in firm and CEO characteristics between CEOs who narrowly miss performance goals and those who narrowly beat them (discussed in Section IV.B) suggest that this is unlikely to be the case. Nevertheless, to rule out the possibility that the results on insider trading profits are driven by pre-existing differences between the two groups of CEOs, I examine discontinuities in abnormal insider trading profits around the cutoff during periods prior to the performance period end. I replace the dependent variable in Eq. (2) with abnormal insider trading profits earned during three 12-month periods before the performance period end. Year t is the 12-month period that ends in the same month as the performance period end. Years $t-1$ and $t-2$ are 12-month periods that end 12 and 24 months before the performance period end, respectively. Table 7 shows that the coefficient of interest is insignificant and close to zero.[23] This result further strengthens the validity of the RDD assumption that there is no discontinuity around the performance cutoff other than the compensation shock.

[Insert Table 7 approximately here]

[23] I conduct similar tests for guidance disclosures and find that there is no discontinuity around the cutoff in guidance disclosures during periods prior to the compensation shock.

2. Placebo Tests Using Awards Without Discontinuity in Payouts Around Relative Performance Goals

To further rule out the possibility that the effect of missing relative performance goals on insider trading profitability arises for reasons other than a discrete decrease in compensation, I conduct a placebo test using a sample of relative performance awards for which the payout structure is smooth around the performance goals. Specifically, the sample includes relative performance awards where incentive compensation is interpolated between the threshold and target goals and hence missing the target goal does not lead to a discrete decrease in compensation. Table 8 estimates Eq. (1) and (2) using this sample. The first two columns show that missing relative performance goals indeed do not result in a discontinuous change in compensation. Thus, if the finding on insider trading profitability is driven by compensation shocks, there should not be a discontinuity in abnormal insider trading profits around relative performance goals in this sample. Consistent with this prediction, the last two columns show no evidence of discontinuous jumps in abnormal insider trading profits around relative performance goals. These findings suggest that missing relative performance goals per se does not increase CEOs' abnormal insider trading profitability around the cutoff; it has to be accompanied by discrete changes in compensation to have an effect. Since managers exhibit a strong tendency to engage in opportunistic insider trading around performance goals with discrete changes in compensation but not around those without discrete changes, these results support the call for removing discontinuities in the pay-performance relations to improve the efficiency of compensation schemes (e.g., Healy (1985); Murphy and Jensen (2011); Edmans et al. (2017)).[24]

[24]It is worth noting that focusing on large compensation shocks around performance goals with a discontinuous payout structure allows me to provide a clean identification of the effect of compensation shocks on opportunistic

[Insert Table 8 approximately here]

3. Alternative Bandwidths

The choice of bandwidths in RDD involves a trade-off between bias and variance (Roberts and Whited (2013)). If there are sufficient data close enough to the cutoff on both sides, comparing the average of the outcomes of those to the left of the cutoff and those to the right will yield the treatment effect. In practice, a wider bandwidth around the cutoff may be necessary to ensure reasonable statistical power. To check the sensitivity of my results to the choice of bandwidths, I repeat the first-stage and reduced-form regressions using different bandwidths ranging from 3% to 15%. For bandwidths of 3% and 5%, I use a piecewise linear function of the running variable. For bandwidths greater than 5%, I use piecewise cubic polynomials, because Figures 2 and 3 suggest that higher order polynomials are necessary to fit the observations. The results, reported in Table 9, show that the coefficient of interest is significant and relatively stable across different bandwidths.

[Insert Table 9 approximately here]

4. Alternative Functional Form Specifications

Since there is no particular reason to assume that abnormal incentive pay and abnormal insider trading profits are linear functions of the running variable, for robustness I conduct two

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insider trading. The non-result for performance goals without a discontinuous payout structure does not imply that managers with such goals do not use insider trading to make up for the loss in compensation. It is just that there is no *discrete* jump in their insider trading profits around performance goals. Thus, removing discontinuities in the pay-performance relations may reduce, but may not eliminate, opportunistic insider trading.

sets of tests to ensure that the main findings are robust to alternative functional form specifications. First, to account for potential nonlinearity in a parametric framework, I augment the baseline models in Eqs. (1) and (2) with a quadratic term for the running variable. This approach follows best practices for RD designs (Gelman and Imbens (2019)). The results, reported in Internet Appendix Table IA-5, show that the RD coefficients remain statistically significant and are very similar in magnitude to the baseline linear estimates.

Second, as a more flexible check, I use a nonparametric approach. Specifically, I follow Calonico, Cattaneo, and Titiunik (2014) and use local linear regression to estimate the discontinuity in abnormal incentive pay and abnormal insider trading profits at the cutoff. This approach uses the mean squared error optimal bandwidth selector of Imbens and Kalyanaraman (2012) and improves the bandwidth selector with a bias-corrected procedure. I perform nonparametric estimations using three different procedures, i.e., conventional RD estimates with a conventional variance estimator, bias-corrected RD estimates with a conventional variance estimator, and bias-corrected RD estimates with a robust variance estimator, all with triangular kernel functions. To minimize the influence of extreme values and errors in the data on performance goals, I restrict the sample to incentive grants for which the running variable falls within the 10th and 90th percentiles. The sample includes 1,034 grants. The optimal bandwidth estimated following Calonico et al. (2014) and Calonico, Cattaneo, Farrell, and Titiunik (2017) is 8.1% for the first stage and 5.5% for the reduced form, which are close to the bandwidth I use in the baseline regressions.

Table 10 reports the results from the nonparametric RD estimation. The magnitude of the coefficient of interest is very similar to that obtained using the baseline regressions. For example,

the discontinuity in abnormal incentive pay ranges from -0.141 to -0.145, and that in abnormal insider trading profits ranges from 0.081 to 0.095.

[Insert Table 10 approximately here]

Taken together, the consistent results across the linear, quadratic, and non-parametric specifications provide strong evidence that the findings are robust to the choice of functional form.

5. Alternative Measures of Abnormal Insider Trading Profits

For robustness, I use alternative measures of abnormal insider trading profits. First, I use three alternative benchmarks to adjust returns. Specifically, I use DGTW characteristics-adjusted returns, size-adjusted returns, and industry-adjusted returns as measures of abnormal returns in the calculation of abnormal insider trading profits. Panel A of Table 11 shows that the results using these alternative measures are largely similar to those obtained using the baseline measures.

Second, I consider three alternative holding periods for measuring trading profits, i.e., one month (21 trading days), three months (63 trading days), and 12 months (252 trading days). Panel B of Table 11 shows that, across all three alternative holding periods, CEOs who just miss their performance goals consistently earn higher abnormal insider trading profits than those who just beat their performance goals. The magnitude of profits as a fraction of lagged total compensation increases as I extend the holding horizon, from 1.6% over a one-month holding period to 14.3% over a one-year holding period. This result suggests that missing relative performance goals induces CEOs to exploit relatively long-lived information about their companies' fundamentals.

[Insert Table 11 approximately here]

F. Discussion of Results

1. Alternative Interpretations

Managerial effort allocation. Missing a relative performance goal might induce CEOs to shift their effort away from improving their firms' performance and towards other activities such as insider trading. This effort allocation story might drive the observed difference in abnormal insider trading profits at the cutoff. To test this, I run RD regressions similar to Eq. (2) with firm performance in the year immediately following the performance period end as the dependent variable. I consider three performance measures, i.e., market-adjusted stock returns (over the next 12 and 36 months), return on assets, and profit margin. The results, reported in Table 12, show that the coefficient on the indicator for missing performance goals is insignificant across all performance measures. In particular, the abnormal stock returns over a one- and three-year period do not exhibit discontinuities at the cutoff. These results suggest that the observed results are unlikely to be driven by managerial effort allocation.

[Insert Table 12 approximately here]

Liquidity constraint. If CEOs that just miss a relative performance goal and hence receive a pay cut happen to be liquidity constrained, they might sell their stock holdings to raise cash to make up for the shortfall in liquidity. I estimate RD regressions with the dollar value of CEOs' insider sales during the one-year period following the performance period end divided by lagged total compensation as the dependent variable. The first two columns of Table IA-6 in the Internet

Appendix show that there is no discontinuity in insider selling around the cutoff.[25] This result casts doubt on the liquidity constraint story.

Tax-loss selling. Compared to CEOs that just miss a relative performance goal, those that just beat one receive higher compensation and hence might have an incentive to engage in tax-loss selling of their stock holdings. Such tax-loss motivated trading might reduce the informativeness of trades placed by CEOs that just beat a relative performance goal and thereby drive the observed discontinuity in abnormal insider trading profits at the cutoff. To rule out this possibility, I test for discontinuity in tax-loss selling following the performance period end. I use the dollar value of stock sold in December with negative 12-month past returns to approximate tax-loss selling (Grinblatt and Keloharju (2001)). I scale the value of tax-loss selling by lagged total compensation. I run RD regressions similar to Eq. (2) with the scaled tax-loss selling as the dependent variable. The last two columns of Table IA-6 in the Internet Appendix show that tax-loss selling does not exhibit discontinuity around the cutoff. Thus, tax-loss motivated trades seem unlikely to explain the observed results.

Involuntary CEO turnover. If missing a relative performance goal increases the likelihood of forced CEO turnover, it might affect CEOs' insider trading behavior. I construct two measures of involuntary CEO departure to test if missing a relative performance goal affects the probability of a CEO getting dismissed by the board. The first measure follows Parrino (1997), which classifies CEO turnover as forced if one of the following two conditions is satisfied: (a) the CEO is reported as fired or forced out or (b) for departing CEOs under the age of 60, there are no clearly reported reasons or the departure is reported as retirement but the firm does not announce

[25] In fact, the sign of the coefficient on the missing indicator is negative (although statistically insignificant), which is opposite to what one might expect under the liquidity constraint hypothesis.

it at least six months in advance. The second measure is based on the CEO dismissal database constructed by Gentry, Harrison, Quigley, and Boivie (2021).

I run RD regressions similar to Eq. (2) with one of the two measures for forced CEO turnover in the year immediately following the performance period end as the dependent variable. Table IA-7 in the Internet Appendix shows that forced CEO turnover does not exhibit discontinuities around the performance cutoff.[26] This result alleviates the concern that the observed increase in opportunistic insider trading by CEOs who narrowly miss a relative performance goal is driven by an increased likelihood of CEO dismissals.

2. Ex-Post Settling Up Through Discretionary Bonuses and Other Compensation

To assess whether boards "make up" a narrow miss with discretionary pay or other compensation, I conduct two tests. First, I examine discontinuity in discretionary bonuses filed within two years following the performance period end.[27] I scale the value of the discretionary pay by the reported total compensation in the year before the performance period to match the

[26]Bennett et al. (2017) shows a positive impact of missing absolute performance goals on forced CEO turnover. The difference in results between my paper and theirs is likely due to differences in the samples. While Bennett et al. (2017) focuses on incentive contracts using absolute performance goals with earnings-based accounting performance measures as performance metrics, the incentive contracts in my sample are based on relative performance goals and a large majority of the performance metrics are based on stock returns. Since the incentive contracts in my sample are less subject to manipulation by managers, firms offering such contracts may rely less on CEO dismissals as a disciplining device.

[27]I use a two-year window for this analysis to account for the significant institutional lags inherent in board compensation decisions, such as delays in bonus determination and disclosure. The results are robust to using a one-year window.

main specification. Following Ederhof (2010), I use textual analysis to identify discretionary bonuses awarded to CEOs in my baseline $\pm 5\%$ sample. Specifically, as firms are required to disclose discretionary bonuses to named executive officers on Form 8-K, I search all 8-K filings and proxy statements (i.e., DEF 14A) filed within two years following the performance period end, using keywords based on Ederhof (2010).[28] I manually identify 11 discretionary CEO awards in my baseline sample: 4 awarded after narrowly missing a goal and 7 after narrowly beating a goal. The first two columns of Panel A of Internet Appendix Table IA-8 estimate Eq. (1) using the amount of discretionary bonuses scaled by the reported total compensation in the year before the performance period as the dependent variable. The results show no significant discontinuity, indicating that the loss in abnormal incentive pay faced by CEOs who narrowly miss relative performance goals is not offset ex post through discretionary bonuses.

Second, I test for discontinuity in other compensation, which is defined as "all other compensations" as reported in the Summary Compensation Table in proxy statements (Incentive Lab item, OTHERCOMP) received within two years following the performance period. I again scale the total value of other compensation by the reported total compensation in the year before the performance period to align with the main specification. The last two columns of Panel A of

[28]The keywords include "discretionary bonus", "special bonus", "additional bonus", "discretionary cash award", "special cash award", "additional cash award", "discretionary cash bonus", "special cash bonus", "additional cash bonus", "discretionary stock award", "special stock award", "additional stock award", "discretionary equity award", "special equity award", "additional equity award", "discretionary rsu", "special rsu", "additional rsu", "discretionary option award", "special option award", "additional option award", "discretionary award", "special award", "additional award", "award additional", "awarded additional", "special unit", "one-time award", "one-time grant", "additional grant", "discretionary grant", "special grant".

Internet Appendix Table IA-8 show no significant discontinuity, suggesting no ex post "settling-up" through other compensation either.

To explore whether this aggregate null result masks heterogeneity, I examine the role of the board's compensation committee by partitioning the sample based on two characteristics of the committee: the presence of social ties with the CEO and the degree of co-option. I identify compensation committee in which at least one member has a social tie to the CEO, defined as overlapping university education or prior employment, following Cohen, Frazzini, and Malloy (2008) and Hwang and Kim (2009). To construct this measure, I obtain the names of compensation committee members from BoardEx and collect the education background and prior work experience of these directors and the CEO from BoardEx and manual internet searches. About 14% of firms in my sample have such social ties. I also measure co-option as the fraction of co-opted directors on the compensation committee. Following Coles, Daniel, and Naveen (2014), I define co-opted directors as those who are appointed during the current CEO's tenure. I construct this measure using appointment dates from ExecuComp and BoardEx, supplemented by web searches.

Panel B of Internet Appendix Table IA-8 presents the results from these subsample analyses. The RD estimates for both discretionary bonuses and other compensation remain statistically insignificant across subsamples. The difference in the coefficients between subsamples is small and statistically insignificant for discretionary bonuses. For other compensation, the difference in the estimates suggests that firms with high committee co-option award *less* other compensation to goal-missing CEOs.[29] These findings suggest that boards do not

[29]This result runs counter to the prediction that more co-opted committees would be more inclined to offset missed performance goals with additional discretionary pay. Given the relatively small subsample sizes, the absence

systematically increase these observable discretionary compensation measures to offset a performance miss, even when the compensation committee is socially connected to or co-opted by the CEO.

3. The Mechanics of Opportunistic Insider Trading

To understand *how* CEOs who narrowly miss performance goals earn abnormal trading profits, I conduct two sets of tests on the characteristics of their trades. First, I examine the observable characteristics of the trades: intensity, frequency, and size. Trading intensity is defined as the total dollar value of trades in the year following the end of the performance period scaled by the prior-year total compensation. Internet Appendix Table IA-9 shows no significant discontinuity in trading intensity around the performance goal cutoff, suggesting that the observed discontinuity in abnormal insider trading profits is not driven by the quantity of trades. Similarly, the same table shows no significant discontinuity in either the number of trades or trade size, suggesting that goal-missing CEOs do not trade more frequently or place larger trades.

Second, I examine the timing of this behavior by analyzing the profitability of trades in each quarter within the one-year window after the performance period. The results, reported in Internet Appendix Table IA-10, show that goal-missing CEOs' abnormal insider trading profits are concentrated in the second and third quarters, while the first and fourth quarters show insignificant effects. This pattern is consistent with the fact that CEO pay is typically formally set or announced about 2–3 months after the performance period ends, so insider trades in the first quarter may not fully reflect CEOs' response to the newly set compensation. The result of

of significant findings across the rest of the panel, and the fact that this effect is in the opposite direction of theoretical expectations, I interpret this isolated result as likely spurious rather than indicative of a robust economic effect.

insignificant abnormal profits in the fourth quarter suggests that the behavior is temporary and does not persist beyond one year.

To shed further light on the timing of these trades, I follow Cohen et al. (2012) to distinguish between routine and non-routine trades, the latter of which are more likely to be motivated by private information. I infer routine and non-routine trades using the pattern of insider trades in the past. If a CEO places trades in the same calendar month for at least three consecutive years in the past, all her subsequent trades placed in that month are classified as routine trades. Trades placed in other months are classified as non-routine trades. Since this classification scheme requires a CEO to be in the insider trading database for at least three consecutive years before the year of the compensation shock, the sample size is reduced by about 45%. Internet Appendix Table IA-11 reports the regression results on the effect of missing relative performance goals on abnormal insider trading profits for routine and non-routine trades separately. The coefficient on the indicator for missing performance goals is around 0.12–0.14 and significant at the 10% level for non-routine trades, but it is close to zero and statistically insignificant for routine trades. These results suggest that CEOs who miss relative performance goals use non-routine trades to make up for the loss in compensation.[30]

[30]In untabulated results, I find no significant discontinuity in trading intensity for either routine or non-routine trades. This result is consistent with the earlier finding that higher insider trading profits is not driven by increased trading volume.

4. The Role of Board Monitoring and Information Frictions

To assess the role of governance and information frictions, I test whether stronger board monitoring or less information asymmetry reduces goal-missing CEOs' abnormal insider trading profits.

I use two measures to capture board monitoring. The first is the percentage of independent directors on the board, and the second is the fraction of co-opted directors. This second measure is defined as the fraction of directors on the full board who were appointed during the current CEO's tenure, following Coles et al. (2014). I partition the sample by the median of each of these two measures and repeat the baseline RD analysis. Panel A of Internet Appendix Table IA-12 reports the results. The Chi-squared tests show that the difference between the coefficients in each split is not statistically significant. Although the point estimates are smaller for firms with stronger board monitoring, i.e., high board independence and low board co-option, which appears consistent with board oversight being a deterrent of insider trading (e.g., Dai, Fu, Kang, and Lee (2016)), I lack the statistical power to conclude that this moderating effect is significant.

I use analyst coverage and firm size (market capitalization) as proxies for information asymmetry. Panel B of Internet Appendix Table IA-12 reports the results from this subsample analysis. Similar to the governance tests, the Chi-squared tests show that the differences between the coefficients are statistically insignificant for both proxies. The direction of the point estimates is mixed. While the RD estimate is smaller for firms with high analyst coverage (consistent with lower information asymmetry limiting insider trading profits), the result for firm size runs in the opposite direction.

Taken together, these results provide no robust evidence that either stronger governance or

reduced information frictions significantly moderate the insider trading response to missing a performance goal. The lack of statistical significance may be attributable to the limited sample size in these subsample analyses.

5. External Validity

While the RD design has strong internal validity, its external validity is generally limited because the estimation is based on a narrow bandwidth around the cutoff, as is the case with studies that employ RD designs. Thus, while the firms/CEOs in the $\pm 5\%$ sample are very similar to those in the full sample of firms with relative performance goals (as Table 1 shows), my findings on the effect of compensation shocks on insider trading profits may not generalize to similar firms away from the cutoff. Nevertheless, since firms with relative performance benchmarks tend to be better governed than those without such benchmarks (Gong, Li, and Shin (2011)), one might speculate that the effect of compensation shocks on abnormal insider trading profits would be stronger among firms that do not use relative performance goals in their compensation contracts.

VI. Conclusion

In this paper, I examine the effect of negative compensation shocks induced by missing relative performance goals on insider trading. Exploiting the discontinuity in the payout structure of CEOs' incentive grants based on relative performance goals, I first show that CEOs that just miss a relative performance goal, relative to those that just beat one, suffer a loss in incentive pay that is roughly 12% to 13% of their total compensation. I then show that CEOs that just miss a

relative performance goal, relative to those that just beat one, make higher abnormal profits from their insider trades following the performance period end, amounting to around 8% of their total compensation. Thus, managers use insider trading to make up for over half of the loss in compensation due to missing performance goals. I also find evidence that managers that narrowly miss a relative performance goal become less likely to make voluntary guidance disclosures, which could increase managers' informational advantage over outside investors.

My paper contributes to our understanding of the efficacy of performance-based compensation. Performance-based awards are designed to incentivize executives to improve shareholder value by increasing the pay-performance sensitivity. If managers can make up their compensation loss due to missing performance goals through higher abnormal profits from insider trading, it could reduce managers' effort provision and mitigate the incentive effect of performance-based pay. Therefore, closer scrutiny of managerial actions, such as insider trading, may be necessary to improve the effectiveness of performance-based pay.

Appendix A. Variable Definitions

This Appendix defines the variables used in the primary analyses.

Variable	Definition	Data Source
Actual minus goal	The difference between a firm's actual performance and its corresponding relative performance goal. For detailed variable construction, please refer to Section III.B.	CRSP/Compustat: actual performance. Incentive Lab: performance goals.
Abnormal incentive pay	The difference between actual incentive compensation earned and the pre-defined incentive compensation payout specified in the grant for meeting the performance goal, divided by the total compensation in the year ending 12 months before the performance period end.	Incentive Lab
Abnormal insider trading profits	The total dollar amount of abnormal profits of all trades placed by the CEO during a 12-month period following the performance period end divided by the total compensation in the year ending 12 months before the performance period end.	Thomson Reuters
Firm-level variables		
Total assets ($ millions)	The book value of total assets of the firm.	Compustat
Market-to-book	The market value of common equity divided by the book value of common equity.	Compustat
Leverage	The ratio of total assets minus the book value of common stock and deferred taxes to total assets.	Compustat
Return on assets	The ratio of income before extraordinary items to the book value of total assets.	Compustat

Investments/Assets	The ratio of the sum of capital expenditure and R&D expenses to total assets.	Compustat
Number of analysts	The number of analysts making quarterly forecasts for earnings per share of the firm's stock.	I/B/E/S
Institutional ownership	The number of shares held by institutional investors as a fraction of the number of shares outstanding.	Thomson Reuters
Prior 1-year abnormal stock return	The cumulative market-adjusted stock return in the 12-month period ending on the performance period-end date.	CRSP
Prior 3-year abnormal stock return	The cumulative market-adjusted stock return in the 36-month period ending on the performance period-end date.	CRSP
Repurchases/Assets	The dollar value of share repurchases divided by lagged total assets. Repurchase is defined following Fama and French (2001).	Compustat
Discretionary accruals	Calculated using modified Jones model with ROA (following Kothari, Leone, and Wasley (2005)).	Compustat
CEO-level variables		
CEO age	The age of the CEO as of the performance period end.	ExecuComp
CEO tenure (years)	The number of full years between the individual's start date as a CEO and the performance period end.	ExecuComp
Total compensation ($ thousands)	The total compensation as reported in SEC filings.	Incentive Lab
Stock awards/Total comp.	The dollar value of stock-based compensation divided by the reported value of total compensation.	Incentive Lab
CEO ownership (%)	The ratio of the total number of shares owned by the CEO to the total number of shares outstanding.	Incentive Lab
Stock-based wealth ($ thousands)	The dollar value of the CEO's common stock holdings in the firm.	Incentive Lab
No. of guidance disclosures	The number of guidance disclosures issued during a 12-month period following a performance period end.	I/B/E/S

Profit margin	Earnings before interest and taxes divided by sales in the fiscal year following the performance period end.	Compustat
Routine trades	Following Cohen et al. (2012), if a CEO places trades in the same calendar month for at least three consecutive years in the past, all her subsequent trades placed in that month are classified as routine trades.	Thomson Reuters
Insider selling	The dollar value of a CEO's insider sales during a 12-month period following the performance period end divided by lagged total compensation.	Thomson Reuters
Tax-loss selling	The dollar value of stock sold in December with negative 12-month past returns is calculated to approximate tax-loss selling, following Grinblatt and Keloharju (2001).	Thomson Reuters
Forced Turnover	An indicator that equals one if the CEO is forced out in the year immediately following the performance period end and zero otherwise, following the measure of Parrino (1997) or from the CEO dismissal database constructed by Gentry et al. (2021).	Incentive Lab
Discretionary bonuses	Following Ederhof (2010), discretionary bonuses filed on 8-K filings and the proxy statements (i.e., DEF 14A) within two years following the performance period end, scaled by the total compensation in the year prior to the performance period.	SEC Edgar filings
Other compensation	The item "all other compensation" reported in the Summary Compensation Table in proxy statements, scaled by the total compensation in the year prior to the performance period.	Incentive Lab

Social ties	Compensation committee with at least one member sharing overlapping university education or prior employment with the CEO, following Cohen et al. (2008) and Hwang and Kim (2009).	BoardEx and web searches
Co-option	The fraction of co-opted directors on the compensation committee. Following Coles et al. (2014), co-opted directors are defined as those who are appointed during the current CEO's tenure.	ExecuComp, BoardEx, and web searches

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FIGURE 1

Distribution of Relative Performance Grants Around the Cutoff

This figure plots the number of relative performance grants as a function of the running variable, *Actual minus goal*, i.e., the difference between actual performance and the corresponding relative performance goal, in a 10% bandwidth around the cutoff of zero. Positive (negative) values on the horizontal axis indicate CEOs that beat (miss) their relative performance goals.

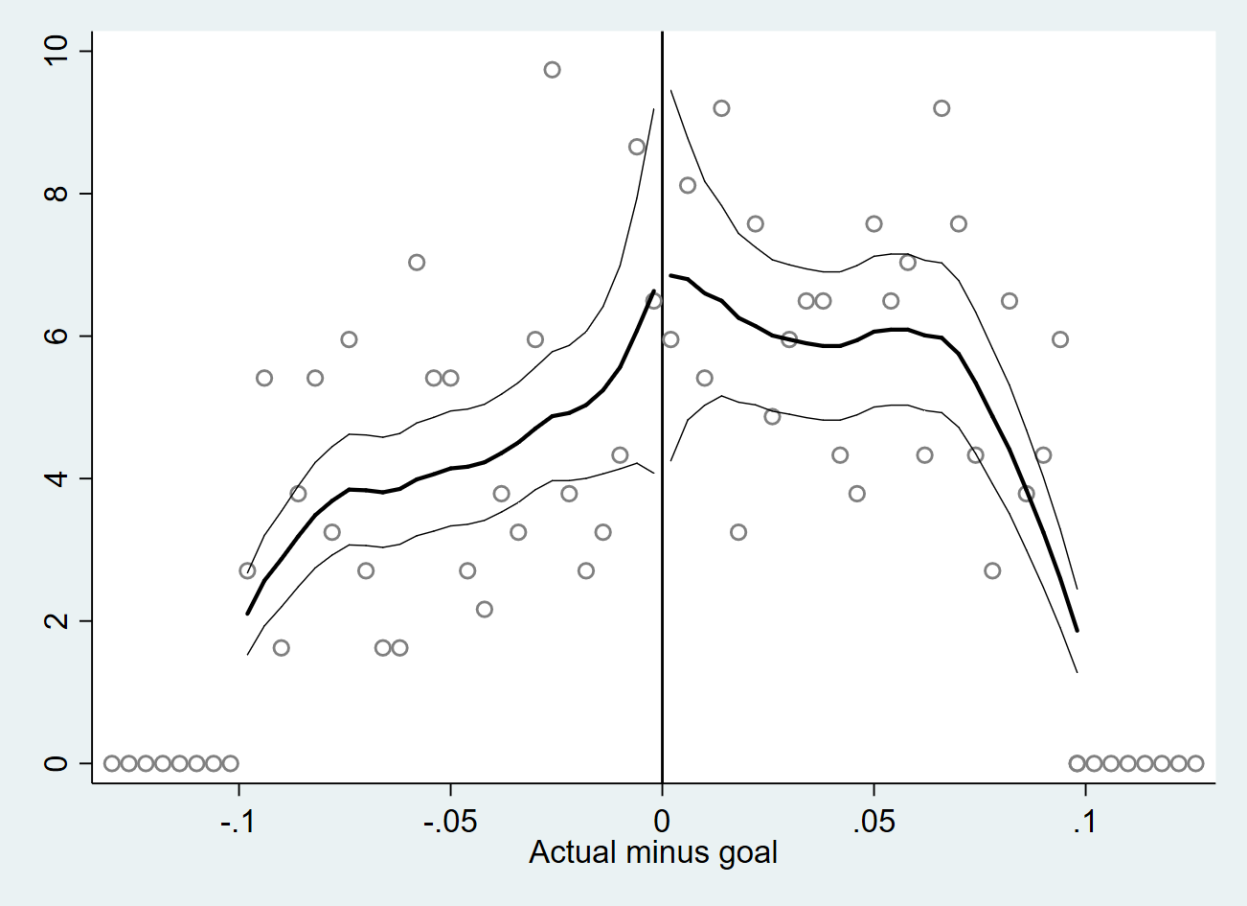

FIGURE 2

Abnormal Performance-Based Compensation Around the Cutoff

This figure plots abnormal incentive pay as a function of the running variable, *Actual minus goal*, i.e., the difference between actual performance and the corresponding relative performance goal, in a 10% bandwidth around the cutoff of zero. Positive (negative) values on the horizontal axis indicate CEOs that beat (miss) their relative performance goals.

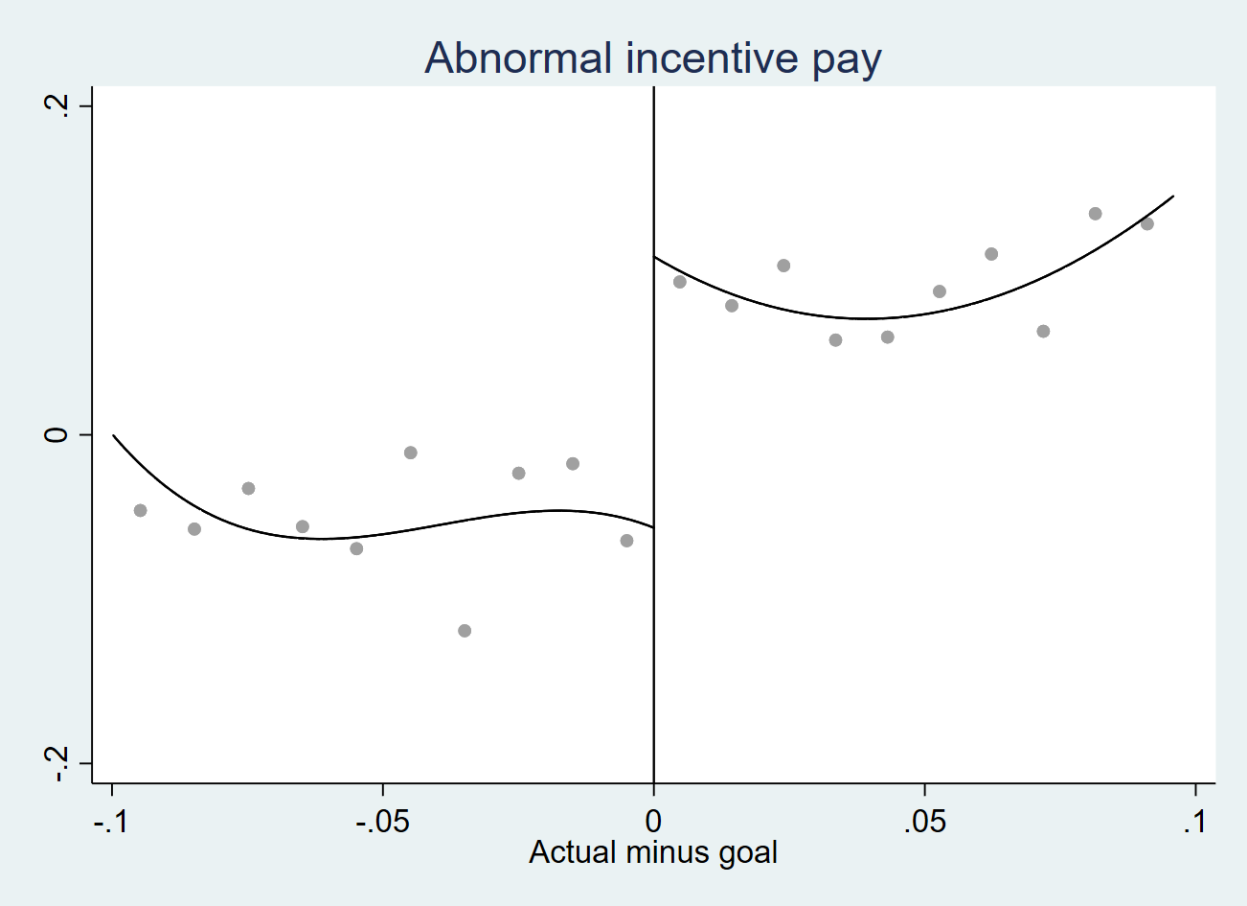

FIGURE 3

Abnormal Insider Trading Profits Around the Cutoff

This figure plots abnormal insider trading profits as a function of the running variable, *Actual minus goal*, i.e., the difference between actual performance and the corresponding relative performance goal, in a 10% bandwidth around the cutoff of zero. Positive (negative) values on the horizontal axis indicate CEOs that beat (miss) their relative performance goals.

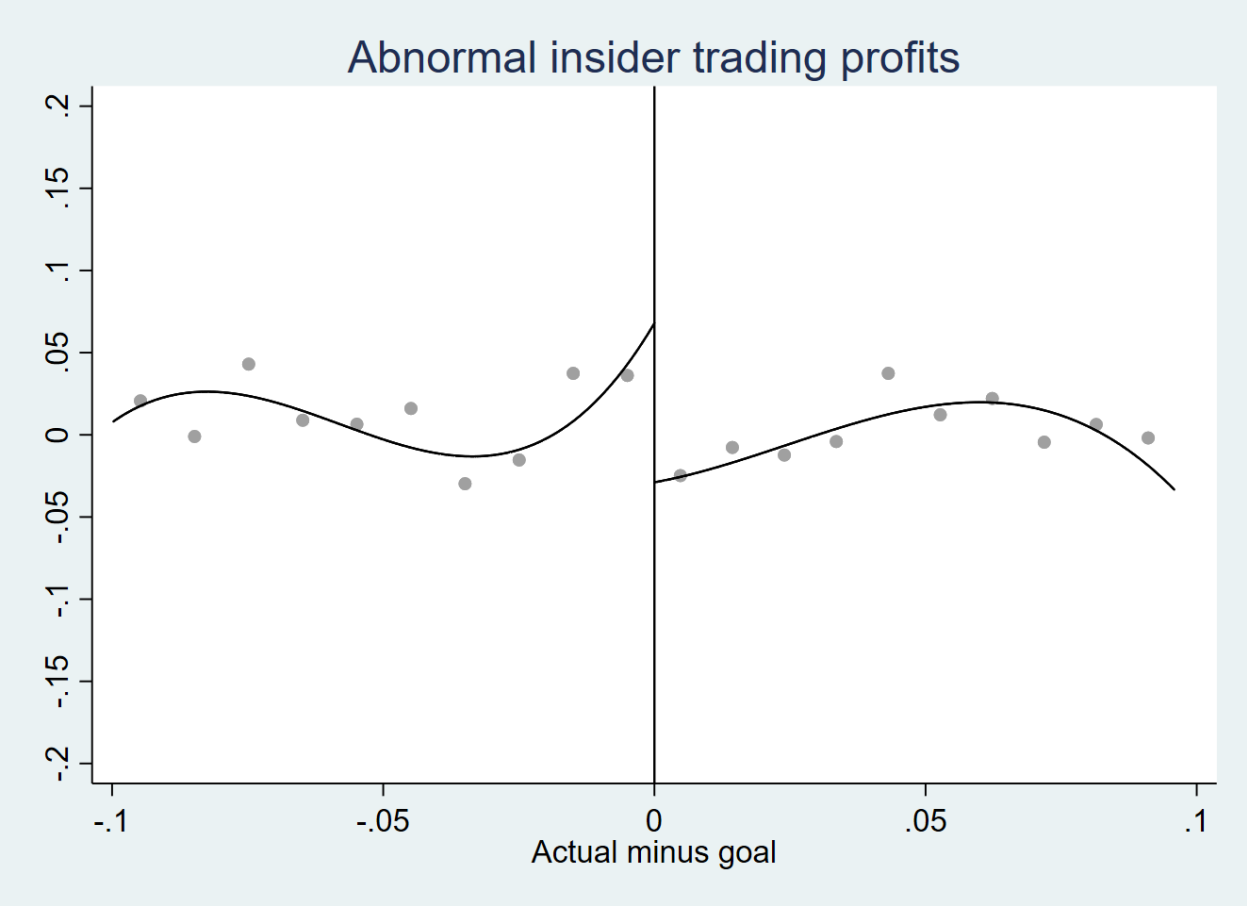

Table 1: Summary statistics

This table reports the summary statistics of main variables and control variables for the full sample (Panel A) and the ±5% sample (Panel B). See Appendix A for the definition of variables. All firm and CEO characteristics except prior stock returns, share repurchases, discretionary accruals, CEO age, and CEO tenure are measured in the fiscal year ending 12 months before the performance period end. I measure prior stock returns, share repurchases, discretionary accruals, CEO age, and CEO tenure as of the end of the performance period. I winsorize the control variables at the 2.5% and 97.5% levels to minimize the effect of outliers.

Panel A: Full sample

	N	Mean	SD	P10	P50	P90
Main variables						
Actual minus goal	1,314	0.08	0.31	-0.28	0.04	0.53
Abnormal incentive pay	1,317	5.35%	21.60%	-20.65%	1.51%	37.83%
Abnormal insider trading profits	1,316	0.56%	6.70%	-6.92%	0.00%	8.49%
Control variables						
Total assets ($ millions)	1,317	27,390	43,905	2,249	9,676	67,801
Market-to-book	1,317	2.50	2.03	0.83	1.78	5.36
Leverage	1,317	0.59	0.17	0.38	0.59	0.86
Return on assets	1,317	0.04	0.05	0.00	0.04	0.11
Investments/Assets	1,317	0.07	0.06	0.00	0.06	0.15
Number of analysts	1,317	9.23	7.71	2.00	7.00	21.00
Institutional ownership	1,317	0.73	0.24	0.46	0.79	0.96
Prior 1*-year abnormal stock return*	1,317	0.03	0.25	-0.27	0.03	0.33
Prior 3*-year abnormal stock return*	1,317	0.10	0.40	-0.41	0.10	0.60
Repurchases/Assets	1,317	0.02	0.03	0.00	0.00	0.07
Discretionary accruals	1,317	-0.01	0.14	-0.17	-0.00	0.13
CEO age	1,302	56.41	5.25	49.00	57.00	63.00
CEO tenure (years)	1,303	6.14	4.77	1	5	13
Total compensation ($ thousands)	1,317	9,686	7,227	2,801	7,718	20,700
Stock awards/Total comp.	1,315	0.39	0.20	0.13	0.39	0.66
CEO ownership (%)	1,300	0.73	1.26	0.07	0.32	1.52
Stock-based wealth ($ thousands)	1,300	58,553	76,640	4,174	27,545	174,412

Panel B: ±5% sample

	N	Mean	SD	P10	P50	P90
Main variables						
Actual minus goal	242	0.00	0.03	-0.03	0.01	0.04
Abnormal incentive pay	242	2.31%	15.89%	-14.63%	0.54%	22.75%
Abnormal insider trading profits	242	0.00%	14.86%	-8.42%	0.00%	6.72%
Control variables						
Total assets ($ millions)	242	36,374	54,024	3,853	12,827	102,908
Market-to-book	242	2.41	1.90	0.89	1.74	5.05
Leverage	242	0.59	0.17	0.39	0.58	0.84
Return on assets	242	0.04	0.04	0.00	0.04	0.10
Investments/Assets	242	0.07	0.06	0.00	0.06	0.14
Number of analysts	242	9.05	7.61	2.00	7.00	21.00
Institutional ownership	242	0.72	0.23	0.46	0.75	0.97
Prior 1*-year abnormal stock return*	242	-0.01	0.18	-0.22	-0.02	0.21
Prior 3*-year abnormal stock return*	242	0.08	0.31	-0.26	0.06	0.49
Repurchases/Assets	242	0.01	0.02	0.00	0.00	0.04
Discretionary accruals	242	0.00	0.11	-0.10	0.01	0.11
CEO age	242	56.43	5.17	49.00	57.00	63.00
CEO tenure (years)	242	6.05	4.70	1	5	13
Total compensation ($ thousands)	242	10,300	8,100	2,629	7,795	25,000
Stock awards/Total comp.	242	0.37	0.20	0.11	0.35	0.63
CEO ownership (%)	242	0.63	1.13	0.05	0.29	1.48
Stock based wealth ($ thousands)	242	68,519	85,250	4,456	31,835	215,549

Table 2: Distribution of firm and CEO characteristics around the cutoff

This table shows the distribution of firm and CEO characteristics in a range of 5% on each side of the cutoff for the running variable, i.e., *Actual minus goal*. See Appendix A for the definition of variables. The first column reports the coefficient on the indicator for missing relative performance goals, i.e., I_{miss}, and the corresponding standard errors (in parentheses) from a linear regression of each characteristic on I_{miss}, *Actual minus goal*, I_{miss}×*Actual minus goal*, and industry and year fixed effects using a 5% bandwidth. The second and third columns report the means of each characteristic for firms that are to the left of the cutoff and those that are to the right within the 5% bandwidth, and the last column reports the *p*-values for differences in the means between the two groups.

	RD estimate	[-5%, 0) Mean	[0, +5%] Mean	Diff. in means *p-value*
Log(*Total assets*)	-0.16	23.44	23.53	0.59
	(0.26)			
Log(*Market-to-book*)	0.17	0.61	0.68	0.38
	(0.12)			
Leverage	0.04	0.59	0.59	0.93
	(0.03)			
Return on assets	0.00	0.04	0.05	0.38
	(0.01)			
Investments/Assets	0.00	0.07	0.06	0.28
	(0.01)			
Log(1+*Number of analysts*)	-0.08	2.06	2.00	0.53
	(0.15)			
Institutional ownership	0.03	0.71	0.72	0.67
	(0.05)			
Prior 1-*year abn. stock return*	0.01	-0.03	0.00	0.23
	(0.04)			
Prior 3-*year abn. stock return*	-0.05	0.05	0.10	0.16
	(0.07)			
Repurchases/Assets	0.00	0.01	0.01	0.47
	(0.00)			
Discretionary accruals	-0.03	0.00	0.00	0.91
	(0.03)			
CEO age	-2.08	56.40	56.45	0.94
	(1.29)			
CEO tenure (years)	-0.43	6.31	5.85	0.45
	(1.20)			
Log(*Total compensation*)	-0.15	15.77	15.90	0.20
	(0.18)			
Stock awards/Total comp.	-0.02	0.35	0.39	0.19
	(0.05)			
CEO ownership (%)	0.45	0.60	0.66	0.69
	(0.29)			
Log(*Stock-based wealth*)	0.21	17.13	17.29	0.39
	(0.34)			

Table 3: The impact of missing relative performance goals on abnormal incentive pay

This table reports the regression results estimating Eq. (1) on the discontinuity in abnormal incentive pay around relative performance goals with a bandwidth of 5%. The variable of interest is the I_{miss} indicator, which equals one for CEOs who miss their relative performance goals and zero otherwise. See Appendix A for the definition of variables. Numbers in parentheses are standard errors clustered by firm. Significance at the 10% (*), 5% (**), or 1% level (***) is indicated.

Dependent =	*Abnormal incentive pay*		
	(1)	(2)	(3)
I_{miss}	-0.131	-0.130	-0.123
	(0.032)***	(0.034)***	(0.036)***
Actual minus goal	-0.237	-0.195	-0.502
	(0.818)	(0.855)	(0.818)
I_{miss} × *Actual minus goal*	0.300	0.134	0.863
	(1.060)	(1.107)	(1.133)
Log(*Total assets*)			0.009
			(0.014)
Log(*Market-to-book*)			0.013
			(0.024)
Leverage			0.012
			(0.087)
Return on assets			0.315
			(0.336)
Investments/Assets			-0.206
			(0.172)
Log(1+*Number of analysts*)			0.009
			(0.019)
Institutional ownership			-0.036
			(0.054)
Prior 1-*year abn. stock return*			0.114
			(0.060)*
Prior 3-*year abn. stock return*			-0.086
			(0.056)
Repurchases/Assets			-0.279
			(0.415)
Discretionary accruals			-0.023
			(0.089)
CEO age			0.003
			(0.003)
CEO tenure			-0.002
			(0.004)
Log(*Total compensation*)			-0.007
			(0.021)
Stock awards/Total comp.			0.135
			(0.067)**
CEO ownership			0.596
			(1.205)
Log(*Stock-based wealth*)			0.005
			(0.015)
Year FEs	No	Yes	Yes
Contract type FEs	No	Yes	Yes
No. of observations	242	242	242
R^2	0.16	0.23	0.30

Table 4: The effect of missing relative performance goals on abnormal insider trading profits

This table reports the regression results estimating Eq. (2) on the discontinuity in abnormal insider trading profits around relative performance goals with a bandwidth of 5%. The variable of interest is the I_{miss} indicator, which equals one for CEOs who miss their relative performance goals and zero otherwise. See Appendix A for the definition of variables. Numbers in parentheses are standard errors clustered by firm. Significance at the 10% (*), 5% (**), or 1% level (***) is indicated.

Dependent =	*Abnormal insider trading profits*		
	(1)	(2)	(3)
I_{miss}	0.077 (0.033)**	0.083 (0.030)***	0.085 (0.032)***
Actual minus goal	1.146 (0.675)*	1.392 (0.698)**	1.457 (0.752)*
I_{miss} × *Actual minus goal*	0.539 (1.206)	0.118 (1.063)	0.102 (0.955)
Log(*Total assets*)			0.007 (0.010)
Log(*Market-to-book*)			0.002 (0.025)
Leverage			-0.164 (0.078)**
Return on assets			-0.275 (0.263)
Investments/Assets			0.089 (0.132)
Log(1+*Number of analysts*)			0.003 (0.015)
Institutional ownership			-0.004 (0.037)
Prior 1-*year abn. stock return*			-0.060 (0.060)
Prior 3-*year abn. stock return*			0.069 (0.060)
Repurchases/Assets			-0.249 (0.416)
Discretionary accruals			0.060 (0.073)
CEO age			-0.003 (0.002)*
CEO tenure			0.000 (0.003)
Log(*Total compensation*)			-0.025 (0.018)
Stock awards/Total comp.			0.048 (0.067)
CEO ownership			-0.235 (0.957)
Log(*Stock-based wealth*)			0.021 (0.010)**
Year FEs	No	Yes	Yes
Contract type FEs	No	Yes	Yes
No. of observations	242	242	242
R^2	0.02	0.11	0.18

Table 5: Cash versus equity awards

This table presents the regression results estimating Eq. (2) on the effect of missing relative performance goals on abnormal insider trading profits for the subsample of cash awards and that of equity awards separately using a bandwidth of 5%. The variable of interest is the I_{miss} indicator, which equals one for CEOs who miss their relative performance goals and zero otherwise. The coefficient estimates for the control variables are omitted for brevity. Numbers in parentheses are standard errors clustered by firm. Significance at the 10% (*), 5% (**), or 1% level (***) is indicated.

Dependent =	*Abnormal insider trading profits*			
	Cash Awards		Equity Awards	
	(1)	(2)	(3)	(4)
I_{miss}	0.078	0.108	0.087	0.080
	(0.054)	(0.060)*	(0.035)**	(0.038)**
Controls	No	Yes	No	Yes
Year FEs	Yes	Yes	Yes	Yes
Contract type FEs	Yes	Yes	Yes	Yes
No. of observations	57	57	185	185
R^2	0.24	0.74	0.14	0.21

Table 6: The effect of missing relative performance goals on manager disclosure choices

This table reports the regression analysis of the effect of missing relative performance goals on earnings and sales guidance disclosures by management. The dependent variable in the first two columns is the logarithm of one plus the number of guidance disclosures issued during a 12-month period following a performance period end, and that in the last two columns is an indicator for whether the firm issues guidance disclosures in the 12-month period. The coefficient estimates for the control variables are omitted for brevity. Numbers in parentheses are standard errors clustered by firm. Significance at the 10% (*), 5% (**), or 1% level (***) is indicated.

Dependent =	*Log*(1+*No. of guidance disclosures*)		*Indicator for guidance disclosures*	
	(1)	(2)	(3)	(4)
I_{miss}	-0.561 (0.282)**	-0.516 (0.275)*	-0.270 (0.118)**	-0.250 (0.122)**
Controls	No	Yes	No	Yes
Year FEs	Yes	Yes	Yes	Yes
Contract type FEs	Yes	Yes	Yes	Yes
No. of observations	242	242	242	242
R^2	0.08	0.20	0.08	0.20

Table 7: Pre-existing differences in abnormal trading profits

This table reports regression analysis of pre-existing differences in abnormal insider trading profits. I use a regression specification similar to Eq. (2), replacing abnormal insider trading profits in year t + 1 with those in years t, $t-1$, and $t-2$, respectively. The variable of interest is the I_{miss} indicator, which equals one if the CEO misses relative performance goals in year t and zero otherwise. Numbers in parentheses are standard errors clustered by firm. Significance at the 10% (*), 5% (**), or 1% level (***) is indicated.

Dependent =	*Abnormal insider trading profits in year*		
	t	$t-1$	$t-2$
I_{miss}	-0.018	0.012	-0.013
	(0.017)	(0.017)	(0.021)
Controls	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes
Contract type FEs	Yes	Yes	Yes
No. of observations	242	240	232
R^2	0.22	0.22	0.15

Table 8: Placebo tests using awards without discontinuity in the payout structure around relative performance goals

This table reports the regression analysis of the effects of missing relative performance goals on abnormal incentive pay and abnormal insider trading profits using a sample of awards without discontinuity in the payout structure around relative performance goals. The sample is restricted to a bandwidth of ±5% around relative performance goals. Columns (1) and (2) present the regression results estimating Eq. (1) on the effect of missing relative performance goals on abnormal incentive pay. Columns (3) and (4) present the regression results estimating Eq. (2) on the effect of missing relative performance goals on abnormal insider trading profits. The variable of interest is the I_{miss} indicator, which equals one for CEOs who miss their relative performance goals and zero otherwise. The coefficient estimates for the control variables are omitted for brevity. Numbers in parentheses are standard errors clustered by firm. Significance at the 10% (*), 5% (**), or 1% level (***) is indicated.

Dependent =	*Abnormal incentive pay*		*Abnormal insider trading profits*	
	(1)	(2)	(3)	(4)
I_{miss}	-0.020	-0.040	-0.003	-0.031
	(0.031)	(0.042)	(0.026)	(0.028)
Controls	No	Yes	No	Yes
Year FEs	Yes	Yes	Yes	Yes
Contract type FEs	Yes	Yes	Yes	Yes
No. of observations	96	96	96	96
R^2	0.18	0.43	0.12	0.42

Table 9: Alternative bandwidths

This table presents the regression analysis of the effects of missing relative performance goals on abnormal incentive pay (Panel A) and abnormal insider trading profits (Panel B) using alternative bandwidths. I use piecewise linear polynomials for bandwidths of 3% and 5%, and piecewise cubic polynomials for bandwidths greater than 5%. The variable of interest is the I_{miss} indicator, which equals one for CEOs who miss their relative performance goals and zero otherwise. Numbers in parentheses are standard errors clustered by firm. Significance at the 10% (*), 5% (**), or 1% level (***) is indicated.

	[-0.03, +0.03]	[-0.05, +0.05]	[-0.07, +0.07]	[-0.09, +0.09]	[-0.15, +0.15]
	(1)	(2)	(3)	(4)	(5)
Panel A: Abnormal incentive pay					
I_{miss}	-0.111	-0.123	-0.142	-0.116	-0.088
	(0.041)***	(0.036)***	(0.045)***	(0.040)***	(0.035)**
Controls	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes
Contract type FEs	Yes	Yes	Yes	Yes	Yes
No. of observations	163	242	347	427	624
R^2	0.35	0.30	0.22	0.22	0.14
Panel B: Abnormal insider trading profits					
I_{miss}	0.080	0.084	0.084	0.089	0.054
	(0.032)**	(0.032)**	(0.047)*	(0.042)**	(0.028)**
Controls	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes
Contract type FEs	Yes	Yes	Yes	Yes	Yes
No. of observations	163	242	347	427	624
R^2	0.28	0.18	0.15	0.15	0.11

Table 10: Nonparametric regressions

This table reports nonparametric RD analysis of the effects of missing relative performance goals on abnormal incentive pay (Panel A) and abnormal insider trading profits (Panel B). Following Calonico et al. (2014) and Calonico et al. (2017), I perform nonparametric estimations using three different procedures, i.e., conventional RD estimates with a conventional variance estimator (Conventional), bias-corrected RD estimates with a conventional variance estimator (Bias-corrected), and bias-corrected RD estimates with a robust variance estimator (Robust), all with triangular kernel functions.

Panel A: Abnormal incentive pay

Dependent =	*Abnormal incentive pay*			
Method	Coef.	Std. Err.	|z|	P>|z|
Conventional	-0.141	0.027	5.188	0.000
Bias-corrected	-0.145	0.027	5.338	0.000
Robust	-0.145	0.032	4.488	0.000

Panel B: Abnormal insider trading profits

Dependent =	*Abnormal insider trading profits*			
Method	Coef.	Std. Err.	|z|	P>|z|
Conventional	0.081	0.040	2.042	0.041
Bias-corrected	0.095	0.040	2.394	0.017
Robust	0.095	0.046	2.065	0.039

Table 11: Alternative measures of abnormal insider trading profits

This table presents the regression analysis of the effect of missing relative performance goals on abnormal insider trading profits using alternative benchmarks to adjust returns (Panel A) and alternative windows for measuring trading profits (Panel B). All regressions include year fixed effects and contract type fixed effects. Numbers in parentheses are standard errors clustered by firm. Significance at the 10% (*), 5% (**), or 1% level (***) is indicated.

Panel A: Alternative benchmarks to adjust returns

	DGTW	Size-adjusted	Industry-adjusted
I_{miss}	0.068 (0.030)**	0.085 (0.032)***	0.103 (0.046)**
Controls	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes
Contract type FEs	Yes	Yes	Yes
No. of observations	241	242	242
R^2	0.18	0.18	0.16

Panel B: Alternative windows for measuring trading profits

	21 days	63 days	252 days
I_{miss}	0.016 (0.008)*	0.032 (0.017)*	0.143 (0.044)***
Controls	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes
Contract type FEs	Yes	Yes	Yes
No. of observations	242	242	242
R^2	0.12	0.12	0.21

Table 12: Subsequent stock and operating performance

This table reports regression analysis of the impact of missing relative performance goals on firms' subsequent stock and operating performance using a bandwidth of 5%. 1-*year (*3-*year) abnormal stock return* is the market-adjusted stock return in the 12 (36) months immediately following the performance period end. *ROA* is income before extraordinary items divided by the book value of total assets in the fiscal year subsequent to the performance period end. *Profit margin* is earnings before interest and taxes divided by sales in the fiscal year following the performance period end. All regressions include year fixed effects and contract type fixed effects. Numbers in parentheses are standard errors clustered by firm. Significance at the 10% (*), 5% (**), or 1% level (***) is indicated.

Dependent =	*1-year abnormal stock return*	*3-year abnormal stock return*	*ROA*	*Profit margin*
I_{miss}	-0.076	-0.030	0.001	-0.001
	(0.055)	(0.091)	(0.009)	(0.026)
Controls	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
Contract type FEs	Yes	Yes	Yes	Yes
No. of observations	242	242	238	238
R^2	0.17	0.33	0.45	0.31

INTERNET APPENDIX FOR

Get the Money Somehow: The Effect of Missing Performance Goals on Insider Trading

(Not Intended for Publication)

Table IA-1: Construction of sample

This table details the construction of the final sample of 1,317 performance-based grants. The initial sample includes all grants from the Incentive Lab database for the period 2006–2016. Each subsequent row shows the number of observations removed due to specific data requirements.

Sampling steps		No. of obs.
Initial sample		4,205
less	incomplete grant data	-249
less	incomplete peer data	-1,808
less	grants without discontinuous payout structure	-710
less	missing compensation data	-121
Final sample		1,317

Table IA-2: Multiple versus single grants

This table reports regression estimates of the effect of narrowly missing a relative performance goal on *Abnormal incentive pay*, estimated separately for CEOs with multiple performance-based grants (columns 1 and 2) and those with a single grant (columns 3 and 4) within the ±5% bandwidth. The variable of interest is the I_{miss} indicator, which equals one for CEOs who miss their relative performance goals and zero otherwise. Numbers in parentheses are standard errors clustered by firm. Significance at the 10% (*), 5% (**), or 1% level (***) is indicated.

Dependent =	*Abnormal incentive pay*			
	Multiple grants		Single grants	
	(1)	(2)	(3)	(4)
I_{miss}	-0.137	-0.140	-0.101	-0.198
	(0.034)***	(0.037)***	(0.082)	(0.115)
Year FEs	No	Yes	No	Yes
Contract type FEs	No	Yes	No	Yes
No. of observations	214	214	28	27
R^2	0.17	0.24	0.12	0.30

Table IA-3: Size of prespecified awards

This table presents the regression results estimating Eq. (2) on the effect of missing relative performance goals on abnormal insider trading profits for two subsamples partitioned by the size of the prespecified award associated with beating the goals using a bandwidth of 5%. I split the sample into high and low groups based on the median of the value of the prespecified award associated with beating the goal as a fraction of the CEO's lagged total compensation. The variable of interest is the I_{miss} indicator, which equals one for CEOs who miss their relative performance goals and zero otherwise. The coefficient estimates for the control variables are omitted for brevity. Numbers in parentheses are standard errors clustered by firm. Significance at the 10% (*), 5% (**), or 1% level (***) is indicated.

Dependent =	*Abnormal insider trading profits*			
	Low prespecified award value		High prespecified award value	
	(1)	(2)	(3)	(4)
I_{miss}	0.043	0.041	0.094	0.110
	(0.020)**	(0.018)**	(0.042)**	(0.056)*
Controls	No	Yes	No	Yes
Year FEs	Yes	Yes	Yes	Yes
Contract type FEs	Yes	Yes	Yes	Yes
No. of observations	119	119	122	122
R^2	0.26	0.38	0.14	0.28

Table IA-4: Quantile regressions

This table reports fixed-effects quantile regression analysis of the effects of missing relative performance goals on abnormal incentive pay (Panel A) and abnormal insider trading profits (Panel B). I perform fixed-effects quantile estimations (following Machado and Silva, 2019) at the 10th, 25th, 50th, 75th, and 90th percentiles.

	10^{th}	25^{th}	50^{th}	75^{th}	90^{th}
	(1)	(2)	(3)	(4)	(5)
Panel A: Abnormal incentive pay					
I_{miss}	-0.104	-0.112	-0.123	-0.133	-0.144
	(0.051)**	(0.041)***	(0.034)***	(0.035)***	(0.045)***
Year FEs	Yes	Yes	Yes	Yes	Yes
Contract type FEs	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
No. of observations	242	242	242	242	242
Panel B: Abnormal insider trading profits					
I_{miss}	0.070	0.078	0.085	0.090	0.097
	(0.040)*	(0.032)**	(0.030)***	(0.033)***	(0.042)**
Year FEs	Yes	Yes	Yes	Yes	Yes
Contract type FEs	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
No. of observations	242	242	242	242	242

Table IA-5: Adding quadratic terms

This table augments the baseline RD specifications by including quadratic terms in the running variable. Columns (1) and (2) use *Abnormal Incentive Pay* as the dependent variable, and columns (3) and (4) use *Abnormal Insider Trading Profits*. The variable of interest is the I_{miss} indicator, which equals one for CEOs who miss their relative performance goals and zero otherwise. Numbers in parentheses are standard errors clustered by firm. Significance at the 10% (*), 5% (**), or 1% level (***) is indicated.

Dependent =	*Abnormal incentive pay*		*Abnormal insider trading profits*	
	(1)	(2)	(3)	(4)
I_{miss}	-0.108	-0.111	0.097	0.102
	(0.038)***	(0.040)***	(0.040)**	(0.044)**
Actual minus goal	1.945	0.923	0.555	0.734
	(1.462)	(1.617)	(1.249)	(1.376)
$I_{miss} \times$ *Actual minus goal*	0.060	0.441	3.505	3.910
	(3.841)	(3.976)	(3.101)	(3.374)
Actual minus goal^2	-54.171	-35.758	19.112	15.089
	(29.983)*	(33.997)	(25.933)	(28.870)
$I_{miss} \times$ *Actual minus goal*^{2}	97.091	56.325	36.346	52.274
	(65.035)	(66.316)	(58.316)	(71.090)
Controls	No	Yes	No	Yes
Year FEs	Yes	Yes	Yes	Yes
Contract type FEs	Yes	Yes	Yes	Yes
No. of observations	242	242	242	242
R^2	0.24	0.30	0.12	0.18

Table IA-6: Alternative interpretations: Liquidity constraint and tax-loss selling

This table reports the regression analysis of the effects of missing relative performance goals on CEOs' insider sales and tax-loss selling. The dependent variable in the first two columns, *Insider selling*, is the dollar value of a CEO's insider sales during a 12-month period following the performance period end divided by lagged total compensation. The dependent variable in the last two columns, *Tax-loss selling*, is the dollar value of stock sold by a CEO in December with negative past 12-month returns divided by lagged total compensation. The variable of interest is the I_{miss} indicator, which equals one for CEOs who miss their relative performance goals and zero otherwise. The coefficient estimates for the control variables are omitted for brevity. Numbers in parentheses are standard errors clustered by firm. Significance at the 10% (*), 5% (**), or 1% level (***) is indicated.

Dependent =	*Insider selling*		*Tax-loss selling*	
	(1)	(2)	(3)	(4)
I_{miss}	-0.154	-0.253	0.004	-0.006
	(0.234)	(0.222)	(0.028)	(0.024)
Controls	No	Yes	No	Yes
Year FEs	Yes	Yes	Yes	Yes
Contract type FEs	Yes	Yes	Yes	Yes
No. of observations	242	242	242	242
R^2	0.11	0.31	0.07	0.13

Table IA-7: Subsequent CEO turnover

This table reports regression analysis of the impact of missing relative performance goals on CEO turnover using a bandwidth of 5%. The dependent variable, *Forced Turnover*, is an indicator that equals one if the CEO is forced out in the year immediately following the performance period end and zero otherwise. The first two columns use the forced turnover measure of Parrino (1997), and the last two columns use the measure from the CEO dismissal database constructed by Gentry, Harrison, Quigley, and Boivie (2021). The variable of interest is the I_{miss} indicator, which equals one for CEOs who miss their relative performance goals and zero otherwise. The coefficient estimates for the control variables are omitted for brevity. Numbers in parentheses are standard errors clustered by firm. Significance at the 10% (*), 5% (**), or 1% level (***) is indicated.

Dependent =	*Forced Turnover*			
	(1)	(2)	(3)	(4)
I_{miss}	-0.013	-0.013	-0.050	-0.044
	(0.037)	(0.039)	(0.043)	(0.045)
Controls	No	Yes	No	Yes
Year FEs	Yes	Yes	Yes	Yes
Contract type FEs	Yes	Yes	Yes	Yes
No. of observations	242	242	242	242
R^2	0.07	0.14	0.06	0.16

Table IA-8: Discretionary bonuses and other compensation

This table presents the regression analysis of the effects of missing relative performance goals on discretionary pay awarded to CEOs. The dependent variables are *Discretionary bonuses*, identified using keyword-based text analysis of DEF 14A and 8-K filings, and *Other compensation*, which is "All other compensation" reported in the Summary Compensation Table in proxy statements (Incentive Lab item, OTHERCOMP), both scaled by the total compensation in the year prior to the performance period. Panel A uses the baseline ±5% sample, and Panel B splits the sample by the presence of social ties between the CEO and the compensation committee members (defined as overlapping university education or prior employment, following Cohen, Frazzini, and Malloy, 2008) and by the level of committee co-option (the fraction of compensation committee members appointed during the CEO's tenure, following Coles, Daniel, and Naveen, 2014). The variable of interest is the I_{miss} indicator, which equals one for CEOs who miss their relative performance goals and zero otherwise. Numbers in parentheses are standard errors clustered by firm. The last row in Panel B reports the *p*-value of a Chi-squared test for the difference between the RD estimates across the two subsamples. Significance at the 10% (*), 5% (**), or 1% level (***) is indicated.

Panel A: Baseline sample

Dependent =	*Discretionary bonuses*		*Other compensation*	
	(1)	(2)	(3)	(4)
I_{miss}	0.001	0.009	0.005	-0.005
	(0.038)	(0.037)	(0.018)	(0.019)
Controls	No	Yes	No	Yes
Year FEs	Yes	Yes	Yes	Yes
Contract type FEs	Yes	Yes	Yes	Yes
No. of observations	242	242	242	242
R^2	0.04	0.18	0.08	0.20

Panel B: Subsamples by social ties and co-option

Dependent =	*Discretionary bonuses*				*Other compensation*			
	Without social ties (1)	With social ties (2)	Low board co-option (3)	High board co-option (4)	Without social ties (5)	With social ties (6)	Low board co-option (7)	High board co-option (8)
I_{miss}	0.004	0.026	0.047	-0.025	-0.006	-0.032	0.031	-0.054
	(0.044)	(0.162)	(0.075)	(0.023)	(0.021)	(0.049)	(0.021)	(0.041)
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Contract type FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of observations	207	34	124	116	207	34	124	116
R^2	0.08	0.44	0.11	0.12	0.09	0.64	0.11	0.17
Diff (p-value)	0.864		0.319		0.526		0.042	

Table IA-9: Trading intensity, frequency, and size

This table presents the regression results on the effect of missing relative performance goals on insider trading intensity, frequency, and size. Trading intensity is defined as the total dollar value of trades in the year following the end of the performance period scaled by the prior-year total compensation. I aggregate trades by a CEO on the same day into a single transaction. Trade frequency is the total count of these daily transactions, and trade size is their average dollar value, scaled by prior-year total compensation. The variable of interest is the I_{miss} indicator, which equals one for CEOs who miss their relative performance goals and zero otherwise. The coefficient estimates for the control variables are omitted for brevity. Numbers in parentheses are standard errors clustered by firm. Significance at the 10% (*), 5% (**), or 1% level (***) is indicated.

Dependent =	Trading intensity		Trade frequency		Trade size	
	(1)	(2)	(3)	(4)	(5)	(6)
I_{miss}	-0.179	-0.253	-0.695	-0.815	0.008	-0.006
	(0.230)	(0.214)	(0.522)	(0.520)	(0.070)	(0.070)
Controls	No	Yes	No	Yes	No	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Contract type FEs	Yes	Yes	Yes	Yes	Yes	Yes
No. of observations	242	242	242	242	242	242
R^2	0.09	0.32	0.03	0.17	0.08	0.24

Table IA-10: Insider trading profitability across quarters

This table presents the regression results on the effect of missing relative performance goals on abnormal insider trading profits. I conduct the analysis separately for four three-month periods ("quarters") within the one-year window following the performance period-end. The variable of interest is the I_{miss} indicator, which equals one for CEOs who miss their relative performance goals and zero otherwise. The coefficient estimates for the control variables are omitted for brevity. Numbers in parentheses are standard errors clustered by firm. Significance at the 10% (*), 5% (**), or 1% level (***) is indicated.

Dependent =	*Abnormal insider trading profits*			
	Quarter 1	Quarter 2	Quarter 3	Quarter 4
	(1)	(2)	(3)	(4)
I_{miss}	0.013	0.032	0.038	-0.002
	(0.008)	(0.015)**	(0.019)*	(0.005)
Controls	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
Contract type FEs	Yes	Yes	Yes	Yes
No. of observations	242	242	242	242
R^2	0.04	0.18	0.08	0.20

Table IA-11: Routine versus non-routine trades

This table presents the regression results on the effect of missing relative performance goals on abnormal insider trading profits, distinguishing between routine and non-routine trades. Following Cohen, Malloy, and Pomorski (2012), I infer routine and non-routine trades using the pattern of insider trades in the past. If a CEO places trades in the same calendar month for at least three consecutive years in the past, all the following trades placed in that month are classified as routine trades. Trades placed in other months are classified as non-routine trades. I require that a CEO be in the insider trading database for at least three consecutive years in the past to be included in this analysis. The variable of interest is the I_{miss} indicator, which equals one for CEOs who miss their relative performance goals and zero otherwise. The coefficient estimates for the control variables are omitted for brevity. Numbers in parentheses are standard errors clustered by firm. Significance at the 10% (*), 5% (**), or 1% level (***) is indicated.

Dependent =	*Abnormal insider trading profits*			
	Non-routine		Routine	
	(1)	(2)	(3)	(4)
I_{miss}	0.119	0.144	-0.002	-0.003
	(0.066)*	(0.074)*	(0.014)	(0.011)
Controls	No	Yes	No	Yes
Year FEs	Yes	Yes	Yes	Yes
Contract type FEs	Yes	Yes	Yes	Yes
No. of observations	134	134	134	134
R^2	0.13	0.23	0.26	0.46

Table IA-12: The role of board monitoring and information asymmetry

This table reports the regression results estimating Eq. (2) on the discontinuity in abnormal insider trading profits around relative performance goals across subsamples partitioned by the strength of board monitoring and level of information asymmetry. Panel A splits the sample by board independence (the fraction of independent directors on the board) and board co-option (the fraction of directors appointed during the CEO's tenure, following Coles, Daniel, and Naveen, 2014). Panel B splits the sample by analyst coverage and firm size (market capitalization). Each split is based on the sample median of the respective measure. The variable of interest is the I_{miss} indicator, which equals one for CEOs who miss their relative performance goals and zero otherwise. Numbers in parentheses are standard errors clustered by firm. The last row reports the *p*-value of a Chi-squared test for the difference between the RD estimates across the two subsamples within each split. Significance at the 10% (*), 5% (**), or 1% level (***) is indicated.

Panel A: Subsample by board monitoring proxies

Dependent =	*Abnormal insider trading profits*			
	Low board independence	High board independence	Low board co-option	High board co-option
	(1)	(2)	(3)	(4)
I_{miss}	0.104	0.055	0.081	0.093
	(0.047)**	(0.027)**	(0.058)	(0.039)**
Year FEs	Yes	Yes	Yes	Yes
Contract type FEs	Yes	Yes	Yes	Yes
No. of observations	119	121	124	117
R^2	0.18	0.16	0.12	0.23
Diff (*p*-value)	0.334		0.845	

Panel B: Subsample by information asymmetry proxies

Dependent =	*Abnormal insider trading profits*			
	Low coverage	High coverage	Small firm size	Large firm size
	(1)	(2)	(3)	(4)
I_{miss}	0.104	0.035	0.055	0.104
	(0.052)**	(0.037)	(0.023)**	(0.048)**
Year FEs	Yes	Yes	Yes	Yes
Contract type FEs	Yes	Yes	Yes	Yes
No. of observations	125	117	122	120
R^2	0.17	0.12	0.23	0.21
Diff (*p*-value)	0.277		0.292	