DO MANAGERS MANIPULATE EMPLOYEE LABOR COSTS TO MEET FIRM- AND EXECUTIVE-LEVEL TARGETS?

by

ROBERT TUCKER DAVIS

THOMAS LOPEZ, COMMITTEE CHAIR
SHAWN MOBBS
TROY POLLARD
SHANE STINSON
QUINN SWANQUIST

A DISSERTATION

Submitted in partial fulfillment of the requirements
for the degree of Doctor of Philosophy
in the School of Accountancy
in the Graduate School of
The University of Alabama

TUSCALOOSA, ALABAMA

2023

ABSTRACT

Fair and equitable employee compensation within firms has received considerable attention by regulators, the media, and academic researchers. For example, many prominent CEOs have made explicit commitments to compensate employees fairly (Business Roundtable, 2019). The objective of this study is to provide evidence on this ongoing controversy by investigating whether managers manipulate non-executive labor costs to meet performance benchmarks. First, I predict and find that managers manipulate labor costs to meet analysts' earnings forecasts. Having established that managers manipulate labor costs to meet firm-level performance benchmarks, I next explore the extent to which managers manipulate non-executive employee labor costs to meet executive-level bonus targets. I find no consistent evidence that managers manipulate labor costs to meet or exceed their individual bonus targets. Collectively, this evidence indicates that managers are more focused on meeting firm-level benchmarks rather than individual level targets, suggesting that managers are, on average, not as self-serving as they are sometimes portrayed.

DEDICATION

To my family who have always supported me throughout my many pursuits.

LIST OF ABBREVIATIONS AND SYMBOLS

BRT	Business Round Table
CEO	Chief Executive Officer
CFO	Chief Financial Officer
CIC	Census Industry Codes
COGS	Cost of Goods Sold
EPS	Earnings Per Share
GAAP	Generally Accepted Accounting Principles
OLS	Ordinary Least Squares
R&D	Research & Development
REM	Real Earnings Management
ROA	Return on Assets
SEC	Securities and Exchange Commission
SG&A	Selling, General, & Administrative
SIC	Standard Industrial Classification

ACKNOWLEDGEMENTS

I thank my dissertation chair, Tom Lopez, for his friendship and commitment to me and this project. I also thank committee members Troy Pollard, Shane Stinson, Quinn Swanquist, and Shawn Mobbs for their helpful comments and feedback on this project.

I am also very grateful to James Chyz and Feng Jiang for their help creating an industry crosswalk between CIC and SIC industry classifications. The comments and data they provided was invaluable to my incorporation of labor unionization rates into the analyses.

I am also very grateful to James Lawson and Daniel Street and my fellow doctoral students at the University of Alabama for their comments on early drafts and encouragement throughout the process.

CONTENTS

LIST OF TABLES

LIST OF FIGURES

I. INTRODUCTION

The academic literature and business media are replete with both empirical and anecdotal evidence that executives' treat employees unfairly in setting employee compensation (e.g., Rouen 2020; Raghunandan 2021; Bobo 2014; Magnan and Martin 2019). In fact, several articles imply executives manipulate employee labor costs to increase their own compensation (e.g., Blake 2022; Kilgore 2022). This evidence exists in spite of the fact that many CEOs of large public companies have signed on to the Business Roundtable's "Statement on the Purpose of a Corporation" (hereafter the BRT Statement), which includes an explicit commitment from the firm to compensate employees fairly (Business Roundtable, 2019).[1] However, as prior anecdotal evidence suggests, some believe that managerial self-interest motivates the manipulation of labor costs. In this study, I provide evidence to address this conjecture by investigating two unique, but related research questions. First, I examine whether and to what extent managers manipulate labor costs to achieve firm-level performance targets (e.g., analyst forecasts). Second, I examine whether and to what extent managers manipulate labor costs to meet their individual-level bonus targets. In so doing, I am able to better focus on managerial self-interest as a motivation for manipulating labor costs.

Prior literature defines real earnings management (REM) as executive actions that deviate from normal business practices undertaken with the primary objective of achieving certain

---

[1] As of September 2022, 267 CEOs have signed on to the Business Roundtable's "Statement on the Purpose of a Corporation" (Business Roundtable, 2019).

reporting goals. Importantly, this literature provides compelling anecdotal and empirical evidence on its existence (e.g., Graham et al., 2005; Kothari et al., 2016; Roychowdhury, 2006). However, this literature has largely ignored the implications of labor costs. In particular, the literature has primarily focused on the manipulation of advertising expenses; research and development expenses (R&D); selling, general, and administrative expenses (SG&A); and production costs with a near total omission of labor costs. Further, this literature relies exclusively on firm-level performance goals (e.g., analyst forecasts) and infers managerial opportunism as a motive for REM. Thus, the prior literature has not separated the motives of well-intentioned managers making difficult decisions that they believe to be in the best interests of firm shareholders from those of self-serving managers seeking to increase their own wealth at the expense of firm stakeholders.

At least four important factors motivate my analyses. First, labor costs represent a material expense for most firms as the median total staffing expense is 26 percent of annual sales revenue.[2] Second, prior literature suggests that REM is the preferred method of executives for managing earnings because of its lack of observability to external stakeholders and because the "business judgment" principle largely shields executives from litigation risk (Graham et al., 2005; Huang et al., 2020). Critically, labor costs may be a particularly attractive mechanism for managers to meet performance targets because labor costs are, in general, not readily observable as a separate line on the income statement. Third, prior literature provides reasons to believe managers manipulate labor costs to achieve firm objectives. For example, literature provides

[2] This sample is restricted to Compustat firm-years between 2006-2020 (128,083 observations) with non-missing total staffing expense (Compustat XLR) and total assets (AT) and net sales (SALE) greater than or equal to zero.

evidence that managers engage in “wage theft” to meet firm level objectives (Bobo, 2014; Raghunandan, 2021; Shields, 2019).[3]

Finally, prior literature on executive compensation and labor costs largely ignores the consequences of executive cash-based bonuses because the pay-performance sensitivities were presumed to be too small to matter (Bushman, 2021; Bushman and Smith, 2001). However, Guay et al. (2019) report that the actual performance sensitivity of bonuses is over ten times larger than regression estimates in prior studies and comparable in scale to equity incentives for many CEOs early in their tenures. In addition, Armstrong et al. (2020) report that managers just meet/beat (within 2 cents) the bonus target 6.14 percent of the time, compared with beating the bonus performance threshold by 1.08 percent and the bonus performance maximum 2.17 percent of the time. Armstrong et al. (2020) note that the more frequent incidence of just meeting or beating the bonus target by small amounts, compared to the bonus threshold and maximum is interesting because, the bonus target can be an important benchmark that boards use to evaluate CEOs’ performance. To the extent that a large discontinuous change in the achievement rate at a bonus goal is indicative of its importance to managers, the evidence in Armstrong et al. (2020) supports the conjecture that managers will manipulate costs to meet that target.

Although there are compelling reasons to believe that managers will manipulate labor costs to meet firm- and individual-level targets, there are equally persuasive arguments to the

[3] This literature defines “wage theft” as the denial of employees’ legally mandated pay or benefits, as measured by the Wage & Hour Division office of the US Department of Labor. Common examples of such actions include forcing employees to underreport their hours worked, not paying for overtime work by misclassifying workers as exempt from overtime requirements, forcing employees to work through legally mandated paid breaks, and paying workers less than the minimum wage.

contrary. For example, prior literature concludes labor costs are sticky because laying off staff can have negative consequences such as the loss of institutional knowledge or expertise. Further, the costs to hire and train new employees often exceed the financial benefits of cutting employees (Anderson et al., 2003; Dierynck et al., 2012; Kama and Weiss, 2013). In addition, finding that some managers steal wages from employees to meet earnings forecasts (Raghunandan, 2021) does not necessarily imply that, on average, managers will manipulate labor costs to achieve performance goals. Importantly, the evidence in Raghunandan (2021) highlights the extreme nature of employee wage theft. Consistent with that notion, Raghunandan (2021) reports that firms caught violating wage theft laws are more likely to commit financial fraud in the future. Further, organized labor may limit executives' ability to use labor costs as a mechanism to manipulate earnings. For example, collective bargaining agreements and other contractual obligations may prevent layoffs, fix nonexecutive bonuses, and/or restrict reduction of hours. Finally, attempts to decrease labor costs may result in severe push back or decreased productivity from employees if they perceive the reduction as unfair, potentially resulting in lower future performance (Rouen, 2020). This could result in other non-monetary costs such as a loss of trust in management and reputational damage. Importantly, prior literature suggests CEOs consider personal, reputational, and labor market consequences when making decisions related to labor costs (Dierynck et al., 2012).[4]

[4] Dierynck et al. (2012) report that when faced with a downturn in activity firms that beat the zero earnings benchmark by only small amounts often dismiss employees, particularly those that are low cost to fire. However, large dismissals have negative reputational effects on the firm and CEO, therefore firms with healthy profits address the downturn in activity by reducing the number of hours that employees work.

I examine manager motivations to manipulate labor costs using a variety of techniques and data. Specifically, I estimate OLS regressions of managerial incentives to manipulate labor costs over the years 1993 to 2021. To proxy for manipulated labor costs, I create a measure of abnormal non-executive labor similar to prior REM literature that examines abnormal discretionary expenses (Huang et al., 2020; Kothari et al., 2016; Roychowdhury, 2006). I use two proxies for labor costs for two separate samples. The first proxy is total staffing expense reported in Compustat less the total compensation of the top five executives as reported in Execucomp. However, the sample of firms with reported total staffing expense is very small because only financial institutions are required to disclose total staffing expense and firms in other industries only do so on a voluntary basis. Thus, non-executive labor cost using this first proxy results in a limited sample that is dominated by financial institutions.

To overcome these limitations, I create a second proxy by imputing missing values for non-executive labor costs in a fashion consistent with prior accounting literature (Bushee and Miller, 2012; Ittner et al., 1997). I use two different methods to estimate missing values of non-executive labor costs. The first is developed using OLS regression of actual labor costs on several income statement and balance sheet items where labor costs are typically included (e.g., SG&A, COGS). The second is developed using a gradient boosting machine learning technique and the same set of input values. I use the parameter estimates from both methods to impute non-executive labor costs for non-reporting firms. To the extent these models accurately predict non-executive labor costs, the use of these proxies will increase the external validity of my study.

In addition to labor costs, my first research question requires a firm-level performance benchmark and corresponding actual performance. I focus on analyst forecasts and collect forecasted and actual performance related to EPS forecasts consistent with prior literature (e.g., Huang et al., 2020; Raghunandan, 2021). To examine my second research question, I require an executive-level performance benchmark. To measure the executive-level performance benchmark, I use bonus targets from CEO bonus packages that are determined at the beginning of the fiscal period. Specifically, I use an EPS bonus target to maintain consistency with the firm-level benchmark.

I begin my empirical analyses by validating the labor cost prediction model. Following Ittner et al. (1997), I first regress observations with available non-executive labor costs (i.e., total staffing expense less the total compensation of the top five executives), on several predictor variables as well as industry and year indicators. I apply estimated parameters to all firm-year observations to impute the missing values of non-executive labor costs. I then validate the predicted values from my model using a hold-out sample of firms with reported non-executive labor costs. Although the predicted labor cost measure has limitations that I address later in the text, validation tests suggest the model predicts non-executive labor costs very well. In particular, I find that the mean model prediction errors (i.e., (predicted less actual)/actual) for non-financial (financial) firm-year observations is 2.8 (0.5) percent when using the OLS prediction model.[5] The mean error is significantly different from zero only for the non-financial firms (p-value<0.05). When using the boosting technique, I find that the error percentage is 0.10 (0.54) percent for non-financial (financial) firm-year observations both of which are insignificantly different from zero.

[5] To train and validate the OLS predictions, I use three different training/validation sample splits. These are 80/60/50 percent for the training samples and 20/40/50 percent for the validation samples. The OLS results for all splits are quantitatively and qualitatively similar. For the Boosting predictions, I only employ an 80-20 split. The results here are from the 80-20 split. I will address this issue more fully later in the text.

To investigate my first research objective, I estimate multivariate OLS regression models of abnormal labor costs on a number of control variables, fixed effects for firm and year, and an indicator variable identifying the firm-year as a suspect year for labor cost manipulation. In particular, consistent with Raghunandan (2021), I define suspect firms-years as those where the EPS surprise is greater than or equal to 0 and less than or equal to 0.02 (i.e., zero to two cent surprise). That is, the firm exactly met or just beat the firm-level EPS analyst forecast.[6] In tests of firm-level EPS forecasts, I find consistent evidence across multiple tests suggesting that managers significantly reduce labor costs to meet or just beat EPS forecasts. Critically, this result is robust to using actual reported labor costs or predicted labor costs from the two different prediction methods described above. Taken together, this evidence suggests that managers manipulate labor costs when faced with financial reporting pressure to meet firm-level targets. This result supports prior research that suggests meeting or beating analysts' earnings forecasts is of importance to managers and investors to build credibility with capital markets and to maintain or increase firm stock price (Brown and Caylor 2005; Graham et al., 2005; Armstrong et al. 2020; Liu et al. 2019).

My second research objective investigates whether managers manipulate labor costs to meet their individual bonus targets. To do so, I alter the definition of a "suspect firm-year" to those where actual bonus surprise (actual EPS less the bonus target) is greater than or equal to

[6] The evidence reported by Keung et al. (2010) suggests that the extent to which managers intervene in the process, by biasing earnings, to report earnings that meet or just beat analysts' expectations is pervasive. Keung et al. (2010) report that there are two-times to five-times as many firms that meet or just beat expectations [0, $0.02] to firms that just miss [$-0.02, 0). They refer to this ratio as the "manipulation index" under the assumption that firms with small positive earnings surprises are more likely to result from earnings management or forecast guidance.

0.00 and less than or equal to 0.02 (i.e., zero to two cent surprise). I define suspect firms in this way to maintain consistency with my firm-level tests. I find strong evidence of a discontinuity in the EPS bonus target surprise around zero consistent with management intervention. However, although this discontinuity is observable in the distribution chart, I find no significant evidence that managers manipulate employee labor costs to meet or just beat their EPS bonus target. That is, I find no association between abnormal labor costs and firms just meeting or beating EPS targets in the CEO's bonus incentive plan.

My research contributes to the literature in a number of ways. First, I extend the literature by developing and validating two labor cost predictive models. These models allow for the investigation of the implications of labor costs in wide variety of contexts. Second, I contribute to literature examining CEO commitment to stakeholder governance (e.g., Bebchuk and Tallarita, 2021). Raghunandan and Rajgopal (2021) find that, "[BRT Statement signatory] firm's proclamations of stakeholder-centric behavior are not backed up by any hard data on these firm's operations." My study provides evidence on the "hard data" Raghunandan and Rajgopal (2021) suggest is missing from the literature. Third, I contribute to the burgeoning stream of literature that reexamines the importance of cash-based bonus plans, particularly in terms of their incentive effects (Bloomfield, 2021; Bushman, 2021; Guay et al., 2019).

II. BACKGROUND LITERATURE AND HYPOTHESIS DEVELOPMENT

Real Earnings Management

An extensive literature examines the various implications and consequences of REM. Graham et al. (2005) report that CFOs often experience the decision to trade off real investments for reported earnings. They report that 75 percent of interviewed executives expressed the view that "every company would/should" take real actions to hit analyst's consensus earnings forecasts "as long as the actions are within GAAP and the real sacrifices are not too large" (Graham et al., 2005, p. 40). They also report that, 80 percent of surveyed executives said they would reduce discretionary spending to meet an earnings-related target and 55.3 percent of surveyed executives said they would do so even if it meant sacrificing a little firm value. Additionally, Graham et al. (2005) report that managers are largely motivated to meet or beat earnings benchmarks by stock price considerations and the desire to enhance reputations with customers, suppliers, and creditors is also very important.

Graham et al. (2005) is an important study that highlights the prevalence of REM among firms. Empirical research provides evidence consistent with the views expressed in the Graham et al. interviews (e.g. Caylor, 2010; Cohen and Zarowin, 2010; Roychowdhury, 2006). This literature measures REM relying on several individual financial reporting line items (e.g., SG&A, R&D, and COGS) and aggregated measures (e.g., production costs). Importantly, while labor costs represent a substantial percentage of annual sales revenue,[7] currently US GAAP does not require firms to disclose these costs separately on the income statement or in notes to the financial statements.[8] Because labor costs are embedded in a variety of accounts on the income statement or balance sheet such as SG&A, COGS, R&D and inventory. they are largely unobservable to external parties.

Firm-Level Performance Benchmarks as Incentives to Use REM

As previously discussed, there are a number of reasons managers would want to avoid missing firm-level performance goals (e.g., analyst forecasts) that are not purely self-serving. CEOs have a fiduciary duty to the shareholders and may view real activities manipulations to achieve these goals to be in the best interests of the shareholders. Consider that missing these goals often results in loss of shareholder value and negative reputational consequences (e.g., Bartov et al., 2002; Burgstahler and Dichev, 1997; Dierynck et al., 2012; Graham et al., 2005). This is perfectly consistent with the survey results in Graham et al. (2005) that managers are motivated to use REM for the purposes of achieving firm-level benchmarks by stock price and reputational concerns rather than their own short-term compensation. However, some studies use firm-level benchmarks, such as meeting or just beating analysts' earnings forecasts, and infer that meeting or beating the benchmark represents managerial opportunism. It is plausible that some of these cases represent managerial opportunism; however, it is equally plausible, if not

---

[7] Based on actual reported staffing expense, I estimate the median labor cost to sales revenue ratio is approximately 26 percent.

[8] However, the Federal Reserve rules require bank holding companies to disclose the total staffing expense in their quarterly filing of FR Y-9C reports (consolidated financial statement). The SEC has recently amended Regulation S-K to include more disclosure related to human capital; however, public corporations are not required to do so.

more so, that many of these represent managers making difficult choices that they believe are in the best interests of the shareholders (i.e., unrelated to bonus incentives). Managers interviewed by Graham et al. (2005) suggest it is the latter explanation.

Manipulation of Labor Costs

Prior literature provides evidence that managers manipulate activities related to human capital. For example, Andreicovici et al. (2021) finds that firms use accruals to manage earnings downward in the year prior to large employee terminations to provide an economic rationale for the layoffs. Kong et al. (2021) find that firms with employee cash profit-sharing plans use accruals to manage earnings downward to reduce firm profit-sharing plan contributions. On the other hand, Dou et al. (2016) report that implicit job security-related contracts with employees are associated with upward management of long-run earnings. That is, firms manage earnings upward to provide the appearance of good performance to promote feelings of job security and a successful firm.

Dierynck et al. (2012) use a sample of private Belgian firms to investigate the influence of earnings-related incentives on labor costs. Although they find that labor costs tend to be sticky, they report these costs are less sticky for firms that just avoid negative earnings and firms that have high labor intensity. This result suggests that firm-level benchmarks, such as avoiding losses, may provide an incentive for managers to manipulate labor costs. My study differs from this study in two important ways. First, I study public U.S. firms that allows for more generalizability across U.S. corporations. Second, I investigate whether managers adjust labor costs to meet analyst forecasts. I do so because Brown and Caylor (2005) report that of the three thresholds commonly examined in the prior literature (analyst forecasts, increase vs decrease in earnings, and profit vs loss), investors unambiguously reward (penalize) firms for reporting quarterly earnings meeting (missing) analysts' estimates more than they do for meeting (missing) the other two thresholds.

Raghunandan (2021) examines the financial reporting implications of wage theft. He finds that firms just meeting or beating analyst forecasts have higher instances of wage theft. My study differs from Raghunandan in at least two important ways. First, my investigation focuses on labor cost manipulations that are within the bounds of the law, whereas Raghunandan (2021) only detects extreme cases of labor cost manipulation. Further underscoring the extreme nature of wage theft and managers who engage in such behavior, Raghunandan (2021) reports that firms caught violating wage theft laws cease committing wage theft and begin fraudulently reporting financial statements. Thus, my investigation focuses on manager actions that are likely more representative of the average manager across firms. Consistent with this notion, Raghunandan (2021) reports that only 9 percent of his sample are wage theft observations, whereas the evidence in Graham et al. (2005) suggests the overwhelming majority of managers engage in REM as long as it is within GAAP.

Second, I differ from Raghunandan (2021) by examining individual-level bonus targets as a proxy for managerial incentives to manipulate labor costs. Raghunandan (2021), along with all prior REM literature that I am aware of, examines only firm-level benchmarks and incentives and infers managerial opportunism and self-dealing. However, prior literature reports that missing a firm-level benchmark like analyst forecasts, is bad for shareholders as well as the manager (e.g., Lopez and Rees, 2002; Matsunaga and Park, 2001). On the other hand, the impact of missing an individual bonus target, likely, only affects the manager. I include CEO bonus targets to distinguish opportunistic, self-serving behavior from other more justifiable motivations (e.g., REM as a signaling device). This is different from all prior literature and enables me to focus on truly self-serving motivations rather than those of a manager making hard choices (e.g., employee terminations) on behalf of their shareholders.

Labor Costs and Firm-Level Performance Benchmarks – H1

Collectively, the prior discussion suggests several reasons why managers will manipulate labor costs to achieve firm-level performance benchmarks. First, Graham et al. (2005) and a sizable literature suggest that managers will manipulate real activities to achieve firm-level goals, with upwards of 80 percent of managers openly admitting that they would cut discretionary expenses to hit earnings targets. Second, executives' candid responses in Graham et al. (2005) suggests that they do not perceive this to be a value-destroying decision. On the contrary, Graham et al. (2005) report managers' view meeting earnings benchmarks as part of their fiduciary duty to the shareholders to avoid reductions in firm value (e.g., Bartov et al., 2002).[9] Thus, managers might consider cuts to labor costs to meet earnings benchmarks to be a difficult but necessary decision that is in the best interest of the firm and its shareholders.

Third, the large magnitude and relative lack of transparency of labor costs make it a particularly appealing tool, creating opportunities for managers to manipulate these costs to meet earnings benchmarks while also avoiding detection and litigation risk. Huang et al. (2020) find that when litigation risk decreases, managers use of REM increases suggesting that managers are seeking to avoid negative outcomes. Additionally, Graham et al. (2005) report that managers prefer the more opaque method of REM to accruals manipulations even though REM is more difficult. Atanasov et al. (2021) state that "manipulation is more likely when it is harder to detect

[9] However, there is also literature that suggests a great deal of firm value is lost in the pursuit of hitting earnings targets (Graham et al., 2006)

and not subject to intense scrutiny." Ultimately, managers know that they have to explain their choices, particularly if outside stakeholders perceive those choices as harmful to long-term firm value (e.g., on analyst conference calls). However, they may only have to do so if those choices are observable to outside stakeholders. Therefore, the most attractive method of manipulating costs may be those that are less transparent, such as labor costs.

Finally, prior literature provides evidence that managers are manipulating labor costs in contexts other than meeting/beating firm-level benchmarks or in extreme cases (Dierynck et al., 2012; Kong et al., 2021; Raghunandan, 2021). There is also anecdotal evidence that supports this notion. For example, one author writes, "These cost savings [from layoffs] make a company's earnings call look good since they will be making more money in the future by spending less" ("How Corporate Layoffs", 2021). Other articles suggests that firms execute layoffs to focus on the bottom line (Stambor, 2022; Vanian, 2022).

Collectively, the preceding discussion suggests that managers believe the benefits of using REM to hit firm-level targets outweighs the costs and that there are good reasons to believe managers will similarly manipulate labor costs. However, this notion is not without tension and there are reasons to believe managers will avoid labor cost manipulations. Human capital has become an important component of firm valuation (Zingales, 2000) and unlike other discretionary expenses; reducing employee pay affects real people in ways that hurt both the employee and the firm. The employee faces negative consequences in the form of reduced compensation and the firm potentially loses employee productivity because of declining employee morale in the face of what are viewed as unjust compensation choices (Ji et al., 2022; Rouen, 2020). Similarly, the presence of labor unions or collective bargaining agreements may place limitations on the manager's ability to manipulate labor costs, whether contractual or because of the specter of union pushback. Finally, it is costly to eliminate labor costs in one period and then reacquire them soon after, particularly if this involves hiring and training new employees.

The above discussion leads to my first hypothesis stated in the null form:

H1: Abnormal labor costs are not associated with meeting or beating analyst's firm-level earnings forecasts.

Labor Costs and Individual-Level Bonus Targets – H2

Executive cash-based bonus plans are a near-universal part of executive compensation packages. Prior literature generally focuses on two unique perspectives of cash-based bonus plans. The first is that cash-based bonuses can create incentives by focusing manager attention on metrics more directly under their control (e.g., Feltham and Xie, 1994; Healy, 1985; Hölmstrom, 1979; Lambert and Larcker, 1987). The second is that the incentive created by cash-based bonus plans is small relative to equity compensation (e.g., Core et al., 2003; Jensen and Murphy, 1990). While the prominence of the second perspective increased with the use of managerial stock and option compensation, new proxy statement data on the specific details of executive bonus plans has motivated an emerging stream of literature that reexamines the role of executive cash-based bonus plans. This stream of literature reinforces the notion that bonus plans create effective incentives for managers. In particular, Guay et al. (2019) provide evidence that bonus incentives are much larger than suggested by prior research.

Additionally, prior literature suggests executives seek to manipulate bonus targets and reported performance. Martin et al. (2022) provide evidence consistent with managers manipulating ex ante performance targets to be more achievable. They find that managers disclose earnings guidance that is more pessimistic than at other times just prior to bonus targets being set by the compensation committee. To examine the extent to which manipulation of reported performance actually occurs, Bennett et al. (2017) and Atanasov et al. (2021) examine discontinuities in the distribution of reported performance around bonus-target performance. Although the two studies examine different samples, both studies find significant discontinuities around bonus targets for a variety of performance metrics. Specifically, they both report a statistically significant higher number of firms reporting performance just above the targets compared to those reporting performance just below the targets. Both studies suggest this evidence is consistent with managers manipulating reported performance to meet performance bonus targets by only small amounts.

As mentioned previously, prior REM-related research focuses exclusively on firm-level benchmarks. Thus, the extant literature provides considerable evidence that managers' use REM to benefit all firm stakeholders, not necessarily themselves. For example, managers have self-reported that they are more concerned with stock price and firm reputation than they are with their own short-term compensation (Graham et al. 2005). Graham et al. (2005) further note that many managers indicate that bonus payout is simply not that important relative to salary and stock compensation. However, they also point out that it is plausible that executives are more willing to admit to a stock price motivation, rather than a bonus motivation, for exercising accounting discretion. In their examination of EPS bonus targets and EPS analyst forecasts, Armstrong et al. (2020) report that when analysts' forecasted EPS is lower than the bonus EPS target, the bonus target is rarely achieved. On the other hand, when the bonus target is lower than the analyst forecast, the bonus target is met more often than not. Armstrong et al. (2020)

conclude that CEOs primarily focus on meeting analysts' EPS forecasts and that meeting their individual bonus targets are a second-order effect.

On the other hand, there is also a significant amount of evidence that managers use REM for self-serving reasons. First, while Armstrong et al. (2020) conclude managers focus on firm-level performance targets, they also report a significant discontinuity in the frequency of managers just meeting their bonus targets. Similarly, Bennett et al. (2017) report a disproportionately large number of firms exceed their bonus targets by a small margin as compared to the number that fall short of the bonus target by a similar margin. They further report that CEOs that miss bonus targets are more likely to experience a forced turnover. Armstrong et al. (2020) note that the more frequent incidence of just meeting the bonus target is consistent with the bonus target being an important benchmark that boards use to evaluate CEO performance. The evidence in Bennett et al. (2017), that CEOs who miss bonus targets experience higher forced turnover, is consistent with the conjecture in Armstrong et al. (2020). Taken together, the evidence in Armstrong et al. (2020) and Bennett et al. (2017) supports the notion that managers will manage labor cost to meet bonus targets.

Second, Guay et al. (2019) report that the sensitivity of executive bonuses to firm performance are substantially larger than previously assumed. Their estimates suggest performance sensitivities are ten times larger than previously thought, thus providing greater motivation for executives to manage earnings upward. Third, Guay et al. (2019) present evidence that the bonus plans also create team incentives. This can lead to peer pressure such that executives push one another to manage performance so they can all obtain bonuses for the year. The incentive effect of the annual cash payments may be particularly salient for non-CEO executives who receive a larger portion of their annual pay from bonuses than from equity grants, relative to the CEO (Armstrong et al., 2020). Fourth anecdotal evidence suggests that managers manipulate labor costs to meet individual bonus targets. For example, the Institute for Policy Studies published an article describing instances of boards changing the terms of executive bonuses so they could continue to reward executives' even while lower-level employees were facing compensation reductions (Anderson, 2021).

Finally, prior research provides evidence suggesting that managers manipulate earnings to meet their own bonus targets. Atanasov et al. (2021) examine the distribution of performance metrics used in setting executive compensation for a sample of US oil and gas firms. They report a significant discontinuity in the distribution of actual minus target performance at zero (i.e., significantly more small positives than small negatives). Similarly, Bennett et al. (2017) report that the density of actual less target performance has a significant discontinuity at zero. They report a disproportionately larger number of firms exceed the performance target by a small amount as compared to the number of firms that fail to meet the performance target by a small amount. Bennett et al. (2017, Tables 5 and 6) also report descriptive evidence on discretionary accruals and changes in SG&A and R&D for firms that exceed bonus targets compared to firms that miss bonus targets. Of the 12 total tests of SG&A and R&D they estimate, they report four significant tests consistent with a REM explanation for just meeting bonus targets. The preceding discussion leads to my second hypotheses stated in the null form:

H2**:** Abnormal labor costs are not associated with meeting or just beating CEO-specific earnings bonus targets.

III. RESEARCH DESIGN

Measuring Non-Executive Labor Costs

To examine the use of non-executive labor costs as a mechanism to meet various performance benchmarks, I create a measure of abnormal labor costs as the residual of a model for normal labor costs consistent with models used in prior literature (Dechow et al., 1998; Huang et al., 2020; Roychowdhury, 2006). To do so, I first require a proxy for firm labor costs. I discuss the measures I rely on for my tests below followed by an explanation of the model used to estimate abnormal labor costs.

Reported Non-Executive Labor Costs

I base the first measure of non-executive labor costs on reported actual total staffing expense (Compustat *XLR*). This item represents salaries, wages, pension costs, profit sharing and incentive compensation, payroll taxes and other employee benefits for all employees. Because I am interested in work force labor costs that are susceptible to managerial manipulation, I follow prior literature that focuses on non-executive employee pay. I calculate non-executive employee pay by subtracting the total compensation of the top five executives (from Execucomp) from the total staffing expense variable (Acharya et al., 2014; Huang and Lu, 2019). I use this measure (*NonExecPay*) to conduct my analyses on a sample of firms with actual reported total staffing expense.

While only 27 percent of firms disclose total staffing expense and Execucomp generally only covers the S&P 1500, prior literature has relied on this variable as a measure of labor costs (Faleye et al., 2013; Huang and Lu, 2019; Rouen, 2020). A benefit of using XLR is that there is a relatively long time-series available for those firms reporting XLR in Compustat. The mean (median) number of years XLR is available is 10 (7). A potential limitation is that financial institutions dominate the sample because the Federal Reserve requires financial institutions to disclose this information. While some firms outside the financial institutions industry voluntarily disclose total labor costs, financial institutions are substantially overrepresented in this sample.[10] Another limitation is the voluntary nature of reporting by non-financial institution firms. This may create some self-selection challenges to the extent that these firms disclose this data because they are not manipulating labor costs. However, this self-selection issue biases against me finding the predicted result.

OLS Prediction of Non-Executive Labor Costs

To overcome the small sample limitation of using the Compustat variable for total staffing expense (*XLR*), I create a non-executive labor cost prediction model. I then use the parameter estimates from the prediction model to estimate non-executive labor costs for firms that do not report total staffing expense. Specifically, I first estimate the following prediction model for firms with available *NonExecPay*.

$$logNonExecPay_{i,t} = \beta_0 + \sum_{k=1}^{K} \beta_k \times Predictors_{i,t} + \delta + \gamma + \varepsilon_{i,t} \quad (1)$$

where *i* indexes firms, and *t* time and $logNonExecPay_{i,t}$ is the natural logarithm of one plus *NonExecPay* described in the previous section. $Predictors$ is a vector of predictor variables used to model non-executive labor costs. Because labor costs are typically aggregated with other costs into specific expense and asset accounts, I include SG&A expenses (*SG&A*), R&D

[10] Of the sample of firms with reported *NonExecPay*, roughly 57 percent are financial institutions (1-digit SIC = 6).

expenses (*R&D*), advertising expense (*ADV*), ending inventory (*INV*), and cost of goods sold (*COGS*) as predictor variables. Additionally, to capture the effect of firm size on non-executive labor costs I include total assets (*Firmsize*, *AT*). I estimate the equation separately for financial (one-digit SIC code of 6) and non-financial firms including 2-digit SIC code industry (δ) and year (γ) indicator variables.[11] I specifically define all variables in EQ (1), as well as all equations that follow, in Appendix B.

After estimating the parameters of EQ (1) for a training sample of firms with *logNonExecPay*, I apply the parameter estimates to the model determinants for a hold-out/testing sample of firms that have *logNonExecPay* and validate the accuracy of the prediction model. I discuss methods used to validate the prediction model and test results of those validation tests in Section 4.3.1 of the manuscript. Upon successful validation of the model, I then apply the parameter estimates to all firms that do not report total staffing expense (Compustat XLR). In this way, I am able to estimate a predicted value of non-executive labor costs for these non-reporting firms ($NonE\widehat{xecP}ay^{OLS}$).

Machine Learning Prediction of Non-Executive Labor Costs

As a second, and more sophisticated, prediction method, I employ gradient boosting, a machine learning technique. Specifically, I use XGBoost in Python's Scikit learn package and use the same model inputs as in the OLS prediction. This process begins in the same way as the OLS prediction, by splitting the sample of firms into a training and testing sample. For this process I used only an 80-20 split of the firms (not observations) to ensure that the testing sample

[11] All model inputs are log transformed and *XSGA*, *XRD*, *XAD*, *INVT*, *COGS*, and *CAPX* are set to zero if missing to accommodate reporting differences for each industry.

of firms did not have any observations in the training sample, thus preventing information leakage. I then use a Group K-fold cross validation to split the training sample into five separate folds grouped by firm identifiers. Essentially this creates five separate training and testing samples within the training sample and allows model hyperparameter tuning.

Using these hyperparameters, I train the model on financial and non-financial firms separately in the same way I trained the OLS model by financial firm type. I do this to account for fundamental differences in (1) the specific accounts used as inputs to the model between financial and non-financial firms, (2) the nature of regulated and non-regulated industries, and (3) firms that may be required to report labor costs and those that choose to disclose voluntary information.[12] After training the boosting model, I then use the it to predict the *logNonExecPay* for the testing sample of firms and validate the accuracy of the prediction model. I discuss the validation procedures and results later in Section 4.3.2. Following a successful prediction, I apply this model to all firms and obtain a second predicted value for non-executive labor costs ($NonEx\widehat{ecP}ay^{Boost}$) for those firms that do not report total staffing expense (Compustat *XLR*).

Abnormal Labor Costs

Using these firm-year proxies for total labor costs (actual non-executive labor costs, predicted non-executive labor costs), I estimate abnormal labor costs similar to the methods used in prior literature for calculating abnormal cash flow, production costs, and discretionary expenses. Specifically, I adapt the fixed-effect first-order autoregressive model for abnormal discretionary expenses used by Huang et al. (2020) as follows:

[12] Note that not all financial firms are required to report labor costs. For some later tests I use a more precise method of identifying firms as mandatory labor cost reporting firms using 3-digit SIC industry classifications. I address this method later in the text.

$$L_{i,t} = \alpha_0 + \alpha_1 \times L_{i,t-1} + \alpha_2 \times \frac{1}{Assets_{i,t-1}} + \alpha_3 \times Sales_{i,t} + \varepsilon_{i,t} \quad (2)$$

where *i* indexes firms, and *t* time. $L_{i,t\text{-}k}$ is one of the three labor cost measures previously discussed ($NonExecPay_{i,t}$, $\widehat{NonExecPay}^{OLS}{}_{i,t}$, $\widehat{NonExecPay}^{Boost}{}_{i,t}$) scaled by the beginning of the year total assets; $Assets_{i,t-1}$ is beginning of the year total assets; and $Sales_{i,t}$ is sales during the year scaled by beginning of the year total assets.

Kothari et al. (2016) suggest that when modelling abnormal expenses, it is important to adjust for year and firm-specific effects because models that ignore these effects can suffer misspecification error.[13] Further, they suggest models that control only for industry effects can suffer similar misspecification errors if firms consistently deviate from industry averages in efforts to stand out (Owens et al., 2017). I follow the procedures outlined in Huang et al. (2020) to adjust for year and firm effects. Specifically, before estimating the model I calculate the annual cross-sectional mean of $L_{i,t}$ ($\overline{L_t}$). I then difference labor costs from the annual cross-sectional mean ($L_{i,t} - \overline{L_t} = L_t^*$). I then subtract the firm-specific lagged value from the mean-adjusted value ($L_t^* - L_{t-1}^* = \Delta L_t^*$). I follow this same procedure to adjust the other explanatory variables in the model ($\frac{1}{Assets_{i,t-1}}$, and $Sales_{i,t}$). Using these adjusted variables as the inputs to the normal labor cost model, I then estimate the model for each year. Finally, the firm-year residual from the estimation of EQ (2) ($\varepsilon_{i,t}$) is differenced from the mean value of $\varepsilon_{i,t}$ across all years for that firm. This value multiplied by -1 is the measure of abnormal labor costs created

[13] For example, misclassification of a firm as being in the high (low) REM group based on firm characteristics such as growth (lack of growth).

using the three different proxies for non-executive labor costs (*AbnLabor, AbnLabor*^{OLS}*, AbnLabor*^{Boost}). Higher values of abnormal labor costs represent firms with greater cuts to labor costs and thus greater REM.

Empirical Test of H1

To test my first hypothesis, I conduct multivariate tests by estimating the following regression equation:

$$AbnL_{i,t} = \alpha_0 + \alpha_1 \times AnalystEPS\_MB_{i,t} + FirmFE + YearFE + controls + \varepsilon_{i,t} \quad (3)$$

where *i* indexes firms, and *t* time. $AbnL_{i,t}$ is either $AbnLabor_{i,t}$, the value of abnormal labor costs developed using actual non-executive labor costs ($NonExecPay_{i,t}$); $Abn\widehat{Labor}^{OLS}{}_{i,t}$, the value of abnormal labor costs developed using the OLS predicted value of non-executive labor costs ($NonE\widehat{xecP}ay^{OLS}{}_{i,t}$); or $AbnL\widehat{abor}^{Boost}{}_{i,t}$, the value abnormal labor costs developed using the machine learning gradient boosting predicted value of non-executive labor costs ($NonE\widehat{xecP}ay^{Boost}{}_{i,t}$). To measure the manager's firm-level incentives to manipulate labor costs, I use the consensus analyst EPS forecasts. Specifically, I obtain the last consensus analyst EPS forecast before the earnings announcement as well as the actual performance value from I/B/E/S. I calculate the EPS forecast surprise (*AnalystEPS_Surprise*) as actual EPS minus the consensus analyst EPS forecast. Consistent with Raghunandan (2021), I then define meet/beat firms-years (*AnalystEPS_MB)* as those whose EPS forecast surprise is greater than or equal to 0 and less than or equal to 0.02 (i.e., zero to two cent surprise).

Empirical Test of H2

To examine H2, I modify EQ (3) by replacing the measure of the manager's firm-level incentives with a measure of their individual-level incentives as follows: ($BonusEPS\_MB_{i,t}$)

which is an indicator equal to one when *BonusEPS_Surprise* (actual EPS bonus performance less the EPS bonus target) is greater than or equal to 0 and less than or equal to 0.02, and zero otherwise.

$$AbnL_{i,t} = \alpha_0 + \alpha_1 \times BonusEPS\_MB_{i,t} + FirmFE + YearFE + controls + \varepsilon_{i,t} \qquad (4)$$

To measure the manager's individual-level incentives to manipulate labor costs, I use the target values for CEO annual bonuses contracts. Specifically, I obtain detailed executive bonus data from Incentive lab on the annual grants and each absolute performance-based metric and retain only those observations with non-missing target performance levels.[14] I focus on EPS performance metrics (metric=EPS) to remain consistent with the prior literature's use of meeting or beating analysts' forecast metrics as incentives to engage in REM (Roychowdhury, 2006). I also use this measure because executives themselves have commented that EPS is the most important performance metric (Graham et al., 2005; Armstrong et al. 2020).

Unlike I/B/E/S, Incentive Lab does not list the actual values that correspond to the performance-based metrics. However, firms are required to disclose the actual performance in the proxy statement of the following year. Therefore, to measure actual performance achieved, I follow Bennett et al. (2017). Because the EPS targets in executive bonus plans may be diluted or basic amounts, Bennett et al. (2017) compare the four EPS measures reported in Compustat (epspi, epspx, epsfi, epsfx) and select the measure that is closest to the target value. I follow this pattern with the exception that I also include the actual value as reported in I/B/E/S because the

[14] I focus on awards with a cash-based payout and omit time-based awards and relative performance-based awards.

EPS measures in bonus plans are often adjusted for non-GAAP exclusions. Importantly, the actual value reported in I/B/E/S is a non-GAAP value if the firm reports non-GAAP earnings in their earnings announcements. As Bennett et al. (2017) point out, "while this is likely to concentrate the distribution of Actual less target EPS around zero, it is not likely to bias our tests that compare the number of firms that just exceed the goal with the number that just miss the goal."

To help disentangle managers' opportunistic motivations for manipulating labor costs from those of managers acting in the best interests of the shareholders, I examine executive-level incentive (e.g., bonus target) measures. If managerial motives are more opportunistic and driven by short-term compensation incentives rather than duty to shareholders, then I expect the executive-level incentive measure (*BonusEPS_MB*) to load with a positive coefficient. If, on the other hand, managers are more motivated to perform on behalf of the shareholders, other stakeholders, or more long-term career concerns, then I anticipate the individual-level incentive measure will not be statistically significant.

Control Variables

In all my empirical tests, I include a vector of control variables, largely adopted from prior literature (Kothari et al., 2016; Raghunandan, 2021; Roychowdhury, 2006), to control for firm-level characteristics expected to be associated with variation in labor costs. I control for firm size (*FirmSize*), firm accounting performance (*ROA*), leverage (*Leverage*), cash holdings (*Cash*), dividends paid (*Dividend*), capital expenditures (*Capex*), debt issuance (*DebtIss*), equity issuance (*EquityIss*), and book-to-market value (*BM*). I control for the number of acquisitions completed during the year (*Acquisitions*), because new acquisitions may influence annual labor costs.1 Finally, I control for the percentage of outstanding shares held by institutional investors

(*InstOwn*), because some activist block holders favor stakeholder theory and the fair treatment of employees.

Raghunandan (2021) controls for the number of employees, the abnormal change in employees, and labor productivity when examining wage theft. However, because I am testing a more general case of labor cost manipulation, I omit these as control variables. That is, unlike wage theft, a more general manipulation of labor cost may involve abnormal changes in the number of employees. Thus, the inclusion of measures based on the number of employees may extract abnormal labor cost manipulation from the model residual.

IV. SAMPLE SELECTION, DESCRIPTIVE STATISTICS AND PREDICTION MODEL VALIDATION

Sample Description

I begin the sample selection process by obtaining firm fundamentals data from the Compustat annual file, total compensation of the top five executives from Execucomp. I also obtain, but do not require, institutional ownership data from the Thomson Reuters 13F dataset. These data sources provide the underlying variables to create measures for the dependent variables (described above) as well as all control variables. To proxy for firm-level incentives to manipulate labor costs, I require analyst forecasts and actual performance data from I/B/E/S. To proxy for executive-level incentives to manipulate labor costs, I obtain detailed CEO bonus plan metrics from Incentive Lab. The requirement of detailed bonus plan data limits the sample period to years after 2005, when the SEC began requiring such disclosures in form DEF-14A (Proxy Statements).

To test my first hypothesis, I retain 19,163 firm-year observations for the years 2006-2021 with all data required to calculate the dependent variables, control variables, as well as firm-level incentives.[15] To test my second hypothesis, I require CEO bonus data from Incentive Lab, which reduces the sample to 712 firm-year observations.[16] The severe reduction in sample

[15] With the exception of Institutional Ownership which is set to zero if unavailable (Huang et al., 2020).
[16] Because I include firm- and year-fixed effects, I also exclude any firm that has only one firm-year observations (singleton observation).

size results because Incentive Lab only covers the 750 largest firms and not all firm-years include an EPS metric in the CEO bonus plan. I conduct all analyses on two separate samples. The first are those firm-year observations with actual labor cost values reported on Compustat, while the second uses predicted labor costs from the prediction model discussed previously. The test samples for the first and second hypothesis using actual reported labor costs are 2,539 and 39 firm-year observations, respectively. The test samples for the first and second hypothesis using predicted labor costs are 16,405 and 591 firm-year observations, respectively. All continuous variables are winsorized at the 1^{st} and 99^{th} percentiles after their construction. Table 1 reports the sample selection process.

Descriptive Statistics

Table 2 presents descriptive statistics for the full sample of firms from 2006 – 2021. All variables are defined in Appendix B. Generally, the sample firms are large with mean assets of $11.2 billion and with a high percentage of institutional owners (72.5 percent). The sample firms are also profitable with mean ROA of 5.7 percent and hold a high percentage of total assets in cash (15.2 percent). I find that the average analyst EPS earnings surprise (*AnalystEPS_Surprise*) is approximately -2¢, and the median is 2¢. However, for bonus EPS metrics, the average surprise is approximately 20¢ while the median is -2¢. In Figure 1, I report the distribution of *AnalystEPS_Surprise* and *BonusEPS_Surprise*. In Panel A, I report the distribution of *AnalystEPS_Surprise* and note the discontinuity around zero. In Panel B, I report the distribution of *BonusEPS_Surprise* and I find a pattern consistent with the discontinuity identified in Bennett et al. (2017) and Atanasov et al. (2021).

Validation of Non-Executive Labor Costs Prediction Models

Validation of OLS Prediction of Non-Executive Labor Costs

To validate the accuracy of the non-executive labor cost prediction model, I conduct multiple analyses on various estimation and validation sample splits (e.g., 80 percent estimation, 20 percent holdout validation). The estimation sample is the portion of firms that actually report total staffing expense for which I estimate the parameters of the prediction model (EQ 1). The validation sample is the firms for which I apply the parameter estimates from EQ (1) to test the accuracy of the predicted value of non-executive labor cost to the actual value reported by these firms.

Before estimating EQ (1), I first identify each firm with available *NonExecPay* at any time. From this sample of firms that report total staffing expense, I hold out a random sample of 20 (40) [50] percent of the firms to validate the accuracy of the model. I do this to validate the accuracy of the prediction model on completely different firms. I then estimate EQ (1) for the other 80 (60) [50] percent of firms that have available *NonExecPay* (i.e., report total staffing expense in Compustat and have the total compensation of the top five executives available in Execucomp). From this estimation, I obtain coefficient weights for each of the six determinants, intercept as well as the industry- and year-fixed effects coefficients. Using these estimated coefficients from EQ (1), I calculate the predicted value of total staffing expense ($logNon\widehat{Exec}Pay^{OLS}$) for the hold-out sample. I validate the measure by (1) examining the Adj-R-square values of the prediction model, (2) checking the error percentage ((Predicted-Actual)/Actual), and (3) examining the Pearson correlation coefficient of the predicted and actual reported values of non-executive labor costs.

Table 3 reports the results of the estimations on the three different estimation/validation samples. Panel A reports the coefficients from the model estimation. In all three estimation samples, the coefficients for each predictor variable are positive and highly significant. Additionally, the coefficients appear to be relatively stable across the different estimation samples (80, 60 or 50 percent).[17] For the purposes of validation, it is important that the model explain a high portion of the variation in non-executive labor costs. In all three estimation samples, the model has an adjusted-R^2 greater than 85 percent, which suggests that the model is capturing a very high proportion of the variation in non-executive pay.

To measure the accuracy of the predictive model, I use the estimated coefficients from EQ (1) to calculate the predicted non-executive labor costs ($logNon\widehat{ExecPay}^{OLS}$) for the corresponding hold-out samples. Panel B of Table 3 reports the descriptive statistics of the predicted and actual values of *logNonExecPay* for the hold-out samples as well as the signed error percentage ((Predicted-Actual)/Actual) of *logNonExecPay* . In all three sample splits both the actual and predicted values of *logNonExecPay* and the errors are slightly right skewed (i.e., mean>median). However, the mean (median) signed errors are very small, ranging between 1.6 and 3.1 (-0.7 and 0.7) percent of actual non-executive pay. Although the mean errors are significantly greater than zero ($p<0.001$) in all three samples, the median values for the 60-40 and 50-50 split are statistically indistinguishable from zero using a Wilcoxon signed rank test.

I report the distribution of the prediction errors for the three hold-out samples in Figure 2. The evidence in Figure 2 clearly suggests that the prediction model does a good job of estimating

[17] Note that randomization was conducted using SAS with a random seed of 12345 for the 80-20 split and a seed of 54321 for the 60-40 and 50-50 split.

non-executive pay. Finally, I examine the Pearson correlations for the predicted and actual *logNonExecPay* for each industry (not tabulated). The correlations are high regardless of the splits for the estimation/validation sample ranging, on average within industries, from 82 to 88 percent. This suggests that despite the prediction error of the model, the predicted values and the actual values of *logNonExecPay* move together within industry quite well.

In Table 4, I report the validation results from estimating the prediction model with an 80-20 estimation/validation split for non-financial firms and financial firms separately.[18] For non-financial firms the coefficients in Panel A suggest that on average all of the predictors have a positive association with labor costs. The one exception being *INVT* (ending inventory), which flips to a negative coefficient due to multicollinearity with *COGS*. When *COGS* is omitted from the model, the coefficient on *INVT* is positive and significant. The adjusted-R^2 for the model is 0.920 suggesting the model explains a substantial portion of the variation in *logNonExecPay* . In Panel B, we see that the signed error percentages are very small ranging between 2.8 (mean) and -0.1 (median) percent. While the mean error is significantly different from zero (t-test, two-tailed p-values < 0.05) the median value is not (Wilcoxon signed rank test, p-value=0.388). Additionally, as Figure 3 Panel A illustrates, the distribution of the errors is approximately normal with the vast majority of observations centered very near zero.

For financial firms, we find very similar results. In Panel A, the adjusted-R^2 for the model is 0.839 suggesting the model explains less of the variation in non-executive labor costs than for non-financial firms, but still a very high amount. Some differences worth noting include the

[18] I also estimated this using 60-40 and 50-50 splits and the estimations in all respects are quantitatively and qualitatively identical to the estimates reported in Table 4.

visually larger coefficient weights on SG&A, advertising (ADV), and assets (ASSETS) for financial firms as well as the much smaller weight on COGS. These differences seem consistent with the nature of the industries and lend credibility to the prediction model. Importantly, for financial firms, the prediction error is very small. In Panel B, I report that the mean and median prediction errors are 0.5 and -0.2 percent, both of which are statistically indistinguishable from zero (p-value=.317 and p-value=.524, respectively). Finally, as Figure 3 Panel B illustrates, the distribution of the errors is approximately normal with the vast majority of observations centered very near zero.

Validation of Machine Learning Prediction of Non-Executive Labor Costs

I validate the machine learning labor cost prediction values in a manner similar to that of the OLS prediction. I first split the sample of firms that have a reported value for *NonExecPay* into financial firms and non-financial firms. Then each of these subsamples is split into a training sample and a testing sample, such that 80 percent of the firms are used to train the model and 20 percent of the firms are used for the testing sample. Again, note that this split is based on unique firms, not observations, and that the number of observations held out differ from the OLS holdout sample because different firms are held out during the randomization. The model is then trained separately on the financial and non-financial training samples using the XGBoost (gradient boosting) command in Python's Scikit Learn package. To train the model, I include the six predictors described in section 3.1.2, indicators for 2-digit SIC code industry classification, and indicators for year. I then tune the model using the procedures described in section 3.1.3.

After training the model, I apply the model of the gradient boosting algorithm to the testing sample and obtain predicted values of labor costs ($logNon\widehat{Exec}Pay^{Boost}$) for the hold-out sample. I validate the measure by (1) checking the error percentage ((Predicted-

Actual)/Actual) distribution and testing the difference in the mean and median signed error from zero and (2) examining the Pearson correlation coefficient of the predicted and actual reported values of non-executive labor costs. I do this separately for financial and non-financial firms. The boosting model appears to be quite accurate. In untabulated results I find that the mean accuracy score for the five separate training folds is 90% with a standard deviation of 2% for financial firms, and 86% with a standard deviation of 4% for non-financial firms, respectively.

Table 5 reports the mean, median, and standard deviation values of the actual and predicted values of *logNonExecPay* as well as the mean signed error. For non-financial firms the mean absolute error is 0.312 and the mean squared error is 0.184, untabulated. The mean prediction error percentage is 0.000 which is insignificantly different from zero using a t-test (p-value=0.491). The median prediction error percentage is -0.011 and is significantly different from zero using a Wilcoxon signed rank test (p-value=0.000. Figure 4 Panel A displays the distribution of the signed prediction error percentage for non-financial firms and shows that the errors are approximately normally distributed around zero. However, even when error exists the predicted values move together directionally very well as demonstrated by a correlation coefficient of 0.962, untabulated.

For financial firms the mean absolute error is 0.239 and the mean squared error is 0.348, untabulated. The mean prediction error percentage is 0.006 and is insignificantly different from zero using a t-test (p-value=0.209). The median prediction error percentage is -0.002 and is significantly different from zero using a Wilcoxon signed rank test (p-value=0.034). Figure 4 Panel B displays the distribution of the signed prediction error percentage and shows that the errors are approximately normally distributed around zero. Moreover, the correlation coefficient between actual and predicted values for the testing sample is 0.942 (untabulated) indicating that even when errors exist predicted values move in directions like the actual values quite well. Overall, these results suggest that the boosting algorithm does a very good job at predicting out of sample labor costs.

V. EMPIRICAL RESULTS

Empirical Results for Tests of H1

In Table 6, I report my first test of H1 by estimating EQ (3) using the proxy for abnormal labor costs derived from actual reported labor costs (*AbnLabor*). Column (1) reports the results using all firms that report actual labor costs. The coefficient of interest is *AnalystEPS_MB* which is equal to 1 if the firm met or just beat (0-2¢) analysts' earnings forecasts, otherwise zero. The positive and significant coefficient (0.250, two-tailed p-value < 0.10) on *AnalystEPS_MB* is consistent with firms facing pressure to meet analysts' expectations cutting labor costs significantly. Columns (2) and (3) separate the sample into those firms that voluntarily (non-required reporters) and mandatorily (required reporters) report labor costs. To separate firms into these categories, I use the following process. First, I first identify 3-digit SIC industries where at least 50 percent of the firms-years disclose total staffing expense in Compustat (XLR). If a firm-year is in one of these 3-digit SIC codes, I include the firm in the mandatory reporting group. If they are not in the 50 percent plus group, I categorize the firm in the voluntary reporting group. Column (2) reports the results using only voluntary reporting firms. The coefficient on *AnalystEPS_MB* is insignificant for this group. There are two potential explanations. First, because these firms voluntarily report this information, they are more careful with respect to discretionary cuts. Second, the insignificant coefficient may be due to a lack of power. In Column (3), I report results for mandatory reporters. The coefficient on *AnalystEPS_MB* is positive (0. 220) and significant (two-tailed p-value < 0.05). Consistent with the results in Column (1), this suggests that mandatory reporting firms cut labor costs to achieve the firm-level performance benchmarks.

In Table 7, I report the empirical tests of H1 using the predicted values of labor costs on the sample of firms that do not report actual labor costs. Results of estimating EQ (3) using the OLS labor cost prediction model are reported in column (1) while the results using the boosted prediction model are reported in column (2). In column (1), the 0.400 coefficient on *AnalystEPS_MB* is significant (two-tailed p-value < 0.05) suggesting that firms cut labor costs to meet or just beat analyst's EPS forecasts. I find similar results in column (2) using the boosted prediction model. In particular, the coefficient on *AnalystEPS_MB* is 0.729 and significant (two-tailed p-value < 0.05).

Overall, the results in Tables 6 and 7 suggest that firms cut labor costs as a means of achieving firm-level performance benchmarks and that this is true for both mandatory reporting firms as well as those that do not report labor costs. This evidence is sufficient to reject the null for H1. Importantly, the results in Table 7 are largely consistent with the results of Table 6. This suggests that abnormal labor cost measures derived from the use of this predicted values for labor costs (from OLS regression or gradient boosting) achieve convergent results to those measures derived from using actual reported labor costs.

Empirical Results of H2

To test my second hypothesis, I estimate EQ (4) using the two predicted values of abnormal labor costs. I report the results of estimating EQ (4) in Table 8. The coefficient of interest in EQ (4) is *BonusEPS_MB*. This variable is equal to 1 if the firm exactly meets the CEOs bonus target or exceeds it by 2 cents or less. Column (1) reports the results using abnormal labor from the OLS prediction model while column (2) relies on abnormal labor cost from the boosted prediction model.[19] The results in both columns are consistent. Specifically, I find no significant evidence that CEOs manipulate labor costs to just meet their individual bonus target. I find that the coefficients on *BonusEPS_MB* is insignificant in both columns. Moreover, the 90 percent confidence intervals for the coefficient Column (1) ranges from -2.351 to 3.447 and the 90 percent confidence intervals for the coefficient in Column (2) ranges from -6.844 to 9.212.[20]

Additional Analyses

Labor Unionization

Prior literature has extensively examined the role of labor unions in a variety of contexts. For example prior literature suggests that labor unions reduce operational flexibility (Chen et al. 2011) forcing managers to be more creative with cost savings such as increased tax aggressiveness (Chyz et al. 2013) and increased real activities management (Chun et al. 2017). Additionally, Andreicovici et al. (2021) find that firms are more likely to manage earnings down using accruals prior to a large layoff when trade unions are stronger to provide an economic justification for the dismissal.

Collectively, this stream of research suggests that stronger industry unionization may have a disciplining effect on the cuts to employee compensation. That is, while managers may creatively use real activities management they may be less likely to use employee labor costs as a mechanism

---

[19] I omit results for the actual sample as there are too few observations for any meaningful test of this hypothesis. However, the lack of a result is consistent with the results displayed in Table 8.

[20] A possible explanation for finding no result in Table 8 is that moving from just missing the target to meeting or just beating the target has a very small economic consequence for the executive because of the linear relationship between payout and performance that is the typical bonus structure between the threshold and the maximum performance benchmarks. However, the difference in the payout when moving from just missing to just beating the threshold benchmark is likely more salient due to the stepwise relationship (i.e., typically going from zero bonus to being awarded 50 percent of the target payout).

to that end. To test this, I modify EQ (3) and EQ (4) to include an interaction of the meet/beat indicator variable and the level of industry unionization.[21] If stronger trade unions reduce the opportunity for managers to manipulate labor costs when seeking to achieve performance benchmarks then I would expect a negative coefficient on this interaction term.

Table 9 reports the results from this modification of EQ (3) and estimating on the sample of firms that do report labor costs. Specifically, Table 9 includes a measure for the level of industry union member coverage (*Unionization*) and the interaction between union coverage and the analyst EPS meet/beat indicator (*MB×Unionization*). Column (1) reports the use of all observation, Column (2) reports the results for the sample of reporting firms that are not in a mandatory reporting industry, and Column (3) reports the results for the firms that are in mandatory industries. The coefficient on the meet/beat indicator (*AnalystEPS_MB)* continues to be positive and significant in Column (1) and Column (3). Specifically, the coefficient is 0.297 in Column (1) (p-value <0.10) and is 0.324 in Column (3) (p-value<0.01), The coefficient on the interaction term (*MB×Unionization*) is also negative in Columns (1) (-0.783) and (3) (-2.758) but is only significant in Column (3) (p-value<0.05). Together, this suggests that managers use abnormal cuts to labor costs as a mechanism for achieving analyst EPS forecast benchmark less often when the industry has strong labor unions.

When I repeat this analysis using the two abnormal labor cost proxies derived from the predicted labor cost values, I find main effect results on the meet/beat measure consistent with results reported previously in Table 7. That is, in the results reported in Table 10, I only find a significant result on the analyst meet/beat measure when using the proxy derived from the gradient

---

[21] Note that the requirement of the industry unionization data reduces the sample sizes slightly.

boosting method. However, in these tests I do not find a significant interaction coefficient in either column. Altogether, I find some evidence that labor unions minimize the use of labor costs as a mechanism for achieving analyst EPS forecast benchmarks, this evidence is not consistent across all of my analyses.

In Table 11, I report the results of modifying EQ (4) to include an interaction (*MB×Unionization*) between the EPS bonus target meet/beat indicator *(BonusEPS_MB*) and the industry unionization measure (*Unionization*). As in Table 8, the main effects of just meeting or beating the EPS bonus target do not appear to be associated with abnormal cuts to labor costs when using either the OLS derived proxy for abnormal labor costs in Column (1) or the gradient boosting derived proxy in Column (2). Additionally, the coefficient on the *MBxUnionization* interaction term is not significant in either Column (1) or Column (2).

Equity and Bonus Compensation

In the final set of analyses, I examine alternative measures that may incentivize managers to manipulate employee labor costs including equity compensation intensity, bonus compensation intensity, and bonus sensitivity to earnings by replacing the analyst forecast meet/beat measure (*AnalystEPS_MB*) with these incentives. I first create a measure of how important equity compensation is to the executive by calculating the equity compensation intensity (*EquityIntensity*) as the CEO's total equity compensation (options and restricted stock) as a proportion of their total compensation. I also create an indicator variable equal to one if the CEO's equity intensity is in the top quartile during the year (*HighEquityIntensity*). I expect that when managers are more incentivized to be concerned with stock price, rather than short-term compensation, they will be less likely to cut labor costs abnormally. If this is the case, I expect a negative coefficient on both measure of equity intensity.

Table 12 reports the results of modifying EQ (3) with these measures. Panel A, Column (1) reports the results using the OLS measure of abnormal labor costs. The coefficient on the equity intensity measure is -0.732 and significant (p-value<0.05). This suggests that as equity intensity increases managers are less likely to cut labor costs. Column (2) reports the results using the boosted measure of abnormal labor costs. Although the coefficient on equity intensity is negative, it is not significant. Panel B replaces the analyst forecast meet/beat measure with the indicator *HighEquityIntensity* and, consistent with Panel A, the coefficient is negative and significant in Column (1) (-0.366, p-value<0.05), but insignificant in Column (2). Taken together these results provide some support for the prediction that when managers are incentivized with a longer horizon they are less likely to cut labor costs.

Table 13 displays the result of replacing the *AnalystEPS_MB* measure in EQ (3) with measure of bonus compensation intensity. I calculate bonus compensation intensity (*BonusIntensity*) as the sum of bonus compensation and nonequity incentive pay divided by the CEO's total compensation. Panel A displays the results when using this continuous measure and Panel B reports the results of using an indicator for observations that are in the top quartile of the year (*HighBonusIntensity*). If managers are more highly incentivized to consider short-term compensation in the form of annual bonuses, I expect them to make larger abnormal cuts to labor costs as a way to achieve short-term performance goals. That is, I expect a positive coefficient on the two measures of bonus intensity.

Panel A, Column (1) reports the results of using the OLS modelled labor costs and Column (2) reports the results when using the boosting technique. Column (1) reports a significant coefficient on *BonusIntensity* of 1.569 (p-value<0.01) suggesting that as more of a CEO's annual compensation comes from bonuses they make more abnormal cuts to labor costs. In Column (2), however, the result is insignificant. In Panel B the coefficient on *HighBonusIntensity* is 0.379 and is significant (p-value<0.05) in Column (1), again suggesting that as managers are paid more with short-term bonus compensation, they are more likely to make abnormal cuts to labor costs. Similar to Panel A, Column (2) is insignificantly different from zero.

Although the results are not consistent across both predicted values of labor costs, the preceding results from Tables 12 and 13 collectively suggest that when managers are incentivized with a longer horizon by receiving more of their annual compensation in the form of options or stock they are less likely to make abnormal cuts to labor. Similarly, when managers are incentivized with a shorter-term focus they are more likely to make cuts to labor costs, arguably with the intent to achieve short-term performance goals such as analyst forecasts or annual EPS bonus targets.

VI. CONCLUSION

Human capital has recently become a major component of firm valuation (Zingales 2000) and commensurate with that change has come a renewed interest in the fair treatment and compensation of employees as stakeholders in the firm. Regulation S-K has twice been amended in recent years to increase disclosures related to human capital and pay inequity. In particular, the requirement of the CEO-to-Median Employee pay ratio is directly related to pay disparities within firms. Additionally, the Business Roundtable and many prominent CEOs have committed to the fair compensation of employees. However, managers serving as agents are often required to make difficult choices to achieve consistent performance on behalf of the shareholders. Anecdotal evidence and prior literature suggest that the labor force may bear an inordinate burden of such decisions.

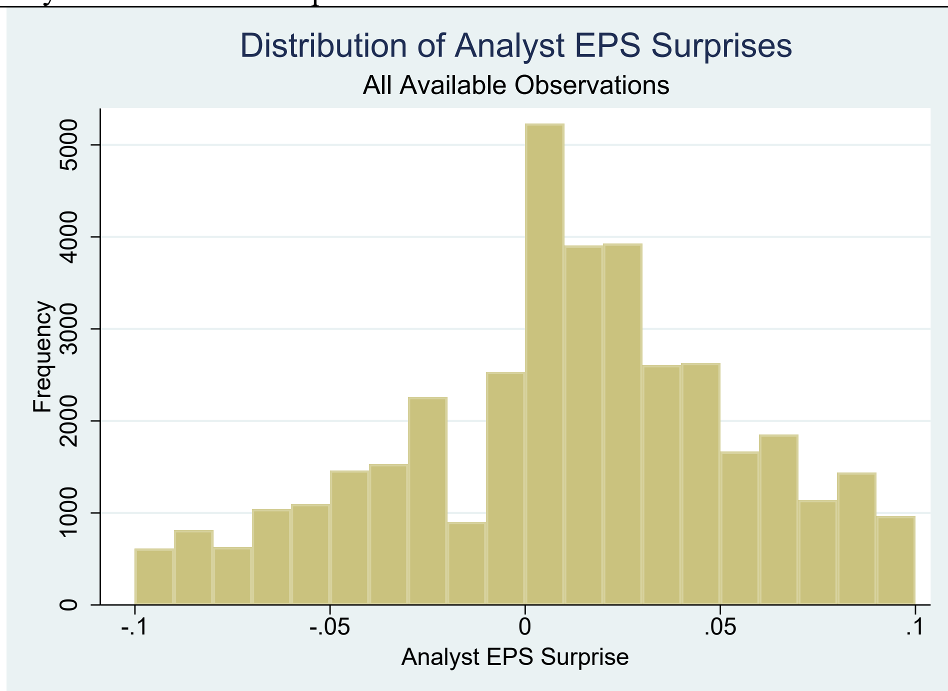
Part of signaling consistent performance involves achieving firm-level performance benchmarks because missing them can have negative effects on shareholder value (Bartov et al., 2002; Caylor et al., 2007). Prior literature suggests that managers are often willing to make real changes to activities and even sacrifice some future value to achieve these benchmarks (e.g., Burgstahler and Dichev, 1997; Graham et al., 2005). Therefore, understanding management's willingness to manipulate labor costs to achieve these benchmarks is important to understand management's perception of labor costs as a discretionary expense. I provide evidence that this is the case, by finding that firms cut employee labor costs to just meet/beat analysts' EPS forecasts. Additionally, I find some evidence that labor unions impede manager's ability to cut labor costs for the purpose of achieving these firm-level benchmarks.

As it relates to fairness, it is crucial to understand managerial motives for manipulating labor costs. That is, reducing labor costs to meet analysts' earnings forecasts may be detrimental to employees, but it may protect shareholder value. While this may appear unfair to the employees, the manager has a fiduciary responsibility to the shareholder despite any commitments they may have made to employees. However, cutting labor costs to achieve performance benchmarks in a manager's own bonus package is much less justifiable and can stoke perceptions of unfair treatment. In my analyses, I do not provide any evidence that managers are using labor cost cuts to meet their individual bonus targets, which may be interpreted as managers are making tough choices that they believe are in the best interests of the shareholders.

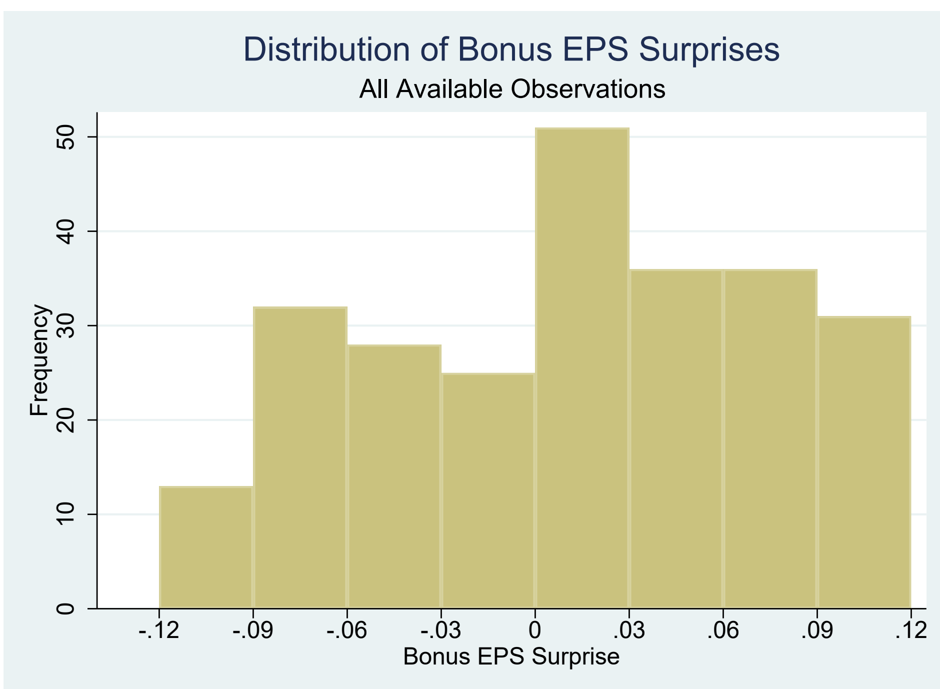
I further investigate manager motives by examining alternative incentives. I find that managers are more likely to manipulate labor costs abnormally when they are compensated with greater annual bonuses as a proportion of their total annual pay. Similarly, I find that when managers receive more of their annual compensation in the form of equity, they are less likely to make these abnormal cuts to labor. Ultimately, this suggests that managers consider labor to be of strategic long-term value but are willing to cut these costs to achieve short-term goals.

Figure 1
Distribution of Analyst EPS Forecast Surprises and Bonus EPS Surprises

Panel A: Analyst EPS Forecast Surprises



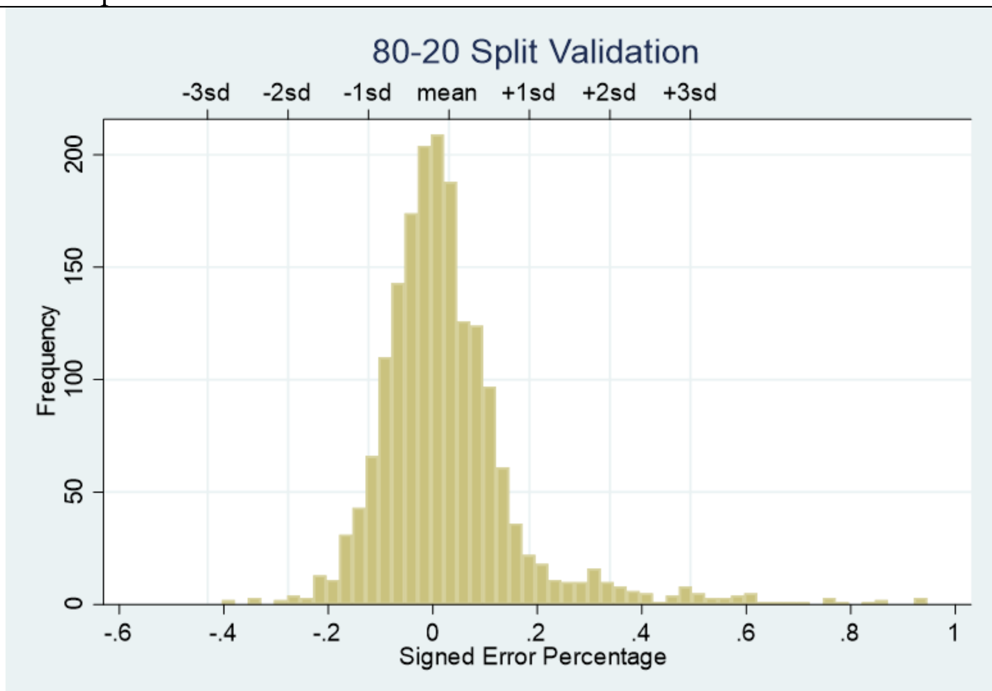
Panel B: Bonus EPS Surprises



Notes: In Panel A values are truncated at -10¢ and 10¢ and the bin width is 1¢. In Panel B values are truncated at -12¢ and 12¢ and the bin width is 3¢

Figure 2
Histograms of the Prediction Error for the Split Samples

Panel A: 80-20 Split



Panel B: 60-40 Split s

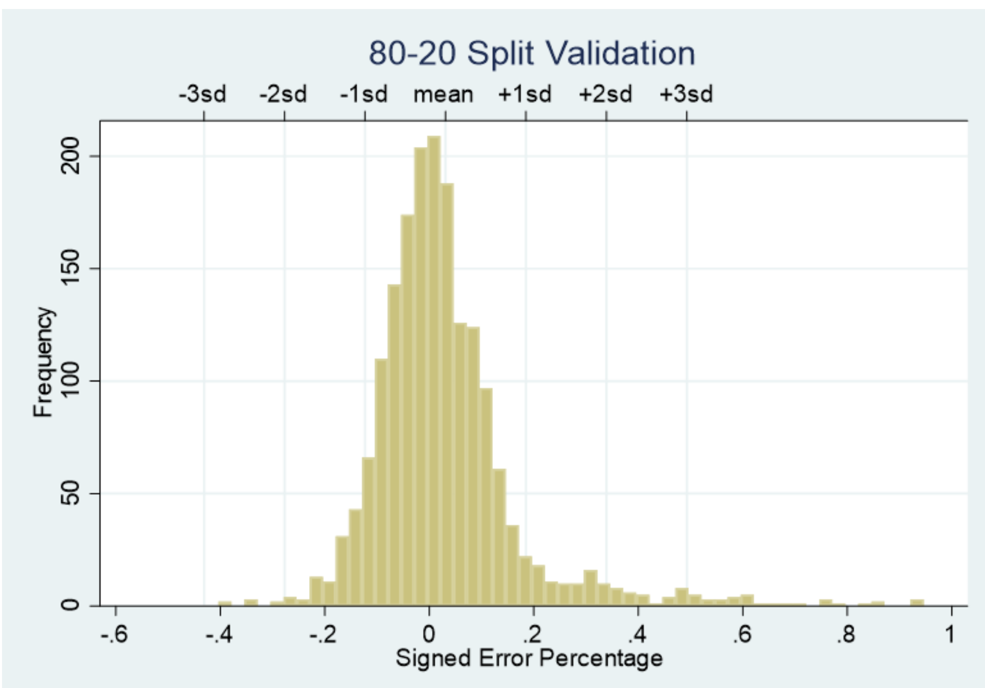
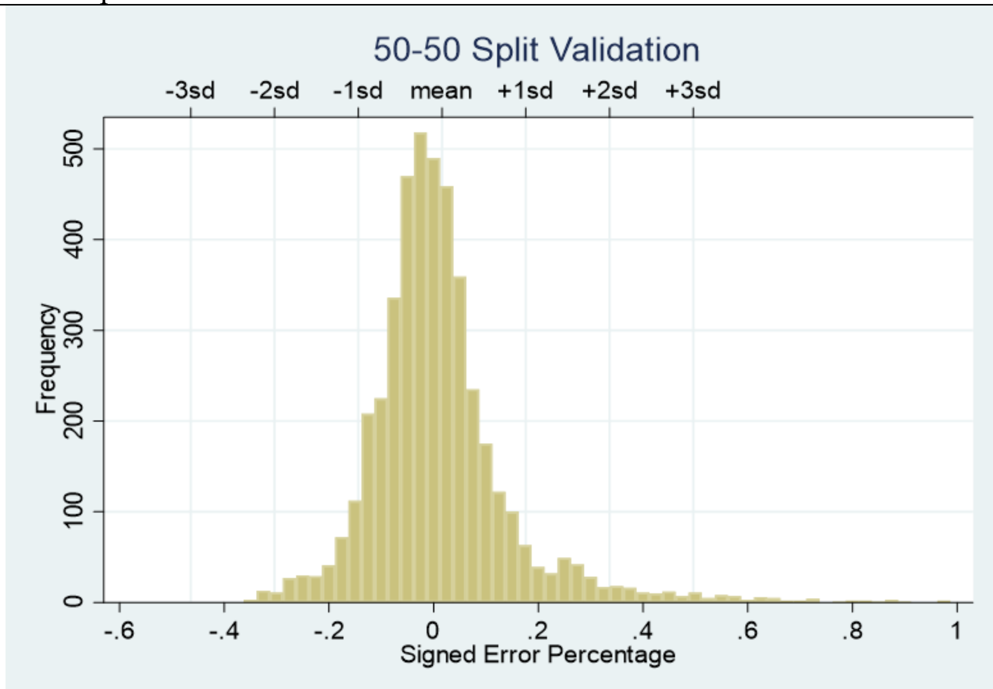


Figure 2 (cont.)
Histograms of the Prediction Error for the Split Samples

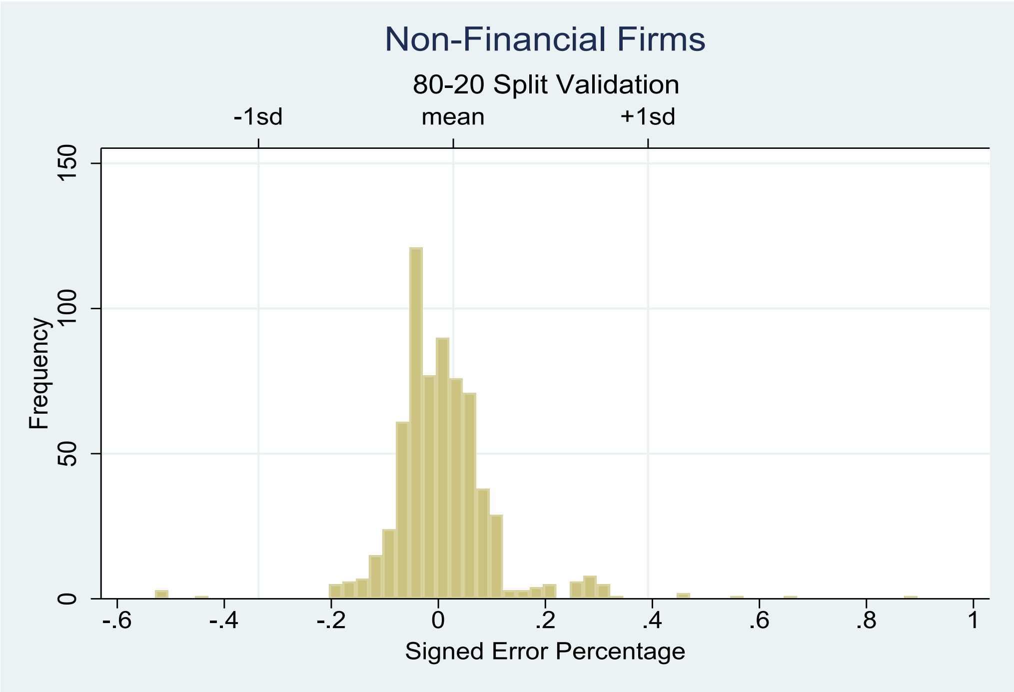
Panel C: 50-50 Split



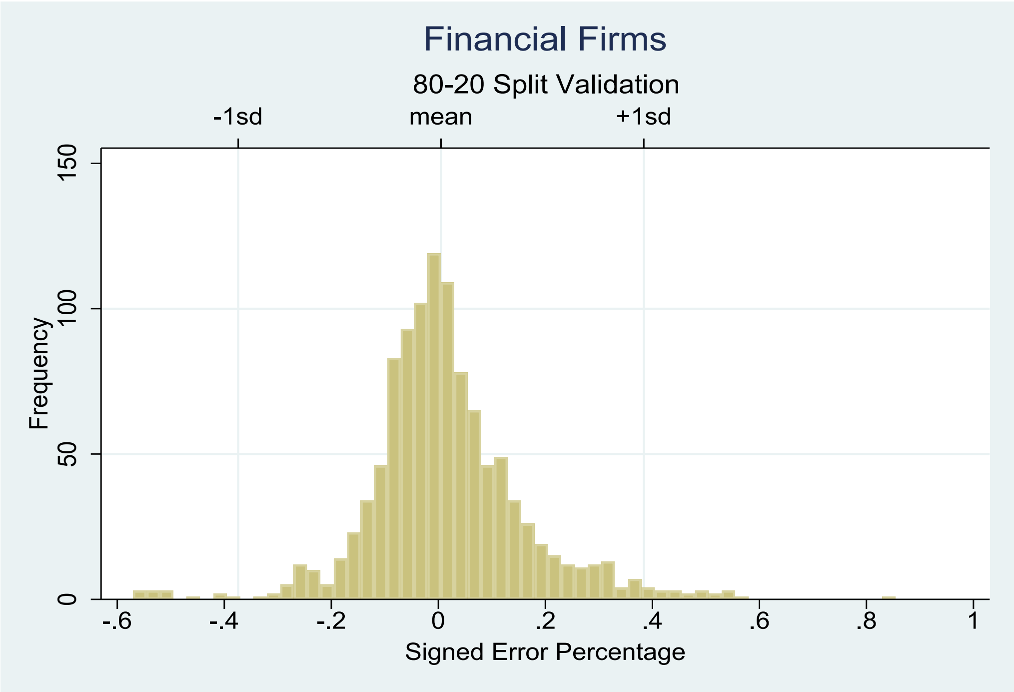
Note: Figures are truncated at -0.6 and 1.0 and the bin width is 0.25 in Panels A, B, and C.

Figure 3
Histograms of the OLS Prediction Error

Panel A: Non-Financial Firms



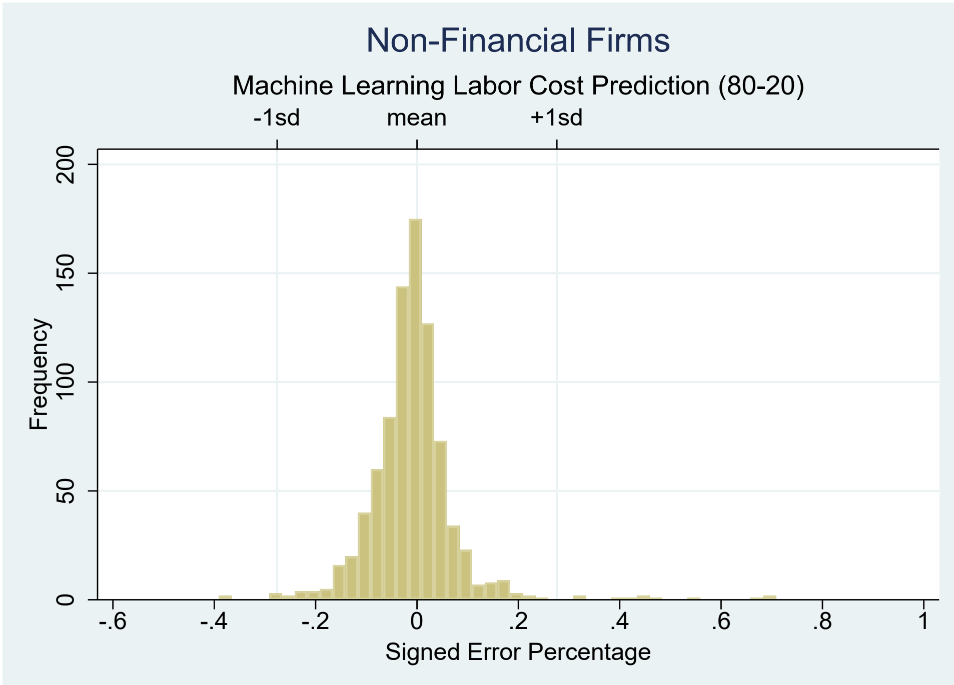
Panel B: Financial Firms



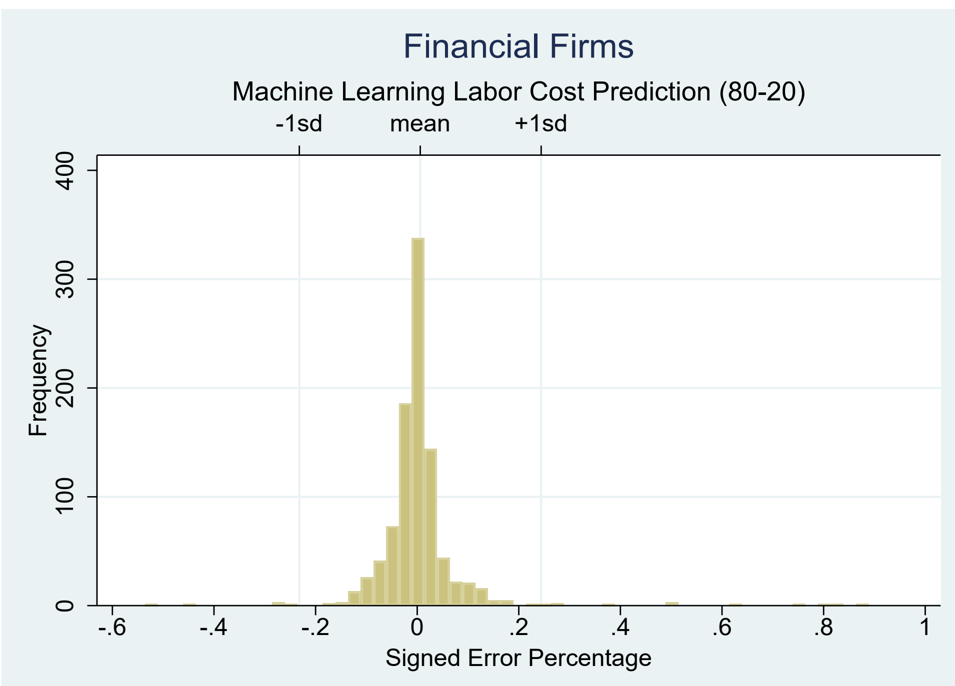
Notes: Figures are truncated at -0.6 and 1 and the bin width is 0.25 in both Panels A and B.

Figure 4
Histograms of the Boosting Prediction Error

Panel A: Non-Financial Firms



Panel B: Financial Firms



Notes: Figures are truncated at -.6 and 1 and the bin width is 0.25 in both Panels A and B.

Table 1
Sample Selection

	Analyst Forecast Sample 2006-2021	Bonus Target Sample 2006-2021
Beginning Compustat observations 2006-2021	179,325	179,325
Less firm-years with		
Missing Predicted Non-Executive Labor Costs	67,153	67,153
Missing Coverage in Execucomp (top5pay)	81,238	81,238
Missing Abnormal REM measures	5,161	5,161
Missing Earnings Forecast data in I/B/E/S	2,583	2,583
Missing Control Variables	4,027	4,027
Missing EPS Bonus data		18,451
Full Sample of firm-years	19,163	712
Firm years with reported labor costs	2,576	54
Less: Singleton Observations	37	15
Actual Labor Cost Sample	2,539	39
Firm-years without reported labor costs	16,587	658
Less: Singleton Observations	182	66
Predicted Labor Cost Sample	16,405	592

Table 2
Sample Descriptive Statistics

Variable	N	Mean	Median	SD
$AbnLabor$	2,576	0.03	0.06	2.56
$Abn\widehat{Labor}^{Reg}$	19,163	-3.42	-5.86	30.28
$Abn\widehat{Labor}^{Boost}$	19,163	-4.94	-8.31	39.16
AnalystEPS_Surprise	19,163	-0.02	0.02	1.28
AnalystEPS_MB	19,163	0.19	0.00	0.40
BonusEPS_Surprise	712	0.19	-0.02	1.95
BonusEPS_MB	712	0.02	0.00	0.13
Assets	19,163	11246.72	2405.11	30440.59
Firmsize	19,163	7.86	7.79	1.68
ROA	19,163	0.06	0.06	0.11
Leverage	19,163	0.24	0.21	0.23
Cash	19,163	0.15	0.09	0.16
Dividend	19,163	0.02	0.00	0.03
Capex	19,163	0.04	0.03	0.05
DebtIssue	19,163	0.01	0.00	0.08
EquityIssue	19,163	0.02	0.00	0.05
BM	19,163	0.46	0.40	0.72
IOR	19,163	0.72	0.82	0.29
Acquisition	19,163	0.03	0.00	0.17
Unionization	18,960	0.06	0.03	0.08

All variables are defined in Appendix.

Table 3
Predicting and Validating Non-Executive Pay (with a Hold-Out Sample)

Panel A: Estimation of EQ (1) logNonExecPay = $\beta_0 + \beta_1$SG&A + β_2R&D + β_3ADV + β_4COGS + β_5INVT + β_6ASSETS + ε

	80-20 Split	60-40 Split	50-50 Split
Intercept	-1.672***	0.092	0.146
SG&A	0.064***	0.073***	0.074***
R&D	0.048***	0.075***	0.082***
ADV	0.087***	0.083***	0.082***
COGS	0.479***	0.438***	0.447***
INVT	0.019***	0.016***	0.012**
ASSETS	0.369***	0.395***	0.382***
Industry Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
N	7,426	5,638	4,782
Adj-R^2	0.878	0.868	0.858

Panel B: Estimations for Hold-Out Sample from Prediction Model

	80-20 Split N = 1,816			60-40 Split N = 3,604			50-50 Split N = 4,460		
	Mean	Med	SD	Mean	Med	SD	Mean	Med	SD
$log\widehat{NonExecPay}^{OLS}$ (P)	5.654	5.570	1.405	5.850	5.649	1.575	5.794	5.585	1.587
logNonExecPay (A)	5.575	5.477	1.507	5.868	5.721	1.731	5.813	5.644	1.731
Prediction Error (P-A)/A	0.031***	0.007***	0.154	0.017***	-0.007	0.164	0.016***	-0.007	0.160
p-value (difference from zero)	<0.001	<0.001		<0.001	0.255		<0.001	0.254	

* two-tailed p-value < 0.1, ** two-tailed p-value < 0.05, *** two-tailed p-value < 0.01

logNonExecPay = log(1 + (Total firm staffing expense less compensation of top five executives));
$log\widehat{NonExecPay}$ = predicted value of *logNonExecPay*
SG&A = log(1+ Selling, General, and Administrative Expenses);
R&D = log(1+ Research and Development Expenses);
ADV = log(1+ Advertising Expenses);
COGS = log(1+ Cost of Goods Sold);
INVT = log(1+ Ending Inventory);
Firmsize = log(1+ Ending Total Assets)

Table 4
Predicting and Validating Non-Executive Pay by Industry Type (using 80-20 Split)

Panel A: Estimation of EQ (1) logNonExecPay = $\beta_0 + \beta_1$SG&A + β_2R&D + β_3ADV + β_4COGS + β_5INVT + β_6ASSETS + ε

	Non-Financial Firms	Financial Firms
Intercept	-2.741***	-3.740***
SG&A	0.035***	0.142***
R&D	0.045***	0.071
ADV	0.046***	0.113***
COGS	0.851***	0.444***
INVT	-0.028***	0.033***
Firmsize	0.184***	0.368***
Industry Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
N	3,325	4,212
Adj-R^2	0.920	0.839

Panel B: Estimations from Prediction Model

	Non-Financial Firms N = 673			Financial Firms N = 1,120		
	Mean	Med	SD	Mean	Med	SD
$log\widehat{NonExecPay}^{OLS}$ (P)	6.291	6.283	1.642	5.045	4.980	1.600
logNonExecPay (A)	6.287	6.179	1.730	5.093	4.916	1.497
Prediction Error (P-A)/A	0.028**	-0.001	0.364	0.005	-0.002	0.379
p-value (difference from zero)	0.025	0.388		0.317	0.524	

Table 5
Predicting and Validating Non-Executive Pay as predicted using Gradient Boosting

Estimations from Prediction Model

	Non-Financial Firms N = 858			Financial Firms N = 969		
	Mean	Med	SD	Mean	Med	SD
$logNon\widehat{ExecPay}^{Boost}$ (P)	6.315	6.223	1.492	5.420	5.143	1.720
logNonExecPay (A)	6.336	6.256	1.487	5.421	5.147	1.752
Prediction Error (P-A)/A	0.000	-0.011***	0.276	0.006	-0.002**	0.238
p-value (difference from zero)	0.491	0.000		0.209	0.034	

* two-tailed p-value < 0.1, ** two-tailed p-value < 0.05, *** two-tailed p-value < 0.01

logNonExecPay = log(1 + (Total firm staffing expense less compensation of top five executives));

$logNo\widehat{nEx}ecPay$ = predicted value of *logNonExecPay*

Table 6
Empirical Tests of Hypothesis 1 – Actual Reported Labor Cost

	All Observations	Non-Required Reporters	Required Reporters
	(1)	(2)	(3)
VARIABLES	*AbnLabor*	*AbnLabor*	*AbnLabor*
AnalystEPS_MB	0.250*	0.405	0.220**
	(0.144)	(0.625)	(0.100)
Firmsize	0.586***	-0.066	0.506***
	(0.213)	(1.034)	(0.147)
ROA	3.204	14.581**	-3.778**
	(2.036)	(5.660)	(1.785)
Leverage	3.495***	9.764**	2.194***
	(0.718)	(3.934)	(0.495)
Cash	1.935	4.117	-0.418
	(1.290)	(3.950)	(1.121)
Dividend	3.382	9.223	0.435
	(5.641)	(16.044)	(4.935)
Capex	-5.169	-8.925	-7.281***
	(3.184)	(8.890)	(2.713)
DebtIssue	0.150	1.067	0.646
	(1.188)	(4.028)	(0.927)
EquityIssue	-15.207***	-20.817**	-4.961*
	(3.215)	(8.095)	(2.938)
BM	-0.045	0.421	-0.015
	(0.117)	(0.423)	(0.087)
IOR	0.450	5.212*	0.326
	(0.476)	(2.884)	(0.327)
Acquisition	-2.444***	-2.984**	0.821
	(0.592)	(1.263)	(0.744)
Constant	-6.244***	-6.622	-5.002***
	(1.888)	(7.845)	(1.350)
Observations	2,539	533	2,005
R-squared	0.093	0.173	0.132
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1
This table tests the association between firms just meeting or beating analyst EPS forecasts and abnormal labor costs as calculated using actual reported values. Column (1) shows the results using the full sample of firms with available data. Column (2) reports results for those firms that are not required to disclose labor costs but do (voluntary). Column (3) reports the results for firms that are required to report labor costs and do (compliant). All variables are defined in Appendix.

Table 7
Empirical Tests of Hypothesis 1 – Predicted Labor Cost

VARIABLES	(1) $AbnLabor^{OLS}$	(2) $AbnLabor^{Boost}$
AnalystEPS_MB	0.400**	0.729**
	(0.196)	(0.301)
Firmsize	-0.193	0.683**
	(0.204)	(0.312)
ROA	4.185***	4.678***
	(0.884)	(1.354)
Leverage	1.827**	2.451**
	(0.723)	(1.108)
Cash	0.260	2.372
	(0.946)	(1.450)
Dividend	3.810	2.839
	(3.340)	(5.118)
Capex	-8.358***	-10.437**
	(3.038)	(4.656)
DebtIssue	-5.187***	-8.084***
	(0.978)	(1.498)
EquityIssue	-11.727***	-12.509***
	(1.562)	(2.394)
BM	-0.269*	-0.433**
	(0.139)	(0.213)
IOR	1.610***	1.846**
	(0.489)	(0.749)
Acquisition	-2.418***	-2.634***
	(0.435)	(0.667)
Constant	-3.132*	-11.743***
	(1.628)	(2.495)
Observations	16,405	16,405
R-squared	0.933	0.900
Firm FE	Yes	Yes
Year FE	Yes	Yes

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1
This table tests the association between firms just meeting or beating analyst EPS forecasts and abnormal labor costs as calculated using predicted labor cost values. The sample is restricted to firms that do not report actual labor costs. Column (1) reports the results for predicted labor costs derived from the OLS model. Column (2) reports the results for predicted labor costs derived from the Boosting model. All variables are defined in Appendix.

Table 8
Empirical Test of Hypothesis 2

VARIABLES	(1) *AbnLabor*^{OLS}	(2) *AbnLabor*^{Boost}
BonusEPS_MB	0.548	1.184
	(1.759)	(4.871)
Firmsize	-1.103	0.483
	(0.980)	(2.713)
ROA	-1.138	4.374
	(5.515)	(15.273)
Leverage	1.099	19.000*
	(3.643)	(10.087)
Cash	6.385	14.903
	(4.165)	(11.533)
Dividend	-41.940	-42.906
	(27.675)	(76.636)
Capex	3.706	26.953
	(14.812)	(41.017)
DebtIssue	-5.099	-42.877***
	(3.518)	(9.741)
EquityIssue	5.641	-34.725
	(12.381)	(34.286)
BM	1.327	-2.338
	(1.988)	(5.506)
IOR	-0.778	-2.827
	(2.287)	(6.332)
Acquisition	1.039	5.493
	(1.247)	(3.453)
Constant	4.064	-16.017
	(9.204)	(25.489)
Observations	591	591
R-squared	0.966	0.880
Firm FE	Yes	Yes
Year FE	Yes	Yes

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1
This table tests the association between firms just meeting or beating EPS bonus targets and abnormal labor costs as calculated using predicted labor cost values. The sample is restricted to firms that do not report actual labor costs. Column (1) reports the results for predicted labor costs derived from the OLS model. Column (2) reports the results for predicted labor costs derived from the Boosting model. All variables are defined in Appendix.

Table 9
Impact of Industry Unionization on Labor Cost Manipulation to Achieve Analyst EPS Forecasts – Actual Reported Labor Cost

	All Observations	Non-Required Reporters	Required Reporters
	(1)	(2)	(3)
VARIABLES	*AbnLabor*	*AbnLabor*	*AbnLabor*
AnalystEPS_MB	0.297*	-0.031	0.324***
	(0.163)	(0.848)	(0.110)
MBxUnionization	-0.783	6.914	-2.758**
	(1.676)	(7.353)	(1.177)
Unionization	7.418***	9.186	7.849***
	(2.012)	(8.236)	(1.513)
Firmsize	0.618***	0.129	0.502***
	(0.212)	(1.045)	(0.146)
ROA	3.331	13.486**	-3.289*
	(2.034)	(5.722)	(1.775)
Leverage	3.512***	9.161**	2.189***
	(0.716)	(3.973)	(0.491)
Cash	2.198*	4.719	-0.003
	(1.287)	(3.993)	(1.111)
Dividend	2.659	9.403	-0.557
	(5.618)	(16.139)	(4.882)
Capex	-6.852**	-10.010	-9.438***
	(3.211)	(9.017)	(2.727)
DebtIssue	0.217	1.247	0.740
	(1.184)	(4.051)	(0.918)
EquityIssue	-15.122***	-20.620**	-5.007*
	(3.199)	(8.121)	(2.905)
BM	-0.072	0.345	-0.016
	(0.117)	(0.429)	(0.086)
IOR	0.547	4.867*	0.471
	(0.475)	(2.924)	(0.324)
Acquisition	-2.306***	-3.023**	1.151
	(0.590)	(1.269)	(0.739)
Constant	-6.960***	-8.523	-5.415***
	(1.884)	(7.945)	(1.336)
Observations	2,527	525	2,001
R-squared	0.099	0.180	0.146
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1
This table tests the influence of industry labor unionization on the association between firms just meeting or beating analyst EPS forecasts and abnormal labor costs as calculated using actual reported values. Column (1) shows the results using the full sample of firms with available data. Column (2) reports results for those firms that are not required to disclose labor costs but do (voluntary). Column (3) reports the results for firms that are required to report labor costs and do (compliant). All variables are defined in Appendix.

Table 10
Impact of Industry Unionization on Labor Cost Manipulation to Achieve Analyst EPS Forecasts – Predicted Labor Cost

VARIABLES	(1) $AbnLabor^{OLS}$	(2) $AbnLabor^{Boost}$
AnalystEPS_MB	0.414	0.935**
	(0.257)	(0.404)
MBxUnionization	-0.369	-3.037
	(2.798)	(4.401)
Unionization	3.045	4.027
	(2.404)	(3.781)
Firmsize	-0.123	0.687**
	(0.202)	(0.317)
ROA	4.038***	4.674***
	(0.869)	(1.367)
Leverage	1.638**	2.283**
	(0.717)	(1.127)
Cash	0.027	2.453*
	(0.932)	(1.465)
Dividend	4.174	2.867
	(3.278)	(5.155)
Capex	-8.977***	-10.199**
	(2.989)	(4.700)
DebtIssue	-5.233***	-8.034***
	(0.963)	(1.514)
EquityIssue	-11.693***	-12.343***
	(1.540)	(2.422)
BM	-0.278**	-0.449**
	(0.136)	(0.214)
IOR	1.758***	1.851**
	(0.481)	(0.757)
Acquisition	-2.459***	-2.649***
	(0.427)	(0.672)
Constant	-4.134**	-12.470***
	(1.619)	(2.545)
Observations	16,212	16,212
R-squared	0.935	0.895
Firm FE	Yes	Yes
Year FE	Yes	Yes

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1
This table tests the influence of industry labor unionization on the association between firms just meeting or beating EPS bonus targets and abnormal labor costs as calculated using predicted labor cost values. Column (1) reports the results for predicted labor costs derived from the OLS model. Column (2) reports the results for predicted labor costs derived from the Boosting model. All variables are defined in Appendix.

Table 11
Impact of Industry Unionization on Labor Cost Manipulation to Achieve EPS Bonus Targets

VARIABLES	(1) $AbnLabor^{OLS}$	(2) $AbnLabor^{Boost}$
BonusEPS_MB	1.875	0.856
	(2.675)	(7.449)
MBxUnionization	-19.177	4.088
	(24.765)	(68.978)
Unionization	8.830	-0.755
	(7.201)	(20.057)
Firmsize	-1.907*	0.456
	(1.100)	(3.064)
ROA	-2.022	4.329
	(5.580)	(15.542)
Leverage	1.346	18.889*
	(3.663)	(10.203)
Cash	6.384	14.995
	(4.169)	(11.612)
Dividend	-50.249*	-43.175
	(28.112)	(78.301)
Capex	-0.721	27.192
	(14.978)	(41.719)
DebtIssue	-4.200	-42.981***
	(3.553)	(9.895)
EquityIssue	7.319	-34.687
	(12.443)	(34.657)
BM	1.072	-2.348
	(1.993)	(5.550)
IOR	-1.122	-2.750
	(2.307)	(6.425)
Acquisition	0.671	5.590
	(1.279)	(3.561)
Constant	11.162	-15.991
	(10.298)	(28.684)
Observations	588	588
R-squared	0.965	0.876
Firm FE	Yes	Yes
Year FE	Yes	Yes

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1
This table tests the influence of industry labor unionization on the association between firms just meeting or beating EPS bonus targets and abnormal labor costs as calculated using predicted labor cost values. Column (1) reports the results for predicted labor costs derived from the OLS model. Column (2) reports the results for predicted labor costs derived from the Boosting model. All variables are defined in Appendix.

Table 12
Impact of the CEO's Equity Compensation Intensity on Labor Cost Manipulation

Panel A: Continuous Equity Intensity Measure

VARIABLES	(1) $AbnLabor^{OLS}$	(2) $AbnLabor^{Boost}$
EquityIntensity	-0.732**	-0.461
	(0.342)	(0.535)
Firmsize	-0.535***	0.180
	(0.190)	(0.296)
ROA	1.144	0.532
	(0.823)	(1.285)
Leverage	0.777	1.138
	(0.677)	(1.058)
Cash	-1.139	0.498
	(0.876)	(1.368)
Dividend	2.887	1.764
	(3.096)	(4.835)
Capex	-7.457***	-9.243**
	(2.826)	(4.414)
DebtIssue	-4.198***	-6.895***
	(0.907)	(1.417)
EquityIssue	-4.886***	-3.378
	(1.455)	(2.272)
BM	-0.272**	-0.467**
	(0.128)	(0.200)
IOR	1.383***	1.446**
	(0.455)	(0.711)
Acquisition	-2.504***	-2.723***
	(0.401)	(0.627)
Constant	0.632	-6.518***
	(1.508)	(2.356)
Observations	16,270	16,270
R-squared	0.943	0.911
Firm FE	Yes	Yes
Year FE	Yes	Yes

Table 12 (cont.)
Impact of the CEO's Equity Compensation Intensity on Labor Cost Manipulation

Panel B: High Equity Intensity Indicator Measure

VARIABLES	(1) *AbnLabor*^{OLS}	(2) *AbnLabor*^{Boost}
EquityIntensity	-0.366**	-0.216
	(0.178)	(0.278)
Firmsize	-0.549***	0.170
	(0.189)	(0.296)
ROA	1.129	0.524
	(0.823)	(1.285)
Leverage	0.797	1.151
	(0.677)	(1.058)
Cash	-1.161	0.485
	(0.876)	(1.368)
Dividend	2.961	1.809
	(3.096)	(4.835)
Capex	-7.562***	-9.313**
	(2.825)	(4.412)
DebtIssue	-4.210***	-6.904***
	(0.907)	(1.417)
EquityIssue	-4.865***	-3.364
	(1.455)	(2.272)
BM	-0.271**	-0.466**
	(0.128)	(0.200)
IOR	1.346***	1.422**
	(0.454)	(0.709)
Acquisition	-2.509***	-2.725***
	(0.401)	(0.627)
Constant	0.507	-6.592***
	(1.510)	(2.358)
Observations	16,270	16,270
R-squared	0.943	0.911
Firm FE	Yes	Yes
Year FE	Yes	Yes

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1
This table tests the association between annual CEO equity compensation intensity and abnormal labor costs as calculated using predicted labor cost values. Panel A uses a continuous measure for equity intensity and Panel B uses an indicator for the top quartile of equity intensity. In each panel, Column (1) reports the results for predicted labor costs derived from the OLS model and Column (2) reports the results for predicted labor costs derived from the Boosting model. All variables are defined in Appendix.

Table 13
Impact of the CEO's Bonus Compensation Intensity on Labor Cost Manipulation

Panel A: Continuous Bonus Intensity Measure

VARIABLES	(1) *AbnLabor*^{OLS}	(2) *AbnLabor*^{Boost}
BonusIntensity	1.569***	-0.486
	(0.481)	(0.752)
Firmsize	-0.548***	0.165
	(0.188)	(0.294)
ROA	0.760	0.728
	(0.832)	(1.299)
Leverage	0.780	1.176
	(0.676)	(1.057)
Cash	-1.190	0.486
	(0.875)	(1.367)
Dividend	3.221	1.613
	(3.096)	(4.837)
Capex	-7.285***	-9.394**
	(2.824)	(4.414)
DebtIssue	-4.162***	-6.954***
	(0.906)	(1.416)
EquityIssue	-5.074***	-3.278
	(1.453)	(2.271)
BM	-0.251*	-0.473**
	(0.128)	(0.200)
IOR	1.354***	1.373*
	(0.454)	(0.709)
Acquisition	-2.513***	-2.719***
	(0.401)	(0.627)
Constant	0.052	-6.469***
	(1.512)	(2.363)
Observations	16,284	16,284
R-squared	0.943	0.911
Firm FE	Yes	Yes
Year FE	Yes	Yes

Table 13 (cont.)
Impact of the CEO's Bonus Compensation Intensity on Labor Cost Manipulation

Panel B: High Bonus Intensity Indicator Measure

VARIABLES	(1) $AbnLabor^{OLS}$	(2) $AbnLabor^{Boost}$
BonusIntensity	0.379**	-0.361
	(0.186)	(0.290)
Firmsize	-0.554***	0.160
	(0.188)	(0.294)
ROA	1.027	0.748
	(0.825)	(1.289)
Leverage	0.784	1.179
	(0.677)	(1.057)
Cash	-1.162	0.501
	(0.875)	(1.367)
Dividend	3.054	1.571
	(3.096)	(4.836)
Capex	-7.464***	-9.480**
	(2.824)	(4.412)
DebtIssue	-4.218***	-6.956***
	(0.906)	(1.416)
EquityIssue	-4.928***	-3.291
	(1.452)	(2.269)
BM	-0.262**	-0.476**
	(0.128)	(0.200)
IOR	1.330***	1.360*
	(0.454)	(0.709)
Acquisition	-2.505***	-2.715***
	(0.401)	(0.627)
Constant	0.375	-6.447***
	(1.507)	(2.355)
Observations	16,284	16,284
R-squared	0.943	0.911
Firm FE	Yes	Yes
Year FE	Yes	Yes

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1
This table tests the association between annual CEO bonus compensation intensity and abnormal labor costs as calculated using predicted labor cost values. Panel A uses a continuous measure for bonus intensity and Panel B uses an indicator for the top quartile of bonus intensity. In each panel, Column (1) reports the results for predicted labor costs derived from the OLS model and Column (2) reports the results for predicted labor costs derived from the Boosting model. All variables are defined in Appendix.

REFERENCES

"How Corporate Layoffs", 2021. How Corporate Layoffs Work – An Insider's Perspective On Why You Will Be Laid Off. Overemployed.com.

Acharya, V., Litov, L.P., Sepe, S.M., 2014. Seeking alpha, taking risk: Evidence from non-executive pay in US bank holding companies. University of Pennsylvania, Wharton School, Weiss Center Working Paper, 13-18.

Anderson, M.C., Banker, R.D., Janakiraman, S.N., 2003. Are selling, general, and administrative costs "sticky"? *Journal of Accounting Research* 41, 47-63.

Anderson, S., 2021. How Corporations Pumped Up CEO Pay While Their Low-Wage Workers Suffered in the Pandemic. Institute for Policy Studies.

Andreicovici, I., Cohen, N., Ferramosca, S., Ghio, A., 2021. Two wrongs make a 'Right'? Exploring the ethical calculus of earnings management before large labor dismissals. *Journal of Business Ethics* 172, 379-405.

Armstrong, C.S., Chau, J., Ittner, C.D., Xiao, J.J., 2020. Earnings per share goals and CEO incentives. Working Paper

Atanasov, V.A., Black, D., Boutchkova, M., 2021. Striking Oil in the Boardroom: Overpaying Executives through Manipulating Performance Metrics. Available at SSRN 3615090.

Bartov, E., Givoly, D., Hayn, C., 2002. The rewards to meeting or beating earnings expectations. *Journal of Accounting and Economics* 33, 173-204.

Bebchuk, L.A., Tallarita, R., 2021. Will Corporations Deliver Value to All Stakeholders? Available at SSRN 3899421.

Bennett, B., Bettis, J.C., Gopalan, R., Milbourn, T., 2017. Compensation goals and firm performance. *Journal of Financial Economics* 124, 307-330.

Blake, M. (2022). Redfin shareholders approve millions in executive payouts same day of mass layoffs. *Real Trends*.

Bloomfield, M.J., 2021. Compensation disclosures and strategic commitment: Evidence from revenue-based pay. *Journal of Financial Economics*.

Bobo, K., 2014. Wage Theft in America: Why Millions of Americans Are Not Getting Paid—And What We Can Do about It: *The New Press*.

Brown, L.D. and Caylor, M.L. 2005. A temporal analysis of quarterly earnings thresholds: Propensities and valuation consequences. *The Accounting Review* 80 (2), 423-440.

Burgstahler, D., Dichev, I., 1997. Earnings management to avoid earnings decreases and losses. *Journal of Accounting and Economics* 24, 99-126.

Bushee, B.J., Miller, G.S., 2012. Investor relations, firm visibility, and investor following. *The Accounting Review* 87, 867-897.

Bushman, R.M., 2021. Cash-based bonus plans as a strategic communication, coordination and commitment mechanism. *Journal of Accounting and Economics*, 101447.

Bushman, R.M., Smith, A.J., 2001. Financial accounting information and corporate governance. *Journal of Accounting and Economics* 32, 237-333.

Business Roundtable, 2019. Business Roundtable “Statement on the Purpose of a Corporation”.

Call, A.C., Chen, S., Tong, Y.H., 2013. Are analysts' cash flow forecasts naïve extensions of their own earnings forecasts? *Contemporary Accounting Research* 30, 438-465.

Caylor, M.L., 2010. Strategic revenue recognition to achieve earnings benchmarks. *Journal of Accounting and Public Policy* 29, 82-95.

Caylor, M.L., Lopez, T.J., Rees, L., 2007. Is the value relevance of earnings conditional on the timing of earnings information? *Journal of Accounting and Public Policy* 26, 62-95.

Chen, H.J., Kacperczyk, M., Ortiz-Molina, H., 2011. Labor unions, operating flexibility, and the cost of equity. *Journal of Financial and Quantitative Analysis* 46, 25-58.

Chun, H.M., Shin, S.Y., 2017. Labor union and real earnings management. *Global Business & Finance Review* (GBFR) 22, 30-49.

Chyz, J.A., Leung, W.S.C., Li, O.Z., Rui, O.M., 2013. Labor unions and tax aggressiveness. *Journal of Financial Economics* 108, 675-698.

Cohen, D.A., Mashruwala, R., Zach, T., 2010. The use of advertising activities to meet earnings benchmarks: Evidence from monthly data. *Review of Accounting Studies* 15, 808-832.

Cohen, D.A., Zarowin, P., 2010. Accrual-based and real earnings management activities around seasoned equity offerings. *Journal of Accounting and Economics* 50, 2-19.

Core, J.E., Guay, W.R., Verrecchia, R.E., 2003. Price versus non-price performance measures in optimal CEO compensation contracts. *The Accounting Review* 78, 957-981.

Dechow, P.M., Kothari, S.P., Watts, R.L., 1998. The relation between earnings and cash flows. *Journal of Accounting and Economics* 25, 133-168.

Dierynck, B., Landsman, W.R., Renders, A., 2012. Do managerial incentives drive cost behavior? Evidence about the role of the zero earnings benchmark for labor cost behavior in private Belgian firms. *The Accounting Review* 87, 1219-1246.

Dou, Y., Khan, M., Zou, Y., 2016. Labor unemployment insurance and earnings management. *Journal of Accounting and Economics* 61, 166-184.

Eldenburg, L.G., Gunny, K.A., Hee, K.W., Soderstrom, N., 2011. Earnings management using real activities: Evidence from nonprofit hospitals. *The Accounting Review* 86, 1605-1630.

Faleye, O., Reis, E., Venkateswaran, A., 2013. The determinants and effects of CEO–employee pay ratios. *Journal of Banking & Finance* 37, 3258-3272.

Feltham, G.A., Xie, J., 1994. Performance measure congruity and diversity in multi-task principal/agent relations. *The Accounting Review*, 429-453.

Givoly, D., Hayn, C., & Lehavy, R. (2009). The quality of analysts' cash flow forecasts. *The Accounting Review, 84*(6), 1877-1911.

Graham, J.R., Harvey, C.R., Rajgopal, S., 2005. The economic implications of corporate financial reporting. *Journal of Accounting and Economics* 40, 3-73.

Graham, J.R., Harvey, C.R., Rajgopal, S., 2006. Value destruction and financial reporting decisions. *Financial Analysts Journal* 62, 27-39.

Guay, W.R., Kepler, J.D., Tsui, D., 2019. The role of executive cash bonuses in providing individual and team incentives. *Journal of Financial Economics* 133, 441-471.

Gunny, K.A., 2010. The relation between earnings management using real activities manipulation and future performance: Evidence from meeting earnings benchmarks. *Contemporary Accounting Research* 27, 855-888.

Healy, P.M., 1985. The effect of bonus schemes on accounting decisions. *Journal of Accounting and Economics* 7, 85-107.

Hölmstrom, B., 1979. Moral hazard and observability. *The Bell Journal of Economics*, 74-91.

Huang, K., Lu, C.-H., 2019. Do Incentivized Managers Pay Their Workers Less? Working Paper

Huang, S., Roychowdhury, S., Sletten, E., 2020. Does litigation deter or encourage real earnings management? *The Accounting Review* 95, 251-278.

Ittner, C.D., Larcker, D.F., Rajan, M.V., 1997. The choice of performance measures in annual bonus contracts. *The Accounting Review*, 231-255.

Jensen, M.C., Murphy, K.J., 1990. Performance pay and top-management incentives. *Journal of Political Economy* 98, 225-264.

Ji, Y., Rozenbaum, O., Welch, K., 2022. Who Experiences Financial Reporting Pressure? Available at SSRN 4094290.

Kama, I., Weiss, D., 2013. Do earnings targets and managerial incentives affect sticky costs? *Journal of Accounting Research* 51, 201-224.

Kilgore, T. (2022). Disney CEO Bob Chapek's 2021 compensation more than doubled to over $32 million, while median employee pay fell. *MarketWatch*.

Keung, E., Lin, Z.X., and Shih, M. 2010. Does the stock market see a zero or small positive earnings surprise as a red flag? *Journal of Accounting Research* 48 (1), 91-121.

Kong, D., Qin, N., Yang, W., Zhang, J., 2021. Employee cash profit-sharing and earnings management. *European Accounting Review*, 1-25.

Kothari, S.P., Mizik, N., Roychowdhury, S., 2016. Managing for the moment: The role of earnings management via real activities versus accruals in SEO valuation. *The Accounting Review* 91, 559-586.

Lambert, R.A., Larcker, D.F., 1987. An analysis of the use of accounting and market measures of performance in executive compensation contracts. *Journal of Accounting research*, 85-125.

Liu, J., Nissim, D., & Thomas, J. (2007). Is cash flow king in valuations? *Financial Analysts Journal, 63*(2), 56-68.

Liberty, S.E., Zimmerman, J.L., 1986. Labor union contract negotiations and accounting choices. *The Accounting Review*, 692-712.

Lopez, T.J., Rees, L., 2002. The effect of beating and missing analysts' forecasts on the information content of unexpected earnings. *Journal of Accounting, Auditing & Finance* 17, 155-184.

Magnan, M., & Martin, D. (2019). Executive compensation and employee remuneration: The flexible principles of justice in pay. *Journal of Business Ethics, 160*(1), 89-105.

Martin, X., Seo, H., Yang, J., Kim, D.S., Martel, J., 2022. Target performance goals in CEO compensation contracts and management earnings guidance. Kelley School of Business Research Paper.

Matsunaga, S.R., Park, C.W., 2001. The effect of missing a quarterly earnings benchmark on the CEO's annual bonus. *The Accounting Review* 76, 313-332.

McInnis, J., Collins, D.W., 2011. The effect of cash flow forecasts on accrual quality and benchmark beating. *Journal of Accounting and Economics* 51, 219-239.

Owens, E.L., Wu, J.S., Zimmerman, J., 2017. Idiosyncratic shocks to firm underlying economics and abnormal accruals. *The Accounting Review* 92, 183-219.

Raghunandan, A., 2021. Financial misconduct and employee mistreatment: Evidence from wage theft. *Review of Accounting Studies* 26, 867-905.

Raghunandan, A., Rajgopal, S., 2021. Do Socially Responsible Firms Walk the Talk? Available at SSRN 3609056.

Rouen, E., 2020. Rethinking measurement of pay disparity and its relation to firm performance. *The Accounting Review* 95, 343-378.

Roychowdhury, S., 2006. Earnings management through real activities manipulation. *Journal of Accounting and Economics* 42, 335-370.

Shields, M., 2019. Wage Theft: A quiet crisis in Ohio. Policy Matters Ohio, April 10.

Stambor, Z., 2022. Layoffs are coming as companies focus on profitability. *Insider Intelligence*.

Vanian, J., 2022. Snap will lay off 20% of staff, report says. *CNBC*.

Vorst, P., 2016. Real earnings management and long-term operating performance: The role of reversals in discretionary investment cuts. *The Accounting Review* 91, 1219-1256.

Zhang, W., 2008. Real activities manipulation to meet analysts' cash flow forecasts. Available at SSRN 1013228.

Zingales, L., 2000. In search of new foundations. *The Journal of Finance* 55, 1623-1653.

APPENDIX: VARIABLE DEFINITIONS

Variable	**Definition**
NonExecPay	Total Staffing Expense minus the total compensation of the top five executives (Source: Compustat and Execucomp)
logNonExecPay	Log(1+*NonExecPay*)
$Non\widehat{ExecPay}^{OLS}$	OLS Predicted value of *NonExecPay*
$logNon\widehat{ExecPay}^{OLS}$	OLS Predicted value of *logNonExecPay*
$Non\widehat{ExecPay}^{Boost}$	Machine Learning (Gradient Boosting) Predicted value of *NonExecPay*
$logNon\widehat{ExecPay}^{Boo}$	Machine Learning (Gradient Boosting) Predicted value of *logNonExecPay*
ADV	Log(1+XAD) where XAD is set to zero when missing (Source: Compustat)
COGS	Log(1+COGS) where COGS is set to zero when missing (Source: Compustat)
INV	Log(1+INVT) where INVT is set to zero when missing (Source: Compustat)
Assets	Total assets (Source: Compustat)
FirmSize	Logarithm of total assets. log(1+AT_t). (Source: Compustat)
ROA	Annual income before extraordinary items divided by lagged total assets. Ib_t/At_{t-1}. (Source: Compustat)
adjROA	Firm ROA less the mean ROA for the 2-digit SIC industry classification. (Source: Compustat)
TobinsQ	Market value of assets over the book value of assets. (((PRCC_F*CSHO)+AT-CEQ)/AT) . (Source: Compustat)
LaborProd	Sales divided by the number of employees. (SALE/EMP). (Source: Compustat)
Leverage	Short-term debt plus long-term debt, divided by total assets. (DLC+DLTT)/At. (Source: Compustat)
Cash	Cash over total assets. (CHE/AT). (Source: Compustat)
Dividend	Dividend divided by total assets. ((DVC+DVP)/AT). (Source: Compustat)
Capex	Capital expenditures divided by total assets. (CAPX/AT). (Source: Compustat)
DebtIssue	Long-term debt issuance minus long-term debt reduction, divided by total assets. ((DLTIS-DLTR)/AT). (Source: Compustat)
EquityIssue	Sale of common or preferred stock divided by total assets. (SSTK/AT). (Source: Compustat)
BM	Book value of equity divided by the market value of equity. (CEQ/(PRCC_F*CSHO)). (Source: Compustat)

Acquisitions	1 if AQS>=0.2*SALE and SALE>0, 0 otherwise. (Source: Compustat)
Unionization	the percentage of employees in a firm's 4-digit SIC code. SIC unionization is derived from a CIC to SIC crosswalk (Source UnionStats.com)
InstOwn	Percent of shares outstanding held by institutional investors.
BonusEPS_Surprise	Actual bonus EPS – Target bonus EPS (Source: Incentive Lab)
BonusEPS_MB	1 if *BonusEPS_Surprise* in [0,0.02], 0 otherwise (Source: Incentive Lab)
AnalystEPS_Surprise	Actual analyst forecast EPS – Target analyst forecast EPS (Source: I/B/E/S)
AnalystEPS_MB	1 if *AnalystEPS_Surprise* in [0,0.02], 0 otherwise (Source: I/B/E/S)
EquityIntensity	(rstkgrnt+option_awards_blk_value)/tdc1 or (stock_awards_fv+option_awards_fv)/tdc1 (Source: Execucomp)
HighEquityIntensity	1 if *EquityIntensity* is in the top quartile during the year (Source: Execucomp)
BonusIntensity	(bonus+ltip)/tdc1 or (bonus+noneq_incent)/tdc1 (Source: Execucomp)
HighBonusIntensity	1 if *BonusIntensity* is in the top quartile during the year (Source: Execucomp)
AbnLabor	I follow Kothari et al. (2016) and estimate abnormal labor costs as residuals from the following first-order autoregressive model incorporating fixed effects: $$L_{i,t}^{proxy} = \alpha_0 + \alpha_1 \times L_{i,t}^{proxy} + \alpha_2 \times \frac{1}{Assets_{i,t-1}} + \alpha_1 \times Sales_{i,t} + \varepsilon_{i,t}$$ Where, $L_{i,t}^{proxy}$ is the labor costs for each of the three proxies (*NonExecPay,* $\widehat{NonExecPay}^{OLS}, \widehat{NonExecPay}^{Boost}$) scaled by the beginning of the year total assets; $L_{i,t-1}^{proxy}$ is its one year lagged value of the labor cost proxy; $Assets_{i,t-1}$ is beginning of the year total assets; and $Sales_{i,t}$ is sales during the year scaled by beginning of the year total assets. Note: I adapt the inputs of the model for estimating abnormal labor costs to control for firm- and year-specific as described in Kothari et al. (2016): ‘‘First, every firm’s [labor] cost is differenced from the cross-sectional mean for that year. Second, for every firm, the annual deviation of [labor] cost from the cross-sectional mean is differenced from the corresponding deviation in the previous year. The explanatory variables in the model are also differenced twice in the same manner. The model is estimated every year.’’ The firm-year residual minus the mean value of the residuals across all years for the corresponding firm, multiplied by -1 yields abnormal labor costs such that positive values indicate greater cuts to labor costs. I also multiply the values by 100 to increase the interpretation of coefficients. (Source: Compustat and Execucomp)