Two Essays on Executive Compensation

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ABSTRACT
(Academic)

This dissertation consists of two essays, both co-authored with Ugur Lel. The first essay (Chapter 1) examines whether high CEO pay inequality (CPI), the share of total managerial pay captured by the CEO, is an outcome of poor corporate governance, and its implications for shareholder wealth. We exploit the 2002 NYSE and NASDAQ governance reforms that mandated firms to have majority independent boards as a quasi-exogenous source of variation in the internal governance environment of firms. Results show that CPI decreases following the passage of these exchange listing regulations, but only in firms with entrenched CEOs affected by the exchange listing regulations. Firm value also increases for these firms. These results are robust to a variety of robustness checks such as a matched sample analysis and placebo tests. Overall, our results suggest that poor governance environments are associated with high managerial pay differences and consequently lower firm valuations, supporting the view that high CEO pay inequality reflects managerial entrenchment.

The second essay (Chapter 2) examines whether shareholders use executive compensation channel to align managerial horizon with their investment horizon. We utilize a newly emerged empirical measure, pay duration, to measure managerial horizon. For shareholder horizon, we use the fraction of long-term institutional ownership in the firm. Results show that there is a positive association between long-term institutional ownership and CEO pay duration, suggesting that shareholder horizon is a determining factor in compensation contracts. We address reverse causality using indexer institutions. We also establish a causal link from investor horizon to CEO pay duration using institution mergers as a source of exogenous variation in investor horizon of the firm. We extend our results to hedge fund activism and document a negative relation between hedge fund activism and pay duration, which is consistent with our argument. Overall our results suggest that shareholders structure CEO pay in a way that is consistent with their investment horizon.

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ABSTRACT
(General Audience)

CEOs play a crucial role in today's financial world. They are the ultimate decision makers in companies and their goal is to maximize the shareholder wealth. Motivating the CEO to work hard and maximize shareholder wealth hinges on optimally designed compensation contracts. Shareholders delegate company directors to design these pay contracts. However, conflicts of interest between directors and CEOs, between shareholders and CEOs, and even among shareholders, affect the design of CEO pay contracts. It is important to study these conflicts of interest and their effect on CEO compensation to ensure well-functioning companies and a fair market.

The objective of the first chapter is to examine whether the CEOs are overpaid when the company directors are not able to monitor the actions and decisions of the CEOs. We document that powerful and established CEOs are overpaid, both in dollar terms and relative to other managers in the company, when they are not properly monitored. We also document that regulations that aim to improve monitoring quality in companies bring CEO pay to fair levels, leading to an increase in company valuations. These findings point out the importance of regulations that improve the governance of companies.

In the second chapter, we examine short-termism (or myopia) in the context of CEO pay. Basically, short-termism is any action that saves today but is costly in future. While short-term shareholders invest in companies for short periods to take advantage of temporary changes in company valuation, long-term investors invest for long periods and aim to benefit from long-term increase in company valuation. We document that the conflict of interest among shareholders with different investment periods is reflected in the design of CEO pay contracts. In particular, CEOs wait more to receive their compensation if the dominant investor type in the company has longer investment period. This finding explains how shareholders use CEO compensation to achieve wealth maximization, highlighting the power and importance of CEO pay contracts.

To my parents, Asuman and Hasan…

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Chapter 1

Does CEO Pay Inequality Reflect Poor Corporate Governance?

1.1 Introduction

There is a significant pay disparity within the corporate top management ranks. Most CEOs earn much more than not only the average employee but other senior executives of the same firm. For example, among the S&P 1,500 firms, the average CEO captures 38% of the aggregate compensation of the top-five executive team. Further, there is substantial variation in such within-firm differences in total executive pay across firms. Given that executive pay policies are set by the board of directors to optimally coordinate the level of effort exerted by not only the CEO but across the top management team, it is important to analyze pay policies of executives as a group.

Why do such pay inequalities exist within the top management teams, and do they harm shareholder wealth? One explanation is that they are the outcome of tournament incentives set by the board of directors to induce greater effort by non-CEO executives (e.g., Lazear and Rosen (1981), Rosen (1986)). Supporting this view, several papers show that CEO pay inequality is associated with higher firm valuations and risk-taking incentives (e.g., Eriksson (1999) and Kale et al. (2009)). Another explanation is that the dominant position of the CEO allows him to extract rents in the form of higher pay relative to other managers at the cost of shareholder wealth (e.g., Bebchuk and Fried (2003)). Several studies provide evidence consistent with this view (e.g., Bebchuk et al. (2011), Chen, Huang, and Wei (2013)).[1] Thus, the empirical evidence on whether executive pay inequality has a positive or adverse effect on shareholder wealth is so far mixed. This is in part because these studies rely on sources of variation in CEO pay inequality that is potentially subject to endogeneity and omitted variable biases. For example, both CEO pay inequality and the structure of the board of directors is likely endogenously related to the governance environment of firms, which can in turn influence firm valuations.

To establish whether managerial pay differentials are an outcome of poor corporate governance and how they influence shareholder wealth, we exploit a quasi-natural experiment that

[1] CEO pay inequality can also reflect relative value creation of the CEO and other managers, talent, and productivity. In our tests, we control for observable and unobservable firm characteristics and various CEO and management team characteristics that proxy for such aspects of CEO pay differentials.

improved the internal governance environment of some, but not all firms. In particular, we use the 2002 NYSE and NASDAQ governance reforms that mandated the boards of listed firms to be at least majority independent and the compensation committees to be fully independent. These exchange listing regulations are particularly interesting in the context of CEO pay inequality, as setting managerial compensation and appropriate incentives for top managers is one of the most important functions of the board of directors. We conjecture that a greater board independence can empower boards of directors in their negotiations with CEOs, potentially decreasing the degree of board capture (e.g., Bebchuk, Friedman, and Friedman (2007)). In addition, boards may be better positioned to set pay policies for top management teams optimally and in line with firm performance when board independence increases. Since some firms were already in compliance with the new stricter governance standards, our setup allows for a difference-in-difference analysis where non-compliant firms constitute our treatment group and the rest of the sample forms the control group. For further identification, we make a distinction within treatment firms based on the degree of managerial and board entrenchment in the pre-regulation period.

How does the CEO pay inequality (CPI) change following the exchange listing regulations? On average, we find no statistically significant change in CPI around the enactment of exchange listing regulations. However, it decreases significantly following the passage of the exchange listing regulations for non-compliant firms with a high degree of managerial entrenchment, the firms that are most likely to be affected by the exchange listing regulations.[2] In terms of economic magnitude, for non-compliant firms with high managerial entrenchment in the pre-regulation period, the CPI level decreases 3 percentage points more in the post-treatment period relative to pre-treatment period and relative to compliant firms. This finding suggests that the strength of governance plays an important role in preventing CEO rent extraction in the form of higher relative compensation.

Do managerial pay differences influence shareholder wealth? In the second part of our analysis, we use the change in pay differences due to exchange listing regulations as a quasi-exogenous shock to identify the effects of CEO pay inequality on firm valuation. Our results show

[2] In our main tests, we focus on board independence rather than compensation committee independence to identify the treatment group as there is more variation in the former measure. Firms are required to have fully independent compensation committees since 1994 in order to be exempt from the $1 million deductibility cap, which many large firms adopted (Murphy (2013)). As we show in later tests, our main results are robust to using compensation committee independence to identify the treatment group.

that the decline in the managerial pay gap around the adoption of 2002 NYSE and NASDAQ governance reforms is associated with an increase in firm value. Again, we find this result only for the subsample of firms that are not in compliance with the new exchange listing regulations and likely suffer the most from high managerial agency conflicts. The existence of alternative monitoring mechanisms in the pre-regulation period lowers the potential improvement in firm value due to changes in CPI around the exchange listing regulations. Overall, our results suggest that higher CEO pay inequality is associated with weak governance environments and lower firm valuations, consistent with the managerial entrenchment argument in Bebchuk et al. (2011).

We subject these results to a variety of additional checks to ensure they are robust. For example, we undertake a nearest neighbor matching strategy where we match the firms subject to the exchange listing regulations due to incompliance with other firms in terms of lagged values of *Log (CEO Pay)*, *CPI*, *Size* and *Industry-Adjusted ROA* at the 2-digit SIC code level and as of the year prior to the adoption of the exchange listing regulations. Such an alternative control sample mitigates the concern that our results are driven by time-varying differences in the pre-regulation period between the firms in compliance and not in compliance with the exchange listing regulations. In addition, we undertake placebo tests where the exchange listing regulations are assumed to be enacted in different years. Further, we use the requirement of fully independent compensation committees to define the treatment group of firms. In all these instances, we find that our results remain robust.

Our study contributes to the literature on executive compensation in two important ways. First, we exploit the adoption of 2002 NYSE and NASDAQ governance reforms as a natural experiment to identify the effect of corporate governance on managerial pay disparity and its firm valuation consequences. Given the mixed findings in the literature, our empirical setup provides a relatively less endogenous estimation of the impact of executive pay disparity on firm value. The most similar study to ours is the cross-country examination of international say on pay laws by Correa and Lel (2015). Our approach differs in the sense that we use a major regulatory change in the United States, and provide an in-depth comparison of the relation between CEO pay gap and governance around a major regulatory change between firms with strong and weak governance environments.

Second, we add to the literature on the relation between executive compensation and firms' governance environment, and in general, changes in managerial pay policies around regulatory changes. Several studies find that executive pay levels and firm valuation change surrounding the passage of 2002 NYSE and NASDAQ governance reforms. For example, Chhaochharia and Grinstein (2009) (henceforth CG) find CEO compensation decreases for non-compliant firms, and Aggarwal, Schloetzer, and Williamson (2014) document improvements in firm valuation following these exchange listing regulations. We provide the first empirical evidence on the managerial pay inequality and related firm valuation effects of 2002 NYSE and NASDAQ governance reforms. The decline in CEO pay disparities is potentially another channel through which firm value increases following the exchange listing regulations. We also extend the literature on the effects of exchange listing regulations on executive pay policies where we employ the pay on senior managers as a control sample in analyzing CEO pay around significant governance reforms, and are thus less subject to any potential endogeneity concerns. These tests are akin to triple difference estimates, as the firm effects on executive pay levels are perfectly controlled for. Our results are consistent with the evidence that executive pay policies are related to the governance environment of firms (e.g., Shivdasani and Yermack (1999), Core, Holthausen, and Larcker (1999), Bebchuk and Fried (2003)).

Although our study resembles CG's methodology and findings, our contribution goes beyond CG in three aspects. First, while legislations are used as quasi-natural experiments in difference-in-difference analyses, they do not guarantee a true random assignment of firms to treatment and control groups. There may still be firm-level time-varying omitted factors that are correlated with the probability of being treated and executive pay policies. Using CPI instead of CEO pay allows us to reduce this type of endogeneity as we use the executive pay in the same firm as a control by design. Further, CPI approach partially eliminates the possible bias of model dependence on valuation of stock options (Hodder, Mayew, McAnally, and Weaver (2006)).[3]

Second, the findings in CG are contested by Guthrie, Sokolowsky, and Wan (2012) who replicate CG and document that the reduction in CEO pay is attributable to 2 outliers. Excluding these outliers leads to statistically insignificant effects. Therefore, the empirical results are at best mixed on the relation between exchange listing regulations and CEO pay levels. Our study helps reconcile these mixed findings. Consistent with Guthrie et al. (2012), our results show that CPI

[3] Hodder et al. (2006) discuss that managerial discretion on model inputs can improve or worsen the predictive accuracy of option values. Although Execucomp standardizes the option valuation across firm and year, the approach may still underestimate or overestimate the true value of the stock options for some firms. Benchmarking the CEO pay to other executives reduces this bias.

and CEO pay level, on average, does not decrease for non-compliant firms following the governance regulations. However, consistent with CG's monitoring argument, we document a decline in both CPI and CEO pay for the non-compliant firms with entrenched CEOs, whose firms are likely the most affected ones by the regulations. Third, CG do not examine directly if lower CEO pay is good or bad for shareholders. The decline in CEO pay distorts the tournament incentives, which can result in lower managerial effort and firm valuation. We take a further step and document that the reduction in CPI and CEO pay is associated with higher firm valuations.

The rest of the paper is organized as follows. Section 1.2 provides background information on 2002 NYSE and NASDAQ governance reforms and the related literature. In the next section, we discuss sample construction and univariate statistics along with an outline of the main empirical specification. In Section 1.4, we present results on changes in CPI and firm valuation following the passage of reforms. We provide robustness checks in Section 1.5 and conclude in Section 1.6.

1.2 2002 NYSE and NASDAQ Governance Reforms

The corporate scandals in 2001 and 2002, involving firms such as Enron, Tyco International, and Worldcom, led to the enactment of Sarbanes-Oxley (SOX) Act. In general, this act aims to increase board oversight and reduce corporate misconduct. It was introduced in the House on February 14, 2002 and signed into law by the president on July 30, 2002.[4] In parallel with the act, on February 13, 2002, the Securities and Exchange Commission (SEC) called on stock exchanges to tighten governance requirements for listed firms. NYSE and NASDAQ boards made proposals in August 2002 and October 2002, respectively. SEC approved new exchange listing requirements in November 2003. The main requirements include boards with majority independent directors and fully independent compensation and nominating committees with the purpose of increasing the monitoring effectiveness of boards of directors.[5]

Several studies provide evidence that the governance environment and valuations of firms improve following the 2002 NYSE and NASDAQ governance reforms. For example, CG show that firms that are less compliant with new exchange listing regulations earn positive abnormal

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[4] The act passed the House on April 24, 2002 and passed the senate on July 15, 2002.

[5] Sarbanes-Oxley Act itself requires full independence of audit committees. Moreover, new exchange listing regulations also include the following main provisions: (i) Compensation and nominating committee must have written charters and self-evaluation procedures, (ii) audit committee must be financially literate and at least one member must have accounting related expertise, (iii) non-executive directors must meet regularly without the executives, (iv) independent director definition becomes tighter.

returns. Similarly, Aggarwal, Schloetzer, and Williamson (2014) find that firm value improves after the regulation for the less compliant firms due to changes in CEO compensation and CEO retention policies. Akhigbe and Martin (2006) relate the firm value improvement to an increase in transparency. Apart from valuation effects, new listing requirements are also linked to enhanced governance environments. For example, CG show that total CEO compensation decreases for non-compliant firms. Our paper adds to this strand of literature by showing that (i) the CEO pay inequality decreases following the 2002 NYSE and NASDAQ governance reforms for the non-compliant firms with high managerial entrenchment, and (ii) this decrease is associated with higher firm valuations for such firms. These results suggest that the governance reforms have effects on the pay policies of not only the CEO but all top managers, and the decline in CPI is another channel through which firm value increases following the exchange listing regulations.

1.3 Data, Variables, and Methodology

1.3.1 Sample Construction

Our data on board structure comes from Riskmetrics and executive compensation comes from Execucomp. We retrieve information on firms' financial characteristics from Compustat and stock returns from CRSP. We use Thomson Reuters to obtain data on institutional ownership.

Since the main feature of our analysis is to use 2002 NYSE and NASDAQ governance reforms as a positive shock to the strength of internal governance environments, we construct our sample around these governance reforms. Our sample period extends from 1998 to 2007, which corresponds to 5 years before and after the passage of the exchange listing regulations. The period of 2003 through 2007 constitutes the post-period years, which is labeled as *Post*. Ending the sample in 2007 ensures that our results are not influenced by the recent financial crisis. Following CG, we choose 2003 as the first year of 2002 NYSE and NASDAQ governance reforms are in effect. Therefore, we classify treatment firms based on their compliance in year 2002. Our main sample criterion requires that the firm is covered by Riskmetrics as of the year prior to the passage of the regulation with non-missing director independence data and exchange listing data from CRSP.[6] This leaves us with 1,319 distinct firms and 12,380 firm-year observations for the whole sample.

[6] Our results are robust to restricting the sample to firms that are listed in NYSE or NASDAQ for the entire sample period.

Based on the data availability of our regression variables and empirical setup, our main regression sample consists of 10,768 firm-year observations with 1,297 distinct firms.

We classify firms based on whether they were compliant or not with the new exchange listing regulations and whether the firm was listed in NYSE or NASDAQ as of 2002. A firm is non-compliant with the new exchange listing regulations if it does not have a majority independent board as of 2002 and is listed on NYSE or NASDAQ. *Treatment* dummy equals one for these firms, and 0 otherwise. In our main regression sample, 281 out of 1,297 (22%) firms belong to the treatment group according to the board independence requirement. This figure is consistent with previous studies (e.g., CG (2009)). In our main tests we use the board independence requirement to identify firms that are affected by the new exchange listing regulations, that is, the treatment sample. In later tests we also show that our results are mostly robust to identifying the treatment sample based on compensation committee independence. In this case, a firm is non-compliant with the new exchange listing regulations if it does not have a fully independent compensation committee as of 2002 and is listed on NYSE or NASDAQ.

We define CEO pay inequality (*CPI*) as the fraction of aggregate compensation of the top 5 executives captured by the CEO as in Bebchuk, Cremers, and Peyer (2011). Total compensation is obtained from Execucomp and is measured in real terms as of year 2002. While we require at least 5 executives with non-missing data in Execucomp for the construction of *CPI*, we adjust this variable in two ways. First, we set *CPI* to missing if there is a CEO turnover in the current fiscal year to prevent a mechanical downward bias in our *CPI* measures due to partial annual compensation. Second, if total CEO compensation is missing or CEO is not identified, we set *CPI* to missing. Table 1.1 provides summary statistics for *CPI* along with all other variables used throughout the paper. It displays an average CPI of 0.379 and a median CPI of 0.382 for the sample firms. These results are in line with the sample in Bebchuk et al. (2011), where the mean CPI is 0.357. We winsorize all continuous variables at the 1% level except for *Industry Median CPI* and use one year lagged control variables. Table 1.1 shows the summary statistics of our sample. We also use the components of CPI in our analysis. We create variables *Log (CEO Pay)* and *Log (Non-CEO Pay)* using total compensation of the CEO and total compensation of top 5 paid executives excluding the CEO, respectively. We adjust them to 2002 dollars by using Consumer Price Index (CPI).

1.3.2 Methodology

We follow the standard methodology in the literature to examine changes in CPI and firm valuation around the passage of 2002 NYSE and NASDAQ governance reforms (e.g. CG (2009) and Aggarwal, Schloetzer, and Williamson (2014)). Specifically, we estimate the following panel data regression with firm and year fixed effects between 1998 and 2007.

$$CPI_{it} = \beta_0 + \beta_1 * Treatment * Post + \beta_2 * Post + Controls_{it} + Firm\ FE_i + Year\ FE_t + \varepsilon_{it} \quad (1)$$

where the dependent variable *CPI* is defined as the fraction of aggregate compensation of the top-five executive team captured by the CEO, *Treatment* refers to firms that are not compliant with the exchange listing regulations and *Post* represents the post-regulation period of 2003 through 2007. We include firm fixed effects to control for unobserved time-invariant firm-related effects and year indicator variables to account for any aggregate time effects. The firm and year fixed effects make *Treatment***Post* similar to the difference-in-difference estimator. We cluster standard errors at firm-period (pre versus post) level in order to account for possible error correlation within firm and pre and post regulation periods.

We use additional variables in Equation (1) to control for tournament and labor market incentives that are shown to influence managerial pay inequality (e.g., Kale, Reis, and Venkateswaran (2009)). CPI levels can be not only an indication of agency problems related to pay practice but an outcome of optimal selection of CEO compensation. First, to control for labor market for CEO talent, we use *Industry Median CPI*, which is constructed at the 2-digit SIC industry level in the Execucomp universe to control for the pool of CEO candidates and CEO employment opportunities in a given industry and year. In other words, we control for labor supply and demand for CEO talent and tournament incentives. Tournament incentives can have both positive and negative effects. Higher CEO compensation incentivizes non-CEO executives to work harder and perform better, but it also hurts the firm by lowering the degree of cooperation among executives. We proxy tournament incentives by number of vice presidents (*Number of VPs*) in the executive team. There are more tournament incentives when there is a greater number of executives with equal job title. Moreover, a higher number of VPs implies that the CEO is differentiated among the executive team, again implying higher tournament incentives. Lastly, we consider whether the CEO is the only director among the executive team. This is similar to differentiating the CEO in the executive team, and also, CEO is given more responsibility compared to other executives and has a compensation package accordingly. We use a dummy variable (*CEO is Only Director*) which is equal to 1 if the CEO is the only director in the top 5 executive team and 0, otherwise.

We also include several variables that are shown by prior studies to influence the pay differentials among top managers. These variables are firm size measured by net sales (*Sales*), firm riskiness measured by the ratio of total long term debt to total assets (*Leverage)* and the annualized standard deviation of previous year's stock returns (*Stock Return Volatility),* investment policies measured by the ratio of R&D expense to net sales (*R&D),* and the ratio of capital expenditures to total assets (*Capex*), profitability defined as the difference between ROA of the firm and its corresponding 2-digit SIC industry median value for a given year (*Industry-Adjusted ROA),* and whether the CEO has high share ownership in the firm by including a dummy variable set to 1 if the CEO has at least 10 % ownership of the firm (*CEO is a Blockholder*).[7]

It is important to note that we also estimate Equation (1) conditional on CEO power as part of our main analysis. If exchange listing regulations improve the governance environment of firms, we should observe lower CPI levels when CEO power is higher around the adoption of these exchange listing regulations. This specification is as follows.

$$CPI_{it} = \beta_0 + \beta_1 * Treatment*Post*CEO\ Power + \beta_2 * Treatment*Post + \beta_3 * Treatment*CEO\ Power + \beta_4 * Post*CEO\ Power + \beta_5 * Post + \beta_6 * CEO\ Power + Controls_{it} + Firm\ FE_i + Year\ FE_t + \varepsilon_{it} \quad (2)$$

We use 5 variables to proxy for CEO power. The first one is whether the CEO is the chairman of the board of directors (*CEO is Chair*). As in Adams, Almeida, and Ferreira (2005), we assume that the CEO is more powerful if he also serves as the chairman. The second proxy is *CEO Tenure* defined as the natural logarithm of years since becoming the CEO. Hermalin and Weisbach (1988) argue that a new CEO is monitored intensely and as she becomes established, less scrutiny is required. Thus, a longer tenure is associated with more CEO power. Our third proxy is the fraction of directors that are appointed to the board after the current CEO. The CEO tries to influence director appointments in such a way that the directors who are unlikely to oppose CEO's decisions are chosen as board members (e.g. Coles, Daniel, and Naveen (2014) and Pan, Wang, and Weisbach (2015)). Thus, a higher fraction of co-opted directors (*Co-opted Directors*) is

[7] Missing values of R&D expenditures are set to 0 and are assigned to a dummy variable (missing R&D) in the regression analysis.

associated with more CEO power. Our last proxy is industry competition. Product market competition can play a monitoring role through its disciplinary effects on executives, and thereby can reduce the power of CEOs (e.g., Giroud and Mueller (2011)). We measure industry competition by the Herfindahl-Hirschman Index (*HHI*) of 2-digit SIC industries in Compustat universe based on net sales. A higher *HHI* value implies lower competition and higher CEO entrenchment. Finally, we create an aggregate measure of CEO power by aggregating the 4 CEO power variables by scaling each CEO power (except *CEO is Chair*) and then adding them up.[8] The resulting variable ranges between 0 and 4. We call this variable as the *Aggregate CEO Power*, which summarizes the effects of different CEO power dimensions in one variable. We also utilize this variable to construct our proxy for the magnitude of managerial agency costs.[9]

1.4 Results

1.4.1 Exchange Listing Regulations, CPI, and CEO Power

Our main hypothesis is that CEO pay inequality is related to problematic corporate governance environments. To test this hypothesis, we estimate our regression specification in Equation (1) to examine the effect of exchange listing regulations on CEO pay inequality. We also examine how this effect varies with the magnitude of CEO power at the cross-section as in Equation (2). Results from these regressions are reported in Table 1.2.

Column (1) reports the average effect of exchange listing regulations on CPI levels. The coefficient on *Treatment*Post* is not statistically significant, suggesting that the exchange listing regulations do not affect CPI levels on average. In the next columns, we examine changes in CPI levels conditional on CEO power. The key variable that denotes the triple difference term is *Treatment*Post*CEO Power*. We use 4 widely used measures of CEO power and an aggregate measure of these 4 variables in this analysis. In Column (2), we report the impact of whether the CEO is also the chairman on changes in CPI levels around the 2002 NYSE and NASDAQ governance reforms. This column shows that the coefficient on *Treatment*Post*CEO Power* is -

[8] The scaling is done as follows (x-min(x))/((max(x)-min(x)) where x is the observed CEO power variable value, min(x) is the sample minimum and max(x) is the sample maximum of the CEO power variable.

[9] We use the current value of CEO power variables in our regression specification. We face a trade-off between using current or ex-ante (pre-treatment) values of CEO power variables. Using current values of CEO power may pose an econometric problem if CEO power variables are affected by the treatment. However, CPI and CEO power variables are not firm level variables in nature (e.g., Tenure, Co-opted Directors, CEO is Chair). Therefore, using ex-ante values are not internally consistent and these variables may change with a CEO turnover. We repeat our regression specification in Equation (2) using ex-ante values of CEO power variables and we obtain similar results.

0.030 (t = -2.58). Economically speaking, for the firms where CEO is also the chairman compared to firms where CEO is not the chairman, the CPI level decreases 3 percentage points more in the post-treatment period relative to pre-treatment period for non-compliant firms relative to compliant firms. This finding suggests that CPI levels decrease more in firms where CEO is also the chairman. This is consistent with the argument that exchange listing requirements allow boards to become more effective in firms with powerful CEOs, and better monitoring leads to a decrease in CPI. In the next column, we report changes in CPI around the adoption of exchange listing regulations based on the length of *CEO Tenure.* It shows a negative and statistically significant coefficient on the triple interaction term (-0.016, t = -2.157). In terms of economic significance, there is a 2.30% more decrease in CPI levels when we move from the 25th percentile to 75th percentile of CEO Tenure in the sample in the post-treatment period relative to pre-treatment period for non-compliant firms relative to compliant firms.

In the fourth column, we use an alternative measure of CEO power, *Co-opted Directors* and again, find that CPI levels decline following the exchange listing regulations for firms where the number of directors who are appointed after the current CEO is higher. In the fifth column, we use an industry level measure of CEO entrenchment, competition. Such a measure is less likely determined by the firm's endogenous governance environment. Our proxy for the degree of competition in the firm's industry is the Herfindahl-Hirschman index (*HHI).* It shows a coefficient estimate of -0.259 (t = -2.76), suggesting that CPI levels decrease more in firms that belong to industries where managers are likely to be less disciplined.[10] Finally, we combine all these measures into one variable labeled as *Aggregate CEO Power*. Column (6) reports results from Equation (2) using this aggregate measure of CEO power. Similar to the individual measures, *Aggregate CEO Power* triple interaction term has a negative and significant coefficient. In terms of economic significance, there is a 2.98% decrease in CPI levels when we move from the 25th percentile to 75th percentile of *Aggregate CEO Power* in the sample.

When we examine the control variables, their coefficient estimates are consistent with previous literature. *Industry Median CPI, Number of VPs,* and *CEO is Only Director* have positive coefficients, which is consistent with the tournament incentive hypothesis. Firm size and

[10] We use hypothetical increases in CEO power variables from 25th percentile to 75th percentile of the sample to assess the economic significance of our coefficient estimates. Such an increase in *Co-opted Directors* and *HHI* translates into 2.24% and 0.92% more decrease in CPI levels in the post-treatment period relative to pre-treatment period for non-compliant firms relative to compliant firms, respectively.

complexity measured by *Sales* do not have any significant effect on CPI level. Risk related variables, *Leverage*, *R&D* and *Capex* have negative coefficients but *Stock Return Volatility* coefficient is not significantly different from zero. The positive coefficient on *Industry-Adjusted ROA* is consistent with Bebchuk, Cremers, and Peyer (2011). The negative coefficient on *CEO is Blockholder* is in line with the argument that these CEOs have relatively small compensation packages but they are compensated through their high ownership stake in the firm.

Overall, results in Table 1.2 show that CEO pay inequality declines following the adoption of 2002 NYSE and NASDAQ governance reforms only for non-compliant firms with high CEO power. This suggests that CPI is higher in firms with problematic corporate governance environments, consistent with Bebchuk, Cremers, and Peyer (2011).

1.4.2 The Effect on the Components of CPI

CPI is constructed using two variables: CEO compensation as the numerator and total executive compensation of top 5 managers as the denominator. The negative sign we observe in the previous table can be attributed to a decrease in CEO compensation, an increase in non-CEO executive compensation or a combined effect. To distinguish among these explanations, we examine changes in the natural logarithm of the numerator and denominator around the passage of NYSE and NASDAQ exchange listing regulations separately, and report the results in Table 1.3.

In Column (1), the negative coefficient on the triple interaction term shows that exchange listing regulations result in a greater decrease in CEO compensation when CEO power is higher. In Column (2) of Table 1.3, the coefficient of the triple interaction term is positive but insignificant. These results suggest that while CEO compensation decreases, total non-CEO compensation increases after the exchange listing regulations in firms with powerful CEOs. It shows that powerful CEOs are transferring wealth from other executives when board monitoring is not effective. Overall, our results in Table 1.3 indicate that the decrease in CPI is mostly due to a decrease in CEO compensation. Furthermore, we estimate Equation (2) using Tobit as a robustness check since the dependent variable is bounded between 0 and 1 and find similar effects of the 2002 NYSE and NASDAQ governance reforms on CEO pay inequality.[11]

[11] We do not employ fixed effects model in our Tobit regressions (only use year dummies) since the coefficients of non-linear models are biased in the presence of fixed effects (e.g. Greene (2004)). Instead, we use random effects model with observed information matrix clustering. We also obtain similar results when we employ Tobit model without firm fixed effects and cluster standard errors at the firm-post level.

1.4.3 Exchange Listing Regulations, CPI, and Firm Value

The previous tables show that the CEO pay inequality shrinks following the passage of 2002 NYSE and NASDAQ governance reforms that mandate stricter board independence requirements for firms with powerful CEOs. To supplement these results, we analyze whether this decrease in managerial pay inequality is for good reasons (i.e., reducing entrenched managers' ability to expropriate wealth from shareholders in the form of higher compensation as in Bebchuk et al. (2011)) or for bad reasons (i.e., reducing tournament incentives or denying premium for highly-talented CEOs thereby dis-incentivizing them) in this section.

Firm value can increase following the adoption of 2002 NYSE and NASDAQ governance reforms due to effects on CEO pay through reductions in abnormal CEO pay and an enhanced sensitivity of CEO pay to firm performance, and better governance environments (CG (2009), CG (2007), and Aggarwal, Schloetzer, and Williamson (2014)). We hypothesize in this paper that the decrease in CPI can be an additional way for the exchange listing regulations to enhance firm value. In particular, several studies show that higher pay differentials amongst senior managers are related to lower firm values (e.g., Siegel and Hambrick (2005), Bebchuck et al. (2011)). However, the literature on tournament incentives suggests that reductions in CEO pay gap can reduce firm value (e.g., Kale et al. (2009)). Alternatively, CEO pay is already set optimally and any deviation from the optimal executive compensation policies due to the new exchange listing regulations reduces firm value.

With these competing hypotheses in the background, we analyze the valuation implications of CEO pay inequality using the 2002 NYSE and NASDAQ governance reforms as a quasi-natural experiment where only a subset of firms is affected by these exchange listing regulations and other firms constitute the control group. In particular, we estimate two different specifications. In the first one (Equation (3)), we do not take into account the effect of the deviation from CPI and test whether 2002 NYSE and NASDAQ governance reforms have an effect on the firm value. In the second one (Equation (4)), we incorporate the effect of CPI to our specification as follows.

$$\textit{Industry-Adjusted Tobin's } Q_{it} = \beta_0 + \beta_1 * \textit{Treatment} * \textit{Post} + \beta_2 * \textit{Post} + \textit{Controls}_{it} + \textit{Firm FE}_i + \textit{Year FE}_t + \varepsilon_{it} \quad (3)$$

$$\textit{Industry-Adjusted Tobin's } Q_{it} = \beta_0 + \beta_1 {*}\textit{Treatment}{*}\textit{Post}{*}\textit{Excess CPI} + \beta_2 {*}\textit{Treatment}{*}\textit{Post} + \beta_3 {*}\textit{Post}{*}\textit{Excess CPI} + \beta_4 {*}\textit{Post} + \textit{Controls}_{it} + \textit{Firm FE}_i + \textit{Year FE}_t + \varepsilon_{it} \quad (4)$$

where *Industry-Adjusted Tobin's Q* is our measure of firm value, defined as the ratio of market value of the firm plus total assets minus book value of equity minus deferred taxes to total assets, in excess of 2-digit SIC industry median values for a given year. We exclude financial firms and utilities in this estimation (2-digit SIC codes of 60-69 and 49, respectively). The control variables in Equation (3) differ slightly from those in the previous equations. Specifically, in Equation (3), we replace *Sales* with *Size*, which is the natural logarithm of total assets. We include the ratio of cash to total assets. We also include *Insider Ownership* and *Insider Ownership Squared* since McConnell and Serveas (1990) find a hump-shaped relation for the relation between insider ownership and firm value. We do not use the variables related to tournament incentives and CEO power proxies.

Results from this estimation are reported in Table 1.4. In the first column of Table 1.4, double interaction term *Treatment*Post* is positive and statistically significant, suggesting that exchange listing regulations have a positive effect on firm value for firms in the treatment group. This result is consistent with the findings of Aggarwal, Schloetzer, and Williamson (2014). They find that poorly governed firms have higher firm value after the exchange listing regulations. In Column (2), we examine changes in firm valuation around 2002 NYSE and NASDAQ governance reforms conditional on the extent of abnormal CEO pay inequality in the pre-regulation period. We calculate abnormal CPI (*Excess CPI*) as residual terms obtained from running the regression specification in Column (6) of Table 1.2 for the pre-regulation period. This variable measures the deviation from the expected CPI levels. Therefore, a higher deviation implies higher compensation of CEO and higher managerial agency costs related to compensation. For robustness, we re-estimate excess CPI by using Column (1) of Table 1.2 that does not include the aggregate CEO power variable in the regression specification, and find similar results. We interact this variable with *Treatment*Post* to test the effect of exchange listing regulations on firm value for firms with high versus low levels of managerial agency problems. Column (2) shows that the coefficient on the triple interaction term is positive, suggesting that the gain in firm value is higher for firms affected by exchange listing regulations and that have higher abnormal CPI levels. In a similar fashion, we repeat our analysis using *Log (CEO Pay)* instead of *CPI* where the regression specification in Column (1) of Table 1.3 is used to calculate abnormal CEO pay (*Excess CEO Pay*). The triple interaction term in Column (3) of Table 1.4 is positive, suggesting that the increase in firm value is higher for treated firms with high abnormal CEO pay. We also undertake an analysis where we examine the impact of the exact average firm-level change in CPI and CEO pay (winsorized at 1 percent) around the regulations on firm value and continue to find that non-compliant firms with a higher decrease in CPI and CEO pay experience higher firm valuations (not tabulated). Overall, these results suggest that following the 2002 NYSE and NASDAQ governance reforms, firm value improves and this improvement is generally higher for firms that have abnormal CPI and CEO pay levels in the pre-regulation period.

1.4.4 Robustness of our Main Results to Alternative Variable Definitions

To check the robustness our main result on firm value to different variable definitions, we create a binary version of *Excess CPI* variable by setting its value equal to 1 if it is greater than or equal to its median value in the sample and 0 otherwise. We also use the natural logarithm of our firm value measure (*Log (Industry-Adjusted Tobin's Q))* as the dependent variable to confirm the robustness of our results on firm valuation. Results from these regressions are reported in Table 1.5. In Column (1), we use the binary version of *Excess CPI* with *Industry-Adjusted Tobin's Q* as the dependent variable. In Column (2), we use *Log (Industry-Adjusted Tobin's Q)* as the dependent variable and the continuous version of *Excess CPI*. In Column (3), we use *Log (Industry-Adjusted Tobin's Q)* as the dependent variable but with the binary version of *Excess CPI*. In all the columns of Table 1.5, the triple interaction term is positive significant suggesting that our main result is robust to alternative variable definitions.

We also replicate our main results by using alternative *Aggregate CEO Power* measures and report the results in Table 1.6. Our first alternative measure is the first principal component of 4 CEO power variables, which are *CEO is Chair, CEO Tenure*, *Co-opted Directors,* and *HHI*. The second alternative measure is an additive measure where each CEO power variable (except *CEO is Chair*) is transformed into a binary variable. If the CEO power measure is greater than or equal to the sample median than the binary variable takes a value of 1 and 0 otherwise. Then, these variables are added to construct the 0-1 additive measure. In Panel A of Table 1.6, we use the regression specification in Column (6) of Table 1.2 with alternative *Aggregate CEO Power* measures. In Column (1), we report the results using the first principal component and in Column (2), we report the results using the 0-1 additive measure. The triple interaction term is negative and statistically significant in both columns.

In Panel B, we use the regression specification in Column (2) of Table 1.4 with alternative *Excess CPI* measures that are obtained from the corresponding alternative *Aggregate CEO Power* measures. In Column (1), we report the results using first principal component and in Column (2), we report the results using the 0-1 additive measures. The triple interaction term is positive and statistically significant in both columns. Overall, the results in Table 1.5 and Table 1.6 suggest that our previous results are robust to alternative definitions of *Aggregate CEO Power*.

1.4.5 The Effect of Monitoring Intensity

Although the exchange listing regulations can improve the monitoring efficiency of the firm by increasing board independence, there are other monitoring mechanisms that can mitigate such agency conflicts. In other words, other mechanisms can substitute for board independence. Thus, the increase in firm valuation around the adoption of 2002 NYSE and NASDAQ governance reforms we document in Table 1.4 is likely to concentrate on firms with weak governance in the pre-regulation period.

To test this hypothesis, we estimate Equation (4) conditional on measures of the degree of monitoring effectiveness of managers. We use 4 different measures for this purpose. The first variable is related to busy boards. Falato, Kadyrzhanova, and Lel (2014) document that busy boards imply low monitoring quality. They discuss that an increase in the workload of the board decreases the monitoring quality. We define *Non-Busy Board* as the ratio of number of non-busy independent directors to number of independent directors. We define a director as busy if he holds at least three directorships. Higher values indicate a less busy board and a higher monitoring quality. We also use *Director Ownership,* as Core, Holthausen, and Larcker (1999) discuss that higher average outside director ownership implies better monitoring. In addition, we use *Institutional Ownership,* since higher institutional ownership can reduce the degree of managerial agency problems (e.g., Hartzell and Starks (2002)). Our last variable is related to debt maturity. Shorter debt maturity can act as an external monitoring mechanism on managers, as firms are subject to more frequent monitoring by the creditors through debt rollovers (e.g., Rajan and Winton (1995)). We define *Short-Term Debt* as one minus the ratio of long-term debt to total debt. Higher values indicate higher monitoring quality. We obtain the values of these 4 alternative measures of the degree of managerial monitoring as of 2002, and use them to split our sample into two sub-groups based on the median values of each monitoring variable, defined as low monitoring and high monitoring.

Results from this estimation are reported in Table 1.7. The key variable of interest is the triple interaction term *Treatment*Post*Excess CPI* across subsamples based on the degree of monitoring effectiveness to test the effect of exchange listing regulations on firm valuation for different levels of managerial agency problems. For ease of interpretation, we do not use quadruple interaction terms and instead focus on subsamples based on alternative monitoring mechanisms.

In the first two columns, we use *Non-Busy Board* variable to measure monitoring effectiveness. The first (second) column includes firms with busy (non-busy) boards and less (more) efficient monitoring. The coefficient estimate on the triple interaction term is greater and statistically more significant in the first column than in the second column. This results suggest that when agency problems related to CPI levels are combined with busy boards, the exchange listing regulations have greater impact on firm value. In Column (3) and (4), we use the average share ownership of independent directors to measure the degree of incentive alignment of directors. The coefficient estimate on the triple interaction term is positive significant for the low group and insignificant for the high group. Another strong monitoring mechanism is institutional share ownership, which is used to proxy for the degree of monitoring effectiveness in Column (5) and (6). The results on the triple interaction term again show that the increase in firm value around exchange listing regulations is higher in firms with low institutional ownership. The last two columns show a positive and statistically significant coefficient on the triple interaction term in the low short-term debt subsample, consistent with a monitoring role of short-term debt.

Overall, these results suggest that the increase in firm value around the adoption of 2002 NYSE an NASDAQ governance reforms due to abnormal CPI levels is concentrated on firms with weak monitoring of managers in the pre-adoption period. Thus, they are consistent with the Bebchuk et al.'s (2011) view that CPI reflects poor governance environments.

1.5 Additional Robustness Checks

In this section, we report results from additional tests that serve as robustness checks for our main results. These tests are a time-trend analysis of changes in firm value around the adoption of the exchange listing regulations; placebo tests where the exchange listing regulations are assumed to be enacted in different years; re-defining the treatment variable in a continuous manner; using compensation committee independence to define the treatment group of firms; a matched sample analysis; and a simulation analysis for t-statistics.

1.5.1 Time Trend Analysis

We examine the time trend of the decrease in CEO pay inequality and the increase in the firm value to analyze the effect of exchange listing regulations year by year and also to confirm that the changes in firm value and CPI level are not due to other confounding events in our sample period (e.g., Aggarwal, Schloetzer, and Williamson (2014), and Li and Sun (2014)). In order to achieve this, we use the specification in Column (6) of Table 1.2 for CEO pay inequality and the specification in Column (2) of Table 1.4 for firm value and replace our *Post* dummy with year dummies. To prevent multi-collinearity, we take 2002 as our base year and do not use it in the regression. Therefore, the coefficients on triple interaction terms with year dummies show the changes in CPI level and firm value relative to year 2002.

Panel A of Table 1.8 reports our results. Column (1) reports the results for CEO pay inequality and Column (2) reports the results for firm value. We only report the triple interaction terms to save space. As evident from Column (1) of Table 1,6, we do not observe any year specific effects before exchange listing regulation years. The coefficients on the triple interaction terms are close to zero. However, starting in year 2003, the coefficients become negative and their statistical significance is higher compared to pre-regulation periods and they vanish as we move away from year 2002. In Column (2), we do not observe any year specific effects before exchange listing regulations years. The coefficients on the triple interaction terms are not statistically different from zero. However, starting in year 2004, the coefficients on the triple interaction term become positive and statistically significant. While we expect that the coefficient for 2003 to be also significant, the adjustment to executive compensation around the exchange listing regulations may take place with lag, especially given that we mainly test the effect of abnormal CPI levels on firm value. Moreover, exchange listing regulations become legally effective in year 2003. Overall, our time trend results suggest exchange listing regulations result in a decrease in CEO pay inequality and they have favorable effect on firm value for the treatment firms with agency problems.

While the time trend analysis suggests that our findings are not confounded by other events during the sample period, we formally test whether our main results are an artifact of 2004 FASB Option Expensing Rule. This is a potential confounding event which requires public firms to use grant date fair value of an option in expensing so that they can no longer use the intrinsic value method, which gives a value of zero for at-the-money options.[12] Due to this new rule, some firms become inclined to switch from option-based awards to stock-based awards or stop granting option awards. Therefore, our results may be capturing this effect if this new rule affects firms differently.

In order to test whether our results are confounded by the new option expensing rule, we construct a new variable *COPI*, CEO Option Pay Inequality, using option values instead of total compensation. Following CG, we compare *COPI* distribution of high CEO power non-complying firms to low CEO power non-complying firms in the pre-event period. The average pre-event period *COPI* of high (low) CEO power non-complying firms is 0.303 (0.343). The 10th percentile value is 0.000 (0.000). The 25th percentile value is (0.000) (0.200). The median value is 0.319 (0.345). The 75th value is 0.475 (0.455). The 90th percentile value is 0.612 (0.641). These numbers show that there is no significant difference in *CPIO* distribution among two groups. As a matter of fact, high CEO power group has lower *COPI*, implying that the decrease in CPI due to new option expensing rule is less likely for these firms. Overall, our main results are not likely to be an artifact of 2004 FASB Option Expensing Rule.

1.5.2 Placebo Tests

Our empirical design relies heavily on the exogenous shock of exchange listing regulations. To strengthen our findings that CPI decreases and firm value increases following the exchange listing regulations, we conduct two different placebo tests to show the validity of our difference-in-difference approach. If our difference-in-difference approach is not misspecified, we should not observe any change in CPI levels or firm value in absence of exchange listing regulations.

In the first placebo test, we hypothetically assume that the exchange listing regulations are passed in year 2000 and accordingly use board independence information in year 2000 to determine the treatment group. We run our placebo tests for both CPI levels and firm value regressions and report the results in Panel B of Table 1.8. In Column (1), we run the regression

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[12] This new option expensing rule does not introduce any mechanical bias in CPI calculation because Execucomp uses the Black-Scholes values of options in the calculation of total compensation (*TDC1*).

specification in Column (6) of Table 1.2, except that we assume 2000 as the year of exchange listing regulations and we restrict our sample period to 1998-2003 to center our sample period around year 2000. The column reports a statistically insignificant coefficient estimate on the triple interaction term. In Column (2), we conduct a similar placebo test on firm value, where *Excess CPI* is estimated using the placebo treatment year. Again, results show that the triple interaction term does not have a statistically significant coefficient.

In our second placebo test, we assume the year of exogenous shock to be 2004. This time, we do not use the board independence of the firms in year 2004 since all the firms theoretically satisfy the board independence requirements. Therefore, we assign uniformly distributed random numbers between 0 and 1 to board independence and choose the treatment firms accordingly. We restrict our sample period to 2002-2007 to center the sample period around year 2004. We report the results from this placebo test in Panel C of Table 1.8, where Column (1) shows the results for CPI levels and Column (2) shows the results for firm value. The triple interaction terms are not statistically different from zero in either column. Overall, the results of placebo tests strengthen our identification strategy and give additional support for our difference-in-difference approach.

1.5.3 Degree of Non-Compliance and Compensation Committee Independence

The Riskmetrics definition of director independence is stricter than the independence definition in exchange listing regulations. As a robustness check, we define *Treatment* in a continuous manner. We measure the degree of non-compliance by transforming the *Treatment* variable as follows. If the board independence percentage is less than 0.5 then *Treatment* = 0.51 – (board independence percentage) and 0, otherwise.[13] Hence, *Treatment* gets larger as the board independence percentage gets smaller and *Treatment* is assigned 0 for the firms that already comply with the board independence requirement, regardless of the ratio of independent directors. We replicate the regression specification in Column (6) of Table 1.2 with the transformed *Treatment* variable and present the results in Column (1) of Table 1.9. Consistent with our hypothesis, the triple interaction term is still negative and significant.

The exchange listing regulations mandate compensation committees to consist of solely independent directors. Since our primary concern is CEO compensation, we also conduct tests on the compliance of compensation committees. Our hypotheses are the same as the ones for the board

[13] We use 0.51 instead of 0.50 to differentiate between the compliant and non-compliant firms.

independence requirement. We classify treatment firms as those without a fully independent compensation committee as of 2002. If the firm does not have compensation committee, we assume that the firm satisfies the compensation committee regulation. We repeat our analysis in Column (6) of Table 1.2, where *Treatment* is based on compensation committee requirements of the exchange listing regulations.

We present the results in Column (2) of Table 1.9. The triple interaction term is negative and statistically significant and larger in magnitude than the one in Column (6) of Table 1.2, in which *Treatment* is based on board independence requirement. We also transform *Treatment* variable to a continuous variable to measure the degree of non-compliance.[14] Using the new definition of *Treatment*, we re-run the regression specification in Column (6) of Table 1.2. The results in Column (3) of Table 1.9 show that the triple interaction term is negative and statistically significant, which is consistent with our previous findings.

We also repeat this robustness test for firm valuation analysis and report the results in Table 1.10. In Column (1) and (2), we use the degree of non-compliance for board independence requirement. In Column (1), the coefficient on double interaction term (*Treatment***Post*) and in Column (2), the coefficient on triple interaction term (*Treatment***Post***Excess CPI)* is positive and significant, consistent with our previous findings on firm value. These suggest that our results are robust to identifying the *Treatment* in a manner that takes into account the degree of non-compliance. In Column (3) and (4) of Table 1.10, we use the compensation committee requirement with a binary measure of *Treatment*. The double interaction term in Column (3) of Table 1.10 still has a positive and significant coefficient but the triple interaction term is not statistically significant. Repeating the same analysis with the degree of non-compliance for compensation committees in the last two columns, we find a statistically significant and positive coefficient on the double interaction term. However, the triple interaction term is not statistically significant. There may be two reasons why we cannot find significant coefficients for triple interaction terms in firm value regressions for compensation committee requirements. First, there is less variation in compensation committee independence requirements than the board independence requirements in identifying the treatment group. This is because firms are required to have fully independent compensation committees since 1994 in order to be exempt from the $1 million deductibility cap,

[14] The transformation is as follows. If the ratio of independent directors in the compensation committee is less than 1, then *Treatment* = 1 – (the ratio of independent directors in the board), and 0 otherwise.

which many large firms adopted (Murphy (2013)). Second, the improvement in board independence is more important than that in compensation committee regarding the negotiation power of directors with CEOs, and for the overall governance quality of firms.

To further support our findings, we include both board independence and compensation committee exchange listing regulations in the same regression (e.g., CG (2009), Dahya, McConnell, and Travlos (2002)). We report the results in Table 1.11. In Column (1), we use CEO pay inequality as the dependent variable. The coefficient on both triple interaction terms (*Treatment (Board)*Post*Aggregate* CEO Power and *Treatment (Comp)*Post*Aggregate CEO Power*) are both negative and significant, suggesting that our previous results on CPI levels are associated with both board independence and compensation committee requirements. In Column (2), we use Industry-Adjusted Tobin's Q as our dependent variable. *Excess CPI* is estimated using the specification in Column (1). While the coefficient on *Treatment (Board)*Post*Excess CPI* is positive and significant, the coefficient estimate on *Treatment (Comp)*Post*Excess CPI* is not significant, suggesting that the increase in firm value is associated with board independence requirement. Overall, board independence and compensation committee requirements both play a role in CPI levels. However, the improvement in firm value is associated with only board independence.

1.5.4 Matched Sample Analysis

Even though our difference in difference approach is plausibly exogenous, we construct a matched sample to mitigate the endogeneity concerns. Since we use observational data to estimate the effect of exchange listing regulations, our treatment group and control group could differ in unobservable dimensions that predict receiving the treatment. This can lead to a biased estimate of our difference in difference variable. Therefore, we use propensity score matching to construct a new control group and test if our results still hold with this matched sample.

In order to do propensity score matching, we choose observations in 2002, which is the year prior to exchange listing regulations. We use lagged values of *Log (CEO Pay)*, *CPI*, *Size*, and *Industry-Adjusted ROA* as covariates that predict receiving the treatment. We also match based on 2-digit SIC industry classification. For each 2-digit SIC industry, we use the Mahalanobis distance to determine the closest match. Thus, we match firms in the same 2-digit SIC industry using the covariates *Log (CEO Pay)*, *CPI, Size*, and *Industry-Adjusted ROA*. If 2-digit SIC industry matching is not possible due to data availability, then we do the matching using the whole sample of firms for that particular industry. Table 1.A.2 in the appendix presents the results for match quality. We run a logit regression for the matched sample based on the matching covariates. The results are in Panel A. The covariates do not predict the treatment in the matched sample. Similarly, in Panel B, we compare the sample means of covariates for treatment and control firms in the matched sample and they are statistically not significant except for *CPI*, where the difference in means is statistically significant only at 10 % level. Overall, the results in Panel A and B of Table 1.A.2 in the appendix imply a good quality match.

We replicate our main results using the matched sample. In Panel A, we replicate our results based on *CPI* and *Log (CEO Pay)*. In Column (1), we replicate the regression specification in Column (6) of Table 1.2. The triple interaction term is again negative and statistically significant. This is consistent with our hypothesis that independent boards lead to a decrease in CPI levels when the CEO is powerful. In Column (2), we replicate the regression specification in Column (1) of Table 1.3, where the dependent variable is *Log (CEO Pay)*. Although the coefficient on the triple interaction term is statistically insignificant, it is negative, implying that overall CEO compensation decreases. We also replicate our main firm value regressions and report the results in Panel B of Table 1.12. Column (1) shows that the triple interaction terms are again positive and significant, implying that the improvement in firm value is higher when managerial agency costs are higher for the treated firms. In Column (2), we use the specification in Column (3) of Table 1.4, where abnormal pay is measured by *Excess CEO Pay*. The coefficient on triple interaction term is again positive and significant. Overall, we confirm our previous findings using the matched sample.

1.5.5 Simulation Analysis

Our results heavily rely on the difference-in-difference methodology. This methodology can over-reject the null hypothesis when observations are serially correlated since the standard-errors are underestimated due to positive serial correlation (e.g. Bertrand, Duflo, and Mullainathan (2004). To overcome this problem, we estimate the distribution of the triple interaction terms *Treatment*Post*Aggregate CEO Power* in Column (6) of Table 1.2 and *Treatment*Post*Excess CPI* in Column (2) of Table 1.4 by using pseudo shocks for treatment firms. The argument is the following. Since these placebo shocks are fictitious, a significant real effect at the 5% level should be found 5% of the time. In order to do this, we randomly choose 286 firms (the number of treatment firms in the original sample) as treated, run the specifications in Column (6) of Table 1.2 and Column (2) of Table 1.4, and obtain the t-statistics. We replicate this procedure for 5,000 times and plot the histogram of t-statistics. Then, we compare our original t-statistics with the simulated t-statistics. We report the results in Figure 1.1. The top graph in Figure 1.1 shows that our original t-statistics for the triple interaction term *Treatment*Post*Aggregate CEO Power* is located to the left of 2.5th percentile of the simulated t-statistics distribution. Similarly, the bottom graph in Figure 1.1 shows that our original t-statistics for the triple interaction term *Treatment*Post*Excess CPI* is between 2.5th and 5th percentile of the simulated t-statistics. These results strengthen our previous findings on CEO pay inequality and firm value.

1.6 Conclusion

In this paper, we examine whether CEO pay inequality is an outcome of poor corporate governance and its implications for shareholder wealth. We use the 2002 NYSE and NASDAQ governance reforms that mandated majority independent boards for listed firms as a plausibly exogenous source of variation in the governance environment of firms. Our results show that within-firm disparity in executive pay decreases following the passage of these regulations only in firms with entrenched CEOs that were affected by the regulations. This finding implies that the strength of governance measured as board independence plays an important role in preventing rent extraction of CEO in the form of relative compensation. We also find that firm value increases on average for the affected firms in the post regulation period, which partially depends on the pre-regulation CPI levels. The existence of other monitoring mechanisms in the pre-regulation period reduces the potential improvement in firm value due to lower CPI levels. These results extent our knowledge of the relation between executive pay inequality and the governance environment of firms, and whether such pay inequalities are beneficial or harmful to shareholder wealth. They also build on the evidence that 2002 NYSE and NASDAQ governance reforms improve firm valuation and strengthen the internal governance environment of firms. Overall, our results suggest that high CEO pay inequality is associated with weak governance environments and low firm valuations.

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Appendix 1.A

Table 1.A.1 Variable Definitions
This table presents brief definitions of the variables, how the variables are constructed, and data sources.

Variables	**Definition**
Exchange Listing Regulation Variables	
Treatment	It is an indicator variable that equals one if the firm meets exchange listing requirement in year 2002, zero otherwise. The board requirement is that majority of the board members must be independent. The compensation committee requirement is that all the committee members must be independent. A board member is set to be independent if *CLASSIFICATION* is equal to '*I*'. We define the degree of non-compliance counterpart for the board regulation as (0.51 - number of independent board members/total number of board members) and for the compensation committee regulation as (1.00 - number of independent committee members/total number of committee members). Source: Riskmetrics
Post	It is an indicator variable that equals one if the year is greater than or equal to 2003, zero otherwise.
Dependent Variables	
CEO Pay	Total annual compensation (*TDC1*) of CEO in 2002 dollars. Adjustment is done by using Consumer Price Index (CPI). We set the value to missing if there is a CEO turnover. Log (CEO Pay) is defined as the natural logarithm of CEO Pay. Source: Execucomp
Non-CEO Pay	Sum of total annual compensation (*TDC1*) of top 4 paid non-CEO executives in 2002 dollars. Adjustment is done by using Consumer Price Index (CPI). The value is set to missing if there are less than 4 executives with non-missing annual compensation item. We set the value to missing if there is a CEO turnover. Log (Non-CEO Pay) is defined as the natural logarithm of Non-CEO Pay. Source: Execucomp
CPI	(CEO Pay)/(CEO Pay + Non-CEO Pay)
COPI	(CEO Option Value)/(CEO Option Value + Non-CEO Option Value) We use OPTION_AWARDS_BLK_VALUE to determine total value of the options granted to each executive. Source: Execucomp
Industry-Adjusted Tobin's Q	The difference between Tobin's Q of the firm and its corresponding 2-digit SIC industry median value for a given year in the Compustat universe. Tobin's Q is the ratio of market value of the firm (*CSHO*PRCC_F)* plus total assets (*AT*) minus book value of equity (*CEQ*) minus deferred taxes (*TXDB*) to total assets (*AT*). We set deferred taxes to zero if it is missing. We set the value to missing if the 2-digit SIC code of the firm is 49 or it is between 60 and 69. Log (Industry-Adjusted Tobin's Q) is defined as the natural logarithm of 10 + Industry-Adjusted Tobin's Q. Source: Compustat
CEO Power Variables	
CEO is Chair	It is an indicator variable that equals one if CEO is also the chairman of the board of directors, zero otherwise. *TITLEANN* is used to identify the chairman. Source: Execucomp
CEO Tenure	Natural logarithm of fiscal year minus the year she became CEO (*BECAMECEO)*. Source: Execucomp
Co-opted Directors	The ratio of "Co-opted Directors" to number of board members. A "Co-opted Director" is a director who becomes a member of the board after the current CEO. It is calculated using the variables *DIRSINCE* in Riskmetrics and *BECAMECEO* in Execucomp. If the *DIRSINCE* is greater than the year of *BECAMECEO*, the director is classified as "Co-opted Director". If either variable or CEO Tenure is missing, it is set to missing and the ratio is calculated by dropping the missing director observation.
HHI	Herfindahl-Hirschman Index of 2-digit SIC industries in Compustat universe. It is calculated using *SALE in* Compustat.

(*Table 1.A.1 continued*)

Aggregate CEO Power	It is the sum of CEO is Chair and other three scaled CEO Power variables. A CEO Power Variable is scaled by subtracting the sample minimum and dividing the result by the difference between sample maximum and sample minimum.
Excess Pay Variables	
Excess CPI	This variable is constructed using the residuals of the regression specification in Column (6) of Table 1.2. The residuals are obtained for each firm in year 2002 and defined as the Excess CPI of the corresponding firm. The binary version of the variable equals one if the firm's Excess CPI is greater than the median value of Excess CPI of firms in year 2002, zero otherwise.
Excess CEO Pay	This variable is constructed using the residuals of the regression specification in Column (1) of Table 1.3. The residuals are obtained for each firm in year 2002 and defined as the Excess CEO Pay of the corresponding firm.
Firm Related Variables	
Industry Median CPI	The firm's corresponding 2-digit SIC industry median CPI value for a given year in EXECUCOMP universe. Source: Execucomp.
Number of VPs	The total number of vice presidents among CEO and top 4 paid non-CEO executives. *TITLEANN* is used to identify vice presidents. Source: Execucomp
CEO is Only Director	It is an indicator variable that equals one if CEO is the only board member among the executives in CPI calculation, zero otherwise. Source: Execucomp.
Size	Natural logarithm of total assets. (*TA*). Source: Compustat.
Sales	Natural logarithm of net sales. (*SALE*). We set the value to missing if it is less than zero. Source: Compustat.
Leverage	Total long-term debt (*DLTT*) divided by total assets (*TA*). Source: Compustat.
Stock Return Volatility	Annualized standard deviation of daily stock returns. Source: CRSP.
Capex	The ratio of capital expenditures (*CAPX*) to total assets (*TA*). Source: Compustat
Cash	The ratio of cash *(CH*) to total assets (*TA*). Source: Compustat
R&D	The ratio of research and development expense (*XRD*) to net sales (*SALE*). We set the value to zero if it is missing. Source: Compustat
R&D is Missing	It is an indicator variable that equals one if research and development expense (*XRD*) is missing, zero otherwise. Source: Compustat
Industry-Adjusted ROA	The difference between return on assets of the firm and its corresponding 2-digit SIC industry median value for a given year in the Compustat universe. Return on assets is defined as the ratio of operating income before depreciation (*OIBDP*) to total assets (*TA*). Source: Compustat.
CEO is Blockholder	It is an indicator variable that equals one if CEO has at least 10 % ownership in the firm, zero otherwise. CEO ownership is defined as (SHROWN_EXCL_OPTS_PCT)/100. If the value is missing, (SHROWN_EXCL_OPTS)/(CSHO)/100 is used. Source: Execucomp and Compustat.
Insider Ownership	The sum of non-director executives' (the ones used in CPI calculation) ownership and total director ownership. If total director ownership is missing, total executive (the ones used in CPI calculation) ownership is used. We set the value to missing if it is greater than one. Source: Execucomp and Riskmetrics.
Non-Busy Board	The ratio of number of non-busy independent directors to number of independent directors. We define a non-busy director who holds less than three directorships (*OUTSIDE PUBLIC BOARDS* is less than two). If *OUTSIDE PUBLIC BOARDS* value is missing for an independent director, then she is not included in the calculation. Source: Riskmetrics.
Director Ownership	The average ownership of the independent directors. To find director ownership, we use (*NUM_OF_SHARES*)/(*SHROUT*)/1000 where *SHROUT* is the number of shares outstanding from CRSP at *MEETINGDATE* reported by Riskmetrics. Missing director ownership values are not included in the calculation of the average. Source: Riskmetrics and CRSP.

(*Table 1.A.1 continued*)

Institutional Ownership	The percentage of a firm's shares owned by institutional investors as of the closest filing to the fiscal year end. We use number of shares held by institutions at *FDATE* of the database and use the quarter end *SHROUT* from CRSP to calculate the percentage. Source: Thomson Reuters and CRSP.
Short-Term Debt	One minus the ratio of long-term debt (*DLTT*) to total debt (*DLTT*+*DLC*). We set the value to missing if it is less than zero or larger than one. Source: Compustat

Table 1.A.2 Matched Sample Quality

This table presents the estimates of the logit regression and sample mean statistics for treatment and control group firms in the matching analysis. Matching is done using a nearest neighbor propensity score matching with Mahalanobis distance and in year 2002 using lagged CPI, Log (CEO Pay), Size, and Industry-Adjusted ROA as the matching variables. The matching procedure is carried out for each 2-digit SIC industry. If matching is not possible for a particular industry due to data availability, then whole sample is used for matching. The variable definitions are given in Table 1.A.1 in the appendix. We winsorize all continuous variables at 1 % level and use one-year lagged values of time-varying continuous independent variables. Panel A shows the logit regression results. Z-values are reported in the parenthesis. Panel B shows the covariate sample means and two sample t-test results for treatment and control groups. ***, **, and * indicate significance at 1%, 5%, and 10% levels, respectively.

Panel A: Logit Regression

Variables	Probability of Being Treated
Log (CEO Pay)	0.008
	(-0.05)
CEO Pay Inequality	-1.445
	(-1.313)
Size	-0.025
	(-0.281)
Industry-Adjusted ROA	1.133
	(-1.012)
Constant	0.775
	(-1.067)
Observations	410
Pseudo R-squared	0.00949

Panel B: Mean Differences

Variables	Treatment Group (N=230)	Control Group (N=180)	t-value
Log (CEO Pay)	7.573	7.721	1.31
CEO Pay Inequality	0.332	0.358	1.939*
Size	7.16	7.276	0.749
Industry-Adjusted ROA	0.086	0.073	-1.277

Figure 1.1 t-statistics Distribution of Pseudo Treatment Effects

This graph shows the simulation results of estimated t-statistics for our main specifications with pseudo treatment effects. For each replication of the simulation, 286 firms (which is the number of treated firms in the original sample) are randomly chosen as treatment firms. The number of replications is 5,000. The top graph shows the t-statistics distribution of the triple interaction term Treatment*Post*Aggregate CEO Power for the regression specification of Column (6) of Table 1.2 with pseudo treatment effects. The bottom graph shows the t-statistics distribution of the triple interaction term Treatment*Post*Excess CPI for the regression specification of Column (2) of Table 1.4 with pseudo treatment effects. The red lines show the percentiles of the simulated t-statistics distribution and the green line shows the realized t-statistics.

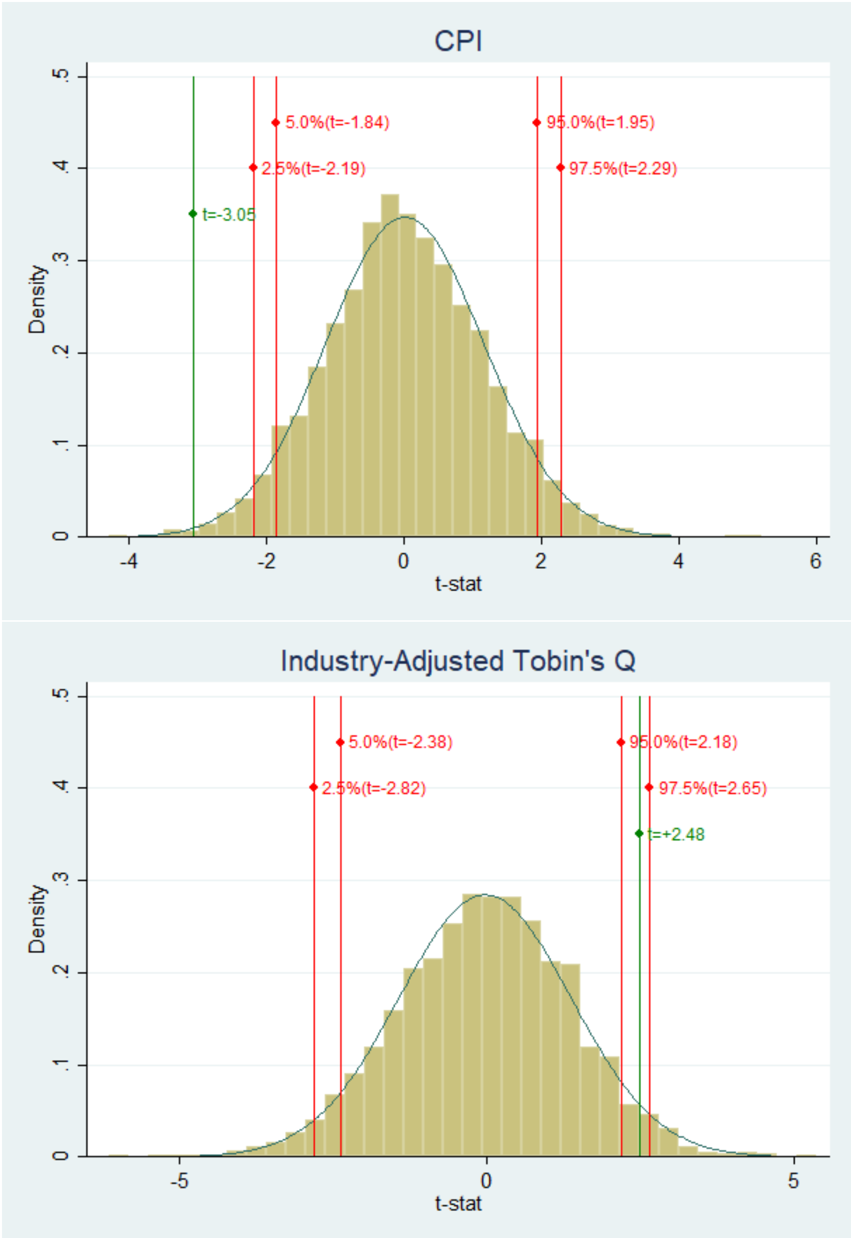

Table 1.1 Descriptive Statistics

This table presents various descriptive statistics of the variables. The variable definitions are given in Table 1.A.1 in the appendix. We winsorize all continuous variables at 1 % level and use one-year lagged values of time-varying continuous independent control variables.

Variables	Obs.	Mean	Std. Dev.	Min.	Median	Max.
Dependent Variables						
CPI	10,261	0.379	0.120	0.063	0.382	0.714
Log (CEO Pay)	10,261	7.999	1.061	5.571	7.975	10.584
Log (Non-CEO Pay)	10,261	8.538	0.885	6.832	8.445	10.970
Industry-Adjusted Tobin's Q	9,929	0.467	1.453	-1.692	0.080	7.619
Log (Industry-Adjusted Tobin's Q)	9,929	2.340	0.123	2.117	2.311	2.869
CEO Power Variables						
CEO is Chair	12,075	0.619	0.486	0.000	1.000	1.000
CEO Tenure	11,665	1.713	0.889	0.000	1.792	3.555
Co-opted Directors	10,394	0.368	0.297	0.000	0.333	1.000
HHI	12,372	0.060	0.051	0.012	0.042	0.282
Aggregate CEO Power	10,319	1.659	0.831	0.000	1.720	3.781
Control Variables						
Industry Median CPI	12,335	0.373	0.038	0.089	0.377	0.548
Number of VPs	12,380	2.664	1.249	0.000	3.000	5.000
CEO is Only Director	12,380	0.489	0.500	0.000	0.000	1.000
Sales	12,207	7.252	1.564	3.582	7.148	11.011
Size	12,210	7.552	1.736	4.175	7.360	12.474
Leverage	12,185	0.177	0.155	0.000	0.156	0.633
Stock Return Volatility	12,129	0.431	0.208	0.143	0.380	1.158
Capex	11,548	0.055	0.051	0.000	0.041	0.272
Cash	12,026	0.088	0.103	0.000	0.045	0.489
R&D	12,210	0.043	0.097	0.000	0.000	0.668
R&D is Missing	12,210	0.464	0.499	0.000	0.000	1.000
Industry-Adjusted ROA	12,117	0.073	0.112	-0.182	0.045	0.462
Insider Ownership	12,000	0.081	0.119	0.000	0.032	0.614
CEO is Blockholder	11,893	0.070	0.256	0.000	0.000	1.000
Excess Pay Variables						
Excess CPI	9,068	-0.001	0.079	-0.229	0.000	0.261
Excess CEO Pay	9,068	0.004	0.493	-1.459	0.004	1.416
Monitoring Variables						
Non-Busy Board	12,380	0.751	0.238	0.000	0.778	1.000
Average Director Ownership	12,320	0.002	0.006	0.000	0.001	0.085
Institutional Ownership	12,380	0.715	0.185	0.079	0.736	1.000
Short-Term Debt	11,030	0.214	0.270	0.000	0.102	1.000

Table 1.2 Exchange Listing Regulations and CEO Pay Inequality

This table presents the estimates of the effect of exchange listing regulations on CEO pay inequality (CPI). The dependent variable is CPI. Column (1) does not incorporate the effect of CEO power. Column (2)-(5) use CEO power variables to estimate the conditional effect. The variable definitions are given in Table 1.A.1 in the appendix. We winsorize all continuous variables at 1 % level and use one-year lagged values of time-varying continuous independent control variables. t-values are reported in the parenthesis. Robust standard errors are clustered at firm-period level. ***, **, and * indicate significance at 1%, 5%, and 10% levels, respectively.

	Base Specification	**CEO is Chair**	**CEO Tenure**	**Co-opted Directors**	**HHI**	**Aggregate CEO Power**
Variables	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Treatment*Post*CEO Power		-0.030***	-0.018**	-0.045**	-0.259***	-0.023***
		(-2.584)	(-2.574)	(-2.038)	(-2.762)	(-3.051)
Treatment*Post	0.004	0.019**	0.039***	0.022**	0.022**	0.044***
	(0.671)	(2.326)	(2.665)	(2.178)	(2.543)	(3.050)
Treatment*CEO Power		-0.012	-0.002	0.002	0.200	0.000
		(-1.007)	(-0.285)	(0.081)	(1.639)	(0.026)
Post*CEO Power		0.000	-0.001	-0.012	0.029	-0.004
		(0.030)	(-0.350)	(-1.194)	(0.660)	(-1.115)
Post	0.014***	0.015**	0.017**	0.018***	0.013**	0.021**
	(2.696)	(2.248)	(2.045)	(2.718)	(2.268)	(2.487)
CEO Power		0.011*	0.003	0.011	-0.113*	0.006*
		(1.884)	(0.862)	(1.334)	(-1.754)	(1.758)
Industry Median CPI	0.592***	0.598***	0.599***	0.593***	0.596***	0.593***
	(12.082)	(12.117)	(11.821)	(11.071)	(12.146)	(11.015)
Number of VPs	0.013***	0.014***	0.013***	0.015***	0.013***	0.016***
	(7.593)	(7.950)	(7.367)	(8.155)	(7.581)	(8.300)
CEO is Only Director	0.030***	0.029***	0.030***	0.028***	0.030***	0.028***
	(8.737)	(8.524)	(8.720)	(7.603)	(8.759)	(7.647)
Sales	-0.006	-0.006	-0.006	-0.002	-0.006	-0.002
	(-1.504)	(-1.553)	(-1.381)	(-0.344)	(-1.540)	(-0.456)
Leverage	-0.031**	-0.034**	-0.035**	-0.031*	-0.030*	-0.032**
	(-2.001)	(-2.215)	(-2.214)	(-1.897)	(-1.951)	(-1.968)
Stock Return Volatility	0.001	0.001	0.003	0.002	0.002	0.004
	(0.068)	(0.080)	(0.277)	(0.183)	(0.149)	(0.297)
Capex	-0.017	-0.021	-0.042	-0.047	-0.014	-0.050
	(-0.395)	(-0.485)	(-0.968)	(-0.973)	(-0.326)	(-1.042)
R&D	-0.049	-0.056	-0.041	-0.071*	-0.050	-0.070*
	(-1.279)	(-1.411)	(-1.055)	(-1.804)	(-1.306)	(-1.779)
R&D is Missing	0.005	0.006	0.008	0.004	0.004	0.005
	(0.555)	(0.592)	(0.834)	(0.353)	(0.456)	(0.419)

(*Table 1.2 continued*)

Industry-Adjusted ROA	0.068***	0.064***	0.069***	0.055**	0.069***	0.056**
	(2.942)	(2.748)	(2.919)	(2.151)	(2.964)	(2.205)
CEO is Blockholder	-0.049***	-0.047***	-0.037***	-0.041***	-0.048***	-0.041***
	(-4.418)	(-4.166)	(-3.003)	(-3.322)	(-4.332)	(-3.251)
Constant	0.152***	0.143***	0.142***	0.113***	0.155***	0.107***
	(4.215)	(3.883)	(3.715)	(2.746)	(4.270)	(2.587)
Firm and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9,242	9,181	8,923	8,147	9,242	8,129
Adjusted R-squared	0.397	0.398	0.400	0.409	0.398	0.410

Table 1.3 Exchange Listing Regulations and Executive Pay

This table presents the estimates of the effect of exchange listing regulations on the components of CPI. The specifications are a replication of Column (6) of Table 1.2 with the components of CPI as the dependent variables. In Column (1), the dependent variable is Log (CEO Pay). In Column (2), the dependent variable is Log (Non-CEO Pay). The variable definitions are given in Table 1.A.1 in the appendix. We winsorize all continuous variables at 1 % level and use one-year lagged values of time-varying continuous independent control variables. All specifications include firm and year fixed effects. t-values are reported in the parenthesis. Robust standard errors are clustered at firm-period level. ***, **, and * indicate significance at 1%, 5%, and 10% levels, respectively.

	Log (CEO Pay)	**Log (Non-CEO Pay)**
Variables	**(1)**	**(2)**
Treatment*Post*Aggregate CEO Power	-0.102**	0.020
	(-2.192)	(0.574)
Treatment*Post	0.194**	-0.045
	(2.040)	(-0.640)
Treatment*Aggregate CEO Power	-0.043	-0.028
	(-0.932)	(-0.829)
Post*Aggregate CEO Power	-0.007	0.007
	(-0.305)	(0.416)
Post	0.126**	0.056
	(2.370)	(1.373)
Aggregate CEO Power	0.084***	0.070***
	(3.959)	(4.101)
Industry Median CPI	2.877***	0.327
	(9.006)	(1.487)
Number of VPs	0.032***	-0.049***
	(2.715)	(-5.825)
CEO is Only Director	0.020	-0.114***
	(0.916)	(-6.567)
Sales	0.159***	0.166***
	(4.897)	(6.716)
Leverage	-0.519***	-0.382***
	(-5.103)	(-4.966)
Stock Return Volatility	0.189**	0.247***
	(2.126)	(3.787)
Capex	-0.634*	-0.505**
	(-1.936)	(-2.254)
R&D	-0.183	0.058
	(-0.545)	(0.238)
R&D is Missing	-0.041	-0.037
	(-0.631)	(-0.815)
Industry-Adjusted ROA	0.705***	0.522***
	(4.271)	(4.470)
CEO is Blockholder	-0.324***	-0.080
	(-3.907)	(-1.499)
Constant	5.534***	7.221***
	(19.513)	(34.191)
Firm and Year Fixed Effects	Yes	Yes
Observations	8,129	8,129
Adjusted R-squared	0.698	0.765

Table 1.4 Exchange Listing Regulations, Excess Pay, and Firm Value

This table presents the estimates of the effect of exchange listing regulations on firm value. The dependent variable is Industry-Adjusted Tobin's Q. Financial and regulated industries are excluded. Column (1) does not incorporate the effect of excess pay. Column (2) and (3) condition the increase in firm value to excess pay. Column (2) uses Excess CPI, which is established using the residuals of the specification in Column (6) of Table 1.2. Column (3) uses Excess CEO Pay, which is established using the residuals of the specification in Column (1) of Table 1.3. The variable definitions are given in Table 1.A.1 in the appendix. We winsorize all continuous variables at 1 % level and use one-year lagged values of time-varying continuous independent control variables. All specifications include firm and year fixed effects. t-values are reported in the parenthesis. Robust standard errors are clustered at firm-period level. ***, **, and * indicate significance at 1%, 5%, and 10% levels, respectively.

	Base Specification	**Excess CPI**	**Excess CEO Pay**
Variables	**(1)**	**(2)**	**(3)**
Treatment*Post*Excess Pay		2.112**	0.459***
		(2.484)	(3.427)
Treatment*Post	0.159***	0.209***	0.199***
	(2.914)	(3.532)	(3.356)
Post*Excess Pay		-0.377	-0.097
		(-0.991)	(-1.463)
Post	0.159***	0.171***	0.169***
	(3.381)	(3.294)	(3.281)
Size	-0.881***	-0.907***	-0.906***
	(-16.017)	(-14.642)	(-14.722)
Leverage	-0.062	0.029	0.029
	(-0.373)	(0.160)	(0.164)
Stock Return Volatility	0.025	0.046	0.036
	(0.214)	(0.359)	(0.278)
Capex	1.090**	0.361	0.367
	(2.118)	(0.695)	(0.709)
Cash	1.125***	1.257***	1.266***
	(4.437)	(4.390)	(4.423)
R&D	-0.731	-0.441	-0.436
	(-1.515)	(-0.755)	(-0.748)
R&D is Missing	-0.109	-0.056	-0.045
	(-1.355)	(-0.688)	(-0.550)
Industry-Adjusted ROA	1.841***	1.921***	1.924***
	(6.997)	(6.797)	(6.837)
Insider Ownership	0.493	0.220	0.240
	(1.010)	(0.399)	(0.435)
Insider Ownership Squared	-0.298	0.292	0.224
	(-0.362)	(0.304)	(0.232)
CEO is Blockholder	0.184**	0.196*	0.195*
	(2.062)	(1.662)	(1.666)
Constant	-0.881***	-0.907***	-0.906***
	(-16.017)	(-14.642)	(-14.722)
Firm and Year Fixed Effects	Yes	Yes	Yes
Observations	9,125	7,230	7,230
Adjusted R-squared	0.623	0.641	0.641

Table 1.5 Exchange Listing Regulations, Excess CPI, and Firm Value with Different Variable Definitions

This table presents the estimates of the effect of exchange listing regulations on firm value using different firm value and excess pay definitions. In Column (1), the dependent variable is Industry-Adjusted Tobin's Q. In Column (2) and (3), the dependent variable is Log (Industry-Adjusted Tobin's Q). Financial and regulated industries are excluded. Column (1) and (3) use the binary version of Excess CPI while Column (2) uses continuous version of Excess CPI. The binary version takes a value of 1 if the Excess CPI is greater than or equal to sample median and 0 otherwise. The variable definitions are given in Table A.1 in the appendix. We winsorize all continuous variables at 1 % level and use one-year lagged values of time-varying continuous independent control variables. All specifications include firm and year fixed effects. The specifications also include associated control variables and are not reported in the table for brevity. t-values are reported in the parenthesis. Robust standard errors are clustered at firm-period level. ***, **, and * indicate significance at 1%, 5%, and 10% levels, respectively.

	Tobin's Q	**Log (Tobin's Q)**	
	Binary Excess CPI	**Excess CPI**	**Binary Excess CPI**
Variables	**(1)**	**(2)**	**(3)**
Treatment*Post*Excess CPI	0.274**	0.154**	0.019**
	(2.336)	(2.278)	(2.005)
Treatment*Post	0.075	0.017***	0.008
	(0.936)	(3.640)	(1.264)
Post*Excess CPI	-0.102*	-0.031	-0.009*
	(-1.793)	(-1.026)	(-1.895)
Post	0.218***	0.010**	0.014***
	(3.660)	(2.357)	(2.907)
Tobin's Q Controls	Yes	Yes	Yes
Firm and Year Fixed Effects	Yes	Yes	Yes
Observations	7,230	7,230	7,230
Adjusted R-squared	0.640	0.666	0.666

Table 1.6 Alternative Measures of Aggregate CEO Power, CPI, and Firm Value
This table presents the estimates of exchange listing regulations on CEO pay inequality and firm value using alternative Aggregate CEO Power measures. Panel A presents the results for CEO pay inequality where dependent variable is CPI and Panel B shows the results for firm value where the dependent variable is Industry-Adjusted Tobin's Q, and where financial and regulated industries are excluded. Column (1) uses the first principal component of four CEO power measures as the Aggregate CEO Power measure. Column (2) uses an additive measure of Aggregate CEO Power where each of the four components is transformed into a binary variable. If the CEO power measure (except CEO is Chair) is greater than or equal to the sample median than the binary variable takes a value of 1 and 0 otherwise. These values are added to construct the 0-1 Additive measure. The variable definitions are given in Table 1.A.1 in the appendix. We winsorize all continuous variables at 1 % level and use one-year lagged values of time-varying continuous independent control variables. All specifications include firm and year fixed effects. The specifications also include associated control variables and are not reported in the table for brevity. t-values are reported in the parenthesis. Robust standard errors are clustered at firm-period level. ***, **, and * indicate significance at 1%, 5%, and 10% levels, respectively.

Panel A: CEO Pay Inequality

	First Principal Component	**0-1 Additive**
Variables	**(1)**	**(2)**
Treatment*Post*CEO Power	-0.011**	-0.015***
	(-2.402)	(-2.887)
Treatment*Post	0.007	0.038***
	(1.128)	(2.853)
Treatment*CEO Power	-0.001	0.002
	(-0.145)	(0.399)
Post*CEO Power	-0.002	-0.003
	(-0.897)	(-1.341)
Post	0.014**	0.020***
	(2.446)	(2.734)
CEO Power	0.003	0.002
	(1.632)	(0.997)
CPI Controls	Yes	Yes
Firm and Year Fixed Effects	Yes	Yes
Observations	8,129	8,129
Adjusted R-squared	0.409	0.410

Panel B: Tobin's Q

	First Principal Component	**0-1 Additive**
Variables	**(1)**	**(2)**
Treatment*Post*Excess CPI	2.093**	2.068**
	(2.465)	(2.413)
Treatment*Post	0.210***	0.210***
	(3.546)	(3.537)
Post*Excess CPI	-0.383	-0.393
	(-1.007)	(-1.034)
Post	0.171***	0.171***
	(3.292)	(3.289)
Tobin's Q Controls	Yes	Yes
Firm and Year Fixed Effects	Yes	Yes
Observations	7,230	7,230
Adjusted R-squared	0.641	0.641

Table 1.7 Exchange Listing Regulations, CEO Pay Inequality, and Firm Value with Differentiated Monitoring Intensity

This table presents estimates of the effect of exchange listing regulations on firm value with different monitoring intensity. The dependent variable is Industry-Adjusted Tobin's Q. Financial and regulated industries are excluded. Each pair of columns splits the sample into two (low and high) based on the 2002 median value of the corresponding variable. Low correspond to low monitoring intensity and high corresponds to high monitoring intensity. The variable definitions are given in Table 1.A.1 in the appendix. We winsorize all continuous variables at 1 % level and use one-year lagged values of time-varying continuous independent variables. t-values are reported in the parenthesis. Robust standard errors are clustered at firm-period level. ***, **, and * indicate significance at 1%, 5%, and 10% levels, respectively.

	Non-Busy Board		**Director Ownership**		**Institutional Ownership**		**Short-Term Debt**	
	Low	**High**	**Low**	**High**	**Low**	**High**	**Low**	**High**
Variables	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Treatment*Post*Excess CPI	4.834***	0.816	2.285**	1.622	3.869***	0.565	2.568**	1.118
	(3.578)	(0.983)	(2.046)	(1.252)	(2.856)	(0.701)	(1.965)	(0.846)
Treatment*Post	0.382***	0.079	0.218***	0.176**	0.343***	0.118*	0.199**	0.163*
	(3.992)	(1.140)	(2.640)	(2.063)	(3.556)	(1.805)	(2.329)	(1.662)
Post*Excess CPI	-0.149	-1.411**	-0.455	-0.047	-0.357	-0.475	-0.257	-0.094
	(-0.323)	(-2.432)	(-0.906)	(-0.080)	(-0.559)	(-1.085)	(-0.596)	(-0.133)
Post	0.219***	0.107	0.134*	0.241***	0.149*	0.200***	0.179**	0.037
	(3.106)	(1.378)	(1.955)	(3.005)	(1.874)	(2.930)	(2.537)	(0.473)
Size	-1.077***	-0.727***	-0.974***	-0.853***	-1.161***	-0.782***	-0.785***	-0.893***
	(-11.967)	(-9.542)	(-9.941)	(-10.701)	(-11.606)	(-10.095)	(-9.186)	(-8.986)
Leverage	-0.214	0.236	-0.330	0.299	0.011	0.055	-0.037	-0.092
	(-0.914)	(0.906)	(-1.287)	(1.256)	(0.036)	(0.265)	(-0.160)	(-0.310)
Stock Return Volatility	0.208	-0.155	0.149	-0.011	-0.066	0.275	0.199	-0.352*
	(1.149)	(-0.874)	(0.768)	(-0.064)	(-0.345)	(1.638)	(1.024)	(-1.804)
Capex	-0.649	0.945	0.350	0.326	0.584	-0.084	0.617	0.839
	(-0.762)	(1.528)	(0.532)	(0.427)	(0.652)	(-0.146)	(1.084)	(0.877)
Cash	1.347***	1.180***	0.984**	1.424***	1.788***	0.780**	0.116	1.853***
	(3.014)	(3.225)	(2.554)	(3.556)	(3.614)	(2.367)	(0.265)	(4.494)
R&D	0.128	-1.219	-1.319	-0.075	-1.155	0.531	0.398	-1.182
	(0.163)	(-1.386)	(-1.344)	(-0.103)	(-1.631)	(0.603)	(0.471)	(-1.224)
R&D is Missing	-0.064	-0.010	0.104	-0.213*	-0.374***	0.089	-0.036	-0.230**
	(-0.652)	(-0.076)	(0.964)	(-1.805)	(-2.858)	(0.883)	(-0.350)	(-2.405)
Industry-Adjusted ROA	1.776***	2.051***	1.663***	2.131***	2.341***	1.618***	1.962***	1.859***
	(4.448)	(5.370)	(3.616)	(6.005)	(5.984)	(3.985)	(4.462)	(4.352)
Insider Ownership	-0.097	0.463	0.194	0.128	-0.259	0.728	-0.370	1.896**
	(-0.107)	(0.747)	(0.242)	(0.169)	(-0.296)	(1.069)	(-0.507)	(2.240)
Insider Ownership Squared	0.531	0.163	0.693	0.249	1.002	-0.523	1.311	-2.778**
	(0.339)	(0.149)	(0.453)	(0.202)	(0.714)	(-0.388)	(0.972)	(-2.078)

(*Table 1.7 continued*)

CEO is Blockholder	0.385**	0.077	-0.002	0.358*	0.303*	-0.006	0.240	0.123
	(2.113)	(0.561)	(-0.014)	(1.847)	(1.958)	(-0.040)	(1.062)	(0.669)
Constant	8.373***	4.883***	7.891***	5.529***	8.655***	5.531***	5.712***	6.902***
	(11.778)	(9.095)	(9.925)	(10.088)	(11.511)	(9.627)	(8.865)	(9.056)
Firm and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,029	3,201	3,678	3,518	3,036	4,194	3,598	2,676
Adjusted R-squared	0.650	0.639	0.675	0.609	0.612	0.675	0.625	0.632

Table 1.8 Time Trend Analysis and Placebo Tests

This table presents the estimates of exchange listing regulations on CEO pay inequality and firm value using time trend analysis and placebo treatment effects. Panel A presents the results for time trend analysis. Panel B and C present the results for placebo treatment effects for Year 2000 and Year 2004, respectively. Column (1) uses CPI as the dependent variable and Column (2) uses Industry-Adjusted Tobin's Q as the dependent variable where financial and regulated industries are excluded. In Panel A, variable Post is replaced by year dummies for each year and year 2002, which is chosen to be the base year, observations are dropped in order to avoid multicollinearity and to make the estimates comparable to year 2002. Panel B assumes that exchange listing regulations become effective in year 2000, the treatment is based on board independence as of year 2000, and the time window is 6 years (1998-2003). Panel C assumes that exchange listing regulations become effective in year 2004, the treatment is based on random assignment, and the time window is 6 years (2002-2007). The variable definitions are given in Table 1.A.1 in the appendix. We winsorize all continuous variables at 1 % level and use one-year lagged values of time-varying continuous independent control variables, except CEO power variables and Industry Median CPI. All specifications include firm and year fixed effects. The specifications also include associated control variables and are not reported in the table for brevity. t-values are reported in the parenthesis Robust standard errors are clustered at firm-period level. ***, **, and * indicate significance at 1%, 5%, and 10% levels, respectively.

Panel A: Time Trend Analysis

	CEO Pay Inequality		**Tobin's Q**
Variables	**(1)**	**Variables**	**(2)**
Treatment*Aggregate CEO Power*Year 1998	0.001	Treatment*Excess CPI*Year 1998	-1.264
	(0.023)		(-0.563)
Treatment*Aggregate CEO Power*Year 1999	0.012	Treatment*Excess CPI*Year 1999	-1.701
	(0.652)		(-0.898)
Treatment*Aggregate CEO Power*Year 2000	0.022	Treatment*Excess CPI*Year 2000	1.217
	(1.192)		(0.793)
Treatment*Aggregate CEO Power*Year 2001	-0.023	Treatment*Excess CPI*Year 2001	0.979
	(-1.622)		(0.983)
Treatment*Aggregate CEO Power*Year 2003	-0.027**	Treatment*Excess CPI*Year 2003	-0.316
	(-2.061)		(-0.294)
Treatment*Aggregate CEO Power*Year 2004	-0.019	Treatment*Excess CPI*Year 2004	1.697*
	(-1.327)		(1.875)
Treatment*Aggregate CEO Power*Year 2005	-0.021	Treatment*Excess CPI*Year 2005	3.270***
	(-1.378)		(2.888)
Treatment*Aggregate CEO Power*Year 2006	-0.024	Treatment*Excess CPI*Year 2006	2.521**
	(-1.529)		(2.334)
Treatment*Aggregate CEO Power*Year 2007	-0.016	Treatment*Excess CPI*Year 2007	3.557**
	(-0.922)		(2.481)
CPI Controls	Yes	Tobin's Q Controls	Yes
Firm and Year Fixed Effects	Yes	Firm and Year Fixed Effects	Yes
Observations	8,129	Observations	7,230
Adjusted R-squared	0.411	Adjusted R-squared	0.641

(*Table 1.8 continued*)

Panel B: Placebo Test Year 2000

	CEO Pay Inequality		Tobin's Q
Variables	**(1)**	**Variables**	**(2)**
Treatment*Post*Aggregate CEO Power	-0.018	Treatment*Post*Excess CPI	0.001
	(-1.458)		(0.001)
Treatment*Post	0.032	Treatment*Post	0.127
	(1.251)		(1.428)
Treatment*Aggregate CEO Power	0.001	Post*Excess CPI	0.987*
	(0.072)		(1.688)
Post*Aggregate CEO Power	-0.006	Post	-0.361***
	(-1.068)		(-6.151)
Post	0.012		
	(1.023)		
Aggregate CEO Power	0.005		
	(0.935)		
CPI Controls	Yes	Tobin's Q Controls	Yes
Firm and Year Fixed Effects	Yes	Firm and Year Fixed Effects	Yes
Observations	4,422	Observations	3,562
Adjusted R-squared	0.439	Adjusted R-squared	0.689

(*Table 1.8 continued*)

Panel C: Placebo Test Year 2004

	CEO Pay Inequality		Tobin's Q
Variables	**(1)**	**Variables**	**(2)**
Treatment*Post*Aggregate CEO Power	-0.001	Treatment*Post*Excess CPI	0.259
	(-0.066)		(0.341)
Treatment*Post	0.003	Treatment*Post	-0.004
	(0.205)		(-0.105)
Treatment*Aggregate CEO Power	0.005	Post*Excess CPI	-0.729
	(0.593)		(-1.201)
Post*Aggregate CEO Power	-0.008	Post	0.100**
	(-1.496)		(2.216)
Post	0.025*		
	(1.960)		
Aggregate CEO Power	0.008		
	(1.310)		
CPI Controls	Yes	Tobin's Q Controls	Yes
Firm and Year Fixed Effects	Yes	Firm and Year Fixed Effects	Yes
Observations	5,182	Observations	4,265
Adjusted R-squared	0.466	Adjusted R-squared	0.777

Table 1.9 Degree of Non-Compliance, Compensation Committee Regulation, and CEO Pay Inequality
This table presents the estimates of the effect of exchange listing regulations on CEO pay inequality using degree of non-compliance and compensation committee compliance for treatment. The dependent variable is CPI. In Column (1), treatment is based on the degree of board non-compliance. Specifically, treatment = (0.51 – board independence). In Column (2), treatment is based on compensation committee compliance. In Column (3), treatment is based on the degree of compensation committee non-compliance. Specifically, treatment = (1 – compensation committee independence). The variable definitions are given in Table 1.A.1 in the appendix. We winsorize all continuous variables at 1 % level and use one-year lagged values of time-varying continuous independent control variables. All specifications include firm and year fixed effects. t-values are reported in the parenthesis. Robust standard errors are clustered at firm-period level. ***, **, and * indicate significance at 1%, 5%, and 10% levels, respectively.

	Degree of Board Non-Compliance	**Compensation Committee Non-Compliance**	**Degree of Compensation Committee Non-Compliance**
Variables	**(1)**	**(2)**	**(3)**
Treatment*Post*Aggregate CEO Power	-0.126**	-0.023***	-0.068***
	(-2.333)	(-3.221)	(-4.110)
Treatment*Post	0.294***	0.046***	0.124***
	(2.881)	(3.567)	(3.969)
Treatment*Aggregate CEO Power	0.007**	-0.002	-0.000
	(2.129)	(-0.302)	(-0.023)
Post*Aggregate CEO Power	-0.023	-0.004	-0.003
	(-0.395)	(-1.033)	(-0.821)
Post	0.025***	0.020**	0.019**
	(3.110)	(2.301)	(2.275)
Aggregate CEO Power	0.007**	0.007*	0.006*
	(2.129)	(1.956)	(1.823)
Industry Median CPI	0.595***	0.588***	0.588***
	(11.056)	(10.861)	(10.895)
Number of VPs	0.016***	0.016***	0.016***
	(8.375)	(8.293)	(8.313)
CEO is Only Director	0.027***	0.028***	0.028***
	(7.565)	(7.649)	(7.657)
Sales	-0.002	-0.002	-0.002
	(-0.483)	(-0.389)	(-0.345)
Leverage	-0.031*	-0.033**	-0.032**
	(-1.902)	(-2.025)	(-1.988)
Stock Return Volatility	0.004	0.004	0.004
	(0.341)	(0.311)	(0.318)
Capex	-0.044	-0.045	-0.049
	(-0.923)	(-0.924)	(-1.017)
R&D	-0.073*	-0.076**	-0.074*
	(-1.855)	(-1.967)	(-1.892)
R&D is Missing	0.004	0.004	0.005
	(0.369)	(0.409)	(0.439)
Industry-Adjusted ROA	0.057**	0.052**	0.053**
	(2.247)	(2.031)	(2.093)
CEO is Blockholder	-0.038***	-0.044***	-0.043***
	(-3.084)	(-3.641)	(-3.569)
Constant	0.105**	0.107***	0.105**
	(2.550)	(2.587)	(2.546)
Firm and Year Fixed Effects	Yes	Yes	Yes
Observations	8,129	8,129	8,129
Adjusted R-squared	0.410	0.410	0.411

Table 1.10 Degree of Non-Compliance, Compensation Committee Regulation, Excess CPI, and Firm Value

This table presents the estimates of the effect of exchange listing regulations on firm value using degree of non-compliance and compensation committee compliance for treatment. The dependent variable is Industry-Adjusted Tobin's Q. Financial and regulated industries are excluded. In Column (1), treatment is based on the degree of board non-compliance. Specifically, treatment = (0.51 – board independence). In Column (2), treatment is based on compensation committee compliance. In Column (3), treatment is based on the degree of compensation committee non-compliance. Specifically, treatment = (1 – compensation committee independence). The variable definitions are given in Table 1.A.1 in the appendix. We winsorize all continuous variables at 1 % level and use one-year lagged values of time-varying continuous independent control variables. t-values are reported in the parenthesis. Robust standard errors are clustered at firm-period level. ***, **, and * indicate significance at 1%, 5%, and 10% levels, respectively.

	Degree of Board Non-Compliance		**Compensation Committee Compliance**		**Degree of Compensation Committee Non-Compliance**	
Variables	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Treatment*Post*Excess CPI		12.037**		-0.796		-1.328
		(2.283)		(-1.043)		(-0.651)
Treatment*Post	0.863**	1.174***	0.171***	0.222***	0.312**	0.463***
	(2.026)	(3.010)	(3.467)	(4.249)	(2.439)	(3.689)
Post*Excess CPI		-0.173		0.182		0.145
		(-0.477)		(0.454)		(0.367)
Post	0.175***	0.190***	0.147***	0.154***	0.159***	0.163***
	(3.748)	(3.688)	(3.097)	(3.015)	(3.365)	(3.181)
Size	-0.878***	-0.898***	-0.875***	-0.890***	-0.877***	-0.890***
	(-15.979)	(-14.544)	(-16.017)	(-14.622)	(-16.008)	(-14.584)
Leverage	-0.070	0.002	-0.076	-0.000	-0.073	0.006
	(-0.423)	(0.011)	(-0.459)	(-0.001)	(-0.442)	(0.035)
Stock Return Volatility	0.025	0.045	0.022	0.044	0.020	0.039
	(0.212)	(0.353)	(0.185)	(0.341)	(0.173)	(0.300)
Capex	1.083**	0.343	1.102**	0.410	1.108**	0.430
	(2.098)	(0.654)	(2.139)	(0.780)	(2.147)	(0.817)
Cash	1.115***	1.230***	1.130***	1.257***	1.126***	1.250***
	(4.399)	(4.289)	(4.452)	(4.379)	(4.430)	(4.352)
R&D	-0.733	-0.456	-0.730	-0.468	-0.730	-0.465
	(-1.515)	(-0.778)	(-1.513)	(-0.799)	(-1.512)	(-0.793)
R&D is Missing	-0.111	-0.054	-0.105	-0.051	-0.108	-0.056
	(-1.366)	(-0.654)	(-1.303)	(-0.632)	(-1.338)	(-0.684)
Industry-Adjusted ROA	1.835***	1.918***	1.822***	1.878***	1.825***	1.880***
	(6.974)	(6.785)	(6.927)	(6.635)	(6.925)	(6.637)
Insider Ownership	0.482	0.231	0.482	0.249	0.464	0.237
	(0.988)	(0.420)	(0.989)	(0.453)	(0.951)	(0.431)
Insider Ownership Squared	-0.280	0.321	-0.343	0.158	-0.316	0.163
	(-0.339)	(0.336)	(-0.417)	(0.166)	(-0.383)	(0.171)

(*Table 1.10 continued*)

CEO is Blockholder	0.171*	0.184	0.156*	0.160	0.158*	0.161
	(1.927)	(1.564)	(1.761)	(1.363)	(1.789)	(1.368)
Constant	6.383***	6.550***	6.364***	6.495***	6.374***	6.498***
	(15.510)	(14.161)	(15.544)	(14.229)	(15.538)	(14.201)
Firm and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9,125	7,230	9,125	7,230	9,125	7,230
Adjusted R-squared	0.623	0.640	0.623	0.640	0.623	0.640

Table 1.11 Board Independence, Compensation Committee Regulation, CEO Pay Inequality, and Firm Value

This table presents the estimates of the effect of exchange listing regulations on CEO pay inequality and firm value, using both board independence and compensation committee requirements in the same regression. Column (1) uses CPI as the dependent variable and Column (2) uses Industry-Adjusted Tobin's Q as the dependent variable where financial and regulated industries are excluded. Treatment (Board) is established using board independence requirement and Treatment (Comp) is established using compensation committee requirement. Column (2) uses Excess CPI, which is established using the residuals of the specification in Column (1). The variable definitions are given in Table 1.A.1 in the appendix. We winsorize all continuous variables at 1 % level and use one-year lagged values of time-varying continuous independent control variables. All specifications include firm and year fixed effects. The specifications also include associated control variables and are not reported in the table for brevity. t-values are reported in the parenthesis. Robust standard errors are clustered at firm-period level. ***, **, and * indicate significance at 1%, 5%, and 10% levels, respectively.

	CEO Pay Inequality		Tobin's Q
Variables	**(1)**	**Variables**	**(2)**
Treatment(Board)*Post*Aggregate*CEO Power	-0.015*	Treatment (Board)*Post*Excess CPI	2.497***
	(-1.774)		(2.612)
Treatment (Comp)*Post*Aggregate*CEO Power	-0.034***	Treatment (Comp)* Post*Excess CPI	-1.893
	(-2.970)		(-1.288)
Treatment (Board)*Post	0.031**	Treatment (Board)*Post	0.201***
	(2.005)		(3.095)
Treatment (Comp)*Post	0.052**	Treatment (Comp)*Post	0.044
	(2.323)		(0.419)
Treatment (Board)*Aggregate CEO Power	-0.002	Post*Excess CPI	-0.300
	(-0.277)		(-0.817)
Treatment (Comp)*Post*Aggregate CEO Power	0.005	Post	0.170***
	(0.419)		(3.282)
Post*Aggregate CEO Power	-0.003		
	(-0.879)		
Post	0.019**		
	(2.302)		
Aggregate CEO Power	0.006*		
	(1.697)		
CPI Controls	Yes	Tobin's Q Controls	Yes
Firm and Year Fixed Effect	Yes	Firm and Year Fixed Effects	Yes
Observations	8,129	Observations	7,230
Adjusted R-squared	0.411	Adjusted R-squared	0.641

Table 1.12 Matched Sample, CEO Pay Inequality, CEO Pay, and Firm Value

This table presents the estimates of exchange listing regulations on CEO pay inequality, CEO Pay, and firm value using matched sample. Matching is done using a nearest neighbor propensity score matching with Mahalanobis distance and in year 2002 using lagged CPI, Log (CEO Pay), Size, and Industry-Adjusted ROA as the matching variables. The matching procedure is carried out for each 2-digit SIC industry. If matching is not possible for a particular industry, then whole sample is used for matching. In Panel A, Column (1) uses CPI as the dependent variable and Column (2) uses Log (CEO Pay) as the dependent variable. In Panel B, the dependent variable is Industry-Adjusted Tobin's Q. Financial and regulated industries are excluded. Column (1) of Panel B uses Excess CPI as the conditioning variable whereas Column (2) of Panel B uses Excess CEO Pay as the conditioning variable. The variable definitions are given in Table 1.A.1 in the appendix. We winsorize all continuous variables at 1 % level and use one-year lagged values of time-varying continuous independent control variables. All specifications include firm and year fixed effects. The specifications also include associated control variables and are not reported in the table for brevity. t-values are reported in the parenthesis. Robust standard errors are clustered at firm-period level. ***, **, and * indicate significance at 1%, 5%, and 10% levels, respectively.

Panel A: CEO Pay Inequality and CEO Pay

	CEO Pay Inequality	**Log (CEO Pay)**
Variables	**(1)**	**(2)**
Treatment*Post*Aggregate CEO Power	-0.025**	-0.081
	(-2.348)	(-1.259)
Treatment*Post	0.039**	0.045
	(1.976)	(0.347)
Treatment*Aggregate CEO Power	0.002	-0.070
	(0.153)	(-1.125)
Post*Aggregate CEO Power	0.002	0.003
	(0.316)	(0.064)
Post	0.017	0.209*
	(1.010)	(1.833)
Aggregate CEO Power	0.001	0.076*
	(0.178)	(1.870)
CPI Controls	Yes	Yes
Firm and Year Fixed Effects	Yes	Yes
Observations	2,592	2,592
Adjusted R-squared	0.408	0.671

Panel B: Tobin's Q

	Excess CPI	**Excess CEO Pay**
Variables	**(1)**	**(2)**
Treatment*Post*Excess Pay	3.573***	0.665***
	(3.687)	(4.399)
Treatment*Post	-0.045	-0.048
	(-0.698)	(-0.750)
Post*Excess Pay	-1.527***	-0.303***
	(-3.142)	(-3.445)
Post	0.286***	0.280***
	(3.521)	(3.504)
Tobin's Q Controls	Yes	Yes
Firm and Year Fixed Effects	Yes	Yes
Observations	2,388	2,388
Adjusted R-squared	0.614	0.616

Chapter 2

Investor Horizon and Managerial Short-Termism

2.1 Introduction

Although shareholders share the same objective of wealth maximization, they can differ in style and constraints in achieving this objective. For instance, mutual fund flows force the fund managers to execute liquidity motivated transactions (Edelen (1999)), while some hedge funds impose lock-up periods for their investors (Aragon (2007)). High-frequency traders become a shareholder for seconds, while pension funds may keep their shares in firms for decades. (Del Guercio and Hawkins, 1999). Whether it is due to style or constraint, shareholder horizon (i.e., investor horizon) is a distinguishing feature of shareholders.

If every action of the manager is observable, financial markets cannot be fooled by myopic corporate actions, which aim to increase the stock price in the short-run but hurt the fundamental value of the firm in the long-run (Stein (1988)). Therefore, shareholder horizon does not matter for corporate policies in a perfect capital market with no information asymmetry (Derrien, Kecskes, Thesmar (2013)). However, if market has incomplete information about the firm, optimal corporate policies depend on the shareholder horizon. For instance, short-term shareholder may desire R&D expenditures to be reduced (Bushee, (1998)) or necessary maintenance to be deferred (Peng and Roell (2014)) to increase short-term earnings. Supporting this view, survey evidence shows that managers forgo profitable long-term projects to meet short-term targets (Graham, Harvey, and Rajgopal (2005)). However, from long-term shareholder's point of view, reducing R&D expenditures or skipping necessary maintenance may be equivalent to forgoing positive net present value projects with distant cash flows. Suppose the market cannot assess the true value of the foregone project due to information asymmetry and overvalues the firm. Short-term shareholders exit while the firm is still overvalued. However, when the truth about the project is revealed, long-term shareholders' wealth is reduced. Therefore, there is a conflict between shareholders with different horizons.

In a world with information asymmetry, shareholders can use their control rights to ensure that corporate policies are in line with their horizon incentives. Evidence shows that corporate decisions are influenced by investor horizon. For instance, undervalued firms with higher short-term investor ownership decrease capital expenditures since investment decisions are not fully reflected in the stock price (Derrien, Kecskes, and Thesmar (2013)). Repurchases are preferred to dividends as short-term investor ownership increases. (Gaspar, Massa, Matos, Patgiri, and Rehman (2012)). Another example is that firms with higher long-term institutional ownership invest more in R&D and improve innovation (Harford, Kecskes, and Mansi (2016)).

However, there is little evidence on the channel through which shareholders ensure corporate policies that are aligned with their horizon. Although a possible channel is direct monitoring, it is costly and limited when the firm has a diffuse ownership structure, leading to free-rider problem (Grossman and Hart, (1980) and Shleifer and Vishny, (1986)). Another possible channel is executive compensation. There is evidence that institutional shareholders affect the compensation characteristics such as level and pay-for-performance sensitivity (Almazan, Hartzell, and Starks (2005)). Naturally, the main issue is not the level of compensation but how the managers are compensated (Jensen and Murphy (1990)) so that managerial incentives are aligned with shareholder horizon.

It has been notoriously difficult to quantify managerial short-termism (i.e., myopia). Some studies argue that executive compensation is too focused on short-term performance, leading to excessive risk taking, and compensation contracts should be based on the long-term fundamental value of the firm even beyond the retirement of the manager (Bebchuk and Fried (2010) and Bhagat and Romano (2010)). Consistent with this view, Dechow and Sloan (1991) document that CEOs cut profitable R&D investment to boost earnings before their retirement. On the other hand, other studies argue that there are market frictions and there is an optimal mix of short-term and long-term pay for managers (Bolton, Scheinkman, Xiong (2006), Laux (2012), Peng and Roell (2014), and Marinovic and Varas (2016)). However, this discussion is mostly based on theoretical models and with limited empirical evidence, partly because of the absence of a good proxy for managerial horizon. Recently, Gopalan, Milbourn, Song, and Thakor (2014) introduced pay duration as a measure of managerial horizon. We use their measure in our study to examine how institutional investors as sophisticated shareholders affect managerial horizon.

CEO compensation can be used as a governance mechanism that can align the horizons of investors and managers. Previous literature has two opposing predictions on the relation between shareholder horizon and managerial horizon through compensation contracts. In the first prediction, existence of short-term investors provides implicit incentives to managers and thus shorten the managerial horizon. To counter these implicit short-term incentives, contract designers provide pay contracts that are focused more on the stock price rather than current earnings (Dikolli, Kulp, and Sedatole (2009)). In the second prediction, short-term shareholders induce short horizon pay contracts for the CEO so that they exit the firm at a higher valuation. For instance, venture capitalists, who have relatively short horizon and control rights, shorten CEO horizon through compensation contracts and exit the firm just after the IPO when the valuation is relatively higher. (Cadman and Sunder (2014)). We conjecture that one way for shareholders to align their horizon incentives with managerial horizon incentives is to use their control rights and influence CEO compensation contracts. We use pay duration of the CEO as a proxy for managerial horizon. To measure shareholder horizon, we use the ownership fraction of long-term institutional shareholders. We identify long-term institutional investors based on their portfolio turnover (e.g. Gaspar, Massa, and Matos (2005) and Derrien, Kecskes, and Thesmar (2013)). Using institutional investors to proxy for overall shareholder horizon is plausible since it is documented that institutional investors have monitoring roles and they are influential on compensation contracts. (Almazan, Hartzell, and Starks (2005)). Moreover, institutional investor ownership has increased to almost 60% among large U.S. firms (Harford, Kecskes, and Mansi (2017)).

To test our prediction, we use a panel data set of firm-year observations and regress pay duration on long-term institutional ownership. We find that pay duration is increasing in the fraction of long-term institutional ownership in the firm. This is consistent with the finding that investors with control rights align their horizon with the managers through compensation channel (Cadman and Sunder (2014)). We also document the robustness of this relation to different measures of investor horizon and pay duration. Moreover, we employ additional control variables such as board independence, deferred compensation, and unvested grants to show that our results are not driven by these factors.

Although we use a large set of lagged control variables and fixed effects in our ordinary least squares (OLS) estimation, our result is merely an association and may be subject to endogeneity. One such endogeneity stems from reverse causality. Since investors choose their portfolio composition, managers may cater to investors. In other words, long-term shareholders choose to invest in firms with high managerial pay duration. To address this, we repeat our tests by splitting long-term institutional ownership into two parts, indexers and non-indexers following Derrien et al. (2013). The motivation behind the approach is that indexer investors have less flexibility to choose their portfolio composition but they actively monitor the firm (Del Guercio and Hawkins (1999), Gillan and Starks (2000), Harford, Kecskes, and Mansi (2017), and Appel, Gormley, and Keim (2016)). Therefore, it is less likely that indexers select the firms whose managerial horizon is aligned with their investment horizon. We find that our results hold for both indexer and non-indexer long-term institutional ownership, suggesting that our main results are unlikely driven by reverse causality.

Our approach in addressing reverse causality alleviates the endogeneity concerns. However, there may still be omitted factors driving the positive relation between investor horizon and pay duration. For instance, any type of unobserved or uncontrolled corporate governance quality or managerial trait may lengthen the pay duration and attract long-term institutions at the same time. Or, some firm specific projects or characteristics affecting pay duration may also determine the type of shareholders investing in the firm. To further address these concerns, we exploit the institution mergers (He and Huang (2017)) and its plausibly exogenous effect on investor horizon. We conjecture that when two institutions merge, the acquirer takes control of the target's holdings and this change of control affects the aggregate investor horizon in the firm. We use this change as a continuous treatment effect and follow the methodology of Acemoglu, Autor, and Lyle (2004) to test its effect on pay duration. We find evidence that a merger deal where a relatively longer-term institution acquiring another institution invested in the firm increases the pay duration of the CEO, suggesting a causal link between investor horizon and pay duration. We further show that these results are robust to different investor horizon measures and a matched sample analysis.

Activist hedge funds causing myopia is extensively discussed in the literature but the results are mixed. While some studies document that hedge fund activism does not hurt long-term fundamental value (Brav, Jiang, Partnoy, and Thomas (2008), Bebchuk, Brav, and Jiang (2015)), others claim the opposite (Cremers, Giambona, Sepe, and Wang (2016), Coffee and Palia (2016)). Moreover, many economists, academics, lawyers, and judges argue the short-term orientation of hedge fund activism (Fried and Wang (2017)). To contribute to this discussion and to strengthen our results on the positive association between pay duration and investor horizon, we conduct additional tests using activist hedge funds. We design our empirical test very similar to a difference-in-difference (DiD) setup where treatment sample consists of firms that are subject to hedge fund activism and the control sample consists of firms that are not subject to hedge fund activism and matched to the treatment sample. We use nearest neighbor matching with Mahalanobis distance based on the variables: size, stock volatility, financial leverage, market-to-book ratio, market adjusted return and institutional ownership. The results of our tests reveal that hedge fund activism leads to a decrease in CEO pay duration. This finding is consistent with the prediction that investors affect compensation policies to align the manager's horizon with theirs.

Finally, we use an alternative institutional investor classification to further support our findings. Based on portfolio turnover, trading frequency, diversification, and expected investment horizon, Bushee (1998) classifies institutions into three groups, which are quasi-indexer, transient, and dedicated. Quasi-indexer institutions are very similar to long-term indexer institutions. Transient institutions are short-term investors and they are not likely to have any material effect on compensation due to their low ownership stakes. Dedicated institutions are typically long-term institutions and they have direct monitoring incentives as a result of their concentrated holdings. We use our base OLS setup to test the effect of these three groups on pay duration. While quasi-indexer ownership is positively related to pay duration of the CEO, we do not document any significant effect of transient institution ownership on pay duration. Interestingly, our results reveal that dedicated institution ownership is negatively associated with pay duration, suggesting that even though these institutions are long-term they may be using direct monitoring instead of the compensation channel since longer pay durations expose the managers to higher risk. Overall, the empirical results are consistent with our previous discussions.

Our study contributes to the debate on the importance of shareholder horizon. Several studies find that shareholder horizon is a determining factor in corporate policies and shareholders affect corporate policies in a way that is consistent with their investment horizon. However, the channels through which shareholders establish this link is not extensively discussed. We propose that shareholders design compensation contracts with horizon incentives to achieve this objective. We also contribute to a newly emerged angle in executive compensation, namely pay duration. Since pay duration is a fairly new concept in compensation studies, the determinants of pay duration are still an unexplored area. Our empirical results indicate that shareholder horizon or long-term institutional ownership is a determinant of pay duration. We also extend the activist hedge fund literature by documenting that activist hedge funds shorten the managerial horizon and they use compensation as a governance mechanism to achieve their objectives.

The rest of the paper is organized as follows. Section 2.2 discusses the data sources, how we construct pay duration measure, and investor horizon. In Section 2.3, we discuss our main empirical methodology. Section 2.4 presents the results of our empirical tests. Section 2.5 concludes.

2.2 Data, CEO Horizon, and Investor Horizon

2.2.1 Sample Construction

Our data on CEO compensation and characteristics come from ISS Incentive Lab and Execucomp. We draw institutional investor ownership information from Thomson Reuters database and institutional investor classification from Brian Bushee's website.[15] We retrieve information on firm's financial characteristics from Compustat. Stock returns and dividend distributions are from CRSP. We obtain daily yield curve rates from U.S. Department of Treasury website. We use ISS Governance database to determine independent board members. We use Compustat to retrieve index constituents.[16] We use Thomson Reuter's SDC Platinum Mergers & Acquisitions database to identify institutional investor mergers. Lastly, our hedge fund activism data is based on Gantchev and Jotikasthira (2017).[17]

The main data source for our study is Incentive Lab. This relatively recent database provides detailed grant level compensation information for the named executives of top 750 companies each year, starting from 1998. The whole database contains around 2,000 unique companies of which nearly 1,200 are active as of today.[18] We drop firm-year observations where we fail to match CRSP main company identifier (PERMCO and PERMNO) or Compustat main company identifier (GVKEY) to the Incentive Lab main company identifier (CIK).[19]

Following Gopalan et al. (2014), we ensure comparability of Execucomp and Incentive Lab databases. In order to do this, we match the CEO names in Incentive Lab to the names in Execucomp. For most observations, the Execucomp and Incentive Lab databases agree on whom the CEO is; however, there are some conflicting observations. We resolve them in the following way. The current CEO identification may differ in the two databases whenever there is a CEO

[15] See http://acct.wharton.upenn.edu/faculty/bushee/IIclass.html
[16] We thank Vijay Singal for providing us Russell index constituents data.
[17] We thank Chotibhak (Pab) Jotikasthira for providing us hedge fund activism data.
[18] Incentive Lab is updated monthly and some previous data entry errors are also corrected. We use the December 2016 version.
[19] We manually match, clean, and correct a few duplicate company filings and fiscal year end dates during matching.

turnover during a firm-year.[20] Since Execucomp definition of current CEO fits better to our study, we use Execucomp to identify the current CEO. Whenever current CEO cannot be identified from Execucomp, we drop the observation. We also require that the executive who is identified as the current CEO should exist in both Execucomp and Incentive Lab databases.

We further restrict our sample to the fiscal years between 1998 and 2014.[21] The number of deleted and remaining observations for each filtering step is provided in Panel A of Table 2.A.1 in the appendix. Panel B of Table 2.A.1 in the appendix presents the number of firm-year observations in each fiscal year.

2.2.2 Measuring CEO Horizon

Our main variable of interest is CEO horizon. We use pay duration as a proxy for CEO horizon. In order to calculate the pay duration, we need detailed information on the grants such as vesting schedule, number of units granted, and the fair value of the grant. The structure of each grant can get complex. Therefore, we first try to group the grants and standardize their defining properties. Then, we determine the grant date value of each grant. Last, we construct our pay duration measure. We closely follow Gopalan et al. (2014) at each step.

2.2.2.1 Grant Classification

First, we classify grants into three groups and call them grant types. In our study, we name these groups as "stock", option", and "cash"[22]. Stock grants are the grants that are recognized as restricted stock or restricted stock unit (RSU) in the compensation literature. Option grants are the ones that are widely known as employee or executive stock options. Cash grants are mostly bonus type grants that are paid in cash.

Second, we identify the vesting schedule of the grants. For each grant, Incentive Lab identifies the first and last vesting month relative to the grant date. Additionally, Incentive Lab also identifies the vesting type of grants. There are two main vesting types. The first one is

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[20] Incentive Lab identifies the current CEO (currentCEO) as the executive who is the CEO at the fiscal year end. Execucomp identifies the current CEO (CEOANN) as the executive who is the CEO for all or most of the fiscal year.
[21] Fiscal year definition of Incentive Lab differs from Compustat. While Compustat cutoff for fiscal year May 31st of the following calendar year, this cutoff is July 14th for Incentive Lab. We use Compustat cutoff.
[22] Incentive Lab has 11 different grant types and we form our groups as follows. Type "cash" is composed of "cashLong", "cashShort", and "unitCash". Type "stock" is composed of "rsu", "stock", and "phantomStock". Type "option" is composed of "Option", "reloadOption", "phantomOption", "sarEquity", and "sarCash".

“graded” vesting where the grant vests gradually over time. For instance, let the number of units granted be 100; the first and last vesting months be 12 and 48 (relative to the grant date), respectively. Then, the first vesting happens 12 months after the grant date and the number of units vested is 100/4=25. The last vesting happens 48 months after the grant date and the number of units vested is again 25.[23] The second type of vesting is “cliff” vesting where the entire grant vests at once at the end of the vesting period. For instance, in the above example, entire 100 units vest 48 months after the grant date. If the vesting schedule is missing or does not fit into two main vesting schedule groups, we assume that the grant has “graded” vesting schedule.[24]

Third, we classify the grants into three groups based on the usage of performance metrics. The first group is the simplest one. The number of units granted is fixed and the vesting does not depend on future performance.[25] The grants in the second group have fixed number of units but the vesting is contingent on future performance. For these grants, Incentive Lab also provides the performance measurement periods.[26] We treat these grants similar to the grants in the first group although their vesting is contingent on future performance.[27] The reason is that we cannot forecast the future performance of the firm. We implicitly assume that the vesting is expected to happen as it is set on the grant date. We believe this is a mild assumption because the CEO forms her expectation and horizon based on exceeding the performance threshold. The third and last group is composed of grants where the number of units vested depends on future performance of the firm. The basic difference between the second and third group is the following. In the second group, the number of units is fixed and the vesting is contingent on future performance. In the third group, the vesting itself does not depend on future performance but the number of units does. For this type of grants, Incentive Lab reports the threshold, target, and maximum number of units. We

[23] Incentive Lab identifies vesting unit length, too. In general, vesting unit length can be monthly, quarterly, semi-annual, or annual. Whenever the vesting unit length is missing or cannot be identified, we assume that it is annual.

[24] Incentive Lab classifies vesting schedules into four groups, “Ratable”, “None”, “Unknown”, and “Cliff”. After examining the SEC filings, we find out that “None” or “Unknown” correspond to complex vesting schedules. (e.g. 1/4 of the units vest in the first year and the remaining units equally vest over 6 years.) “Ratable” and “Cliff” fit into our “graded” and “cliff” vesting schedule definitions, respectively.

[25] Incentive Lab has 4 different performance metric types. These are “Time”, “Abs”, “Rel”, and “AbsRel”. If the type is “Time”, then the number of units granted or vesting does not depend on future performance of the firm. For the other types, the number of units granted or vesting depends on the future performance of the firm.

[26] For the same grant, there may exist more than one performance metric and consequently different measurement periods. We consider the performance measurement period to be the longest one.

[27] For some grants, the performance measurement period ends later than the reported last vesting period. To deal with this inconsistency, we assume that the last vesting happens at the end of the performance measurement period. We choose the performance measurement period because the executive’s decisions and actions should be rather related to performance measurement period.

choose the target number of units to be the total number of units to vest.[28] We use the same line of reasoning as in the second group and assume that the CEO's expectation is based on reaching the target performance. The grants in the third group may also have vesting provisions besides variable number of units. However, we do not create a new group of grants where both number of units and vesting are contingent on future performance. If the number of units is contingent on the future performance, the grant is directly classified to be in the third group. After determining the number of units, we treat these grants similar to the grants in the first group.

Although Gopalan et al. (2014) do not incorporate cash grants in their duration calculation, we include cash grants in our CEO horizon measures for two reasons. First, almost all cash grants are contingent on some performance metric and therefore these grants may have material effect on CEO decisions. Second, our sample period is 1998-2014 and the reporting format of compensation components changes in year 2006 due to SEC Compensation Disclosure Rule.[29] In pre-2006 period, "Bonus" includes compensation that is not recognized as a grant and cash grants that are contingent on short-term (annual) performance. In pre-2006 period, long-term (multi-year) performance contingent cash grants are reported under "LTIP" (Long-Term Incentive Plans).[30] In post-2006 period, "Bonus" only includes compensation that is not recognized as a grant. Short-term and long-term cash grants are reported under "Non-Equity Incentive Plan" in post-2006 period. Since we use "Bonus" as an input to construct our main pay duration measure and it is hard to separate the short-term performance contingent cash grants from total bonus amount in pre-2006 period, we add short-term cash grants to total bonus amount for post-2006 period to achieve consistency in the "Bonus" component of two reporting formats.

Panel A of Table 2.1 presents the distribution of stock, option, and cash type grants together with their usage of performance metric in a two-way fashion. We only report the grants whose grant date values and vesting schedules can be determined. While cash grants only constitute 5.57% of total number of grants, total number of stock grants (45.31%) and option grants (49.12%) have similar frequency in our grant sample. Among 22,895 option grants, 97.68% of them have simple time-based vesting schedules. On the other hand, time-based vesting and performance

[28] For some grants, the target number of units is missing but the threshold and maximum number of units are available. In these cases, we use the average of threshold and maximum number of units as the target.
[29] We differentiate two reporting formats by the help of Execucomp indicator variable "OLD_DATAFMT_FLG" that equals one for pre-2006 reporting format, and zero for post-2006 reporting format.
[30] Short-term and long-term cash grants are classified as "cashShort" and "cashLong or unitCash" in Incentive Lab database, respectively.

metric usage for unit determination are both common in stock grants. Among 2,597 cash grants, 94.15% of them have performance metrics determining the number of units vested. There are only 25 time-based cash grants.[31] Panel B of Table 2.2 presents the distribution of vesting schedules. While the vesting length of stock grants (56.89%) and cash grants (75.36%) are clustered around 3 years, most option grants have vesting length of 3 years (36.17 %) or 4 years (31.71 %). The longest vesting period length we observe is 20 years.[32] A significant fraction of stock and option grants have graded vesting schedules whereas most of the cash grants have cliff type vesting schedules.

2.2.2.2 Grant Valuation

After determining and standardizing the type, vesting schedule, and number of units; we need to determine the value of the grants as of the grant date. We have two possible approaches for grant value determination.

In the first approach, we can use the grant date fair value of the grants in Incentive Lab. This is what the firms report for the value of the grants. There are two drawbacks of this approach. One is the missing values in pre-2006 period, and the other is non-standard techniques in reporting the grant date fair values. In pre-2006 period, the firms are not required to disclose grant date fair value details in SEC filings. However, in post-2006 period, new disclosure rules require the firms to report grant date fair value of each grant together with other details such as performance metrics used, performance targets, peer firms used in performance benchmarking, etc. For stock grants, grant date fair value is the number of units times the closing stock price on the grant date. Therefore, the method is straightforward and standard for all the firms with few exceptions.[33] However, calculating the value of option grants is more complicated. Firms are required to use option pricing models after the implementation of FAS 123R in 2006 in order to expense the employee stock options in financial statements. The widely known and used option pricing model

[31] When we examine these grants more closely, we observe an inconsistency in Incentive Lab data. These grants are reported in a way that the number of units vested is contingent on performance metrics. However, they are classified as time-based grants. This inconsistency does not affect the construction of pay duration measures and we treat them as time-based grants for classification purposes.
[32] It is granted to J. Powell Brown by Brown & Brown, Inc. for fiscal year 2009.
[33] When the number of units are contingent on future performance, firms generally use the target number of units. However, there are few cases where they use the threshold or maximum number of units. If the vesting is contingent on future performance, firms still use the number of units granted or target number of units (still threshold or maximum number of units are possible). Moreover, some firms use the average of bid price and ask price on the grant date close to determine the value per unit.

is the Black-Scholes model (Black and Scholes (1973)). One basic version of this model requires 6 inputs which are current stock price, exercise price, time to expiration, risk-free rate, stock volatility, and dividend yield. Although some of these inputs such as exercise price and current stock price can be measured objectively, other inputs need estimation. Firms can employ their own estimation techniques especially for stock volatility which is a sensitive parameter in determining the value of an option.[34] Managers use discretion in determining these parameters (Hodder, Mayew, McAnally, and Weaver (2006)). Therefore, there is not a standard method among firms to report the grant date fair value of the option grants.

In the second approach, we can create and use our own grant valuation method to standardize the grant values so that our pay duration measure becomes comparable among firms.

For stock grants, we need the number of units and price per unit. Incentive Lab already provides the number of units and we obtain the grant date closing price from CRSP.[35] We find the grant date value of the stock grant as the number of units times the unit price.[36]

For option grants, we use the Black-Scholes option pricing model with continuous dividends.[37] Similar to stock grants, we first obtain the grant date closing price from CRSP. To estimate the expected stock volatility, we use 3-year historical daily stock returns series.[38] Incentive Lab provides the exercise price and expiration date of the option[39]. We estimate risk-free

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[34] In SEC filings, some firms report the parameter values they use in the Black-Scholes model.

[35] We observe that the median number of days between the start of fiscal year and grant date is 58 in our grant sample. Therefore, for missing grant dates (more prevalent in pre-2006 period), we assume that the grant date is 2-months after the start of the fiscal year. Some grant dates are likely erroneous. We use 2-months convention for the grants whose reported grant dates are earlier than 90 days before the start of the fiscal year or later than 366 days after the start of the fiscal year. If grant date corresponds to a non-trading day, then we choose the closest trading day that is before the grant date.

[36] We assume that the reported number of units is as of the grant date even though the SEC filing is as of the fiscal year end. Otherwise, we need to adjust the number of units or the unit price for stock split type events.

[37]
$$C=Se^{-q\tau}N(d_1)-Xe^{-r\tau}N(d_2)$$
where
$$d_1=\frac{\ln(S/X)+\left(r-q+\frac{1}{2}\sigma^2\right)\tau}{\sigma\sqrt{\tau}}$$
$$d_2=d_1-\sigma\sqrt{\tau}$$
and, where

C is the price of the call option. S is the current stock price. q is the dividend yield. τ is time to expiration in years. r is the risk-free rate. N(.) is cumulative standard normal distribution function. X is the exercise price. σ is the standard deviation of the underlying stock return process.

[38] We annualize the daily volatility by multiplying by the square root of 250.

[39] We divide days to expiration by 365 to convert it to years. We also assume that the exercise price is as of the grant date event though the SEC filing is as of the fiscal year end.

rate by daily treasury yield curve.[40] We match the maturity of the option with the term structure of the treasury rates.[41] The last parameter we need is the dividend yield. To estimate the future dividend yield, we use the average historical annual dividend yield of the last three fiscal years. To find the annual dividend yield, we utilize CRSP daily distributions. We calculate the annual dividend yield as the total dividend paid over the fiscal year divided by the closing price at fiscal year-end.[42] Finally, we take the average of previous 3-year dividend yield. After obtaining the unit price of the option grant using Black-Scholes model, we calculate the grant date value as the number of units times the unit price.[43]

For cash grants, the dollar values are directly reported and we do not need any further calculation.

We drop the grants for which we cannot clearly determine the vesting schedule or the value due to missing components.

2.2.2.3 Pay Duration Measures

We employ the pay duration measure used by Gopalan et al. (2014) in our study. It is the weighted average vesting length of different compensation components. These are salary, bonus, stock grants, option grants, and cash grants. The weighting is based on the value of each grant. CEO pay duration in firm i at fiscal year t is

$$\text{Duration}_{it}=\frac{\sum_{k=1}^{n_{it}} V_{itk}\tau_{itk}}{\text{Salary}_{it}+\text{Bonus}_{it}+\sum_{k=1}^{n_{it}} V_{itk}} \tag{5}$$

where $Salary_{it}$ and $Bonus_{it}$ are the Incentive Lab salary and bonus amounts. V_{itk} is the grant date value of grant k and τ_{itk} is the vesting length of grant k, n_{it} is the total number of grants. In the above formulation, salary and bonus are assumed to have a vesting length of zero. If the grant has a graded vesting schedule, vesting length is approximated by $(\tau_{itk}+1)/2$.[44]

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[40] We take the natural logarithm of 1 plus risk-free rate to convert it to continuously compounded return.

[41] We use linear interpolation for matching. For instance, if time to expiration is 540 days then we use 1-year and 2-year rates and assume that a year has 360 days.

[42] We only include ordinary (first digit of *DISTCD* is 1) and recurring (third digit *DISTCD* is either 2, 3, 4, 5, or 6) dividends in our dividend yield calculations. We also use price adjustment factor from CRSP and set the dividend yield to missing if it is larger than 1. We transform annual dividend yield to a continuous scale by taking the natural logarithm of one plus the dividend yield.

[43] In general, option grants are American type options. However, our model prices European type options. Therefore, we slightly underestimate the value of option grants.

[44] If we assume that equal amount of units vests annually over the vesting period, then we can replace $V_{itk}\tau_{itk}$ by $V_{itk}\left(\frac{1}{\tau_{itk}}+\frac{2}{\tau_{itk}}+\ldots+\frac{\tau_{itk}}{\tau_{itk}}\right)=\frac{V_{itk}}{\tau_{itk}}\frac{\tau_{itk}(\tau_{itk}+1)}{2}=V_{itk}\,(\tau_{itk}+1)/2$.

It is possible to modify the pay duration to better measure the incentive of CEO to increase the short-term performance. Particularly, the option grants may have low stock price sensitivity (low delta), leading to low incentive for short-term stock price performance. To account for this possibility, we replace the grant date value of the grants with their grant date pay-for-performance sensitivity (PPS). In order to measure PPS, we follow Core and Guay (2002) and define PPS as the change in the value of the grant for 1% change in the stock price. For stock grants, a 1% change in stock price corresponds to a 1% change in the value of the unit grant. For option grants, we use the delta of the option to measure PPS.[45] For cash grants, we assume that the sensitivity is zero.[46] Since salary and bonus are assumed to be fixed components of the pay, their PPS is equal to zero. Therefore, salary, bonus, and cash grants are not included in the calculation. The duration measure based on PPS (*DPPS*) is

$$\mathrm{DPPS}_{it}=\frac{\sum_{k=1}^{n_{it}} \mathrm{PPS}_{itk}\tau_{itk}}{\sum_{k=1}^{n_{it}} \mathrm{PPS}_{itk}} \tag{6}$$

where PPS_{itk} is the grant date PPS of grant k in firm i at fiscal year t. Other parameters are as previously defined.

Although this new measure has an appealing feature that it uses PPS, which is a better measure to gauge the incentive of the CEO to boost short-term stock price, it sacrifices the scale of fixed pay components. If the value of stock and option grants are small compared to the fixed components of pay and cash grants, then *DPPS* overestimates the CEO horizon.

In Panel B of Table 2.1, we observe that the vesting schedules of grants are clustered around three years. Therefore, *Duration* may capture mostly the variation in salary and bonus relative to the value of other awards. Even though *DPPS* is not subject to this problem, we create two additional duration measures to address this concern. First measure (*DVW*) just ignores salary and bonus and calculates the duration as value-weighted vesting length of the grants. Second measure (*DEW*) ignores salary and bonus, and it treats all grant values equal. In other words, it calculates the average vesting length of the grants. *DVW* and *DEW* are expressed as

[45] Unit PPS of an option grant can be expressed as

$$\mathrm{PPS}=e^{-q\tau}N(d_1)S/100$$

where all parameters are as previously defined in the Black-Scholes model.

[46] Cash grants may have stock return based performance metrics. However, the contingencies may be complex to determine their PPS.

$$DVW_{it}=\frac{\sum_{k=1}^{n_{it}} V_{itk}\tau_{itk}}{\sum_{k=1}^{n_{it}} V_{itk}} \quad (7)$$

$$DEW_{it}=\frac{\sum_{k=1}^{n_{it}} \tau_{itk}}{n_{it}} \quad (8)$$

where all parameters are as previously defined.[47] To conduct our empirical tests with a clean sample, we set all four duration measures to missing if any grant in a given firm-year has a missing input that hinders the calculation of its grant date value or vesting schedule.[48]

2.2.3 Measuring Investor Horizon

We adopt two different approaches to measure the investor horizon. In both approaches, we classify an institution as short-term or long-term based on its trading behavior. Then, we measure investor horizon of the firm by aggregating the ownership of long-term institutions. We use Thomson Reuters database and 13F filings to determine the stock holdings and trading behavior of institutions in each quarter.[49]

Our first approach is from Derrien et al. (2013). We determine short-term and long-term institutions based on their portfolio turnover at each quarter. We first measure stock turnover for an individual stock k held by institution j at quarter t as follows.

$$STO_{kjt}=\begin{cases} \frac{S_{kj,t-12}-S_{kjt}}{S_{kj,t-12}}, & S_{kj,t-12}>S_{kjt} \\ 0, & S_{kj,t-12}\leq S_{kjt} \end{cases} \quad (9)$$

where S_{kjt} denotes the number of stock k shares held by institution j at quarter t. If institution j is a net buyer (compared to t-12) of firm k shares at quarter t, the turnover is set to zero. We aggregate the individual stock turnovers to find the portfolio turnover of institution j at quarter t.

$$PTO_{jt}=\sum_{k\in K_{jt}} \omega_{kjt} STO_{kjt} \quad (10)$$

[47] If the number of grants (n_{it}) is zero, then we set *DPPS*, *DVW*, and *DEW* equal to zero.

[48] We face a trade-off here. If we choose not to drop the firm-year observation, we implicitly assume that the problematic grant has an average value and vesting schedule compared to other grants for the same firm-year observation. If we choose to drop the problematic firm-year observation, then we decrease the sample size.

[49] We set shares held by institution (*SHARES*) to zero if the firm has missing end of month price information in CRSP or file date (*FDATE*) and report date (*RDATE*) variables in Thomson Reuters database are not in the same quarter. Setting it equal to zero biases us against classifying an institution as a long-term. We also set it to zero if number of shares held by the institution is greater than number of outstanding shares (SHROUT) from CRSP. Lastly, we adjust SHARES using shares adjustment factor (*CFACSHR*) from CRSP.

where ω_{kjt} denotes the weight of stock k holdings in investor j's portfolio at quarter t and K_{jt} denotes the portfolio of institution j at quarter t. We take the average of previous four quarter's portfolio turnover to smooth possible extreme turnover in a quarter. Therefore, the turnover for institution j at quarter t becomes,

$$ITO_{jt}=\sum_{\tau=0}^{3}\frac{PTO_{j,t-\tau}}{4} \quad (11)$$

By construction, portfolio turnover is a variable between zero and one. Based on the portfolio turnover, we classify the institutions as short-term or long-term. For this purpose, we sort institution-quarter observations based on *ITO*. Then, we classify an institution as long-term if its *ITO* is less than or equal to median *ITO* in the sample period. Otherwise, it is classified as short-term, including the institutions for which *ITO* is missing. In the last step, we aggregate the ownership of long-term institutions to find the investor horizon of each firm-year observation in our sample. Long-term institutional ownership of firm i in quarter t is defined as,

$$T\_LTIO_{it}=\frac{1}{SHROUT_{it}}\sum_{j\in L_{it}}S_{ijt} \quad (12)$$

where L_{it} denotes the set of long-term investors that hold firm i's shares at quarter t, S_{ijt} denotes the number of firm i shares held by institution j at quarter t, and $SHROUT_{it}$ is the number of outstanding shares of firm i at quarter t. Short-term institutional ownership (*T_STIO*) is defined in a similar fashion. Long-term institutional ownership and short-term institutional ownership constitute the total institutional ownership. If there is no institutional ownership information for a particular firm in Thomson Reuters, we set *T_LTIO* equal to zero. Since *LTIO* and *STIO* are obtained quarterly and our sample is composed of firm-year observations, we choose the one that is four quarters before the fiscal year-end, which roughly corresponds to fiscal year-start.[50]

Our second approach is from Gaspar et al. (2005) and is very similar to the first one except how we measure the turnover. It is called churn ratio and it is a measure of how frequently and to what extent an institution changes its portfolio composition. It is formally defined as

$$CR_{jt}=\frac{\sum_{k\in K_{jt}}\left|S_{kj,t}P_{k,t}-S_{kj,t-1}P_{k,t-1}-S_{kj,t-1}\Delta P_{k,t}\right|}{\sum_{k\in K_{jt}}\frac{S_{kj,t}P_{k,t}+S_{kj,t-1}P_{k,t-1}}{2}} \quad (13)$$

[50] Our main hypothesis asks whether investors affect pay duration. We use the fiscal year start because the grants are granted to executives during the fiscal year.

where P_{kt} is the price of stock k at quarter t, ΔP_{kt} is equal to $P_{k,t}$ minus $P_{k,t-1}$, and K_{jt} denotes the portfolio of institution j at quarter t and t-1. By construction, churn ratio is a variable between zero and two.[51] After finding the churn ratio, we follow the same procedure as in turnover case. We take the average of previous four quarter's churn ratio and call it *ICR*. Then, we classify an institution as long-term or short-term based on the median churn *ICR* in the sample period. Lastly, we find long-term institutional ownership of firm i at quarter t (*C_LTIO*).

For additional robustness checks, we created three additional investor horizon variables. First one is T*_LTIO-T_STIO* (*C_LTIO-C_STIO*), which is simply the difference between long-term and short-term institutional ownership. Second one is *T_LTIO/IO* (*C_LTIO/IO*), which measures the ratio of long-term institutional ownership to total institutional ownership. For the third one, we value weight both *ITO* and *ICR* by institutional holdings and call them *VW_ITO* and *VW_ICR*. We further classify institutions as indexer and non-indexer based on their portfolio composition. We define long-term indexer and long-term non-indexer institutional ownership as *T_LTIO_IND* (*C_LTIO_NIND*) and *T_LTIO_NIND* (*C_LTIO_NIND*), respectively. The details are provided in Section 2.4.3.

In addition to the turnover based approaches, we use institutional investor classification of Bushee (1998). Based on portfolio turnover, diversification, and expected investment horizon, the institutions are classified as quasi-indexer, transient, or dedicated. Similar to previous measures, we find each type's ownership amount and call them *QIX_IO*, *TRA_IO*, and *DED_IO*, respectively. The details are provided in Section 2.4.7.

2.3 Empirical Framework

We predict that longer investor horizon is associated with longer pay duration. To test this prediction, we estimate the following panel data regression model using OLS regression.

$$Duration_{it} = \beta_0 + \beta_1 * Horizon_{it} + Firm\ Controls_{it} + CEO\ Controls_{it} + Industry\ FE_{it} + Year\ FE_t + \varepsilon_{it} \quad (14)$$

where $Duration_{it}$ is the pay duration of the CEO, $Horizon_{it}$ is a measure of investor horizon. We use one-year lagged values of time-varying firm control variables to reduce endogeneity. We

[51] In an extreme case where the institution sells all shares of a particular stock, churn ratio may exceed 2. Suppose the firm holds 100 shares at a price of \$1 at t-1 and 0 shares at a price of \$2 at t. The individual churn ratio for this transaction is 4, which may cause the aggregate churn ratio to exceed 2.

include industry and year fixed effects to control for unobserved heterogeneity at the industry level and time trend.[52] We use Fama-French 48 industries (Fama and French (1997)) to define the industry and fiscal year to define the year. In particular, we use industry fixed effects to control for project duration since it is closely related to the nature of the industry and a determinant of pay duration (Gopalan et al. (2014)). Following Gopalan et al. (2014), we cluster standard errors at the industry level in order to account for possible error correlation within industry.

We use additional variables to control for other firm related factors possibly affecting the pay duration. Again, we closely follow Gopalan et al. (2014) and use their base control variables. Our first control variable is firm size (*Size*). Firm size is a standard control variable for the executive compensation studies. Our next control variable is firm risk. Gopalan et al. (2014) suggest that riskier firms choose less performance sensitive compensation contracts. Since distant cash flows are riskier and higher pay duration increases the riskiness of CEO pay, riskier firms choose short-term pay duration to decrease the risk faced by the CEO. We use stock return volatility (*Volatility*) and financial leverage (*Leverage*) to measure firm risk.

Next, we control for project duration. Gopalan et al. (2014) state that incentivizing managers to choose short-term projects is costlier when the firm has valuable long-term projects. Therefore, long-term project duration should be associated with longer pay duration. We use fraction of long-term assets (*Long-Term Assets*), market-to-book ratio (*Market to Book*), and R&D intensity (*R&D*) to proxy the project duration of the firm. To account for missing R&D values, we create an indicator variable (*R&D is Missing*) which equals one if R&D is missing and zero, otherwise. Then, we set missing R&D values equal to zero.

Our next control variable is related to stock performance. Gopalan et al. (2014) predict and find positive association between stock performance and pay duration. If stock performance is considered to be a proxy for CEO ability, then the firm increases pay duration to retain the CEO. Increasing the pay duration increases the cost of departure for the CEO since executives generally lose the unvested portions of their grants when they leave the firm. Therefore, firms increase pay duration to keep skilled CEOs. We use previous year's market adjusted stock return (*Excess Return*) to measure stock performance.

We also use bid-ask spread (*Spread*) to control for risk and information environment of the firm. If higher bid-ask spread implies higher risk, then we expect a negative association between

[52] Our results are qualitatively similar when we use industry times year fixed effects.

bid-ask spread and pay duration. If bid-ask spread proxies information asymmetry, then higher bid-ask spread firms may design higher pay duration contracts to deter the manager from earnings management. (Gopalan et al. (2014)).

Our investor horizon variable is based on institutional investors. The horizon of an average institutional investor can be different from an average block-holder or retail investor of the same firm. To control for this possible difference, we included total institutional ownership (*IO*) as a control variable in our regressions. Therefore, we capture the effect of long-term institutional investors above and beyond the effect of total institutional ownership.

In addition to firm characteristics, we also control for four CEO characteristics. First, we use an indicator variable that identifies the CEO as a block-holder (*CEO is BH*). Through high ownership stake and founder role, a block-holder CEO has incentives for long-term value creation. Therefore, the firm does not need to offer a long duration pay contract, which imposes high risk on the CEO. Our second and third variables are age of the CEO (*CEO Age*) and tenure of the CEO (*CEO Tenure*). If the CEO's age or tenure is high and she is close to retirement, providing a long duration pay contract does not incentivize her for long-term value creation since the unvested portions of the grants are forfeited upon retirement. Her horizon is determined mostly by her expected time to retirement. Moreover, as Gopalan et al. (2014) discuss, older or longer-tenured executives may have more reputational capital at the firm so they behave like a block-holder and longer pay durations are not necessary. Our fourth and last CEO characteristic is an indicator variable (*CEO is Chair*) which identifies whether the CEO is the chair of the board of directors. From a corporate governance point of view and assuming that CEO-Chair duality is a sign of weak governance or CEO entrenchment, the relation can go in both directions. (Gopalan et al. (2014)). While a long duration pay contract can act as a monitoring mechanism when other monitoring mechanisms are weak, a long duration pay contract can also be the outcome of strong monitoring mechanisms. Similarly, if older and longer-tenured executives are entrenched, the relation between pay duration and CEO age/tenure can also go in both directions.[53]

[53] In this discussion, we assume that the CEO prefers a shorter pay duration than other investors. This is a plausible assumption because a risk averse CEO would prefer a larger fixed component in her compensation mix. As the salary and bonus portion of the compensation get larger, the pay duration becomes shorter. Even the shortest horizon investor would prefer stock and option type compensation for the CEO.

We also use additional control variables such as deferred compensation (*Pension*), unvested portions of previous grants (*D-Unvested*), and board independence (*Board Independence*) for robustness checks. The details are provided in Section 2.4.4.

Table 2.A.2 of the appendix includes the variable definitions. We winsorize all continuous firm characteristic variables at 1% level. We replace institutional ownership values with one whenever they are greater than one.

Table 2.2 provides summary statistics for the variables used in our analyses. Sample means of our main duration variables *Duration* (1.459) and *DPPS* (2.167) are comparable in magnitude to Gopalan et al. (2014) where they report 1.440 and 2.209, respectively.[54] As evidenced by Duggal and Millar (1999), firm size is positively associated with institutional ownership. Since Incentive Lab coverage is biased towards large firms, we observe high average institutional ownership in our sample (0.702). As previously mentioned, the horizon of an average institutional investor can be different from an average non-institutional investor of the same firm. Therefore, our investor horizon measures may not fully reflect the aggregate investor horizon of a firm and we implicitly assume that institutional investor horizon proxies for aggregate investor horizon. High average institutional ownership in our sample supports our implicit assumption. We further try to overcome this problem by using total institutional ownership as a control variable.

2.4 Results

Our main hypothesis is that firms with higher long-term shareholder ownership provide longer duration compensation contracts to their CEOs. First, we estimate the panel data regression specification in Equation (14) to see the direction and magnitude of the correlation between investor horizon and pay duration. Second, we conduct robustness checks using alternative measures of our main variables. Third, we address the endogeneity concerns by splitting long-term institutional ownership into endogenous and exogenous parts. Fourth, we do additional analysis addressing concerns about other control variables. Fifth, we conduct further tests using institution mergers as a source of exogenous variation in investor horizon. Sixth, we use hedge fund activism

[54] It is just a mere coincidence that the median and maximum values of *DPPS*, *DVW*, and *DEW* are the same. However, t-tests on the mean values of these three variables reveal that they are statistically different from each other. The reason why their maximum or median values are exactly equal is that the value is computed for a CEO who has only one grant and that is a stock grant.

to provide additional evidence for our hypothesis. Last, we use an alternative institutional investor classification to gain more insight.

2.4.1 Baseline Results

Table 2.3 presents the results of our baseline regression specification. We begin our empirical analysis by estimating our regression specification in Equation (14). We have two different specifications for control variables. While Column (1) and (2) use only firm characteristics, Column (3) and (4) use firm and CEO characteristics as control variables. Our main variable of interest is *T_LTIO* (*C_LTIO*). We expect that higher long-term institutional ownership is positively associated with longer pay duration. Consistent with our hypothesis, the coefficient estimates are positive and statistically significant in all the regression specifications. One standard deviation increase in long-term institutional ownership (16.76%) is associated with 22 to 25 days of increase in pay duration. This is approximately a 4.5% increase relative to the mean.

The positive coefficient on *IO* suggests that higher institutional ownership is associated with longer pay duration. This finding is consistent with the empirical evidence that institutions do not exacerbate managerial myopia (Wahal and McConnell, (2000)). When we examine the other control variables, their coefficient estimates are generally consistent with our predictions. The positive coefficient on *Size* indicates that larger firms provide longer duration pay contracts. A plausible explanation is that larger firms may want to retain their CEO by increasing the pay duration because larger firms are more complex and it may be costlier to replace the CEO. Therefore, increasing pay duration increases the cost of departure for the CEO. We also find the coefficient on *Excess Return* to be positive significant. In a similar line of thought with firm size, firms force skilled CEOs to stay in the firm by increasing their pay duration. The negative coefficient on volatility is in line with the discussion that riskier firms design less performance sensitive compensation contracts. On the contrary, our second risk measure *Leverage* has positive coefficient but it is not statistically significant. The variables *Market to Book, Long-Term Assets*, and *R&D* all have positive and statistically significant coefficients, suggesting that firms with longer duration projects provide longer duration pay contracts to their CEOs. We find a negative coefficient on *Spread* and it is consistent with the risk explanation. The coefficient estimates of CEO characteristics are also in line with our predictions. A block-holder CEO has a short pay duration since she has a long investment horizon in the firm. As the CEO's age or tenure increases, the pay duration becomes shorter since her natural horizon becomes shorter. The positive coefficient on *CEO is Chair* implies that pay duration and corporate governance are substitutes. Potentially entrenched CEOs are given longer duration pay contracts as an alternative to direct monitoring.

2.4.2 Robustness to Alternative Measures

To strengthen our empirical findings, we use different approaches to measure investor horizon and pay duration. Then, we run our baseline regression specification in Equation (14). In each Column of Table 2.4, we use a different pay duration measure (*Duration*, *DPPS*, *DVW*, and *DEW*). Panel A and B of Table 2.4 report the estimated coefficients of investor horizon variables based on turnover and churn ratio, respectively. For expositional simplicity, we do not report the coefficient estimates of our control variables.

If investors try to force compensation contracts aligned with their horizon incentives, then there will be a conflict of interest between short-term and long-term investors. Thus, the pay duration will be aligned with the horizon of investor group whose ownership is dominant in the firm. Therefore, we expect that the larger the gap between long-term and short-term shareholders, the longer the pay duration. To test this, we use the difference between long-term and short-term ownership (*T_LTIO-T_STIO* and *C_LTIO-C_STIO*). We also employ the ratio of long-term institutional ownership to total institutional ownership (T*_LTIO/IO* and *C_LTIO/IO*) to gauge the dominant investor type. Additionally, we use value weighted turnover (*VW_ITO*) and churn ratio (*VW_ICR*) of institutions to measure the overall institutional investor horizon. This measure takes into account the following concern. Long-term investors with lower ownership in the firm cannot play a major role in determining the compensation contracts due to their low control rights. On the other hand, long-term investors with high turnover or churn ratio (not as high to be classified as short-term) and high ownership in the firm do not try to increase the pay duration as much as the longer horizon investors. To account for these possibilities, we use the value-weighted horizon of the institutions in the firm. This measure also addresses possible problems with our seemingly arbitrary cutoff for identifying long-term investors. When we examine Table 2.4 as a whole, we see that the positive relation between investor horizon and pay duration holds regardless of how we measure investor horizon or pay duration. *VW_ITO* and *VW_ICR* measure the average turnover and they are inversely related to investor horizon. The coefficient estimates of both variables are negative and consistent with our predictions.

2.4.3 Addressing Endogeneity with Indexers

Our baseline results show that there is a positive association between pay duration and investor horizon. However, these results are prone to endogeneity. One possible problem is that long-term investors may choose to invest in firms whose CEOs have a longer pay duration. To overcome this self-selection problem, we make use of indexing behavior of long-term investors. Derrien et al. (2013) discuss that indexer institutions cannot select the firms or have less flexibility in selecting the firms in which they invest in. Therefore, these institutions do not have full control of their investment portfolios. However, they have an influence on corporate decisions (Appel, Gormley, and Keim (2016), Harford, Kecskes, and Mansi (2017)). Therefore, their horizon is plausibly exogenous to pay duration. We refer the reader to Kecskes, Mansi, and Nguyen (2016) for a detailed discussion.

To implement our identification strategy, we classify institutions as indexer or non-indexer following Derrien et al. (2013). We use the "active share" measure by Cremers and Petajisto (2009). Active share measures the investor's deviation from the index and thus the degree of active management. If the degree of active management is low, then the investor follows the index closely. We define active share for an institution j at quarter t as

$$AS_{jt}=\frac{1}{2}\sum_{i\in K_t}\left|w_{kjt}-\theta_{kt}\right| \tag{15}$$

where K_t denotes the set of stocks in the index at quarter t, w_{kjt} is the weight of stock k in portfolio of institution j at quarter t, and θ_{kt} is the weight of stock k at quarter t in the index. Since we do not explicitly know which index the institution tracks more closely, we calculate the active share value for 14 different indices and choose the one with the lowest active share value as the index the institution is possibly tracking. These indices are S&P 500, S&P 500 Growth, S&P 500 Value, S&P Midcap 400, S&P Small Cap 600, S&P 100, Nasdaq 100, S&P 1500 Super Composite, S&P 1500 Growth, S&P 1500 Value, Russell 1000, Russell 2000, and Russell 3000, and market portfolio in CRSP universe.[55] Following Qin and Singal (2015), we classify an institution as an

[55] If we cannot calculate a stock's weight in the index or in the portfolio of the institution due to missing price or number of shares, we set the weight equal to zero.

indexer if its active share is less than or equal to 0.10. All other institutions are classified as non-indexers. By construction, sum of long-term indexer and long-term non-indexer institutional ownership is equal to total long-term institutional ownership. After identifying long-term indexer and long-term non-indexer institutions, we aggregate the ownership amount of each group. As a result, we split the long-term institutional ownership into two parts, a plausibly exogenous long-term indexer ownership (*T_LTIO_IND* and *C_LTIO_IND*) and a possibly endogenous long-term non-indexer ownership (*T_LTIO_NIND* and *C_LTIO_NIND*).

To test our main hypothesis, we assume that long-term indexers are exogenous to our pay duration regression model. If we find that our results hold for both indexer and non-indexer long-term investors, then we can infer that long-term investors do not necessarily choose to invest in firms whose CEOs have a longer pay durations. Therefore, our results are not due to self-selection bias or reverse causality. We modify our baseline regression specification in Equation (14) to separate long-term investor ownership into indexers and non-indexers. We estimate the following regression.

$$\begin{aligned} Duration_{it} &= \beta_0 + \beta_1 * LTIO\_IND_{it} + \beta_2 * LTIO\_NIND_{it} \\ &+ Firm\ Controls_{it} + CEO\ Controls_{it} + Industry\ FE_{it} + Year\ FE_t \\ &+ \varepsilon_{it} \end{aligned} \tag{16}$$

We present the results of our regression estimation in Table 2.5. As usual, we present turnover based and churn ratio based horizon variables in Panel A and B, respectively. In each column, we use a different pay duration variable. For brevity, we report only the coefficient estimates of long-term indexer and non-indexer institutional ownership. In each column of both panels, we see that long-term indexer and non-indexer coefficient estimates are positive and statistically significant. This finding suggest that both indexer and non-indexer long-term investors have positive effect on the pay duration of the CEO. Our finding is also consistent with the view that long-term shareholders whether indexer or non-indexer affect corporate policies (Harford, Kecskes, and Mansi (2017)). When we compare the magnitude of coefficient estimates, we see that indexers have much larger effect than non-indexers. This is consistent with the idea that indexers do not have the exit threat that as non-indexers have (Appel, Gormley, and Keim (2016)). Therefore, they use the compensation channel more aggressively than the non-indexers to align CEO horizon with their investment horizon.

2.4.4 Other Robustness Checks

Although we use a vast variety of control variables in our regression specifications, we still need to address other possible concerns and determinants of pay duration. In this section, we check our main results by running our regression specifications in Equation (14) and (16) with additional control variables. These variables are level of deferred compensation, unvested portions of previous grants, and board independence. We continue to use the same control variables from our main specification. Table 2.6 presents the results of our robustness analysis.

First, we address the effect of post retirement or other types of deferred compensation on the CEO horizon. We do not have explicit vesting information for this type of compensation and that is why we exclude them in pay duration calculations. We construct a variable (*Pension*) to measure the extent of deferred compensation and use it as a control variable. *Pension* is the ratio of sum of change in pension value, nonqualified deferred compensation earnings, and all other compensation to total compensation. We include all "other compensation" items because nonqualified deferred compensation earnings start to be reported after 2006 SEC Compensation Disclosure Rule. Therefore, there is an inconsistency between pre-2006 and post-2006 periods for deferred compensation. Because of this inconsistency and lack of vesting information for deferred compensation, we do not use deferred compensation in our main tests but in the robustness analysis. If there exists an optimal level of overall pay duration including deferred compensation, and if deferred compensation has the longest vesting length, then we should observe a negative relation between *Pension* and pay duration. In Column (1) of Table 2.6, we present the coefficient estimate of our investor horizon variables and *Pension*. In both Panels A and B, the coefficient estimates of investor horizon variables are positive and statistically significant. Same result holds when we split our investor horizon variable into indexer and non-indexer ownership in Column (4). The coefficient estimate of *Pension* has a negative association with pay duration as we predict.

Second, we use the duration of unvested portions of previous grants as a control variable since they may affect CEO decisions. However, we believe that the exclusion of these unvested portions in pay duration construction does not cast a doubt on our results since the tension between long-term and short-term investor groups about the current fiscal year's pay duration still exists regardless of the horizon of the unvested grants. If our conjecture is correct, then long-term shareholders prefer a longer pay duration contract than the short-term shareholders do, even it means shortening the pay duration relative to unvested portion of previous grants. Nevertheless, we introduce the duration of these unvested portions as a control variable to strengthen our conclusion. We construct our control variable (*D-Unvested*) in the following fashion. For the previous grants, we have the information on how many units vest and when they vest as of the grant date. For each firm-year, we update the value, stock price sensitivity, and remaining vesting length of these unvested portions at fiscal year end, and calculate the duration using these updated values.[56] Since we do not have detailed vesting information on the unvested portions of previous grants, we assume that the vesting happens with expected units and at the expected times. Unlike dropping the whole firm-year observation where we cannot determine the vesting schedule and value of any grant, we simply drop the grant itself in the calculation of *D-Unvested*. Furthermore, we start our sample period from 2001 for this analysis since grant information starts in 1998 and most grants have 3 year vesting schedules, as evident from Panel B of Table 2.1.[57] Due to all these shortcomings and assumptions, we do not include *D-Unvested* in our main regression specification as a control variable. Nevertheless, our main results are robust to its inclusion as a control variable. In Column (2) of Table 2.6, we present the coefficient estimate of our investor horizon variables and *D-Unvested*. In both Panels A and B, the coefficient estimates of investor horizon variables are positive and statistically significant. Similar results hold for indexer and non-indexer ownership in Column (5). When we examine the coefficient estimate of *D-Unvested,* it has a positive association with the pay duration. It indicates that firms, on the average, have stable pay duration policies.

Our third and last control variable is about corporate governance. As discussed in Section 2.3, corporate governance can go in both directions with pay duration. We partially control for this by CEO characteristics such as *CEO is Chair* and *CEO Tenure*. However, we also check our results by using board independence, which is a more traditional measure of corporate governance quality. We defined board independence as the fraction of independent directors in the board (*Board Independence*). The reason we do not use board independence in our main regression specification

[56] For instance, a stock grant with 300 units is originally granted on 3/31/2009 with a graded vesting schedule for three years. After one year, on 3/31/2010, there are two remaining unvested portions where 100 units vest on 3/31/2011 with a vesting length of one year and another 100 units vest on 3/31/2012 with a vesting length of 2 years. If the vesting schedule was cliff type, then there is one remaining unvested portion where 300 units vest on 3/31/2011 with a vesting length of two years. We update the value of these unvested portions using the information on day 3/31/2010.

[57] Dropping a grant assumes that the dropped grant and its unvested portions have an average value and vesting schedule as the other unvested portions. Similarly, we may be ignoring some pre-1998 grants which still have not fully vested. Therefore, we again assume that these possible missing pre-1998 grants have average value and vesting schedule as the other unvested portions.

is that we lose around 13% of our observations due to the smaller coverage of ISS Governance database. Moreover, Gopalan et al. (2014) report conflicting results on the relation between corporate governance and pay duration. In Column (3) of Table 2.6, we present the coefficient estimate of our investor horizon variables and *Board Independence*. In both Panels A and B, the coefficient estimates of investor horizon variables are positive and statistically significant. Similar results hold for indexer and non-indexer ownership in Column (6). When we examine the coefficient estimate of *Board Independence*, it has a positive association with pay duration. This is consistent with the findings of Gopalan et al. (2014) but contrary to what we find for *CEO is Chair* variable in our main regression specification.

Overall, our main results are robust to the concerns about deferred compensation, unvested portions of previous grants, and board independence. In unreported tests, we find that these results also hold when we use other three pay duration measures or investor horizon variables.

2.4.5 Addressing Endogeneity with Institution Mergers

So far, our findings indicate a strong positive association between investor horizon and CEO pay duration after controlling for a variety of firm and CEO characteristics, and industry and year fixed effects. Our results are also robust to different measures and regression specifications. Furthermore, we address possible reverse causality by separating long-term investors into indexers and non-indexers. However, our results may still be subject to endogeneity. As previously discussed, there may exist omitted variables which correlate with both the pay duration and investor horizon. To overcome these concerns, we utilize the institution mergers as a source of plausibly exogenous variation in investor horizon of a firm. We borrow the idea from He and Huang (2017) and follow their approach. First, we discuss why institution mergers create a plausibly exogenous variation in investor horizon. Second, we present our empirical methodology. Third, we show our main results. Last, we replicate our results with a matched sample.

2.4.5.1 Institution Mergers as a Source of Exogenous Variation in Investor Horizon

When two institutions merge, the acquirer takes control of the target's holdings and retains the portfolio position of the target for a period of time. Therefore, if the merging institutions have different investment horizons, then the aggregate investment horizon of each firm in target's portfolio will change. For instance, suppose institution T holds firm F in its portfolio. If institution

A, which has a longer investment horizon than institution T, acquires institution T, then the overall investment horizon of firm F increases due to the transfer of control rights from T to A. Hence, we obtain a plausibly exogenous variation in investor horizon of firm F.

Our identification relies on the fact that institutions merge for reasons that are unrelated to their portfolio holdings in a specific stock. In our case, it is highly unlikely that the acquirer institution acquires the target for a reason which is related to the pay duration of the target's holding firms. As discussed in He and Huang (2017), most of the institution mergers are related to financial sector deregulations, business strategy considerations, economies of scale, and building market share.

2.4.5.2 Empirical Methodology

We use SDC Platinum Mergers & Acquisitions database to identify the institution mergers. We apply some filters. First, we require the announcement date to be between 1/1/1998 and 12/31/2013 so that the mergers are aligned with our sample period and our event window. Second, we require both the acquirer and target firm to be a financial firm.[58] After the first two steps, we hand match target and acquirer firm names with the manager names in Thomson Reuters institutional investor database. Third, we require that there is a change of control in terms of percentage acquired.[59] Fourth, we require that the merger is completed within one year of the announcement date and that the target institution stops reporting right after the deal completion and the last reporting date is not more than one quarter before the deal completion. Finally, we require that we can calculate *ITO* or *ICR* of both the acquirer and the target. After all these filters, we identify 31 institution mergers and report them in Table 2.A.3 in the appendix.

Our setup does not allow us to conduct a typical DiD estimation for four reasons. First, a merger may create a positive or negative shock depending on the horizon of merging institutions. While a long-term institution acquiring a short-term institution creates a positive effect on investment horizon of the firm, the effect reverses when a short-term institution acquires a long-term institution. Second, the magnitude of the effect depends on the percentage ownership of target

[58] We use SDC Platinum definitions and codes to identify financial firms. We choose Commercial Banks, Bank Holding Companies (DA), Credit Institutions (DC), Insurance (DF), Investment & Commodity Firms/Dealers/Exch. (DE), Other Financial (DG), Real Estate; Mortgage Bankers and Brokers (DD), Savings and Loans, Mutual Savings Banks (DB). The codes in parentheses are used by SDC Platinum.

[59] We require percentage of shares owned after transaction (PCTOWN) to be at least 50% and percentage of shares before transaction to be less than 50% (PCTOWN – PCTACQ < 50%).

institution and the difference in the horizon of acquiring institutions. If the turnover rates of merging institutions are very close, then a high percentage ownership does not create much variation in investor horizon. A similar idea holds when the percentage ownership of target institution is low albeit a high difference in turnovers of merging institutions. Third, there may be more than one merger in a fiscal year. Fourth, we explicitly need to use the differences in turnover of merging institutions because there is not enough variation when the institutions are classified as either long-term or short-term. We use a continuous, instead of a binary, treatment variable to measure the exogenous change in investor horizon. Even though a continuous type treatment variable is not common in DiD type studies, there are examples in the literature.[60] As previously discussed, a merger of institutions can create positive or negative effect and there can be multiple megers. Therefore, we aggregate the effect of all the institution mergers in a given fiscal year with their signs and their magnitude as a function of shares affected and differences in institution turnover or churn ratio. We formulate our treatment effect as,

$$\text{ITO\_M\_TREAT}_{it} = \sum_{k \in M_{i,t-1}} (\text{ITO}_{Tk} - \text{ITO}_{Ak})(w_{iTk}) \tag{17}$$

where M_{it} denotes the set of institution mergers affecting firm i at fiscal year t.[61] $\text{ITO}_{T,k}$ and ITO_{Ak} denote the *ITO* of target and acquirer institutions for merger k as of the effective date of the merger, respectively. w_{iTk} is the percentage ownership of target institution in firm i as of the effective date of the merger. For robustness checks, we create a similar variable (*ICR_M_TREAT*) using *ICR* instead of *ITO* merging institutions. Furthermore, we create an additional treatment variable where we do not incorporate the magnitude of differences in *ITO* or *ICR* but which institution has higher *ITO* or *ICR*. It is formally defined as

$$\text{ITO\_S\_TREAT}_{it} = \sum_{k \in M_{i,t-1}} \text{sgn}(\text{ITO}_{Tk} - \text{ITO}_{Ak})(w_{iTk}) \tag{18}$$

where *sgn* denotes the signum function and all other variables are as previously defined. Unlike *ITO_M_TREAT*, this variable measures how much ownership is transferred to the control of a longer or shorter term institution. We similarly create the *ICR* version of this variable (*ICR_S_TREAT*).

[60] Acemoglu, Autor, and Lyle (2004) exploits the variation in military mobilization for World War II to investigate the effects of female labor supply on the wage structure. In their study, the mobilization rate is used as a continuous treatment variable.
[61] We use the mergers during fiscal year t-1 because the pay duration is determined at the start of the fiscal year.

We follow Chen, Harford, and Lin (2015) to choose our event period. They use brokerage house closures and mergers as an exogenous event and their event period runs from t-1 to t+1, dropping year t.[62] We require that there is no confounding institutional merger for the same firm during the event period. This results in 2470 (2479) events including 1336 (1348) distinct firms for *ITO* (*ICR*) sample. To test the effect of institutional mergers on the pay duration, we run the following regression.

$$Duration_{it} = \beta_0 + \beta_1 * Treat * Post + \beta_2 * Treat + \beta_3 * Post + Firm\ Controls_{it} + CEO\ Controls_{it} + Event\ FE_{it} + Year\ FE_t + \varepsilon_{it} \quad (19)$$

where i indexes firms and t indexes fiscal years. *Treat* is the treatment effect for each event. *Post* is an indicator variable which equals one if the observation is after the event and zero otherwise. We include the usual firm and CEO controls, and year fixed effects. Unlike the previous regressions, we use event fixed effects to make our regression specification closer to DiD estimation.[63] In this empirical setting, Treat is not identified due to event fixed effects and dropped from the regression. We cluster standard errors at the firm level to account for possible error correlation within a firm. Similar to Chen, Harford, and Lin (2015) and Acemoglu, Autor, and Lyle (2004), our main regression specification does not include any control sample. This does not create an identification problem since our treatment variable is continuous and there are multiple events. As a robustness check, we also estimate the regression specification in Equation (19) with a matched sample. The details of our matched sample procedure are provided in Section 2.4.5.4.

2.4.5.3 Estimation Results

Table 2.7 presents the results of the regression specification in Equation (19). As Gormley and Matsa (2011) suggest, we use neither fixed effects nor control variables in Column (1). In Column (2), we introduce event and year fixed effects to our estimation. We add firm characteristics in Column (3) and CEO characteristics in Column (4) as control variables. In all the specifications, the positive and statistically significant coefficient estimate on the interaction

[62] We face a trade-off here. If we increase the length of our event period, then there are confounding treatments for the same firm from different years and our sample size shrinks significantly when we drop confounding treatments.
[63] Following Gormley and Matsa (2011), we use event fixed effects instead of firm fixed effects because using event fixed effects is more conservative and subsumes the firm fixed effects. Our results are qualitatively similar with firm fixed effects.

term (*ITO_M_TREAT*POST*) indicates that the higher the merger related increase in investor horizon the higher the change in pay duration. The coefficient estimate of *ITO_M_TREAT*POST* in Column (4) of Table 2.7 indicates that a 5% ownership transfer from the target to the acquirer institution where target institution has 20% higher turnover than the acquirer institution translates into a roughly 63 days of higher pay duration for the CEO.

We check the robustness of our results to the definition of pay duration and treatment effect. We present these results in Table 2.8. Each column uses the regression specification in Column (4) of Table 2.7 with different pay duration measures. For expositional simplicity, we only report the coefficient estimates of the interaction terms. In Panel A, the coefficient estimates of the interaction term are positive and statistically significant in all the columns, suggesting that our baseline results are robust to different pay duration measures. In Panel B, we use *ICR_M_TREAT* as our treatment effect. Even though the coefficient estimates are not statistically significant, their sign and magnitude are comparable to our baseline results. In Panel C, we use *ITO_S_TREAT* as the treatment effect. We obtain a positive and statistically significant coefficient estimate of the interaction term for our main pay duration variable. Other columns also yield a positive sign though not statistically significant. The economic magnitude of our estimates is comparable to our baseline case. A 5% ownership transfer from the target to a longer-term acquirer translates into a roughly 65 days of higher pay duration for the CEO. In Panel D, we use *ICR_S_TREAT* as our treatment effect. The coefficient estimates of the interaction term are positive and statistically significant in all four columns except the first one. However, its t-statistics is very close to a 10% significance level. Overall, our results show that the increase in investor horizon causes an increase in CEO pay duration.

2.4.5.4 Estimation with Matched Sample

Our empirical approach is different from the classical DiD approach as our identification relies on the continuous variation in investor horizon. Therefore, we do not include any control observations in our sample. In other words, we do not have any firm-year observations that do not experience treatment effect from institution mergers. Adding control observation to our sample merely achieves a larger sample with added observations having a zero treatment effect and results in less variation in treatment effect variable. However, such additional observations may help to partially control for possible omitted factors that affect the possibility of being in the treatment sample. Thus, we replicate our main results by extending our sample with control observations.

Since our treatment effect is a continuous variable, we use nearest neighbor matching with Mahalanobis distance, instead of a classical propensity score matching. For each event, we choose the best 20 matches, which do not have any confounding event between t-1 and t+1, along lagged values of *Size*, *Volatility*, *Leverage*, *Market to Book*, *Excess Return*, and *IO* as of the event year. The reason we use best 20 matches is that we do the matching with replacement.[64]

Table 2.9 presents the results of the regression specifications with a matched sample and is a replication of Table 2.8. The coefficient estimates are very similar in both magnitude and statistical significance in both tables. This confirms our previous results that there is a positive and causal relation between investor horizon and pay duration.

2.4.6 Additional Evidence: Hedge Fund Activism

There is an ongoing debate on whether activist investors have short investment horizon. These activist investors try to enforce firms to take actions that are profitable in the short-run but detrimental to long-term fundamental value. Many economists, professors, and business professionals share the view that activist hedge funds have short horizons and their activism is based on exploiting short-term stock price increases. In the context of our main hypothesis, we conjecture hedge fund activism as an event that reduces the investor horizon of the firm. Therefore, we predict a decrease in pay duration of the CEO due to the hedge fund activism. To test our prediction, we use the empirical setup of classical DiD estimation. However, we do not claim that hedge fund activism is a fully exogenous event in our empirical setup. It only serves as an additional evidence to our main hypothesis.

Our hedge fund activism sample runs from 2000 to 2011. The initial sample consists of 1849 distinct hedge fund activism event. We use a 3-year (t-3, t+3) event window around each hedge fund activism event. However, our results are robust to 2-year and 1-year event windows.

[64] Matching with replacement may cause a particular control observation to be matched to more than one treatment. Since we use event fixed effects in our regression, we need to determine which treatment each control observation is matched to. In order to do this, we assign the control observation to the treatment observation for which the Mahalanobis distance is the smallest. Similarly, if the matching leads to overlapping periods, we choose the best match. For instance; a treatment firm A in year 2002 is matched to control firm C in year 2002 and another treatment firm B in year 2003 is matched to the same control firm C in year 2003. Even though the fiscal years are not the same, there is an overlap between two matches since our event period runs from t-1 to t+1. We choose the match with the smallest Mahalanobis distance and leave the other treatment unmatched.

We drop year t, the event year. After merging with our main sample and dropping the confounding events for the same firm, we end up with 181 hedge fund activism including 180 firms. We closely follow the empirical approach we use for institution mergers and construct the matched sample in a similar fashion.[65] Specifically, we run the following regression.

$$Duration_{it} = \beta_0 + \beta_1 {*} Treat {*} Post + \beta_2 {*} Treat + \beta_3 {*} Post + Firm\ Controls_{it} + CEO\ Controls_{it} + Event\ FE_{it} + Year\ FE_t + \varepsilon_{it} \quad (20)$$

where i indexes firms and j indexes fiscal years. Treat is an indicator variable that equals one for treatment observations and zero otherwise. Post is an indicator variable that equals one if the observation is post hedge fund activism period and zero otherwise. Similar to institution mergers specification, we include firm and CEO controls, year fixed effects, and event fixed effects. We cluster standard errors at the firm level.

Table 2.10 presents the results of the regression specification in Equation (20). We first examine Panel A. In Column (1), we use our plain specification and do not include any controls or fixed effects. In Column (2), we introduce event and year fixed effects to our estimation. We add firm characteristics as control variables in Column (3) and CEO characteristics in Column (4). In Column (5) and Column (6), we control for long-term institutional ownership with *T_LTIO* and *C_LTIO*, respectively. In all the specifications, the negative and statistically significant coefficient estimate on the interaction term (HF*_TREAT*POST*) suggests that firms that are exposed to hedge fund activism experience a greater decrease in pay duration relative to the control firms (firms that are not exposed to hedge fund activism). In terms of economic magnitude, the interaction term in Column (4) indicates that relative to control firms, hedge fund activism translates into 80 days of lower pay duration for the CEO in the post-event period. This is consistent with the view that activist hedge funds affect corporate policies through shorter pay duration. Overall, activist hedge funds, which are considered as short-term investors, change the pay duration in a consistent way with their investment horizon.

As for robustness checks, we run the regression specification in Column (4) of Table 2.10 with other pay duration measures and report the results in Panel B of the same table. We only report the interaction terms and all the coefficient estimates are negative and statistically

[65] Unlike the institution merger case, we choose only one match for each hedge fund activism event.

significant, confirming that our results on hedge fund activism are robust to the measurement of pay duration.

2.4.7 Alternative Classification of Institutional Investors

We conjecture that institutional shareholders exercise their control rights and use the executive compensation channel to align CEO horizon with theirs. However, compensation is not the only tool to achieve the horizon alignment. For instance, direct monitoring is another way to accomplish the same objective. Therefore, we would like to examine whether other institutional investor classifications yield results that are plausible and consistent with our conjecture and hypotheses. One such alternative classification is based on Bushee (1998). The study classifies institutions into 3 different groups, quasi-indexers, transient, and dedicated, based on not only portfolio turnover or trading frequency but also diversification and expected investment horizon.

The first group is quasi-indexer (*QIX*) institutions, which use indexing or buy-and-hold strategies. They include index funds that are passively managed and funds that are actively managed but closely follow an index. These institutions generally have diversified portfolios and low portfolio turnover. They are passively managed but they are not passive in terms of their ability to affect corporate policies and governance (Appel, Gormley, and Keim, 2016). They are similar to our definition of long-term indexer institutions. Therefore, these investors are expected to exert influence on executive compensation. Since they are classified as long-term institutions, we predict a positive association between quasi-indexer ownership and pay duration.

The second group is transient (*TRA*) institutions which have small stakes in firms and very high portfolio turnover. They mostly trade based on short-term mispricing. Therefore, a myopic CEO boosting short-term performance is strongly preferred by these institutions. However, due to their short-term holding periods and low ownership, they unlikely have a significant effect on the compensation policy of the firm. Consequently, we predict a negative or no significant correlation between transient institution ownership and pay duration.

The third and last group is dedicated (*DED*) institutions. These investors have large and long-term holdings in firms. Due to their concentrated holdings, they choose direct monitoring and they rely more on direct information rather than reports such as earnings, which can be manipulated using earnings management, to assess performance (Bushee (1998), Boone and White (2015)). Hence, contrary to transient institutions, they prefer a CEO who is focused on long-term value creation. However, direct monitoring can be a substitute for a long pay duration contract if it is less costly. Therefore, the relation between dedicated institutional ownership can go in both directions.

We define quasi-indexer institutional ownership (*QIX_IO*), transient institutional ownership (*TRA_IO*), and dedicated institutional ownership (*DED_IO*) as the total number of shares held by each group divided by total number of outstanding shares. Then, we run the following regression specification to test our prediction[66].

$$\begin{aligned} Duration_{it} &= \beta_0 + \beta_1 * QIX\_IO_{it} + \beta_2 * TRA\_IO_{it} + \beta_3 * DED\_IO_{it} \\ &+ Firm\ Controls_{it} + CEO\ Controls_{it} + Industry\ FE_{it} + Year\ FE_t + \varepsilon_{it} \end{aligned} \quad (21)$$

Table 2.11 presents the results of the regression specification in Equation (21). In each column, we use a different CEO pay duration measure. As we predict, the coefficient estimate of *QIX_IO* is positive and statistically significant, suggesting that quasi-indexer institutional ownership is positively associated with pay duration. The coefficient estimate of *TRA_IO* is negative in three of the columns but they are all statistically insignificant. This finding is consistent with the idea that transient investors do not exert influence on executive compensation. Our results on *DED_IO* is interesting. Even though dedicated institutions have long-term investment horizon, the coefficient estimate is negative and statistically significant. As previously discussed, one reason for this finding can be that instead of using compensation channel, dedicated investors use the direct monitoring channel to ensure CEO actions that are aligned with their investment horizon. Overall, our findings are generally consistent with our main hypothesis that institutional investors' investment horizon and trading behavior are determinants of CEO pay duration.

2.5 Conclusion

In this paper, we examine whether shareholders align managerial incentives with their horizon incentives so that the managers implement the corporate policies that are in line with shareholder horizon. In particular, we test whether shareholders use compensation channel to achieve their horizon goals. Our results show that long-term institutional ownership, measured by the trading frequency of institutions, lengthens managerial horizon, measured by pay duration. Our findings are robust to the definition and measurement of investor and CEO horizon. Using indexer

[66] We use all three groups of investors and *IO* in the same regression because there are institutions which do not necessarily belong to one of these three groups. Therefore, there is no multicollinearity.

institutions and institution mergers, we show that our results are not driven by reverse causality or omitted variables. We also document that activist hedge funds who are short-term investors shorten the managerial horizon, which is consistent with our previous results. Alternative investor classification of Bushee (1998) further supports our conjecture that shareholders use their control rights through compensation channel to affect pay duration. Overall, we document that shareholder horizon is a determinant of managerial horizon.

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Appendix 2.A

Table 2.A.1 Sample Filters and Number of Observations by Year
This table presents our filtering steps and number of observations in each fiscal year. Panel A shows the number of firm-year observations deleted and remaining after each filtering step. Panel B shows the number of firms for each year in the filtered sample.

Panel A: Sample Filters

Filter	Obs. Deleted	Obs. Remaining
Incentive Lab Database (December 2016)	-	24,832
Match GVKEY and PERMNO	63	24,769
Identify CEOs	5,992	18,777
Restrict sample to 1998-2014	877	17,900

Panel B: Number of Observations by Year

Year	Obs.	Year	Obs.
1998	927	2006	1,089
1999	1,046	2007	1,153
2000	1.065	2008	1,118
2001	1,025	2009	1,099
2002	1,037	2010	1,086
2003	1,062	2011	1,055
2004	1,059	2012	1,026
2005	1,060	2013	1,006
		2014	987

Table 2.A.2 Variable Definitions

This table presents the brief definitions, constructions, and data sources of the variables, except the ones related to institution mergers and hedge fund activism.

Variables	Definition
Pay Duration Variables	
Duration	$$Duration_{it} = \frac{\sum_{k=1}^{n_{it}} V_{itk} \times \tau_{itk}}{Salary_{it} + Bonus_{it} + \sum_{k=1}^{n_{it}} V_{itk}}$$ where $Salary_{it}$ and $Bonus_{it}$ are dollar amount of salary and bonus, V_{itk} is the grant date dollar value, τ_{itk} is the vesting length, n_{it} is the total number of grants. See Section 2.2.2 for a detailed discussion. Source: ISS Incentive Lab, CRSP, and Compustat
DPPS	$$DPPS_{it} = \frac{\sum_{k=1}^{n_{it}} PPS_{itk} \times \tau_{itk}}{\sum_{k=1}^{n_{it}} PPS_{itk}}$$ where PPS_{itk} is the dollar change in grant value for a 1% change in stock price, τ_{itk} is the vesting length, n_{it} is the total number of grants. See Section 2.2.2 for a detailed discussion. Source: ISS Incentive Lab, CRSP, and Compustat
DVW	$$DVW_{it} = \frac{\sum_{k=1}^{n_{it}} V_{itk} \times \tau_{itk}}{\sum_{k=1}^{n_{it}} V_{itk}}$$ where V_{itk} is the grant date dollar value. τ_{itk} is the vesting length. n_{it} is the total number of grants. See Section 2.2.2 for a detailed discussion. Source: ISS Incentive Lab, CRSP, and Compustat
DEW	$$DEW_{it} = \frac{\sum_{k=1}^{n_{it}} \tau_{itk}}{n_{it}}$$ where τ_{itk} is the vesting length. n_{it} is the total number of grants. See Section 2.2.2 for a detailed discussion. Source: ISS Incentive Lab, CRSP, and Compustat
D-Unvested	Pay duration (*DVW*) of the unvested grants. See Section 2.4.4. for a detailed discussion. Source: ISS Incentive Lab, CRSP, and Compustat
Investor Horizon Variables	
T_LTIO (C_LTIO)	Number of shares held by long-term institutions divided by number of shares outstanding (*SHROUT*). Long-term institutions are identified using turnover (churn ratio). See Section 2.2.3 for a detailed discussion. Source: Thomson Reuters and CRSP.
T_LTIO IND (C_LTIO_IND)	Number of shares held by indexer long-term institutions divided by number of shares outstanding (*SHROUT*). Indexer institutions are identified using active share. Long-term institutions are identified using turnover (churn ratio). See Section 2.2.3 and 2.4.3 for a detailed discussion. Source: Thomson Reuters, CRSP, Compustat, and Russell Index Constituents.
T_LTIO_NIND (C_LTIO_NIND)	Number of shares held by non-indexer long-term institutions divided by number of shares outstanding (*SHROUT*). Non-indexer institutions are identified using active share. Long-term institutions are identified using turnover (churn ratio). See Section 2.2.3 and 2.4.3 for a detailed discussion. Source: Thomson Reuters, CRSP, Compustat, and Russell Index Constituents.
T_LTIO/IO (C_LTIO)/IO	Number of shares held by long-term institutions divided by number of shares held by all institutions. Long-term institutions are identified using turnover (churn ratio). See Section 2.4.2 for a detailed discussion. Source: Thomson Reuters and CRSP.
T_LTIO-T_STIO (C_LTIO-C_STIO)	Number of shares held by long-term institutions minus the number of shares held by short-term institutions divided by number of shares outstanding (*SHROUT*). Long-term and short-term institutions are identified using turnover (churn ratio). See Section 2.4.2 for a detailed discussion. Source: Thomson Reuters and CRSP.
VW_ITO (VW_ICR)	Value-weighted turnover (churn ratio) of institutions. See Section 2.4.2 for a detailed discussion Source: Thomson Reuters and CRSP.
IO_QIX	Number of shares held by quasi-indexer institutions divided by number of shares outstanding (*SHROUT*). Quasi-indexer institutions are identified using Brian Bushee's Website. See Section 2.4.7 for a detailed discussion. Source: Thomson Reuters and Brian Bushee's Website (http://acct.wharton.upenn.edu/faculty/bushee/IIclass.html) .

(*Table 2.A.2 continued*)

IO_TRA	Number of shares held by transient institutions divided by number of shares outstanding (*SHROUT*). Transient institutions are identified using Brian Bushee's Website. See Section 2.4.7 for a detailed discussion. Source: Thomson Reuters and Brian Bushee's Website (http://acct.wharton.upenn.edu/faculty/bushee/IIclass.html).
IO_DED	Number of shares held by dedicated institutions divided by number of shares outstanding (*SHROUT*). Dedicatated institutions are identified using Brian Bushee's Website. See Section 2.4.7 for a detailed discussion. Source: Thomson Reuters and Brian Bushee's Website (http://acct.wharton.upenn.edu/faculty/bushee/IIclass.html).
Firm and CEO Variables	
Size	Natural logarithm of total assets (*TA*) Source: Compustat.
Volatility	Annualized standard deviation of daily stock returns in current fiscal year. The daily standard deviation is annualized by multiplying by square root of 250. Source: CRSP
Leverage	The ratio of sum of total long-term debt (*DLTT*) and total debt in current liabilities (*DLC*) to total assets (*AT*). Missing items are set equal to zero. Source: Compustat
Market to Book	The ratio of sum of market value of equity (*CSHO*PRCC_F*) and book value of total liabilities (*LT*) to total assets (*AT*). Source: Compustat.
Long-Term Assets	The ratio of sum of total net property, plant, and equipment (*PPENT*) and goodwill (GDWL) to sum of total assets (*AT*) less cash and short-term investments. (CHE). Missing items are set equal to zero. Source: Compustat.
R&D	The ratio of research and development expense (*XRD*) to total assets (*AT*). We set the value equal to zero, if it missing. Source: Compustat.
R&D is Missing	It is an indicator variable that equals one if *XRD* or *AT* is missing and zero otherwise. Source: Compustat.
Excess Return	The total stock return minus value-weighted return of stocks in CRSP universe. Source: CRSP
Spread	Average daily bid-ask spread in current fiscal year. Bid-ask spread is defined as the difference between ask price (*ASK*) and bid price (*BID*) divided by the average of ask price (*ASK*) and bid price (*BID*). Source: CRSP.
IO	Number of shares held by all institutions divided by number of shares outstanding (*SHROUT*). Source: Thomson Reuters and CRSP
CEO is BH	It is an indicator variable that equals one if CEO ownership is greater than or equal to 5%. CEO ownership is defined as *SHROWN_EXCL_OPTS*/*CSHO*/1000. If CEO ownership is larger than 100% then it is set to missing. Source: Execucomp and Compustat.
CEO Age	Natural logarithm of CEO age (*AGE*) in current fiscal year. Source: Execucomp
CEO Tenure	Natural logarithm of CEO tenure. CEO tenure is defined as (*DATADATE-BECAMECEO*)/365. If CEO tenure is negative, then it is set to missing. Source: Execucomp and Compustat
CEO is Chair	It is an indicator variable that equals one if the CEO also serves as the chairperson in the board of directors. Chairperson is identified using *TITLEANN*. Source: Execucomp
Pension	It is the ratio of sum of non-qualified deferred compensation (*PENSIONNQDC*) and other compensation (*OTHERCOMP* and *LEGACYOTHERCOMP*) to total compensation (*TOTALCOMP*). If OLDDATAFMT is equal to one, then *TOTALCOMP* is set to the sum of *SALARY*, *BONUS*, *STOCKAWARDS*, *OPTIONAWARDS*, *NONEQUITYCOMP*, *PENSIONNQDC*, *OTHERCOMP*, *LEGACYLTIP*, and *LEGACYOTHERCOMP*. Missing items are set equal to zero. If *OLD_DATAFMT_FLAG* is equal to one, then *PENSIONNQDC* is set equal to zero. Source: ISS Incentive Lab and Execucomp.
Board Independence	The ratio of independent board members to the number of board members. A board member is classified as independent if *CLASSIFICATION* is equal to "*I*". Source: ISS Governance.

Table 2.A.3 Institution Mergers

This table presents the institution mergers in our sample. We report the acquirer institution, target institution, announcement date, and effective date of the merger.

Acquirer Institution	Target Institution	Announcement Date	Effective Date
BANC ONE Corp., Columbus, Ohio	First Chicago NBD Corp., Chicago, Illinois	4/13/1998	10/2/1998
Travelers Group Inc.	Citicorp	4/6/1998	10/8/1998
Firstar Corp, Milwaukee, Wisconsin	Mercantile Bancorp, St Louis, Missouri	4/30/1999	9/20/1999
Neuberger Berman Inc.	Fasciano Co.	10/16/2000	3/26/2001
Federated Investors Inc.	Edgemont Asset Management Corp.	10/20/2000	4/23/2001
Chittenden Corp, Burlington, Vermont	Maine Bank Corp., Portland, Maine	1/26/2001	4/30/2001
American International Group Inc.	American General Corp.	4/3/2001	8/30/2001
New York Life Investment Management Holdings LLC	QED Investments LLC	10/4/2001	10/4/2001
Voyageur Asset Management Inc.	Daniel S Kampel Associates Inc.	2/13/2002	2/13/2002
Walnut Asset Management LLC	Addison Capital Management LLC	7/18/2002	7/18/2002
Northern Trust Corp.	Legacy South Inc.	11/25/2002	4/30/2003
PNC Financial Services Group Inc.	United National Bancorp, Bridgewater, New Jersey	8/21/2003	12/31/2003
Bank of America Corp.	FleetBoston Financial Corp., Boston, Massachusetts	10/27/2003	4/1/2004
Denver Investment Advisors LLC	Tempest Investment Counselors Inc.	5/21/2004	6/30/2004
Marshall & Ilsley Corp., Milwaukee, Wisconsin	FirstTrust Indiana	10/21/2005	1/4/2006
Mid-Continent Capital LLC	Bufka & Rodgers LLC	5/2/2006	5/2/2006
PNC Financial Services Group Inc.	Mercantile Bankshares Corp., Baltimore, Maryland	10/9/2006	3/2/2007
Chittenden Corp., Burlington, Vermont	Merrill Merchants Bancshares Inc., Bangor, Maine	1/19/2007	5/31/2007
Wachovia Corp., Charlotte, North Carolina	AG Edwards Inc.	5/31/2007	10/1/2007
Fisher Investments LLC	Lighthouse Capital Management	8/15/2008	8/15/2008
Lehman Brothers Holdings Inc.	David J Greene & Co. LLC	4/23/2008	9/30/2008
Silvercrest Asset Management Group LLC	Marathon Capital Group LLC	10/7/2008	10/7/2008
RiverSource Investments LLC	J&W Seligman & Co.	7/7/2008	11/7/2008
Beck Mack & Oliver LLC	Austin Investment Management Inc.	4/10/2009	4/10/2009
MDAM Asset Management Co. Ltd.	Yasuda Asset Management Co. Ltd.	3/3/2010	10/1/2010
Visium Asset Management LP	Catalyst Investment Management Co.	4/6/2011	4/6/2011
Silvercrest Asset Management Group LLC	Milbank Winthrop & Co. Inc.	11/1/2011	11/1/2011
Evercore Wealth Management LLC	Mt Eden Investment Advisors LLC	11/7/2012	12/28/2012
Parametric Portfolio Associates LLC	The Clifton Group Investment Management Co.	11/12/2012	12/31/2012
Fiera Capital Corp.	Bel Air Investment Advisors LLC	9/3/2013	10/31/2013
Fiera Capital Corp.	Wilkinson O'Grady & Co. Inc.	9/3/2013	10/31/2013

Table 2.1 Grant Type and Vesting Schedule Distribution

This table presents the distribution of type and vesting schedules of the grants that we use to construct pay duration measures. The table includes the grants whose values and vesting schedules can be identified. Panel A shows the number of grants based on type and usage of performance metric. The percentage terms are based on the total number of grants. Panel B shows the distribution of grants based on type and vesting schedule. We round the vesting length of grants to the nearest integer. Freq. column reports the number of grants for each group. Percent column reports the percentage of each vesting length group among each type. Fraction graded column reports the fraction of grants that have graded vesting schedules in each vesting length group.

Panel A: Grant Type Distribution

Performance Metric Usage	**Stock Type**	**Option Type**	**Cash Type**	**Total**
No Performance Metric (Time-Based)	10,825 (23.22%)	22,363 (47.98%)	25 (0.05%)	33,213 (71.26%)
Performance Metric Determines Vesting	899 (1.93%)	330 (0.71%)	127 (0.27%)	1,356 (2.91%)
Performance Metric Determines Units	9,395 (20.16%)	202 (0.43%)	2,445 (5.25%)	12,042 (25.84%)
Total	21,119 (45.31%)	22,895 (49.12%)	2,597 (5.57%)	46,611 (100.00%)

Panel B: Vesting Schedule Distribution

	Stock Type			**Option Type**			**Cash Type**		
Vesting Length (Years)	**Freq.**	**Percent**	**Fraction Graded**	**Freq.**	**Percent**	**Fraction Graded**	**Freq.**	**Percent**	**Fraction Graded**
0	505	2.39	0.00	958	4.18	0.01	9	0.35	0.00
1	937	4.44	0.07	2,178	9.51	0.05	50	1.93	0.02
2	1,229	5.82	0.37	888	3.88	0.74	271	10.44	0.10
3	12,015	56.89	0.31	8,282	36.17	0.86	1,957	75.36	0.06
4	3,721	17.62	0.73	7,260	31.71	0.96	186	7.16	0.23
5	2,167	10.26	0.65	2,720	11.88	0.87	108	4.16	0.56
6	222	1.05	0.62	210	0.92	0.63	14	0.54	0.57
7	123	0.58	0.72	151	0.66	0.45	1	0.04	1.00
8	34	0.16	0.56	55	0.24	0.27	1	0.04	0.00
9	21	0.10	0.43	70	0.31	0.34			
10	128	0.61	0.73	123	0.54	0.38			
11	7	0.03	0.71						
12	2	0.01	1.00						
13	2	0.01	1.00						
14	1	0.00	1.00						
15	4	0.02	0.50						
20	1	0.00	0.00						

Table 2.2 Summary Statistics of Variables

This table presents various descriptive statistics of the variables we use in our sample. The variable definitions are given in Table 2.A.2 in the appendix. We winsorize *D-Unvested* and all continuous firm and CEO variables at 1% level, except *Board Independence*, and *IO*. We set *IO* and all investor horizon variables, except *VW_ITO* and *VW_ICR*, equal to one if they are greater than one. The sample is constructed after our filtering steps in Table 2.A.1 in the appendix.

Variables	**Obs.**	**Mean**	**Std. Dev.**	**Min.**	**Median**	**Max.**
Pay Duration Variables						
Duration	15,141	1.459	0.929	0.000	1.558	14.542
DPPS	15,141	2.167	1.224	0.000	2.491	20.014
DVW	15,141	2.220	1.208	0.000	2.501	20.014
DEW	15,141	2.207	1.194	0.000	2.501	20.014
D-Unvested	17,900	0.866	0.773	0.000	0.781	4.170
Investor Horizon Variables						
T_LTIO	17,306	0.570	0.168	0.000	0.586	1.000
T_LTIO_IND	17,306	0.028	0.020	0.000	0.028	0.118
T_LTIO_NIND	17,306	0.542	0.166	0.000	0.556	1.000
T_LTIO/IO	17,299	0.810	0.105	0.067	0.832	1.000
T_LTIO-T_STIO	17,306	0.434	0.182	-0.869	0.447	0.983
C_LTIO	17,306	0.468	0.169	0.000	0.469	1.000
C_LTIO_IND	17,306	0.030	0.021	0.000	0.029	0.124
C_LTIO_NIND	17,306	0.438	0.170	0.000	0.436	1.000
C_LTIO/IO	17,299	0.664	0.157	0.000	0.685	1.000
C_LTIO-C_STIO	17,306	0.230	0.234	-0.991	0.238	0.952
VW_ITO	17,299	0.361	0.067	0.014	0.359	0.817
VW_ICR	17,299	0.264	0.074	0.035	0.254	1.097
IO_QIX	17,306	0.455	0.197	0.000	0.463	5.180
IO_TRA	17,306	0.171	0.113	0.000	0.148	2.708
IO_DED	17,306	0.057	0.086	0.000	0.020	0.922
Firm and CEO Variables						
Size	17,827	15.357	1.581	11.778	15.255	19.693
Volatility	17,562	0.413	0.219	0.137	0.358	1.271
Leverage	17,827	0.248	0.189	0.000	0.230	0.877
Market to Book	17,478	2.035	1.458	0.817	1.525	9.313
Long-Term Assets	17,827	0.416	0.253	0.000	0.434	0.911
R&D	17,827	0.025	0.049	0.000	0.000	0.242
R&D is Missing	17,827	0.471	0.499	0.000	0.000	1.000
Excess Return	17,560	0.080	0.463	-0.737	0.015	2.238
Spread	17,532	0.470	0.675	0.012	0.138	3.432
IO	17,306	0.702	0.189	0.000	0.730	1.000
CEO is BH	17,300	0.083	0.276	0.000	0.000	1.000
CEO Age	17,879	4.012	0.125	3.689	4.025	4.317
CEO Tenure	17,071	1.837	0.747	0.406	1.833	3.584
CEO is Chair	17,790	0.609	0.488	0.000	1.000	1.000
Pension	17,784	0.095	0.142	0.000	0.038	0.782
Board Independence	15,069	0.732	0.159	0.000	0.769	1.000

Table 2.3 Pay Duration and Investor Horizon

This table presents the coefficient estimates of the effect of investor horizon on CEO pay duration. The regression specifications are based on Equation (14). The dependent variable is *Duration*. *T_LTIO* (*C_LTIO*) measures the long-term institutional ownership. Columns (1) and (2) include only firm controls. Columns (3) and (4) include both firm and CEO controls. Column (1) and (3) use turnover based investor horizon measure. Column (2) and (4) use churn ratio based investor horizon measure. The variable definitions are given in Table 2.A.2 in the appendix. We use one-year lagged values of time-varying continuous control variables, except CEO characteristics. All regression specifications include Fama-French 48 industry fixed effects. t-values are reported in the parenthesis. Robust standard errors are clustered at Fama-French 48 industry level. ***, **, and * indicate significance at 1%, 5%, and 10% levels, respectively.

Variables	**(1)**	**(2)**	**(3)**	**(4)**
T_LTIO	0.359***		0.413***	
	(3.145)		(3.874)	
C_LTIO		0.270***		0.319***
		(3.547)		(3.466)
Size	0.122***	0.124***	0.112***	0.114***
	(7.464)	(7.509)	(6.649)	(6.617)
Volatility	-0.103	-0.107	-0.164**	-0.167**
	(-1.310)	(-1.332)	(-2.293)	(-2.278)
Leverage	0.085	0.085	0.005	0.005
	(1.074)	(1.066)	(0.073)	(0.069)
Market to Book	0.054***	0.054***	0.055***	0.056***
	(3.883)	(3.877)	(4.511)	(4.489)
Long-Term Assets	0.139	0.137	0.126*	0.124*
	(1.596)	(1.572)	(1.773)	(1.752)
R&D	0.830**	0.841**	0.799**	0.812**
	(2.092)	(2.109)	(2.210)	(2.254)
R&D is Missing	-0.086*	-0.086*	-0.054	-0.053
	(-1.760)	(-1.720)	(-1.304)	(-1.245)
Excess Return	0.039**	0.038**	0.043**	0.042**
	(2.030)	(2.062)	(2.278)	(2.318)
Spread	-0.122***	-0.123***	-0.127***	-0.128***
	(-5.200)	(-5.260)	(-5.109)	(-5.189)
IO	0.231**	0.351***	0.009	0.141*
	(2.060)	(3.680)	(0.100)	(1.680)
CEO is BH			-0.370***	-0.371***
			(-5.458)	(-5.510)
CEO Age			-0.682***	-0.685***
			(-4.626)	(-4.662)
CEO Tenure			-0.046*	-0.045*
			(-1.859)	(-1.836)
CEO is Chair			0.058**	0.059**
			(2.425)	(2.454)
Constant	-0.643**	-0.646**	2.316***	2.331***
	(-2.281)	(-2.267)	(3.911)	(3.928)
Industry and Year FE	Yes	Yes	Yes	Yes
Observations	14,738	14,738	13,758	13,758
Adjusted R-squared	0.063	0.063	0.089	0.089

Table 2.4 Alternative Measures of Pay Duration and Investor Horizon

This table presents the coefficient estimates of alternative investor horizon variables obtained by running the regression specification in Column (3) of Table 2.3 with alternative CEO pay duration measures as the dependent variable. In Panel A, investor horizon measures are based on turnover. In Panel B, investor horizon measures are based on churn ratio. Each column uses a different CEO pay duration measure. For brevity, we only report the coefficient estimates of the investor horizon variables, number of observations (N), and Adjusted R-squared (Adj. R-sq.) of the regressions. The variable definitions are given in Table 2.A.2 in the appendix. We use one-year lagged values of time-varying continuous control variables, except CEO characteristics. All regression specifications include Fama-French 48 industry fixed effects. t-values are reported in the parenthesis. Robust standard errors are clustered at Fama-French 48 industry level. ***, **, and * indicate significance at 1%, 5%, and 10% levels, respectively.

Panel A: Turnover Based Investor Horizon

	Duration	**DPPS**	**DVW**	**DEW**
Variables	**(1)**	**(2)**	**(3)**	**(4)**
T_LTIO	0.413***	0.734***	0.773***	0.734***
	(3.874)	(4.151)	(4.383)	(4.404)
	N=13,758	N=13,758	N=13,758	N=13,758
	Adj. R-sq.=0.089	Adj. R-sq.=0.054	Adj. R-sq.=0.057	Adj. R-sq.=0.054
T_LTIO/IO	0.326***	0.710***	0.720***	0.688***
	(3.599)	(4.670)	(4.586)	(4.631)
	N=13,752	N=13,752	N=13,752	N=13,752
	Adj. R-sq.=0.089	Adj. R-sq.=0.056	Adj. R-sq.=0.058	Adj. R-sq.=0.055
T_LTIO-T_STIO	0.204***	0.377***	0.399***	0.383***
	(3.705)	(4.205)	(4.394)	(4.454)
	N=13,752	N=13,810	N=13,810	N=13,810
	Adj. R-sq.=0.088	Adj. R-sq.=0.054	Adj. R-sq.=0.057	Adj. R-sq.=0.054
VW_ITO	-0.539***	-0.962***	-1.082***	-1.081***
	(-3.259)	(-3.484)	(-4.438)	(-4.733)
	N=13,758	N=13,804	N=13,804	N=13,804
	Adj. R-sq.=0.089	Adj. R-sq.=0.055	Adj. R-sq.=0.057	Adj. R-sq.=0.055

Panel B: Churn Ratio Based Investor Horizon

	Duration	**DPPS**	**DVW**	**DEW**
Variables	**(1)**	**(2)**	**(3)**	**(4)**
C_LTIO	0.319***	0.577***	0.587***	0.589***
	(3.466)	(3.868)	(3.961)	(4.030)
	N=13,758	N=13,758	N=13,758	N=13,758
	Adj. R-sq.=0.089	Adj. R-sq.=0.054	Adj. R-sq.=0.057	Adj. R-sq.=0.054
C_LTIO/IO	0.263***	0.527***	0.520***	0.523***
	(3.260)	(4.231)	(4.212)	(4.350)
	N=13,752	N=13,752	N=13,752	N=13,752
	Adj. R-sq.=0.089	Adj. R-sq.=0.056	Adj. R-sq.=0.057	Adj. R-sq.=0.0545
C_LTIO-C_STIO	0.159***	0.296***	0.303***	0.307***
	(3.363)	(3.964)	(4.069)	(4.130)
	N=13,758	N=13,758	N=13,758	N=13,758
	Adj. R-sq.=0.088	Adj. R-sq.=0.054	Adj. R-sq.=0.057	Adj. R-sq.=0.054
VW_ICR	-0.704***	-1.239***	-1.244***	-1.254***
	(-3.737)	(-4.015)	(-4.036)	(-4.088)
	N=13,752	N=13,752	N=13,752	N=13,752
	Adj. R-sq.=0.089	Adj. R-sq.=0.055	Adj. R-sq.=0.058	Adj. R-sq.=0.056

Table 2.5 Pay Duration and Investor Horizon with Indexer Non-Indexer Split

This table presents the coefficient estimates of long-term institutional ownership split into indexer and non-indexer ownership. *T_LTIO_IND* and *T_LTIO_NIND* (*C_LTIO_IND* and *C_LTIO_NIND*) measure the long-term indexer and non-indexer institutional ownership, respectively. The regression specifications are based on Column (3) of Table 2.3. In Panel A, investor horizon is based on turnover. In Panel B, investor horizon is based on churn ratio. Each column uses a different CEO pay duration measure. For brevity, we only report the coefficient estimates of the long-term indexer and long-term non indexer variables. The variable definitions are given in Table 2.A.2 in the appendix. We use one-year lagged values of time-varying continuous control variables, except CEO characteristics. All regression specifications include Fama-French 48 industry fixed effects. t-values are reported in the parenthesis. Robust standard errors are clustered at Fama-French 48 industry level. ***, **, and * indicate significance at 1%, 5%, and 10% levels, respectively.

Panel A: Turnover Based Investor Horizon

	Duration	**DPPS**	**DVW**	**DEW**
Variables	**(1)**	**(2)**	**(3)**	**(4)**
T_LTIO_IND	2.948***	3.274***	3.574***	3.797***
	(3.765)	(3.495)	(3.835)	(4.093)
T_LTIO_NIND	0.334***	0.651***	0.681***	0.634***
	(3.049)	(3.514)	(3.676)	(3.649)
Firm and CEO Controls	Yes	Yes	Yes	Yes
Industry and Year FE	Yes	Yes	Yes	Yes
Observations	13,758	13,758	13,758	13,758
Adjusted R-squared	0.090	0.055	0.057	0.055

Panel B: Churn Ratio Based Investor Horizon

	Duration	**DPPS**	**DVW**	**DEW**
Variables	**(1)**	**(2)**	**(3)**	**(4)**
C_LTIO_IND	2.982***	3.213***	3.555***	3.733***
	(3.854)	(3.659)	(4.022)	(4.241)
C_LTIO_NIND	0.274***	0.530***	0.535***	0.534***
	(2.935)	(3.489)	(3.544)	(3.598)
Firm and CEO Controls	Yes	Yes	Yes	Yes
Industry and Year FE	Yes	Yes	Yes	Yes
Observations	13,758	13,758	13,758	13,758
Adjusted R-squared	0.090	0.055	0.057	0.055

Table 2.6 Controlling for Deferred Compensation, Unvested Grants, and Board Independence

This table presents the results of the regression specification in Column (3) of Table 2.3 and Column (1) of Table 2.5 with additional control variables. The dependent variable is *Duration*. *T_LTIO* (*C_LTIO)* measures the long-term institutional ownership. *T_LTIO_IND* and *T_LTIO_NIND* (*C_LTIO_IND* and *C_LTIO_NIND*) measure the long-term indexer and non-indexer institutional ownership, respectively. In Panel A, investor horizon is based on turnover. In Panel B, investor horizon is based on churn ratio. Column (1) and (4) use *Pension* as the additional control. Column (2) and (5) use *D-Unvested* as the additional control. Column (3) and (6) use *Board Independence* as the additional control. For brevity, we only report the coefficient estimates of the investor horizon or long-term indexer and long-term non indexer variables. The variable definitions are given in Table 2.A.2 in the appendix. We use one-year lagged values of time-varying continuous control variables, except CEO characteristics. All regression specifications include Fama-French 48 industry fixed effects. t-values are reported in the parenthesis Robust standard errors are clustered at Fama-French 48 industry level. ***, **, and * indicate significance at 1%, 5%, and 10% levels, respectively.

Panel A: Turnover Based Investor Horizon

Variables	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
T_LTIO	0.415***	0.400***	0.307**			
	(3.797)	(3.315)	(2.193)			
T_LTIO_IND				2.800***	2.343***	1.817**
				(3.701)	(3.586)	(2.402)
T_LTIO_NIND				0.340***	0.331**	0.260*
				(3.071)	(2.605)	(1.896)
Pension	-0.399***			-0.399***		
	(-4.030)			(-4.026)		
D-Unvested		0.226***			0.225***	
		(6.695)			(6.645)	
Board Independence			0.382***			0.378***
			(4.191)			(4.138)
Firm and CEO Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry and Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	13,674	11,798	12,006	13,674	11,798	12,006
Adjusted R-squared	0.091	0.126	0.081	0.092	0.127	0.081

Panel B: Churn Ratio Based Investor Horizon

Variables	**(1)**	**(2)**	(3)	**(4)**	**(5)**	**(6)**
C_LTIO	0.303***	0.305***	0.268**			
	(3.299)	(3.220)	(2.424)			
C_LTIO_IND				2.814***	2.415***	1.891**
				(3.773)	(3.936)	(2.448)
C_LTIO_NIND				0.260***	0.268***	0.246**
				(2.828)	(2.694)	(2.263)
Pension	-0.398***			-0.399***		
	(-4.020)			(-4.017)		
D-Unvested		0.226***			0.225***	
		(6.677)			(6.626)	
Board Independence			0.383***			0.376***
			(4.182)			(4.116)
Firm and CEO Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry and Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	13,674	11,798	12,006	13,674	11,798	12,006
Adjusted R-squared	0.091	0.126	0.081	0.092	0.127	0.082

Table 2.7 Pay Duration and Institution Mergers

This table presents the results of the effect of institution merger related exogenous change in investor horizon on CEO pay duration. The regression specifications are based on Equation (19). Change in investor horizon due to institution mergers is measured by *ITO_M_TREAT*. *POST* is an indicator variable that equals one for post-event period and zero otherwise. The event window is between t-1 and t+1, excluding t. The details of the methodology are provided in Section 2.4.5.2. The dependent variable is *Duration*. Column (1) does not include any control variables or fixed effects. Column (2) uses event and year fixed effects. Column (3) introduces firm controls and Column (4) introduces CEO controls to the regression specification. The variable definitions are given in Table 2.A.2 in the appendix. We use one-year lagged values of time-varying continuous control variables, except CEO characteristics. All regression specifications except, Column (1), include event and year fixed effects. t-values are reported in the parenthesis. Robust standard errors are clustered at firm level. ***, **, and * indicate significance at 1%, 5%, and 10% levels, respectively.

Variables	**(1)**	**(2)**	**(3)**	**(4)**
ITO_M_TREAT*POST	23.035***	30.494***	31.765***	17.234***
	(2.789)	(5.196)	(5.311)	(2.796)
ITO_M_TREAT	-19.577***			
	(-3.906)			
POST	-0.016	-0.268***	0.116***	0.115***
	(-0.259)	(-5.690)	(2.803)	(2.825)
Size			0.001	-0.010
			(0.010)	(-0.135)
Volatility			-0.214	-0.227
			(-1.169)	(-1.280)
Leverage			-0.005	-0.164
			(-0.018)	(-0.602)
Market to Book			-0.006	-0.003
			(-0.225)	(-0.095)
Long-Term Assets			0.342	0.314
			(1.219)	(1.048)
R&D			-1.111	-0.892
			(-1.008)	(-0.775)
R&D is Missing			0.507**	0.415**
			(2.170)	(1.962)
Excess Return			0.124***	0.108**
			(2.802)	(2.296)
Spread			-0.036	-0.034
			(-0.489)	(-0.455)
IO			0.368	0.161
			(1.606)	(0.637)
CEO is BH				-0.536*
				(-1.663)
CEO Age				-1.667**
				(-2.351)
CEO Tenure				-0.030
				(-0.510)
CEO is Chair				-0.095
				(-1.193)
Constant	1.987***	1.641***	0.194	7.411**
	(12.226)	(9.732)	(0.160)	(2.448)
Event and Year FE	No	Yes	Yes	Yes
Observations	3,840	3,840	3,796	3,442
Adjusted R-squared	0.014	0.021	0.033	0.078

Table 2.8 Robustness Checks for Pay Duration and Institution Mergers

This table presents the coefficient estimates of the regression specification in Column (4) of Table 2.7 using alternative institution related exogenous change in investor horizon. In Panel A, B, C, and D, we use *ITO_M_TREAT*, *ICR_M_TREAT*, *ITO_S_TREAT*, and *ICR_S_TREAT* as the treatment effect, respectively. *POST* is an indicator variable that equals one for post-event period and zero otherwise. The event window is between t-1 and t+1, excluding t. The details of the empirical methodology are provided in Section 2.4.5.2. Each column uses a different CEO pay duration measure. For brevity, we only report the coefficient estimates of the interaction terms. The variable definitions are given in Table 2.A.2 in the appendix. We use one-year lagged values of time-varying continuous control variables, except CEO characteristics. All regression specifications include event and year fixed effects. t-values are reported in the parenthesis. Robust standard errors are clustered at firm level. ***, **, and * indicate significance at 1%, 5%, and 10% levels, respectively.

Panel A: Treatment Effect Based on *ITO_M_TREAT*

	Duration	DPPS	DVW	DEW
Variables	**(1)**	**(2)**	**(3)**	**(4)**
ITO_M_TREAT*POST	17.234***	22.976**	24.864**	27.767**
	(2.796)	(1.975)	(2.199)	(2.484)
POST	0.115***	0.062	0.067	0.084*
	(2.825)	(1.008)	(1.173)	(1.723)
Firm and CEO Controls	Yes	Yes	Yes	Yes
Event and Year FE	Yes	Yes	Yes	Yes
Observations	3,442	3,442	3,442	3,442
Adjusted R-squared	0.069	0.069	0.068	0.070

Panel B: Treatment Effect Based on *ICR_M_TREAT*

	Duration	DPPS	DVW	DEW
Variables	**(1)**	**(2)**	**(3)**	**(4)**
ICR_M_TREAT*POST	22.091	42.009	46.394	46.122
	(1.085)	(0.937)	(1.073)	(1.025)
POST	0.062	0.104	0.175**	0.177**
	(1.264)	(0.995)	(2.208)	(2.196)
Firm and CEO Controls	Yes	Yes	Yes	Yes
Event and Year FE	Yes	Yes	Yes	Yes
Observations	3,458	3,458	3,458	3,458
Adjusted R-squared	0.090	0.077	0.080	0.080

(*Table 2.8 continued*)

Panel C: Treatment Effect Based on *ITO_S_TREAT*

	Duration	DPPS	DVW	DEW
Variables	**(1)**	**(2)**	**(3)**	**(4)**
ITO_S_TREAT*POST	3.574**	2.845	3.487	4.239
	(2.009)	(0.883)	(1.157)	(1.504)
POST	0.116***	0.063	0.069	0.085*
	(2.835)	(1.032)	(1.197)	(1.749)
Firm and CEO Controls	Yes	Yes	Yes	Yes
Event and Year FE	Yes	Yes	Yes	Yes
Observations	3,442	3,442	3,442	3,442
Adjusted R-squared	0.077	0.067	0.066	0.068

Panel D: Treatment Effect Based on *ICR_S_TREAT*

	Duration	DPPS	DVW	DEW
Variables	**(1)**	**(2)**	**(3)**	**(4)**
ICR_S_TREAT*POST	3.016	4.943**	5.367**	5.974***
	(1.639)	(2.079)	(2.332)	(2.643)
POST	0.061	0.102	0.173**	0.174**
	(1.231)	(0.975)	(2.180)	(2.159)
Firm and CEO Controls	Yes	Yes	Yes	Yes
Event and Year FE	Yes	Yes	Yes	Yes
Observations	3,458	3,458	3,458	3,458
Adjusted R-squared	0.090	0.077	0.081	0.082

Table 2.9 Pay Duration and Institution Mergers with Matched Sample

This table is a replication of Table 2.8 using a matched sample. Matching is based on lagged values of *Size*, *Volatility*, *Leverage*, *Market to Book*, *Excess Return*, and *IO* as of the event year. We use nearest neighbor matching with Mahalanobis distance as the matching criterion. In Panel A, B, C, and D, we use *ITO_M_TREAT*, *ICR_M_TREAT*, *ITO_S_TREAT*, and *ICR_S_TREAT* as the treatment effect, respectively. *POST* is an indicator variable that equals one for post-event period and zero otherwise. The event window is between t-1 and t+1, excluding t. The details of the empirical methodology are provided in Section 2.4.5.2 and 2.4.5.4. Each column uses a different CEO pay duration measure. For brevity, we only report the coefficient estimates of the interaction terms. The variable definitions are given in Table 2.A.2 in the appendix. We use one-year lagged values of time-varying continuous control variables, except CEO characteristics. All regression specifications include event and year fixed effects. t-values are reported in the parenthesis. Robust standard errors are clustered at firm level. ***, **, and * indicate significance at 1%, 5%, and 10% levels, respectively.

Panel A: Treatment Effect Based on *ITO_M_TREAT* with Matched Sample

	Duration	**DPPS**	**DVW**	**DEW**
Variables	**(1)**	**(2)**	**(3)**	**(4)**
ITO_M_TREAT*POST	21.472***	30.826*	32.735**	35.636**
	(3.427)	(1.960)	(2.117)	(2.308)
ITO_M_TREAT	30.274	27.775	6.808	-1.895
	(0.703)	(0.534)	(0.128)	(-0.036)
POST	0.102	-0.000	-0.004	-0.024
	(0.872)	(-0.004)	(-0.054)	(-0.268)
Firm and CEO Controls	Yes	Yes	Yes	Yes
Event and Year FE	Yes	Yes	Yes	Yes
Observations	5,035	5,035	5,035	5,035
Adjusted R-squared	0.381	0.385	0.391	0.401

Panel B: Treatment Effect Based on *ICR_M_TREAT* with Matched Sample

	Duration	**DPPS**	**DVW**	**DEW**
Variables	**(1)**	**(2)**	**(3)**	**(4)**
ICR_M_TREAT*POST	37.965	69.138	68.867	68.888
	(1.338)	(1.042)	(1.058)	(1.023)
ICR_M_TREAT	53.626	12.197	26.859	19.572
	(1.146)	(0.159)	(0.362)	(0.258)
POST	0.107	0.015	0.020	0.038
	(1.054)	(0.170)	(0.231)	(0.413)
Firm and CEO Controls	Yes	Yes	Yes	Yes
Event and Year FE	Yes	Yes	Yes	Yes
Observations	4,795	4,795	4,795	4,795
Adjusted R-squared	0.370	0.371	0.380	0.389

(*Table 2.9 continued*)

Panel C: Treatment Effect Based on *ITO_S_TREAT* with Matched Sample

	Duration	DPPS	DVW	DEW
Variables	**(1)**	**(2)**	**(3)**	**(4)**
ITO_S_TREAT*POST	4.493**	4.730	5.389	6.172*
	(2.253)	(1.180)	(1.474)	(1.824)
ITO_S_TREAT	6.738	6.902	3.669	2.389
	(0.876)	(0.685)	(0.357)	(0.231)
POST	0.105	0.002	-0.002	-0.021
	(0.892)	(0.023)	(-0.023)	(-0.235)
Firm and CEO Controls	Yes	Yes	Yes	Yes
Event and Year FE	Yes	Yes	Yes	Yes
Observations	5,035	5,035	5,035	5,035
Adjusted R-squared	0.381	0.385	0.391	0.401

Panel D: Treatment Effect Based on *ICR_S_TREAT* with Matched Sample

	Duration	DPPS	DVW	DEW
Variables	**(1)**	**(2)**	**(3)**	**(4)**
ICR_S_TREAT*POST	4.100*	6.780**	7.067**	7.649**
	(1.737)	(2.094)	(2.250)	(2.505)
ICR_S_TREAT	16.689*	16.761	17.579	14.255
	(1.890)	(1.237)	(1.324)	(1.051)
POST	0.105	0.010	0.015	0.033
	(1.030)	(0.120)	(0.177)	(0.353)
Firm and CEO Controls	Yes	Yes	Yes	Yes
Event and Year FE	Yes	Yes	Yes	Yes
Observations	4,795	4,795	4,795	4,795
Adjusted R-squared	0.370	0.372	0.381	0.390

Table 2.10 Pay Duration and Hedge Fund Activism

This table presents the results of hedge fund activism on CEO pay duration. The regression specifications are based on Equation (20). *HF_TREAT* is an indicator variable that equals one if the firm is exposed to a hedge fund activism event and zero if it is a control firm. *POST* is an indicator variable that equals one for post-event period and zero otherwise. The event window is between t-3 and t+3, excluding t. The details of the empirical methodology are provided in Section 2.4.6. In Panel A, the dependent variable is *Duration*. Column (1) of Panel A does not include any control variables or fixed effects. Column (2) of Panel A uses event and year fixed effects. Column (3) of Panel A introduces firm controls and Column (4) introduces CEO controls to the regression specification. Column (5) and Column (6) of Panel A additionally control for long-term institutional ownership with *T_LTIO* and *C_LTIO*, respectively. Panel B reports the coefficient estimates of the regression specification in Column (4) of Panel A with a different CEO pay duration measure. For brevity, we only report the coefficient estimates of the interaction terms in Panel B. The variable definitions are given in Table 2.A.2 in the appendix. We use one-year lagged values of time-varying continuous control variables, except CEO characteristics. All regression specifications except, Column (1) of Panel A, include event and year fixed effects. t-values are reported in the parenthesis. Robust standard errors are clustered at firm level. ***, **, and * indicate significance at 1%, 5%, and 10% levels, respectively.

Panel A: Different Specifications with Baseline Pay Duration Measure

Variables	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
HF_TREAT*POST	-0.224**	-0.229**	-0.212**	-0.218**	-0.213**	-0.220**
	(-2.205)	(-2.198)	(-2.077)	(-2.128)	(-2.094)	(-2.144)
HF_TREAT	0.136	0.157**	0.168**	0.168**	0.170**	0.168**
	(1.442)	(2.033)	(2.207)	(2.248)	(2.260)	(2.240)
POST	0.103	0.042	0.048	0.049	0.047	0.051
	(1.280)	(0.338)	(0.378)	(0.392)	(0.369)	(0.400)
Size			0.064	0.066	0.063	0.066
			(1.251)	(1.267)	(1.218)	(1.261)
Volatility			-0.550***	-0.530***	-0.524***	-0.531***
			(-3.251)	(-3.196)	(-3.210)	(-3.203)
Leverage			0.147	0.146	0.157	0.143
			(0.538)	(0.548)	(0.592)	(0.536)
Market to Book			0.058	0.065*	0.065*	0.065*
			(1.457)	(1.749)	(1.743)	(1.721)
Long-Term Assets			-0.101	0.010	0.012	0.009
			(-0.665)	(0.067)	(0.082)	(0.063)
R&D			0.985	1.031	1.040	1.025
			(1.474)	(1.485)	(1.501)	(1.474)
R&D is Missing			-0.010	0.013	0.014	0.012
			(-0.166)	(0.214)	(0.235)	(0.202)
Excess Return			0.062	0.069	0.073	0.067
			(1.147)	(1.277)	(1.325)	(1.220)
Spread			-0.109	-0.132	-0.133	-0.132
			(-1.205)	(-1.468)	(-1.468)	(-1.468)
IO			0.715***	0.667***	0.514	0.722**
			(2.792)	(2.651)	(1.293)	(2.125)
CEO is BH				-0.319**	-0.316**	-0.321**
				(-2.066)	(-2.022)	(-2.074)
CEO Age				0.053	0.046	0.056
				(0.189)	(0.161)	(0.200)
CEO Tenure				-0.074*	-0.074*	-0.074*
				(-1.700)	(-1.688)	(-1.688)
CEO is Chair				0.024	0.024	0.024
				(0.370)	(0.371)	(0.366)
T_LTIO					0.193	
					(0.528)	
C_LTIO						-0.077

(*Table 2.10 continued*)

						(-0.266)
	1.460***	1.939***	0.412	0.228	0.285	0.213
Constant	(17.019)	(8.061)	(0.498)	(0.154)	(0.192)	(0.144)
Event and Year FE	No	Yes	Yes	Yes	Yes	Yes
Observations	1,345	1,345	1,345	1,345	1,345	1,345
Adjusted R-squared	0.008	0.176	0.198	0.205	0.205	0.204

Panel B: Baseline Specification with Alternative Duration Measures

	Duration	**DPPS**	**DVW**	**DEW**
Variables	**(1)**	**(2)**	**(3)**	**(4)**
HF_TREAT*POST	-0.218**	-0.415***	-0.414***	-0.402***
	(-2.128)	(-3.097)	(-3.072)	(-2.954)
HF_TREAT	0.168**	0.226**	0.220**	0.208**
	(2.248)	(2.097)	(2.071)	(1.980)
POST	0.049	0.109	0.120	0.096
	(0.392)	(0.672)	(0.741)	(0.572)
Firm and CEO Controls	Yes	Yes	Yes	Yes
Event and Year FE	Yes	Yes	Yes	Yes
Observations	1,345	1,345	1,345	1,345
Adjusted R-squared	0.205	0.178	0.191	0.181

Table 2.11 Pay Duration and Bushee Classification

This table presents the estimates of the effect of quasi-indexer, transient, and dedicated institutional investors on the pay duration. The institutional investor classification is based on Bushee (1998). Each column uses a different CEO pay duration measure. The variable definitions are given in Table 2.A.2 in the appendix. We use one-year lagged values of time-varying continuous control variables, except CEO characteristics. All regression specifications include Fama-French 48 industry fixed effects. t-values are reported in the parenthesis. Robust standard errors are clustered at Fama-French 48 industry level. ***, **, and * indicate significance at 1%, 5%, and 10% levels, respectively.

	Duration	DPPS	DVW	DEW
Variables	**(1)**	**(2)**	**(3)**	**(4)**
QIX_IO	0.284***	0.387***	0.400***	0.378***
	(3.942)	(3.885)	(3.823)	(3.939)
TRA_IO	0.044	-0.085	-0.127	-0.143
	(0.460)	(-0.632)	(-0.928)	(-1.021)
DED_IO	-0.345***	-0.359**	-0.335*	-0.386**
	(-2.992)	(-2.084)	(-1.846)	(-2.067)
Size	0.118***	0.046**	0.054***	0.052***
	(6.932)	(2.217)	(2.704)	(2.714)
Volatility	-0.165**	-0.563***	-0.616***	-0.604***
	(-2.290)	(-5.938)	(-6.781)	(-5.816)
Leverage	0.015	0.022	0.006	-0.040
	(0.214)	(0.228)	(0.058)	(-0.421)
Market to Book	0.057***	0.007	-0.002	-0.007
	(4.631)	(0.572)	(-0.173)	(-0.630)
Long-Term Assets	0.125*	0.071	0.087	0.102
	(1.715)	(0.721)	(0.888)	(1.037)
R&D	0.780**	0.202	0.153	0.151
	(2.082)	(0.354)	(0.258)	(0.262)
R&D is Missing	-0.054	0.005	-0.006	-0.001
	(-1.246)	(0.104)	(-0.099)	(-0.026)
Excess Return	0.036*	0.012	0.025	0.031
	(1.816)	(0.542)	(1.185)	(1.488)
Spread	-0.125***	-0.092**	-0.088**	-0.090**
	(-4.899)	(-2.269)	(-2.018)	(-2.037)
IO	0.199**	-0.024	-0.027	-0.010
	(2.073)	(-0.192)	(-0.201)	(-0.073)
CEO is BH	-0.367***	-0.533***	-0.515***	-0.496***
	(-5.434)	(-5.215)	(-4.721)	(-4.543)
CEO Age	-0.678***	-0.670***	-0.678***	-0.610***
	(-4.613)	(-3.292)	(-3.246)	(-3.010)
CEO Tenure	-0.046*	-0.044	-0.040	-0.042
	(-1.890)	(-1.430)	(-1.389)	(-1.460)
CEO is Chair	0.058**	0.048*	0.066**	0.061**
	(2.476)	(1.739)	(2.199)	(2.126)
Constant	2.206***	4.478***	4.439***	4.171***
	(3.739)	(5.881)	(5.834)	(5.464)
Industry and Year FE	Yes	Yes	Yes	Yes
Observations	13,758	13,758	13,758	13,758
Adjusted R-squared	0.090	0.055	0.057	0.055