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Essays on Executive Turnover

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by

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I dedicate this thesis to my mother, father and sister, whose unfailing support has been invaluable over the years.

ABSTRACT OF THE DISSERTATION

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Chapter 1 deviates from the conventional practice by highlighting an alternative to forced CEO turnover. An interesting puzzle in corporate finance is the week sensitivity of disciplinary action against CEO to poor firm performance. I show that this weak relation is in part driven by an overlooked alternative to firing, which in practice takes the form of splitting the CEO-Chairman role or demoting the incumbent CEO to the executive Chairman position. I first document that such demotions are a frequently used alternative disciplinary mechanism, accounting for nearly 40% of all involuntary CEO transitions. I further show that the use of this mechanism is concentrated among firms in which the CEO is most entrenched or the cost of firing its CEO is high, i.e. CEOs with firm or industry-specific managerial skills and those with strong long-term performance and weak governance. Market reactions to CEO demotions are positive, on average. Finally, I show that classifying CEO demotions as an alternative form of involuntary turnover magnifies the sensitivity of involuntary turnover to firm performance and eliminates the relation between performance and voluntary turnover.

In chapter 2, we examine the role of deferred vesting of stock and option grants in reducing executive turnover. To the extent an executive forfeits all unvested stock and option grants if she leaves the firm, deferred vesting will increase the cost (to the executive) of early exit. Using pay *Duration* proposed in Gopalan, et al., (forthcoming) as a measure of the length of managerial pay, we find that CEOs and non-CEO executives with longer pay *Duration* are less likely to leave the

firm voluntarily. Employing the vesting of a large prior-year stock/option grant as an instrument for *Duration*, we find the effect to be causal. CEOs with longer pay *Duration* are also less likely to experience a forced turnover and the sensitivity of forced CEO turnover to firm performance is significantly lower in firms that offer longer duration pay. Overall, our study highlights a strong link between compensation design and turnover for top executives.

Finally, in chapter 3, we develop and test a new explanation for forced CEO turnover. Investors may disagree with management on the optimal course of corporate actions due to heterogeneous prior beliefs. Such disagreement may be persistent and costly to firms, and thus create incentives for firms to replace CEOs who investors tend to disagree with. We use this logic to develop and provide evidence for three hypotheses. First, firms with higher investor-management disagreement are more likely to fire their CEOs, and this effect is more pronounced in more-financially-constrained firms as well as those with less-entrenched CEOs and stronger shareholder governance. Second, firms are more likely to hire an external CEO as a successor if investor-management disagreement with the departing CEO is higher. Third, investor-management disagreement declines following forced CEO turnover. Thus, the evidence sheds new light on how disagreement between management and investors shapes one important aspect of corporate governance—the replacement of CEOs.

Chapter 1: CEO-Chair Duality Split: An Alternative to Firing

1.1 Introduction

A puzzling stylized fact in the corporate governance literature is the surprisingly low sensitivity of disciplinary action against CEOs to firm performance. The literature has viewed "disciplinary action" as being synonymous with forced CEO turnover and measured it accordingly. The prior literature (for example, see Murphy (1999), and Kaplan and Minton (2012)) documents at most a 35% increase in the likelihood of forced CEO turnover for a one-standard deviation decline in a firm's industry-adjusted stock return. Taylor (2010) argues this corresponds to a well below-optimal rate,[1] and attributes this modest level of firing to entrenchment costs faced by firms. But, despite substantial changes in internal governance mechanism, the sensitivity of forced turnover to firm performance has not changed significantly over time (see Huson, Parrino, and Starks (2001)). Another puzzle is that "routine departures" of CEOs− say due to retirement (e.g., Parrino (1997))− appear to be negatively related to firm performance (see Kaplan and Minton (2012)). This is puzzling since voluntary turnover should not have any relationship to firm performance, or else such turnover should be classified as forced.[2] Kaplan and Minton (2012) attribute this to the misclassification of some forced turnovers as voluntary. In this paper, I show that these two puzzles are in part driven by an overly narrow definition of "disciplinary action", which leads to overlooking an alternative mechanism for disciplining an underperforming CEO,

[1] This corresponds to an average of 2% of CEOs of large public companies getting fired every year, whereas the optimal annual rate of CEO firings is estimated to be six times this figure (see Taylor (2010)).

[2] One possible hypothesis is that CEOs 'jump ships' when firm performance is good. However, this doesn't seem to be the case for two reasons. First, this hypothesis predicts a positive relationship with firm performance, while previous literature documents a negative correlation. Second, there aren't many instances of such jumps in my sample (56 in total) which suggest that the labor market for CEO is relatively immobile.

namely splitting the CEO-Chair role or demoting the incumbent CEO to the executive Chairman position.[3] In particular, I comprehensively study the use, implications, and effectiveness of this mechanism, and its impact on performance-turnover sensitivity.

In reality, firms need not always fire its CEO as a penalty for poor firm performance. Firms can reduce the day-to-day responsibilities of an underperforming CEO and put extra oversight on him by either splitting the CEO's dual role or by demoting the incumbent CEO to the executive Chairman position. Anecdotal evidence of this is plentiful. For instance, following months of poor returns, "Chesapeake Energy stripped its co-founder and chief executive officer, Aubrey McClendon of his chairmanship … to placate shareholders anger."[4] And more recently (May 15, 2014), the French cable maker Nexans split chairman and chief executive Frederic Vincent's dual role, citing underperformance since he took over, "although shareholders also rejected a motion to oust him."[5] Although some press articles report the transition of a CEO to the executive Chairman position as a "promotion", I label these transitions as well as CEO-Chair splits as demotions for two reasons. First, there is general consensus that the CEO is the company's top decision-maker, the one who runs the company and to whom all other executives report. Since the CEO is heavily involved in the strategic direction of the firm, all analyst reports, and the media usually emphasize how a CEO's management style and vision shape a firm's major decisions. As such, losing the CEO title (or the executive Chairman title) leads to loss of authority. Second, the average drop in

[3] In the analyses that follows, CEO-Chair splits that are a result of ``passing-of-batons'' are classified as retirements and thus, not flagged as CEO demotions. Please refer to Section 4 for more details.

[4] An excerpt from an article published on *Pittsburgh Post-Gazette* entitled "Chesapeake strips CEO of chairmanship" available via http://www.post-gazette.com/business/businessnews/2012/05/02/Chesapeake-strips-CEO-of-chairmanship/stories/201205020239. Firms typically do not cave in to shareholders' proposal to separate the duality role when the firm is performing well. For instance, an article in *Forbes* report that "approximately 200 shareholder proposals to split the CEO-Chairman roles were filed by shareholders in the last two years [2012 and 2013], and of the 200 proposals filed, only four non-binding proposals won shareholder approval. Further, of these four, none ended up splitting the chairman/CEO roles." For more detail, please refer to the article entitled "Combined Chairman/CEO Roles: Easier Than You Think" available via http://www.forbes.com/sites/robinferracone/2014/03/05/combined-chairmanceo-roles-easier-than-you-think

[5] An excerpt from an article on *Reuters* entitled "Cable maker Nexans splits chairman, chief executive roles" available via http://www.reuters.com/article/2014/05/15/nexans-shareholders-idUSL1N0O11HM20140515

the CEO's total compensation following such transition is $1.5 million on average, which corresponds to 30% of the CEO's ex-ante compensation.[6]

The question this paper confronts empirically is− if disciplinary action against the CEO is classified more broadly as including both forced CEO turnover as well as demotions, do we observe a sensitivity of disciplinary action to poor firm performance that is more in line with the theory? In addressing this question, I begin by documenting that CEO demotions are not rare events. By implementing a classification criterion that rules out the classification of obvious retirements as CEO demotions, I find that CEO demotions are a frequently used mechanism in response to poor firm performance, accounting for nearly 40% of all involuntary CEO transitions. From 2000 to 2010, S&P 1500 firms in the lowest quartile of the industry adjusted annual stock return (*Ind. adj. stock return*) distribution demoted their CEOs at an average rate of 1.79% per year. This compares to a rate of 0.85% per year for firms in the upper quartile.[7] The quantitative relationship between CEO demotions and firm performance is striking− I find that a one standard deviation decrease in *Ind. adj. stock return* is associated with 37.9% increase in the likelihood of CEO demotion. I find that the link between firm performance and the probability of CEO demotions is just as strong when alternative measures of firm performance are used.

Why would firms use this alternative disciplinary mechanism? In many cases, firing the CEO may be very costly for the firm. These costs often involve CEO entrenchment-related intangibles in addition to multi-million dollar severance payments. Taylor (2010) calculates these intangible costs (which includes board's perceived stress from making a management change) to approach $1 billion per firing. The entrenchment costs, combined with the direct cost of toppling

[6] When the CEO stays on the company's payroll as an executive Chairman (a well-compensated full-time position), the Chairman is deeply involved in the day-to-day operations. In most cases, the duties of the executive Chairman are clearly outlined, and the stated responsibilities covers numerous typical CEO functions such as corporate strategy and finance. For instance, when JDA Software demoted James D. Armstrong, they released a press statement reporting that Armstrong will remain "actively engaged in the business focusing on strategic planning, mergers and acquisitions, major product direction and key customer relationships."

[7] Please refer to Table 1.1 for a complete statistics.

the CEO (including severance pay and replacement cost), generate high total costs of firing CEOs. Firing an experienced CEO could also disrupt relationships with key customers and suppliers, be internally disruptive, and could prematurely end any strategic initiatives. However, maintaining the status quo with the CEO might threaten the firm's market share, and worst yet, the competitiveness of the firm. In such cases, CEO demotion may be the Board's optimal decision that minimizes the cost of disciplining the CEO, while also making a change that alters the status quo.

One potential source of cost firms face in firing the CEO is the loss of firm/industry-specific knowledge of the CEO. While poor performance often requires a change from the status quo, firing the CEO may result in the loss of specialized competencies and firm-specific knowledge. These can be especially valuable to retain when performance over the CEO's tenure has historically been strong.

Consistent with this conjecture, I find that the likelihood of CEO demotion following poor firm performance is significantly higher among CEOs identified as firm/industry specialists, using a novel measure constructed based on the CEO's lifetime work experience. I also find that CEOs with long history of good prior firm performance are more likely to be demoted (rather than fired) following a year of bad performance. Moreover, I find that the sensitivity of such demotion to firm performance is concentrated entirely among specialist CEOs, and CEOs with good prior performance histories. This is in sharp contrast to the case of forced CEO turnover− I find that the performance-turnover sensitivity, measured by forced CEO turnover, is entirely confined to non-specialist CEOs and CEOs with poor historical firm performance. These findings are robust to the introduction of controls for various factors previously documented to be related to CEO turnover as well to the inclusion of firm and year fixed effects.

Another potential factor that could influence a firm's decision to demote its CEO instead of firing him is the strength of its corporate governance. The Board of Directors would prefer not

to fire the CEO if the entrenchment cost is high. But at the same time, when faced with poor firm performance, the Board of Directors, will wish to cater to the demands of investors to penalize the under-performing CEO to fulfill their fiduciary duty to shareholders and maintain their reputation as experts. Prior studies (for example, see Fishman et al. (2014)) find that firms with weak corporate governance are less likely to be influenced by investor pressure and are, therefore, less likely to fire the underperforming CEO, especially when shareholders' signals about management ability are noisy. Firms with weak corporate governance also have relatively higher entrenchment costs and as such are more likely to benefit from retaining the incumbent CEO. These two reasons suggest that the likelihood of a firm to demote its CEO, in response to poor firm performance, should decrease as its corporate governance gets stronger. Using three proxies for the efficacy of corporate governance, I find results consistent with this conjecture. Irrespective of the proxies used, results suggest that the sensitivity of CEO demotion to firm performance is higher for firms with weaker corporate governance.

It is important to note that the CEO turnover classification algorithm used in previous studies (for example, see Parrino (1997)) classifies as "voluntary turnover" cases of CEO-Chair splits, where the departing CEO gives up the CEO title but remains as or becomes executive Chairman. Meanwhile, since CEO-Chair splits that involve the CEO giving up the executive Chairman title do not involve a CEO change, the common turnover algorithm fails to flag such transitions as turnovers. However, it is the Board of Directors who make the decision of whether or not to demote the CEO. As such, there is no reason to believe that such CEO transitions are in fact voluntary. Consistent with this argument, I find that the previously documented negative relation between firm performance and voluntary CEO turnover becomes statistically insignificant when CEO demotions are re-classified as involuntary turnovers. In addition, the sensitivity of involuntary CEO turnover to poor firm performance increases two-fold after such demotions are re-classified as involuntary turnovers. These findings suggest that the two previously highlighted

puzzles are in fact two sides of the same coin. On the one hand, some instances of turnover, classified as "voluntary" by the existing CEO turnover algorithm, are not truly voluntary. These are demotions that diminish the CEOs' authority and scope. Therefore, consistent with this intuition, I find a negative correlation of demotions with firm performance. On the other hand, the weak sensitivity of disciplinary action to poor performance documented previously is partly due to the narrow definition of disciplinary action as forced CEO turnover and the failure to capture CEO demotions as instances of involuntary turnover.

The majority of the extant literature on forced CEO turnover treats the Board's decision to oust the CEO as an isolated event undisturbed by competing events.[8] As such, the literature ignores competing events and estimate the cause-specific hazard of forced CEO turnover, either using the semi-parametric COX proportional hazard or logit models. This approach gives unbiased coefficients if the competing risks are rare (see Pintilie (2006)), or censoring due to competing events is independent of the occurrence of forced CEO turnover (Putter et al. (2007)). However, as discussed above, CEO demotions are a frequently used alternative disciplinary mechanism. Therefore, failing to account for the competing risk of CEO demotion leads to overestimation of the risk of forced CEO turnover and of performance-turnover sensitivity. One way of overcoming this potential problem is to adopt a competing risk methodology that directly factors in alternative mechanism available to the board when making the CEO retention decision. In doing so, I find the effect of firm performance on forced CEO turnover as well as on CEO demotion to be persistent even after factoring in the competing risk, although the economic magnitude decreases slightly.

I take several approaches to mitigate the concern that both firm performance and CEO demotion are related to an omitted variable, and thus the documented correlation may be spurious.

[8] An exception to this include Jenter and Kanaan (2014), Gregory-Smith et al. (2009), Hazarika et al. (2012), and Coates IV and Kraakman (2010), in which voluntary CEO turnover is treated as a competing risk. Evans et al.(2010) models turnover decision as a choice between three options: retain, replace but retain the CEO on the board, or fire the CEO without any future ties with the firm. I discuss their paper in detail further down the section.

First, I include industry (firm) fixed effects in all COX (linear probability) regressions to control for unobserved time invariant industry (firm) heterogeneity. In addition, I also include time fixed effects in all regression to control for the influence of time-varying variables not included in the model. Second, to address omitted variable issues that could be industry-time specific, I follow the Instrumental Variable approach outlined in Jenter and Kanaan (2014) by regressing the daily stock returns on daily industry returns to decompose firm performance into a predictable component caused by the peer group performance and a residual component attributable to the firm performance. The idiosyncratic component is then used as a proxy for firm performance in the second stage.

Third, to provide more supporting evidence for my main hypothesis, I exploit a quasi-natural experiment in which a group of firms experienced an exogenous increase in institutional ownership. The literature suggests that institutional investors play a significant role in corporate governance through different channels. They are generally involved in shareholder activism (see Gillan and Starks (2007)). Crane et al. (2014) suggest that even non-activist investors like index funds and ETFs have incentives to intervene and can influence corporate management through proxy voting and private communication with management if index tracking error constraints prevent them from selling their shares. Therefore, a greater concentration of share ownership in the hands of institutional investors may enable shareholders to exert more influence on corporate decisions. To this end, I examine how the turnover-demotion sensitivity changes in response to an exogenous increase in institutional ownership. If the demotion-performance relation is driven by an omitted variable, I do not expect a change in the performance-demotion sensitivity because the exogenous shock is unlikely to affect the omitted variable such as uncertainty. However, since I have shown that corporate governance is important for the performance-demotion sensitivity, my hypothesis predicts that an exogenous increase in institutional ownership (improvement in governance) will decrease this sensitivity.

To this end, I exploit the discontinuity in institutional ownership around the Russell 1000 and 2000 index cutoff during the annual rebalancing of the Russell indexes as a quasi-natural experiment. Since the selection of a company into Russell 1000 and 2000 index only depends on the end-of-May market capitalization, and since I restrict my analysis to a narrow bandwidth of firms around the index cutoff, it is unlikely that the annual reconstitution directly affects the performance-demotion sensitivity. All results provide strong evidence that omitted variable bias is not a serious concern.

Finally, since I use lagged explanatory variables in all of my analyses, the issue of reverse causality is fairly minimal. The positive market reaction at the announcement of CEO demotion further mitigates the concern. Nevertheless, the difficulty of fully identifying the effect naturally remains.

The rest of the paper is structured as follows. In sections 2 and 3, I discuss related literature and establish my testable hypotheses. Section 4 describes the data sample and variables used in the empirical tests. Section 5 presents the main empirical analysis on firm performance and CEO demotion. Section 6 test the impact of the composition of managerial skills, tenure-long performance, and the strength of corporate governance on performance-demotion sensitivity. Section 7 revisits performance-CEO turnover sensitivity. Section 8 describes and reports results from the quasi-experimental set up provided by the Russell Index reconstitution. Section 9 discusses the market reaction to the news of CEO demotion. Finally, section 10 concludes. Definitions of all variables appear in the Appendix.

1.2 Related Literature

This paper is closely related to a large literature on forced CEO turnover. An extensive literature on CEO turnover focuses on the sensitivity of forced CEO turnover to firm performance

(proxies include stock returns, return on assets, earnings, and earnings surprises).[9] The overall conclusion from these studies is that the sensitivity is weaker than what is predicted by theoretical models. Researchers, working in this area, have made some strides in exploring factors that affect a firm's decision to fire its CEO, factors that go beyond firm performance.[10] While the existing literature has focused almost exclusively on exploring various cost and benefits of firing a CEO, this paper highlights an alternative mechanism to discipline an underperforming CEO when faced with these costs and benefits.

This paper also contributes to the literature on relative performance evaluation (RPE). Prior studies on the relationship between peer group performance and forced CEO turnover have produced mixed results. Morch et. al (1989), Barro and Barro (1990), and Gibbons and Murphy (1990) find a strong evidence that industry performance is completely filtered from stock price performance when making CEO firing decision. Meanwhile, Warner et al. (1988), and Jenter and Kanaan (2014) find a strong evidence against RPE and conclude that peer firm performance is not completely filtered from CEO dismissal decision. I find that boards benchmark CEO performance against peer performance when deciding on CEO demotion and the result persist for a broader definition of the peer. The finding is broadly consistent with the theory that boards fail to filter exogenous peer performance when making CEO retention decision.

This study also sheds insight into the literature on corporate governance and CEO turnover. Economists, dating back to Adam Smith (1776) and Berle and Means (1932), have raised concern about CEOs using their discretion to benefit their private interests at the expense of shareholders if left unchecked. CEOs may also entrench themselves in their position if left unmarked, making it

[9] For example, see Coughlan and Schmidt (1985), Weisbach (1988), Warner et al. (1988), Murphy and Zimmerman (1993), Parrino (1997), Jenter and Kanaan (2014), Jenter and Lewellen (2010), and Kaplan and Minton (2012).

[10] Captured boards (Fisman et al.(2014)), investor-management disagreement due to heterogeneous prior beliefs (Huang, Maharjan, and Thakor (2014)), importance of firm-specific skills (Gopalan, Huang, and Maharjan (2014)), peer firm performance (Jenter and Kanaan (2014)), product market competition (Dasgupta et al. (2014)), and various CEO attributes such as optimism (Campbell et al. (2011)) are some of the factors that have been found to affect a firm's decision to oust its CEO.

difficult to fire them when their performance is poor (Sheifer and Vishny (1989)). Consistent with this theory, empirical evidence find that the turnover decision in firms with captured boards is relatively less sensitive to poor performance. While this highlights the importance of strong corporate governance, Fisman et al. (2014) argue that the observed lower performance-turnover sensitivity in entrenched firms could be a favorable outcome where unlucky CEOs are retained by entrenched boards who do not cater to the tastes of shareholders with noisy beliefs. My findings supplement this literature by providing suggestive evidence that firms with dysfunctional governance system are relatively more likely to retain the underperforming CEO.

Yet another strand of literature that is of immediate relevance to this study is the literature on the importance of managerial heterogeneity for corporate actions and executive compensation. Using a market based model, Murphy and Zabojnik (2007) posit that the increase in executive compensation observed over the past three decades is largely due to the increase in the importance of general managerial skills. Findings of Custadio et al. (2013) and Aivazian et al. (2009) are in line with this argument. Cappelli and Hamori (2008) document that firm-specific skills may be valued during growth, while general managerial skills may be valued during recessions. This is supplemented by the works of Eisfeld and Kuhnen (2009) and Gabarro (2010), who document a higher likelihood of external hire following forced CEO turnover. Custadio et al. (2013) document a higher likelihood of forced turnover for generalist CEO, but they find that this effect is not triggered by poor firm performance. However, neither the theoretical nor empirical studies have provided any guidance concerning which particular CEO abilities affect Boards' decision to split CEO duality role. I extend the extant literature by showing that firm-specific skills are an important factor that governs a firm's decision to demote its CEO, including its sensitivity to firm performance.

Lastly, this paper contributes to the recent policy debate on separating the CEO and Chairman role. Majority of extant literature on this topic mainly explores the merits and issues of

duality splits, but fail to provide a unified consensus. Advocates of splitting the roles of CEO and board Chair (which includes Jensen (1976, 1993), Fama and Jensen (1983), and Lipton and Lorsch (1992)) suggest that CEO duality increases agency costs since it will hinder Board's ability to monitor management. Meanwhile, proponents of vesting the two positions to one individual (for example, see Anderson and Anthony (1986), and Brickley et al. (1997)) argue that CEO duality reduces information cost by removing any ambiguity of accountability and responsibility for firm processes and allows an incentive mechanism to new CEOs during management transition. This paper opens a new dimension to this discussion by arguing that certain types of duality splits could serve as an alternative mechanism to discipline an underperforming CEO.

A recent paper by Evans et al. (2010) is similar in spirit to my analysis. Evans et al. (2010) characterize the Boards' decision as a choice between three options− retain the CEO, replace him as CEO but retain him on the board, or let him go and sever all ties. They exclude "passing of baton" cases by requiring the former CEO to remain on the board for at least two fiscal years after exiting as CEO. Using a sample consisting of firms covered by *ExecuComp* and spanning a time period of 1998 through 2001, they show that pre-turnover financial performance and greater bargaining power of CEOs are positively associated with the former CEO retaining a board position after leaving the CEO post.

My paper complements and differs from the above in multiple dimensions. First, my analysis focuses only on those CEO successions where the departing CEO either retains the executive Chairman title or becomes an executive Chairman. As such, my classification of CEO demotion excludes cases where the former CEO becomes an independent Chairman or a (executive) director. One such example would be the CEO succession of John H. Maxheim. When Maxheim left the helm of Piedmont Natural Gas Co. Inc. in March of 2000, he remained as the director for three years. Second, I also consider duality splits where the CEO abdicates the Chairman position but remains as CEO. Third, in addition to screening out obvious "passing of

baton" cases, I also filter out cases which are due to retirements. For instance, August Busch III retired as the CEO of Anheuser-Busch Cos. Inc. in July of 2002 but remained as an executive Chairman for four years following the succession. No prior announcements were made regarding his plans to leave the helm at Anheuser-Busch. However, the press reports convincingly explain the departure as a planned succession and as such, is not classified as a demotion. Fourth, any CEO successions which are classified as forced turnover are flagged as such in my study, even if the departed CEO remains as a director. And finally, I also address the effect of a CEO's tenure long performance on a firm's decision to demote its CEO.

1.3 Hypotheses Development

Firms, where the CEO also holds the Chairman title, typically do not cave in to investors' proposal to separate the dual role when the firm performance is good. For instance, out of approximately 200 shareholder proposals filed by shareholders during 2012 and 2013 to split the dual role of the CEO, only four non-binding proposals won shareholder approval. Further, of these four, none ended up splitting the role. However, when the firm performance is poor, maintaining the status quo might threaten the firm's market share and the competitiveness of the firm. Such situations may necessitate an extra oversight on the CEO. But, firing the incumbent CEO may not be optimal if the intangible costs associated with firing is substantial. In such situations, splitting the CEO-Chair role or demoting the incumbent CEO to the executive Chairman position may be the optimal course of action since such transition would result in the loss of CEO's authority, while also makes a change that alters the status quo. This leads to my first hypothesis:

Hypothesis 1: *Ceteris paribus, CEO demotion is more likely in firms with a lower level of firm performance.*

One potential source of switching cost firms face in firing the CEO may be the loss of firm/industry-specific knowledge of the CEO. The Board of Directors may demote an under-

performing CEO instead of firing him if he has specialized competencies that the firm highly values. They may also discipline the CEO for recent under-performance by demoting him instead of rocking the boat if he has proven his worth by delivering good firm performances throughout his tenure as CEO. The senior management shakeup at Nautilus Group Inc. in August of 2003 serves as a good anecdotal evidence. Gregg Hammann succeeded Brain Cook as the CEO of Nautilus Group Inc. in the wake of revenue revisions and ensuing drops of stock prices.[11] The Nautilus Group Board of Directors were quick to mention in a statement that Nautilus "has accomplished a great deal over the last 17 years during Brian Cook's tenure. The company's leadership position in the fitness and healthy lifestyle markets and financial strength are a true testament to the strength of our organization and Brian's leadership and vision during this time period."[12] Similarly, when Dan Warmenhoven relinquished his helm at NetApp in August of 2009, the press article stressed that "Warmenhoven has done an excellent job leading the company from the startup to a roaring tech hot shot. NetApp even noted in its press release that he also helped create the company's unique corporate culture."[13] The same press release also highlighted that NetApp "needs to return to growth after a battering from the global recession" and that the "new chief executive faces some challenges as he takes the helm."[14] This leads to my second hypothesis:

Hypothesis 2: *Ceteris paribus, the sensitivity of CEO demotion to poor firm performance is stronger for (a) CEOs with firm/industry specific skills, and (b) CEOs with good CEO tenure-long performance.*

Boards may prefer to demote the under-performing CEO in lieu of firing him if the above outlined intangible costs are high, and if boards, to some degree, want to cater to shareholders' demand, of penalizing the under-performing CEO, to maintain their reputation as

[11] Cook remained with the company as the executive Chairman.
[12] See "Nautilus' Troubles Lead to Shakeup" by *Primedia Business Magazines & Media Inc.* on August 1, 2003.
[13] See "NetApp's new CEO has plenty of Challenge" by *MarketWatch, Inc.* on August 19, 2009.
[14] The firm performances (measured by industry-adjusted stock return) of Nautilus and NetApp during Cook's and Warmenhoven's tenure as CEO (excluding the year prior to the split date), respectively, were in the top performance quartile in my sample. However, the firm performance a year prior to the split date was in the lowest quartile.

experts. However, boards' tendency to demote the under-performing CEO may be higher when the corporate governance of the firm is weak. This is because (1) prior literature has found that firms with strong corporate governance are more likely to be heavily influenced by shareholders and are more likely to fire underperforming CEO even when shareholders' beliefs about the management is inaccurate (Fisman et al. (2014)), (2) entrenched boards are more likely to benefit from retaining the CEO in an authoritative position, and (3) entrenched boards are more likely to retain good but unlucky CEOs by being less concerned about shareholders' perception of their decision, especially when shareholders' beliefs about the management are susceptive to noise (Fisman et al. (2014)). These reasons are not mutually exclusive. Therefore, following along this line of reasoning, I postulate that:

Hypothesis 3: *The performance-demotion sensitivity should be higher for firms with weaker corporate governance.*

My final hypothesis corresponds to the performance-CEO turnover sensitivity. The current CEO turnover classification algorithm classifies a CEO turnover that involves the CEO giving up the helm in favor of executive Chairman position as a voluntary turnover since the departing CEO stays with the firm as an executive Chairman ex-post of such split. Furthermore, since a duality split that involves the CEO giving up the executive Chairman title does not involve a CEO change, the current algorithm does not classify such split as a CEO turnover. However, to the extent that CEO demotion is an act of diminishing the CEOs' role and authority, regardless of the type, and that such decisions are made by the Board, these splits are not truly voluntary. And if, consistent with *Hypothesis 1*, CEO demotion is more likely to follow a poor firm performance, then classifying such splits as involuntary may weaken the sensitivity of voluntary turnover to firm performance. The same line of reasoning also implies that the sensitivity of involuntary CEO turnover to firm performance should be stronger once CEO demotions are factored in as involuntary turnovers. This leads to my final testable hypothesis:

Hypothesis 4: *Ceteris paribus, the sensitivity of (a) involuntary turnover to firm performance should increase, and (2) voluntary turnover to firm performance should decrease, once CEO demotions are classified as involuntary turnovers.*

I test these hypotheses in a large sample of public firms. In the next section, I describe my data sample.

1.4 Data and Description of Variables

1.4.1 Data and Sample

I draw data from a variety of sources. My sample construction starts with all U.S. firms in *COMPUSTAT* from 2000 to 2010 that list their common stock in *NYSE*, *NASDAQ*, or *AMEX*.[15] I require non-missing data on CEO characteristics (age, tenure, chairmanship, and stock ownership) from *ExecuComp*[16], firm-level accounting variables, and stock price and return data from *CRSP*.

- I identify CEO turnovers and CEO-Chair duality splits from *ExecuComp* and use news reports, *BoardEx*, and other public sources to classify the turnover as voluntary, forced, or duality splits.
- I obtain data on CEO age, tenure, chairmanship, and stock ownership from *ExecuComp* and whenever needed, supplement it with data from *BoardEx*.
- I obtain firm-level accounting data from *COMPUSTAT*, stock price and return data from *CRSP*, institutional ownership data from *CDA/Spectrum*, and board and director characteristics from *BoardEx*, and whenever needed, supplement it with data from *RiskMetrics*.

[15] My sample spans the year 2000-10 because the coverage of firms in *BoardEx* database begins from 2000 and testing *Hypothesis 2* requires tracking each CEO for two years into the future from the date of turnover or duality split.
[16] In instances where age and tenure data are missing from *ExecuComp*, I manually search *BoardEx* database to fill the void.

- I manually match CEOs in each year (as identified from *ExecuComp*) with profiles in the *BoardEx* database to have data on their characteristics including their past and future professional experience.
- I obtain analyst's earnings forecast data and actual earnings per share data from *I/B/E/S*.
- I obtain Russell index constituents, index weights, and proprietary float-adjusted market capitalization measures from Russell Investments for 1985-2012. Russell Investment only has its proprietary float-adjusted market capitalization data for 1998 and later.

The final sample consists of 2,430 unique firms and 18,817 firm-year observations. In some of the tests, I also use other auxiliary data sources; I mention these when describing the respective tests below.

1.4.2 Key Variable Construction

In this section, I describe the methodology I employ to identity CEO demotion and turnover. I start by identifying changes in CEO designations as documented in *ExecuComp*.[17] I then search Factiva, LexisNexis, social media, and the internet (in the respective order) for news reports coincident with the change in designation (and backdating it to two years) to identify the causes of the change. From the list of potential turnovers, I drop instances that are due to misclassification in *ExecuComp*, mergers, takeovers or spinoffs, bankruptcy filings, interim positions, sudden death, and turnovers of turnaround CEOs.

I start with using the criteria of Parrino (1997), with some modifications, to classify the turnover as voluntary or involuntary. All turnovers for which the press reports that the CEO is fired, is forced out, or departs due to difference of opinion, pressure from shareholders or union, or unspecified policy differences with the board are classified as forced. In addition, turnovers due to

[17] The earlier literature identifies the samples of CEO turnovers using Forbes annual compensation surveys (e.g. Borokhovich, Parrino, and Trapani (1996), Parrino (1997), Huson, Parrino, and Stark (2001), Huson, Malatesta, and Parrino (2004), Murphy and Minton (2008)). Most recent studies (e.g. Jenter and Kanaan (2014), and Falato, Li, and Milbourn (2014)) use the changes in the CEO position in *ExecuComp* to classify CEO turnovers.

the board not renewing the contract[18], and turnovers triggered by scandals, probes, or class action lawsuits are also flagged as forced. Of the remaining turnovers, if the departing CEO is under the age of 60, it is classified as forced if either (1) the reported reason for the departure does not involve death, poor health, or acceptance of another position elsewhere or within the firm (including the chairmanship of the board), or (2) the CEO is reported to be retiring but there is no announcement about the retirement made at least two months prior to the departure, and the CEO declines to make any comments. I then complement these criteria with few of my own. I reclassify a forced turnover (identified through the steps described above) as voluntary if either (1) the press doesn't specify any reason for the departure or there aren't any press reports on the departure, and the CEO's employment record, obtained from *BoardEx* and *Marquis Who's Who publications*, suggests that the CEO obtained a comparable position elsewhere within three months, or (2) the press reports convincingly explain that the departure is due to previously undisclosed personal or business reasons that are unrelated to the firm's activities, and/or the departing CEO steps forward to make comments regarding the departure. All the CEO successions not flagged as forced are classified as voluntary.

Of the turnovers identified as voluntary, I flag turnovers where the CEO, who is also the Chairman, abdicates the CEO title but remains as executive Chairman, or turnovers where the CEO renounces the CEO title and becomes executive Chairman. I, then, search *Factiva*, *LexisNexis*, and the internet (in the respective order) to see if such restructuring of the executive suit was pre-announced. Any such turnover which were pre-announced and the age of the departing CEO on the effective date of turnover is 60 or above are retained as voluntary turnovers. The remaining successions, where (1) the announcement coincided with the turnover date (i.e., the turnover was immediate), or (2) the first available press report on the succession occurred prior to the turnover date, and the age of the CEO on the effective date of turnover is under 60, are classified

[18] There are five such instances in my sample.

as CEO demotion if the press reports do not convincingly explain the succession to be a "passing-of-baton" or due to retirement. I manually cross-reference each CEO with profiles in the *BoardEx* database to ensure that they remain with the firm as executive Chairman for at least three years ex-post of the turnover date. Successions in which the departing CEO gives up the executive Chairman position within three years of being appointed in that position are re-classified as retirements. This is to ensure that retirements via "passing-of-baton" are excluded from being classified as demotions. There are all together 213 such demotions in my sample.

Lastly, I flag instances in *ExecuComp* where the CEO, who is also designated as executive Chairman in the previous year, no longer holds the Chairman position. In many instances, *ExecuComp* fails to identify such transitions. Therefore, for firms jointly covered by *ExecuComp* and *BoardEx* databases, I repeat the process using *BoardEx* database to identify such CEO transitions not otherwise identified via *ExecuComp*. I drop instances that are due to mergers and acquisitions. There are 35 instances of such demotions in my sample.

In panel A of Table 1.2, I assign each CEO turnover into a single category based on the reason cited or deduced from press reports. There are 1,552 qualified CEO departures in my sample, implying an annual departure rate of 8.25%. The frequency of departures assigned to each category is also reported. As expected, a substantial number of CEO departures (51.9%) are due to retirements. A fair number of CEOs (9.21%) leave the office due to performance related reasons. It is important to note that in many instances, the press does not explicitly mention the reason for departure to be performance related. The press reports either do not provide a reason or state that the CEO is leaving the office "to pursue other interests". In such cases, I carefully read all available press articles surrounding the turnover date and flag turnovers as performance related if there were any instances of negative press coverage on the firm's performance within three months a priori of the turnover date. 6.06% of CEO terminations are due to board, union, or shareholder pressure or due to strategic differences. 5.03% of CEO departures is accounted by scandals, accounting probes,

or class action lawsuits. For 4.06% of departures, I was either unable to find any article that covered the turnover story or the text describing the departure simply indicates that the CEO left the office but there was paucity of articles covering the firm prior to the turnover date for me to deduce any meaning reason for the turnover. Since I flag these turnovers as forced, I examine the proxy statements issued ex-post of the CEO departure date to see if any severance payment was made to the departing CEO. In all of the 63 cases, the departing CEO received some form of severance pay. Meanwhile, CEO demotions, with the departing CEO retaining the title of (or becoming) executive Chairman, account for 13.72% of all departures.

[TABLE 1.2 GOES HERE]

Panel B of Table 1.2 presents the overview of forced CEO turnover and CEO demotion. It shows the yearly distribution and frequency of forced CEO turnover and CEO demotions between 2000 and 2010. Out of 1,552 CEO successions that occur between 2000 and 2010, 381 (about 24.55% of all successions) are forced, while 248 (about 15.98% of all successions) are CEO demotions. Both, CEO demotion and forced CEO turnover, increase over time, except in the last two years of the sample coverage period, where it exhibits a decline. These statistics suggest that there exist some extent of time-series variations in the number and frequency of forced turnover as well as demotion, and that the nature of the CEO labor market has changed to a certain level over the last decade. I, therefore, include year fixed effects in all of my regressions to control for potential time effects.[19]

[19] Table IA1 (online internet appendix) reports the first ten industries (classified based on two-digit SIC code) with the highest frequencies of CEO demotion and forced CEO turnover in the overall sample. There seems to be a fair amount of overlap between industries that have the higher instances of forced CEO turnover and industries that experience the higher frequencies of CEO demotion. These figures provide the first clue that demotion occurs concurrently with forced CEO turnover and that some unobserved industry attributes are not behind a firm's decision to favor splitting the role over ousting the CEO.

1.4.3 Summary Statistics

Table 1.3 presents descriptive statistics of the key variables I use in my analysis. All variables are winsorized at the 1% and 99% level to mitigate the potential impact of outliers. These statistics are based on the regression sample and, as such, require non-missing value for all variables used in the baseline regression simultaneously. Detailed definitions of these variables are provided in the Appendix. There are all together 18,817 CEO-year observations in my sample. For some variables, the total count is less than 18,817 due to missing data in some years. The average age of a CEO in my sample is 55.04 years and have been in office as CEO for an average of 7.6 years. 24.9% of CEOs in my sample have at least 5% stock ownership in their firm. The mean (median) industry-adjusted annual stock return[20] in my sample firms is 0.8% (-4.5%) while the average (median) volatility is 0.476 (0.413). The average equal-weighted as well as value-weighted annual firm specific returns are comparable to the average industry-adjusted annual stock return and are 0.6% and 0.5% respectively. Meanwhile, the raw annual stock return has a mean (median) of 16.3% (7.7%). Average equal-weighted (value-weighted) industry return is 15.8% (19.9%), which is comparable to findings in the prior literature (see Jenter and Kanaan (2014)). The same pattern exists for equal-weighted and value-weighted industry specific return but is slightly lower. The average size of a firm (natural log of total assets) in my sample is 7.511. Since I obtain sample firms from *ExecuComp* which covers S&P 1,500 firms, the firm characteristics of my sample are not significantly different from those reported in the existing literature on CEO turnover that also uses *ExecuComp* as the primary data source.

[TABLE 1.3 GOES HERE]

Next, I conduct univariate tests to test the premise that CEO demotion tends to follow poor-firm performance. Panel A of Table 1.4 classifies CEO-year in my sample into those

[20] Industries are defined using the two-digit SIC code. All of my findings are robust to alternative definition of industries including using the Fama and French (1997) classification of firms into 48 industries.

involving CEO demotions and those not involving demotions. Here, I exclude forced CEO turnovers. I have 248 instances of demotions in my sample. I find a lower level of firm performance in the CEO demotion group, and the t-test conducted on the difference of the mean firm performance parameters shows that the difference is statistically significant for all but one measure of firm performance. Notably, the average industry return is higher before CEO demotion subsample. Differences in the average industry return between the two groups for all four measures are statistically significant. This suggests that CEO demotions are more common in industries that have performed relatively better and that peer performance might also trigger such splits. On average, CEOs who demoted have longer tenures and higher stock holding in their firm than those who either voluntarily left their post or remained as CEO. In regressions that explore the performance-demotion sensitivity, I include these variables as controls to ensure that they do not bias my conclusions.

[TABLE 1.4 GOES HERE]

Panel B reports results from the univariate test that assort the CEO years in the sample into those involving forced CEO dismissals and those involving CEO demotions. Notably, none of the firm and peer performance measures are statistically different across the two subsamples. This suggest that the state of the firm in terms of performance that triggers the Board to fire the CEO is no different from the level of firm performance that prompts them to demote the CEO. And the fact that demoted CEOs (compared to fired CEOs) are on average older, have had longer tenure, and more of them own 5% or more stake at their respective firms, suggest that the composition of managerial skills as well as entrenchment level may affect CEO retention decision.

1.5 Main Empirical Analysis

In this section, I test the effect of firm performance on CEO demotion in a multivariate regression setting so as to control for various firm and CEO characteristics that could affect a firm's decision to demote its CEO.

1.5.1 Baseline Analysis

Table 1.5 presents the coefficient estimates. I first employ the Cox proportional hazard model (Cox (1972)) to conduct the test since survival model is arguably more suitable to study the likelihood of an event taking place. Several prior literature (e.g., Hazarika, Karpoff, and Nahata (2012), and Jenter and Kanaan (2014)) implement Cox proportional hazard model to analyse the likelihood of forced CEO turnovers. The general form of the model is:

$$\lambda(t|X) = \lambda_0(\beta_0\, I * t)e^{\beta' X}$$

where $\lambda(t)$ represents the probability that CEO demotion occurs in year t (the observed hazard rate), conditional on such event not happening prior to t. λ_0 is the baseline hazard, and X is a matrix containing the variables that predict demotion. The hazard model accounts for both the occurrence and timing of demotion and allows for the inclusion of time-varying covariates. I allow baseline hazards to vary across industries to capture the difference in demotion patterns in different industries. I treat voluntary CEO turnovers as right-censored observations in the estimation. The primary variable of interest is the *Stock return* variable. *Hypothesis 1* predicts that it has a negative coefficient. The univariate test, from the previous section, suggest that industry performance have predictive power for the likelihood of CEO demotion. Therefore, I also include industry return as a control variable. In addition, I include a number of firm and CEO specific controls that prior literature has shown to affect the probability of CEO turnover. The firm-specific control variables I include are *Firm Size, Leverage,* and *Volatility*. The set of CEO characteristics I include are *Age, Tenure,* and *Stock ownership*. I add square of *Age* as an additional covariate to account for the non-

linear relationship of demotion with CEO's age. In all regressions, I include year fixed effects to account for time trends, and the standard errors are robust to heteroskedasticity and clustered at the firm level.

[TABLE 1.5 GOES HERE]

Columns 1, 3 and 5 of Table 1.5 present coefficient estimates from the Cox hazard model. Since coefficient estimates are reported instead of hazard ratios, a positive coefficient implies a positive marginal impact on the hazard and therefore, higher likelihood of CEO demotion. On the other hand, a negative coefficient implies a negative marginal impact on the hazard. Column 1 uses *Ind. adj. stock return* as the measure of firm performance, while columns 3 and 5 use *Raw stock return*. Since the earlier univariate analyses were supportive of the prevalence of peer performance benchmarking, I also include equal-weighted (value-weighted) industry returns as an additional control in column 3 (5). In all three models, stock return is negatively related to the hazard rate of CEO demotion and is statistically significant at 1% level. The coefficient estimates on *Stock return* is negative and the size of the coefficient indicates that the impact is economically significant. Conditional on the CEO not having experienced the event (demotion) as of the end of the last year, a percent decrease in stock return is associated with an increase in the hazard of demotion of 39-42% using coefficients in models 1, 3, and 5. This indicates that CEO demotion is more likely when the firm performance is poor.

Consistent with the results in the univariate test, the coefficient estimates on industry return are positive and statistically significant, which indicates that the effect of industry performance on the frequency of CEO demotions is economically large. Ceteris paribus, a percent increase in equal-weighted industry return is associated with slightly more than 300% increase in the hazard of CEO demotion. The effect of value-weighted industry return is slightly lower (115%). This statistics suggest that Boards take industry performance into account when assessing the retention decision. The relatively lower point estimates for *VW industry return* compared to *EW*

industry return suggest that Boards take the overall performance of their industry into account instead of benchmarking the firm performance against the largest and most visible firms in their industry. This is in sharp contrast to the findings on forced CEO turnover, where prior literature has found that Boards put relatively higher emphasis on the largest and most visible firms in their industry to form a benchmark (see Jenter and Kanaan, (2014)). For the coefficient estimates on control variables, I find that CEO demotion is less likely when the CEO owns a large portion of the firm's stock, and in firms with greater stock volatility. The magnitude and direction of the effect of these variables are consistent with findings on forced CEO turnover (e.g., see Gibbons and Murphy (1990), Barro and Barro (1990), and Jenter and Kanaan (2014)).

In columns 2, 4 and 6, I repeat my estimates employing a linear probability model. The reason for doing this is two-fold. First, employing the linear probability model helps estimate the economic significance of my results more easily and in an intuitive manner. Second, linear probability model allows me to include firm fixed effects to control for time-invariant firm characteristics. Firm fixed effects cannot be included in non-linear COX proportional hazard model because of the *incidental parameters problem* (see Neyman and Scott (1948)). I include *Ln(1+Tenure)* as an additional control since linear probability model does not account for the effect of tenure. The results parallel earlier findings. In particular, the negative coefficient on *Stock return* implies that a one standard deviation decline in *Stock return* results in an increase in the annual probability of CEO demotion by 31.56%. Similarly, one standard deviation increase in the industry return results in a 43% increase in the likelihood of CEO demotion.

Prior literature on CEO turnover implement logit model. To ensure readers that my results are not driven by my choice to use hazard model, I re-estimate all the regressions using logit model. The results from the logit regressions are consistent with those obtained using the COX and OLS models. I choose not to tabulate the results for brevity. The marginal effect of one percent decrease in *Stock return* (all other independent variables left at their mean value) on the implied

probability of CEO demotion is 0.5%. Meanwhile, a percent increase in *EW (VW) industry return*, holding rest constant at their mean value, leads to 1.1% (0.8%) increase in the implied probability of CEO demotion. The average probability of CEO demotion increases to 1.4% (1.3%) when stock return (industry-adjusted stock return) is at its lowest quintile, compared to 0.7% when the performance is at the highest quintile.

1.5.2 Instrumental Variable Approach

Thus far, results are in line with the main hypothesis that poorly performing firms are more likely to demote their CEOs. It also suggest that Boards consider peer group performance while making these decisions. However, since the models described above include both firm and peer performances in the same regression, it may not completely filter peer performance from the firm performance. The implication of this would be that the coefficient estimates on *Stock return* in the previous table might be underestimating the impacts of firm performance. To this end, I follow the procedure outlined in Jenter and Kanaan (2014) to estimate the sensitivity of CEO demotion to firm and industry performance using a two-stage regression approach. In the first stage, I regress the daily stock returns on daily industry returns (equal-weighted and value-weighted, separately) to decompose firm performance into a predictable (systematic) component caused by the peer group performance and an idiosyncratic component attributable to firm performance. This procedure is effectively an instrumental variable approach where the firm performance is instrumented by the peer group performance. Panel A of Table 1.6 shows results from the first stage. The coefficient estimates are similar to those in Jenter and Kanaan (2014). In the second stage, I re-run the baseline regression with *Stock return* and industry return replaced by the estimated idiosyncratic firm performance component and the estimated industry performance component from the first stage. The results are reported in panel B of Table 1.6.

[TABLE 1.6 GOES HERE]

The coefficient estimates on *Stock return* from both COX hazard and linear probability models are identical to those from the previous table and are statistically significant at 1% level. However, the coefficient estimates on predicted equal-weighted industry return component are slightly weaker in magnitude and in significance as compared to the point estimates from the previous table. The point estimates are 0.709 in the COX hazard model and 0.006 in the linear probability model. The point estimate on predicted industry return is of almost the same magnitude as in the previous table when industry returns are value weighted, although the significance level drops. These findings are in sharp contrast to the ones in forced CEO turnover literature. Jenter and Kanaan (2014) document a negative relationship between the likelihood of forced CEO turnover and the systematic component of firm performance.

1.5.3 Robustness Check: Alternative Measures of Firm Performance

Prior literature studying the performance-forced CEO turnover sensitivity suggests that firms may use different measures of firm performance in their decisions to oust the CEO. As for instance, Engle, Hayes, and Wang (2003) document an interesting cross-sectional variation in the weights placed on accounting-based and market-based firm performance measures, which they relate to the properties of these performance measures. In this section, I re-estimate the baseline model outlined above using alternative measures of firm performance. In particular, I examine whether my results are robust to using *Actual - mean forecasted EPS* and *Ind. adj. ROA* instead of stock return in the baseline regression. *Actual - mean forecasted EPS* is defined as the difference between a firm's earnings per share (EPS) and its mean analyst forecast. It proxies for the investors'/Boards' expectation of firm performance and as such, any deviation of the realized earnings from analysts' expectations is attributable, to a degree, to CEO performance. Prior literature on forced CEO turnover (see Puffer and Weintrop (1991), DeFond and Park (1999), Goyal and Park (2002), and Farrell and Whidbee (2003)) find an inverse relation between forced

CEO turnover and the realized earnings falling short of mean consensus earnings. On a different note, *Ind. adj. ROA* is defined as the net income scaled by the mean of lagged and current total assets, adjusted for the median ROA of the respective industry. This measure of performance provides an indication of a firm's recent accounting performance relative to the peer group performances. Extant literature provides ample evidence on the impact of ROA on forced CEO dismissals (e.g., see Huson, Parrino, and Starks (2001)). Therefore, to the extent that demotion is an alternative to firing, I hypothesize that the likelihood of CEO demotion will increase when (1) realized earnings fall short of the mean consensus, and (b) industry-adjusted level of accounting performance decline, controlling for other CEO and firm-specific attributes.

[TABLE 1.7 GOES HERE]

The point estimates are reported in Table 1.7. Both measures of firm performance are lagged by a year. In regressions that employ *Actual - mean forecasted EPS*, I also include analyst dispersion to control for difference in opinions among analysts. Irrespective of the alternative measures of firm performance used, the results support the hypothesis that poor firm performance increases firm's tendency to demote its CEO. Point estimates from linear probability model (column 2) indicates that a standard deviation decrease in *Actual - mean forecasted EPS* results in 25% increase the likelihood of CEO demotion. The negative point estimate of -0.693 on *Actual - mean forecasted EPS* obtained using COX hazard model (column 1) also suggest that the likelihood of demotion increases after bad firm performance. Using *Ind. adj. ROA* as firm performance measure yields similar results. The probability of demotion increases by 40.6% for one standard deviation drop in *Ind. adj. ROA*. This finding is augmented by the negative coefficient on *Ind. adj. ROA* from COX hazard model (column 3). The implied marginal probabilities of CEO demotion calculated from the logit models are -0.6% and -1.5% for one unit increase in *Actual - mean forecasted EPS* and *Ind. adj. ROA*, respectively (leaving the rest of the controls at their mean values). All in all, my earlier findings are robust and conclusions derived from earlier models remain intact in both

statistical significance and economic magnitude when alternative measures of firm performance are used.

1.6 Factors that Affect a Firm's Decision to Demote the CEO

Analyses in the previous section suggest that poorly performing firms may decide to retain the service of the incumbent CEO in the capacity of executive Chairman, or make him abdicate the Chairman position, instead of ousting him. CEO demotion is more attractive when the incumbent CEO has firm/industry-specific skills that are hard to acquire, replace and highly valued by the firm. CEO demotion is also more attractive if the CEO has performed well throughout his CEO tenure but the market conditions dictate the need for a new direction. In addition, the Board of Directors would prefer not to fire the CEO if the entrenchment cost is high. In this section, I formally test if the composition of managerial skills, CEO's tenure-long performance, and the strength of a firm's corporate governance are determinants of CEO retention decision.

1.6.1 Firm/Industry-Specific Skills

I construct a measure of managerial firm/industry-specific skills using data on CEOs' lifetime work experience in publicly traded firms prior to the year being considered. In particular, I consider the following five facets of a CEO's professional career[21]:

- Founder (*Founder*): Starting-up a firm and keeping it profitable/operational requires a comprehensive knowledge of the product market, and the industry. Therefore, I flag CEOs who are also the founder of the company.

[21] Custodio et al. (2013) also construct a measure of general managerial skills using a CEO's past number of positions, firms, industry in which he has worked, whether he held a CEO position at a different company, and whether he worked for a conglomerate. Although this measure yields significant results, I chose slightly different CEO attributes in constructing my version of firm/industry-specific measure because these are more relevant in this study.

- Serve as director of firms operating in the same industry (*Directorship Dummy*): Serving on the board of other firms operating in the same industry exposes the CEO to different industry-specific business environment. *Directorship Dummy* flags all CEOs who have only sat on the board of directors of firms that operate in the same industry prior to the observation year.
- Tenure with the firm: The longer the tenure of the CEO with the firm (in any capacity), the more firm/industry-specific skills he will amass.
- Number of firms previously worked in: A CEO who have worked for multiple firms is more likely to acquire general skills as opposed to firm-specific skills. This is also true for industry-specific skills if those firms (where the CEO was previously employed) operated in different industries.
- Number of industries previously worked in: A CEO who have worked in multiple industries is less likely to acquire a industry-specific skills and more likely to acquire general ability.

Since the above five CEO attributes are correlated, including all five variables in the same regression could lead to multicollinearity issues and minimize measurement error. Therefore, I use principal component analysis with orthogonal, varimax rotation method (see Kaiser (1958)) to create an aggregated firm/industry-specific ability (*specialist*) index. Panel A of Table 1.8 reports the loadings on the factor with an eigenvalue (1.852) higher than one and with easy to interpret loadings.[22] It has negative loadings on the first three CEO characteristics mentioned above, and positive loading on the last two. Thus, we can interpret a lower value of this index as measuring firm/industry-specific ability. The *specialist* index of a CEO in a given year is calculated by applying the scores to the variable. As a robustness, in addition to this continuous variable, I use a

[22] The second factor (with an eigenvalue of 1.16) had positive loading on all but one variable (*Directorship Dummy*). This is a typical problem with factor analysis. However, note that the principal factor with orthogonal, varimax rotation method produces completely independent factors.

dummy variable (s*pecialist index dummy*), which takes a value of one for CEO-year observations with an index value below the yearly 40th percentile and zero if it is above the 60th percentile.

[TABLE 1.8 GOES HERE]

Panel B reports coefficient estimates from COX proportional hazard model that re-estimates the baseline model with an additional explanatory variable− *specialist index dummy* in columns 1, and 3, and *specialist index* in columns 2, and 4. The coefficient estimates on both measures of firm/industry-specific skills have predicted signs and are statistically significant. In particular, for a CEO classified as a specialist, the hazard of CEO demotion increases by 137.8%, while the hazard of forced CEO turnover decreases by 40.9%.

Panel C presents frequencies of CEO demotion and forced CEO turnover for different quintiles of *specialist* index. Consistent with the prediction, the frequency of CEO demotion is the highest for the lowest quintile of the index and it monotonously decreases as we move towards the top quintile. The spread in the instances (frequencies) of demotion between the lowest and the highest quintile is 78 (2.14%). But then, the frequencies of forced CEO turnover is lowest for bottom quintiles, while it is significantly higher for the top two quintiles.

To test *Hypothesis 2(a),* firms with specialist CEOs (i.e. *specialist index dummy* = 1) are grouped and labeled as "Specialist", while firms with general ability CEOs (*specialist index dummy* = 0) are grouped and labeled as "Non-Specialist". Panel D reports coefficient estimates from COX proportional hazard model that re-runs the baseline regressions on the two groups of firms, respectively. I find that the previously documented effect of firm performance on CEO demotion is restricted to specialist CEOs. The point estimates on stock return for the non-specialist sample is not statistically different from zero. On the contrary, the performance-forced CEO turnover sensitivity is restricted only to non-specialist CEO subsample. The marginal effects calculated from logit regressions show an increase of 58% in the average implied probability of CEO demotion for

one standard deviation decrease in the *Ind. adj. stock return*. For specialist CEOs, the average probability of demotion is 1.3% when the firm performance is at the top quintile of firm performance, which increases to 2.6% when the firm performance is at the lowest quintile. For non-specialist CEOs, the corresponding average probabilities are 0.2% and 0.4%, respectively. Similarly, the increase in the average implied probability of forced CEO turnover for a standard deviation decrease in *Ind. adj. stock return* is 53% for non-specialist CEOs, while it is statistically indifferent from zero for specialist CEOs.

1.6.2 CEO Tenure Performance

My analyses, to this point, focuses on the effect of short-term firm performance on CEO demotion and implicitly assumes that Boards ignore firm performance (under the incumbent CEO) that are older than a year prior to the split date. However, Boards could very well take the CEO's entire performance history into account when evaluating him. Furthermore, his entire performance history could also govern the amount of weight Boards put on his recent performance when making retention decision.

To examine this (*Hypothesis 2(b)*), I factor in the CEO's entire performance history in the baseline regression. Following Jenter and Lewellen (2010), I define the CEO's tenure-long firm performance as the average abnormal return from his first month in office as CEO through the end of year t-2, where t is the year of duality split or turnover.[23] Therefore,

$$Tenure\ return_{\Lambda} = \frac{\sum_{k=1}^{E} w_{(k,\Lambda)} R_{E-k}}{\sum_{k=1}^{E} w_{(k,\Lambda)}}$$

where E is the total months the CEO has been in office as of the end of year t-2, R is the excess return in E-k month as CEO, and $w_{(k,\Lambda)} = \left(\frac{E-k}{E}\right)^{\Lambda}$ is the weighting function with the slope

[23] A similar weighting function is implemented in Malmendier and Nagel (2011) to calculate weighted average of past stock returns and inflation.

determined by the parameter Λ. It is in the Boards' discretion to decide if they want to equally weigh all past return or overweigh the more recent performances. To allow for this possibility, I use a range of values for the parameter Λ. Λ = 0 implies that boards place the same weight on performance in the most recent and distant past. Meanwhile, a higher values of Λ imply that boards put more emphasis on the most recent performance.

Table 1.9 presents the results. Panel A re-estimate the baseline regression of Table 1.5 with *Tenure return* as an additional control variable. For brevity, only results from COX hazard model are reported. The coefficient estimates on *Tenure return* is positive and statistically significant only for Λ={0,1}, which suggest that Boards factor in the entire history and past same weight on performance in the most recent and more distant place when considering the option of demoting the CEO. In contrast, the coefficient estimates of *Tenure return* in panel B (which is similar to Panel A but uses forced CEO turnover as the dependent variable) is negative, and the magnitude is higher for higher values of Λ, implying that the Boards consider multiple years of firm performance when making firing decision, but that they put higher weights on performance from the most recent past. Interestingly, the coefficient estimates of *Ind. adj. stock return* is statistically insignificant for higher values of Λ, which, again, stress that Boards consider more recent firm performance in addition to the performance a year prior to the turnover date in making the firing decision.

[TABLE 1.9 GOES HERE]

In Panel C, I divide the sample into quintiles based on the entire performance history of the CEO. The first three columns corresponds to Λ = 0, while the last three corresponds to Λ = 3. For Λ = 0, the frequency as well as the number of CEO demotions increases monotonously as we move from the lowest to the highest quintiles of *Tenure return*. The spread between demotion frequencies at the lowest and highest quintiles is 1.35%. The corresponding spread for forced CEO turnover is -1.42%. When Λ = 3, the monotonous increase in the instances of CEO demotion that was observed

in the previous case disappears. 1.41% of CEOs in the lowest quintile, and 1.39% of CEOs in the highest quintile were demoted, resulting in a spread of 0.02%. On the contrary, the corresponding spread for forced CEO turnover is more pronounced when $\Lambda = 3$, and is equal to -2.76%.

In Panels D and E, I test the above findings in a regression framework. Specifically, I run the baseline regressions separately on two subsamples of firms− one where the *Tenure return* is less than 40^{th} percentile in the sample, and the other with *Tenure return* greater than 60^{th} percentile.[24] The point estimates from COX hazard model, reported in panel D, suggest that the effect of firm performance on the likelihood of CEO demotion (documented earlier) is concentrated entirely in the subsample where the *Tenure return* (regardless of the value of Λ) is at the top two quintiles. The marginal effect of a standard deviation decrease in *Ind. adj. stock return* on the implied probability of CEO demotion is 32 − 53% for subsample of firms in the top two quintiles of *Tenure return*. Meanwhile, the performance-forced CEO turnover sensitivity is entirely concentrated in the subsample where the *Tenure return* is relatively bad. To put things into prospective, the marginal effect of a standard deviation decrease in stock return increases the implied probability of forced CEO turnover by 111 − 126% if the *Tenure return* is relatively bad.

1.6.3 Corporate Governance

I now test *Hypothesis 3*. In particular, I examine the impact of firm performance on CEO demotion based on the strength of firms' corporate governance. Since there are no ultimate measures of corporate governance, I proxy for the efficacy of corporate governance in three ways. First, I replicate the baseline analysis of Table 1.5 for two subsamples of the data divided based on the Corporate Governance Index constructed by Gompers et al. (2003).[25] This index is based on the prevalence of 24 corporate governance provisions at each firm published by the *Investor Responsibility Research Center (IRRC)*. These provisions are associated with the balance of power

[24] Using 50^{th} percentile as a cutoff works equally well.
[25] Data on this is available from Andrew Metrick's webpage.

between shareholders and management, and as such the GIM index proxies for the strength of shareholder rights. The higher the index score, the more tools the management has to resist shareholder activism, and therefore, the more restricted the shareholders are to discipline the management. Therefore, higher values of the GIM index are associated with weaker governance, while lower values of the index correspond to stronger corporate governance.[26] In my sample, the index ranges from 1 to 17, with 9 being the median. As such, I employ 9 as a cutoff to classify firms into strongly and weakly governed.

Second, I re-classify the sample into two subsamples based on the entrenchment index (E-index) constructed by Bebchuk et al. (2009).[27] E-index is composed of six out of the 24 provisions featured by the *IRRC*− staggered board, limit to shareholder amendments of bylaws, supermajority approval requirement for mergers and charter amendments, poison pill and golden parachute. Therefore, E-index is inversely related to the strength of shareholder rights. In my sample, the index ranges from 0 to 6, with the median value of 3. Thence, firms for which an E-index is less than 3 are classified as having weak governance, whereas firms with an E-index of 3 or more are classified as strongly governed.

Finally, I replicate the baseline analysis for two subsamples of the data divided based on the fraction of independent (outside) directors on the Board of Directors. To the extent that independent directors monitor the management more effectively, the presence of relatively higher proportion of outsiders on the Board should proxy for better governance. The median fraction of outsiders in my sample is 0.84 and as such the distribution of fraction of outsiders is right skewed.

[26] Following Gompers et al. (2003), I assume that firms have the same governance provisions as in the previous publication year during the years between two consecutive publications. The results are quantitatively similar if I assume that firms have the same governance provisions as in the next publication year. The same is true when E-index is used.

[27] Data on E-index is available from Lucian A. Bebchuk's webpage.

Therefore, I use 40th and 60th percentiles as cutoffs when dividing the sample into low and high fraction of outsiders subsample.

Table 1.10 reports the results. For brevity, I only report coefficient estimates from COX proportional hazard model and firm performance proxied by *Ind. adj. stock return*.[28] When either GIM- or E- indices are used, the point estimates on *Ind. adj. stock return* in both subsamples are negative and statistically significant. Notably, the magnitude of point estimates on stock return for the subsample of firms with weak governance is twice of those in the strong governance subsample. However, when fraction of outsiders are used as the proxy for the strength of corporate governance, only the coefficient estimates on *Ind. adj. stock return* for weakly governed firms are statistically significant. All in all, the results are consistent with *Hypothesis 3*.

[TABLE 1.10 GOES HERE]

1.7 Involuntary and Voluntary CEO Turnover

In this section, I re-estimate the sensitivities of forced and voluntary CEO turnover to firm and industry performances, after re-classifying CEO demotions as involuntary turnovers.

1.7.1 Involuntary CEO Turnover

To the extent that the act of diminishing the role of the CEO is a disciplining mechanism, and is enforced by the Board of Directors, CEO demotions are involuntary in nature. Therefore, in this section, I re-estimate the performance-forced CEO turnover sensitivity after classifying CEO demotions as involuntary turnovers.

[TABLE 1.11 GOES HERE]

[28] As in previous cases, the results are identical when linear probability or logit models are used.

Results reported in Table 1.11 are consistent with *Hypothesis 4(a)*. For an ease of interpretation, the coefficient estimates from linear probability models are tabulated. Models 1-3 estimate the likelihood of forced CEO turnover without accounting CEO demotions as involuntary. Meanwhile, models 4-6 re-estimate the performance-involuntary CEO turnover sensitivity after classifying demotions as involuntary. The sensitivity of involuntary CEO turnover to firm performance are stronger in the latter case, irrespective of the firm performance measured used. For instance, model 1 suggest that a standard deviation decrease in *Ind. adj. stock return* increases the probability of involuntary CEO turnover by 31.6%, while this sensitivity increases to 68.4% after demotions are factored in as involuntary turnovers . Similarly, a unit decrease in *Actual - mean forecasted EPS* is associated with 0.9% and 1.4% increase in the likelihood of involuntary CEO turnover before and after accounting for CEO demotions as involuntary turnovers, respectively.

1.7.2 Voluntary CEO Turnover

Studies that examine performance-voluntary CEO turnover sensitivity document a negative and statistically significant relation (see Kaplan and Minton (2012), and Jenter and Lewellen (2010)). This is a surprising stylized fact since voluntary CEO turnover, by definition, should be uncorrelated to firm fundamentals. Kaplan and Minton (2012) assert that the negative sensitivity of voluntary CEO turnover to firm and industry performances is due in part to a number of involuntary turnovers being classified as voluntary. In this study, apart from modifying the current classification criterion for forced CEO turnover, I also argue that firms may demote CEOs when faced with poor firm performance. The current CEO turnover classification algorithm classifies duality splits, where the departing CEO abdicates the CEO title, as voluntary turnovers. Furthermore, since CEO-Chair splits that involves the CEO giving up the executive Chairman title do not involve a CEO change, the existing turnover algorithm fails to flag such demotions as turnovers. To the extent that the decision to demote the CEO is made by the Board, there is no reason to believe that such splits are voluntary in nature. As such, I re-examine the sensitivity of

firm and industry performances on unforced turnover after factoring demotions as involuntary turnovers.

[TABLE 1.12 GOES HERE]

Table 1.12 reports the coefficient estimates from COX proportional hazard model.[29] The dependent variable is the voluntary CEO turnover which excludes forced CEO turnovers and CEO demotions. Not surprisingly, the point estimates of firm as well as industry performances are statistically insignificant. The results are robust to alternative measures of firm and industry performances.

1.7.3 Competing Risk Framework

Prior literature on forced CEO turnover treats Board's firing decision as an event that is undisturbed by competing events. As such, the extant literature ignore competing events and estimate the cause-specific hazard of the event of interest, either using the semi-parametric COX proportional hazard or logit models. This approach gives unbiased coefficients if either competing risks are rare (Pintilie (2006)) or censoring due to competing events is independent of the occurrence of the event of interest (Putter et al. (2007)). However, my above findings suggest that Boards have an alternative to firing the CEO− split the CEO-Chair role or demote the incumbent CEO to the executive Chairman position. Therefore, in the presence of this competing risk, the coefficient estimates from the standard COX hazard regression (and logit regression) could overestimate the risk of forced CEO turnover and thus, the coefficient estimates of firm performance may not reflect the actual sensitivity.

[29] Results are qualitatively similar when linear probability or logit models are used.

In this section, I adopt a competing risks methodology of Fine and Gray (1999)[30] to directly compare the above two alternative options available to the Board.[31] The risk of forced CEO turnover and demotion are competing in the sense that these events are mutually exclusive and thus, each event censors the other event. Competing-risks methodology proposed by Fine and Gray (1999) is quite similar to COX regression. However, unlike COX model which focuses on the survivor function for the event of interest, the semiparametric proportional hazard model of Fine and Gray (1999) focuses on the failure function, also known as the *cumulative incidence function*. Given covariates **X**, the model is of the following form:

$$\bar{\lambda}(t|X) = \bar{\lambda}_0(\beta_0\, I * t)e^{\beta' X}$$

where $\bar{\lambda}$ is the hazard of the subdistribution, interpreted as the hazard that generates the failure event of interest in the next period while being aware that the subject at risk has not experienced the event of interest but it might already have experienced the competing event. $\bar{\lambda}_0$ is the baseline subdistribution hazard for the event of interest. A positive (negative) point estimates imply a positive (negative) effect of increasing the covariate on the sub-hazard and thus, on the *cumulative incidence function*.

[TABLE 1.13 GOES HERE]

Results from the Fine and Gray regression model are displayed in Table 1.13. For brevity, only results for *Ind. adj. stock return* are tabulated. As expected, the coefficient estimate on *Ind. adj. stock return* is negative and statistically significant. The hazard of forced CEO turnover increases by 20.62% for one standard deviation decrease in stock return, taking into account the possibility that the Board could demote the CEO. Similarly, a standard deviation decrease in stock

[30] Multinomial logistic regression models can also be implemented in the presence of competing risks. However, the competing risk model of Fine and Gray (1999) is more suitable since multinomial logit models are less appropriate when data is censored or truncated, or when covariates are time dependent.
[31] I exclude voluntary turnover since these decisions are primarily made by the CEO and not by the Board.

return corresponds to 17.1% increase in the likelihood of CEO demotion, accounting for the possibility of CEO being fired.

1.8 Evidence from a Quasi-Natural Experiment

In this section, I explore how the sensitivity of CEO demotion to firm performance changes in response to an exogenous increase in institutional ownership. To this end, I exploit the quasi-experimental set up provided by the Russell Index reconstitution to estimate the effect of higher institutional investors on the performance-demotion sensitivity. If the above documented relation between firm performance and CEO demotion is driven by an omitted variable (such as uncertainty), then this sensitivity should not change following the Russell Index reconstitution since the annual reconstitution is unlikely to affect the omitted variable. However, prior literature find convincing evidence that institutional investors are relatively effective monitors of management because of their sophistication and lower-cost of monitoring (e.g., see Black (1991), and Gillan and Starks (2000)). As such, the previous analysis on corporate governance shows that it is important to the performance-demotion sensitivity.

I will first describe the construction of the Russell Index and how the annual reconstitution of the index leads to an exogenous shock to the institutional holdings. It is followed by a discussion of the empirical design, results, and a detailed discussion on the identification.

1.8.1 Russell Index Construction

Russell indexes are constructed using an open, published, market capitalization based methodology making it transparent and predictable. As of December 2012, "$4.1 trillion in assets are benchmarked to the Russell Indexes and more institutional funds track them than all other U.S. equity indexes combined."[32] It captures 98% of investable global equity. The broadest U.S. index

[32] "Russell U.S. Equity Indexes Construction and Methodology" (March 2014)

is the Russell 3000 index, which contains the largest 3000 U.S. companies. The members of the Russell 3000 index and its subsets are determined each year during annual reconstitution. On the last trading day in May each year, all eligible securities[33] are ranked by Russell Investments by their proprietary market capitalization measure. The first 1000 largest firms, based on stock market capitalization, fall under Russell 1000 and the remaining 2000 firms are classified into Russell 2000. The breakpoint of Russell 1000 and 2000 indexes is the rank of 1000th. Once the index membership is defined, the membership is locked for an entire year. That is, a stock that is classified as a member of Russell 1000 will remain in Russell 1000 for the entire year even if its market capitalization in the next twelve months (before the next reconstitution) falls below that of stocks in the Russell 2000. Likewise, a stock in the Russell 2000 will remain in the Russell 2000 index even if the market capitalization in the next twelve months exceeds that of stocks in the Russell 1000.

Annual reconstitution occurs on the last Friday in June. After membership to Russell 1000 and 2000 is determined, Russell adjusts security shares of firms to include only those shares that are available to the public. Unlisted share classes, IPO lock-ups, ESOP or LESOP shares, shares held by large corporate and private holdings, and government holdings are excluded during the free float adjustment.[34] The main objective of this "free float" adjustment is to exclude shares that are not available for purchase and is not part of the investable opportunity set. The float adjustment only determine which price to use in the case of multiple share classes and do not affect the membership of indexes.

[33] Companies with a total market capitalization of less than $30 million or companies with 5% or less shares available in the marketplace are not eligible for inclusion in Russell U.S. indexes. Furthermore, a stock with a closing price at or above $1 on its primary exchange on the last trading day in May will be ineligible for inclusion if the average of the daily closing prices from its primary exchange during the month of May is also less than $1.

[34] Please see "Russell U.S. Equity Indexes Construction and Methodology" (March 2014) for more on this and Russell indexes construction.

Once the index membership is determined, stocks in each index are assigned a value weighted index weight. The weights are determined by the float-adjusted market capitalization at the end of June. Since weights assigned to stocks in Russell 1000 and 2000 indexes are value weighted, the index weights for stocks just included in Russell 2000 (i.e. stocks in Russell 2000 just below the 1000th index cutoff) are significantly larger than the index weights for stocks just above the 1000th cutoff. It is because stocks that just missed being included to Russell 1000 will be compared to other small stocks in Russell 2000 index, while stocks that just made it to Russell 1000 index will be weighed against other relatively larger firms. Indeed, in the time period covered by our sample, the mean index weight for the top ten firms in Russell 2000 was 0.002, while the mean index weight for the bottom ten firms in Russell 1000 was 0.00004.[35]

The dollar amount and total number of products benchmarked to Russell 2000 is substantially larger than that for Russell 1000. Chang et al. (2014) report that the dollar amount benchmarked to Russell 2000 (Russell 1000) in 2002 was 198.2 billion (47.6 billion). The figures were 201.4 billion (90 billion) in 2005, and 291.4 billion (172.7 billion) in 2007. Similarly, the number of products benchmarked to Russell 2000 (Russell 1000) were 289 (29) in 2002, 275 (48) in 2005, and 511 (52) in 2007 (see Chang et al. (2014)). Therefore, when benchmarking is concerned, the Russell 2000 is more popular than the Russell 1000. This coupled with the fact that firms just excluded from Russell 1000 have a large index weight while firms just included into Russell 1000 have trivial weights suggest that most index funds or funds benchmarked to Russell 1000 or 2000 are unlikely to hold the smallest stocks but are likely to hold the largest stocks in the index in order to track it. This is because funds benchmarked to either of the two indexes can minimize the tracking error by simply holding stocks with the highest index weights while the exclusion of stocks with small index weights will have little to no real impact on performance

[35] Chang et al. (2014) report that the mean index weights for top 10 stocks in Russell 2000 that were just excluded from Russell 1000 to be 10 times the index weights of bottom 10 stocks just included in Russell 1000. However, their sample spans the period of 1996 to 2012 while ours spans 1984 to 2006.

metrics. (Roll (1992)). The popularity (based on total amount and total number of products benchmarked) and the higher index weights of largest firms in Russell 2000 relative to smallest firms in Russell 1000 are the primary vehicles that drives the differences in institutional ownership between these two groups of firms, which are otherwise identical in other firm characteristics.

[FIGURE 1.1 GOES HERE]

1.8.2 Empirical Design

The plausibly exogenous variation in institutional ownership I explore centers on the annual rebalancing of the Russell 1000 and 2000 indexes. Specifically, my empirical strategy utilizes the discontinuity in institutional ownership around the Russell 1000 and Russell 2000 index cutoff. To the extent that the shock to institutional ownership is exogenous and that the exclusion restriction is valid, I can investigate how the sensitivity of CEO demotion to firm performance evolve around the index breakpoint. Specifically, I will use specifications of the following form:

$$y_{it} = \beta_0 + \alpha_1 Stock\ Return + \alpha_2 Stock\ Return\ X\ R2 + \beta_1 R2 + \beta_2\ Rank + \beta_3\ R2\ X\ Rank + \Omega\ Controls + \gamma_t + \gamma_j$$

where the subscripts i, j, and t refer to the firm, the industry, and the time in years, respectively. *R2* is a dummy variable that takes a value of one if the firm is a member of Russell 2000 after reconstitution and is zero if the firm is listed as a member of Russell 1000 after reconstitution. *Rank* is the market capitalization ranking centered at zero around the 1000th cutoff. I include *Rank* and the interaction term between *Rank* and *R2* to identify variation near the index breakpoint. My discussion above implies that the difference in institutional ownership will be more pronounced among firms just excluded from Russell 1000 and firms just included in Russell 1000. Therefore, I isolate any discontinuity in the outcome variable around the index cutoff by controlling for any mechanical relationship of my outcome variables with market capitalization ranking on either side of the benchmark cutoff point and the distance to the breakpoint on either side. For the same reason,

i.e. to isolate any discontinuity near the threshold, all the regressions are estimated by triangular kernel weighted local linear regressions separately on either side of the threshold.[36] Although I run a local linear regression in a narrow bandwidth around the index cutoff to non-parametrically estimate the effect of institutional ownership on our outcome variables of interest, thus deeming it unnecessary to control for other variables or fixed effects, I do include a vector of control variables and industry fixed effects for robustness. All regressions include year and firm fixed effects to control for unobserved time-related effects. All standard errors are clustered at the industry level. The expectation from our test is that $\alpha_1 < 0 < 0$ and $\alpha_2 > 0$.

[TABLE 1.14 GOES HERE]

Table 1.14 reports the results. Ideally, we would like to focus only on firms that are similar in every firm characteristics but differ only in terms of total institutional holdings. This can be achieved by focusing on a narrow bandwidth of firms around the index cutoff point since the choice of bandwidth governs the number of firms to be included on each side of the cutoff. However, there is a tradeoff. A narrow bandwidth decreases bias as firms within that narrow bandwidth will be more alike. But, small bandwidth also means lower statistical power. As we widen the bandwidth, we gain additional statistical power but at a cost of increased bias. Since not all firms in Russell 3000 are covered by *ExecuComp*, I choose a bandwidth of 300 firms on each side of the threshold to have at least 1000 observations on both sides.[37] Panel A of Table 1.14 shows that most of the firm characteristics on either side of the cutoff are not statistically different. Panels B present the coefficients from local linear regressions (with triangle kernel) of the difference at the cutoff of the regression functions to the left and to the right of the Russell 1000/2000 threshold. As predicted, the coefficient on *Stock return* is negative, whereas the coefficient on the interaction term is

[36] Results are robust when rectangular kernel is used instead of triangular kernel.
[37] In an untabulated result, I use a bandwidth of 400 firms, in which both magnitude and significance decreases.

positive, suggesting that the effect of firm performance on demotion is stronger in weakly governed firms.

1.8.3 Identification

The validity of this natural experiment hinges on the fact that the exogenous shock to institutional holdings is driven only by index inclusion and not by differences in policy choices. Here, I provide discussions to mitigate threats to the identification strategy.

First, my identification strategy relies on the fact that firms' inclusion to Russell 1000 and 2000 indexes are not a function of firms' corporate policy. The fact that Russell indexes are constructed using an open, published, market capitalization based methodology makes it transparent and the assignment procedure a sole function of stock market capitalization. Given the nature of the index construction, reverse causality is out of the window. However, starting with its 2007 reconstitution, Russell implemented a banding methodology based on the market capitalization of the 1000^{th} largest firm to mitigate index turnover. Based on this "banding" policy, firms that were in the Russell 1000 (Russell 2000) in the previous year but rank below (above) the end of May breakpoint between the Russell 1000 and the Russell 2000 need not be slated to be moved to the Russell 2000 (1000). The move takes place only if an index member's market capitalization deviate far enough (outside of the cumulative 5% market capitalization range around the new 1000^{th} breakpoint) to warrant an index membership change. For example, a firm that is ranked 990 in year t-1 and 1020 in year t stays in Russell 1000 in year t if its cumulative market capitalization falls within the cumulative 5% of the market capitalization breakpoint. Therefore, starting with 2007, the assignment of firms into Russell 1000 and 2000 is not completely determined by the end of May market capitalization. Therefore, I end my sample period in 2006.

Second concern would be the possibility for firms to self-select into Russell 1000 and Russell 2000 indexes. Indeed, if firms are able to manipulate their end of May market capitalization,

especially firms around the breakpoint, then the shock to institutional holding will no longer be independent of firm policy. However, as pointed out by Chang et al. (2014) and Crane et al. (2014), there is no incentive for hedge funds to manipulate when there is a price impact of trading and firms cannot precisely control their ranking especially when other firms are also simultaneously manipulating. Since firms at the threshold are identical in terms of market capitalization, firms are very unlikely to be able to precisely control their rank relative to other firms near the cutoff (Crane et al. (2014). Even hedge funds have no incentive to unload their position in stocks that are in Russell 2000 but whose market capitalization at the end of May (before reconstitution) is enough to place it in Russell 1000. For instance, hedge fund holding stocks of firm A which was ranked 1001 in year t-1 and has a market capitalization rank of 999 at the end of May, may wish to short A. But shorting stock A will make the price of A fall causing its market capitalization to drop as well. If this drop in market capitalization is sufficient to move the stock of A above the rank of 1000, then it will remain in Russell 2000 index, making the shorting self-defeating (Chang et. al. (2014)).

Third, since I capitalize on the actual index assignment made by Russell, I can perfectly identify firms assigned to the Russell 2000 index (treatment group) and firms that are placed into Russell 1000 index (control group). Finally, since I are running a local linear regressions in a narrow bandwidth of firms around the 1000^{th} rank firm, ideally, I would like to have comparable firms on either side of the threshold. Although the shock to institutional ownership is a combined effect of index assignment and index weights received by each stock (as we have argued above), using index weight ranks instead of market capitalization rank may lead to the comparison of firms that are not identical in firm characteristics (as pointed out by Chang et al. (2014), Crane et al. (2014), and Mullins (2014)). This is because once index membership is determined by Russell based on end of May market capitalization data, the market capitalization is adjusted for free floats based on Russell's proprietary float calculation. Note that this adjustment does not re-assign firms

into indexes; the float adjustment only influences the weights received by each stock after the reconstitution. Nonetheless, the float adjustment could potentially shift the ranking of the firms within each index as compared to the market capitalization ranking. For example, a firm that is ranked 1000th based on market capitalization might be ranked 900th based on index weights received, meanwhile a firm ranked 500th based on market capitalization but with larger free float might end up being the lowest ranked firm in terms of index weights. Although possible, I don't see a large difference in ranks generated by market capitalization and index weights.[38] Panel A of Table 1.14 shows that most of the firm characteristics of firms on either side of the cutoff are not statistically significant. Although I include control variables as well as firm fixed effects in all regressions, in an untabulated results, I also remove all observations with unadjusted end of May market capitalization rank greater than 1300 and less than 700 and re-run all baseline regressions. The objective here is to remove any observation with large float adjustment so that we can ensure local continuity in potential firm characteristics around the threshold. The results are similar, which suggest that my identification strategy is unlikely to be invalid.

1.9 Market Reaction

Prior studies document a significant positive abnormal return following forced CEO turnover (see Furtado and Rozeff (1987), Denis and Denis (1995), Kang and Shivadasani (1996), and Huson et al. (2001)).[39] These results imply that Boards act in favor of shareholders' interest. However, if the CEOs' firm/industry specific skills are hard to acquire and replace, and the recent poor firm performance dictates a need for a fresh perspective going forward, then demoting the CEO might be the optimal course of action for shareholder's interest. In addition, CEO demotion, in theory, increase the Board's independence from management and thus, improve monitoring,

[38] I obtained Russell's proprietary end of May/June float-adjusted market capitalization data for 1998-2013.
[39] An exception to this is Jenter and Kanaan (2014) and Kind and Schlapfer (2011). Kind and Schlapfer (2011) find a positive abnormal return to forced turnovers of underperforming CEOs, while a negative market reaction to over performing CEO turnover.

accountability, and provides checks and balances in the boardroom, all of which are a welcome news to shareholders. Therefore, even an entrenched boards' decision to demote the CEO may be viewed positively by investors since it diminishes the CEO's authority and scope.

To assess market reactions to news of CEO demotion, I follow the standard event-study methodology. For each firm i, the abnormal return for the stock on day t, AR_{it}, is specified as:

$$AR_{it} = R_{it} - E(r_{it}|X_t)$$

where R_{it} and $E(r_{it}|X_t)$ are the actual and normal returns respectively for day t, and X_t is the conditioning information for the model. Assuming that stock returns follow a single factor market model, the abnormal return is defined as:

$$AR_{it} = R_{it} - \hat{\alpha}_i - \hat{\beta}_i R_{mt}$$

where R_{it} is the return on the stock of i^{th} firm on day t; R_{mt} is the return of a market index on day t, proxied with the *CRSP* value-weighted index. The coefficients $\hat{\alpha}_i$ and $\hat{\beta}_i$ are ordinary least squares estimates of coefficients from the single factor market model, estimated over a 250 trading-day period ending 11 days before the split announcement date. I employ different event windows that extends from two days before to two days after the split announcement date. I include days prior to the first announcement date to account for potential information leakage. The cumulative average abnormal return (CAR) over an interval $[T_1, T_2]$ is given by:

$$CAR_t = \frac{\sum_{i=1}^{N} \sum_{t=T_1}^{T_2} A_{it}}{N}$$

Table 1.15 reports the average CAR along with test statistics obtained from the Standardized Cross-Sectional test (Boehmer et al. (1991) and the nonparametric rank test introduced by Corrado (1989).[40] I drop 35 observations corresponding to CEO demotions because

[40] Non-parametric rank test does not rely on specific assumptions about the stock return distribution.

the announcement of such splits coincided with other firm/performance related announcements.[41] Panel A shows that the average CAR is significantly positive for all event windows considered. The average CAR is highest (1.46%) for the period covering two days before and two days after the day of press release, and is lowest (1.13%) for the period covering one day before and one day after the announcement date. Panel B (which corresponds to duality splits involving the CEO abdicating the Chairman title) shows that the average CAR is positive and statistically significant in all but one case. The return is highest (4.52%) for the event window [-2, +2], whereas it is statistically indifferent from zero for the event window [-1, +1].

[TABLE 1.15 GOES HERE]

I also investigate the cross-sectional variation of CARs around the announcement of CEO demotion, based on whether or not the CEO is a specialist. Interestingly, the positive CAR documented above is concentrated entirely among specialist CEOs. Figure 1.2 further illustrates this finding. These results suggest that the market, on average, views CEO demotions positively.

1.10 Conclusion

In this paper, I deviate from the conventional practice and analyze the role of poor firm performance on CEO demotion. I find that poorly performing firms have a higher tendency of demoting their CEOs. My results are robust to alternative firm performance measures, as well as to the inclusion of firm and time fixed effects. Such splits are more likely if the CEO has firm/industry specific skills and good performance throughout the tenure as CEO. Using the annual reconstitution of Russell indices as an exogenous shock to institutional holdings, I show that such splits are more pronounced among entrenched CEOs. Market reactions to such splits are positive on average. Finally, I revisit the sensitivity of voluntary and forced CEO turnover to firm

[41] The news were typically about poor firm performance and as such, inclusion of these observations decreases the significance of the results.

performance taking into account CEO demotions as involuntary turnovers. I find that (1) the previously documented negative performance-voluntary CEO turnover becomes statistically insignificant, and (2) the sensitivity of involuntary turnover to firm performance increases significantly, when demotions are classified as involuntary turnovers. These results suggest that the two puzzles documented in corporate governance literature are, in fact, two sides of the same coin. On the one hand, some voluntary turnovers are not truly voluntary− they are demotions to diminish the CEO's authority and scope. Therefore, it is natural that we see a correlation of voluntary turnovers with firm performance. And since these demotions are not captured as involuntary turnover, we find a weaker relation with poor firm performance.

All in all, the purpose of this paper is to take a step towards understanding the role of performance and the disciplinary action against the CEO. This study highlights an alternative to forced CEO turnover. Although this study only looks at the effect of poor firm performance on CEO demotion, other factors may play a role. For instance, in January of 2011, Eric Schmidt left the helm of Google Inc. to take on the role of executive Chairman at a time when Google reported "earnings that comfortably topped Wall Street forecasts".[42] However, the same press article reported that the management shake-up "comes as Google faces a new competitive challenge from Facebook, which has taken over from the search company as the internet's most-watched company. Google's failure to come up with a response to Facebook, despite several attempts at developing its own social networking services, has prompted the first deep-soul searching at the company, which has faced few direct challenges up until now." I leave it for future research to explore such potentially interesting factors that could lead to CEO demotion, and in the process, widen our understanding of the corporate governance mechanisms.

[42] An excerpt from an article in *Financial Times* entitled "Page becomes Google chief executive".

1.11 Appendix

1.11.1 Variable Definitions

- *Actual – mean forecasted EPS* is the difference between a firm's earnings per share (EPS) and its mean analyst forecast.
- *Age* is the age of the CEO (in years).
- *Age*^{2} is the square of *Age*.
- *Dispersion* is the standard deviation of raw (i.e. not split-adjusted) analysts' forecasts.
- *E index* is the Bebchuk et al. (2009) entrenchment index.
- *EW firm specific return* is the residual component from the regression that decomposes firm performance into a systematic component caused by peer group performance (equally weighted) and a firm-specific component. Put differently, we run the following regression: $r_{i,t-1} = \beta_0 + \beta_1 r_{peer\ group,t-1} + \varepsilon_{i,t-1}$. The $\hat{\varepsilon}_{i,t-1}$ term is the estimated idiosyncratic stock return.
- *EW industry return* is the equally-weighted average stock returns for all firms on CRSP from the same two-digit SIC industry as the sample firm. I exclude each sample firm from the construction of its industry benchmark to eliminate any artificial correlation.
- *EW industry specific return* is $\hat{\beta}_0 + \hat{\beta}_1 r_{peer\ group,t-1}$ from the above regression.
- *Firm size* is the natural log of the total assets of the firm.
- *Fraction of outsiders* is the fraction of outsiders on the firm's Board of Directors in a given year.
- *G index* is the Corporate Governance Index constructed by Gompers et al. (2003).
- *Ind. adj. ROA* is the net income scaled by the mean of lagged and current total assets, adjusted for the median ROA of the respective industry.

- *Ind. adj. stock return* is the two-digit SIC industry adjusted daily stock return compounded for the four quarters.
- *Leverage* is the total book value of debt normalized by the sum of the total book value of debt and market value of equity.
- *Ln(1+Tenure)* is the natural log of 1 + number of years the CEO was in office.
- *Raw stock return* is a dummy variable that takes a value of one if the fraction of outstanding shares owned by the CEO is greater than 5%, and 0 otherwise.
- *Volatility* is the volatility in the firm's stock return over the 12 months.
- *VW firm specific return* is the residual component from the regression that decomposes firm performance into a systematic component caused by peer group performance (value weighted) and a firm-specific component. Put differently, we run the following regression: $r_{i,t-1} = \alpha_0 + \alpha_1 r_{peer\ group,t-1} + \gamma_{i,t-1}$. The $\hat{\gamma}_{i,t-1}$ term is the estimated idiosyncratic stock return.
- *VW industry return* is the value-weighted average stock returns for all firms on CRSP from the same two-digit SIC industry as the sample firm. I exclude each sample firm from the construction of its industry benchmark to eliminate any artificial correlation.
- *VW industry specific return* is $\hat{\alpha}_0 + \alpha_1 r_{peer\ group,t-1}$ from the above regression.

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1.11.3 Figures and Tables

TABLE 1.1

CEO Demotions and Forced Turnover by Performance Level

The following table shows the number and the frequency of (i) CEO-Chair duality splits where the incumbent CEO leaves the post of CEO but remains as (or becomes) the executive Chairman, (ii) CEO-Chair duality splits where the incumbent CEO abdicates the Chairman position but retains the CEO position, and (iii) forced CEO succession, in the subsample of firms divided based on the firm performance (industry adjusted stock performance). The sample consists of all firms between 2000 and 2010 covered mutually by ExecuComp, Compustat and CRSP databases. Successions due to mergers, spin-offs, bankruptcies and interim appointments are excluded.

Quartiles	**Demoted to exec. Chairman position (%)**	**Retained CEO title but left as Chairman (%)**	**Forced successions (%)**
1 (Lowest)	66 (1.44%)	16 (0.35%)	161 (3.47%)
2	68 (1.48%)	7 (0.15%)	97 (2.12%)
3	42 (0.91%)	9 (0.20%)	58 (1.26%)
4 (Highest)	37 (0.79%)	3 (0.06%)	65 (1.39%)
Total (average %)	213 (1.16%)	35 (0.19%)	381 (2.06%)

TABLE 1.2

CEO Turnover Classification

The sample consists of 1,552 CEO successions between 2000 and 2010 for firms whose CEOs are covered by *ExecuComp*, *Compustat* and *CRSP* databases. Panel A presents reasons (based on the Factiva news database search) behind each CEO turnover. Successions due to mergers, spin-offs, bankruptcies and interim appointments are excluded. Panel B shows the number and the frequency of CEO demotions and forced CEO successions in the sample. Classification of each succession into demotions or forced is based on press releases surrounding the succession.

Panel A

Reasons	# of turnovers
Voluntary	
CEO Jump	56
Death/Health related	47
Family Related	7
Retire	805
Within Firm Shuffle	27
Resigned due to personal reason	14
Forced	
Board/Shareholder/ Union Pressure or Strategic Differences	93
Contract not renewed	5
No reason available	62
Performance Related	143
Scandal/Probe	78
Demoted to Chairman Position	
Became Executive Chairman	36
Duality Split	177
Total	1,552

Panel B

Year	Demoted to exec. Chairman position (%)	Retained CEO title but left as Chairman (%)	Forced successions (%)
2000	15 (1.02%)	0 (0.00%)	53 (3.54%)
2001	15 (0.98%)	2 (0.13%)	25 (1.62%)
2002	19 (1.20%)	1 (0.06%)	46 (2.90%)
2003	19 (1.15%)	2 (0.12%)	28 (1.69%)
2004	24 (1.49%)	6 (0.38%)	35 (2.15%)
2005	20 (1.27%)	7 (0.45%)	36 (2.27%)
2006	25 (1.44%)	2 (0.12%)	34 (1.96%)
2007	21 (1.13%)	4 (0.22%)	33 (1.77%)
2008	31 (1.70%)	3 (0.17%)	44 (2.44%)
2009	14 (0.78%)	5 (0.28%)	25 (1.38%)
2010	10 (0.56%)	3 (0.17%)	22 (1.22%)
Total	213 (1.16%)	35 (0.19%)	381 (2.06%)

TABLE 1.3

Summary Statistics

The following table presents descriptive statistics of the sample. All variables are defined in Appendix. All continuous control variables are winsorized at 1 and 99 percent level. *, **, and *** indicate significance at 10%, 5%, and 1%, respectively.

	N	Mean	SD	Median
Firm performance measures				
Ind. adj. stock return	18817	0.008	0.526	-0.045
Raw stock return	18817	0.163	0.601	0.077
EW firm specific return	18817	0.006	0.522	-0.05
VW firm specific return	18817	0.05	0.527	-0.015
Actual - mean forecasted EPS	15787	-0.083	0.352	-0.008
Ind. adj. ROA	16998	0.039	0.123	0.026
Industry performance measures				
EW industry return	18817	0.158	0.389	0.114
VW industry return	18817	0.199	0.388	0.17
EW industry specific return	18817	0.157	0.361	0.115
VW industry specific return	18817	0.113	0.244	0.095
CEO characteristics				
Age	18817	55.043	7.174	55
Tenure (in years)	18817	7.575	7.32	5.332
Stock ownership (dummy)	18817	0.249	0.432	0
Firm characteristics				
Volatility	18817	0.476	0.249	0.413
Firm size	18817	7.511	1.747	7.363
Leverage	18817	0.344	0.281	0.325
Dispersion	16037	0.22	0.262	0.124
Number of segments	15879	2.723	1.829	3
Board characteristics				
Fraction of outsiders	16608	0.825	0.096	0.857
G index	11991	9.22	2.576	9
E index	12105	2.485	1.286	3

TABLE 1.4

Univariate Test

The following tables present univariate evidence of the relation between relatively poor firm performance and CEO demotion/turnover. Panel A pertain to subsamples of CEO demotions, while panel B pertain to subsamples of forced CEO turnover. In each panel, the sample is further segmented into two groups of CEO demotion/turnover versus non-turnover years. T-test is conducted on the difference between the two groups, which is reported in the last column. *, **, and *** indicate significance at 10%, 5%, and 1%, respectively. All variables are defined in Appendix. All continuous control variables are winsorized at 1 and 99 percent level. *, **, and *** indicate significance at 10%, 5%, and 1%, respectively.

Panel A

	Demotion years		**Non-Demotion years**		
	N	**Mean**	**N**	**Mean**	**Difference**
Ind. adj. stock return	248	-0.085	18101	0.012	-0.097***
Raw stock return	248	0.101	18101	0.165	-0.064*
EW firm specific return	248	-0.086	18101	0.01	-0.096***
VW firm specific return	248	-0.043	18101	0.054	-0.097***
EW industry return	248	0.196	18101	0.157	0.039
VW industry return	248	0.252	18101	0.198	0.030**
EW industry specific return	248	0.191	18101	0.155	0.036
VW industry specific return	248	0.146	18101	0.112	0.034**
Age	248	55.768	18101	55.069	0.700
Tenure	248	10.606	18101	7.562	3.045***
Stock ownership (dummy)	248	0.423	18101	0.248	0.175***

Panel B

	Demotion years		**Forced turnover years**		
	N	**Mean**	**N**	**Mean**	**Difference**
Ind. adj. stock return	248	-0.085	381	-0.127	0.042
Raw stock return	248	0.101	381	0.062	0.039
EW firm specific return	248	-0.086	381	-0.127	0.041
VW firm specific return	248	-0.043	381	-0.079	0.036
EW industry return	248	0.196	381	0.196	0
VW industry return	248	0.252	381	0.261	-0.009
EW industry specific return	248	0.191	381	0.191	0
VW industry specific return	248	0.146	381	0.152	-0.006
Age	248	55.768	381	52.877	2.891***
Tenure	248	10.606	381	5.507	5.101***
Stock ownership (dummy)	248	0.423	381	0.152	0.271***

TABLE 1.5

Main Empirical Analysis

This table presents coefficient estimates from COX proportional hazard model and linear probability model that examine the likelihood of CEO demotions. Time-to-turnover is right censored. Industry adjusted stock return is employed in columns 1 and 2, while annual unadjusted stock return is used in columns 3-6. Other explanatory variables are defined in Appendix. Robust standard errors are clustered by firm and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% respectively.

	Ind. adj. stock return		Raw stock return			
	(1)	(2)	(3)	(4)	(5)	(6)
	COX	OLS	COX	OLS	COX	OLS
Stock return	-0.536***	-0.006***	-0.515***	-0.006***	-0.490***	-0.006***
	(0.146)	(0.002)	(0.137)	(0.002)	(0.136)	(0.002)
EW industry return			1.118***	0.011**		
			(0.357)	(0.005)		
VW industry return					0.773***	0.010***
					(0.299)	(0.004)
Age	-0.041	-0.000	-0.038	-0.000	-0.043	-0.000
	(0.097)	(0.003)	(0.096)	(0.003)	(0.096)	(0.003)
Age^2	0.000	-0.000	0.000	-0.000	0.000	-0.000
	(0.001)	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)
Ln(1+ Tenure)		0.026***		0.026***		0.026***
		(0.003)		(0.003)		(0.003)
Stock ownership	0.537***	0.022***	0.543***	0.022***	0.532***	0.022***
	(0.151)	(0.005)	(0.152)	(0.005)	(0.151)	(0.005)
Volatility	0.997***	0.003	1.051***	0.004	0.987***	0.003
	(0.366)	(0.007)	(0.365)	(0.007)	(0.363)	(0.007)
Firm Size	0.066	0.001	0.071	0.001	0.066	0.001
	(0.050)	(0.004)	(0.050)	(0.004)	(0.050)	(0.004)
Leverage	-0.343	-0.001	-0.346	-0.001	-0.329	-0.000
	(0.284)	(0.008)	(0.283)	(0.008)	(0.285)	(0.008)
Constant		-0.027		-0.032		-0.033
		(0.092)		(0.092)		(0.092)
Observations	18349	18349	18349	18349	18349	18349
Adjusted R^2		0.034		0.034		0.034
Pseudo R^2	0.032		0.033		0.032	
Fixed effects	Time & Industry	Time & Firm	Time & Industry	Time & Firm	Time & Industry	Time & Firm

TABLE 1.6

Instrumental Variable Approach

Panel A presents the coefficient estimates from the first stage regressions that uses industry stock returns to predict firm stock returns. Column 1 uses equally weighted industry return while column 2 uses value weighted industry stock return. Panel B presents coefficient estimates from COX proportional hazard model and linear probability model that examine the likelihood of CEO demotions using the predicted values and residuals from the first stage regression as estimates of peer-group performance and idiosyncratic component of firm stock performance, respectively. All controls used in the baseline model are also included but is not reported for brevity. Robust standard errors are clustered by firm and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% respectively.

Panel A

	(1)	(2)
EW industry return	0.927***	
	(0.008)	
VW industry return		0.628***
		(0.007)
Constant	0.010***	-0.012***
	(0.003)	(0.003)
Adjusted R^2	0.106	0.065

Panel B

	(1)	(2)	(3)	(4)
	COX	OLS	COX	OLS
EW firm specific return	-0.510***	-0.006***		
	(0.140)	(0.002)		
EW industry specific return	0.709*	0.006		
	(0.387)	(0.005)		
VW firm specific return			-0.508***	-0.007***
			(0.144)	(0.002)
VW industry specific return			0.764	0.011*
			(0.474)	(0.006)
Observations	18349	18349	18349	18349
Adjusted R^2		0.034		0.034
Pseudo R^2	0.033		0.032	
Fixed effects	Time & Industry	Time & Firm	Time & Industry	Time & Firm

TABLE 1.7

Alternative Measures of Firm Performance

This table presents coefficient estimates from COX proportional hazard model and linear probability model that examine the likelihood of CEO demotions. Time-to-turnover is right censored. Actual - mean forecasted EPS is used in columns 1-2, while industry adjusted Return on Assets (ROA) is employed in columns 3-4. Other explanatory variables are defined in Appendix. Robust standard errors are clustered by firm and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% respectively.

	(1) COX	(2) OLS	(3) COX	(4) OLS
Actual - mean forecasted EPS	-0.475*	-0.006*		
	(0.283)	(0.003)		
Ind. adj. ROA			-1.274**	-0.033**
			(0.587)	(0.016)
Age	-0.165	-0.003	-0.048	0.000
	(0.101)	(0.004)	(0.099)	(0.004)
Age^2	0.001*	0.000	0.000	-0.000
	(0.001)	(0.000)	(0.001)	(0.000)
Ln(1+ Tenure)		0.028***		0.029***
		(0.003)		(0.003)
Stock ownership	0.556***	0.024***	0.599***	0.024***
	(0.168)	(0.005)	(0.155)	(0.005)
Volatility	1.423***	0.010	0.741*	-0.001
	(0.407)	(0.009)	(0.386)	(0.008)
Firm Size	0.097*	0.001	0.076	0.002
	(0.056)	(0.004)	(0.050)	(0.004)
Leverage	-0.447	-0.004	-0.269	0.002
	(0.304)	(0.010)	(0.296)	(0.009)
Dispersion	-1.147**	-0.009*		
	(0.490)	(0.006)		
Constant		0.043		-0.051
		(0.109)		(0.102)
Observations	15376	15376	16569	16569
Adjusted R^2		0.031		0.031
Pseudo R^2	0.036		0.022	
Fixed effects	Time & Industry	Time & Firm	Time & Industry	Time & Firm

TABLE 1.8

Firm/Industry Specific Skills

Panel A presents the results of applying principal component analysis to five proxies of specialist based on CEOs' past work experience. The index is calculated by applying the scores to each components. Panel B re-estimates the baseline regression (COX regression) with an additional control variable: Specialist index dummy (columns 1, and 3), and Specialist index (columns 2, and 4). Panel C summarizes the number and frequency of CEO demotion and forced CEO turnover in different sub-samples. Finally, panel D presents coefficient estimates from COX proportional hazard model that examine the likelihood of CEO demotion (columns 1 and 2), and forced CEO turnover (columns 3 and 4) in a subsample of firms divided based on whether the CEO is a specialist. All controls used in the baseline model are also included but is not reported for brevity. Time-to-turnover is right censored. All explanatory variables are defined in Appendix. Robust standard errors are clustered by firm and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% respectively.

Panel A

	Founder	**Directorship Dummy**	**Tenure with the firm**	**# of firms previously worked in**	**# of industries previously worked in**
Loadings	-0.273	-0.020	-0.600	0.882	0.825
Scores	-0.059	-0.173	-0.217	0.462	0.503
Eigenvalue			1.852		

Panel B

Dependent Var:	**CEO Demotion**		**Forced Turnover**	
	(1)	(2)	(3)	(4)
Ind. adj. stock return	-0.585***	-0.523***	-0.466***	-0.484***
	(0.163)	(0.144)	(0.146)	(0.134)
Specialist index dummy	0.866***		-0.526***	
	(0.180)		(0.130)	
Specialist index		-0.495***		0.118***
		(0.131)		(0.044)
Observations	14288	17889	14396	18024
Fixed effects	Time & Industry	Time & Industry	Time & Industry	Time & Industry

Panel C

	Demoted to exec. Chairman position	**Retained CEO title but left as Chairman**	**Forced turnover**
1 (lowest)	2.67% (97)	0.26% (9)	1.49% (53)
2	1.19% (43)	0.14% (5)	1.30% (47)
3	0.88% (32)	0.22% (8)	1.63% (59)
4	0.62% (22)	0.17% (6)	3.11% (112)
5 (highest)	0.53% (19)	0.17% (6)	2.67% (97)

Panel D

Dependent Var:	**CEO Demotion**		**Forced Turnover**	
	(1)	(2)	(3)	(4)
	Specialist	Non-Specialist	Specialist	Non-Specialist
Ind. adj. stock return	-0.709***	-0.364	-0.417	-0.456***
	(0.213)	(0.226)	(0.271)	(0.162)
Observations	7208	7079	7161	7235
Fixed effects	Time & Industry	Time & Industry	Time & Industry	Time & Industry

TABLE 1.9

CEO Tenure Performance

Panel A re-estimates the baseline regression with an additional control variable: Tenure return$_\Lambda$, which for a CEO in year t is the weighted average of the monthly excess return from the CEO's start in office to the end of t-2. Slope of the weighting function is governed by Λ, where Λ=0 implies that Tenure return is equally weighted while higher values of Λ implies that return from months further away from current year are given smaller weights. Panel B is similar to panel A but uses forced CEO turnover as the dependent variable. Panel C summarizes the number and frequency of CEO demotions and forced CEO turnover in different sub-samples. Panel D (and E) presents coefficient estimates from COX proportional hazard model that examine the likelihood of CEO demotion (forced CEO turnover) in a subsample of firms divided based on Tenure return. All controls used in the baseline model are also included but is not reported for brevity. Time-to-turnover is right censored. All explanatory variables are defined in Appendix. Robust standard errors are clustered by firm and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% respectively.

Panel A

Dependent Var:	CEO Demotion						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Λ=0	Λ=0.5	Λ=1	Λ=1.5	Λ=2	Λ=2.5	Λ=3
Ind. adj. stock return	-0.555***	-0.549***	-0.543***	-0.533***	-0.520***	-0.507***	-0.497***
	(0.155)	(0.158)	(0.161)	(0.165)	(0.169)	(0.175)	(0.180)
Tenure return$_\Lambda$	3.765**	2.777*	2.084	1.473	0.895	0.411	0.072
	(1.679)	(1.666)	(1.720)	(1.853)	(2.089)	(2.361)	(2.559)
Observations	14616	14616	14616	14616	14616	14616	14616
Pseudo R^2	0.034	0.033	0.032	0.032	0.032	0.032	0.032
Fixed effects	Time & Industry	Time & Industry	Time & Industry	Time & Industry	Time & Industry	Time & Industry	Time & Industry

Panel B

Dependent Var:	Forced Turnover						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Λ=0	Λ=0.5	Λ=1	Λ=1.5	Λ=2	Λ=2.5	Λ=3
Ind. adj. stock return	-0.525***	-0.455***	-0.390**	-0.340*	-0.306*	-0.288	-0.283
	(0.172)	(0.173)	(0.174)	(0.175)	(0.176)	(0.178)	(0.179)
Tenure return$_\Lambda$	-4.165	-6.394*	-7.784**	-8.479**	-8.635***	-8.429***	-8.014***
	(3.014)	(3.330)	(3.376)	(3.301)	(3.178)	(3.045)	(2.916)
Observations	14680	14680	14680	14680	14680	14680	14680
Pseudo R^2	0.066	0.068	0.069	0.070	0.070	0.070	0.070
Fixed effects	Time & Industry	Time & Industry	Time & Industry	Time & Industry	Time & Industry	Time & Industry	Time & Industry

Panel C

	Demoted to exec. Chairman position	**Retained CEO title but left as Chairman**	**Forced turnover**	**Demoted to exec. Chairman position**	**Retained CEO title but left as Chairman**	**Forced turnover**
Tenure return$_\Lambda$ quintile	Λ=0	Λ=0	Λ=0	Λ=3	Λ=3	Λ=3
1 (lowest)	0.76% (22)	0.14% (4)	3.50% (105)	1.05% (30)	0.36% (10)	4.29% (126)
2	0.99% (29)	0.31% (9)	1.65% (48)	1.80% (53)	0.28% (8)	1.44% (42)
3	1.59% (47)	0.07% (2)	1.06% (31)	1.62% (48)	0.10% (3)	1.23% (36)
4	1.49% (44)	0.31% (9)	1.27% (37)	1.21% (36)	0.14% (4)	1.11% (33)
5 (highest)	2.08% (62)	0.17% (5)	2.08% (61)	1.25% (37)	0.14% (4)	1.53% (45)

Panel D

Dependent Var:	CEO Demotion			
	Tenure return$_{\Lambda=0}$ ≥ 60th percentile	Tenure return$_{\Lambda=0}$ ≤ 40th percentile	Tenure return$_{\Lambda=3}$ ≥ 60th percentile	Tenure return$_{\Lambda=3}$ ≤ 40th percentile
Ind. adj. stock return	-0.644***	-0.370	-0.632***	-0.345
	(0.194)	(0.573)	(0.221)	(0.462)
Observations	5871	5798	5896	5770
Pseudo R^2	0.047	0.064	0.081	0.044
Fixed effects	Time & Industry	Time & Industry	Time & Industry	Time & Industry

Panel E

Dependent Var:	Forced Turnover			
	Tenure return$_{\Lambda=0}$ ≥ 60th percentile	Tenure return$_{\Lambda=0}$ ≤ 40th percentile	Tenure return$_{\Lambda=3}$ ≥ 60th percentile	Tenure return$_{\Lambda=3}$ ≤ 40th percentile
Ind. adj. stock return	-0.178	-1.284***	-0.108	-1.105***
	(0.161)	(0.305)	(0.197)	(0.310)
Observations	5867	5882	5897	5852
Pseudo R^2	0.053	0.100	0.085	0.091
Fixed effects	Time & Industry	Time & Industry	Time & Industry	Time & Industry

TABLE 1.10

Corporate Governance

This table presents coefficient estimates from COX proportional hazard model that examine the cross-sectional variation in the likelihood of CEO demotion. The sample is divided into two subsamples based on the corporate governance level of the firm. Three different measures of corporate governance are used: G-Index (constructed by Gompers et al. (2003)), E-index (constructed by Bebchuk et al. (2009)), and fraction of outsiders on the board. Since the distribution of fraction of outsiders is right skewed, we use 40^{th} and 60^{th} percentiles as cutoffs when dividing the sample into low and high fraction of outsiders subsample. Time-to-turnover is right censored. Time-to-turnover is right censored. Industry-adjusted stock return is used as the Stock return measure. Other explanatory variables are defined in Appendix. Robust standard errors are clustered by firm and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% respectively.

	(1) G -index< 9	(2) G-index ≥ 9	(3) E -index< 3	(4) E-index ≥ 3	(5) Fraction of outsider≥60^{th} percentile	(6) Fraction of outsider≤40^{th} percentile
Stock return	-0.457**	-0.831**	-0.499**	-0.855**	-0.876	-0.542***
	(0.221)	(0.362)	(0.230)	(0.406)	(0.554)	(0.197)
Age	0.338**	-0.165	-0.084	0.053	0.510	0.069
	(0.163)	(0.221)	(0.129)	(0.318)	(0.442)	(0.126)
Age^2	-0.003**	0.001	0.001	-0.000	-0.004	-0.001
	(0.001)	(0.002)	(0.001)	(0.003)	(0.004)	(0.001)
Stock ownership	0.454	1.139***	0.637**	0.738***	0.916**	0.114
	(0.289)	(0.270)	(0.266)	(0.282)	(0.422)	(0.201)
Volatility	2.497***	0.276	1.940***	0.133	1.137	0.745
	(0.651)	(0.676)	(0.592)	(0.729)	(0.945)	(0.530)
Firm Size	0.149	0.086	0.187**	0.002	-0.080	0.128*
	(0.092)	(0.083)	(0.079)	(0.105)	(0.135)	(0.068)
Leverage	-0.326	-0.038	-0.330	0.263	-0.287	-0.465
	(0.520)	(0.462)	(0.434)	(0.583)	(0.693)	(0.372)
Observations	4613	7077	5742	6067	6468	6542
Pseudo R^2	0.075	0.067	0.067	0.048	0.100	0.044
Fixed effects	Time & Industry	Time & Industry	Time & Industry	Time & Industry	Time & Industry	Time & Industry

TABLE 1.11

Involuntary CEO Turnover Revisited

This table presents coefficient estimates from linear probability model that examine the likelihood of involuntary CEO turnover. Models in columns 1-3 follows traditional approach and as such treats CEO demotions as voluntary. Meanwhile, models in columns 4-6 treat CEO demotions as involuntary turnover. Time-to-turnover is right censored. All controls used in the baseline regression are also used but is not tabulated for brevity. All explanatory variables are defined in Appendix. Robust standard errors are clustered by firm and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% respectively.

	CEO demotions treated as voluntary			CEO demotions treated as involuntary		
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	OLS	OLS	OLS	OLS
Ind. adj. stock return	-0.006**			-0.013***		
	(0.003)			(0.003)		
Actual - mean forecasted EPS		-0.009***			-0.014***	
		(0.003)			(0.004)	
Ind. adj. ROA			-0.130***			-0.157***
			(0.023)			(0.027)
Observations	18817	15727	16998	18817	15727	16998
Adjusted R^2	0.054	0.008	0.057	0.051	0.008	0.055
Fixed effects	Time & Firm	Time & Firm	Time & Firm	Time & Firm	Time & Firm	Time & Firm

TABLE 1.12

Voluntary Turnover Revisited

This table presents coefficient estimates from COX proportional hazard model that examine the likelihood of voluntary CEO turnover. Models in columns 1-3 follows traditional approach and as such treats CEO demotions as voluntary. Meanwhile, models in columns 4-6 treat CEO demotions as involuntary turnover. All controls used in the baseline model are also included but is not reported for brevity. Time-to-turnover is right censored. All explanatory variables are defined in Appendix. Robust standard errors are clustered by firm and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% respectively.

	CEO demotions treated as voluntary			CEO demotions treated as involuntary		
	(1)	(2)	(3)	(4)	(5)	(6)
Ind. adj. stock return	-0.150**			-0.108		
	(0.066)			(0.069)		
EW firm specific return		-0.146**			-0.104	
		(0.066)			(0.069)	
EW industry specific return		0.144			0.144	
		(0.182)			(0.190)	
VW firm specific return			-0.153**			-0.108
			(0.068)			(0.070)
VW industry specific return			0.210			0.260
			(0.198)			(0.207)
Observations	18436	18436	18436	18188	18188	18188
Pseudo R^2	0.069	0.069	0.069	0.080	0.080	0.080
Fixed effects	Time & Industry	Time & Industry	Time & Industry	Time & Industry	Time & Industry	Time & Industry

TABLE 1.13

Competing Risk Framework

The table below reports the coefficient estimates from competing risk survival regressions. The dependent variable used in each regression is indicated in the first row while the competing risk is indicated in the third row. Time-to-turnover is right censored. Other explanatory variables are defined in Appendix. Robust standard errors are clustered by firm and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% respectively.

Dependent Var:	**Type 1 and Type 2 Splits**	**Forced Turnover**
	(1)	(2)
Competing Risk:	**Forced Turnover**	**Type 1 and Type 2 Splits**
Ind. adj. stock return	-0.392***	-0.498***
	(0.142)	(0.128)
Age	-0.073	0.202
	(0.095)	(0.123)
Age^2	0.001	-0.002*
	(0.001)	(0.001)
Stock ownership	0.656***	-0.657***
	(0.144)	(0.152)
Volatility	0.803**	1.725***
	(0.323)	(0.226)
Firm Size	0.053	0.051
	(0.044)	(0.042)
Leverage	-0.645**	0.109
	(0.283)	(0.173)
Observations	18727	18817
Log pseudolikelihood	-1791.17	-2868.99
Fixed effects	Time & Industry	Time & Industry

TABLE 1.14

Quasi-Natural Experiment: Russell Index Reconstitution

Panel A of this table presents the baseline summary statistics for a bandwidth of 300 firms on each side of the index cutoff. All continuous control variables are winsorized at 1 and 99 percent level. Panel B presents coefficients from local linear regressions (with triangle kernel) of the difference at the cutoff of the regression functions to the left and to the right of the Russell 1000/2000 threshold. The dependent variable is the indicator variable for CEO demotion. Coefficients should be interpreted as the effect on the dependent variable of being assigned to the top of Russell 2000 index instead of bottom of Russell 1000 index among firms close to the cutoff, in 12 months following Index reconstitution. To avoid misclassification, only those firms that are also covered by COMPUSTAT, CRSP, and ExecuComp are included in the regression. Robust standard errors are used. *, **, and *** indicate significance level of 0.1, 0.05, and 0.01 respectively.

Panel A

	Firms in the Russell 1000 Index			Firms in the Russell 2000 Index			
Variable	N	Mean	SD	N	Mean	SD	Difference
Ind. adj. stock return	1086	0.065	0.472	1242	0.085	0.468	-0.02
EW firm specific return	1086	0.066	0.471	1242	0.086	0.467	-0.02
VW firm specific return	1086	0.126	0.472	1241	0.153	0.467	-0.027
EW industry specific return	1086	0.145	0.226	1242	0.148	0.235	-0.003
VW industry specific return	1086	0.083	0.181	1241	0.08	0.19	0.003
Age	1086	54.346	7.119	1242	54.306	6.872	0.04
Tenure (in years)	1086	7.693	7.587	1242	7.031	6.907	0.662**
Stock ownership (dummy)	1086	0.021	0.04	1242	0.015	0.03	0.006***
Volatility	1086	0.407	0.218	1242	0.436	0.214	-0.029**
Firm size	1086	7.513	1.042	1242	7.02	0.975	0.493***
Leverage	1086	0.347	0.263	1242	0.346	0.266	0.001

Panel B

	(1) Ind. adj. stock return	(2) EW firm specific return	(3) VW firm specific return
R2	-0.010	-0.013	-0.012
	(0.022)	(0.022)	(0.023)
Stock Return	-0.027**	-0.025**	-0.023**
	(0.010)	(0.011)	(0.011)
R2 X Stock Return	0.021**	0.022**	0.025**
	(0.010)	(0.010)	(0.010)
Observations	2319	2319	2318
Bandwidth	300	300	300
Controls	Yes	Yes	Yes
Fixed Effects	Time & Firm	Time & Firm	Time & Firm

TABLE 1.15

Market Reaction

This table presents the mean cumulative abnormal stock returns surrounding the CEO demotion date for different event windows. The parameters for the market model are estimated over a period of 250 trading days ending 11 days prior to the CEO demotion announcement date. Standardized cross-sectional test statistics (introduced by Boehmer et. al. (1991)) and non-parametric rank test statistics (introduced by Corrado (1989)) are reported. *, **, and *** indicate significance at 10%, 5%, and 1% respectively.

Panel A

Demoted to exec. Chairman position

					Specialist				Non-Specialist			
Days	N	Mean CAR	Standardized cross-sectional test	Non-parametric rank test	N	Mean CAR	Standardized cross-sectional test	Non-parametric rank test	N	Mean CAR	Standardized cross-sectional test	Non-parametric rank test
[0,+1]	183	1.21%	2.038**	2.959***	110	1.97%	3.266***	3.720***	40	0.35%	0.494	0.530
[0,+2]	183	1.32%	2.116**	3.054***	110	2.05%	2.935***	3.496***	40	0.34%	0.311	0.200
[-1.+1]	183	1.13%	1.865**	2.339***	110	1.89%	2.945***	3.009***	40	-0.34%	0.002	-0.549
[-2,+2]	183	1.46%	2.419***	2.564***	110	2.04%	2.676***	2.428***	40	-0.50%	-0.113	-0.777

Panel B

Retained CEO title but left as Chairman

					Specialist				Non-Specialist			
Days	N	Mean CAR	Standardized cross-sectional test	Non-parametric rank test	N	Mean CAR	Standardized cross-sectional test	Non-parametric rank test	N	Mean CAR	Standardized cross-sectional test	Non-parametric rank test
[0,+1]	30	2.14%	1.490*	0.729	13	3.30%	1.463*	1.307*	12	0.91%	0.347	-0.826
[0,+2]	30	4.40%	2.304**	1.999**	13	7.04%	2.230**	2.597***	12	1.74%	0.500	-0.497
[-1.+1]	30	1.05%	0.562	0.210	13	1.83%	0.647	0.997	12	-0.59%	-0.597	-1.532*
[-2,+2]	30	4.52%	2.488***	2.248**	13	6.80%	1.914**	2.055**	12	1.34%	0.344	-0.007

FIGURE 1.1

Institutional Holdings after Annual Russell Index Reconstitution

The graphs in this section presents the total institutional holding in a narrow window centered on the index cutoff. Firms to the left of the cutoff are in the larger Russell 1000 index while firms to the right of the cutoff are in the Russell 2000. Figure 1.1 adds local linear regression estimates (with triangle kernel) and the associated 95% confidence bands while figure 1.2 adds local polynomial regression estimates (with triangle kernel) and the associated 95% confidence bands using the Rule of Thumb (ROT) optimal plug-in bandwidth estimate. There is also a superimposed scatterplot of the data's average value in bins. No year fixed effects are included in either specificaiton for the purpose of clarity regarding the ultimate data. Institutional holding data is winsorized at 1 and 99% level to mitigate the effects of outliers.

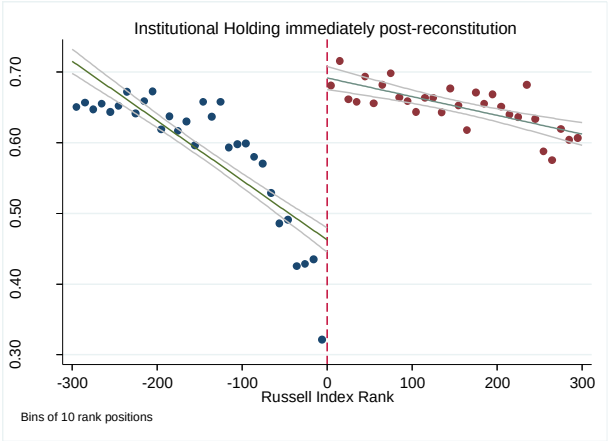


Figure 1.1.1

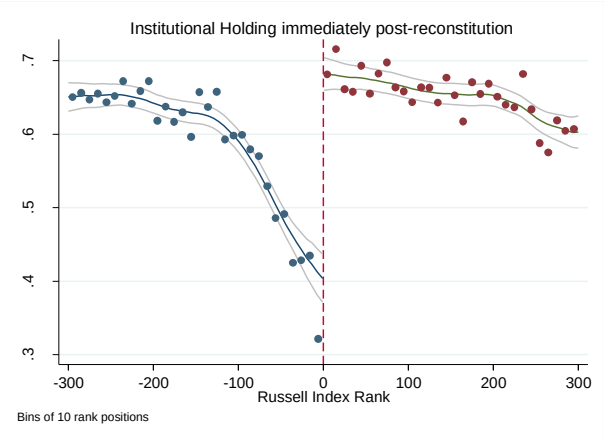


Figure 1.1.2

FIGURE 1.2

Cumulative Abnormal Returns

Figure 2.1 (figure 2.2) shows the 5-day average cumulative abnormal return around the announcement date of demotion of the CEO to the executive Chairman position (CEO remains as CEO but abdicates the Chairman title) estimated using the market model where the parameters for the model are estimated over a period of 250 trading day ending 11 days prior to the duality split announcement date.

Fig. 1.2.1: Demoted to exec. Chairman position

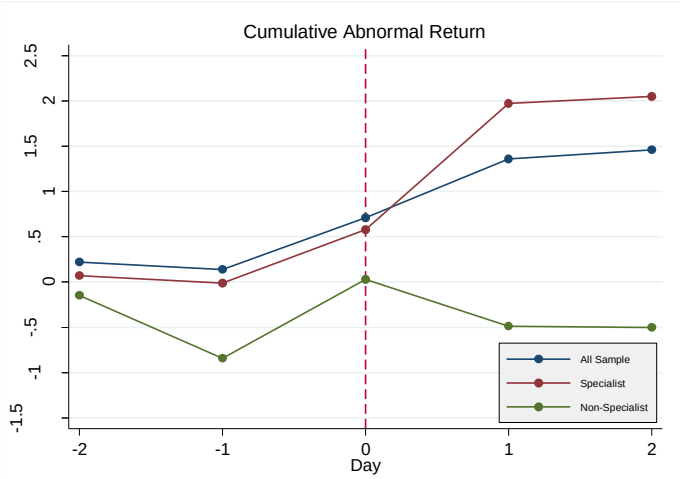
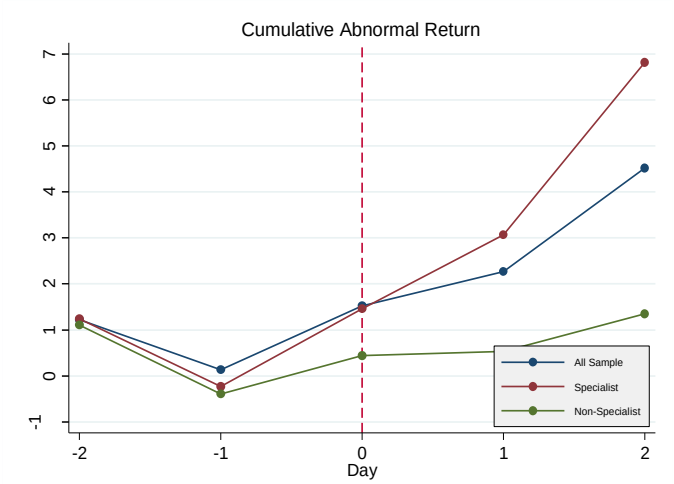


Fig. 1.2.2: Retained CEO title but left as Chairman



Chapter 2: The Role of Deferred Pay in Retaining Managerial Talent

2.1 Introduction

Retaining and motivating talented executives is a key ingredient for firms to create shareholder value. This is especially the case for human capital-intensive firms where value creation requires executives to acquire and develop firm-specific knowledge. Frequent turnover among executives will not only result in loss of valuable firm-specific knowledge but will also affect the incentives of the executive to acquire the knowledge in the first place. Understanding this, in practice, firms use a number of implicit and explicit contractual features to retain talented executives. The recent controversy about information technology firms colluding in their hiring practices to limit poaching talent from each other highlights the extent to which firms are willing to go to retain talent.[43] Chief among the explicit (and legal!) contractual provisions employed by firms to retain talent is deferred pay. To the extent an executive forfeits all deferred pay if she leaves the firm, deferring pay will increase the cost (to the executive) of early exit.

Despite its importance, the effectiveness of deferred pay in helping firms retain talent has received limited research attention. This is mainly because of lack of data on the extent of deferred pay. In this paper, we use information on the vesting provisions of stock and option grants for a sample of S&P 1500 firms to study the role of deferred stock and option grants (deferred pay from now on) in executive turnover. Our analysis also helps us understand the importance of executive talent and firm-specific knowledge for both the design of deferred pay and executive turnover.

[43] See Wall Street Journal articles titled "Ebay settles recruiting allegations" dated May 1, 2014 and "Tech companies agree to settle wage suit" dated April 24, 2014.

Specifically, we study the effectiveness of deferred pay in helping firms retain the top five highest paid executives of the firm. We focus on the top five executives because of availability of detailed data on deferred pay and also because the highest paid executives are likely to be among the most valuable employees of the firm and their retention should be of utmost importance to the firm.

A typical compensation package for a top executive includes both a cash (salary and bonus) and a stock component (restricted stock and stock options). Firms typically defer the stock component of pay. Every stock and option grant is associated with a vesting schedule and the manager is not allowed to exercise or sell (or hedge) the grant until it vests. A manager who voluntarily or involuntarily leaves the firm typically forfeits all the unvested grants. The retention incentives provided by a stock or option grant depends both on the size of the grant and the length of the remaining vesting schedule. All else equal, a larger grant and one with a longer vesting schedule will provide greater retention incentives. To capture these twin effects, we employ the measure of executive pay duration (*Duration*), introduced by Gopalan et. al., (forthcoming), to quantify the extent of long-term retention incentives provided by an incentive contract. *Duration* is the weighted average of the vesting periods of all four components of pay (salary, bonus, restricted stock, and stock options), with each component's weight being the fraction of that component in the executive's total compensation.[44] In our empirical analysis, we estimate the effect of *Duration* on the probability of an executive turnover.[45]

We begin our empirical analysis by estimating the relation between pay *Duration* and the likelihood of voluntary executive turnover. We classify a CEO turnover as involuntary following

[44] The vesting periods of cash and bonus are zero, and thus the magnitude of the calculated pay duration depends on the vesting periods of stock options and restricted stocks, and their relative weights in the total compensation.

[45] In constructing *Duration*, we do not include any vesting provisions embedded in the pension provisions of the firm. To this extent, *Duration* may be a noisy measure of the extent of deferred pay. We discuss this further in Section 3.2.

the criteria in Parrino (1997) complemented by two of our own. We reclassify an involuntary turnover according to Parrino (1997) as voluntary if the CEO's employment record, obtained from Boardex and Marquis Who's Who publications, suggests that the CEO takes a comparable position elsewhere, or if the press reports announcing the turnover convincingly explain that the departure is due to previously undisclosed personal or business reason that is unrelated to the firm's activities. All the CEO turnovers not classified as forced or due to mandatory or planned retirements are classified as voluntary. We identify turnover of other senior executives using ExecuComp and BoardEx. We classify a non-CEO senior executive turnover as voluntary if it is not due to retirement, health reasons and if we are able to identify the executive's new employment from ExecuComp or BoardEx. We call such voluntary turnovers as executives "jumping ship".

We expect a longer pay *Duration* to reduce the likelihood of a voluntary turnover because any outside opportunity should be sufficiently valuable to the executive so as to compensate for the lost unvested pay. Consistent with this conjecture, we find that senior executives with longer pay *Duration* are less likely to voluntarily leave the firm. This is true both for CEOs and for other senior executives. The effects we document are economically very large. We find that a one standard deviation increase in *Duration* (an increase by 0.97 years) is associated with a 58% decrease in the likelihood of a voluntary CEO turnover in a given year. For other senior executives, we find that a one standard deviation increase in *Duration* (an increase by 0.88 years) is associated with a 150% reduction in the probability of an executive jumping ship. We find that the link between pay duration and executive turnover becomes stronger when we complement our measure of pay duration by including unvested stock and option grants from prior years (*Duration-2*).

A negative correlation between *Duration* (or *Duration-2*) and voluntary executive turnover may not imply a causal effect of deferred pay on turnover. Firm-level and executive-level omitted variables can bias the coefficient. The important firm-level omitted variable that may affect both *Duration* and the likelihood of executive turnover is the importance of firm-specific knowledge.

All else equal, in situations where firm-specific knowledge is more important, firms are likely to offer longer *Duration* pay and executives of such firms may also remain longer with the firm because of the difficulty of applying their (firm-specific) knowledge elsewhere. The important executive-level omitted variable is executive talent. All else equal, executives perceived to be more talented by the board will obtain longer duration pay and such executives may also remain longer with the firm, given their favorable internal perception.[46]

To overcome this endogeneity issue, we implement an IV regression where we instrument for *Duration-2*. Our instrument identifies years in which a large prior-year stock or option grant vests (*Large vesting*) so that the executive's overall pay duration decreases. To ensure our instrument is truly exogenous, we focus on grants that were granted more than two years prior. A similar instrument is used by Edmans, Fang and Lewellen (2014) to study the effect of stock vesting schedules on managerial myopia. We use these vesting episodes as instances that shock pay duration and estimate its effect on executive turnover. To the extent that these grants were awarded in the distant past, their vesting is unlikely to be correlated with (time-varying) firm and executive-level omitted variables. In the IV specification, we control for all time invariant firm-specific factors by including firm fixed effects. Our IV results corroborate our OLS estimates and indicate a causal effect of *Duration* on executive turnover. We find our IV estimates to be larger than the OLS estimates. The relationship between stock and option vesting and executive turnover that we uncover may also help explain why such episodes are associated with managerial myopia (Edmans, Fang, and Lewellen 2014).

In our next set of tests, we estimate the effect of pay duration on the likelihood of involuntary executive turnover. To the extent that the decision to remove a CEO is taken by the

[46] Since neither firm-specific knowledge nor executive talent is observable, we implicitly use these terms as a catch-all for all firm-specific and executive-specific factors that may both increase pay duration and decrease the likelihood of a voluntary turnover.

board in the shareholders' best interest, one does not expect a causal relationship between *Duration* and the likelihood of forced turnover. The board should not be concerned with the executive losing (or not losing) her unvested stock/option grants when deciding to remove her. On the other hand, there are three non-mutually exclusive reasons why *Duration* and forced executive turnover may be negatively correlated. The negative correlation may arise due to the two omitted variables mentioned before, executive talent and the importance of firm-specific knowledge. Whenever an executive is perceived to be talented or firm-specific knowledge is more important, not only will the board offer a longer *Duration* pay ex ante, but will also be reluctant to fire such an executive. *Duration* and forced executive turnover may also be negatively related due to poor firm-level corporate governance. To the extent the executive forfeits unvested pay when being forced out, boards (that act in the executive's best interest) may be reluctant to fire an executive with long-duration pay. We perform tests to understand the validity of these three reasons.

We find that CEOs (and non-CEO executives) are less likely to experience an involuntary turnover if they have a longer *Duration* pay. The effect of *Duration* on involuntary turnover is also economically significant. A one standard deviation increase in *Duration* is associated with a 57.5% (60.3%) decrease in the likelihood of an involuntary CEO (senior executive) turnover.

To the extent executive talent is time invariant, we expect time-invariant executive-specific factors to proxy for executive talent. To isolate the effect of CEO talent on the relationship between pay duration and forced CEO turnover, we repeat our baseline estimation after including CEO fixed effects. We find that the inclusion of CEO fixed effects marginally reduces the size of the coefficient on *Duration* but significantly increases the standard errors and hence the coefficient becomes insignificant. This is consistent with time-invariant executive-level factors having an effect on the relation between *Duration* and forced CEO turnover (Graham et al. 2011). In additional tests looking at time-invariant executive factors, we find that an executive's pay duration in prior employment (*Prior duration*) is negatively related to the likelihood of a forced turnover.

This offers further support for the effect of time-invariant executive-level factors on the duration-forced turnover relationship.

To isolate the effect of the importance of firm-specific knowledge on the relationship between pay duration and forced executive turnover, we perform two sets of tests. First, we repeat our baseline estimation after including firm fixed effects. To the extent that the importance of firm-specific knowledge is time-invariant, firm fixed effects should capture it. Surprisingly, we find that inclusion of firm fixed effects does not significantly affect the size of the coefficient on *Duration* as compared to the OLS estimate, where we include industry fixed effects. This highlights that time-invariant firm-specific factors do not appear to have a significant effect on the relation between *Duration* and forced CEO turnover.

The importance of firm-specific knowledge could be time varying. In situations where firm-specific knowledge is important, firms are likely to offer longer *Duration* pay for *all* the top executives of the firm. The average duration of the other senior executives in the firm in any year could therefore measure the importance of firm-specific knowledge for that firm during that year. Consistent with the importance of firm-specific knowledge, we find that CEOs are less likely to experience forced turnover if the firm offers higher average duration pay to the other senior executives in the firm.[47]

To estimate the extent to which the negative association between pay duration and forced executive turnover is due to poor firm-level corporate governance, we repeat our tests after including an interaction term between *Independent*, a dummy variable that identifies firms with above-median fraction of independent directors in the board and *Duration*. We find that the coefficient on the interaction term is negative and significant. Thus, the negative relation between

[47] The average duration of the other executives could proxy for factors other than the importance of firm-specific knowledge. To this extent, we do not interpret this evidence as causal.

Duration and forced CEO turnover is stronger in the subsample of firms with more independent boards. This suggests that poor corporate governance is unlikely to account for the negative association between pay duration and forced turnover.

An interesting puzzle in the empirical corporate governance literature is the low sensitivity of forced CEO turnover to firm performance. That is, CEOs are not fired immediately on poor stock performance. Taylor (2010) uses a structural model to argue that the low correlation may be due to firms facing switching costs when they replace CEOs. To the extent that *Duration* captures these switching costs, one would expect executives with longer *Duration* to have a lower performance-turnover sensitivity. Our results support this conjecture. We find that the forced turnover of CEOs with above-median *Duration* is not sensitive to stock return. On the other hand, for CEOs with below-median *Duration*, turnover is very sensitive to stock return.

In our final set of tests, we look at the choice between an insider versus an outside to replace the firm's CEO. Consistent with longer pay duration indicating the importance of firm-specific knowledge, we find that firms that offer a longer-duration pay contract to their executives are more likely to recruit an insider to replace the CEO. Hiring CEOs from inside the firm, in turn, also help retain insiders and motivate them to invest in the acquisition of firm-specific knowledge.

Our paper makes a number of contributions to the empirical compensation literature. We are the first to use detailed information on vesting schedules to estimate the effect of deferred pay on executive turnover. Prior research that looks at the link between compensation and turnover relate the level of stock-based pay to managerial turnover (e.g., Balsam and Miharjo 2007; Fee and Hadlock 2003; Hasenhuttl and Harrison 2002; Mehran and Yermack 1997). In comparison, our duration measure, which accounts for both the level and the vesting period of stock-based pay, better captures the cost that managers incur when they leave the firm and reveals the firm's

retention intention. Our detailed vesting data also allow us to design sharper tests to estimate the causal effect of deferred pay on turnover.

Our paper also contributes to the literature that studies the performance-turnover sensitivity of CEOs. Prior literature shows that, in contrast to what economic theories predict, the sensitivity of forced CEO turnover to firm performance is rather modest (e.g., Coughlan and Schmidt 1985; Denis, Denis, and Sarin, 1997; Huson, Parrino, and Starks 2001; Warner, Watts, and Wruck 1988; Weisbach 1988). We find that pay *Duration* has an important moderating role on the performance-turnover relationship. This suggests that the switching costs in replacing talented executives may go towards explaining the weak performance- turnover relationship.

The rest of this paper is organized as follows. Section 2 develops testable hypotheses. Section 3 describes the sample and variables used in the empirical tests. Section 4 presents our main analysis on pay duration and executive turnover. Section 5 concludes. Definitions of other variables appear in Appendix B.

2.2 Hypotheses Development

In this section, we outline the hypotheses that have predictions for our setting. Firms often defer pay to retain valuable talent. Deferring pay will also enable the firms to provide incentives for the executives to invest in firm-specific knowledge. Lately, the preferred mode for firms to defer pay is to award a large part of the executive's annual compensation in the form of restricted stock and stock options and to get these awards to vest over a long period. The awards are structured such that the executive will forfeit the unvested portion of the grant if she leaves the firm. This increases the cost to the executive of leaving the firm. Any organization that wants to hire the executive has to compensate for the loss resulting from the forfeiture of unvested options and restricted stocks. This would predict that the amount and vesting schedule of option and stock

grants will affect the executive's likelihood of voluntarily leaving the firm. To test this prediction, we employ the measure of pay duration developed in Gopalan et al. (forthcoming). The advantage of the duration measure is that it takes into account both the amount of unvested grants and their remaining vesting schedule. This leads to our first hypothesis.

Hypothesis 1: Managers with longer pay durations are less likely to leave the firm voluntarily.

To the extent that long pay duration indicates the importance of firm-specific knowledge or/and the perceived managerial talent, the boards of such firms may be more reluctant to fire such executives and choose to wait longer before reaching the firing decision even following poor firm performance. This would predict that executives with longer pay duration are less likely to be forced out. This forms our second hypothesis.

Hypothesis 2: Managers with longer pay duration are less likely to experience involuntary turnover.

If, consistent with *Hypothesis 2*, boards are less likely to force out executives with longer pay duration, it could be due alternatively to poor corporate governance. That is, a captured board (that acts in the interest of executives instead of shareholders) may be reluctant to force out executives, who otherwise would have had to forfeit all unvested pay if being forced out. We examine these different explanations underlying *Hypothesis 2* in our empirical analysis later.

An important puzzle in the corporate governance literature is the low explanatory power (in economic magnitude) of stock performance on forced CEO turnover. That is, CEOs are not significantly more likely to be fired following poor stock performance. Taylor (2010) uses a structural model to argue that the low turnover-performance correlation may be due to firms facing switching costs when they replace CEOs. One possible source of switching cost may be the loss of firm-specific knowledge from the departing CEO. That may explain why boards have greater forbearance in tolerating poor performance and wait longer before the turnover decision in some

firms. To the extent that pay duration is longer in situations where firm-specific knowledge is more important (see *Hypothesis 1*), we expect the correlation between firm performance and forced CEO turnover to be especially low in the subsample of executives with long pay duration. This forms our next hypothesis.

Hypothesis 3: The sensitivity of forced turnover to firm performance should be lower for CEOs with longer pay duration.

To the extent that firms offer longer-pay duration in situations where firm-specific knowledge is more important, it has implications for the firm's choice between an insider versus an outsider for the new CEO, conditional on CEO succession. To the extent that an insider has better firm-specific knowledge, we expect firms that on average offer longer-pay duration to be more likely to hire an internal candidate. Hiring CEOs from inside the firm, in turn, also motivate talented insiders' investment in the acquisition of firm-specific knowledge and help retain them. This leads to our final hypothesis:

Hypothesis 4: Firms that on average offer longer duration pay contracts to their executives are more likely to hire an internal candidate as a CEO.

2.3 Data and Variables

2.3.1 Data and Sample

We obtain the data for our analysis from six sources: *Equilar Consultants, Execucomp, Riskmetrics, Thomson Reuters Institutional Holdings (13f) database, the Center for Research in Security Prices (CRSP),* and *Compustat*.

- We identify executive turnovers from *ExecuComp* and use news reports, *Boardex* and other public sources to classify the turnover as voluntary or involuntary.

- Data on the vesting schedules of restricted stock and stock options are obtained from *Equilar Consultants* (hereafter, *Equilar*). Similar to S&P (provider of *ExecuComp*), *Equilar* collects compensation data from firms' proxy statements. We obtain details of all stock and option grants to all named executives covered by *Equilar* for the years 2006-2009. *Equilar* also provides the grant date and the present value of the grants as reported in the proxy statements. *Equilar* also identifies if the size or the vesting schedule of the grant is linked to firm performance.
- We obtain data on other components of executive pay, such as salary and bonus, from *ExecuComp*. We carefully hand-match *Equilar* and *ExecuComp* using firm tickers and executive names. Since prior studies on executive compensation predominantly use *ExecuComp*, we ensure comparability of *Equilar* and *ExecuComp* by making sure the total number of options granted during the year for each executive in our sample is the same across the two data sets.[48]
- We obtain data on the composition of the Board of Directors from *RiskMetrics* and whenever needed, supplement it with data from *Boardex*. Our data on block holders is from *Thomson Reuters Institutional Holdings (13f)* database.
- We complement the compensation and board data with stock returns from *CRSP* and firm accounting data from *Compustat*.

Our final sample consists of the executives covered by both *Equilar* and *ExecuComp* for the time period 2006-2009. This results in 6,127 firm-years involving 1,803 firms, 2,406 CEOs and 6,974 other senior executives.

[48] We drop 2,470 executive-year observations for which we cannot match the number of option grants across *Equilar* and *ExecuComp*. This amounts to 9.3% of the total executive-year observations in our sample.

2.3.2 Key Variables

Pay duration

We follow Gopalan et al. (forthcoming) to construct our measure of pay duration ("*Duration*"). To construct this measure, we use data on annual stock and option grants. Specifically, it is the weighted average of the lengths of the vesting periods of the four pay components (i.e., salary, bonus, restricted stocks, and stock options), with the weight for each component being the fraction of that component in the executive's total compensation. If the stocks and options are granted with a cliff vesting schedule, we calculate pay duration as:

$$Duration = \frac{(Salary + Bonus) \times 0 + \sum_{si=1}^{S} Stock_{si} \times t_{si} + \sum_{oi=1}^{O} Option_{oi} \times t_{oi}}{Salary + Bonus + \sum_{si=1}^{S} Stock_{si} + \sum_{oi=1}^{O} Option_{oi}},$$

where $Salary$ and $Bonus$ are the dollar values of salary and bonus as of the year end. Since salary and bonus are paid out in full by the end of the year they have a vesting period of zero in the above formula. $Stock_{si}$ and $Option_{oi}$ are the dollar value of restricted stock grant *si* and stock option grant *oi*, which have a final vesting period of t_{si} and t_{oi} years, respectively. The value of a restricted stock grant is estimated as the product of the stock price on the grant date and the number of stocks granted, while the value of a stock option grant is estimated using Black-Scholes option pricing model by Equilar. *S* and *O*, respectively, are the total number of stock and option grants to the executive in a year. If the stock and option grants vest equally over the vesting periods (graded vesting schedule), t_{si} (t_{oi}) is replaced with $\frac{t_{si}+1}{2}$ ($\frac{t_{oi}+1}{2}$).

Our baseline measure of pay duration does not include grants from prior years. To account for such grants, we construct an alternative measure ("*Duration-2*") by expanding the estimation to include all unvested stock and option grants from prior years. Specifically, *Duration-2* is calculated as:

$$Duration-2 = \frac{(Salary + Bonus) \times 0 + \sum_{si=1}^{\hat{S}} Stock_{si} \times t_{si} + \sum_{oi=1}^{\hat{O}} Option_{oi} \times t_{oi}}{Salary + Bonus + \sum_{si=1}^{\hat{S}} Stock_{si} + \sum_{oi=1}^{\hat{O}} Option_{oi}},$$

where $\hat{S}$ is the sum of the number of stock grants during the year and the number of unvested stock grants from prior years. For an unvested stock grant *si*, t_{si} is the remaining time to final vesting. Similarly, $\hat{O}$ is the sum of the number of option grants during the year and the number of unvested option grants from prior years, and for an unvested option grant *oi*, t_{oi} is the remaining time to final vesting. As before, if the stock and option grants vest equally over the vesting periods (graded vesting schedule), t_{si} (t_{oi}) is replaced with $\frac{t_{si}+1}{2}$ ($\frac{t_{oi}+1}{2}$). More details on the construction of *Duration-2* are provided in the Appendix.

Our measure of duration does have a limitation as a proxy for the extent of deferred pay. We do not include severance and post-retirement benefits that may be important for providing long-term incentives. The main reason for this exclusion is the difficulty in obtaining the vesting schedules of these benefits. To the extent that the retirement benefits are in the form of a defined contribution retirement account and to the extent that the executive has spent sufficient time with the firm, the retirement account is likely to have vested fully and is unlikely to prove problematic. Furthermore, our subsequent empirical analysis shows that our measure of pay duration is significantly associated with the likelihood of voluntary turnover.

Management turnover

In this section, we describe the methodology we employ to identify turnover of a named executive of the firm for whom we can obtain pay data from the firm's proxy statements. We start

by identifying changes in executive designations as documented in *ExecuComp*.[49] We then search *Factiva*, *LexisNexis*, and *Boardex* for news reports coincident with the change in designation to identify the causes for the change. From our list of potential turnovers, we drop instances that are due to misclassification in *ExecuComp*, takeovers or spinoffs, interim positions, sudden death of the manager and mandatory or planned retirement. Our final sample includes 1081 management turnovers, of which 239 involve a CEO.

For turnovers involving a CEO, we start with using the criteria in Parrino (1997) to classify the turnover as voluntary or involuntary. All turnovers for which the press reports that the CEO is fired, is forced out, or departs due to difference of opinion or unspecified policy differences with the Board, are classified as forced. Of the remaining turnovers, if the departing CEO is under age 60, it is classified as forced if either (1) the reported reason for the departure does not involve death, poor health, or acceptance of another position elsewhere or within the firm (including the chairmanship of the board)[50], or (2) the CEO is reported to be retiring but there is no announcement about the retirement made at least two months prior to the departure. We then complement these criteria with two of our own. We reclassify a forced turnover (identified using the steps described above) as voluntary if either (1) the CEO's employment record, obtained from *Boardex* and Marquis Who's Who publications, suggests that the CEO obtained a comparable position elsewhere, or (2) the press reports convincingly explain that the departure is due to previously undisclosed personal or business reasons that are unrelated to the firm's activities. All the CEO

[49] The earlier literature identifies the samples of CEO turnovers using Forbes annual compensation surveys (e.g., Borokhovich, Parrino, and Trapani 1996; Huson, Malatesta, and Parrino 2004; Huson, Parrino, and Stark 2001; Parrino 1997). More recent studies (e.g., Jenter and Kanaan forthcoming) use the changes in the CEO position in ExecuComp to classify CEO turnovers.

[50] In case of health being a reported reason for the departure, we track backward the press reports about the CEO's health status, and ensure that the departure is indeed due to the health problem. Otherwise, we still treat the departure as being forced.

turnovers not classified as forced or due to mandatory or planned retirements are classified as voluntary.[51]

For some of our tests, we classify a new CEO as being external/internal to the firm if she has been with the firm for no more/more than a year before the succession. We do this by relying on *ExecuComp* and *Boardex* for information on a manager's career path, supplemented by Marquis Who's Who publications, Bloomberg Businessweek, and Standard & Poor's register of corporations, directors, and executives.

For turnovers involving other senior executives, there are fewer detailed press reports about the circumstances involving their departure. Hence, it is difficult to employ the same criteria as those for CEOs to distinguish between forced and voluntary turnovers. We thus employ an alternative classification. We first try to identify if a departing executive takes up a position in a new firm. Specifically, we classify an executive turnover as "jump-ship" (employing the terminology in Fee and Hadlock (2003)) either if (1) the press reports that the executive is leaving to join another firm, or (2) the employment record of the executive as obtained from *Boardex* and *Marquis Who's Who publications* indicates that the executive took up a position in a new firm within three months of departure from the old firm and there is no convincing evidence in the press that the executive was ousted by the old firm. All other senior executive turnovers except those involving mandatory retirements are classified as involuntary.

Given the paucity of information about non-CEO executive turnovers, we are likely to classify some voluntary executive turnovers as involuntary. This is unlikely to be a problem for us because our primary interest is in understanding how pay duration affects the probability of a

[51] Among CEOs who depart voluntarily in our sample, 27 join other firms as CEOs. Given the small number of them, we do not conduct a separate analysis of them from the overall group of voluntary turnovers.

voluntary executive turnover. Moreover, *Hypotheses 1 & 2* predict that pay duration is negatively correlated with both voluntary and involuntary executive turnover.

2.3.3 Summary Statistics

Table 2.1 presents summary statistics of the key variables we use in our analysis. All variables are winsorized at the 1% and 99% level to mitigate the potential impact of outliers. Detailed definitions of these variables (except pay duration and management turnover that are discussed earlier) are provided in the Appendix. Panel A summarizes the data for CEOs while Panel B summarizes the data for non-CEOs. From Panel A, we find that the average *Duration* (*Duration-2*) for CEOs in our sample is 1.45 (1.48) years, consistent with the numbers in Gopalan et al. (forthcoming). We find that the average CEO is 54.87 years old, has spent 7.48 years in her current position and has about 0.13% of the firm's equity. We also find that about 51% of the CEOs in our sample are also the Chairman of their board as seen from the mean value of *Duality*.

From Panel B, we find that the mean value of *Duration* (*Duration-2*) for non-CEOs in our sample is 1.26 (1.29) years. The non-CEOs have an average age of 50.39 years and have spent 14.84 years in the firm. Note that while *Tenure* for CEOs indicates the number of years the executive has been the CEO, for non-CEOs, *Tenure* refers to the number of years the executive has been with the firm.

In Panel C, we present the summary statistics of the characteristics of the firms in our sample. We use industry adjusted stock return (*Ind. adj. stock return*), which is the difference between the annual return on the firm's stock and the average stock return of firms in the same industry defined at the two-digit SIC code level as our main measure of firm performance. [52] We

[52] Our main findings hold for alternative measures of firm performance, namely, two year industry adjusted stock returns, industry adjusted returns using Fama-French 49 industry classification and industry adjusted performance measure used by Jenter and Kanaan (forthcoming). The latter is estimated as the annualized

find that, on average, firms in our sample outperform the industry as seen from the mean value of *Ind. adj. stock return* of 0.03%. The average *Volatility* of the firms in our sample is 42%. The firms in our sample are on average large, as seen from the mean value of *Firm size,* of 7.75. In comparison, the average value for all firms in Compustat during the same sample period is 5.47. The firms in our sample have valuable growth opportunities as seen from the average value of *Market to book* ratio of 1.71. The average *Leverage* of our sample firms is 23%, and on average, they spend about 2.4% of the book value of total assets on R&D as seen from the mean value of *R&D/Asset*.

In Table 2.2, we conduct a set of univariate tests on the different turnovers in our sample. In Panel A, we classify the CEO years in our sample into those involving a voluntary turnover and those not involving a voluntary turnover, and provide the average CEO and firm characteristics. We have 125 voluntary CEO turnover events during our sample period. The average value of *Duration* of CEOs who voluntarily leave the firm is 1.07, significantly below the average value of *Duration* for CEOs who stay with the firm (1.46). We observe a similar pattern with *Duration-2*. This is consistent with *Hypothesis 1*. We also find that firm-years with a voluntary CEO turnover have lower industry adjusted stock return. CEOs who voluntarily leave their firm are younger, have shorter tenures and lower stock holding in their firm. In our regressions that explore the effect of *Duration* and *Duration-2* on voluntary CEO turnover, we include these variables as controls to ensure that they do not bias our conclusions.

In Panel B, we classify the non-CEO executive years in our sample into those before a non-CEO executive jumps ship and others, and present the average executive and firm characteristics.[53] We have 289 instances where a non-CEO executive leaves the firm for another firm. We find that

residual obtained from regressing the monthly return on the firm's stock on the return of the value weighted index of all firms in the same industry.

[53] We focus on the year before the executive jumps ship because executive pay information is usually not available in the proxy statements if the executive leaves in the middle of a year.

the average value of *Duration* (*Duration-2*) of non-CEO executives who "jump-ship", 0.41 (0.46), is significantly below the average value for non-CEO executives who stay with the firm, 1.21 (1.31). This again is consistent with *Hypothesis 1*. We also find that firm-years before a non-CEO executive "jumps-ship" have lower industry adjusted stock return. Non-CEO executives who voluntarily leave their firm are younger and non-surprisingly have shorter tenures with their firm.

In Panel C, we divide our sample into firm-years before a forced CEO turnover and other firm-years, and present the average CEO and firm characteristics. We have 114 forced CEO turnover events during our sample period. We find that while the average *Duration* (*Duration-2*) of CEOs who are forced out of their firms is 0.87 (0.87), it is significantly below the average value for CEOs who stay with the firm, 1.47 (1.50). This is consistent with *Hypothesis 2*. We also find that firm-years with a forced CEO turnover have lower *Ind. adj. stock return*. CEOs who are forced to leave their firm are also younger, have shorter tenures and lower stock holding in their firm, and are less likely to be the Chairman of their Board. In our regressions exploring the effect of *Duration* on forced CEO turnover, we include these variables as controls to ensure that they do not bias our conclusions.

Finally, in Panel D, we compare non-CEO executive-years before those involving an involuntary turnover to all others. We have 553 non-CEO turnover events in our sample that we classify as involuntary. We find that while the average value of *Duration* (*Duration-2*) of non-CEO executives who involuntarily leave the firm is 0.79 (0.80), it is significantly below the average value for non-CEO executives who stay with the firm, 1.21 (1.31). This again is consistent with *Hypothesis 2*. We also find that executive-years with an involuntary turnover have lower *Ind. adj. stock return*. Non-CEO executives who involuntarily leave their firm are younger and have shorter tenures with their firm.

To summarize, our univariate evidence indicates that executives (both CEOs and non-CEOs) with longer pay duration are less likely to leave their firms. This evidence is consistent with both *Hypotheses 1 & 2*.

2.4 Main Analysis of Pay Duration and Turnover

In this section, we conduct multivariate tests of the effect of pay duration on executive turnover. We first discuss the tests that study voluntary turnovers, which are followed by those that look at involuntary turnovers.

2.4.1 Pay Duration and Voluntary Turnover

Baseline analysis

In Table 2.3, we test *Hypothesis 1* by relating CEO pay *Duration* to the likelihood of voluntary turnover. Following prior literature (e.g., Hazarika, Karpoff, and Nahata 2012; Jenter and Kanaan forthcoming), we first employ the Cox proportional hazard model (Cox 1972) to conduct our test: [54]

$$\lambda(t|X) = \lambda_0(\beta_0 I * t)\exp(\beta' X).$$

The dependent variable in the model is whether there is a voluntary CEO turnover at the time of a year. The hazard model accounts for both the occurrence and timing of turnover and allows for the inclusion of time-varying co-variates. We allow baseline hazards to vary across industries to capture the difference in turnover patterns in different industries. Our key independent variable is *Duration*, and *Hypothesis 1* predicts that it has a negative coefficient. We also include as controls a number of firm and CEO characteristics that prior literature has shown to affect the

[54] We repeat all the analyses using a Logit model and find that the results, not reported for brevity, are robust.

probability of CEO turnover. The firm characteristics we include are *Ind. adj. stock return*, *Firm size*, *Volatility* and *Block holder*. The last variable is a dummy variable that identifies the presence of a block holder with more than 5% shareholding in the firm. The set of CEO characteristics we include are *Tenure*, *Age*, *Stock holding*, and *Duality*. In all regressions, we also include year fixed effects, and the standard errors we estimate are robust to heteroskedasticity and clustered at the three-digit SIC code industry level.

The results from Column (1) of Table 2.3 show that the coefficient on *Duration* is negative and significant. This indicates that a CEO with longer pay duration is less likely to leave the firm voluntarily. From the coefficient on the control variables, we find that older CEOs and CEOs who have higher equity ownership in the firm are less likely to leave the firm voluntarily. In Column (2), we repeat our estimates employing a linear probability model. We do this for two reasons. First, employing the linear probability model helps us estimate the economic significance of our results more easily and in an intuitive manner. Second, with the linear probability model, we can control for firm fixed effects. The inclusion of firm fixed effects ensures that we control for all time-invariant firm characteristics. We are unable to include firm fixed effects in the non-linear COX hazard model because of the incidental parameters problem (Neyman and Scott 1948). Our results in Column (2) are consistent with those in Column (1) and show that CEOs with longer duration pay are less likely to voluntarily leave their firm. Our findings are also economically significant. The negative coefficient on *Duration* in Column (2) implies that a one standard deviation increase in *Duration* (0.97) results in a decrease in the annual probability of a voluntary CEO turnover by 1.16%. In comparison, the unconditional probability of a voluntary CEO turnover any year in our sample is 2.0% with a standard deviation of 14.1%. Thus, a one standard deviation increase in *Duration* is associated with a 58% decrease in the likelihood of a voluntary CEO turnover as compared to the sample mean or a 8.2% decrease in the standard deviation of the voluntary CEO turnover probability.

In Columns (3)-(4), we estimate the effect of pay duration on the likelihood of a non-CEO executive jumping ship. In Column (3), we employ the COX model with the dependent variable being the indicator of a senior executive jumping ship at the time of a year. Apart from the usual set of controls, we also control for incidences of CEO turnover during the previous two years (*CEO Turnover*) and for incidences when there is an external hire in replacing the departing CEO during the previous two years (*External hire*). We do this to ensure that executive turnovers, which may result from a change in the top management of the firm, do not affect the coefficient on *Duration*.

The result in Column (3) shows that the coefficient on *Duration* is negative and statistically significant. This highlights that a longer pay duration lowers the likelihood of a senior executive jumping ship. We also find that senior executives of larger firms (positive coefficient on *Firm size*), those with shorter tenure (negative coefficient on *Ln(Tenure)*), and younger executives (negative coefficient on *Age*) are more likely to jump ship. Also, the likelihood of an executive jumping ship does not appear to be associated with firm performance. Column (4) presents the result of the linear probability model with time and firm fixed effects. Here again, we find that the coefficient on *Duration* is negative and significant. Our estimates are also economically significant. The coefficient on *Duration* in Column (4) implies that a one standard deviation increase in *Duration* (0.88) is associated with a 2.4% reduction in the probability of an executive jumping ship. In comparison, the unconditional probability of an executive jumping ship in our sample is 1.6% with a standard deviation of 12.6%. Thus, our estimates are extremely significant.

In Table 2.4, we repeat our tests in Table 2.3 with our alternative measure of pay duration that includes all the unvested stock and option grants from prior years. In this sense, it is a more comprehensive measure of all outstanding deferred pay from stock and option grants. We find that the coefficient on *Duration-2* is negative and significant in all the columns. The economic significance of the result is comparable to those in Table 3. The coefficient in Column (2) (Column (4)) implies that a one standard deviation increase in *Duration-2* that is 0.86 years (0.80 years) is

associated with a 1.29% (2.48%) reduction in the probability of a voluntary CEO turnover (non-CEO executive jumping ship). Thus, our results are robust to the alternative measure of pay duration.

Test of causality

The negative correlation between *Duration* (or *Duration-2*) and voluntary executive turnover, documented in Tables 3 and 4, may be subject to an omitted variable bias, and thus may not necessarily imply a causal effect of deferred pay on turnover. The important firm- and executive-level omitted variable that may affect both pay duration and the likelihood of executive turnover are the importance of firm-specific knowledge and perception of executive talent by the board, respectively. Specifically, as discussed earlier, firms where firm-specific knowledge is valuable may offer longer duration pay to incentivize executives' investment in such knowledge. To the extent such firm-specific knowledge is less valuable outside the firm, executives of such firms may have less valuable outside options and hence may stay longer with the firm. Similarly, all else equal, boards may grant longer duration pay to executives who they perceive to be more talented. Given the favorable internal perception, such executives may also find it optimal to remain with the firm.

We implement a two-stage instrumental variable regression to examine the causal effect of pay duration on voluntary turnover. Our strategy is to identify executive-years in which a large prior-year stock or option grant vests (*Large vesting*). We use these lumpy vesting episodes as instances that significantly reduce an executive's pay duration, and estimate its effect on executive voluntary turnover. To circumvent the endogeneity of stock/option grant, we focus on grants that were awarded more than two years ago. To the extent that these grants were awarded in the distant past, their vesting is unlikely to be correlated with time-varying firm- and executive-level omitted variables and executive voluntary turnover. Edmans, Fang and Lewellen (2014) use a similar

instrument to study the effect of stock vesting schedules on managerial myopia as reflected in corporate investment decisions.

Our identifying assumption in this test is two-fold. First, we assume *Large vesting* will be correlated with *Duration-2*. This is mechanical because *Duration-2* includes prior year grants in its calculation. Vesting of a large stock or option grant during a year is likely to reduce *Duration-2*.[55] The second identifying assumption is the exclusion restriction which assumes that *Large vesting* is correlated with voluntary turnover only to the extent it affects *Duration-2*. We believe this is reasonable because, a) *Duration-2* adequately captures the effect of *Large vesting* on the amount and length of deferred pay; b) since we focus on the vesting of grants that are more than two years old, the vesting is unlikely to be correlated with time-varying firm and executive specific factors.

We present the results of this two-stage IV regression in Table 2.5. Given the consistent effect of pay duration on voluntary turnover for CEOs and non-CEO executives, as shown in Tables 3 and 4, and the expected identical marginal impact of *Large vesting* on pay duration across these two groups of executives, we pool CEOs and non-CEOs in this regression. Moreover, IV regression only allows the linear probability model to be employed here. In the first stage, we regress *Duration-2* on *Large vesting* and the set of control variables in Table 2.4, where *Large vesting* is defined as a dummy that equals one if the largest stock/option grant in prior years vests and zero otherwise. Consistent with our conjecture, we find that *Large vesting* results in a decrease in *Duration-2* and the effect is significant. We also find that *Large vesting* is a strong instrument as seen from the F-value for the first stage regression of 12.51.[56] More interestingly, the results of the second stage regression show that the coefficient on *Duration-2* remains negative and significant,

[55] Note that vesting of a large grant will increase *Duration-2* only if the firm does replenish the vested stock and options with an equal sized grant with a longer vesting schedule. In our data, we find that the correlation between *Large vesting* and *Duration-2* is negative and significant. This is consistent with firms not replenishing a vested grant.

[56] Note that a F-value over 10 is typically considered the sign of a strong instrument (Cameron and Trivedi 2005).

consistent with the findings in Table 4. In Columns (3) and (4), we repeat the estimate after including firm fixed effects and again find that the coefficient on *Duration-2* is negative and significant.

In comparing Column (4) to Column (2), we find that the coefficient on *Duration-2* drops to a sixth after inclusion of firm fixed effects. This highlights the importance of unobserved, firm-level, time-invariant factors for executive turnover. The second interesting fact is when we compare the coefficient on *Duration-2* in Column (4) to those in Columns (2) and (4) of Table 2.4, we find that the coefficient in our IV specification is significantly larger than that in the OLS specification. Note that when we estimate with an OLS specification combining CEO and non-CEO voluntary turnover, we find that the coefficient on *Duration-2* is -0.049 and statistically significant. The larger coefficient in the IV specification as compared to that in the OLS specification indicates that unobserved omitted factors that affect both *Duration-2* and voluntary turnover are likely to be biasing the coefficient downward.

A possible reason for the negative bias is the presence of other factors that may bond an executive to the firm and reduce the likelihood of voluntary turnover. In the presence of such factors, firms may find it optimal to reduce the risk imposed on the executive and award pay with short vesting schedule. One such bonding mechanism could be if the executive is also one of the promoters. Such executives are unlikely to leave the firm voluntarily and in response, firms may award a low duration pay. Similarly, older executives with significant firm-specific skill may also be less likely to leave the firm voluntarily. For such executives as well, firms may find no need to award pay with long vesting schedule, especially if the executive's remaining time to retirement is short.

To summarize, consistent with *Hypothesis 1*, we find that the likelihood of a voluntary CEO turnover and that of a non-CEO executive jumping ship are lower when they have longer pay

duration. And our further test suggests that the effect of pay duration on voluntary executive turnover is causal.

2.4.2 Pay Duration and Forced Turnover

In Table 2.6, we analyze the effect of CEO pay duration on the likelihood of a forced CEO turnover. To the extent that a longer pay duration identifies firms with higher costs of changing CEOs, *Hypothesis 2* predicts that a CEO with longer pay duration is less likely to experience a forced turnover. In Column (1), we present the results of estimating the Cox hazard model on forced CEO turnovers. Consistent with *Hypothesis 2,* the negative and significant coefficient on *Duration* shows that CEOs with longer pay duration are less likely to be forced out. We also find that firms with lower industry adjusted stock returns (negative coefficient on *Ind adj. stock return*) and firms with more volatile stock (positive coefficient on *Volatility*) are more likely to experience a forced CEO turnover. Also, CEOs of larger firms (positive coefficient on *Size*), younger CEOs (negative coefficient on *Age*) and those with lower shareholding (negative coefficient on *Shareholding*) are more likely to be forced out.

In Column (2), we repeat our estimates with a linear probability model and find consistent results as in Column (1). Our estimates are economically significant. The coefficient on *Duration* in Column (2) indicates that a one standard deviation increase in CEO pay duration (0.97) is associated with a 1.07% reduction in the probability of a forced CEO turnover. In comparison, the average probability of a forced CEO turnover in our sample is 1.86%. Another way to put the economic significance of the effect of *Duration* in context is to compare its effect to that of firm performance. The coefficient on *Ind adj. stock return* in Column (2) implies that a one standard deviation increase in *Ind adj. stock return* (0.33) is associated with a 0.76% reduction in the annual probability of a forced CEO turnover. Thus, the effect of pay duration is about 1.4 times that of firm performance.

Although the effect of pay duration on forced turnover is not expected to be causal, there are three potential explanations, outlined in Section 2, that might account for a negative correlation between pay duration and forced turnover. These are managerial talent, the importance of firm-specific knowledge, and weak corporate governance. In the subsequent tests, we explore the importance of these explanations for the observed negative correlation.

Boards may grant a longer duration pay to more talented CEOs and may also be more reluctant to fire such CEOs. To the extent managerial talent is time invariant, we follow prior literature and use managerial fixed effect to proxy for managerial talent (Bertrand and Schoar 2003) and repeat our tests in Column (3). Thus, the specification in column (3) includes CEO fixed effects in addition to industry and time fixed effects. Comparing the coefficient in column (3) to that in column (2), we find that inclusion of executive fixed effect has a marginal effect on the size of the coefficient on *Duration* but significantly increases the standard errors which results in the coefficient turning insignificant. This provides evidence consistent with manager fixed effects being important for the *Duration*-forced turnover relationship.

One disadvantage of using manager fixed effects to account for executive talent is that one does not have an average point estimate of the effect of talent. To get such an estimate in alternate tests (results are untabulated, but available upon request), we identify a set of CEOs for whom we can obtain pay duration in their prior employment, *Prior duration*. To the extent executive talent is time invariant and to the extent it affects pay duration, we expect talented executives to obtain longer duration pay in their prior employment as well. To the extent executive talent affects the duration-turnover relationship, we expect a negative correlation between *Prior duration* and the likelihood of forced turnover. Consistent with CEO talent being an important explanation for the negative pay-duration-forced-turnover correlation, we find that the coefficient on *Prior duration* is negative and statistically significant.

Second, in firms where managers' firm-specific knowledge is more important, the board is likely to be more tolerant in its executive force-out decision in order to encourage executives' investment in acquiring firm-specific knowledge. There are two possible ways to isolate the effect of the importance of firm-specific knowledge in driving the forced turnover-duration relationship. Similar to using manager fixed effects to estimate the role of talent, one can use firm fixed effects to proxy for the importance of firm-specific knowledge and estimate its effect on the coefficient on *Duration*. We do this in column (4) and find that inclusion of firm fixed effects does not significantly affect the size of the coefficient on *Duration*. Thus, the negative correlation between *Duration* and forced CEO turnover in Column (2) appears mainly due to within-firm changes in *Duration* and forced CEO turnover.

Note that the small effect of firm fixed effects on the duration-forced CEO turnover relationship does not necessarily imply that investment in firm-specific knowledge is not important for the duration-forced CEO turnover relationship. The need for investment in firm-specific knowledge could be time varying and firm fixed effects are unlikely to capture this. To isolate the effect of time-varying importance of firm-specific knowledge on the duration-forced turnover relationship, in unreported tests, we estimate the effect of *Average duration* on the likelihood of a forced CEO turnover. *Average duration* is the mean pay duration of all other senior executives in the firm in a given year. When firms want executives to invest in firm-specific knowledge, they are likely to offer long duration pay to *all* the top executives. We find that the coefficient on *Average duration* is negative and significant. This is consistent with the importance of firm-specific knowledge as an important channel that underlies the negative correlation between CEO pay duration and forced turnover. [57]

57 We have *Prior Duration* for only a small fraction of our executives. This limits our ability to compare the relative importance of firm-specific knowledge and executive talent in affecting the Duration-forced turnover relationship by including both *Average duration* and *Prior duration* in the same specification. When we do so in unreported tests, we find that the coefficients on both variables are negative but insignificant.

Third, we examine if the negative pay-duration-forced-turnover correlation may result from poor corporate governance. This can happen if a captured board (that acts in the interest of the CEO) is reluctant to fire a CEO with significant unvested stock and option grants as the CEO may lose the unvested grants. To test this, we use the fraction of independent directors on the firm's board as our proxy for board strength and corporate governance, and create a dummy variable, *Independent,* that takes a value one if the fraction of outsiders on a firm's board of directors is above the sample median in a given year. We then repeat our tests after including an interaction term between *Independent* and *Duration* to see if the negative effect of pay duration on the likelihood of forced CEO turnover is concentrated in firms with less independent boards. This would imply a positive coefficient on the interaction term. We present the results in Table 7. In Column (1), we repeat the estimate from Column (1) of Table 6 (the Cox hazard model) which relates CEO pay duration to the likelihood of a forced turnover. In Column (2), we repeat this estimate after including *Independent* and an interaction term *Duration X Independent.* We find that the coefficient on the interaction term is negative and significant, which suggests that the correlation between pay duration and forced CEO turnover is stronger in firms with more independent boards. In the next two columns, we repeat the analysis with the linear probability model and find consistent results. This finding is inconsistent with poor corporate governance as an explanation for the negative duration-forced-turnover relation. Instead, combined with our findings about the other two explanations, it suggests that more independent boards are more likely to incorporate the considerations of CEO talent and the importance of firm-specific knowledge into their CEO force-out decisions.

We also analyze the effect of pay duration on the likelihood of involuntary turnover for non-CEO senior executives. As mentioned before, due to the paucity of details on non-CEO turnovers from public sources, we classify non-CEO turnovers that do not involve the executive retirement or the executive jumping ship to another firm as being involuntary. Note that this

classification is bound to be noisy, and this set of turnovers could also include some voluntary turnovers. This is not a serious problem for us qualitatively, because we expect *Duration* to lower the likelihood of both voluntary and involuntary turnovers according to *Hypotheses 1 & 2*. The caveat is with the precision of the estimate of the pay duration sensitivity of non-CEO executives' involuntary turnover. To this end, we choose to not tabulate the results of our analysis in this part (available upon request). The untabulated results strongly support *Hypothesis 2*. They show that non-CEO executives with longer pay duration are less likely to experience an involuntary turnover. Also, the estimates of the effect of pay duration are highly significant in economical magnitudes. The coefficient estimated from the linear probability model implies that a one standard deviation increase in *Duration* (0.88) is associated with a 1.85% decrease in the probability of an involuntary executive turnover. In comparison, the average probability of an involuntary executive turnover in our sample is 3.07%.

Overall, our findings are consistent with *Hypothesis 2* that executives with longer pay duration are less likely to experience an involuntary turnover. And this negative duration-turnover association is not due to poor corporate governance, but can be explained by the importance of firm-specific knowledge and managerial talent, both of which are positively related to pay duration and negatively related to forced turnover.

2.4.3 Pay Duration and Performance-Sensitivity of Forced Turnover

To test *Hypothesis 3*, we estimate how pay duration affects the sensitivity of forced CEO turnover to firm performance in Table 2.8. In Column (1), we repeat the estimate from Column (1) of Table 2.6 which relates CEO pay duration to the likelihood of a forced turnover. In Column (2), we repeat the estimation of the Cox hazard model after including an interaction term *Duration X Ind. adj. stock return.* We find that the coefficient on the interaction term is positive and significant. This indicates that, for CEOs with longer duration pay, the likelihood of a forced turnover is less

sensitive to stock returns. In Column (3), we repeat the estimates with a linear probability model and again find that the coefficient on the interaction term is positive and significant. In Column (4), we repeat our estimates with firm (in addition to time) fixed effects and obtain similar results.

Since our duration measure is a continuous variable, it is difficult to interpret the economic significance of the coefficient on the interaction term. To get a better sense of the economic significance, in Column (5), we repeat our estimation after replacing *Duration* with a dummy variable, *High duration,* that takes a value one for the CEOs whose pay duration is above the sample median for that year. The coefficient on *Ind. adj. stock return* is an estimate of the sensitivity of forced CEO turnover to stock returns for a CEO with below the sample median pay duration, while the sum of the coefficients on *Ind. adj. stock return* and *Duration* X *Ind. adj. stock return* is an estimate of the sensitivity of forced CEO turnover to stock returns for a CEO with above the sample median pay duration. Our estimates show that the coefficient on *Ind. adj. stock return* is -.038. This is twice the estimate in Column (3). It indicates that forced CEO turnover is twice as sensitive to stock returns for CEOs with below the sample median pay duration as compared to the sample average sensitivity. We also find that the coefficient on the interaction term is .034. And in unreported tests, we find that we cannot reject the null that the sum of the coefficients on *Ind. adj. stock return* and the interaction term *Duration X Ind. adj. stock return* is equal to 0. This indicates that in our sample, forced CEO turnover is not sensitive to stock performance for CEOs with above the sample median pay duration. That is, all the sensitivity to stock returns found in Column (3) is driven by CEOs with below the sample median pay duration.

Our finding is consistent with perceived switching costs affecting the turnover-performance sensitivity (Taylor (2010)). Specifically, higher perceived switching cost may explain the board's greater forbearance in tolerating poor firm performance and waiting longer before firing the CEO. Possible sources of switching costs could be the loss of firm-specific knowledge and high perceived managerial talent. Our results of the lower performance-turnover sensitivity among CEOs with

high duration pay is consistent with pay duration capturing both the importance of firm-specific knowledge and managerial talent.

2.4.4 Pay Duration and Internal CEO Hiring

In this section, we perform tests of *Hypothesis 4* that has predictions on whether a firm will select an internal or external candidate as the replacement CEO. To the extent that the average duration of the top executives of a firm is a proxy for the importance of firm-specific knowledge in the firm, we expect firms that offer longer average pay duration to their top executives to be more likely to hire an internal candidate to replace the CEO. On the other hand, internal hiring is also an important means of talent retention because insiders would have stronger incentives in investing in acquiring firm-specific knowledge. In Table 2.9, we estimate a linear probability model where the dependent variable is *External*, a dummy variable that identifies firms that select an external candidate as the replacement CEO. Our main independent variable is *Average duration*, the average pay duration of all senior executives (except the departing CEO) of the firm included in ExecuComp. We include as a control variable, *Forced turnover*, a dummy variable that identifies if the departing CEO was forced out. We include this variable because prior research shows that firms are more likely to hire an outsider if the predecessor was forced out (e.g., Parrino 1997). We also include *Ind. adj. stock return*, *Volatility*, *Firm size*, and *Block holder* as additional control variables.

The result in Column (1) of Table 2.9, estimated with industry and year fixed effects, shows that firms are less likely to hire an external candidate if the senior executives in the firm have longer pay duration. This finding is robust after including other control variables in the regression, as shown in Column (2). One concern with our estimates is that they could be biased by the quality of the internal candidate who is chosen to be the CEO. Firms that have a better quality internal candidate are likely to offer her a contract with longer pay duration and also select her to be the

replacement CEO. To address this concern, we repeat our tests by calculating *Average duration* after excluding the pay duration of the internal candidate who becomes the replacement CEO. In untabulted results, we again find that firms that offer longer duration pay contracts to their senior executives are less likely to hire an external candidate.

To the extent that the importance of firm-specific knowledge is common across firms in an industry, we expect firms in industries with higher pay duration to be more likely to hire an internal candidate to replace their CEO. We test this in Column (3) after replacing *Average duration* in Column (1) with *Industry duration*, which is defined as the average pay duration of CEOs in the industry (defined based on three-digit SIC codes).[58] We find that firms from industries with higher average pay duration are more likely to hire an internal candidate, as seen from the negative and significant coefficient on *Industry duration*. In Column (4), the coefficient on *Industry duration* remains negative but becomes insignificant after including other control variables. The impact of *Industry duration* appears to be encapsulated by that of the variations of firm characteristics across industries.

As a summary, we document that firms are more likely to choose an insider as their new CEO if other senior executives have been granted pay with longer duration. It is consistent with the firm-specific knowledge being an important consideration in firms' CEO succession decision.

2.5 Conclusion

We argue that deferred pay enables firms to retain managerial talent. Firms typically defer the stock component of pay. The forfeiture of all unvested stock pay upon executive turnover, voluntarily or involuntarily, increases the cost of managerial departure. Using the duration measure of executive compensation, introduced by Gopalan, et al. (forthcoming), that captures both the

[58] In this test, only year fixed effect is included.

magnitude and the vesting length of stock pay, we find that there is a negative causal effect of pay duration on voluntary executive turnover. We also find that pay duration is negatively correlated with involuntary executive turnover, the sensitivity of involuntary turnover to firm performance, and the likelihood of external hiring in CEO succession. These findings are consistent with a significant role of the importance of firm-specific knowledge and managerial talent that underlie both the design of pay duration and executive turnover decisions in firms.

Our study suggests that firms' compensation policy and management turnover decisions are interlinked. It highlights the effectiveness of explicit compensation contract in talent retention, which has received little attention in the prior literature on managerial compensation. We leave it for future research to explore potentially interesting implications of the joint roles of managerial compensation contract – incentive provision and talent retention – on firms' financial policies and corporate decisions.

2.6 Appendix

2.6.1 Construction of the Alternative Duration Measure – *Duration-2*

Duration-2 augments the baseline duration measure *Duration* by including all grants awarded in prior years. Among them, all vested stocks and stock options awarded in prior years are assigned a vest period of 0; detailed vesting schedule of all unvested grants that were awarded during 2006-2009 is obtained from *Equilar*; for all unvested grants that were awarded prior to 2006, we need to estimate their vesting schedule using the detailed information provided in *ExecuComp* on the total outstanding unvested stocks and stock options as of each year end. The procedure of estimating the vesting schedule of unvested pre-2006 grants is described as follows.

For stock options, we first isolate the unvested pre-2006 grants by subtracting the unvested post-2006 grants (aggregated from *Equilar*) from the total outstanding unvested grants obtained from *ExecuComp*. To do so, we need to merge *Equlilar* and *ExecuComp* using executive identity, year, exercise price, and expiration date. We then use the year-on-year change in the total unvested pre-2006 grants to gauge their vesting schedule with the assumption that these grants vest at the end of 2011. For restricted stocks, we do not need such an assumption since there is no expiration date or exercise price for restricted stocks. And we follow the same procedure in the estimation of their vesting schedule except that we merge *Equilar* and *ExecuComp* using executive identity and year only.

2.6.2 Variable Definitions

- *Age* is the age of the executive (in years)
- *Block holder* is an indicator variable that takes a value of one if there is at least one institution holding more than 5% of the firm's outstanding stocks, and zero otherwise.
- *Duality* is an indicator variable that takes a value of one if the CEO is also the chairperson, and zero otherwise.
- *External hire* is an indicator variable that takes a value of one if an outsider is hired as a CEO, and zero otherwise.
- *Firm size* is the natural log of the total assets of the firm.
- *High duration* is an indicator variable that takes a value of one if the pay duration of the executive is above the sample median, and zero otherwise.
- *Ind. adj. stock return* is the firm's annual stock return from the previous year net of the mean industry stock return.
- *Independent* is an indicator variable that takes a value of one if the fraction of outsiders on the firm's Board of Directors is above the median in a given year. Any director who is an employee of the firm or has some affiliation with the firm is classified as an insider.
- *Leverage* is the sum of long-term debt and debt in current liabilities divided by total assets.
- *Market to book* is the ratio of market value of total assets to book value of total assets.
- *R&D/Asset* is the ratio of research and development expenditure over the book value of total assets. Missing values are replaced with zero.
- *Stock holding* is the fraction of outstanding shares owned by the executive.
- *Tenure* is the number of years an executive has been in office.
- *Volatility* is the standard deviation of the firm's stock return over the 12 months.

2.6.3 References

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2.6.4 Tables

Table 2.1

Summary Statistics

This table presents descriptive statistics of our sample. Panels A and B present characteristics of CEOs and other executives, respectively. Panel C presents firm characteristics. *Duration* and *Duration-2* are measures of executive pay duration discussed in Section 3.2. All other variables are defined in Appendix B.

PANEL A: CEOs

	N	Mean	SD	P25	Median	P75
Duration	6127	1.455	0.969	0.637	1.658	2.134
Duration-2	6100	1.487	0.858	0.947	1.624	2.040
Age	6127	54.865	7.194	50	55	60
Tenure	6127	7.481	7.096	2.499	5.419	10
Stock holding	6127	12.724	23.884	0.71	2.911	10.381
Duality	6127	0.514	0.5	0	1	1

PANEL B: Other executives

	N	Mean	SD	P25	Median	P75
Duration	18005	1.259	0.882	0.491	1.377	1.891
Duration-2	17979	1.291	0.804	0.722	1.397	1.826
Age	18005	51.101	7.358	46	51	56
Tenure	18005	14.873	11.707	6	12	22
Stock holding	18005	3.31	14.956	0.11	0.495	1.456

Panel C: Firm characteristics

	N	Mean	SD	P25	Median	P75
Ind adj. stock return	6127	0.03	0.328	-0.172	-0.003	0.184
Volatility	6127	0.42	0.228	0.26	0.361	0.509
Firm size	6127	7.745	1.747	6.489	7.622	8.894
Market to book	6108	1.706	0.99	1.073	1.367	1.967
Leverage	6104	0.226	0.196	0.058	0.197	0.341
R&D/Asset	6125	0.024	0.047	0	0	0.024

Table 2.2

Univariate Evidence on Pay Duration and Turnover

This table presents univariate evidence on pay duration and turnover. Panels A through D pertain to subsamples of voluntary CEO turnover, senior executives jump-ships, forced CEO turnover, and involuntary executive turnovers, respectively. *Duration* and *Duration-2* are measures of executive pay duration discussed in Section 3.2. All other variables are defined in Appendix B. In each panel, the sample is further segmented into two groups of turnover vs non-turnover years. T-test is conducted on the difference between the two groups, which is reported in the last column. *, **, and *** indicate significance at 10%, 5% and 1%, respectively.

PANEL A: Voluntary CEO Turnover

	Turnover years		Non-turnover years		
	N	Mean	N	Mean	Difference
Duration	125	1.066	6002	1.463	-0.397***
Duration-2	124	1.077	5976	1.496	-0.419***
Ind adj. stock return	125	-0.044	6002	0.031	-0.075**
Age	125	52.744	6002	54.909	-2.165***
Tenure	125	6.661	6002	7.499	-0.838
Stock holding	125	6.661	6002	12.851	-6.19***
Duality	125	0.464	6002	0.515	-0.051

PANEL B: Executives jumping ship

	Turnover years		Non-turnover years		
	N	Mean	N	Mean	Difference
Duration	289	0.413	17716	1.273	-0.86***
Duration-2	289	0.461	17690	1.310	-0.844***
Ind adj. stock return	289	-0.009	17716	0.041	-0.05**
Age	289	49.197	17716	51.132	-1.935***
Tenure	289	12.014	17716	14.92	-2.906***

PANEL C: Forced CEO turnover

	Turnover years		Non-turnover years		
	N	Mean	N	Mean	Difference
Duration	114	0.869	6013	1.466	-0.597***
Duration-2	113	0.869	5987	1.499	-0.630***
Ind adj. stock return	114	-0.18	6013	0.034	-0.214***
Age	114	51.579	6013	54.927	-3.348***
Tenure	114	5.001	6013	7.528	-2.527***
Stock holding	114	6.614	6013	12.84	-6.226***
Duality	114	0.307	6013	0.518	-0.211***

PANEL D: Involuntary turnover of other executives

	Turnover years		Non-turnover years		
	N	Mean	N	Mean	Difference
Duration	553	0.794	17452	1.274	-0.48***
Duration-2	552	0.802	17427	1.307	-0.505***
Ind adj. stock return	553	-0.142	17452	0.046	-0.188***
Age	553	48.915	17452	51.17	-2.255***
Tenure	553	12.221	17452	14.957	-2.736***

Table 2.3

Pay Duration and Voluntary Turnover

This table presents coefficient estimates from Cox proportional hazard model and linear probability model that examine the likelihood of voluntary executive turnovers. Time-to-turnover is right censored. *Duration* is the baseline measure of executive pay duration discussed in Section 3.2. Other explanatory variables are defined in Appendix B. Robust standard errors are clustered by three-digit SIC industry and are reported in parentheses. *, **, and *** indicate significance at 10%, 5% and 1% respectively.

	Voluntary CEO turnover		Voluntary executive turnover	
	(1)	(2)	(3)	(4)
	Cox	OLS	Cox	OLS
Duration	-0.604***	-0.012***	-1.518***	-0.027***
	(0.118)	(0.004)	(0.121)	(0.003)
Ind adj. stock return	-0.429	-0.011	0.083	0.002
	(0.283)	(0.007)	(0.137)	(0.003)
Volatility	-0.307	-0.012	-0.493	-0.016
	(0.780)	(0.022)	(0.455)	(0.010)
Firm size	-0.017	-0.010	0.187***	0.002
	(0.065)	(0.012)	(0.054)	(0.006)
Block holder	-0.121	0.002	-0.237	0.007
	(0.206)	(0.011)	(0.177)	(0.005)
Ln(Tenure)	-0.093	0.037***	-0.185***	-0.002
	(0.158)	(0.008)	(0.063)	(0.002)
Age	-0.034**	-0.001	-0.028***	-0.000**
	(0.013)	(0.001)	(0.007)	(0.000)
Stock holding	-0.023***	-0.000		
	(0.008)	(0.000)		
Duality	0.303	0.008		
	(0.249)	(0.014)		
CEO turnover			0.254	0.005
			(0.216)	(0.006)
External hire			-0.110	-0.011
			(0.371)	(0.011)
Constant		0.125		0.048
		(0.106)		(0.047)
Observations	6113	6127	17986	18005
Adjusted R^2		-0.015		0.053
Pseudo R^2	0.060		0.174	
Fixed effects	Time & Industry	Time & Firm	Time & Industry	Time & Firm

Table 2.4

Pay Duration and Voluntary Turnover – Alternate Duration Measure

This table presents coefficient estimates from Cox proportional hazard model and linear probability model that examine the likelihood of voluntary executive turnovers. Time-to-turnover is right censored. *Duration-2* is the alternative measure of executive pay duration discussed in Section 3.2. Other explanatory variables are defined in Appendix B. Robust standard errors are clustered by three-digit SIC industry and are reported in parentheses. *, **, and *** indicate significance at 10%, 5% and 1% respectively.

	Voluntary CEO turnover		Voluntary executive turnover	
	(1)	(2)	(3)	(4)
	Cox	OLS	Cox	OLS
Duration-2	-0.841***	-0.015***	-1.670***	-0.031***
	(0.152)	(0.005)	(0.119)	(0.003)
Ind adj. stock return	-0.444	-0.012	0.131	0.002
	(0.286)	(0.007)	(0.135)	(0.003)
Volatility	-0.555	-0.012	-0.522	-0.015
	(0.823)	(0.022)	(0.447)	(0.010)
Firm size	-0.006	-0.010	0.199***	0.002
	(0.061)	(0.012)	(0.053)	(0.006)
Block holder	-0.112	0.002	-0.203	0.008*
	(0.204)	(0.011)	(0.183)	(0.005)
Ln(Tenure)	-0.102	0.037***	-0.168***	-0.002
	(0.153)	(0.008)	(0.063)	(0.002)
Age	-0.035**	-0.001	-0.026***	-0.000**
	(0.014)	(0.001)	(0.007)	(0.000)
Stock holding	-0.024***	-0.000		
	(0.008)	(0.000)		
Duality	0.326	0.008		
	(0.252)	(0.014)		
CEO turnover			0.240	0.005
			(0.225)	(0.006)
External hire			-0.156	-0.010
			(0.417)	(0.012)
Constant		0.128		0.056
		(0.108)		(0.047)
Observations	6086	6100	17960	17979
Adjusted R^2		-0.016		0.056
Pseudo R^2	0.072		0.185	
Fixed effects	Time & Industry	Time & Firm	Time & Industry	Time & Firm

Table 2.5

Pay Duration and Voluntary Turnover – IV estimation

This table presents the results of a two-stage instrument variable regression that regress voluntary turnover of CEOs and non-CEO executives on instrumented *Duration-2*. In the first stage regression, *Duration-2* is regressed on *Large vesting*, an indicator variable that takes a value of one if the largest stock and option grants from prior years (at least two years prior) vest, and other explanatory variables. *Duration-2* is the alternative measure of executive pay duration discussed in Section 3.2. Other explanatory variables are defined in Appendix B. Robust standard errors are clustered by firm and are reported in parentheses. *, **, and *** indicate significance at 10%, 5% and 1% respectively.

	Voluntary turnover (CEOs and non-CEOs combined)			
	(1)	(2)	(3)	(4)
	Stage 1	Stage 2	Stage 1	Stage 2
Large vesting	-0.025***		-0.162***	
	(0.013)		(0.011)	
Duration-2		-1.336*		-0.219***
		(0.686)		(0.021)
Ind adj. stock return	0.124***	0.160*	-0.022	-0.006
	(0.015)	(0.088)	(0.015)	(0.004)
Volatility	-0.567***	-0.755*	-0.123***	-0.043***
	(0.039)	(0.393)	(0.042)	(0.012)
Firm size	0.162***	0.216*	-0.014	-0.004
	(0.004)	(0.111)	(0.023)	(0.006)
Block holder	0.159***	0.205*	0.035	0.014**
	(0.014)	(0.110)	(0.022)	(0.006)
Ln(Tenure)	-0.047***	-0.067**	-0.069***	-0.016***
	(0.005)	(0.033)	(0.005)	(0.002)
Age	-0.007***	-0.009**	-0.003***	-0.001***
	(0.001)	(0.005)	(0.001)	(0.000)
Constant	0.674***	0.949**	1.871***	0.439***
	(0.053)	(0.470)	(0.178)	(0.064)
Observations	24079		24079	
F-statistic	12.51		9.51	
Fixed effects	Time & Industry		Time & Firm	

Table 2.6

Pay Duration and Forced CEO Turnover

This table presents coefficient estimates from Cox proportional hazard model and linear probability model that examine the likelihood of forced CEO turnover. Time-to-turnover is right censored. *Duration* is the baseline measure of executive pay duration discussed in Section 3.2. Other explanatory variables are defined in Appendix B. Robust standard errors are clustered by three-digit SIC industry and are reported in parentheses. *, **, and *** indicate significance at 10%, 5% and 1% respectively.

	(1)	(2)	(3)	(4)
	Cox	OLS	OLS	OLS
Duration	-0.662***	-0.011***	-0.010	-0.012**
	(0.132)	(0.003)	(0.006)	(0.005)
Ind adj. stock return	-1.251***	-0.023***	-0.010	-0.014*
	(0.417)	(0.006)	(0.007)	(0.008)
Volatility	1.960**	0.053***	0.055*	0.049*
	(0.773)	(0.018)	(0.033)	(0.026)
Firm size	0.191***	0.004***	-0.022**	-0.003
	(0.067)	(0.001)	(0.010)	(0.010)
Block holder	0.299	0.003	-0.003	0.004
	(0.264)	(0.004)	(0.006)	(0.006)
Ln(Tenure)	-0.064	0.003	-0.017	0.054***
	(0.168)	(0.003)	(0.015)	(0.011)
Age	-0.047***	-0.001***	0.010**	-0.004**
	(0.017)	(0.000)	(0.005)	(0.002)
Stock holding	-0.016**	-0.000**	-0.000	-0.000
	(0.008)	(0.000)	(0.000)	(0.000)
Duality	-0.184	-0.005	-0.014	-0.020
	(0.255)	(0.004)	(0.010)	(0.013)
Constant		0.044*	-0.328	0.163
		(0.025)	(0.268)	(0.121)
Observations	6113	6127	6127	6127
Adjusted R^2		0.030	0.355	-0.002
Pseudo R^2	0.118			
Fixed effects	Time & Industry	Time & Industry	Time, Industry & CEO	Time, Industry & Firm

Table 2.7

Pay Duration and Forced CEO Turnover: Variation with Corporate Governance

This table presents coefficient estimates from Cox proportional hazard model and linear probability model that examine the likelihood of forced CEO turnover. Time-to-turnover is right censored. *Duration* is the baseline measure of executive pay duration discussed in Section 3.2. *Independent* is an indicator variable that takes a value of one if the fraction of outsiders on the firm's board of directors is above the sample median in a given year. Other explanatory variables are defined in Appendix B. Robust standard errors are clustered by three-digit SIC industry and are reported in parentheses. *, **, and *** indicate significance at 10%, 5% and 1% respectively

	(1)	(2)	(3)	(4)
	COX		Linear probability model	
Duration	-0.662***	-0.351*	-0.011***	-0.005
	(0.132)	(0.201)	(0.003)	(0.003)
Ind adj. stock return	-1.251***	-1.299***	-0.023***	-0.022***
	(0.417)	(0.496)	(0.006)	(0.006)
Independent		-0.034		0.007
		(0.228)		(0.004)
Independent X Duration		-0.655***		-0.017***
		(0.250)		(0.005)
Volatility	1.960**	1.714*	0.053***	0.041**
	(0.773)	(1.015)	(0.018)	(0.019)
Firm size	0.191***	0.204**	0.004***	0.004**
	(0.067)	(0.084)	(0.001)	(0.002)
Block holder	0.299	0.410	0.003	0.006
	(0.264)	(0.340)	(0.004)	(0.004)
Ln(Tenure)	-0.064	0.056	0.003	0.005
	(0.168)	(0.170)	(0.003)	(0.003)
Age	-0.047***	-0.054***	-0.001***	-0.001***
	(0.017)	(0.016)	(0.000)	(0.000)
Stock holding	-0.016**	-0.010	-0.000**	-0.000
	(0.008)	(0.007)	(0.000)	(0.000)
Duality	-0.184	-0.185	-0.005	-0.005
	(0.255)	(0.261)	(0.004)	(0.005)
Constant			0.044*	0.031
			(0.025)	(0.028)
Observations	6113	5304	6127	5316
Adjusted R^2			0.030	0.029
Pseudo R^2	0.118	0.121		
Fixed effects	Time & Industry	Time & Industry	Time & Industry	Time & Industry

Table 2.8

Pay Duration and the Sensitivity of Forced CEO Turnover to Firm Performance

This table presents coefficient estimates from Cox proportional hazard model and linear probability model that examine the likelihood of forced CEO turnover. Time-to-turnover is right censored. *Duration* is the baseline measure of executive pay duration discussed in Section 3.2. *High duration* is a dummy variable that takes the value of one if *Duration* is above the sample median in a given year. Other explanatory variables are defined in Appendix B. Robust standard errors are clustered by three-digit SIC industry and are reported in parentheses. *, **, and *** indicate significance at 10%, 5% and 1% respectively.

	(1)	(2)	(3)	(4)	(5)
	COX		Linear probability model		
Duration	-0.662***	-0.574***	-0.011***	-0.012**	
	(0.132)	(0.130)	(0.003)	(0.005)	
Ind adj. stock return	-1.251***	-0.977**	-0.019***	-0.013	-0.038***
	(0.417)	(0.397)	(0.005)	(0.008)	(0.011)
Duration X Ind. adj. return		0.571**	0.025***	0.018*	
		(0.263)	(0.007)	(0.010)	
High duration					-0.019***
					(0.005)
High duration X Ind. adj. stock return					0.034***
					(0.013)
Volatility	1.960**	1.888**	0.050***	0.050*	0.053***
	(0.773)	(0.771)	(0.018)	(0.026)	(0.018)
Firm size	0.191***	0.192***	0.004***	-0.003	0.003***
	(0.067)	(0.068)	(0.001)	(0.010)	(0.001)
Block holder	0.299	0.321	0.003	0.005	0.002
	(0.264)	(0.269)	(0.004)	(0.006)	(0.004)
Ln(Tenure)	-0.064	-0.069	0.003	0.053***	0.003
	(0.168)	(0.172)	(0.003)	(0.011)	(0.003)
Age	-0.047***	-0.046***	-0.001***	-0.004**	-0.001***
	(0.017)	(0.017)	(0.000)	(0.002)	(0.000)
Stock holding	-0.016**	-0.016*	-0.000**	-0.000	-0.000**
	(0.008)	(0.008)	(0.000)	(0.000)	(0.000)
Duality	-0.184	-0.188	-0.005	-0.020	-0.006
	(0.255)	(0.256)	(0.004)	(0.013)	(0.004)
Constant			0.041	0.158	0.037
			(0.025)	(0.124)	(0.025)
Observations	6113	6113	6127	6127	6127
Adjusted R^2			0.033	-0.000	0.029
Pseudo R^2	0.118	0.121			
Fixed Effects	Time & Industry		Time & Industry	Time & Firm	Time & Industry

Table 2.9

Effect of Pay Duration of Other Senior Executives on CEO Succession Decision

This table presents the results of a linear probability model that examine the likelihood of outside CEO succession following CEO turnover. The dependent variable takes a value of one if the newly appointed CEO has been with the firm for less than a year prior to the appointment and zero otherwise. *Average duration* is the average pay duration of other top executives than the departing CEO in the firm included in ExecuComp. *Industry avg. duration* is the average pay duration of CEOs from the same industry. Other explanatory variables are defined in Appendix B. Robust standard errors are clustered by clustered by three-digit SIC industry and are reported in parentheses. *, **, and *** indicate significance at 10%, 5% and 1% respectively.

	(1)	(2)	(3)	(4)
	Probability of an external hire			
Average. duration	-0.12***	-0.08*		
	(0.04)	(0.04)		
Industry avg. duration			-0.10*	-0.05
			(0.06)	(0.06)
Ind adj. stock return		-0.11		-0.16**
		(0.10)		(0.07)
Volatility		0.16		0.12
		(0.40)		(0.20)
Firm size		-0.03		-0.05***
		(0.03)		(0.01)
Block holder		-0.15*		-0.11*
		(0.08)		(0.06)
Constant	0.55***	0.81**	0.57***	0.90***
	(0.06)	(0.34)	(0.08)	(0.20)
Observations	429	429	437	437
Adjusted R^2	0.06	0.08	0.01	0.05
Fixed effect	Time and Industry		Time	

Chapter 3: Disagreement-induced CEO Turnover

3.1 Introduction

Under what conditions are CEOs fired and how do boards determine who to replace them with? This is a crucial issue in corporate governance, and has generated an extensive literature that focuses primarily on the relationship between firm performance and CEO turnover and the factors that affect this relationship.[59] While there is broad consensus that firm performance has a statistically significant impact on forced CEO turnover, its economic significance is modest relative to what extant theory suggests.[60] Moreover, Huson, Parrino, and Starks (2001) find that, despite substantial changes in internal governance mechanisms, the relationship between forced CEO turnover and firm performance does not change significantly over time.[61] These are puzzling stylized facts, and suggest the possibility of as-yet-unexplored factors that affect CEO turnover and replacement, factors that go beyond firm performance (see, for example, Brickley (2003)). The purpose of this paper is to propose and test a new explanation for forced CEO turnover that is not directly related to firm performance, and thereby illuminate another determinant of this corporate governance practice.

Our analysis focuses on potential disagreement between management and investors as a determinant of CEO turnover. The basic idea is simple. The decision of whether to continue with

[59] See, for example, Coughlan and Schmidt (1985), Warner, Watts, and Wruck (1988), Weisbach (1988), Jensen and Murphy (1990), Denis and Denis (1995), Denis, Denis, and Sarin (1997), Mikkelson and Partch (1997), Parrino (1997), DeFond and Park (1999), Murphy (1999), Huson, Parrino, and Starks (2001), Engle, Hayes, and Wang (2003), Farell and Whidbee (2003), Huson, Malatesta, and Parrino (2004), Song (2008), Bushman, Dai, and Wang (2010), and Dasgupta, Li, and Wang (2014). Other studies (e.g., Kaplan and Minton, 2012; Jenter and Kanaan, forthcoming) suggest that CEOs are forced out not only due to firms' own performance but also peer firms'.

[60] According to the summary of existing research in Brickley (2003), moving from the top decile to the bottom decile of firm performance increases the probability of a forced CEO turnover by four percentage points.

[61] Kaplan and Minton (2012) find that this relation appears to have intensified in the last few years.

a CEO or force the CEO out depends on the board's assessment of how the firm will perform in the *future.* This depends on strategic decisions the CEO is making at present, whose cash flow implications cannot be unambiguously determined *ex ante* because they are estimates that depend on assumptions that have limited justification based on historical data. This means rational agents may disagree on whether a particular strategy will enhance or destroy firm value (see Kurz (1994)). When investors and the board have a high degree of confidence in the CEO, as reflected in a high level of agreement, they are more likely to endorse the CEO's strategies and continue with the CEO. However, low levels of agreement with the CEO can induce "second guessing" of the CEO's decisions, and even relatively small performance shortfalls may trigger CEO dismissal because they tend to reinforce initial misgivings. This paper uses this intuition and builds on the prior literature on investor-management disagreement (e.g., Garmaise, 2001; Van den Steen, 2005 and 2010b; Boot, Gopalan, and Thakor, 2006 and 2008; Dittmar and Thakor, 2007; Boot and Thakor, 2011) to examine its implications for CEO turnover. We document empirically that disagreement between investors and management on the optimal course of corporate actions has a statistically and economically significant impact on forced CEO turnover.

Specifically, we argue that investors and management may have heterogeneous prior beliefs about the profitability of a firm's future investment opportunities or the strategy of how best to implement its investment decisions. The heterogeneous priors constitute "rational beliefs" in the sense of Kurz (1994), and can generate different opinions about the optimal course of actions for the firm. Moreover, with rational beliefs, they will not revise their beliefs even though it is common knowledge that different prior beliefs exist (Kreps, 1990a); nor will they converge to a common prior even with sufficient additional information provision (Andreoni and Mylovanov, 2012).

The persistence of investor-management disagreement is costly to the firm because higher disagreement implies a lower valuation of the firm and a higher cost of external finance, thereby affecting the firm's security issuance decision (see Dittmar and Thakor (2007)). This means that

the firm may forgo even those projects that its management believes have positive NPV because investor-management disagreement makes the cost of raising external financing prohibitive. Thus, firms with relatively high levels of investor-management disagreement may benefit by replacing their CEOs. Of course, this does not mean that all firms will expeditiously fire CEOs when the level of disagreement exceeds some threshold—the board's ability to do this may be constrained by the "power" of the CEO and the level of entrenchment.[62] Each firm will trade off the benefit of reduced investor-management disagreement due to CEO dismissal against the entrenchment-induced costs/difficulties faced by the board in dismissing the CEO. Cross-sectional heterogeneity in entrenchment means that firms will therefore differ in the extent to which disagreement leads to the CEO being fired.

We use this reasoning to develop three testable hypotheses. First, CEOs are more likely to be forced out when the level of investor-management disagreement is higher, *ceteris paribus*. Second, to the extent that those within the executive suite of the firm are more likely to have similar beliefs among each other than with investors, firms with higher levels of investor-management disagreement are more likely to hire replacement CEOs from outside the firm because an internal successor is likely to be burdened, like her predecessor, with a high level of disagreement with investors. Third, disagreement declines following forced CEO turnover.

Using various measures of investor-management disagreement used in the prior literature, we find strong empirical support for these hypotheses. As for the first hypothesis, we test it using both the Cox proportional hazard model and the logit model, and find that the impact of disagreement on forced turnover is both statistically and economically significant. The odds of forced CEO turnover are 0.42-1.27 times higher following a one-standard-deviation increase in the level of different disagreement measures. Our findings are robust to the introduction of controls for various measures of firm performance as well as other firm characteristics and CEO attributes that

[62] Taylor (2010) suggests that the empirically observed low forced CEO turnover rate is, to a large extent, due to CEO entrenchment.

are previously documented to be related to forced CEO turnover. Moreover, the stock market responds more positively to announcements of disagreement-induced forced turnovers (especially those with an outsider being hired to replace the fired CEO) than to announcements of other instances of forced turnover, which is consistent with the market's anticipation of a post-dismissal decline in disagreement in these firms that will lead to an increase in firm valuation.[63]

We further examine cross-sectional variations in the turnover-disagreement sensitivity that are related to varying costs of disagreement and varying constraints that different firms have in their CEO firing decisions. Our findings buttress support for the disagreement hypothesis in explaining forced CEO turnover, and can be summarized as follows. First, the turnover-disagreement sensitivity is greater for firms that are financially more constrained and thus are likely to be more dependent on external equity financing. To the extent that the adverse impact of disagreement on firm valuation (and thus cost of external financing) is more pronounced in more-constrained firms, these firms tend to be more responsive to disagreement in their forced turnover decisions. Second, consistent with the impact of CEO entrenchment and shareholder governance, firms are less responsive to disagreement in their turnover decisions when CEOs are more entrenched and board oversight is weaker, but more responsive when shareholders have more concentrated ownership and thus can exert more influence on firm decisions.

As for the second hypothesis, we test it using a logit estimation model and find that an external replacement for the departing CEO is more likely when the level of investor-management disagreement is higher. This effect is both statistically significant and economically meaningful. The odds of an external CEO hire are 0.49-1.34 times higher for a one-standard-deviation increase in the level of different disagreement measures.

[63] The average five-day (-2, +2) cumulative abnormal returns around CEO turnover announcements are 1% for disagreement-induced forced turnovers (and 2.54% for those with external CEO successions among them) and -2% for other instances of forced turnover, and the difference is statistically significant.

As for the third hypothesis, we find that investor-management disagreement declines following forced CEO turnover, and the decline in disagreement is statistically significant for all of the disagreement proxies. Moreover, the decline in disagreement is greater if the fired CEO is replaced by an external hire.

We note that disagreement and its impact on forced CEO turnover can survive as equilibrium phenomena even if investors can "vote with their feet" by selling their shares when disagreeing with management. To see this, imagine that there is cross-sectional heterogeneity among investors in their propensity to agree with management. Due to risk aversion, wealth endowment constraints, or both, investors who display higher agreement with management might not be capable or willing to absorb all the shares sold by the selling investors who have lower agreement levels. Consequently, depending on the firm, the equilibrium level of agreement of the *marginal* investor might very well remain relatively low, despite high levels of agreement for inframarginal investors. This may thus lead to forced CEO turnover. This reasoning is consistent with the empirical finding by Parrino, Sias, and Starks (2003) that institutional selling is followed by forced CEO turnover.

Huang and Thakor (2013) use this idea of equilibrium cross-sectional heterogeneity in investor-management disagreement to show that firms can reduce this disagreement by conducting open-market and privately-negotiated share repurchases. Repurchases buy out investors who are more likely to disagree with management and concentrate share ownership in the hands of investors who are less likely to disagree. However, managing disagreement through share repurchases is costly because firms have to use internal cash that may have otherwise been invested in positive-NPV projects. This means that disagreement may be reduced but not eliminated via repurchases, implying that forced CEO turnover in response to disagreement can remain as an equilibrium outcome even when firms can use share repurchases. Moreover, the impact of disagreement on

forced CEO turnover is likely to be stronger and more persistent for financially-constrained firms since they have lesser access to the liquidity needed to repurchase stock and reduce disagreement.

Lastly, one might be concerned that both disagreement and CEO turnover are related to an omitted variable, and thus their correlation might be spurious. For example, an elevation in uncertainty about a firm's growth opportunities or technological development may increase the possibility of different interpretations of the same information by investors and management, and this elevated uncertainty may also induce higher management turnover. To see whether our finding is affected by this possible omitted variable bias, we conduct three additional tests.

First, we conduct a falsification test by examining the relationship between disagreement and voluntary CEO turnover. If it is an omitted variable (uncertainty) that generates the relation between disagreement and forced CEO turnover, then we should expect a similar relation between disagreement and voluntary CEO turnover (not due to mandatory or planned retirement) because uncertainty increases voluntary management turnover too. In contrast, our disagreement hypothesis does not predict such a correlation. Because they believe that their decisions are value-maximizing, CEOs will choose not to depart voluntarily, regardless of the level of investor-management disagreement.

Second, we employ an exogenous shock, caused by distressed mutual fund fire sales, to the composition of investor base and thus investor-management agreement, and examine how it may affect forced CEO turnover. In mutual fund fire sales induced by extreme capital outflows, distressed funds are forced to sell their equity holdings with significant discounts to liquidity providers (Coval and Stafford, 2007). Existing shareholders who are not distressed are unlikely to absorb all these shares due to the holding-capacity limitations explained earlier. It follows that, in equilibrium, the new *marginal* investors in the stock are other liquidity providers who have a lower level of agreement than the existing shareholders (but trade to avail of a liquidity premium). Such a shock that results in a decline in agreement is exogenous because fund fire sales are driven by

extreme capital outflows at the fund level (and the resulting need for liquidity), as opposed to changes in firm fundamentals for the affected stocks. We expect that the exogenous decline in agreement leads to an increased probability of forced CEO turnover.

Third, we take the inclusion of a firm in the S&P 500 as a quasi-natural experiment that causes an increase in institutional ownership of the firm. To the extent that a greater concentration of share ownership in institutional investors enhances shareholder governance, our disagreement hypothesis predicts a stronger turnover-disagreement sensitivity following the S&P 500 inclusion.[64] In contrast, the S&P 500 inclusion has no plausible effect on the uncertainty of a firm and the turnover-disagreement sensitivity in the firm as a result. The results of all the three tests provide strong support for our disagreement hypothesis and show that the omitted variable bias is not likely to be a serious concern.

Our study has several intended contributions. First, it seeks to add to the literature on corporate governance and CEO turnover by showing that investor-management disagreement is an important and previously-ignored factor in the firm's CEO turnover decision, and that the impact of this factor is attenuated by governance variables like CEO entrenchment. Consistent with Taylor (2010), the latter finding explains the low forced CEO turnover rate despite the wide existence of investor-management disagreement in practice. Our study departs from the conventional focus of the prior literature on firm performance in examining CEO turnover. A recent study by Jenter and Lewellen (2014) finds a closer link between CEO turnover and firm performance if the distinction between voluntary and forced turnover is dropped. We show that the impact of investor-management disagreement persists even after controlling for accounting-based and market-based firm performance as well as industry performance.

[64] For the role of institutional investors, both passive and active, in corporate governance, see, for examples, Hartzell and Starks (2003), Chen, Harford, and Li (2007), Gillan and Starks (2007), Appel, Gormley, and Keim (2014), and Crane, Michenaud, and Weston (2014). Crane, Michenaud, and Weston (2014) suggest that even passive investors like index funds have incentives to intervene and exert influence on corporate management through proxy voting and private communication with management. Appel, Gormley, and Keim (2014) show that an increase in ownership by passive institutions is associated with an improvement in corporate governance.

Our study also seeks to improve our understanding of a firm's choice between an internal and an external CEO. Specifically, it shows that CEO selection is a process that seeks a *CEO-firm match*, consistent with the literature in which CEO turnover is an efficient outcome in a competitive assignment framework in which CEOs and firms match on multiple dimensions (e.g., Eisfeldt and Kuhnen, 2013). We provide suggestive evidence that belief-alignment is a consideration in this matching process.

Lastly, our paper contributes to another strand of the literature that has used the idea of disagreement based on differences in beliefs to examine a variety of issues in finance and contracting. They include financing of new industries and technologies (Allen and Gale, 1999), the entrepreneur's choice of private versus public ownership (Boot, Gopalan, and Thakor, 2006 and 2008), optimal capital structure (Boot and Thakor, 2011), financial intermediation (Coval and Thakor, 2005), the firm's choice of debt versus equity financing (Dittmar and Thakor, 2007), security design (Garmaise, 2001), share repurchase (Huang and Thakor, 2013), trade around public announcement (Kandel and Pearson, 1995), the co-evolution of banks and market in financial system (Song and Thakor, 2010), financial innovation and crises (Thakor, 2012), corporate investment (Thakor and Whited, 2011), "endogenous optimism" (Van den Steen, 2004), the allocation of control (Van den Steen, 2010a), and the theory of firms (Van den Steen, 2010b).

The paper proceeds as follows. Section 2 develops testable hypotheses. Discussions of the data and sample are in Section 3. In Section 4, we conduct the main analysis of investor-management disagreement and forced CEO turnover, and test the predictions of the hypotheses. Robustness checks and discussions are in Section 5. This section focuses on four issues: the reaction of the stock market to disagreement-induced turnover, the robustness of our finding to alternative measures of firm and industry performance, endogeneity concerns, and the extent to which our disagreement proxies may be measuring things other than disagreement. Section 6 concludes.

3.2 Hypothesis Development

While the prior literature on CEO turnover focuses primarily on performance-induced CEO dismissals and the impact of corporate governance on turnover-performance sensitivity, anecdotal evidence suggests that forced CEO departures are often not merely due to poor firm performance. Rather, they sometimes result from disagreement between management and shareholders on the optimal course of corporate decisions. Examples abound. For instance, Associated Press Newswires reported on November 9, 2000, that Lloyd Ward resigned as Maytag Corp's Chairman and CEO over "a difference (of opinion) on the company's strategic outlook and direction". Similarly, Curtis Huff was ousted as CEO from Grant Prideco over frictions during the implementation of its predetermined acquisition strategy, although analysts credited Huff with "leaving the company in good shape".[65] There are numerous other reports of CEOs being forced out due to difference of opinion over corporate strategy, direction, and implementation.[66]

As suggested in these anecdotes, investors and managers can have divergent opinions about the optimal course of actions based on the same evidence. That is, disagreement between investors and management arises not because they have different information sets, but because they interpret information in different ways. Divergent interpretations can arise from heterogeneous prior beliefs – that are all rational in the sense of Kurz (1994) – about the profitability of a firm's future investment opportunities or the strategy of how best to implement its investment decisions.

Disagreement is costly to firms because it lowers firm valuation and makes external financing more expensive (Dittmar and Thakor, 2007), and this cost is higher for firms that are financially more constrained and hence more dependent on external equity financing. This creates

[65] See "Grant Prideco Shake-up Has BJ's McShane in Charge" by *Platts Oilgram News* on June 25, 2002.
[66] For examples, see the resignations of CEO Richard White from Veritas DGC, of CEO Bruce Albertson from Iomega, of CEO Warren Musser from Wayne, and of CEO Edwin Russell from Allete Inc., among many others.

a rationale for such a firm to replace a CEO who has a relatively low agreement with investors with one who investors agree more with, everything else being equal.

However, CEO entrenchment and weakness in shareholder governance can make the board reluctant to fire a CEO who may have hand-picked most of the board members or one who is deemed to be "powerful" due to tenure in office or other considerations. The prior literature suggests that involuntary CEO turnover is less likely and also more costly if the CEO is more entrenched and governance is weaker (e.g., Weisbach, 1988; Borokhovich, Parrino, and Trapani, 1996; Denis, Denis, and Sarin, 1997; Hermalin and Weisbach, 1998; Huson, Parrino, and Starks, 2001; Taylor, 2011). Therefore, firms balance the disagreement-decline benefit of CEO dismissal against the cost/difficulty of firing an entrenched CEO. This leads to our first main hypothesis as follows.

Hypothesis 1: Ceteris paribus, forced CEO turnover is more likely in firms with higher investor-management disagreement.

It should be noted that our analysis does not yield a similar prediction for *voluntary* CEO turnover. Such turnover is unaffected by investor-management disagreement because the CEO believes that her decisions are value-maximizing.

It is plausible to postulate that those within the executive suite of the firm will share similar views and beliefs due to constant interactions and being part of the same corporate culture (e.g., Kreps, 1990b; Van den Steen, 2010c; Bouwman, 2013), making them more likely to agree with each other than with investors. An immediate implication of this is that investors, who had a higher level of agreement with a departing CEO, are more likely to endorse an insider to succeed the departing CEO, since this higher agreement level is likely to persist with the successor. When agreement with the departing CEO is relatively low, investors prefer an outsider to be the successor. This is consistent with the evidence of management turnover, shown by Fee and Hadlock (2004),

that senior executive managers are evaluated as a group. Although it might be more costly to search for an external CEO than to select one from an internal talent pool (due to search frictions on the labor market), the benefit of having an external CEO with a higher level of agreement with investors may outweigh the search costs. We therefore have our second testable prediction below.

Hypothesis 2: Firms are more likely to select an external replacement CEO if investor-management disagreement with the existing CEO is higher.

It follows that investor-management disagreement is likely to decline when a new external CEO successor is selected subsequent to a CEO being forced out. Even if an internal CEO is selected to replace the fired CEO in some of the cases (possibly due to a high external search cost or the importance of firm-specific knowledge), we expect firms to select an internal successor with a higher level of agreement with investors than that enjoyed by the departing CEO, everything else being equal. This means that investor-management disagreement is expected to decline following a forced CEO turnover, leading to our third testable prediction.

Hypothesis 3: Investor-management disagreement declines following forced CEO turnover.

Also, our analysis above indicates that the decline in disagreement will be greater if the replacement CEO is selected externally.

3.3 Data and Variables

3.3.1 Data and Sample

Our sample construction starts with all U.S. firms in *ExecuComp* from 1993 to 2010 that list their common stock in NYSE, NASDAQ, or AMEX. We exclude all financial (primary SIC codes 6000 – 6999) and utility (primary SIC codes 4900 – 4999) firms. We include data on CEO characteristics (age, tenure, chairmanship, and stock ownership), firm-level accounting variables

(e.g., assets, leverage, book value of equity, and net income), stock price, institutional ownership, and proxies for investor-management disagreement.

Turnover data: We identify CEO turnover from *ExecuComp* and use news reports, *Boardex*, and other public sources to classify the turnover as voluntary or involuntary.

Disagreement proxies: We construct proxies for disagreement using analysts' earnings forecast data from *I/B/E/S*, and using data on shareholder proxy proposals (1996–2010), shareholder voting (2003–2010), and Institutional Shareholder Services (ISS) vote recommendations in director elections (2003–2010) from Voting Anlytics.[67] We follow Del Guercio, Seery, and Woidtke (2008) and search news reports to collect data on shareholders' "just vote no" campaign from 2003 to 2010.

CEO attributes: We obtain data on CEO age, tenure, chairmanship, and stock ownership from *ExecuComp* and whenever needed, supplement it with data from *Boardex*.

Firm attributes: We obtain firm-level accounting data from *COMPUSTAT*, stock price and return data from *CRSP*, institutional ownership data from *CDA/Spectrum*, and board and director characteristics data from *RiskMetrics* and *Boardex*.

As we discuss below, our final sample size varies with different disagreement proxies due to different degrees of data availability.[68]

[67] We thank Stuart Gillan for sharing the shareholder proxy proposal data before 1996.
[68] The resulted samples of CEO turnover corresponding to different disagreement proxies are smaller than the universe of CEO turnover for firms in ExecuComp during the sample period. However, as we discuss later, the rate of CEO turnover and the rate of forced versus voluntary turnover in our samples are consistent with those reported in the prior literature.

3.3.2 Key Variable Construction

CEO turnover

As discussed earlier, our disagreement hypothesis predicts forced, but not voluntary, CEO turnover. In this section, we describe the classification of CEO turnover as voluntary or involuntary. We start with identifying turnover from changes in CEO designation as documented in *ExecuComp*. We then search using *Factiva* and *LexisNexis* for news reports coincident with the change in designation to identify the causes for the change. We drop instances that are due to misclassification in *ExecuComp*, takeovers or spinoffs, sudden death, or departures from interim positions. To classify a turnover as voluntary or involuntary, we start with using a similar algorithm as in Parrino (1997). Any turnover for which the press reports that the CEO is fired, is forced out, or resigns due to poor performance, difference of opinion, or unspecified policy differences is classified as forced. Of the remaining instances of turnover, if the departing CEO is under age 60, it is classified as forced if either (1) the reported reason for the departure does not involve death, poor health, or acceptance of another position elsewhere or within the firm, or (2) the CEO is reported to be retiring but there is no announcement about the retirement made at least two months prior to the departure.

We then complement the above algorithm with a modification as in Huson, Parrino, and Starks (2001) and also in more recent studies (e.g., Taylor, 2010; Hazarika, Karpoff, and Nahata, 2012; Jenter and Kanaan, forthcoming). We reclassify a forced turnover (identified using the steps described above) as voluntary if either (1) the CEO's employment record, obtained from Boardex, Marquis Who's Who publications, and other press reports, suggests that the CEO obtained a comparable position elsewhere upon or immediately following the turnover announcement, or (2) the press reports convincingly explain that the departure is due to previously undisclosed personal or business reasons that are unrelated to the firm's activities. All instances of CEO turnover not

classified as forced are classified as voluntary, some of which are due to mandatory or planned retirements.[69]

We classify a new CEO as being external to the firm if she has been with the firm for no more than one year before the succession. We do this by relying on *ExecuComp* and *Boardex* for information on a manager's career path, supplemented by *Marquis Who's Who publications*, *Bloomberg Businessweek*, and *Standard & Poor's register of corporations, directors, and executives.*

Investor-management disagreement

Following the literature (e.g., Dittmar and Thakor, 2007; Huang and Thakor, 2013), we use four proxies for investor-management disagreement: (1) The difference between the analyst forecast consensus of a firm's earnings per share (EPS) and its actual value ("*Forecast-Actual EPS*"), (2) the number of proxy proposals that a firm receives in a year ("*Proxy proposal*"), (3) the vote recommendation in directors' elections ("*Vote recommendation*"), and (4) actual voting that director candidates receive in directors' elections ("*Actual voting*"). Details on these variables along with a discussion of the economic rationale for viewing each variable as a proxy for investor-management disagreement are provided in the Appendix.

3.3.3 Summary Statistics

Table 3.1 presents the yearly distribution of the number and frequency of CEO turnover between 1993 and 2009.[70] Due to its most complete coverage of sample firms, we take the sample

[69] Kaplan and Minton (2012) suggest that the usual approach of CEO turnover classification tends to misclassify some forced turnovers as voluntary. We note that such a misclassification, if present, results in a smaller sample of forced CEO turnover and thus causes a downward bias in the estimated effect of disagreement on forced turnover. That is, the documented impact of investor-management disagreement on forced CEO turnover may be an underestimate of the actual impact.

[70] Data on CEO turnover end in 2009 instead of 2010 because our analysis requires one more year of data on disagreement proxies in examining the change in disagreement following forced CEO turnover.

corresponding to the measure of disagreement using the difference between the analyst forecast of a firm's EPS and its actual value in presenting the distribution. Overall, there are 1691 CEO successions that occur in about 10% of the sample firm-years. Among them, 345 (about 20% of all successions) are forced, and in 520 (about 29%) of all successions, the new CEOs are hired from outside the firm. There exist some extent of time-series variations in the number and frequency of overall, forced, and external successions. We include year dummies in all of our regressions to control for possible time effects.

[TABLE 3.1 GOES HERE]

Table 3.2 reports summary statistics of the key variables we use in our analysis. All variables are winsorized at the 1% and 99% levels to mitigate the potential impact of outliers. Detailed definitions of these variables (except CEO turnover that is discussed earlier) are provided in the Appendix. The upper part of Panel A provides summary data on disagreement proxies and on forced CEO turnover in each of the four samples with different disagreement proxies. Similar to the finding in the prior literature, the unconditional probability of forced CEO turnover in a year is between 1.9% and 2.2% across the four samples. The sample firm's mean (median) *Forecast-Actual EPS* is 0.04 (-0.01). Among firms that have received at least one shareholder proxy proposal during the sample years 1993-2010, an average of 0.55 proposals are submitted in a year. On average, 10% of candidates in a firm-year receive a "withhold" or "against" vote recommendation before the director election. Also, 23% of director candidates in an average sample firm-year receive a percentage of yes-votes in the election below the yearly median.[71]

[71] As discussed in the construction of the *Actual voting* measure in Appendix, the yearly median percentage of yes-votes is defined based on the universe of firms with available actual voting data during 2003-2010, but not on our final sample firms. The smaller fraction (23%) of directors in our sample firms receiving below-yearly-median percentage of yes-votes than 50% (by construction) suggests a higher average percentage of yes-votes received by director candidates in our sample firms (i.e., relatively large firms in ExeuComp) than in firms in the universe.

In the lower part of Panel A, we conduct a univariate test of the relation between forced CEO turnover and disagreement. We classify the CEO years into two groups – those involving forced turnover and those not involving forced turnover, and compare the disagreement parameters in the two groups as of the year prior to turnover. We find a higher level of disagreement in the forced-turnover group, and the *t*-test conducted on the difference of the mean disagreement parameters shows that the difference is significant at 1% level for all four disagreement proxies. This is consistent with *Hypothesis 1*. For example, on average, 14% of candidates receive an unfavorable voting recommendation before the director election in the year prior to forced CEO turnover, while that number is 9% only during other years. Also, 40% of candidates in our sample receive a below-yearly-median percentage of yes-votes in the director election in the year prior to forced CEO turnover, a number significantly more than 23% – the counterpart statistic during other years.

In Panels B and C, we present summary statistics of firm and CEO characteristics, respectively. As in Table 3.1, we take the sample corresponding to the measure of disagreement using the difference between the analyst forecast of a firm's EPS and its actual value due to its most complete coverage of sample firms. On average, 11% of CEOs have over 5% of stock ownership in the firm and 62% of CEOs are also Chairmen of the board. The average tenure of the CEOs is about 8.6 years. Since we obtain sample firms from *ExecuComp* (which covers S&P 1500 firms), the firm characteristics of our sample are similar with those in the prior literature on CEO turnover since those papers also use *ExecuComp* as the major data source.

[TABLE 3.2 GOES HERE]

3.4 Main Empirical Analysis of Disagreement and Turnover

In this section, we discuss the empirical tests of our three hypotheses, and examine the cross-sectional variations in the turnover-sensitivity of disagreement.

3.4.1 Test of Hypothesis 1: High investor-management disagreement leads to a higher likelihood of forced CEO turnover

Baseline analysis

We test *Hypothesis 1* by relating investor-management disagreement to the likelihood of forced CEO turnover while controlling for a number of firm and CEO characteristics that the prior literature has shown to affect CEO turnover. We follow previous studies (e.g., Hazarika, Karpoff, and Nahata, 2012; Jenter and Kanaan, forthcoming) and employ the Cox proportional hazard model (Cox, 1972) to conduct our test:

$$\lambda(t|X) = \lambda_0(\beta_0 I * t)\exp(\beta' X).$$

The hazard model presents a CEO's hazard rate (the dependent variable) – approximately, the likelihood that the incumbent CEO will be dismissed in the next year – as a function of the CEO's tenure and other CEO as well as firm characteristics. It thus takes into account both the occurrence and timing of forced turnover. The model also accounts for the right-censoring of the data that arises from the fact that some CEOs in our sample remain in office by the end of 2009. We allow baseline hazards to vary across industries to capture the difference in turnover patterns in different industries.

Our key independent variable is investor-management disagreement, proxied by the four disagreement measures, lagged by one year relative to the dependent variable because it is the disagreement parameter in place at the end of the previous year that drives the turnover decision

this year. A positive coefficient on the disagreement measures implies a positive marginal impact on the hazard and thus a shorter expected time as CEO. The firm characteristics we include in the regressions, also lagged by one year, are *Firm size*, *Market-to-Book*, *Stock return*, *EW Industry stock return*, *Leverage*, *Stock volatility* and *Institutional blockholding.*[72] When *Forecast-Actual EPS* is used as the disagreement measure, we also include *Analyst dispersion* to control for difference of opinions among analysts. We include *Total directors* to control for the number of director candidates up for elections when the last two disagreement measures regarding director election are used. The set of CEO characteristics we include are *Age*, *Age square*, *CEO blockholding*, and *CEO-Chair Duality*. In all regressions, we also include year fixed effects, and the standard errors we estimate are robust to heteroskedasticity and clustered at the firm level.

The results are reported in Table 3.3. Consistent with the prediction of *Hypothesis 1*, we find that the coefficients of all four disagreement measures are positive and statistically significant.[73] This indicates that the likelihood of forced CEO turnover increases when investors are more likely to disagree with management. From the coefficient estimates of the control variables, we find that the likelihood of forced CEO turnover is higher in poorly-performing firms and in firms with greater stock volatility. Also, CEOs who are also chairmen of the board of directors are less likely to be forced out.

[TABLE 3.3 GOES HERE]

We repeat our estimates using a logit model.[74] Employing the logit model helps us interpret the economic significance of our results in a more intuitive manner. In the interest of brevity, we

[72] Following Kaplan and Minton (2012) and Jenter and Kanaan (forthcoming), we control for firms' stock returns (*Stock return*), industry returns (*EW Industry stock return*), as well as market returns through yearly fixed effect.
[73] Farrell and Whidbee (2003) find a similar association between the likelihood of CEO turnover and industry-adjusted analyst EPS forecast errors for an earlier sample from 1986 to 1997.
[74] We include CEO tenure (*Ln(Tenure)*) in the logit regressions as an additional control to account for the impact of tenure on the likelihood of CEO dismissal. Unlike the Cox proportional hazard model, the logit model by itself does not take into account the effect of CEO tenure.

do not tabulate the results. The results are consistent with those obtained using the Cox hazard model that CEOs are more likely to be forced out when the level of disagreement is higher. The impact of disagreement is also economically significant. Specifically, in accordance with the odds ratios obtained from the logistic regressions, the odds of forced turnover are 0.42 times higher after a one-standard-deviation (0.27) increase in *Forecast-Actual EPS*, and 1.27 times higher after a one-standard-deviation (1.09) increase in the number of proxy proposals received.[75] Also, there is an increase in the odds of forced turnover by 0.53/1.44 times following a one-standard-deviation (0.22/0.34) increase in the proportion of director candidates receiving an unfavorable vote recommendation/receiving less than the yearly-median percentage of yes-votes, among all candidates in the firm who are up for election in a given year.[76]

Cross-sectional analysis: Is the effect stronger in more-financially-constrained firms and weaker in firms with more-entrenched CEOs?

Although we find a significant relation between disagreement and forced turnover on average, we expect some heterogeneity in the strength of the correlation cross-sectionally. Because the cost of disagreement and constraints in forced CEO turnover can differ across firms, firms will vary in the disagreement sensitivity of forced turnover.

First, for firms that are financially more constrained, the cost of disagreement is higher because equity is a more important source of financing to them, and yet investors may either decline to fund investments or may only be willing to provide financing at a higher cost to the firm. *Ceteris paribus*, these firms may thus be more responsive to investor disagreement in forcing out CEOs to pursue successors with higher levels of agreement with investors. To test this prediction, we run the baseline regressions in Table 3.3 separately on two subsamples of firms – one group consisting

[75] The odds ratios are 1.549 and 1.167, respectively.
[76] The odds ratios are 2.409 and 3.350, respectively.

of firms that are financially more constrained and another group consisting of firms that are less constrained. Specifically, we classify firms as being financially more (less) constrained if their Whited and Wu (2006) index is in the top (bottom) tercile of the sample.

The results, reported in Panel A of Table 3.4, are consistent with our prediction. The coefficient estimates of the disagreement proxies have predicted signs for both subsamples but are statistically significant only for the more constrained subsample (except the *Actual voting* proxy, the coefficients of which are both significant in the two subsamples). We include all other explanatory variables in Table 3.3 in the regressions here. Their coefficient estimates are similar to those in Table 3.3, and thus we do not report them in the interest of brevity. In untabulated findings for robustness, we repeat our analysis using a direct measure of equity dependence which is constructed following Rajan and Zingales (1998), and find qualitatively-similar results.

Second, the cost of disagreement notwithstanding, firms may be constrained in CEO-firing decisions, especially when their CEOs are entrenched. Taylor (2010) uses a structural model to argue that the low forced turnover rate at large US firms may be due to switching costs that firms face in CEO succession, mainly reflecting CEO entrenchment. Combining this insight with our framework, we obtain the prediction that the turnover-disagreement sensitivity is weaker in firms with more-entrenched CEOs. Moreover, we expect effective corporate governance to at least partially overcome the effect of entrenchment. This yields the prediction that the turnover-disagreement sensitivity is higher in firms with stronger corporate governance.

To measure the extent of which a firm's CEO is entrenched, we construct an index of CEO entrenchment based on the following observations. There is greater entrenchment when: (i) The CEO is also the chairman of the board; (ii) the fraction of outsiders on the board (board

independence) is below the sample average;[77] and (iii) the stock ownership by executive directors is greater than the sample average. To the extent that executive directors are more likely to be aligned with the CEO and their higher stock ownership gives them greater voice on the board, it is intuitive that higher ownership by executive directors is associated with greater CEO entrenchment.[78] The entrenchment index takes a value of zero to three, depending on the number of the three observations that are true. Therefore, a firm's CEO is regarded as least entrenched when the index equals zero and most entrenched when the index equals three. We divide our sample into two groups based on the entrenchment index. Firms with the index being two or three are grouped and labeled as "Entrenched", and other firms are grouped and labeled as "Less entrenched". In testing our prediction, we run the baseline regressions in Table 3 on the two groups of firms, respectively.

We measure the strength of shareholder governance through their ownership concentration. The free-rider problem arising from ownership dispersion (Grossman and Hart, 1980) has long been argued as one of the major factors contributing to the weakness of shareholder monitoring in corporate governance. For any active shareholder in a firm with dispersed ownership, the cost of shareholder intervention, typically borne by the initiating shareholder, often outweighs the benefit of the intervention (if any) that is shared with all other shareholders. More concentrated ownership can mitigate this free-ride problem and incentivize shareholders with large ownership to use "voice" (intervene) when necessary. Moreover, unlike smaller shareholders, shareholders with large and concentrated ownership are less likely to exit by selling shares when they disagree with management, because of the potentially large price impact of their selling. On the other hand, the potentially large price impact of selling allows large shareholders to use exit as a potent threat to

[77] For the impact of outside directors on CEO succession, see Weisbach (1988) and Borokhovich, Parrino, and Trapani (1996) for examples.
[78] Denis, Denis, and Sarin (1997) find that top executive turnover is less likely when the ownership of officers and directors in the firm is higher.

improve governance in firms.[79] Therefore, shareholder governance, through both "voice" and the threat of "exit", is arguably more effective with more concentrated ownership.

Specifically, we classify firms as "Concentrated" if their largest five institutional investors hold more than 20% of the shares outstanding in aggregate.[80] All other firms are classified as "Less concentrated". Hartzell and Starks (2003) show that the share of institutional ownership by the five largest holders is positively related to executives' pay-for-performance sensitivity and negatively related to the level of compensation, and thus suggest an effective monitoring role played by the top five institutional investors. To test the impact of shareholder governance, we regress forced CEO turnover on our disagreement proxies for the two groups of firms, respectively.

In Panels B and C of Table 3.4, we present the results that are consistent with our predictions on the impact of CEO entrenchment and share ownership concentration. Although the effect of disagreement on forced CEO turnover is mostly consistent across the subsamples of "Entrenched" and "Less entrenched" and the subsamples of "Concentrated" and "Less concentrated", it is only statistically significant in the subsample of "Less entrenched" and the subsample of "Concentrated". The exceptions are that the coefficient estimates of *Actual voting* and *Proxy proposal* are also significantly positive in the subsamples of "Entrenched" and "Less concentrated", respectively. But the level of significance is marginal in both cases. As in Panel A, all other explanatory variables are included in the regressions but are not tabulated.

[TABLE 3.4 GOES HERE]

In sum, we find that the likelihood of forced CEO turnover is positively related to the level of investor-management disagreement, and the effect of disagreement is more pronounced in firms

[79] See Edmans (forthcoming) for a summary of the literature on both "voice" and "exit" by blockholders.
[80] Our finding remains qualitatively the same if we use a different share ownership cut point, e.g., 15% or 25%. Also, it is robust if we use the shareholding by top five active institutional investors that are defined as quasi-indexers and dedicated institutions based on Bushee (2001).

that are more constrained financially, have less entrenched CEOs, and have stronger shareholder governance.

3.4.2 Test of Hypothesis 2: CEOs who investors disagree with more are more likely to be replaced with external hires.

We test Hypothesis 2 by examining the effect of disagreement on a firm's choice of an external CEO, conditional on CEO succession. To do this, we apply a logit estimation model where the dependent variable is an indicator that identifies if a new CEO has been with the firm for less than a year prior to the CEO appointment. The main independent variable is disagreement. Those firm-level variables that are used to estimate the likelihood of forced turnover in Table 3 are also included as controls here in addition to the yearly and industry dummies. Robust standard errors are clustered at the firm level in all regressions.

The results presented in Table 3.5 strongly support *Hypothesis 2*. The reported coefficients of the marginal effect are positive and statistically significant for all the disagreement proxies. It suggests that an external replacement CEO is more likely to be selected when the level of disagreement between investors and incumbent management is higher. The effect of disagreement on external CEO hiring is also economically meaningful. Specifically, the odds ratios obtained from the logistic regressions suggest that the odds of an external CEO hire are 0.49 times higher following a one-standard-deviation (0.27) increase in a firm's *Forecast-Actual EPS*, and 1.34 times higher following a one-standard-deviation (1.09) increase in the number of proxy proposals received.[81] Also, there is an increase in the odds of an external CEO hire by 1.01/0.95 times following a one-stand-deviation (0.22/0.34) increase in the proportion of director candidates

[81] The odds ratios are 1.81 and 1.23, respectively.

receiving an unfavorable vote recommendation/receiving less than the yearly-median percentage of yes-votes, among all candidates in the firm who are up for election in a given year.[82]

[TABLE 3.5 GOES HERE]

3.4.3 Test of Hypothesis 3: Investor-management disagreement declines following forced CEO turnover.

We test this hypothesis by examining how investor-management disagreement changes following forced CEO turnover based on the following specification:

$$\textit{Change in Disagreement}_{i,t\text{-}1\ to\ t+1} = \alpha + \beta_1 * \textit{Forced turnover}_{i,t} + \beta_2 * \textit{Controls} + \mu_{industry} + \eta_t + \varepsilon_{i,t},$$

where *Forced turnover* $_{i,t}$ is an indicator variable that equals one if the firm experiences forced CEO turnover in year *t* and zero otherwise. We also include other explanatory variables as controls for public information about the firm as of year *t*, such as *Firm size*, *Market-to-Book*, *Stock return*, *Stock volatility*, and accounting performance *ROA*. Year and industry fixed effects are also included to all regressions. The dependent variable, *Change in Disagreement* $_{i,t\text{-}1\ to\ t+1}$, measures the change in disagreement from the year prior (year $t - 1$) to the year subsequent (year $t + 1$) to the turnover. We explain below in more details on how we construct this dependent variable, for each of our disagreement proxies.

Consider *Proxy proposal* first. We note that, conditional on the occurrence of a proxy proposal submission, the average firm receives two proposals in a year. Therefore, a drop of two in the number of proposals received in the average firm implies an aligned view between investors and the new management in the year subsequent to CEO turnover. We thus define the change-in-disagreement variable as a dummy, which equals one if the number of proxy proposals received in

[82] The odds ratios are 4.57 and 2.80, respectively.

year $t + 1$ drops by at least two relative to the number of proxy proposals received in year $t - 1$, representing a decline in disagreement, and is zero otherwise. Similarly, in the case of *Actual voting*, the change-in-disagreement variable is also defined as an indicator variable that equals one if the fraction of directors receiving less than the yearly-median percentage of yes-votes among all candidates up for election in year *t+1* is less than that in year *t-1*, and is zero otherwise, i.e., a value of one means a drop in disagreement. For the other two disagreement proxies—*Forecast-Actual EPS* and *Vote recommendation*, the change in disagreement is the simple difference of the continuous measure from year $t - 1$ to year $t + 1$.

When the change-in-disagreement is defined as a continuous variable, as is the case for *Forecast-Actual EPS* and *Vote recommendation*, we employ an OLS regression model in estimating the effect of forced turnover, and we expect a significantly negative impact in both cases. For the other two indicator change-in-disagreement variables, we apply a logistic model in estimating the effect of forced turnover (and coefficients of the marginal effect are reported), and we expect a significantly positive impact in both cases. The yearly and industry dummies are included and robust standard errors are clustered at the firm level in all regressions. The results presented in Table 3.6 are consistent with the prediction of Hypothesis 3. The coefficients of *Forced turnover* have the expected signs for all four change-in-disagreement proxies and are statistically significant. It suggests that disagreement declines following forced CEO turnover.

[TABLE 3.6 GOES HERE]

In results that are untabulated for brevity, we also find that the decline in disagreement is greater following forced CEO turnover if a replacement CEO is hired externally. This is consistent with *Hypothesis 2* that firms tend to employ an external successor when the level of disagreement is high, because an internal successor is more likely to share similar views and beliefs with the departing CEO than an external successor.

3.5 Discussions and Robustness Tests

In this section, we discuss four main issues: (i) how does the stock market react to disagreement-induced CEO turnover? (ii) the robustness of our findings to using alternative measures of firm and industry performance, (iii) endogeneity concerns, and (iv) the extent to which our disagreement proxies may be measuring things other than disagreement.

3.5.1 Market Response to Announcements of Forced CEO Turnover

The disagreement hypothesis suggests that, when the board fires a CEO who had a higher level of disagreement with investors, the market should react more positively in anticipation of the post-dismissal decline in disagreement with the next CEO. And it should be more so if the next CEO is hired externally because the decline in disagreement is expected to be greater, as discussed above.

To confront this reasoning with the data, we examine the five-day (-2, +2) cumulative abnormal returns (CARs) around the forced turnover announcements. We estimate CARs using the market model and the CRSP equal-weighted stock return as the market return. Specifically, we take the sample of forced CEO turnover that corresponds to the measure of disagreement using the difference between the analyst forecast of a firm's EPS and its actual value due to its most complete coverage of sample firms.[83] We divide the sample into two subsamples – a "high disagreement" subsample in which a firm's EPS falls below its analyst forecast consensus and a "low disagreement" subsample in which a firm's EPS equals or beats its forecast in the year prior to turnover. We then compare the CARs between the two subsamples to contrast the market's

[83] Our findings are similar for samples of forced turnover corresponding to other disagreement proxies.

response to forced CEO turnover due to disagreement with its response to other types of forced turnover.

Our finding confirms the prediction of our disagreement hypothesis. The average CARs are 1% and marginally significant for the "high disagreement" subsample, while the average CARs are -2% and significant for the "low disagreement" subsample. And the difference is statistically significant at 1% level.[84] Moreover, the average CARs for firms with an external replacement for the departing CEO in the "high disagreement" subsample are higher at 2.54% and statistically significant. In comparison, the average CARs for their counterparts in the "low disagreement" subsample are -1.2% and statistically insignificant. The difference is also statistically significant.

3.5.2 Alternative Measures of Firm and Industry Performance

CEOs are often fired for poor performance, and it is possible that this can happen even when investor-management disagreement is low. To deal with this potential commingling of the influences of firm performance and disagreement on forced CEO turnover, we have controlled for the firm's stock performance and also the performance of its industry peers. The literature, however, suggests that firms may use measures of firm performance other than stock returns in their decisions of CEO firing. For instance, Engle, Hayes, and Wang (2003) find interesting cross-sectional variation in the weights placed on accounting-based and market-based firm performance measures and relate it to the properties of these performance measures. Moreover, CEOs can be fired for poor performance that is beyond their control (Jenter and Kanaan, forthcoming).

We test the robustness of our main finding to alternative performance measures by proceeding as follows. First, we replace stock performance with a measure of operating performance, return on assets (ROA). Denis and Denis (1995) find in an early sample of top

[84] In untabulated regression results, we find that the difference in CARs is significant even after controlling for various firm and CEO characteristics.

management turnover that forced CEO turnover is preceded by a significant decline in operating performance. Second, we follow Jenter and Kanaan (forthcoming) and decompose firm stock performance into a systematic component caused by industry peers' performance (that is out of the CEO's control) and a firm-specific component that should reflect the CEO's ability.[85] We then include both components of performance in the regression of forced turnover. Table 3.7 presents the results with these alternative measures of firm performance. We find that the effect of disagreement on the likelihood of forced CEO turnover is robust to controls involving these performance measures. The coefficient estimates of the four disagreement proxies remain almost intact in both statistical significance and economic magnitude, compared with the results in Table 3.3. This confirms that our disagreement hypothesis has incremental power relative to firm performance in explaining forced CEO turnover.

[TABLE 3.7 GOES HERE]

3.5.3 Endogeneity of Disagreement and Turnover

One might be concerned that both disagreement and forced turnover can be related to an unobserved omitted variable, and therefore the relation between them might be spurious. One such variable is the uncertainty that a firm faces in its growth opportunities or its technological development. For instance, such uncertainty is prevalent in high-tech industries with abundant investment opportunities. Uncertainty increases the likelihood that agents will arrive at different interpretations of the same information set, and thus may contribute to disagreement. Meanwhile, we also observe more frequent management turnover in an uncertain growth/technological environment.

[85] See more details on the methodology of the decomposition in Jenter and Kanaan (forthcoming).

If the omitted variable is time-invariant within the firm, we can tackle the issue by running a firm fixed effects estimation of disagreement on forced CEO turnover, using a linear probability model.[86] The firm fixed effects estimation eliminates the impact of any unobserved firm-specific factors in exploiting the within-firm variations of the variables over time. Results of this estimation method confirm the robustness of our main finding. Of course, we are aware of the linearity limitation involved in this linear-probability estimation. Therefore, we take it as an ancillary approach and discuss the results, but do not tabulate them in the interest of brevity.

If the omitted variable is time varying, then a firm fixed effects estimation will not be effective in addressing the omitted variable bias concern. We deal with this possibility in three different ways: (i) by running a falsification test, (ii) by examining the impact of an exogenous shock to agreement, and (iii) by exploiting a quasi-natural experiment. Each of these tests is discussed below.

A falsification test

Under our disagreement explanation, a CEO always believes she is maximizing firm value, so she has no reason to depart voluntarily when disagreement is high. Therefore, disagreement may not affect the likelihood of voluntary CEO turnover in a systematic way. In contrast, under the uncertainty (the omitted variable discussed above) view, if the difficulty in coping with uncertainty increases the likelihood of forced management turnover, we expect to see a similar effect of uncertainty on voluntary turnover. This is because managers are more likely to jump ship to other firms for better perceived opportunities in industries with greater uncertainty, as highlighted by the recent controversy about information technology firms colluding in their hiring practices to limit poaching talent from each other.[87] That might explain the prevalence of talent retention measures

[86] We are unable to include firm fixed effects in the non-linear Cox hazard model and logit model because of the incidental parameters problem (Neyman and Scott, 1948).
[87] See Wall Street Journal articles titled "Ebay settles recruiting allegations" dated May 1, 2014 and "Tech companies agree to settle wage suit" dated April 24, 2014.

in those firms such as non-compete agreements (Garmaise, 2011) and long-duration pay (Gopalan, Huang, and Maharjan, 2014). Thus, the contrasting prediction regarding voluntary CEO turnover under the uncertainty view provides an opportunity to conduct a falsification test of our disagreement hypothesis.

In the falsification test, we repeat the baseline analysis about the effect of disagreement in Table 3.3 with a replacement of the dependent variable by the hazard rate of voluntary CEO turnover. In doing this, we focus on incidents of voluntary turnover that are not due to mandatory or planned retirements, although our results are not sensitive to this exclusion. The results, presented in Table 3.8, do not support the uncertainty view. Unlike the case of forced turnover, the estimated coefficients are negative for all the disagreement proxies, and none of them are statistically significant, except *Vote recommendation*. It shows that disagreement is not relevant to voluntary CEO turnover, consistent with our disagreement hypothesis.

[TABLE 3.8 GOES HERE]

Impact of an exogenous shock to investor-management agreement

Next, to disentangle the impact of disagreement from that of the unobserved omitted variable on forced turnover, we identify an exogenous shock to agreement (through an exogenous change in the firm's investor base) that is not related to the omitted variable or other firm characteristics and then examine how it may affect forced CEO turnover. Flow-induced mutual fund fire sales (Coval and Stafford, 2007) constitute an ideal setting for this purpose. Distressed funds that have experienced extreme capital outflows are forced to sell their holdings with significant discounts. Existing investors who are not distressed are unlikely to absorb, within a short time period, all these shares due to risk aversion, wealth endowment constraints, or both. It follows that, in equilibrium, the new *marginal* investors in the stocks under fire sales are other liquidity providers who have a lower level of agreement than the existing shareholders. The change

in the investor base results in a decline in the level of agreement between investors and management. This decline in agreement, arising from distressed funds' liquidity demand, is exogenous to changes in firm fundamentals for the affected stocks. We expect that such a negative shock to agreement would lead to an increased probability of forced CEO turnover according to our disagreement hypothesis.[88]

We follow Coval and Stafford (2007) and Khan, Kogan, and Serafeim (2012) to construct fund-flow-induced trading pressure for each stock held by mutual funds during our sample period.[89] Specifically, we define fund flows as

$$FLOW_{j,s} = \left[TNA_{j,s} - TNA_{j,s-1} \cdot \left(1 + R_{j,s}\right)\right] / TNA_{j,s-1}$$

to fund *j* during month *s*, where $TNA_{j,s}$ is total net assets for fund *j* as of the end of the month *s* and $R_{j,s}$ is the monthly return for fund *j* at the month *s*. The data of funds' total net assets and returns are from CRSP mutual fund monthly net returns database. To match with the quarterly fund holding data from Thomson Financial, we sum the monthly flows over the quarter to obtain quarterly fund flows $FLOW_{j,t}$ for quarter *t*. We calculate flow-induced trading pressure for stock *i* in quarter *t* as

$$Pressure_{i,t} = \left[\sum_j \left(max\left(0,\ \Delta Holdings_{j,i,t}\right) | flow_{j,t} > Percentile(90th)\right) - \right.$$

$$\left. \sum_j \left(max\left(0,\ -\Delta Holdings_{j,i,t}\right) | flow_{j,t} < Percentile(10th)\right)\right] / SharesOutstnding_{i,t-1}.$$

As in Coval and Stafford (2007), stocks in the bottom decile of *Pressure* are considered to be experiencing excess selling demand from mutual funds with large capital outflows.

[88] Although it is possible that agreement may improve if new investors, who have a more aligned view with management, start buying the stock later, Coval and Stafford (2007) find that this does not seem to occur in a short time. Moreover, the impact of such equity funds' fire sales on affected firms appears to be substantial, as suggested by several recent studies (e.g., Hau and Lai, 2013; Lou and Wang, 2014) that have documented a significant decline in investment and employment in these firms following the fire sales. Such changes in affected firms can be plausibly explained by the decline in investor-management agreement resulted from the equity fire sales (see Thakor and Whited (2011)).

[89] As in the previous studies, we focus on open-end U.S. equity funds only.

To ensure that the flow-induced selling is not driven by information about potential changes in firm characteristics, we first calculate unforced trading pressure for stock *i* in quarter *t* following Khan, Kogan, and Serafeim (2012) as

$$UPressure_{i,t} = \left[\sum_j \Delta Holdings_{j,i,t} | Percentile(10th) \leq flow_{j,t} \leq Percentile(90th)\right] / SharesOutstnding_{i,t-1}.$$

It captures widespread net trading activity by mutual funds with mild capital flows (the middle eight deciles). Stocks in the top and bottom deciles of *UPressure* are thus expected to be experiencing information-driven purchases and sales, respectively. To identify an exogenous shock to agreement unrelated to firm unobservables, we focus on stocks that are not subject to widespread net trading pressure by other mutual funds than funds with extreme flows, i.e., those in the middle three deciles of *UPressure* (deciles four, five, and six). Among them, we define a stock in the bottom decile of *Pressure* to experience a negative shock to agreement.

We regress forced CEO turnover on *Shock to agreement* and other control variables using the baseline Cox proportional hazard model as in Table 3.3. *Shock to agreement* is defined as a dummy that equals one if the stock is in the bottom decile of *Pressure* and the middle three deciles of *UPressure* during any of the four previous quarters and zero otherwise, i.e., a value of one means a decline in agreement. The results, presented in Table 3.9, suggest that the likelihood of forced CEO turnover is significantly greater following a negative shock to agreement. The estimated coefficient of *Shock to agreement* is positive and statistically significant at 1% level. This finding overcomes the omitted variable bias concern and provides strong support to our disagreement hypothesis.

[TABLE 3.9 GOES HERE]

Evidence from a quasi-natural experiment

Lastly, to further check the robustness of our disagreement hypothesis, we exploit a quasi-natural experiment in which a group of firms experienced an exogenous increase in institutional ownership. The literature suggests that institutional investors, active or passive, play a significant role in corporate governance through different channels. They are generally involved in shareholder activism (e.g., Gillan and Starks, 2007) and other means of intervention and monitoring (e.g., Hartzell and Starks, 2003; Chen, Harford, and Li, 2007; Appel, Gormley, and Keim, 2014; Crane, Michenaud, and Weston, 2014). Crane, et al. (2014) suggest that even passive investors like index funds have incentives to intervene and can influence corporate management through proxy voting and private communication with management if index-tracking-error-constraints or other reasons prevent them from selling their shares. Appel, et al. (2014) specifically show that an increase in ownership by passive institutional investors is associated with an improvement in corporate governance. We expect that a greater concentration of share ownership to institutional investors can induce shareholders to exert more influence on corporate decisions.

We examine how the turnover-disagreement sensitivity changes in response to the exogenous increase in institutional ownership. If the turnover-disagreement relation is driven by an omitted variable, we do not expect it to change because the exogenous shock is unlikely to affect the omitted variable (e.g., uncertainty). However, since we have shown that shareholder governance is important to the disagreement-turnover relation, our disagreement hypothesis predicts that an exogenous improvement in institutional ownership will increase the turnover-disagreement sensitivity.

We take the inclusion of a firm in the S&P 500 as the exogenous shock to institutional ownership of the firm. S&P 500 inclusion increases a firm's institutional ownership for the following reason. Besides the fact that index funds tracking S&P 500 will add the holding of the

company, nonindex funds that typically weigh their managers' performance against the benchmark of S&P 500 will also have an incentive to hold companies in S&P 500. Such an increase in institutional ownership, both active and passive, in the newly included company is expected to be exogenous to expected performance. According to Standard & Poor's, the inclusion of a company in the index does not imply an endorsement of that company's investment potential. Aghion, Van Reenen, and Zingales (2013) use S&P 500 inclusion as an instrument for institutional ownership in their examination of the impact of institutional investors on corporate innovation.

S&P 500 inclusion is unlikely to affect the uncertainty in a firm's growth opportunity and therefore should not impact the turnover-disagreement sensitivity under the alternative omitted variable story. Furthermore, although the selection of a company in the index is not entirely random, the exclusion of firms that have serious bankruptcy risk and the inclusion of firms with good past performance in the selection both work against us finding an increase in forced CEO turnover (which is supposed to be negatively related to past performance). Therefore, we argue that S&P 500 inclusion can affect the disagreement-turnover relation only through its exogenous impact on institutional ownership.

To test the impact of S&P 500 inclusion on the turnover-disagreement sensitivity, we focus on the sample of firms that are included in S&P 500 during our sample period and examine the difference in the turnover-disagreement sensitivity between firm-years before the inclusion in S&P 500 and firm-years after the inclusion in S&P 500.[90] Specifically, we augment the baseline analysis in Table 3.3 by adding *S&P 500 inclusion*, a dummy that equals one for firm-years after the inclusion in S&P 500 and zero otherwise, and an interaction term of it with disagreement. For our disagreement hypothesis to hold, we expect the interaction term to have the same signs with the disagreement proxies as reported in Table 3.3 and to be statistically significant. In comparison, we

[90] Firms that are already in S&P 500 before the start of our sample period or are included in S&P 500 after the end of our sample period 1993-2010 are not included in the sample for this test.

do not expect the interaction term to be significant if it is the omitted variable explanation that holds.

The results, presented in Table 3.10, are consistent with the prediction of our disagreement hypothesis. We find a significantly greater sensitivity of forced CEO turnover to disagreement after a firm's inclusion in S&P 500. Overall, it suggests that the potential omitted variable bias is unlikely to be a major concern here.

[TABLE 3.10 GOES HERE]

3.5.4 Could our Disagreement Proxies be Measuring Other Things?

While we take measures of investor-management disagreement that have been used in the prior literature, one might be concerned that some of these measures – specifically, *Proxy proposal*, *Vote recommendation*, *Actual voting* – could also be related to investors' concern with potential agency issues in the firm in addition to disagreement between investors and management. That is, it is likely that investors may submit proxy proposals, recommend "vote no" or cast votes against certain directors when they are concerned with the agency problems in the firm, even though they share an aligned view with management. This measurement error in these disagreement proxies, if exists, might thus confound the interpretation of our finding.

We show with significant evidence that the issue of measurement error is of little concern and our finding is consistent with our disagreement explanation of forced CEO turnover. First, if our *Proxy proposal* measure mainly captures investors' agency concerns, we would expect to observe a less frequent occurrence of proxy proposal submissions following the enactment of the Sarbanes-Oxley Act in 2002 which arguably enhanced corporate governance standards in public firms. We find this is not the case.[91] Second, if the agency concerns do not vary over time within

[91] We cannot conduct similar checks for *Vote recommendation* and *Actual voting* because the data coverage for these two measures starts from 2003.

a firm and thus is more of an issue cross sectionally, our firm-fixed-effect estimation, discussed earlier, will be effective in accounting for it. Third, our examination using mutual fund flow-induced fire sales as an exogenous shock to investor-management agreement, which does not rely on any measures of disagreement, confirms the significant impact of disagreement on forced CEO turnover.

Lastly, we conduct an additional check that addresses this issue more directly. For each of the three disagreement measures concerned, we estimate an adjusted measure of disagreement after filtering out potential agency concerns in a firm from the original measure and then examine the impact of this adjusted disagreement measure on forced CEO turnover. This is done with a two-stage regression approach. Specifically, in the first stage, we regress each of these disagreement measures on a set of variables that are widely used as proxies for potential agency problems in a firm as well as yearly and industry dummies. These variables include *Abnormal accruals*, *Market-to-book*, *Free cash flow*, *GIM index* (Gompers, Ishii, and Metrick, 2003), and *Entrenched* (the CEO entrenchment index that we develop in the previous section).[92] Firms with higher abnormal accruals (proxy for a higher likelihood of misaligned managerial incentives), lower market-to-book ratios (proxy for fewer growth opportunities), higher free cash flows, more anti-takeover measures, or higher entrenchment indices are more likely to be subject to more severe agency problems. In the second stage, we repeat our baseline analysis in Table 3.3 with each disagreement measure being replaced by the estimated residual in the first stage (which is the adjusted disagreement measure).

The results of the analyses in both stages are presented in Table 3.11. In Panel A for the first-stage analysis, we indeed do not find evidence that these widely-accepted proxies for agency

[92] We note that, depending on how well this set of variables capture potential agency problems in a firm, the extent of which the adjusted disagreement measure is free of potential agency concerns varies across firms. However, on average, it helps to mitigate the impact of potential agency concerns that might be captured in the original disagreement measure.

problems are correlated with our disagreement measures in a consistent way. For example, while *Abnormal accrual* and *Entrenched* are positively related to *Proxy proposal*, *GIM index* is negatively related to it. Also, inconsistent with the agency interpretation of our disagreement measure, we find that *Entrenched* is negatively related to both *Vote recommendation* and *Actual voting*; *Market-to-book* is positively related to *Vote recommendation* and *Free cash flow* is negatively related to *Actual voting*. Nevertheless, in Panel B for the second-stage analysis, we find that the estimated coefficients of all the three adjusted disagreement measures have predicted signs and are statistically significant, consistent with our disagreement hypothesis. The coefficients of all other control variables, which we do not report in the interest of brevity, are comparable to those in Table 3. Overall, the results reassure us that three of our disagreement measures are not subject to measurement error, and that even if measurement error exists, it does not affect our disagreement explanation significantly.

[TABLE 11 GOES HERE]

3.6 Conclusion

Our paper deviates from the conventional focus on firm performance in the study of involuntary CEO turnover, and examines instead the power of investor-management disagreement as a driver of CEO turnover. A CEO is more likely to be forced out if there is a higher level of investor-management disagreement. And this is more likely to be the case when the firm is financially more constrained and thus equity financing is more likely to be needed, and when the CEO is less entrenched or shareholder governance is stronger in the firm. Investor-management disagreement declines after forced CEO turnover, and anticipation of this results in a stock price reaction to the announcement of the firing of a CEO with low agreement with investors that is more positive than the announcement effects associated with other types of forced turnover.

We also examine the impact of investor-management disagreement on a firm's choice of an internal versus external CEO. We find that the firm is more likely to select an external CEO when the departing CEO has higher disagreement with investors. Overall, our study sheds light on the factors affecting CEO turnover and CEO selection, and highlights the role of a previously-ignored factor – investor-management disagreement.

3.7 Appendix

3.7.1 Variable Definitions

Investor-management disagreement

A.1.1. Difference between the analyst forecast of a firm's EPS and its actual value

Our first measure of investor-management disagreement, adopted by Dittmar and Thakor (2007), is the difference between the analyst forecast consensus of a firm's earnings per share (EPS) for a fiscal year and the actual EPS value, scaled by the absolute value of the actual EPS. The analyst forecasts, chosen as the ones most close to the actual EPS disclosure, are made no more than 120 days ago. The idea is that investors' propensity to disagree with management increases in the amount of the firm's EPS falling below the analyst forecast. The lower a manager's ability to outperform beyond expectation, the more likely investors are to question her decisions. Thus, a more positive number of this proxy implies a higher level of disagreement. Our final sample using this disagreement proxy spans 1990 firms and 17568 firm-years from 1993 to 2010.

A.1.2. Submission of proxy proposals in a given year

Our other three disagreement measures are defined following Huang and Thakor (2013). The second proxy for disagreement exploits the idea that investors may submit proxy proposals for a shareholder vote when they disagree and therefore press for changes, but the private communication with management for changes is not effective or fails. Institutional investors, in particular, public and union pension funds, investment firms, and coordinated investors, are found to be the most active sponsors of proxy proposals (e.g., Gillan and Starks, 2000; Thomas and Cotter, 2007; Renneboog and Szilagyi, 2011). To capture our idea of disagreement, we focus on

governance-related proposals only.[93] The issues addressed in such proposals include, but are not limited to, shareholder voting, takeovers, selection of directors, executive compensation, and the sale of the company. Despite the nonbinding nature of voting on shareholder proxy proposals, proposal submission sponsored by shareholders is a conspicuous sign of investor-management disagreement. We use the number of shareholder proxy proposals that a firm receives in a given year to measure the level of disagreement. In untabulated results for brevity, we find that our findings are robust if we use an indicator variable of whether or not a firm receives proxy proposal submissions.

Note that our use of proxy proposal as a disagreement measure does not necessarily suggest that investors' beliefs are always aligned with the firm's management if we do not observe the proposal submissions. It is likely that, in some firms, investors may choose not to submit proxy proposals as a means to challenge managerial decisions, because some unobservable factors may prevent them from doing so at any time. Therefore, to examine whether investors are more likely to disagree with management based on proxy proposal submissions, we follow Huang and Thakor (2013) and exclude firms from our analysis that are never observed to have any shareholder proxy proposals in any given year of the sample period 1993-2010. In focusing on firms that have experienced at least one proxy proposal submission over the sample period, we argue that investors are more likely to disagree with management in the years they submit proxy proposals than in the years in which they do not. Our final sample in using shareholder proxy proposal as a disagreement proxy covers 972 firms and 13121 firm-years from 1993 to 2010.

[93] The other type of proposals is social responsibility related and typically submitted by religious/socially responsible investors.

A.1.3. Vote recommendations in director election

Investors can signal their disagreement with management in the case of director elections. Our third and fourth proxies for disagreement exploit this idea. It is observed that some investors organize "just vote no" campaigns against one or more director candidates to be elected before a director election. Conducted via letters, press release, or internet communications, such campaigns encourage fellow shareholders to withhold votes for the candidate(s). More recently, third-party proxy advisors like Institutional Shareholder Services (ISS) also start issuing vote recommendations for all director candidates who are up for election every year. Voting Analytics (a product of ISS) provides detailed records of such vote recommendations, either "for" or "withhold" ("against"), issued by ISS starting from 2003 for elections in most of the Russell 1000 firms and many of the Russell 2000 firms.

Therefore, for our third disagreement proxy, we relate it to the extent to which a firm's director candidates will receive objections from shareholders or unfavorable recommendations from independent proxy advisors *before* the election. The number of director candidates who are up for election may vary across firms and over time, which affects the extent of potential objections received in different firm-years. To account for this, we define the measure as the proportion of director candidates receiving a "withhold" or "against" recommendation from ISS or/and objections from certain shareholders in a "just vote no" campaign, among all candidates in the firm who are up for election in a given year. A greater magnitude of this measure shall indicate a higher level of disagreement. Our final sample in using this disagreement proxy includes 1613 firms and 8138 firm-years during 2003–2010.

A.1.4. Actual voting in director elections

The fourth proxy relates to actual shareholder voting *during* the director election. Shareholders may express their disagreement by withholding votes for or voting against certain

candidates in the election of directors. Candidates are normally elected with high "for" votes.[94] Therefore, an even slightly lower vote may indicate shareholders' disagreement. As such, we define this proxy as the proportion of director candidates receiving a below-yearly-median percentage of "for" votes in a given firm-year, where the yearly median is the median percentage of "for" votes of director candidates in the universe of firms with available actual voting data in that year. After merging actual voting data with our sample from *ExecuComp*, the final sample with this fourth disagreement proxy covers 1585 firms and 6729 firm-years from 2003 to 2010.[95]

Other variables

- *Abnormal Accruals* is the difference between total accruals and normal accruals where normal accruals is estimated from the Jones abnormal accrual model:

$$TA_{it}/A_{it-1} = \beta\,[1/A_{it-1}] + \alpha_1\,[\Delta REV_{it}/A_{it-1}] + \alpha_2[PPE_{it}/A_{it-1}] + \varepsilon_{it}$$

  where TA is the total accruals, A is total assets, REV is revenues, and PPE is gross property, plant, and equipment.
- *Age* is the age of the CEO (in years).
- *Age squared* is the square of *Age.*
- *Analyst dispersion* is the standard deviation of raw (i.e. not split-adjusted) analysts' forecasts.
- *CEO-Chair duality* is an indicator variable that takes a value of one if the CEO is also the chairperson of the board, and zero otherwise.
- *CEO blockholding* is an indicator variable that takes a value of one if the fraction of outstanding shares owned by the CEO is greater than 5%, and zero otherwise.

[94] For instance, Cai, Garner, and Walkling (2009) find that an average director across all firms receives just over 94% of the "for" votes for the period of 2003–2005.
[95] The smaller sample size here, compared to that of the vote recommendation sample, is due to the missing information in actual votes for many firm-years.

- *EW firm specific return* is the residual component from the regression that decomposes firm performance into a systematic component caused by peer group performance (equally weighted) and a firm-specific component. Put differently, we run the following regression: $r_{i,t-1} = \beta_0 + \beta_1 r_{peer\ group,t-1} + \varepsilon_{i,t-1}$. The $\hat{\varepsilon}_{i,t-1}$ term is the estimated idiosyncratic stock return.
- *EW industry return* is the equally-weighted average stock returns for all firms on CRSP from the same two-digit SIC industry as the sample firm. I exclude each sample firm from the construction of its industry benchmark to eliminate any artificial correlation.
- *EW industry specific return* is $\hat{\beta}_0 + \hat{\beta}_1 r_{peer\ group,t-1}$ from the above regression.
- *Firm size* is the natural log of the total assets of the firm.
- *Free Cash Flow* is the operating income before depreciation deducted by the sum of interest expense, total income tax, preferred dividends and common dividends.
- *GIM index* is the anti-takeover measure index constructed by Gompers, Ishii, and Metrick (2003).
- *Institutional blockholding* is an indicator variable that takes a value of one if there is at least one institutional investor holding more than 5% of the firm's outstanding shares, and zero otherwise.
- *Leverage* is the total book value of debt normalized by the sum of the total book value of debt and market value of equity.
- *Ln(Tenure)* is the natural logarithm of one plus the number of years the CEO was in office.
- *ROA* is the net income deflated by total assets.
- *Stock return* is the Fama-French 48-industry adjusted daily stock return compounded for the previous 12 months.
- *Stock volatility* is the volatility in the firm's stock return over the previous 12 months.
- *Total directors* is the total number of directors who are up for (re)election in a given year.

3.7.2 References

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3.7.3 Tables

Table 3.1

Year-wise distribution of CEO turnover

This table presents the distribution by year of the number and frequency of overall, forced, and external CEO successions for sample firms with no missing *Forecast-Actual EPS* data and covered in ExecuComp between 1993 and 2009. Successions due to mergers, spin-offs, and interim CEO changes are excluded. *Forecast-Actual EPS* is the difference between the mean analyst forecast of a firm's EPS for a fiscal year and its actual value, scaled by the absolute value of actual EPS.

Year	**All successions**		**Forced successions**		**External successions**	
	N	% of all firms	N	% of succession firms	N	% of succession firms
1993	39	7.69%	4	10.26%	9	23.08%
1994	53	8.48%	9	16.98%	6	11.32%
1995	79	11.67%	9	11.39%	13	16.46%
1996	74	10.25%	13	17.57%	22	29.73%
1997	82	10.69%	12	14.63%	24	29.27%
1998	95	11.11%	15	15.79%	19	20.00%
1999	117	12.79%	25	21.37%	26	22.22%
2000	110	11.49%	32	29.09%	30	27.27%
2001	90	9.06%	14	15.56%	27	30.00%
2002	91	8.71%	16	17.58%	30	32.97%
2003	101	8.96%	25	24.75%	40	39.60%
2004	94	8.01%	19	20.21%	27	28.72%
2005	152	12.39%	24	15.79%	49	32.45%
2006	126	9.13%	35	27.78%	55	43.65%
2007	131	8.53%	29	22.14%	52	39.69%
2008	151	9.90%	38	25.17%	48	31.79%
2009	106	6.90%	26	24.53%	43	40.95%
Total	1691	9.75%	345	19.45%	520	29.36%

Table 3.2

Summary statistics

The upper part of Panel A presents summary statistics for the four measures of investor-management disagreement and for forced CEO turnover in each sample of the four measures. The lower part of Panel A reports the univariate evidence of the relation between disagreement and forced CEO turnover. The last column of it reports the difference of the mean disagreement measure for firm-years prior to forced CEO turnover and other firm-years in the sample. *, **, and *** indicate significance at 10%, 5% and 1%, respectively, from t-test conducted on the difference between the two groups. *Forecast-Actual EPS* is the difference between the mean analyst forecast of a firm's EPS for a fiscal year and its actual value, scaled by the absolute value of actual EPS. *Proxy Proposals* is the number of proxy proposals a firm receives in a given year. *Voting Recommendation* is the proportion of director candidates receiving a "withhold" or "against" recommendation from ISS or/and certain shareholders in a "just vote no" campaign, among all candidates in the firm who are up for election in a given year. *Actual Voting* is defined as the fraction of directors receiving less than the yearly median yes-vote casted in all firms with available actual voting data, among all candidates in the firm who are up for election in a given year. Panels B and C provides summary statistics of firm and CEO characteristics for an unbalanced panel of firms from 1993 to 2010 that have non-missing *Forecast-Actual EPS* data. Definitions of these variables are in Appendix.

Panel A: Forced CEO Turnover and Measures of Investor-Management Disagreement

	Mean	Median	S.D.	N
Forecast – Actual EPS	0.04	-0.01	0.27	17568
Forced CEO turnover	0.020	0	0.141	17568
Proxy Proposals	0.55	0	1.09	13121
Forced CEO turnover	0.020	0	0.143	13121
Voting Recommendation	0.10	0	0.22	8138
Forced CEO turnover	0.019	0	0.138	8138
Actual Voting	0.23	0	0.34	6727
Forced CEO turnover	0.022	0	0.148	6727

	Forced CEO turnover		Other Firm-years		Difference
	N	Mean	N	Mean	
Forecast – Actual EPS	345	0.10	17223	0.04	0.06***
Proxy Proposals	267	0.75	12854	0.55	0.20***
Voting Recommendation	153	0.14	7985	0.09	0.05***
Actual Voting	148	0.40	6581	0.23	0.17***

	Mean	Median	S.D.	N
Panel B: Firm Characteristics				
Firm size (log)	7.61	7.48	1.70	17568
Market-to-Book	1.70	1.24	1.46	17568
Leverage	0.22	0.17	0.21	17568
Stock return	0.03	-0.01	0.42	17568
Stock volatility	0.41	0.36	0.21	17568
ROA	0.04	0.05	0.14	17568
Analyst dispersion	0.24	0.12	0.33	17568
Institutional blockholding	0.77	1	0.42	17568
EW Idiosyncratic stock return	0.05	-0.01	0.56	17562
EW Industry stock return	0.10	0.09	0.32	17568
EW Industry-induced stock return	0.11	0.11	0.31	17562
Panel C: CEO Characteristics				
CEO blockholding	0.11	0	0.31	17568
Age	55.78	56	7.10	17568
Tenure	8.64	6.25	7.61	17568
CEO-Chair Duality	0.62	1	0.48	17568

Table 3.3

The effect of disagreement on forced CEO turnover

This table presents coefficient estimates from Cox proportional hazard regressions that examine the likelihood of forced CEO turnovers. The investor-management disagreement proxy used in each regression is indicated at the top of the table. *Forecast-Actual EPS* is the difference between the mean analyst forecast of a firm's EPS for a fiscal year and its actual value, scaled by the absolute value of actual EPS. *Proxy Proposals* is the number of proxy proposals a firm receives in a given year. *Voting Recommendation* is the proportion of director candidates receiving a "withhold" or "against" recommendation from ISS or/and certain shareholders in a "just vote no" campaign, among all candidates in the firm who are up for election in a given year. *Actual Voting* is defined as the fraction of directors receiving less than the yearly median yes-vote casted in all firms with available actual voting data, among all candidates in the firm who are up for election in a given year. All other explanatory variables are defined in Appendix. Year fixed effects are included in all regressions. Different industries (as defined using Fama-French 48 industries) are allowed to have different baseline hazards. Robust standard errors are clustered by firm and reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1%, respectively.

	Disagreement Proxies			
	Forecast-Actual EPS	Proxy Proposal	Voting Recommendation	Actual Voting
$Disagreement_{t-1}$	0.40***	0.16***	0.75**	1.08***
	(0.13)	(0.06)	(0.32)	(0.26)
$Stock\ return_{t-1}$	-1.27***	-1.36***	-1.17***	-1.18***
	(0.23)	(0.27)	(0.32)	(0.33)
$EW\ Industry\ stock\ return_{t-1}$	-0.70**	-0.77**	-0.35	-0.51
	(0.29)	(0.32)	(0.72)	(0.73)
$CEO\ blockholding_{t-1}$	-0.86***	-0.94***	-0.04	-0.02
	(0.30)	(0.32)	(0.33)	(0.34)
Age_t	0.05	-0.12	0.19	0.19
	(0.10)	(0.12)	(0.16)	(0.16)
$Age\ squared_t$	-0.00	0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
$CEO\text{-}Chair\ duality_t$	-0.86***	-0.75***	-0.69***	-0.74***
	(0.12)	(0.13)	(0.20)	(0.21)
$Firm\ size_{t-1}$	0.18***	0.04	0.19***	0.24***
	(0.05)	(0.06)	(0.07)	(0.07)
$Institutional\ blockholding_{t-1}$	0.20	-0.13	0.24	0.16
	(0.15)	(0.14)	(0.30)	(0.29)
$Market\text{-}to\text{-}Book_{t-1}$	-0.08	-0.09	-0.05	-0.01
	(0.06)	(0.08)	(0.09)	(0.09)
$Stock\ volatility_{t-1}$	1.55***	1.22***	2.74***	2.33***
	(0.37)	(0.44)	(0.61)	(0.60)
$Leverage_{t-1}$	0.10	0.84**	-0.36	-0.59
	(0.36)	(0.40)	(0.54)	(0.57)
$Analyst\ dispersion_{t-1}$	0.11			
	(0.18)			
$Total\ directors_{t-1}$			0.03	0.03
			(0.03)	(0.03)
Observations	17568	13121	8138	6727

Table 3.4

The effect of disagreement on forced CEO turnover: Cross-sectional study

This table presents coefficient estimates from Cox proportional hazard regressions that examine the likelihood of forced CEO turnovers in different subsample of firms. Panel A reports results for the subsamples of "Constrained" and "Not constrained". Firms grouped into the "Constrained" subsample are those that have their Whited and Wu (2006) index in the top tercile of the sample and thus are most likely to be financially constrained. Firms grouped into the "Not constrained" are those that have the index in the bottom tercile of the sample and thus are least likely to be financially constrained. Panel B reports results for the subsamples of "Less entrenched" and "Entrenched". Firms in which at least two of the followings are true are grouped into the "Entrenched" subsample: The CEO is also the chairman of the board; the fraction of outsiders on the board is below the sample average; and the fraction of stock ownership by the executive directors is greater than the sample average, and therefore their CEOs are more likely to be entrenched. All other firms are labeled as "Less entrenched". Panel C reports results for the subsample of "Concentrated" and "Less concentrated". Firms whose largest five institutional investors hold more than 20% of their shares outstanding in aggregate are grouped into the "Concentrated" subsample, and all other firms are in the "Less concentrated" subsample. The investor-management disagreement proxies used in each regression is indicated at the top of the table. All other explanatory variables used in Table 3 and yearly dummies are included but not reported for brevity. Different industries (as defined by Fama-French 48 industries) are allowed to have different baseline hazard. Robust standard errors are clustered by firm and reported in parentheses. *, **, and *** indicate significance at 10%, 5% and 1% respectively.

	Disagreement Proxies							
	Forecast – Actual EPS		Proxy Proposal		Voting Recommendation		Actual Voting	
	Panel A: Firms' financial constraints							
	Constrained	Less constrained	Constrained	Less constrained	Constrained	Not constrained	Constrained	Not constrained
$Disagreement_{t-1}$	0.45**	0.13	0.28**	0.08	1.00*	0.86	1.22***	1.68***
	(0.21)	(0.33)	(0.12)	(0.08)	(0.58)	(0.56)	(0.47)	(0.42)
	Panel B: CEO entrenchment							
	Less entrenched	Entrenched	Less entrenched	Entrenched	Less entrenched	Entrenched	Less entrenched	Entrenched
$Disagreement_{t-1}$	0.41**	0.70	0.19***	-0.02	1.12***	1.04	1.24***	1.98*
	(0.19)	(0.43)	(0.07)	(0.15)	(0.38)	(1.38)	(0.31)	(1.09)
	Panel C: Stock ownership concentration by institutional investors							
	Concentrated	Less concentrated	Concentrated	Less concentrated	Concentrated	Less concentrated	Concentrated	Less concentrated
$Disagreement_{t-1}$	0.54***	0.34	0.23***	0.22*	0.84**	0.08	1.06***	0.90
	(0.19)	(0.33)	(0.08)	(0.12)	(0.40)	(1.28)	(0.32)	(0.75)

Table 3.5

The effect of disagreement on external CEO hiring

This table presents results from logit regressions (coefficients of the marginal effect are reported) that examine the impact of investor-management disagreement on the likelihood of external CEO selection, conditional on CEO succession. The dependent variable takes a value of one if the newly appointed CEO has been with the firm for less than a year prior to the appointment and zero otherwise. The investor-management disagreement proxy used in each regression is indicated at the top of the table. *Forecast-Actual EPS* is the difference between the mean analyst forecast of a firm's EPS for a fiscal year and its actual value, scaled by the absolute value of actual EPS. *Proxy Proposals* is the number of proxy proposals a firm receives in a given year. *Voting Recommendation* is the proportion of director candidates receiving a "withhold" or "against" recommendation from ISS or/and certain shareholders in a "just vote no" campaign, among all candidates in the firm who are up for election in a given year. *Actual Voting* is defined as the fraction of directors receiving less than the yearly median yes-vote casted in all firms with available actual voting data, among all candidates in the firm who are up for election in a given year. Other explanatory variables are defined in Appendix. Year and industry dummies are included in the regressions. Robust standard errors are clustered by firm and reported in parentheses. *, **, and *** indicate significance at 10%, 5% and 1% respectively.

	Disagreement Proxies			
	Forecast-Actual EPS	Proxy Proposal	Voting Recommendation	Actual Voting
$Disagreement_{t-1}$	0.12^{***}	0.04^{***}	0.36^{***}	0.21^{***}
	(0.04)	(0.01)	(0.08)	(0.05)
$Stock\ return_{t-1}$	-0.13^{***}	-0.15^{***}	-0.20^{***}	-0.19^{***}
	(0.03)	(0.04)	(0.05)	(0.06)
$Firm\ size_{t-1}$	-0.01	-0.02^{**}	-0.01	-0.01
	(0.01)	(0.01)	(0.02)	(0.02)
$Institutional\ blockholding_{t-1}$	0.01	-0.05^{*}	-0.01	0.01
	(0.03)	(0.03)	(0.06)	(0.06)
$Market\text{-}to\text{-}Book_{t-1}$	-0.01	-0.02	0.01	0.02
	(0.01)	(0.01)	(0.02)	(0.02)
$Stock\ volatility_{t-1}$	0.30^{***}	0.34^{***}	0.19	0.27
	(0.08)	(0.09)	(0.15)	(0.15)
$Leverage_{t-1}$	-0.06	-0.09	-0.04	-0.00
	(0.07)	(0.07)	(0.11)	(0.12)
$Analyst\ dispersion_{t-1}$	-0.02			
	(0.03)			
$Total\ directors_{t-1}$			-0.00	0.001
			(0.01)	(0.01)
Observations	1687	1298	689	670
Pseudo R^2	0.074	0.082	0.076	0.072

Table 3.6

The effect of forced CEO turnover on agreement

This table presents results from OLS regressions (columns (1) and (3)) and logit regressions (columns (2) and (4)) of forced CEO the change in investor-management agreement from year *t-1* to year *t+1* on forced CEO turnover in year *t*. The dependent variables, defined as follows, are the changes in the four disagreement proxies which are indicated at the top of columns: a simple difference of *Forecast-Actual EPS* from year t-1 to year t+1, where *Forecast-Actual EPS* is the difference between the mean analyst forecast of a firm's EPS for a fiscal year and its actual value, scaled by the absolute value of actual EPS; a dummy that equals one if the number of proxy proposals received in year t+1 drops by at least two relative to the number of proxy proposals received in year t-1, and zero otherwise; a simple difference of *Vote Recommendation* from year t-1 to year t+1, where *Vote Recommendation* is the proportion of director candidates receiving a "withhold" or "against" recommendation from ISS or/and certain shareholders in a "just vote no" campaign, among all candidates in the firm who are up for election in a given year; and a dummy that equals one if the fraction of directors receiving less than the yearly-median percentage of yes-votes among all candidates up for election in year t+1 is less than that in year t-1, and zero otherwise. *Forced turnover* is a dummy that equals one if a forced CEO turnover occurs in year t and zero otherwise. Other explanatory variables are defined in Appendix. All regressions include year and Fama-French 48 industry dummy variables. Coefficients of the marginal effect are reported in the logit regressions. Robust standard errors are clustered by firm and reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% levels, respectively.

	Proxies for Change in Agreement			
	Forecast–Actual EPS	Proxy Proposal	Voting Recommendation	Actual Voting
	(1)	(2)	(3)	(4)
Forced turnover$_t$	-0.07*	0.013*	-0.06***	0.06**
	(0.04)	(0.007)	(0.02)	(0.03)
Stock return$_t$	-0.00	0.0004	-0.04***	0.04***
	(0.01)	(0.005)	(0.01)	(0.01)
Stock volatility$_t$	0.13***	-0.004	0.04*	0.001
	(0.03)	(0.011)	(0.02)	(0.03)
ROA$_t$	-0.05	-0.026	0.07	-0.04
	(0.06)	(0.033)	(0.06)	(0.09)
Market-to-Book$_t$	0.00	-0.0004	-0.01**	-0.02**
	(0.00)	(0.002)	(0.00)	(0.01)
Firm size$_t$	0.01***	0.009***	0.00	-0.005
	(0.00)	(0.001)	(0.00)	(0.003)
Observations	14993	9851	7503	6564
Adjusted/Pseudo R^2	0.017	0.132	0.020	0.170

Table 3.7

The effect of disagreement on forced CEO turnover: Alternative measures of firm/industry performance

This table presents coefficient estimates from Cox proportional hazard regressions that examine the likelihood of forced CEO turnovers. The investor-management disagreement proxy used in each regression is indicated at the top of the table. *Forecast-Actual EPS* is the difference between the mean analyst forecast of a firm's EPS for a fiscal year and its actual value, scaled by the absolute value of actual EPS. *Proxy Proposals* is the number of proxy proposals a firm receives in a given year. *Voting Recommendation* is the proportion of director candidates receiving a "withhold" or "against" recommendation from ISS or/and certain shareholders in a "just vote no" campaign, among all candidates in the firm who are up for election in a given year. *Actual Voting* is defined as the fraction of directors receiving less than the yearly median yes-vote casted in all firms with available actual voting data, among all candidates in the firm who are up for election in a given year. All other explanatory variables are defined in Appendix. Year fixed effects are included in all regressions. Different industries (as defined using Fama-French 48 industries) are allowed to have different baseline hazards. Robust standard errors are clustered by firm and reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1%, respectively.

	Disagreement Proxies							
	Forecast-Actual EPS		Proxy Proposal		Voting Recommendation		Actual Voting	
$Disagreement_{t-1}$	0.43***	0.41***	0.17***	0.16***	0.69**	0.74**	1.08***	1.08***
	(0.13)	(0.13)	(0.06)	(0.06)	(0.32)	(0.32)	(0.26)	(0.26)
ROA_{t-1}	-1.13**		-1.51*		-1.65*		-1.41	
	(0.55)		(0.81)		(0.93)		(0.95)	
EW Idiosyncratic stock return $_{-1}$		-1.30***		-1.44***		-1.15***		-1.18***
		(0.23)		(0.28)		(0.32)		(0.33)
EW Industry-induced stock $return_{t-1}$		-0.65**		-0.77**		-0.32		-0.44
		(0.31)		(0.36)		(0.85)		(0.87)
$CEO\ blockholding_{t-1}$	-0.83***	-0.86***	-0.97***	-0.93***	-0.01	-0.04	0.01	-0.02
	(0.29)	(0.30)	(0.32)	(0.32)	(0.32)	(0.33)	(0.33)	(0.34)
Age_t	0.07	0.05	-0.09	-0.13	0.18	0.19	0.20	0.19
	(0.10)	(0.10)	(0.12)	(0.12)	(0.16)	(0.16)	(0.17)	(0.16)
Age $squared_t$	-0.00	-0.00	0.00	0.00	-0.00	-0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
CEO-Chair $duality_t$	-0.89***	-0.86***	-0.77***	-0.75***	-0.70***	-0.69***	-0.78***	-0.74***
	(0.12)	(0.12)	(0.13)	(0.13)	(0.20)	(0.20)	(0.21)	(0.21)
Firm $size_{t-1}$	0.19***	0.19***	0.04	0.04	0.20***	0.19***	0.24***	0.24***
	(0.05)	(0.05)	(0.06)	(0.06)	(0.07)	(0.07)	(0.07)	(0.07)
Institutional $blockholding_{t-1}$	0.20	0.20	-0.13	-0.13	0.26	0.25	0.17	0.16
	(0.14)	(0.15)	(0.15)	(0.14)	(0.29)	(0.30)	(0.28)	(0.29)
Market-to-$Book_{t-1}$	-0.18***	-0.08	-0.18**	-0.08	-0.10	-0.05	-0.07	-0.02
	(0.06)	(0.06)	(0.07)	(0.08)	(0.09)	(0.09)	(0.10)	(0.09)
Stock $volatility_{t-1}$	1.91***	1.56***	1.50***	1.18***	2.66***	2.74***	2.37***	2.33***
	(0.37)	(0.37)	(0.45)	(0.44)	(0.60)	(0.61)	(0.58)	(0.60)
$Leverage_{t-1}$	0.17	0.09	0.92**	0.85**	-0.31	-0.37	-0.47	-0.59
	(0.37)	(0.36)	(0.40)	(0.40)	(0.55)	(0.54)	(0.57)	(0.57)
Analyst $dispersion_{t-1}$	0.08	0.10						
	(0.17)	(0.18)						
Total $directors_{t-1}$					0.03	0.03	0.03	0.03
					(0.03)	(0.03)	(0.03)	(0.03)
Observations	17568	17562	13121	13112	8138	8134	6727	6724

Table 3.8

The effect of disagreement on voluntary CEO turnover

This table presents coefficient estimates from Cox proportional hazard regressions that examine the likelihood of voluntary CEO turnovers. The investor-management disagreement proxy used in each regression is indicated at the top of the table. *Forecast-Actual EPS* is the difference between the mean analyst forecast of a firm's EPS for a fiscal year and its actual value, scaled by the absolute value of actual EPS. *Proxy Proposals* is the number of proxy proposals a firm receives in a given year. *Voting Recommendation* is the proportion of director candidates receiving a "withhold" or "against" recommendation from ISS or/and certain shareholders in a "just vote no" campaign, among all candidates in the firm who are up for election in a given year. *Actual Voting* is defined as the fraction of directors receiving less than the yearly median yes-vote casted in all firms with available actual voting data, among all candidates in the firm who are up for election in a given year. All other explanatory variables are defined in Appendix. Year fixed effects are included in all regressions. Different industries (as defined using Fama-French 48 industries) are allowed to have different baseline hazards. Robust standard errors are clustered by firm and reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1%, respectively.

	Disagreement Proxies			
	Forecast-Actual EPS	Proxy Proposal	Voting Recommendation	Actual Voting
$Disagreement_{t-1}$	-0.05	-0.01	-0.63*	-0.37
	(0.10)	(0.05)	(0.34)	(0.23)
$Stock\ return_{t-1}$	-0.38***	-0.25*	-0.34*	-0.43**
	(0.11)	(0.14)	(0.21)	(0.20)
$EW\ Industry\ stock\ return_{t-1}$	-0.58**	-0.14	-0.14	-0.06
	(0.23)	(0.25)	(0.46)	(0.49)
$CEO\ blockholding_{t-1}$	-0.24	-0.51*	-0.45	-0.63*
	(0.19)	(0.27)	(0.34)	(0.36)
Age_t	0.49***	0.60***	0.88***	0.81***
	(0.09)	(0.12)	(0.18)	(0.17)
$Age\ squared_t$	-0.01***	-0.01***	-0.01***	-0.01***
	(0.00)	(0.00)	(0.00)	(0.00)
$CEO\text{-}Chair\ duality_t$	-0.70***	-0.89***	-0.28**	-0.35**
	(0.10)	(0.11)	(0.14)	(0.14)
$Firm\ size_{t-1}$	0.08**	0.05	0.02	0.09*
	(0.03)	(0.04)	(0.05)	(0.05)
$Institutional\ blockholding_{t-1}$	-0.05	-0.20*	0.05	0.07
	(0.10)	(0.10)	(0.18)	(0.19)
$Market\text{-}to\text{-}Book_{t-1}$	-0.00	-0.01	0.08	0.13**
	(0.03)	(0.04)	(0.05)	(0.06)
$Stock\ volatility_{t-1}$	0.63**	0.93***	-0.30	-0.31
	(0.28)	(0.33)	(0.53)	(0.56)
$Leverage_{t-1}$	-0.09	-0.21	0.37	0.24
	(0.28)	(0.31)	(0.42)	(0.42)
$Analyst\ dispersion_{t-1}$	0.17			
	(0.12)			
$Total\ directors_{t-1}$			-0.00	-0.02
			(0.02)	(0.02)
Observations	16591	12295	8011	6609

Table 3.9

The effect of disagreement on forced CEO turnover: Evidence from an exogenous shock to agreement

This table presents coefficient estimates from Cox proportional hazard regressions that examine the likelihood of forced CEO turnover following an exogenous shock to agreement. *Shock to agreement* is defined as a dummy that equals one if the stock is in the bottom decile of *Pressure* and the middle three deciles of *UPressure* during any of the four previous quarters and zero otherwise. *Pressure* is mutual fund flow-induced trading pressure defined as in Coval and Stafford (2007). *UPressure* is unforced trading pressure, defined as in Khan, Kogan, and Serafeim (2012), by mutual funds that experience mild capital flows (the middle eight deciles of flows). All other explanatory variables are defined in Appendix. Year fixed effects are included in all regressions. Different industries (as defined using Fama-French 48 industries) are allowed to have different baseline hazards. Robust standard errors are clustered by firm and reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1%, respectively.

Shock to agreement$_{t-1}$	0.71***
	(0.25)
Stock return$_{t-1}$	-1.19***
	(-0.36)
EW Industry stock return$_{t-1}$	-0.69
	(-0.42)
CEO blockholding$_{t-1}$	-0.72*
	(-0.40)
Age$_t$	0.05
	(0.13)
Age squared$_t$	0.00
	(0.00)
CEO-Chair duality$_t$	-1.05***
	(-0.17)
Firm size$_{t-1}$	0.21***
	(0.06)
Institutional blockholding$_{t-1}$	0.29
	(0.20)
Market-to-Book$_{t-1}$	-0.09
	(-0.08)
Stock volatility$_{t-1}$	1.82***
	(0.51)
Leverage$_{t-1}$	0.82*
	(0.48)
Observations	10095

Table 3.10

The effect of disagreement on forced CEO turnover: Evidence from S&P 500 addition

This table presents coefficient estimates from Cox proportional hazard regressions that examine the likelihood of forced CEO turnovers for firms that are included in S&P 500. The investor-management disagreement proxy used in each regression is indicated at the top of the table. *Forecast-Actual EPS* is the difference between the mean analyst forecast of a firm's EPS for a fiscal year and its actual value, scaled by the absolute value of actual EPS. *Proxy Proposals* is the number of proxy proposals a firm receives in a given year. *Voting Recommendation* is the proportion of director candidates receiving a "withhold" or "against" recommendation from ISS or/and certain shareholders in a "just vote no" campaign, among all candidates in the firm who are up for election in a given year. *Actual Voting* is defined as the fraction of directors receiving less than the yearly median yes-vote casted in all firms with available actual voting data, among all candidates in the firm who are up for election in a given year. *S&P 500 inclusion* takes a value of one for subsequent years after the inclusion of the firm to the S&P 500 index, and zero for years prior to the year of inclusion. All other explanatory variables are defined in Appendix. Year fixed effects are included in all regressions. Different industries (as defined using Fama-French 48 industries) are allowed to have different baseline hazards. Robust standard errors are clustered by firm and reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1%, respectively.

	Disagreement Proxies			
	Forecast – Actual EPS	Proxy Proposals	Voting Recommendation	Actual Voting
Disagreement **X** S&P 500 inclusion	2.42***	0.32***	5.84***	1.57*
	(0.80)	(0.13)	(0.03)	(0.88)
Observations	3171	1441	184	140
Controls	YES	YES	YES	YES

Table 3.11

The effect of disagreement on forced CEO turnover: Adjusted measures of disagreement

Panel A presents coefficient estimates from OLS regressions of the disagreement measures on proxies for agency problems. Panel B presents coefficient estimates from Cox proportional hazard regressions of forced CEO turnovers on the estimated residuals obtained in the first-stage regressions of Panel A and other control variables as in Table 3.3. The investor-management disagreement proxy used in each regression is indicated at the top of the table. *Proxy Proposals* is the number of proxy proposals a firm receives in a given year. *Voting Recommendation* is the proportion of director candidates receiving a "withhold" or "against" recommendation from ISS or/and certain shareholders in a "just vote no" campaign, among all candidates in the firm who are up for election in a given year. *Actual Voting* is defined as the fraction of directors receiving less than the yearly median yes-vote casted in all firms with available actual voting data, among all candidates in the firm who are up for election in a given year. All other explanatory variables are defined in Appendix. Year and industry fixed effects are included in all regressions. Different industries (as defined using Fama-French 48 industries) are allowed to have different baseline hazards in Panel B. Robust standard errors are clustered by firm and reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1%, respectively.

Panel A: First-stage regression of disagreement measures on proxies for agency problems

	Proxy proposal	Voting Recommendation	Actual Voting
Abnormal Accruals	0.047***	0.003	0.003
	(0.007)	(0.002)	(0.003)
Market-to-Book	-0.012	0.005***	-0.000
	(0.095)	(0.000)	(0.001)
Free Cash Flow	-0.379	-0.080	-0.339***
	(0.233)	(0.054)	(0.080)
GIM index	-0.046***	-0.000	0.002
	(0.006)	(0.001)	(0.002)
Entrenched	0.130***	-0.041***	-0.023**
	(0.030)	(0.008)	(0.012)
Constant	1.717**	0.174***	-0.087
	(0.846)	(0.060)	(0.085)
Observations	6832	4977	4052
Adjusted R^2	0.107	0.031	0.207

Panel B: Second-stage regression of forced CEO turnover on adjusted disagreement measures

	Proxy proposal	Voting Recommendation	Actual Voting
Adjusted Disagreement$_{t-1}$	0.220***	1.045**	1.374***
	(0.066)	(0.418)	(0.394)
Observations	6671	4662	4006
Controls	Yes	Yes	Yes
Pseudo R^2	0.146	0.126	0.165