ESSAYS IN CORPORATE GOVERNANCE

By

Lixiong Guo

Dissertation

Submitted to the Faculty of the

Graduate School of Vanderbilt University

in partial fulfillment of the requirements

for the degree of

DOCTOR OF PHILOSOPHY

in

Management

August, 2012

Nashville, Tennessee

Approved:

Professor Ronald W. Masulis

Professor William G. Christie

Professor Craig M. Lewis

Professor Alexei V. Ovtchinnikov

Professor Hans R. Stoll

Professor Richard H. Willis

To my parents, my wife and my beloved children, Bill and Audrey.

ACKNOWLEDGEMENT

I want to thank faculty members at Vanderbilt who have taught me or contributed to my research, and especially members of my dissertation committee. Each member of my dissertation committee has provided me with extensive personal and professional guidance. Among them, I want to particularly thank Professor Hans Stoll for being a long-time mentor and for spending many hours with me in discussing my research and Professor Craig Lewis whose door was always open for discussions and who contributed tremendously in directing me through the final stage of my dissertation. I am especially indebted to Professor Ronald Masulis, the chair of my dissertation committee, for his guidance and support throughout my doctoral study at Vanderbilt. His academic achievements and his seemingly inexhaustible energy and enthusiasm for research have inspired me all the way along and will continue to do so in the years to come.

I also want to acknowledge the generous financial support from the Doctoral Fellowship offered by Owen Graduate School of Management at Vanderbilt University and the Harold Stirling Vanderbilt Scholarship offered by the Graduate School at Vanderbilt University, without which I would not have been able to begin my doctoral study and this work would not have been possible.

Lastly, I am grateful to my parents for their unconditional love and support. I especially want to thank my father whose story of going from a farm boy to a graduate of Peking University in China and a scientist has always been a source of strength and inspiration that drives me forward.

TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

CHAPTER I

BOARD STRUCTURE AND MONITORING: NEW EVIDENCE FROM CEO TURNOVER

1. Introduction

Are independent directors more effective than inside and grey directors in monitoring the CEOs? Do board and board committee independence, as measured by the proportion of independent directors, matter for board monitoring? These are fundamental questions in corporate governance. The conventional wisdom is that independent directors are more effective. By definition, independent directors have no familial or economic ties to the CEO and the firm they monitor other than that through their directorship and ownership. Thus they are believed to be more willing to challenge CEOs than other directors who could be unduly influenced by the CEO due to career concerns and other ties to the CEO. In addition, Fama (1980) argue that outside directors have incentives to develop their reputations as decision control experts in the labor market for directors, which provides them with incentives to monitor more carefully. This view has greatly influenced regulators and investors. For example, NYSE and Nasdaq issued new listing rules following the passage of the Sarbanes-Oxley Act (SOX) that require boards in listing companies to meet certain independence standards. CALPER, a large pension fund, recommends that an ideal board should be consisted of all independent directors except the CEO. TIAA-CREF, another large pension fund, states in their policy statement on corporate governance that "the board should be composed of a substantial majority of independent directors." However, arguments have also been made which suggest that independent directors may not be more effective at monitoring. First, as pointed out by Hermalin and Weisbach (1998), a director reputation of not making trouble for CEOs can also be potentially valuable to independent directors. This can offsets the reputation benefits to directors of close monitoring of CEOs described in Fama (1980). Second, independent directors often face severe limitations on their access to firm specific information, which can seriously undercut their ability to effectively monitor the CEO (Jensen, 1993). In fact, Masulis and Mobbs (2011) find that having better informed inside directors who hold outside directorships at unaffiliated firms appears to benefit shareholders more than greater board independence. Third, independent directors typically have small equity stakes in the firm they monitor, so they lack strong financial incentives to carefully monitor the CEO. Lastly, increasing the number of independent directors on the board aggravates the independent director's free-rider problem (Harris and Raviv, 2008). As a result, the net benefit of monitoring by independent directors is theoretically ambiguous and ultimately an important empirical question.

Empirical evidence thus far on the effectiveness of monitoring by independent directors is mixed and inconclusive. This mainly reflects the empirical challenge of addressing the problem of endogeneity of board structure. This endogeneity problem has plagued the interpretation of results in most prior studies. Coefficients estimates on key explanatory variables in these studies are likely to be biased because the endogeneity of board structure is usually not directly accounted for in these studies (See discussions in Bhagat and Black, 2002; Hermalin and Weisbach, 2003). Thus, solving this endogeneity problem is crucial to assessing the importance of director independence. Recently, Chhaochharia and Grinstein (2009) and Guthrie, Sokolowsky and Wan (2010) use the mandatory change in board structure required by the new NYSE and Nasdaq listing rules of 2003 to examine the effect of board and compensation committee independence on CEO compensation[1,2]. They find that CEO compensation did not significantly

[1] This question is first studied by Chhaochharia and Grinstein (2009). Guthrie et al. (2010) revisit the results in Chhaochharia and Grinstein (2009) and find different results. Given the outlier issues in the Chaochharia and Grinstein (2009) paper, I mainly discuss the results in the Guthrie et al. (2010) paper.

[2] Other studies that have directly addressed the endogeneity problem include Duchin, Matsusaka and Ozbas (2010) and Linck, Netter and Wintoki (2011). Duchin et al. (2010) uses "exogenous" shocks from regulatory changes between 1999 and 2002 in the U.S. to study the relation between board independence and firm performance. They find that independent directors can have a positive effect on firm performance in firms with low information costs, but a negative effect in firms with high information costs. Linck et al. (2011) use a dynamic model to address endogeneity. They find that there is no causal relation between board structure and current firm performance. Unlike us, these

change in firms that were forced to have a majority of independent directors on the board and, surprisingly, CEO total pay actually increased in firms that were forced to adopt a fully independent compensation committee, relative to firms that were not affected by these new exchange listing rules. Given the enormous growth in CEO pay before SOX that has drawn intense public criticism, this evidence raises further questions about the effectiveness of independent directors in monitoring CEOs.

In this study, I shed new light on these questions by examining changes in sensitivity of forced CEO turnovers to firm performance in publicly listed U.S. firms before and after the issuance of new NYSE and Nasdaq listing rules following the passage of SOX. In an agency setting, the strength of the relation between forced CEO turnover and firm performance provides a relativley clean and direct measure of the effectiveness of board monitoring. A higher sensitivity of forced CEO turnover to firm performance is widely taken to be an indication of more effective board monitoring (Weisbach, 1988; Lel and Miller, 2008; Jenter and Lewellen, 2010; Kaplan and Minton, 2011). In contrast, although a higher level of CEO compensation could indicate less effective monitoring by the board, it can also be consistent with models of optimal contracting. For example, in the model of Hermalin (2005), an increase in intensity of board monitoring reduces the CEO's job stability, which in turn requires higher CEO pay due to the rise in CEO risk bearing. Here, higher CEO pay is a consequence of more intensive monitoring rather than an indication of less effective monitoring. This highlights the importance of examining CEO turnover, rather than relying solely on evidence on CEO compensation, before we draw any conclusions about the effectiveness of independent director monitoring.

To overcome the well-known problem of endogeneity of board structure, I use the issuance of new NYSE and Nasdaq listing requirements following the passage of SOX as an exogenous shock and examine how sensitivity of forced CEO turnover to firm performance

---

authors examine the net effect of board's monitoring and advising roles. My results help to shed light on one potential mechnism that is at work.

changes in firms that were affected by the new exchange listing rules relative to firms that were already in compliance with the new exchange listing rules in a difference-in-difference (DD) setting. Unlike prior studies such as Dahya et al. (2002), Chhaochharia and Grinstein (2009), and Guthrie et al. (2010), I match treatment and control firms based on Fama-French 48 industry and a propensity score before performing the DD analysis to mitigate the bias that could result from violation of the common temporal trend assumption of this method.

To see the importance of using "exogenous" shocks to study the causal relation between board structure and CEO turnover, we can consider a modified version of the Hermalin and Weisbach (1998) model in which the board learns about CEO ability from prior firm performance, while the CEO bargains with the board over new director nominations. A CEO wants to nominate directors with whom they have close ties, while the board of directors wants the opposite. As perceived CEO talent increases, so does the bargaining power of the CEO. Hence, more talented CEOs are on average associated with less independent boards. At the same time, it takes a bigger drop in firm performance for the board to change their belief in a CEO's ability from positive to negative, given strong prior performance. As a result, the empirically observed positive cross-sectional relation between board independence and sensitivity of CEO turnover to firm performance can be actually driven by the fact that boards act more slowly in firing a highly talented CEO in response to poor short-run performance. The endogenous relation between board independence and turnover-performance sensitivity can also arise for other reasons. For example, if independent directors, for example, out of concern for their reputations on the directors market, choose to serve on boards that conduct more effective monitoring, then this will lead us to observe a stronger relation between firm performance and CEO turnover in firms with more independent boards. However, this relation is really driven by independent directors choosing to sit on boards that are more effective at monitoring to begin with not by board independence. It would be misleading if we conclude from this correlation that increasing board independence leads to improved board monitoring. In reality, the endogenous relations between board (committee) independence and board decisions can be quite complicated. Since the omitted variables that drive the endogenous relations are usually unobservable, using exogenous shocks that substantially alter board structure represents an effective way to overcome this endogeneity problem.

In 2001 and 2002, major scandals occurred in the U.S. at a number of prominent public companies such as Enron, Tyco, and WorldCom, which shocked investors and highlighted serious shortcomings in the oversight of US public firms by boards of directors and external auditors. To bolster investor confidence and "strengthen corporate governance practices of listed companies", the U.S. Congress passed the Sarbanes-Oxley Act in 2002. Pursuant to SOX and at the SEC "urging", the NYSE and Nasdaq proposed new listing rules in August and October of 2002 respectively that set specific requirements on board and committee independence. The SEC approved these new listing rules on November 4, 2003 with minor changes. The main provisions of the approved new listing rules for corporate boards require the board of each listed company to have (i) a majority of independent directors, (ii) nominating, compensation, and audit committees composed entirely of independent directors, and (iii) regular executive sessions attended solely by non-management directors[3,4].

I identify three new exchange listing rules that are likely to have significant impacts on sensitivity of forced CEO turnover to firm performance in affected firms. The first is the rule that requires a majority of independent directors on the board. If the net benefit of independent director monitoring is positive, then I expect that compliance with this rule leads to an increase in sensitivity of forced CEO turnover to firm performance in affected firms. The second is the rule that requires a fully independent nominating committee. One major task of the nominating committee is to select nominees to stand for director election. When the nominating committee is

[3] "Controlled" companies are exempt from the requirements for a majority independent board and fully independent compensation and nominating committees. A controlled company is a company of which more than 50% of the voting power is held by an individual, group or another company.

[4] Nasdaq allows a majority of independent directors or a fully independent nominating committee to nominate directors. Similarly, Nasdaq allows a majority of independent directors or a fully independent compensation committee to set CEO compensation.

not fully independent or when the firm does not have a nominating committee, the CEO is almost always involved in the nominating process and usually has significant say on whom to be nominated (Shivdasani and Yermack, 1999). This can reduces the willingness of even independent directors to challenge the CEOs because unsuccessful challenges may greatly reduce their chances of being nominated to the board again. The influence of the CEO is reinforced by her ability to appointing directors who have close ties to her. One important implication of the requirement for fully independent nominating committee is that the CEO is excluded from the process of nominating directors for election. This can be crucial to board monitoring and the dynamics of board decision making. Now, even the incumbent independent directors can become more effective at monitoring because they need to concern less about CEO displeasure with their queries hurting their chance of being re-nominated to the board. Furthermore, delegating the director selection process to independent directors reduces the chance that the CEO can game the regulation by appointing gray directors who fit the definition of independence under the regulation, but are strongly connected to the management in other ways. Hence, I expect to observe an increase in turnover-performance sensitivity in firms forced to move to fully independent nominating committees. The third is the rule that requires firms to have fully independent audit committees. I epxect that compliance with this ruel lead to an increase in sensitivity of forced CEO turnvoer to firm performance especially accounting performance in affected firms. This is because the move to fully independent audit committee may improve the quality of reported accounting numbers. If the board consequently puts higher weight on the firm’s accounting performance when making CEO turnover decisions then there should be an increase in sensitivity of forced CEO turnover to acccounting performance. If the effect of higher quality of reported accounting numbers spills over to reduce the noise in stock prices, then there may be even an increase in sensitivity of forced CEO turnover to stock performance. It is worth noting that, unlike my first two hypotheses, the effect of change in audit committee composition on turnover-performance sensitivity is hypothesized to be through its effect on quality of reported accounting numbers rather than on director incentives.

Consistent with the first two hypotheses, I find firms forced to adopt a majority independent board experienced a significantly larger increase in sensitivity of forced CEO turnover to firm performance after SOX than firms that before SOX already had a majority independent board. Similarly, I find firms forced to adopt a fully independent nominating committee also experienced a significantly larger increase in sensitivity of forced CEO turnover to firm performance after SOX than firms before SOX already had a fully independent nominating committee. However, I find no evidence that firms that were noncompliant prior to SOX with the rule that requires a fully independent audit committee experienced any significant change in sensitivity of forced CEO turnover to firm performance after SOX relative to firms that were previously compliant with the rule. The results hold regardless of whether I measure firm performance by market-adjusted stock return or industry-adjusted change in ROA or whether I include just one treatment effect or all three treatment effects simultaneously.

In terms of economic significance, my estimates suggest that, for an average firm that was forced to adopt a majority independent board, the increase in implied probability of a CEO being fired for a drop in market-adjusted stock return (industry-adjusted change in ROA) from the 75^{th} to the 25^{th} percentile of the sample is about 3 times (14 times) higher post-SOX than pre-SOX. For an average firm that was forced to adopt a fully independent nominating committee, the corresponding increase in the implied probability of a CEO being fired is about 2 times (9 times) higher post-SOX than pre-SOX. The evidence supports the view that independent directors conduct more intensive internal monitoring than insiders and gray directors *ceteris paribus*. Furthermore, the evidence suggests that having fully independent *nominating committees* is important to board monitoring even in firms that previously had a majority of independent directors.

To see if the increase in turnover-performance sensitivity in affected firm after SOX can be interpreted as evidence of more effective monitoring, I first examine change in firm performance around forced CEO turnovers. Consistent with more effective monitoring, I find that on average stock performance and operating performance improve following forced CEO turnovers in affected firms in the post-SOX period. Also, there is some evidence that treatment firms experience a smaller decline in firm performance prior to and a quicker recovery after a forced CEO turnover in the post-SOX period than in the pre-SOX period.

Then, I compare stock market reactions to forced CEO turnover announcements in the pre- and post-SOX period in affected firms to see if quicker firing in the post-SOX period leads to a significant increase in making type I errors and thus outweighs the potentially positive effect of more intensive monitoring on firm value. Theoretically, the increase in performance sensitivity of CEO turnover can potentially have two effects on firm value – one positive and the other negative. On the positive side, the higher sensitivity provides stronger incentives to the CEO to work harder. At the same time, more rapid termination of poorly performing CEOs shortens the period of performance decline prior to CEO turnover and allows a firm to avoid severe damage to its competitive position. On the negative side, a quicker termination of a poorly performing CEO can increase the probability of making type I errors, i.e. prematurely firing an otherwise talented CEO, who happens to realize a bad performance draw. Both effects should be impounded in the stock returns upon the news of the CEO turnover. If the premature firing effect dominates in the post-SOX period, I expect to observe less positive or more negative market reactions to CEO turnover announcement in the post-SOX period than in the pre-SOX period.

My empirical results show that the abnormal stock returns around forced CEO turnover announcements are on average higher in the post-SOX period than in the pre-SOX period in firms that are affected by either the rule requiring a majority of independent directors on board or the rule requiring a fully independent nominating committee, though the difference is statistically insignificant. This suggests that the quality of CEO firing decisions is at least as good in the post-

SOX period as in the pre-SOX period in affected firms, if not better. This finding is inconsistent with the premature firing hypothesis. Since higher turnover-performance sensitivity should always increases CEOs' incentives to work harder, the lack of evidence of premature firing suggests that board and nominating committee independence appears to lead to more effective monitoring of CEOs.

This study makes three contributions to the corporate governance literature and policy debates on regulation of boards of directors. First, I provide the most credible statistical evidence to date on a *causal* relation between overall board independence and CEO turnover. Weisbach (1988) is the first to establish a *correlation* between overall board independence and CEO turnover. He finds that CEO turnover is more sensitive to firm performance in firms with an outsider-dominated board (more than 60% outside directors) than in firms with an insider-dominated board (less than 40% outside directors). Taking a different approach from Weisbach, Dahya et al. (2002) examine change in CEO turnover in U.K. firms following the issuance of the Cadbury Commission recommendations, which recommended that each firm to have at least three nonexecutive directors and a separate CEO and Chairman. They find U.K. firms that adopted the Cadbury Commission recommendations increased their sensitivity of CEO turnover to performance in the following years. Since the adoption of Cadbury Committee recommendations is not mandatory, the shock to board composition they study is not as "exogenous" as the shock in this study where compliance with new exchange listing rules is mandatory for firms wishing to keep their exchange listing. I also use more rigorous econometric techniques than Dahya et al. (2010) to address the endogeneity of board structure[5].

Neither Weisbach (1988) nor Dahya et al. (2002) examines the relation between board *committee* independence and CEO turnover. This leads to my second contribution. To my

---

[5] Dahya et al. (2002) does not use a difference-in-difference approach. Their approach does not allow a statistical test on whether the change in sensitivity of CEO turnover to firm performance in the adoption firms is driven by change in board composition or by a common temporal trend in all firms. I replicated their approach by estimating separate regressions for firms in the treatment and control groups of my sample and comparing changes in sensitivity of forced CEO turnover to firm performance before and after SOX. I find that their approach does not always give me the same inferences as the difference-in-difference approach because it does not control for time trend in CEO turnover.

knowledge, this is the first study to document a relation between nominating committee independence and forced CEO turnover. While it is exactly the obervation that management has control of the board selection process that leads Berle and Means (1932) and more contemporary authors like Jensen (1993) to question the effectiveness of board monitoring, existing corporate governance literature has largely focused on how the outcome of this selection process – i.e. the insider-outsider ratio – affects board oversight. Surprisingly, it pays little attention to the more important question of who selects the board of directors and how this arrangement affects board monitoring. Hence, this study fills an important gap in existing literature[6]. The evidence is especially important to the current debate on board reforms and it has important policy implications. Opponents of board reforms often point out that inside directors play important advisory roles on boards, hence mandatory increases in the percentage of independent directors on boards may push firms away from their optimal inside-outside director ratio and thus be value decreasing (Boone et al., 2007; Adams and Ferreira, 2007; Coles et al., 2008; Linck et al., 2008). Based on the typical advisory roles that insiders play, it appears that, although this advisory function may be important in major decisons that requires the entire board to participate, it is much less so in decisions such as nominating directors for election to the board. One policy implication of my evidence is that one way to avoid the cost associated with one-size-fits-all board regulations while still improve monitoring is to mandate nominating committee composition, but leave the decision on the composition of the overall board to the firms themselves.

Lastly, I document an important mechanism through which the stock exchanges' new listing rules seem to have benefited shareholders. While the intent of the new exchange listing rules is clear, its ultimate effects are much less so. First, it is doubtful whether board independence can be truly increased by setting numerial targets for representation of independent

[6] Berle and Means (1932 p.87): "… control will tend to be in the hands of those who select the proxy committee and by whom, the election of directors for the ensuing period will be made. Since the proxy committee is appointed by the existing management, the latter can virtually dictate their own successors."

directors. After all, firms can offset the intended effects by appointing directors who are independent by definition, but have other strong ties to the management, such as close social ties or ties through interlocking boards (Hallock, 1997; Hwang & Kim, 2009). For example, in commenting on board reforms, Hermalin and Weisbach (1998) observe that "requiring a specific fraction of the board to be outsiders would result in an outsider-dominated board, but not necessarily one that is more independent than the insider-domianted board that would otherwise prevail – the CEO and board members will have latitude in the selection process to offset whatever benefits are created by exogenously imposed "independence." This statement emphasizes the distinction between "policies that will affect the bargaining process and those that will not." Hermalin and Weisbach argue that as long as the bargaining process between the CEO and the board is itself not affected by the reforms, then board monitoring will be little affected. In this sense, whether the new exchange listing rules on board structure has actually affected the "bargaining process" is an important, unanswered empirical question. Second, cristics are quick to point out that the new board rules may forced firms to adopt suboptimal board structures and thus, hurt shareholders. My evidence confirms that at least some of the intended benefits of the new exchange listing rules are realized and its effects have not been all negative. To the extent that the passage of SOX placed increased pressure on stock exchanges to issue stricter corporate governance rules than they would do otherwise, this is also an indirect benefit of SOX.

The rest of the paper is organized as follows. Section 2 describes the empirical methodology. Section 3 describes the data, sample and variables. Section 4 reports the empirical results. Section 5 reports robustness checks. Section 6 examines performance changes around turnover events and stock market reactions to forced CEO turnover announcements. Section 7 concludes.

2. Empirical Methodology

2.1. Difference-in-Difference (DD) specification

To measure the treatment effect of the new exchange listing rules on sensitivity of forced CEO turnover to firm performance, a challenge is to establish a counterfactual, i.e. what would the pattern of forced CEO turnover be in the absence of the new listing rules in noncompliant firms. One simple approach is to use the pre-SOX period as the counterfactual. We can simply compare the turnover-performance sensitivity in pre- and post-SOX period and see if there are any differences. The strong assumption behind this approach is that a board's CEO firing decision does not change over time for reasons other than change in board structure. However, this is probably violated in my sample period because the passage of SOX and the issuance of new exchange listing rules are themselves prompted by high-profile corporate scandals in the U.S. that shook investor confidence in 2001 and 2002. The broader shift in public attitude toward corporate governance issues in U.S. publicly listed firms around these events could have affected forced CEO turnover in all firms even without the implementation of the new exchange listing rules. To control for this temporal shift, I therefore adopt a difference-in-difference (DD) approach. I use firms that were previously in compliance with the new exchange listing rules as control firms and identify the effect of board structure on forced CEO turnover by the difference in increase in sensitivity of forced CEO turnover to firm performance from before to after SOX between the treatment and control firms. Here, the change in turnover-performance sensitivity in control firms is used to benchmark a common time trend.

To implement this DD approach, I estimate variants of the following logit regression:

$$\begin{aligned} P(Y_{i,t+1}=1) \quad = \quad & F(\alpha_0 + \alpha_1 \times TREAT_{ij} + \alpha_{12} \times TREAT_{ij} \times POST_{ijt} \\ & + \beta_0 \times RET_{i,t} + \beta_1 \times TREAT_{ij} \times RET_{it} \\ & + \beta_2 \times POST_{ijt} \times RET_{it} \\ & + \beta_{12} \times TREAT_{ij} \times POST_{ijt} \times RET_{it} \\ & + \gamma_1 \times X_{it} + \gamma_2 \times \delta_t + \gamma_2 \times \delta_{\mathrm{k}} + \varepsilon_{it}) \end{aligned} \tag{1.1}$$

where i indexes firm, j indexes the three exchange listing rules being studied, k indexes industry and t indexes year. The dependent variable $Y_{i,t+1}$ equals to one if there is a forced CEO turnover in year $t+1$ in firm i and equals zero otherwise. $TREAT_{ij}$ equals to one if firm i was noncompliant with exchange listing rule j in year 2001 and zero otherwise. For treatment firms, $POST_{ijt}$ equals to one if firm i becomes compliant with exchange listing rule j before or in year t and zero otherwise. For control firms, $POST_{ijt}$ equals to one when t is after 2003 (inclusive) and equals zero otherwise. Firm performance, RET_{it}, is measured either by market-adjusted stock return or industry-adjusted change in ROA. X_{it} is a vector of control variables. δ_t is a vector of year fixed effects and δ_k a vector of industry fixed effects based on the Fama-French 48 industry classification.

In Equation (1.1), α_1 measures the difference in rate of CEO turnover between the treatment and control firms in the absence of the treatment, where treatment refers to the change in board structure imposed by the particular new exchange listing rule. Similarly, β_1 measures the difference in sensitivity of CEO turnover to firm performance between the treatment and control firms in the absence of the treatment. β_2 captures change in sensitivity of CEO turnover to firm performance from pre- to post-SOX period. The effect of the new exchange listing rule on forced CEO turnover is captured by α_{12} and β_{12}, where α_{12} captures the differential effect on the *rate* of CEO turnover and β_{12} captures the differential effect on *sensitivity* of CEO turnover to firm performance. If the new exchange listing rule leads to an increase in the rate of CEO turnover in treatment firms, then I expect to observe a positive and statistically significant α_{12}. Similarly, if the new exchange listing rule leads to an increase in the sensitivity of CEO turnover to firm performance, I expect that estimate for β_{12} to be negative and statistically significant.

For the time being, I only consider the treatment effect of a single listing rule. However, some firms were noncompliant with more than one new exchange listing rules before SOX. To better isolate the treatment effect due to each listing rule when a firm was noncompliant with more than one new exchange listing rules, I estimate specifications in which I simultaneously include the treatment effects of all three listing rules that I study in this paper. This is done by adding two new sets of terms that are the same as those between coefficients α_1 and β_{12} in Equation (1.1) except I replace $TREAT_{ij}$ and $POST_{ijt}$ by indicator variables that correspond to each of the other two listing rules. The treatment effect of individual listing rule is identified by cross sectional and time series variations in compliance with the three listing rules.

2.2. Propensity-Score Matching

The difference-in-difference approach allows for heterogeneity in treatment and control firms, which in my case means that treatment and control firms can have different sensitivities of forced CEO turnover to firm performance both before and after SOX. However, the difference-in-difference effect is identified by assuming a similar temporal trend in the turnover-performance relation in the period from before to after SOX in both the treatment and control firms in the absence of treatment, again the treatment here refers to the change in board structure imposed by the new exchange listing rule. To the extent that firm characteristics that determine the endogenous choice of board structure before SOX also affect the time trend, the difference-in-difference estimates can be biased due to difference in the time trend in the treatment and control firms. To mitigate this bias, I match treatment and control firms along a variety of firm characteristics that are suggested by recent theories of corporate boards influence the choice of board structure. Specifically, for each of the three new exchange listing rules I study, I estimate a probit model to predict the likelihood that a firm was noncompliant with the rule in 2001 using data before 2001. The estimated likelihood is called the propensity score. I then match each treatment firm in year 2001 to one or more control firms that satisfy the following conditions: (1) the control firms are in the same Fama-French 48 industry group as the treatment firm; (2) the propensity scores of the control firms fall within a predefined neighborhood of the propensity score of the treatment firm. This is known as *Radius Matching* in the propensity score matching literature. The choice of the radius is based on a tradeoff between two considerations. On the one hand, a smaller radius increases the quality of the match. On the other hand, it also increases the probability that a match will not be found. For my main analysis, I choose a radius (i.e. neighborhood) of 0.10. In unreported robustness checks, I also try radius of 0.15 and found qualitatively similar results. The matched treatment firms and control firms are then pooled together to form the matched sample for the particular new exchange listing rule. This process is repeated for all three new exchange listing rules I study to obtain three matched samples. One limitation of this matching procedure is that I can only match firms on observable characteristics, thus treatment firms may still differ from control firms on some unobservable characteristics. However, I believe that performing a matching procedure is still better than not doing it because this procedure at least helps to mitigate biases due to observable differences between treatment and control firms, which linear controls may be unable to fully adjust for in a multivariate regression framework.

3. Data, sample and variables

3.1. Data and sample

In this section, I discuss the construction of the full sample, the propensity score matching procedure and the construction of the matched sample for each of the three new exchange listing rules I study.

3.1.1. Construction of full sample

I obtain my CEO turnover sample from the Execucomp database from 1996 to 2008. This database contains information on annual compensation for up to five top executives in firms in the S&P 1500 index currently or from 1992 onward. The database allows me to track a firm's CEO identity over time and thus, identify a CEO turnover when there is a change in CEO from one fiscal year to another. Since I associate each CEO turnover with the year prior to the turnover announcement, my sample period is from 1996 to 2007, where year 1996 is the first year that boards of directors data is available from RiskMetrics (formerly IRRC) and year 2007 is the last year I have information about CEO turnovers announced in the following year. This CEO turnover sample is then combined with the no CEO turnover years of these firms to form a panel data from 1996 to 2007. I then merge this data with the boards of directors data from RiskMetrics. I use the boards of directors data reported at the last meeting date of each fiscal year to represent board characteristics prior to a CEO turnover that occurs in the following year. The information that is most important to me is whether a director is independent and which committees she serves on. Lastly, I add stock return data from the CRSP database and firm financial data from Compustat to data.

To ensure that my results are not driven by firms entering and exiting the sample around the issuance of the new exchange listing rules, I require that all sample firms to (i) have non-missing director data on RiskMetrics in fiscal year 2001 and (ii) be listed on NYSE or Nasdaq from 2001 until at least 2004. This reduces my final sample to 14,151 firm year observations from 1996 to 2007, which are associated with 1,280 firms in existence in 2001. Since not all firms enter the sample in 1996 and exit the sample in 2007 and also some variables may have missing values in certain firm-years, there are some variations in the number of firms with data available across the sample period. For each of the three new listing rules I study, I divide the 1,280 sample firms in existence in 2001 into a treatment group and a control group based on whether they were in compliance with that particular new listing rule in 2001, where the treatment group consists of firms that were noncompliant with that new exchange listing rule in year 2001 and the control group includes the rest of the firms.

I use board structure in fiscal year 2001 to classify firms into treatment and control groups for the following reasons. According to the NYSE and Nasdaq, firms are required to comply with the new exchange rules during their first annual meeting after January 15, 2004, but no later than October 31, 2004. For firms with classified boards, the deadline for compliance is the second annual meeting after January 15, 2004, but no later than December 31, 2005. However, both anecdotal and time series evidence suggest that firms began to make board structure changes as early as the dates that the NYSE and Nasdaq proposals are first made public in 2002[7] and many firms became compliant with the new exchange rules between 2002 and 2003 – before the 2004 deadlines. For example, Chhaochharia and Grinstein (2009) find that the largest increase in the percentage of firms having a majority of independent directors occurs between 2002 and 2003. Thus, they use a firm's board structure in year 2002 to determine which firms are affected by the new exchange rules. However, I use year 2001 for this purpose for two reasons. First, board data in fiscal year 2001 represent the most recent board structures that were clearly not influenced by the new listing rules since early compliance could occur in fiscal year 2002 for firms with fiscal year-end months after August. Second, even for board structure changes in year 2002 which occur prior to when the new exchange proposals were publicly announced, they are likely to be affected by the major corporate scandals and heightened public concerns about corporate governance quality that led to the passage of SOX and the new exchange rule proposals in 2001 and 2002. Hence, I believe that board structure in fiscal year 2001 is more representative than that in fiscal year 2002 of the equilibrium board structure over the years prior to the implementation of the new exchange rules.[8]

3.1.2. Estimating propensity scores

As I discussed in Section 2.2, I match treatment and control firms by industry and propensity score before conducting the difference-in-difference analysis to mitigate potential bias due to differential temporal trend. In this section, I discuss the specification of the propensity

[7] NYSE and Nasdaq sent their proposals for new exchange listing requirements to SEC in August 2002 and October 2002 respectively.

[8] As a robustness check, I also repeat my analysis on a sample that uses board structure on the last meeting date of fiscal year 2002 to classify firms into treatment and control groups. The results are qualitatively unchanged.

score models and report the coefficient estimates and a measure of matching success for these models.

To predict membership in the treatment group defined by the new exchange listing rule that requires firms to have a majority of independent directors on the board, I estimate a probit model where the dependent variable is an indicator for whether a firm had a majority of independent directors on the board in year 2001. The explanatory variables are selected to cover a wide range of firm and governance characteristics. I include firm size measured by log of total assets, growth opportunities measured by Tobin's Q, fundamental risk measured by volatility of earnings, and a list of corporate governance variables which include antitakeover provisions (i.e. the GIM index), an indicator variable for CEO-Chairman duality, an indicator for dual class firms, an indicator for the presence of a non-employee blockholder on the board, the percentage of voting power held by inside and gray directors (including the CEO), an indicator for founder and heir CEOs, log of CEO tenure, and CEO age. A non-employee blockholder is defined as a non-employee director who holds more than 1% of a firm's voting power. The non-employee blockholder indicator equals to one if at least one non-employee blockholder is on the board. The founder and heir information is hand-collected from an extensive search for CEO biographic information on firms' proxy statements, firms' websites and other electronic news sources, such as *Factiva*, etc.

To predict membership in the treatment group defined by the new exchange listing rule that requires firms to have a fully independent nominating committee or a fully independent audit committee, I add to the above probit specification the fraction of independent directors on the board as an additional explanatory variable. The reasoning is that, with more independent directors on a board, the board is more likely to have a fully independent nominating or audit committee because more independent directors are available to sit on these committees and the presence of a larger fraction of independent directors on the board are also likely to give independent directors greater bargaining power to push for more independent nominating or audit committees.

Table 1.1 reports the coefficient estimates of the three probit models specified above. They are estimated using data from 1996 to 2001, i.e. the time period before the assignment of firms to treatment and control groups. Column 1 reports coefficient estimates for the probit model used to predict membership in treatment group defined by the rule requiring a majority of independent directors on the board. I find that most of the coefficient estimates are statistically significant at conventional levels, suggesting that the model captures important characteristics that are systematically associated with membership in the treatment group. Although a couple of variables are statistically insignificant, I still include them because each of them captures a unique firm or governance characteristic. For example, earnings volatility captures a firm's fundamental risk. The model has a concordant rate of 80.3%, meaning that among the total number of distinct pairs with one case being a treatment firm and the other case being a control firm, the treatment firm has a higher predicted mean score than the control firm 80.3% of the time.

Similarly, column 2 reports coefficient estimates for the probit model used to predict membership in the treatment group defined by the rule requiring a fully independent nominating committee. I find that except for two variables, namely earnings volatility and the voting power of inside and gray directors, all the other predictors are statistically significant. As expected, the fraction of independent directors is negatively associated the probability of being in the treatment group. The model has a 79% concordant rate. Column 3 reports estimates for the probit model used to predict membership in the treatment group defined by the rule requiring a fully independent audit committee. Again, the coefficient on fraction of independent directors is negative and statistically significant. The model has a 74.7% concordant rate.

I also tried alternative specifications in which I include additional variables such as board size, firm age, etc. in the probit models. However, the concordance rate is not improved. Hence, the models reported in Table 1.1 seem to be the most parsimonious specifications for the

Table 1.1: Coefficient estimates of the propensity score models

The table reports the coefficient estimates of the probit models used to predict membership in the treatment group defined by the new listing rules on board independence (Column 1) and on nominating committee independence (Column 2) and on audit committee independence (Column 3) respectively. The dependent variable in each column is, from columns 1 to 3, *Treat1*, *Treat2* and *Treat3*, respectively. All variables are defined in Appendix A. The sample consists of all firm years from 1996 to 2001in my full sample. The numbers in parentheses are p-values and ***, **, and * denote significance at the 1, 5 and 10 percent levels, respectively. The percent concordant is defined as the percent of distinct pairs of observations with one case having a positive response (for example *Treat1*=1) and the other having a negative response (for example *Treat1*=0) such that observation with the negative response has a lower predicted mean score than the observation with the positive response.

VARIABLES	Predict for insider-dominated board	Predict for non-independent Nominating Committee	Predict for non-independent audit committee
	(1)	(2)	(3)
Log of total assets	-0.090***	-0.095***	0.093***
	(0.000)	(0.000)	(0.000)
Tobin's Q	-0.014	0.032**	0.025*
	(0.312)	(0.035)	(0.058)
Earnings volatility	0.228	0.117	0.043
	(0.519)	(0.735)	(0.450)
Independent directors (%)		-3.016***	-2.904***
		(0.000)	(0.000)
G-index	-0.055***	-0.032***	0.029***
	(0.000)	(0.000)	(0.000)
Dual class firm	-0.062	0.268***	-0.052
	(0.472)	(0.004)	(0.526)
CEO-Chairman	-0.378***	-0.163***	-0.043
	(0.000)	(0.001)	(0.372)
Insider-linked voting	0.015***	-0.001	-0.002
	(0.000)	(0.772)	(0.128)
Nonemployee blockholders	0.150***	0.230***	0.268***
	(0.004)	(0.000)	(0.000)
Log CEO tenure	0.203***	0.174***	0.034
	(0.000)	(0.000)	(0.281)
Founder-Heir CEO	0.238***	0.278***	-0.190***
	(0.000)	(0.000)	(0.002)
CEO age	0.014***	-0.014***	-0.001
	(0.000)	(0.000)	(0.716)
Constant	-1.200	4.032	-0.185
	(0.909)	(0.394)	(0.983)
Observations	4871	4871	4871
Chi sq	988.4	1260.5	869.0
Pseudo R-squared	0.184	0.228	0.163
Percent concordant	80.3	79.0	74.7

concordance rate reported. Although imperfect, the concordance rates suggest that these models do a reasonable job of predicting membership in the treatment groups.

3.1.3. Construction of matched samples

Using a pre-specified radius of 0.10, I obtain three matched samples based on propensity score matching – one for each of the three new listing rules. For treatment and control firms defined by the rule requiring a majority of independent directors on the board, I find a total of 789 matched firms in year 2001, among which 188 are treatment firms and 601 are control firms. I then add time series data to these 789 firms to obtain a final sample of 8,782 firm-year observations between 1996 and 2007. These firms span 36 of the Fama-French (1997) 48 industry groups, so they represent a wide range of industries. For treatment and control firms defined by the rule requiring a fully independent nominating committee, I find a total of 758 matched firms in 2001, among which 453 are treatment firms and 305 are control firms. I then add time series data to these 758 firms and obtain a final sample of 8,461 firm-year observations. These firms span 38 of the Fama-French 48 industry groups. Lastly, for treatment and control firms defined by the rule requiring a fully independent audit committee, I find a total of 847 matched firms in 2001, among which 250 are treatment firms and 597 are control firms. I then add time series data to these 847 firms and obtain a final sample of 9,441 firm-year observations. These firms span 34 of the Fama-French 48 industry groups.

3.2. Variables

In this section, I discuss the construction of dependent and independent variables that are used in estimating Equation (1.1). For my main analysis, the dependent variable is an indicator for forced CEO turnover. For robustness check, I also define an alternative dependent variable which I call involuntary CEO turnover. Both variables are defined in this section. The sample period of this paper sees an increasing role played by government in corporate governance matters of firms. A significantly larger number of violation-related CEO turnovers happened during this period due to Security and Exchange Commission (SEC) and Department of Justice (DOJ) investigation into option backdating, earnings management and other inappropriate business practices. In Section 3.2.3, I discuss this special type of forced CEO turnover and explain the reason for excluding them from the regression analysis.

3.2.1. Forced CEO turnover

For my analysis, I am mainly interested in CEO turnovers that are due to the board's disciplinary actions. However, many CEO turnovers occur for reasons like retirements, major health problems, deaths, departures for more attractive positions, etc. Consequently, I classify CEO turnovers into forced and voluntary following the method used by Parrino (1997). This method has been commonly used in recent CEO turnover studies (e.g., Huson, Malatesta and Parrino, 2004; Hazarika, Karpoff and Nahata, 2009; Jenter and Kanaan, 2010). Specifically, a CEO turnover is classified as forced if (i) news articles mention that a CEO was fired, forced out or left due to unspecified policy differences; (ii) the CEO is under the age of 60 and the news did not mention death, poor health, or the acceptance of another position (within the firm or elsewhere) as the reason for the departure; (iii) a reported CEO retirement is not announced at least six months in advance. For this third group, I check a wider range of news to make sure that no other articles suggest that the turnover is voluntary in nature. These CEO turnovers are reclassified as voluntary if the incumbent takes a comparable position elsewhere or departs for reasons previously undisclosed that are unrelated to firm activities. The remaining CEO turnovers are classified as voluntary.

3.2.2. Involuntary CEO turnover

The classification method above relies on news reports to classify CEO turnover types. If, in the post-SOX period, reporters become more aggressive at seeking out the true reason for a

CEO turnover in non-compliant firms than in compliant firms, or if non-compliant firms are more candid about reasons of CEO turnovers than compliant firms in the post-SOX period, then the use of this classification could introduce a bias into my results. Thus, to check the robustness of my results, I introduce an alternative classification method which does not rely on news reports to separate CEO turnovers into what I call involuntary and voluntary turnovers. Specifically, I define a CEO turnover as involuntary if (i) the departing CEO leaves office before the age of 65 (ii) the turnover is unrelated to interim CEOs, co-CEOs, poor health, death, mergers and spin-offs and (iii) the departing CEO does not take a CEO position in another S&P 1500 firm within one year of the initial CEO turnover announcement. The rationale behind this classification is that CEOs usually retire at the age of 65, so after excluding CEO turnovers that are related to interim CEOs, co-CEOs, poor health, death, mergers and spin-offs, any CEO who leaves office before 65 and does not take an equivalent or better position in another firm is likely to have done so involuntarily. The advantage of this classification over the forced CEO turnover classification in Parrino (1997) is that it is less restrictive, involves less manual coding, and does not rely on news reports. Hence, it can capture a larger proportion of the actual forced CEO turnovers and is less prone to systematic human errors than is the forced CEO turnover classification. At the same time, the disadvantage of the involuntary turnover definition is that it is more likely to include some voluntary CEO turnovers than is the forced CEO turnover definition. The purpose of this new classification is to provide a robustness check on the results based on forced CEO turnovers.

3.2.3. Violation-related CEO turnover

The years following the passage of SOX have seen some high-profile investigations by regulators and prosecutors into earnings manipulation, option backdating and other inappropriate business practices in U.S. public firms. Under SEC pressure, many firms also conducted their own internal investigations into these matters. These investigations have resulted in a number of firings of top executives including CEOs. Since these CEO turnovers decisions were made under outside pressure, they may not reflect a change in board monitoring. Furthermore, since the main reason for removal in these cases is violation not poor firm performance, including these forced CEO turnovers in my analysis can potentially lower the estimated sensitivity of forced CEO turnover to firm performance. For example, the resignation of Hewlett-Packard Co.'s CEO Mark Hurd in 2010 was requested by its board for violation of the company's business standards. It occurred at a time when H-P's stock outperformed the broad market by 101% over his five-year tenure. Obviously, trying to explain this forced CEO turnover by firm performance will be seriously misleading. If these violation-related CEO turnovers are evenly distributed among the treatment and control firms, then including them won't affect my inference. However, if they are not evenly distributed, then including them may bias my comparison of turnover-performance sensitivity between treatment and control firms. To the extent that treatment firms were poorly governed before SOX, we could expect to observe a higher percentage of violation-related CEO turnovers in treatment firms than in control firms after SOX. This could make it harder to detect the differential increase in turnover-performance sensitivity in the treatment and control firms. I therefore exclude CEO turnovers that resulted from revealed violations when I conduct my difference-in-difference analysis.

To identify violation-related CEO turnovers, I read news articles around each CEO turnover announcement to see if violation is mentioned as a reason for the CEO's departure. Based on this procedure, I identify 16 forced CEO turnovers as being related to violations, all of which happened in the post-SOX period. Confirming my suspicion, violation-related CEO turnovers account for 25% of the post-SOX forced CEO turnovers in firms that were noncompliant with the rule requiring a majority independent board in the pre-SOX period and only 8% of the post-SOX forced CEO turnovers in firms that were compliant with the rule in the pre-SOX period.

3.2.3. Firm performance measures

The stock performance measure I use is market-adjusted stock return. It is calculated as the firm's monthly stock return minus the return on CRSP value-weighted market index cumulated over the 12-month period that ends one month before the CEO turnover announcement for firm-years associated with CEO turnovers and over the fiscal year for firm-years associated with no CEO turnover. This measure is chosen because recent CEO turnover studies find that only highly visible market benchmarks, such as the value-weighted S&P 500 stock return, are taken into account in CEO turnover decisions (see Jenter and Kanaan, 2010; Kaplan and Minton, 2011). Consistent with these studies, in unreported results, I find that industry stock performance adjusted for the market is significantly negatively related to the likelihood of forced CEO turnover in my sample. This finding suggests that the industry component of stock performance is not completely filtered out when boards of directors make CEO retention decisions. Kaplan and Minton (2011) interpret this evidence as suggesting that boards of directors perform the additional role of punishing CEOs for industry-wide underperformance, similar to what happened in the takeover markets of the 1980s.

The accounting performance measure I use is industry-adjusted ROA. ROA is defined as the annual earnings before interests and taxes (EBIT) divided by lagged total assets. The industry-adjusted ROA is calculated as the change in a firm's ROA from previous year to current year minus the median change in ROA in the firm's Fama and French 48 industry over the annual period that best reflect the information available to the board when the CEO turnover decision is made. If the CEO turnover is announced in the first half of a fiscal year, then change in ROA is calculated over the fiscal year prior to the CEO turnover announcement. If the CEO turnover is announced in the second half of a fiscal year, then change is ROA is calculated over the year the CEO turnover is announced. For brevity, I invariably call the annual period over which the market-adjusted stock return or change in ROA is measured the year prior to CEO turnover in the paper.

3.2.4. Treatment and post dummies

For each of the three new exchange listing rules I study, a firm is assigned to a treatment group if it was noncompliant with the listing rule in year 2001 and to a control group otherwise. To indicate membership in the treatment group associated with each of the three listing rules, I define three indicator variables *Treat1*, *Treat2*, and *Treat3* where *Treat1* equals 1 if a firm was noncompliant with the rule requiring a majority of independent directors on the board in 2001 and 0 otherwise, *Treat2* equals to 1 if a firm was noncompliant with the rule requiring a fully independent nominating committee in 2001 and 0 otherwise, and *Treat3* equals to 1 if a firm was noncompliant with the rule requiring a fully independent audit committee in 2001 and 0 otherwise. Corresponding to each treatment dummy, I define an indicator variable for the period after the treatment, i.e. the period after the firm complied with the particular listing rule. For a treatment firm, the indicator variable equals to 0 in the years before the firm complied with the rule and equals to 1 in the years after the firm complied with the rule. For a control firm, I use year 2003 as the breaking year such that the indicator variable equals to 1 after 2003 (inclusive) and equals to 0 before 2003. As I discussed earlier, although noncompliant firms are required to comply with the new exchange listing rules in year 2004 and 2005, most firm complied with the new listing rules between 2002 and 2003. I denote the three indicator variables by *Post1*, *Post2*, and *Post3* where *Post1* denotes the period after a firm complied with the rule requiring a majority of independent directors on the board as so on. For brevity, I use the term pre-SOX to refer to the period before compliance and post-SOX to refer to the period after compliance in this paper.

3.2.5. Control variables

In this section, I introduce the control variables in Equation (1.1). They are included to control for any remaining differences in firm and governance characteristics between treatment and control firms and changes in these variables over time. Prior studies show that firm size and firm risk are positively related to the likelihood of CEO turnover (Huson, Parrino, & Starks, 2001; Hazarika, Karpoff, & Nahata, 2009). So I control for firm size as measured by the natural log of sales in millions of dollars and firm risk as measured by the standard deviation of a firm's monthly stock returns over the 12 months before CEO turnover. Denis, Denis, & Sarin (1997) argue that board's monitoring of CEO is affected by a firm's ownership structure so I control for percentage of voting power held by the CEO and a dummy for the presence of non-employee blockholders owning 1% or more of the shares on the board. Goyal and Park (2002) find that CEOs who are chairmen of the board are less likely to be removed and their removal is less sensitivity to firm performance so I control for a dummy for CEO-Chairman duality. CEOs who are founders or who are from the founding family usually have large influence over the board and also tend to have large ownership in the firm, which may affect the likelihood of CEO turnover and turnover-performance sensitivity (Huson, Parrino, & Starks, 2001). Hence, I include a dummy variable, *Founder-heir CEO*, which equals to 1 if the CEO is a member of the founding family and 0 otherwise. These CEOs are identified by reading firm's proxy statements and new articles and through extensive search over the internet. Dual-class firms tend to have higher agency costs (Masulis, Wang, & Xie, 2009) which may affect CEO turnover decisions so I include a dummy for dual-class firms. Lastly, I control for CEO age and CEO tenure measured by the natural log of one plus CEO tenure in years (Brickley, 2003; Goyal & Park, 2002). I also tried specifications which include board size, industry competition, busy board, etc. However, they are insignificant in most specifications so I choose not to include them in the tables I report.

3.3. Descriptive statistics

Table 1.2 reports the summary statistics of firms in my full sample in year 2001, which is the year I separate firms into treatment and control groups. The three treatment (control) groups in the table correspond to the three new listing rules I study. In this section, I summarize the compliance pattern with the three new listing rules of my sample firms in year 2001 and descrisbe how board structure changed in these firms over time.

Table 1.2: Summary statistics for treatment and control firms in year 2001

The table reports the summary firm and governance characteristics in year 2001 of firms in my full sample. The table is broken down by treatment and control firms defined by the new listing rules that require firms to have a majority-independent board (columns 1-3), a fully independent nominating committee (columns 4-6), and a fully independent audit committee (columns 7-9). The numbers reported are sample means. Column (3), (6) and (9) contain p-values of t-tests for differences in means between the treatment and control group. All variables are defined in Appendix A.

	Majority-independent board			Nominating Committee Independence			Auditing Committee Independence		
	Control	Treatment	p-value for t-test	Control	Treatment	p-value for t-test	Control	Treatment	p-value for t-test
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Total Assets ($ mil)	14362.00	11831.88	0.51	19014.73	11439.03	0.08	11414.17	18955.35	0.09
Sales ($ mil)	6072.98	4391.18	0.10	8506.27	4417.95	0.00	4987.15	7168.88	0.03
Tobin's Q	1.95	2.10	0.09	1.78	2.07	0.00	1.97	2.02	0.57
Earnings volatility	0.06	0.06	0.48	0.05	0.06	0.26	0.06	0.05	0.07
G-index	9.57	8.34	0.00	10.06	8.92	0.00	9.34	9.18	0.34
Dual Class firms	0.08	0.16	0.00	0.05	0.12	0.00	0.09	0.11	0.24
Classified board	0.62	0.54	0.02	0.65	0.58	0.04	0.61	0.60	0.92
CEO-Chairman	0.70	0.59	0.00	0.76	0.64	0.00	0.68	0.66	0.52
Independent directors (%)	0.73	0.40	0.00	0.77	0.59	0.00	0.70	0.53	0.00
Board size	9.59	9.04	0.00	10.03	9.20	0.00	9.31	9.78	0.01
Nonemployee blockholders	0.26	0.46	0.00	0.19	0.36	0.00	0.27	0.41	0.00
Insider-linked voting (%)	5.73	20.32	0.00	4.08	11.59	0.00	7.96	12.32	0.00
Founder-Heir CEO	0.14	0.31	0.00	0.09	0.22	0.00	0.18	0.18	0.89
CEO tenure	6.73	10.12	0.00	5.63	8.42	0.00	7.36	8.01	0.19
CEO age	54.99	56.22	0.03	55.42	55.23	0.66	54.96	56.03	0.03
N	966	314		389	891		885	395	

3.3.1. Distribution of treatment and control firms

Table 1.3 reports the distribution of firms by their compliance status with the three new listing rules. The table shows that the majority of the sample firms already had a majority of independent directors on board in 2001. For example, among the 1280 firms in the sample in 2001, only 25% of them (314 firms) lacked a majority of independent directors on the board. This is consistent with empirical evidence that shows a trend toward more independent boards among U.S. public firms throughout the 1990s (Linck, Netter & Yang, 2008). Also, most firms (69%) already had a fully independent audit committee by 2001. This is mainly because NYSE and Nasdaq began to require listing firms to have fully independent audit committees in 1999 due to a listing rule change in December 1999. However, since the 1999 listing rule has a loose definition for independence and gives exemptions to some firms, we still observe a number of firms that did not have a fully independent audit committee in 2001. In contrast to the high compliance rate with the previous two listing rules, most firms did not have a fully independent nominating committee in 2001. According to the table, 70% of the sample firms (891 firms) were noncompliant with the rule that requires a fully independent nominating committee in 2001.

Table 1.4 shows the distribution of treatment and control firms by listing on NYSE or Nasdaq. As we can see, a considerably higher percentage of NYSE firms than Nasdaq firms already had a majority of independent directors on the board or a fully independent nominating committee in year 2001. However, the percentage of firms that had a fully independent audit committee in 2001 is quite similar among NYSE and Nasdaq firms. Again, this is probably due to the implementation of the 1999 listing rule that required listing firms to have a fully independent audit committee.

3.3.2. Change in board and board committee independence over time

To see how independence of board and board committees changes from before to after the issuance of the new exchange listing rules, I plot the proportion of firms that were in

Table 1.3: Distribution of firms by compliance with the three new exchange listing rules in year 2001

This table reports the frequency count (first row), row percent (parenthesis) and column percent (bracket) of sample firms by compliance status with three new listing rules – the rule on board independence, the rule on nominating committee independence and the rule on auditing committee independence – in year 2001. The sample consists of 1280 firms that have board data available on RiskMetrics in 2001 and have survived from 2001 through 2004 on the ExecuComp database. Firms without nominating (auditing) committee in 2001 are classified as non-compliant with the rule on independent nominating (auditing) committee.

		Nominating Committee Independence		Audit Committee Independence		Total
		Compliant	Non-compliant	Compliant	Non-compliant	
Board Independence	Compliant	374	592	755	211	966
		(39%)	(61%)	(78%)	(22%)	
		[96%]	[66%]	[85%]	[53%]	[75%]
	Non-compliant	15	299	130	184	314
		(5%)	(95%)	(41%)	(59%)	
		[4%]	[34%]	[15%]	[47%]	[25%]
Nominating Committee Independence	Compliant	-	-	328	61	-
		-	-	(84%)	(16%)	-
		-	-	[37%]	[15%]	-
	Non-compliant	-	-	557	334	-
		-	-	(63%)	(37%)	-
		-	-	[63%]	[85%]	-
Total		389	891	885	395	-
		(30%)	(70%)	(69%)	(31%)	-

Table 1.4: Distribution of firms by membership in treatment and control groups and by listing stock exchange in 2011

This table reports the distribution of NYSE and Nasdaq firms in 2001 in the treatment and control groups defined by compliance status with the three new listing rules I study respectively. The sample consists of 1280 firms that have board data available on RiskMetrics in 2001 and have survived from 2001 through 2004 on the ExecuComp database. Column 1 and 2 are for the new listing rule on board independence, Column 2 and 3 are for the new listing rule on nominating committee independence, and Column 5 and 6 are for new listing rule on audit committee independence. Number of firms, row percent and column percent are reported in each cell with the row percents in parentheses and column percents in brackets.

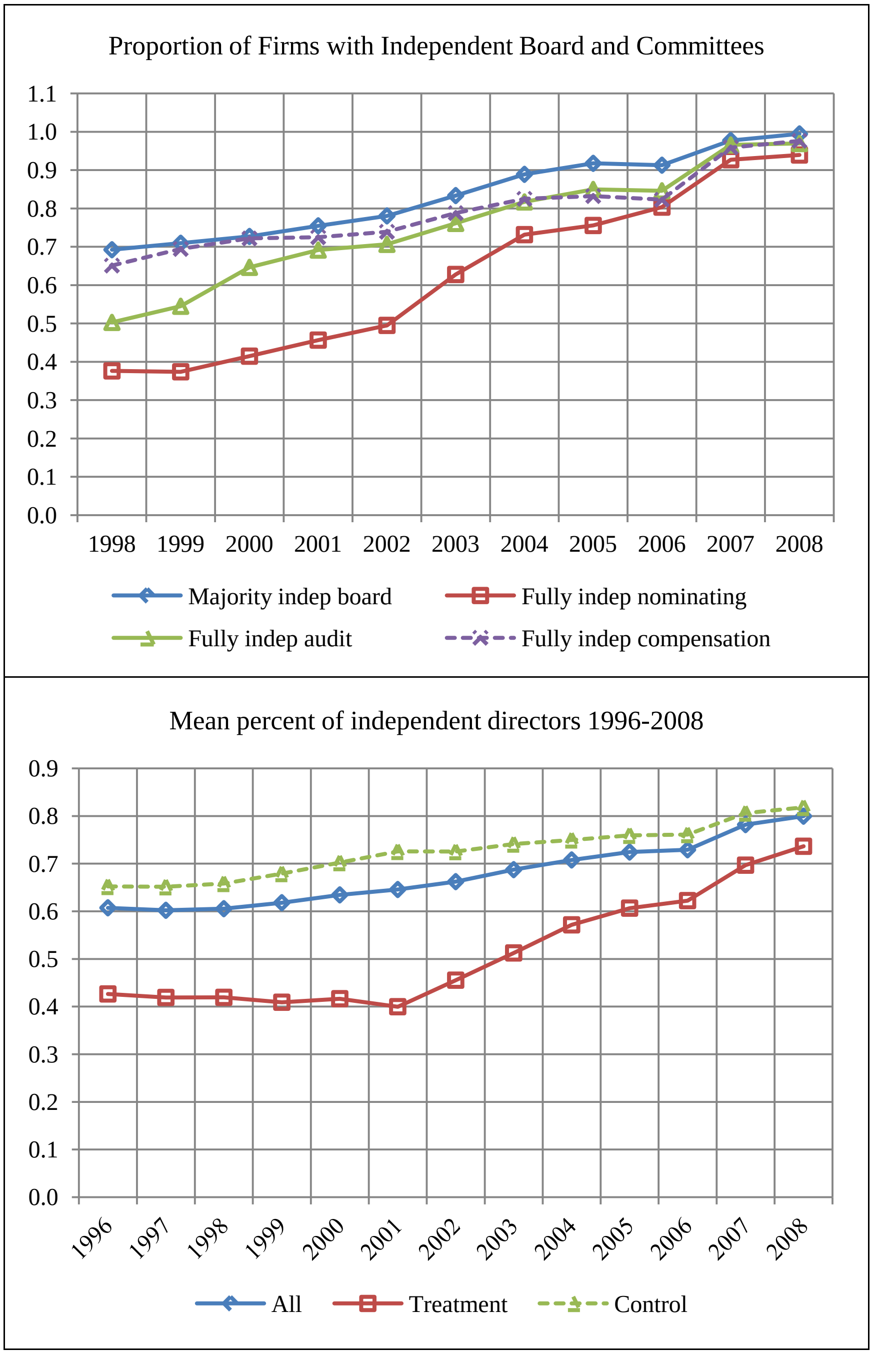
	Board Indep		Nom Com Indep		Audit Com Indep	
	NYSE	Nasdaq	NYSE	Nasdaq	NYSE	Nasdaq
	(1)	(2)	(3)	(4)	(5)	(6)
Control	698	268	323	66	620	265
	(72%)	(28%)	(83%)	(17%)	(70%)	(30%)
	[78%]	[71%]	[36%]	[17%]	[69%]	[70%]
Treatment	202	112	577	314	280	115
	(64%)	(36%)	(65%)	(35%)	(71%)	(29%)
	[22%]	[29%]	[64%]	[83%]	[31%]	[30%]

compliance with the new exchange listing rules over time in the top panel of Figure 1.1. Since board committee data in RiskMetrics is very spotty before year 1998, I begin the plot in year 1998. As we can see, the proportion of firms with a majority of independent directors on the board and with a fully independent nominating, audit or compensation committee increases significantly from 1998 to 2008. There is a 5% increase in the proportion of firms with a majority independent board and a 14% increase in the proportion of firms with a fully independent nominating committee from 2002 to 2003, while the corresponding increase is 2% and 5% respectively from 2000 to 2001. As for the proportion of firms with a fully independent audit committee, the largest increase (11%) occurs between 1999 and 2000 and the next largest increase (5%) occurs between 2002 and 2003. The first increase reflects the change in NYSE and Nasdaq listing rules in December 1999 that required all listing firms to have fully independent audit committees. The second largest increase in proportion of firms with a fully independent audit committee occurred between 2002 and 2003. These findings support the use of year 2003 as the breaking year between the pre- and post-SOX period for control firms. In 2005 – the last year for mandatory compliance with all new exchange listing rules, the compliance rate with the board, nominating, audit and compensation committee rules is 92%, 76%, 85% and 83%, respectively. The lack of full compliance in year 2005 is due to two reasons. First, as Chhaochharia and Grinstein (2009) notice, the RiskMetrics definition of independent directors is stricter than that of NYSE and Nasdaq[9]. For example, NYSE and Nasdaq define former employees as independent if three years has passed since their last employment in the firm ended; however, RiskMetrics considers all former employees non-independent. The NYSE and Nasdaq definition of independence also allows for the existence of “insignificant” business relations between independent directors and the firm, while RiskMetrics definition forbids any business relations between the director and the firm. Hence, not all independent directors according to the

[9] Although affected firms should be determined based on the exchange definition, this is not practical because the size of business relations between directors and firms are generally unobservable.

Figure 1.1: Changes in board structure from 1996 to 2008

The sample consists of all firms on ExecuComp and RiskMetrics (formerly IRRC) that have board data for 2001 available on RiskMetrics and survived from 2001 until at least 2004. The data source is the RiskMetrics database. The top panel shows the percentage of sample firms that have majority of independent directors on board, fully independent nominating, audit and compensation committees respectively from 1998 to 2008. The bottom panel shows the mean percentage of independent directors on board from 1996 to 2008 for the full sample and the subsamples of treatment firms and control firms classified by whether a firm had a majority of independent directors on board in 2001.



NYSE and Nadaq definition are independent according to the RiskMetrics definition. Second, under the NYSE and Nasdaq rule, controlled companies are exempted from the requirements for having a majority of independent directors on the board and a fully independent nominating and compensation committee. To the extent that I misclassify some compliant firms or "controlled companies" as treatment firms, this misclassification should bias against my finding significant differential effects of change in board structure on turnover-performance sensitivity in the treatment and control firms.

The bottom panel of Figure 1.1 shows the mean percentage of independent directors from 1996 to 2008 in firms with and without a majority of independent directors on the board in 2001. I observe that, in firms without a majority of independent directors on the board, i.e. treatment firms, the proportion of independent directors is relatively stable over time before 2001 but has increased significantly since 2001. On the other hand, in firms with a majority of independent directors on the board in 2001, the proportion of independent directors only has increased slightly over the entire sample period. This suggests that the new exchange listing rules have a significant effect on board structure change around the passage of SOX.

3.3.3. How noncompliant firms complied with the new exchange listing rules

To get a sense of how noncompliant firms complied with the new exchange listing requirements for board and board committee independence, I report in Tables 1.5 and 1.6 the difference ways in which my sample firms complied with the new exchange listing rules that require listing firms to have a majority of independent directors on the board and to have a fully independent nominating committee. To save space, I only report the pattern of compliance with these two listing rules. They are chosen because, as I show later, they have significant effects on changes in turnover-performance sensitivity in noncompliant firms.

Table 1.5 Panel A reports three ways in which my sample firms that did not have a majority independent board in 2001 met the requirement by 2005. I observe that 92% of the firms became compliant by both adding new directors and dropping existing directors, while 5% of firms became compliant by retaining existing directors and adding new directors and the remaining 3% of firms became compliant by only dropping existing directors. Table 1.5 Panel B reports the mean and median number of different types of directors that arrived, remained or departed the boards of this sample of firms between 2001 and 2005. I observe that these firms add a mean (median) of 2.8 (3) independent directors, 1.2 (1) linked directors and 1.1 (1) employee directors. At the same time, these firms replaced a mean (median) of 1.5 (1) independent directors, 1.7 (1) linked directors and 1.9 (1) employee directors. The mean (median) numbers of independent, linked and employee directors that remained on the board are 2.6 (3), 2.2 (2) and 2.2 (2) respectively. Although the mean number of departures of linked and employee directors is only slightly higher than the mean number of departures of independent directors, the mean number of new independent directors is significantly higher than the mean number of new linked and employee directors. To get a sense of how much of a culture change this means for these firms, observe that the mean (median) number of new independent directors is similar to the mean (median) number of continuing independent directors and larger than the mean (median) number of continuing linked or employee directors. Hence, it is reasonable to expect that these changes in board composition would result in observable changes in board monitoring if independent directors are indeed more effective at monitoring the CEOs than gray and inside directors.

Table 1.6 Panel A summarizes the different ways in which my sample firms that lacked a fully independent nominating committee in 2001 complied with this rule by 2005. Unlike in Table 1.5, I use the term “new” here to refer to directors who were new to the nominating committee and the term “continuing” to refer directors who continued to serve on the nominating committee from 2001 to 2005. These firms fall into two categories: 46% of firms had no nominating committee in 2001 and the rest 54% had a nominating committee that was not fully independent. Firms in the first category complied by appointing independent directors to a newly

Table 1.5: How firms complied with the rule requiring a majority independent board between 2001 and 2005

The sample consists of 174 firms on RiskMetrics database that did not have majority of independent directors on the board in 2001 and survived to 2005. Panel A presents the percentage of firms that complied with the rule in different ways. Panel B presents the number, mean and median number of new arrivals, continuing directors and departures between 2001 and 2005 per firm. The means and medians are calculated over all firms with non-missing data in that category. "New" and "Continuing" are relative to the boards in 2001.

Panel A: Compliance by Arrivals and Departures	
Only arrivals of new directors (%)	5%
Only departures of existing directors (%)	3%
Both arrivals of new directors and departures of existing directors (%)	92%

Panel B: Compliance by Director Types									
	Arrival			Continuing			Departure		
Director type	N	Mean	Median	N	Mean	Median	N	Mean	Median
Independent	163	2.8	3	167	2.6	3	112	1.5	1
Linked	30	1.2	1	138	2.2	2	106	1.7	1
Employee	54	1.1	1	168	2.2	2	97	1.9	1

Table 1.6: How firms complied with the rule requiring a fully independent nominating committee between 2001 and 2005

The sample consists of 693 firms on RiskMetrics database that did not have a fully independent nominating committee in 2001 and survived to 2005. Panel A presents the percentage of firms that complied with the rule in different ways by 2005. Panel B presents the number, mean and median number of new arrivals, continuing directors and departures between 2001 and 2005 per firm. The means and medians are calculated over all firms with non-missing data in that category. "New" "Continuing" and "Departures" are relative to the nominating committee composition in 2001.

Panel A: Compliance by Arrivals and Departures	
New nominating committee (%)	46%
Only departures of existing nominating committee members (%)	3%
Both new arrivals and departures of existing nominating committee members (%)	51%

Panel B: Compliance by Director Type			
	N	Mean	Median
Firms without nominating committee in 2001			
New independent directors	312	3.6	3
Firms with nominating committee in 2001			
New independent directors	359	2.4	2
Continuing directors	319	1.9	2
Departures :			
Independent	276	1.7	1
Linked	210	1.2	1
Employee	169	1.2	1

installed nominating committee. There is a mean (median) arrival of 3.6 (3) independent directors joining the nominating committee in these 312 firms as shown in Panel B of Table 1.6. Firms in the second category complied with the rule in one of two ways: (i) departures of existing committee members (3% of all noncompliant firms) and (ii) arrival of new committee members and departures of existing committee members (51% of all noncompliant firms). As Panel B of Table 1.6 shows, the mean (median) number of new committee members for this sample of noncompliant firms is 3 (3), the mean (median) number of continuing committee members is 1.9 (2) and the mean (median) numbers of departures of independent, linked and employee committee members are 1.7 (1), 1.2 (1) and 1.2 (1) respectively. Again, these noncompliant firms went through economically significant changes in the composition of their nominating committees from 2001 to 2005.

4. Empirical Results

4.1. Incidence and rate of CEO turnover by turnover type and pre- and post-SOX periods

In this section, I describe the CEO turnover sample and provide some preliminary comparisons of the rate of CEO turnover in treatment and control firms in the pre- and post-SOX periods. The results based on the three matched samples are reported in Table 1.7 Panel A, B, and C respectively. Within each panel, I report the incidences and rates of all CEO turnovers, forced CEO turnovers, and involuntary CEO turnovers by treatment and control firms and by pre- and post-SOX periods. The rate of CEO turnover is the annualized rate calculated as the incidence of CEO turnover divided by the total number of firm-year observations in the time period. I exclude CEO turnovers related to interim CEOs, co-CEOs, poor health, death, mergers and spin-offs. The pre- and post-SOX period are defined by the three indicator variables *Post1*, *Post2* and *Post3*.

In panel A of Table 1.7, the treatment and control firms are defined by the new exchange rule requiring a majority of independent directors on the board. I find that, in treatment firms, the rates of all three types of CEO turnover increase noticeably from the pre- to post-SOX period, while, in control firms, the corresponding rates change little. The rate of forced CEO turnover increases from 2.07 percent to 5.06 percent in treatment firms, which is statistically significant at the 0.01 level. Likewise, the rate of involuntary CEO turnover increases from 6.29 percent to 8.58 percent, which is statistically significant at the 0.05 level. When I compare control firms with treatment firms, I find that the rates of all three types of CEO turnover are significantly lower in treatment firms than in control firms during the pre-SOX period; however, these differences between treatment and control firms decrease and become statistically insignificant during the post-SOX period for all CEO turnovers and involuntary CEO turnovers, whereas the rate of forced CEO turnover increases significantly in treatment firms that the rate is significantly higher in treatment firms than in control firms during the post-SOX period. The evidence is consistent with the hypothesis that having a majority of independent directors improves board oversight. In particular, the evidence suggests that board oversight is weaker in treatment firms than in control firms in the pre-SOX period, but the treatment firms catch up with the control firms in the post-SOX period. In fact, treatment firms fire a larger proportion of CEOs than control firms in the post-SOX period, which is probably because bad CEOs, who in the past were entrenched, are being disciplined in the post-SOX period. However, the evidence is only suggestive because I do not know if the increases in rate of CEO turnover are related to poor performance.

In panel B of Table 1.7, I report results for the rule requiring a fully independent nominating committee. I find that the rate of forced CEO turnover increases significantly in treatment firms, while it barely increases in control firms. Also, the rate of involuntary CEO turnover is significantly lower in treatment firms than in control firms during the pre-SOX period, but the difference becomes smaller and statistically insignificant during the post-SOX period.

Table 1.7: Incidence and rate of CEO turnover (1996-2008)

The table shows the incidence and rate of CEO turnover in the matched sample defined by the new exchange rule on majority of independent directors (Panel A), fully independent nominating committees (Panel B) and fully independent audit committees (Panel C) respectively. The pre- and post-SOX periods are defined using firm-specific post-SOX indicators as explained in Section 3.2 and Appendix A. Three definitions of CEO turnovers are used – all CEO turnovers, forced CEO turnovers and involuntary CEO turnovers. Forced CEO turnover is classified using the method in Parrino (1997). A CEO turnover is defined as involuntary if the age of the departing CEO is under 65 and she does not take a CEO position in another S&P 1500 firm within one year of the initial turnover announcement. I compare the mean rate of CEO turnover between the pre- and post-SOX periods within each treatment and control group and use superscripts ***, **, * to denote significance at the 1, 5 and 10 percent levels, respectively, for t-test of difference in the means. I also compare the mean rate of CEO turnover between control and treatment firms within each time period and the p-values for t-test of difference in the means are reported in the last column of the table.

Type of turnover	Period	Incidence		Rate		p-values
		Control (1)	Treatment (2)	Control (3)	Treatment (4)	(3) – (4) (5)
Panel A: Majority of independent directors						
All CEO turnover	Pre-SOX	478	133	11.93	9.84	0.03
	Post-SOX	322	79	11.87	11.11	0.57
Forced CEO turnover	Pre-SOX	132	28	3.29	2.07***	0.01
	Post-SOX	88	36	3.24	5.06	0.04
Involuntary CEO turnover	Pre-SOX	384	85	9.58	6.29**	0.00
	Post-SOX	264	61	9.73	8.58	0.33
Panel B: Fully independent nominating committee						
All CEO turnover	Pre-SOX	263	374	12.66	11.43	0.18
	Post-SOX	173	199	12.95	11.20	0.14
Forced CEO turnover	Pre-SOX	67	90	3.23	2.75*	0.32
	Post-SOX	46	65	3.44	3.66	0.75
Involuntary CEO turnover	Pre-SOX	202	272	9.73	8.31	0.08
	Post-SOX	139	159	10.40	8.95	0.18
Panel C: Fully independent audit committee						
All CEO turnover	Pre-SOX	461	205	11.69	11.02	0.45
	Post-SOX	317	111	11.85	11.53	0.79
Forced CEO turnover	Pre-SOX	111	56	2.82	3.00	0.68
	Post-SOX	93	32	3.48	3.32	0.82
Involuntary CEO turnover	Pre-SOX	348	155	8.83	8.33	0.53
	Post-SOX	256	85	9.57	8.83	0.49

Overall, the evidence suggests that rate of forced and involuntary CEO turnover has increased more in treatment firms than in control firms.

In panel C of Table 1.7, I report results for the rule requiring a fully independent audit committee. I find that the rates of all three types of CEO turnover increase slightly from the pre- to post-SOX period in all firms, but the increases are not significantly different between treatment and control firms. Also, there is no evidence that the rate of CEO turnover is significantly different between the treatment and control firms for any of the three types of CEO turnover.

4.2. Regression analysis on rate of forced CEO turnover

In this section, I carry the univariate analysis in last section one step further by conducting a difference-in-difference analysis of the effect of the new exchange listing rules on rate of forced CEO in a multivariate setting. For this analysis, I purposely leave out the three interaction terms between *Treat*, *Post* and firm performance in Equation (1.1) to evaluate what the effect on the rate of forced CEO turnover would be if the turnover-performance sensitivity did not change. The purpose of this analysis is to gain additional insights into what have changed from before to after SOX not to test the main hypotheses of this paper, which I leave to the next three sections.

The dependent variable in these logit regressions is the indicator for forced CEO turnover and the key explanatory variable is the interaction between *Treat* and *Post*. Since most of the violation-related forced CEO turnovers in my sample happened under outside pressure, it is unclear if it reflects quality of board monitoring. To be conservative, I exclude violation-related forced CEO turnover from this analysis. I control for both stock and accounting performance to make sure that any difference in rate of forced CEO turnover between the treatment and control firms or any change in rate of forced CEO turnover from pre- to post-SOX period is not simply driven by difference or change in firm performance. In addition, I control for a list of other variables that have been shown in prior literature to affect the likelihood of forced CEO turnover.

They include firm size, firm risk, CEO voting power, CEO-Chairman duality, CEO tenure, CEO founder status, the presence of non-employee blockholders on the board and dual class firms. For each of the three new exchange listing rules that I study, I estimate two empirical specifications using the matched sample for that listing rule. In the first specification, I only include the treatment effect related to the listing rule being studied, for example the treatment due to the rule requiring a majority-independent board; however, in the second specification, I also control for the treatment effects related to the other two listing rules. The results of these logit regressions are reported in Table 1.8. Column 1 shows that the rate of forced CEO turnover increased significantly in firms that were forced to adopt a majority of independent directors on the board after SOX relative to firms that already had a majority-independent board before SOX. The coefficient of the interaction between *Treat1* and *Post1* is statistically significant at the 5% level. Besides, the coefficient of *Treat1* is negative and statistically significant at the 10% level, suggesting firms that did not have a majority of independent directors on the board before SOX had a lower rate of forced CEO turnover than firms that did before SOX. In column 2, I also control for whether a firm was compliant with the rules that require a fully independent nominating and auditing committee before SOX. As we can see, the result still holds with the interaction between *Treat1* and *Post1* being positive and still statistically significant (at the 10% level). In columns 3 and 4, I conduct similar analysis but on the rule that requires listing firms to have a fully independent nominating committee. The key coefficient is the interaction between *Treat2* and *Post2*. Although it has a positive sign, it is not statistically significant at conventional levels. In columns 5 and 6, I examine the rule that requires listing firms to have a fully independent audit committee. The key coefficient on the interaction between *Treat3* and *Post3* is not statistically significant either. Overall, when turnover-performance sensitivity in treatment and control firms are forced to be the same, I find a significant increase in rate of forced CEO turnover after SOX in firms that lacked a majority-independent board before SOX. However, no

Table 1.8: The effect of the new exchange listing rules on the rate of forced CEO turnover

The table shows coefficient estimates from logit regressions that examine the treatment effect of three listing rules on the likelihood of forced CEO turnover. The dependent variable is an indicator for forced CEO turnover in year t, while the independent variables are defined in Appendix A and are all measured in year t-1. Columns 1 and 2 are estimated using the matched sample based on the rule requiring a majority-independent board, columns 3 and 4 are estimated using the matched sample based on the rule requiring a fully independent nominating committee, and columns 5 and 6 are estimated using the matched sample based on the rule requiring a fully independent audit committee. I exclude CEOs who were fired for violations. Year and industry fixed effects are included in all columns where industry is defined by Fama and French (1997) 48 industry groups. The numbers in parentheses are robust p-values and ***, **, and * denote significance at the 1, 5 and 10 percent levels, respectively.

	Majority of Independent Directors		Fully independent nominating committee		Fully independent audit committee	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat1	-0.515*	-0.449		-0.415		-0.213
	(0.056)	(0.110)		(0.242)		(0.443)
Post1 × Treat1	0.783**	0.756*		0.787		0.573
	(0.032)	(0.056)		(0.125)		(0.153)
Treat2		-0.259	-0.264	-0.223		-0.222
		(0.158)	(0.154)	(0.235)		(0.222)
Post2 × Treat2		0.119	0.197	0.141		-0.016
		(0.684)	(0.488)	(0.632)		(0.955)
Treat3		0.025		-0.035	0.014	0.103
		(0.904)		(0.877)	(0.939)	(0.594)
Post3 × Treat3		-0.017		-0.142	-0.083	-0.183
		(0.960)		(0.714)	(0.788)	(0.579)
Ret	-2.261***	-2.258***	-1.984***	-1.992***	-2.253***	-2.246***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Change in EBIT	-3.770**	-3.719**	-4.435**	-4.332**	-4.908***	-4.854***
	(0.031)	(0.034)	(0.013)	(0.016)	(0.005)	(0.006)
Log of sales	0.053	0.046	0.045	0.046	0.072	0.060
	(0.354)	(0.424)	(0.441)	(0.439)	(0.198)	(0.283)
Stock volatility	5.256***	5.339***	6.973***	7.029***	4.775***	4.948***
	(0.001)	(0.001)	(0.000)	(0.000)	(0.002)	(0.002)
CEO Age	-0.017	-0.017*	-0.023**	-0.023**	-0.012	-0.013
	(0.107)	(0.099)	(0.036)	(0.043)	(0.213)	(0.200)
Founder-heir CEO	-1.141***	-1.125***	-1.221***	-1.243***	-1.288***	-1.254***
	(0.001)	(0.001)	(0.002)	(0.001)	(0.000)	(0.001)
Log CEO tenure	0.140	0.145	0.201*	0.206*	0.133	0.144
	(0.230)	(0.214)	(0.079)	(0.073)	(0.228)	(0.199)
CEO-Chairman	-0.285*	-0.297*	-0.217	-0.227	-0.356**	-0.373**
	(0.094)	(0.079)	(0.197)	(0.192)	(0.028)	(0.024)
CEO voting	-0.108***	-0.105***	-0.111***	-0.108***	-0.121***	-0.117***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.003)
Non-emp block	0.174	0.193	0.210	0.213	0.247	0.252
	(0.303)	(0.253)	(0.222)	(0.224)	(0.127)	(0.126)
Dual class firm	0.315	0.333	0.346	0.359	0.058	0.091
	(0.245)	(0.219)	(0.249)	(0.233)	(0.838)	(0.753)
Constant	-3.383***	-3.195***	-3.632***	-3.617***	-3.891***	-3.653***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	7089	7089	6890	6890	7703	7703
Chi sq	254.8	257.7	245.6	267.1	268.7	277.0
Pseudo R-squared	0.147	0.148	0.153	0.154	0.157	0.159

such evidence is found in firms that did not have a fully independent nominating or audit committee before SOX. The stronger results in columns 1 and 2 are expected because, as shown in Table 1.2, 95% of firms without a majority-independent board lacked a fully independent nominating committee before SOX, on the other hand, 66% of the firms without a fully independent nominating committee already had a majority-independent board before SOX. To the extent that board and board committee independence are both important for board monitoring, firms without a majority-independent board on average should have poorer governance than firms without a fully independent nominating committee before SOX, and consequently the improvement in corporate governance is expected to be greater after SOX. The results provide some evidence that increases in board and board committee independence lead to improved board monitoring.

However, it is important to note that the results above only provide corroborating not concluding evidence on the main hypotheses of this paper. In particular, lack of evidence of a significant increase in rate of forced CEO turnover after SOX does not mean a lack of improvement in board monitoring. This is because CEOs anticipating more intensive monitoring after SOX may exert more effort after SOX and thus reduce the occurrences of CEO firings that would otherwise prevail after SOX. Therefore, in the next three sections, I estimate the full model of Equation (1.1) to examine the effect of the new listing rules on change in sensitivity of forced CEO turnover to firm performance, which is not affected by change in CEO effort, to test the main hypotheses of this paper.

4.3. Majority-independent board and turnover-performance sensitivity

In this section, I investigate the differential effect of the new exchange rule that requires listing firms to have a majority of independent directors on the board on the sensitivity of forced CEO turnover to firm performance in treatment and control firms. To do this, I estimate the full model in Equation (1.1). The coefficient of most interests to us is β_{12} because it captures the additional effect of the board structure change on *sensitivity* of CEO turnover to firm performance in treatment firms relative to that in control firms. Firm performance is measured by market-adjusted return in columns 1 and 2, while it is measured by industry-adjusted change in ROA in columns 3 and 4. For each performance measure, I estimate two specifications. The first specification only includes the treatment effect of the new listing rule requiring a majority of independent directors on the board. In the second specification, I expand Equation (1.1) to also include treatment effects of the other two listing rules – the rules on nominating and audit committee independence. If the distribution of firms with or without an independent nominating committee or audit committee in 2001 is largely random across the treatment and control firms defined by the rule requiring a majority of independent directors on the board, then estimates from the first specification are still unbiased. However, if the distribution of firms with and without an independent nominating committee or audit committee is correlated with membership in the treatment group, then estimates from the first specification may capture more than one treatment effect. The main purpose of estimating the second specification is to check the robustness of the results in the first specification to controlling for the effects of the other two listing rules.

The results are reported in Table 1.9. As expected, I find a statistically significant negative coefficient on firm performance in all the columns. Furthermore, the coefficient of most interest to us – the coefficient on the triple interaction of *Treat1*, *Post1* and *Performance* – has negative sign (negative sign means that the negative relation between firm performance and forced CEO turnover is strengthened) in all columns and is statistically significant in columns 1, 3, and 4 under two-sided tests and in column 2 under a one-sided test. The evidence suggests that, after controlling for time trends, forced CEO turnover in treatment firms has become more sensitive to firm performance after SOX. Furthermore, this result does not appear to be driven by change in the other two listing rules.

Table 1.9: The effect of the rule requiring a majority independent board on sensitivity of forced CEO turnover to firm performance (1996-2007)

The table shows coefficient estimates from logit regressions where the dependent variable is an indicator for forced CEO turnover in year t. The independent variables are defined in Appendix A and are all measured in year t-1. The sample consists of all observations from 1996 to 2007 in the matched sample defined by the new exchange rule requiring majority of independent directors on boards. I exclude forced CEO turnovers that resulted from punishment for violations because poor firm performance is not the main reason for these turnovers and treatment firms contain a disproportionately higher fraction of such type of forced CEO turnovers. In column 1 and 2, *Performance* is measured by market-adjusted stock returns, while in column 3 and 4 by industry-adjusted change in ROA, both measures are defined in Appendix A. Year and industry fixed effects are included in all columns. Industries are defined by Fama and French (1997) 48 industry groups. The numbers in parentheses are robust p-values and ***, **, and * denote significance at the 1, 5 and 10 percent levels, respectively.

VARIABLES	Market-adjusted return		Industry-adjusted change in ROA	
	(1)	(2)	(3)	(4)
Performance	-2.751***	-3.347***	-7.054***	-10.889***
	(0.000)	(0.000)	(0.000)	(0.001)
Treat1	-0.593	-0.535	-0.483*	-0.484*
	(0.125)	(0.146)	(0.072)	(0.080)
Post1 × Treat1	0.218	0.255	0.378	0.455
	(0.676)	(0.638)	(0.364)	(0.320)
Treat1 × Performance	-0.133	-0.193	3.211	2.261
	(0.877)	(0.823)	(0.510)	(0.653)
Post1 × Performance	1.035	1.199	-6.014**	10.785
	(0.161)	(0.564)	(0.049)	(0.225)
Post1 × Treat1 × Performance	-2.877*	-2.395	-20.858**	-21.516**
	(0.064)	(0.135)	(0.017)	(0.031)
Treat2		0.078		-0.190
		(0.761)		(0.332)
Post2 × Treat2		-0.392		0.053
		(0.293)		(0.862)
Treat2 × Performance		1.392*		6.476*
		(0.052)		(0.064)
Post2 × Performance		2.589**		8.668
		(0.046)		(0.271)
Post2 × Treat2 × Performance		-3.289**		-7.207
		(0.013)		(0.232)
Treat3		-0.218		0.071
		(0.441)		(0.740)
Post3 × Treat3		0.257		-0.128
		(0.557)		(0.739)
Treat3 × Performance		-0.800		-0.232
		(0.263)		(0.954)
Post3 × Performance		-1.299		-21.856**
		(0.511)		(0.031)
Post3 × Treat3 × Performance		1.259		2.563
		(0.407)		(0.755)
Log of sales	0.050	0.051	0.091	0.090
	(0.393)	(0.382)	(0.118)	(0.125)
Stock volatility	4.856***	4.994***	6.864***	6.748***
	(0.004)	(0.002)	(0.000)	(0.000)
CEO Age	-0.017	-0.016	-0.016	-0.017
	(0.122)	(0.137)	(0.152)	(0.125)

Table 1.9, continued

Founder-Heir CEO	-1.341***	-1.305***	-1.394***	-1.397***
	(0.000)	(0.001)	(0.000)	(0.000)
Log CEO tenure	0.143	0.140	0.152	0.166
	(0.229)	(0.243)	(0.203)	(0.169)
CEO-Chairman	-0.313*	-0.321*	-0.351**	-0.374**
	(0.064)	(0.056)	(0.036)	(0.026)
CEO voting power	-0.118***	-0.113***	-0.118***	-0.113***
	(0.002)	(0.002)	(0.002)	(0.002)
Nonemployee blockholders	0.172	0.195	0.256	0.276
	(0.315)	(0.252)	(0.136)	(0.109)
Dual class firm	0.345	0.337	0.283	0.271
	(0.208)	(0.222)	(0.302)	(0.326)
Constant	-3.322***	-3.363***	-3.619***	-3.507***
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	7095	7095	7087	7087
Chi sq	247.0	272.7	245.3	255.6
Pseudo R-squared	0.153	0.160	0.117	0.122

For other variables, I find that the coefficient on *Treat1* is negative in all columns and is statistically significant in columns 3 and 4. The negative sign suggests that the rate of forced CEO turnover is lower in treated firms than in control firms during the pre-SOX period. The sign and statistical significance of the control variables are in general consistent with prior findings and economic intuition. For example, the negative coefficients on stock returns and accounting performance suggest that CEOs in firms with poor performance are more likely to be fired (Warner, Watts, and Wruck, 1988; Weisbach, 1988). The positive coefficient on stock return volatility suggests that CEOs in riskier firms are more likely to be fired, probably because a bad outcome that would trigger a forced CEO turnover is more likely to occur in these firms. Consistent with the view that CEOs who have more power and influence over the board are less likely to be fired, I find that CEOs who are founders or heirs of founders are less likely to be fired, CEOs who have more voting power are less likely to be fired, and CEOs who are chairmen are also less likely to be fired.

To get a sense of the economic significance of the results, I estimate logit regressions that are similar to those in column 1 and 3 of Table 1.9 except I replace year dummies by a post-SOX dummy which equals to one in the post-SOX period and zero otherwise. Based on the parameter estimates of these two models, I compute the implied probability of a CEO being fired when market-adjusted stock return (industry-adjusted change in ROA) is at the 75th percentile of the sample and the 25th percentile of the sample respectively, while holding all other variables at the sample means. I find that when the market-adjusted stock return declines from 24% (the 75th percentile) to -22% (the 25th percentile), the implied annual probability of being fired in treatment firms during the pre-SOX period goes up from 0.47% to 1.67% - a rise of 1.2%. However, in the post-SOX period, the implied probability of being fired goes up from 0.6% to 4.46% - a rise of 3.86%, which is three times the rise in the pre-SOX period. When I measure firm performance by industry-adjusted change in ROA, the implied probability of being fired goes from 1.23% to 1.37% when the accounting performance measure declines from 2.3% (the 75th percentile) to -1.3% (the 25th percentile) in treatment firms in the pre-SOX period – a change of merely 0.14%. In the post-SOX period, the implied probability of being fired goes from 1.19% to 3.11% – a rise of 1.92% – for the same drop in accounting performance. These increases in sensitivity are economically quite significant relative to those in the pre-SOX period. Figure 1.2 illustrates the economic effects using a bar chart.

4.4. Nominating committee independence and turnover-performance sensitivity

In this section, I study the treatment effect of the new listing rule that requires nominating committees to be comprised entirely of independent directors. The sample for this analysis is the matched sample of treatment and control firms as defined by the independent nominating committee requirement, where the treatment firms are matched to control firms in the same industry and with close propensity scores.

In Table 1.10, I find that the coefficient on the interaction between *Treat2* and *Performance* is positive in all the columns and is statistically significant in columns 3 and 4 where firm performance is measured by industry-adjusted change in ROA. The positive sign is

Figure 1.2: Economic effects of the rule requiring a majority of independent directors on the board

The following graphs show the implied probability of a hypothetical CEO in the average firm of the sample being fired when firm performance goes from the 75th percentile of the sample to 25th percentile by pre- and post-SOX period and firm types. The estimates are based on model 1 and 3 in Table 1.8 with year dummies replaced by a post-SOX dummy. Treatment and control firms are defined by the rule that requires a majority dependent board. Firm performance equals to market-adjusted stock return over the year prior to CEO turnover in the top panel and equals to industry-adjusted change in ROA in the bottom panel.

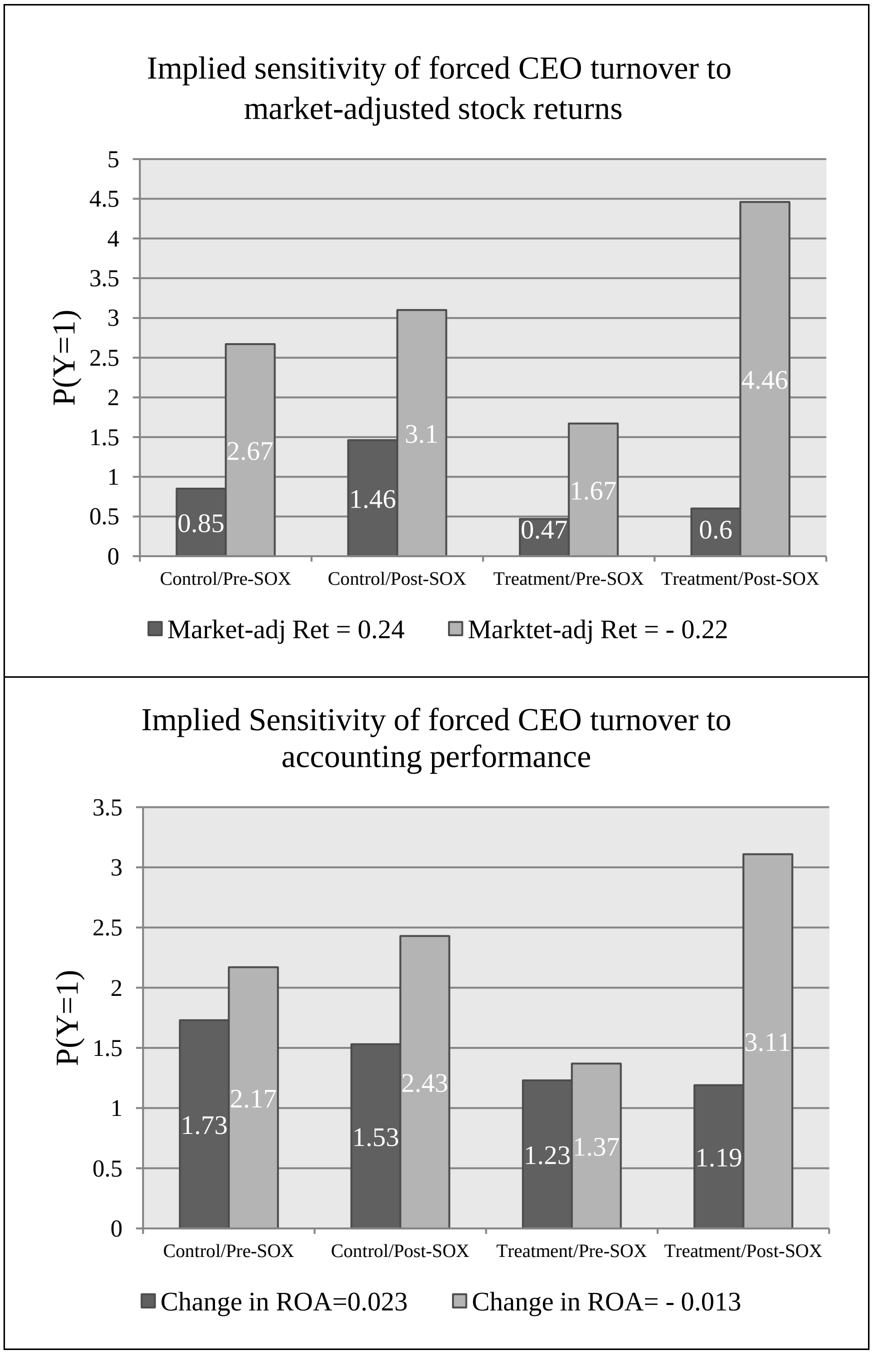


Table 1.10: The effect of the rule requiring a fully independent nominating committee on sensitivity of forced CEO turnover to firm performance (1996-2007)

The table shows the coefficient estimates from logit regressions where the dependent variable is an indicator for forced CEO turnover in year t. The independent variables are defined in Appendix A and are all measured in year t-1. The sample consists of all observations from 1996 to 2007 in the matched sample defined by the new exchange rule on independent nominating committees. I exclude forced CEO turnovers that resulted from punishment for violations because poor firm performance is usually not the main reason for these turnovers and treatment firms contain a disproportionately higher fraction of such type of forced CEO turnovers. In column 1 and 2, *Performance* is measured by market-adjusted stock returns, while in column 3 and 4 by industry-adjusted change in ROA, both measures are defined in Appendix A. Year and industry fixed effects are included in all columns. Industries are defined by Fama and French (1997) 48 industry groups. The numbers in parentheses are robust p-values and ***, **, and * denote significance at the 1, 5 and 10 percent levels, respectively.

VARIABLES	Market-adjusted return		Industry-adjusted change in ROA	
	(1)	(2)	(3)	(4)
Performance	-2.728***	-2.509***	-11.484***	-11.366***
	(0.000)	(0.000)	(0.000)	(0.000)
Treat2	-0.144	-0.019	-0.116	-0.099
	(0.557)	(0.939)	(0.557)	(0.621)
Post2 × Treat2	-0.277	-0.354	-0.017	-0.003
	(0.433)	(0.333)	(0.957)	(0.993)
Treat2 × Performance	0.651	0.892	8.288**	8.384**
	(0.324)	(0.176)	(0.015)	(0.013)
Post2 × Performance	1.677**	2.014	-3.637	6.869
	(0.037)	(0.142)	(0.396)	(0.348)
Post2 × Treat2 × Performance	-3.172***	-3.246***	-13.012**	-9.926*
	(0.006)	(0.005)	(0.028)	(0.089)
Treat1		-0.904		-0.457
		(0.206)		(0.186)
Post1 × Treat1		0.522		0.007
		(0.568)		(0.993)
Treat1 × Performance		-1.029		5.601
		(0.493)		(0.372)
Post1 × Performance		1.096		19.049**
		(0.656)		(0.016)
Post1 × Treat1 × Performance		-2.401		-46.722***
		(0.322)		(0.002)
Treat3		-0.437		-0.041
		(0.225)		(0.866)
Post3 × Treat3		0.377		-0.062
		(0.447)		(0.887)
Treat3 × Performance		-1.336		-1.626
		(0.116)		(0.703)
Post3 × Performance		-1.691		-30.732***
		(0.553)		(0.002)
Post3 × Treat3 × Performance		2.500*		10.159
		(0.084)		(0.269)
Log of sales	0.039	0.054	0.080	0.091
	(0.519)	(0.377)	(0.187)	(0.143)
Stock volatility	6.843***	6.784***	8.124***	8.069***
	(0.000)	(0.000)	(0.000)	(0.000)
CEO Age	-0.023**	-0.023**	-0.023**	-0.022*
	(0.045)	(0.049)	(0.041)	(0.064)

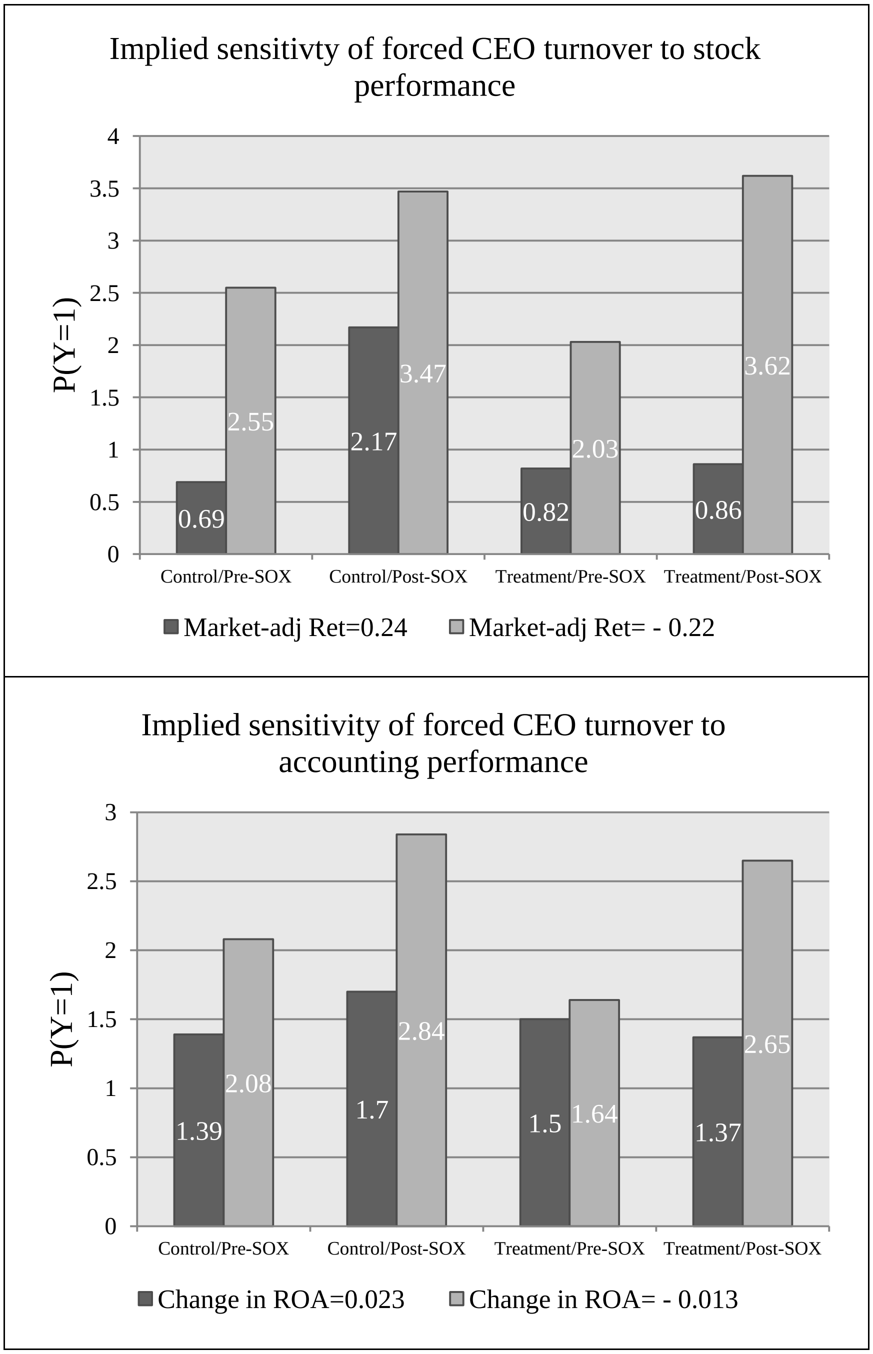
Table 1.10, continued

Founder-Heir CEO	-1.473***	-1.466***	-1.616***	-1.814***
	(0.001)	(0.001)	(0.000)	(0.000)
Log CEO tenure	0.209*	0.208*	0.214*	0.228*
	(0.073)	(0.083)	(0.064)	(0.054)
CEO-Chairman	-0.226	-0.254	-0.244	-0.290*
	(0.179)	(0.144)	(0.141)	(0.093)
CEO voting power	-0.126***	-0.120***	-0.122***	-0.115***
	(0.001)	(0.001)	(0.001)	(0.001)
Nonemployee blockholders	0.206	0.213	0.264	0.287
	(0.230)	(0.230)	(0.129)	(0.109)
Dual class firm	0.343	0.396	0.352	0.318
	(0.257)	(0.198)	(0.238)	(0.309)
Constant	-3.620***	-3.536***	-3.867***	-4.006***
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	6896	6896	6888	6888
Chi sq	247.3	282.3	242.1	274.9
Pseudo R-squared	0.159	0.166	0.133	0.144

consistent with forced CEO turnover being less sensitive to firm performance in treatment firms than in control firms in the pre-SOX period. The coefficient of most interest to us – the coefficient on the triple interaction of *Treat2, Post2* and *Performance* – is negative and statistically significant in all the columns, suggesting a larger increase in sensitivity of forced CEO turnover to stock return and accounting performance in treatment firms than in control firms during the post-SOX period, and this effect is robust to controlling for the potential confounding effects of the other two listing rules. I also find that the coefficient on the interaction of *Treat1*, *Post1* and *Performance* is negative in columns 2 and 4 and is statistically significant in column 4, which is consistent with the findings in Table 1.9. All the control variables have expected signs and most are also statistically significant. Overall, the evidence shows that the requirement for a fully independent nominating committee has significantly increased the sensitivity of forced CEO turnover to firm performance in firms that lacked a nominating committee or lacked a fully independent nominating committee during the pre-SOX period.

Figure 1.3: Economic effects of the rule requiring a fully independent nominating committee

The following graphs show the implied probability of a hypothetical CEO in the average firm of the sample being fired when firm performance goes from the 75th percentile of the sample to 25th percentile by pre- and post-SOX period and firm types. The estimates are based on model 1 and 3 in Table 1.8 with year dummies replaced by a post-SOX dummy. Firm performance equals to market-adjusted stock return over the year prior to CEO turnover in the top panel and equals to industry-adjusted change in ROA in the bottom panel.



To assess the economic significance of the results, I perform similar analysis as I do in last section except this analysis is based on Table 1.9. For the treatment firms in this sample, I find that the implied annual probability of being fired during the pre-SOX period goes up from 0.82% to 2.03% – a rise of 1.21% when the market-adjusted stock return declines from 24% (the 75^{th} percentile of the sample) to -22% (the 25^{th} percentile of the sample); while the corresponding probability of being fired increases from 0.86% to 3.62% in the post-SOX period – a rise of 2.76%. When I measure firm performance by industry-adjusted change in ROA, the implied probability of being fired goes from 1.5% to 1.64% when the accounting performance measure declines from 2.3% (the 75^{th} percentile of the sample) to -1.3% (the 25^{th} percentile of the sample) in the pre-SOX period – a change of merely 0.14%. In the post-SOX period; while in the post-SOX period, the corresponding implied probability of being fired goes from 1.37% to 2.65% – a rise of 1.28%. These increases in sensitivity are also economically significant on relative terms. Figure 1.3 illustrates the economic effects using bar charts.

4.5. Audit and compensation committee independence and turnover-performance sensitivity

In this section, I study the effects of the new exchange listing rules that require publicly listed firms to have a fully independent audit and compensation committee. Unlike change in independence of the overall board and the nominating committee, it is less clear how change in independence of the audit and the compensation committee would directly affect the board's CEO replacement decisions. However, moving towards a fully independent audit committee may have an *indirect* effect on sensitivity of forced CEO turnover to accounting performance. This is because the move may improve the quality of reported accounting numbers. If the board consequently puts higher weight on the firm's accounting performance when making forced CEO turnover decisions than they did before the change, then we can expect to observe an increase in sensitivity of forced CEO turnover to accounting performance. If the effect on quality of reported accounting numbers spills over to the stock market and reduces noises in stock prices, then we may even observe an increase in sensitivity of forced CEO turnover to stock performance following the change. In this section, I test this hypothesis.

There is no strong reason to believe that moving towards a fully independent compensation committee should systematically affect sensitivity of forced CEO turnover to firm performance. Its direct effect should be on CEO compensation. As a matter of fact, Guthrie et al. (2010) do find a significant change in CEO compensation associated with the mandatory change in composition of compensation committee, though the direction of the change is difficult to interpret based on CEO compensation evidence alone. However, I still test the effect of independence of compensation committee on turnover-performance sensitivity here. If I do not find the rule to have a significant effect on turnover-performance sensitivity, it would reassure us that my previous findings are driven by changes in the most relevant aspects of board structure, rather than by some unobservable changes in internal governance that happen to be correlated with the general change in board structure.

The logit regressions in this section are estimated using matched samples constructed by matching industry and propensity score. I have discussed the matched sample for the rule requiring a fully independent audit committee in Section 3.1. Following a similar procedure and propensity score model, I construct a matched sample for the rule requiring a fully independent compensation committee. To save space, the details are not reported. Table 1.11 reports difference-in-difference results on the effect of the mandatory changes in composition of audit and compensation committees on sensitivity of forced CEO turnover to firm performance. The structure of the analysis is similar to that in Tables 1.9 and 1.10. I denote the treatment associated with the rule requiring fully independent compensation committees by *Treat4* and the corresponding firm-specific post-SOX indicator by *Post4*. *Treat4* and *Post4* are defined similarly to *Treat3* and *Post3*. I find that the coefficient on the triple interaction of *Post3 (Post4)*, *Treat3 (Treat4)* and *Performance* is statistically insignificant in all the columns. Hence, there is no

Table 1.11: The effect of the rules requiring a fully independent audit and compensation committee on sensitivity of forced CEO turnover to firm performance (1996-2007)

The table shows coefficient estimates from logit regressions where the dependent variable is an indicator for forced CEO turnover in year t. Column 1 and 3 examine the rule that requires fully independent audit committee, while column 2 and 4 examine the rule that requires fully independent compensation committee. In column 1 and 2, *Performance* is measured by market-adjusted stock returns, while in column 3 and 4 by industry-adjusted change in ROA, both measures are defined in Appendix A. The independent variables are defined in Appendix A and are all measured in year t-1. The sample consists of all observations from 1996 to 2007 in the matched sample defined by the new exchange rule on audit committee independence. I exclude forced CEO turnovers that resulted from punishment for violations because poor firm performance is normally not the main reason for these turnovers and treatment firms contain a disproportionately higher fraction of such type of forced CEO turnovers. Year and industry fixed effects are included in all columns. Industries are defined by Fama and French (1997) 48 industry groups. The numbers in parentheses are robust p-values and ***, **, and * denote significance at the 1, 5 and 10 percent levels, respectively.

VARIABLES	Market-adjusted return		Industry-adjusted change in ROA	
	Audit	Compensation	Audit	Compensation
	(1)	(2)	(3)	(4)
Performance	-2.565***	-2.309***	-7.255***	-7.570***
	(0.000)	(0.000)	(0.000)	(0.001)
Treat3	-0.071		0.088	
	(0.797)		(0.653)	
Post3 × Treat3	-0.132		-0.291	
	(0.773)		(0.412)	
Treat3 × Performance	-0.244		0.811	
	(0.736)		(0.828)	
Post3 × Performance	0.040		-11.010***	
	(0.955)		(0.001)	
Post3 × Treat3 × Performance	-0.319		-3.972	
	(0.864)		(0.607)	
Treat4		-0.206		0.019
		(0.510)		(0.922)
Post14× Treat4		0.514		0.224
		(0.207)		(0.467)
Treat4 × Performance		-0.250		4.721
		(0.752)		(0.202)
Post4× Performance		-0.404		-8.612***
		(0.565)		(0.007)
Post4 × Treat4 × Performance		0.942		-3.790
		(0.538)		(0.572)
Log of sales	0.065	0.052	0.113**	0.098*
	(0.255)	(0.318)	(0.049)	(0.064)
Stock volatility	4.507***	4.937***	6.666***	6.439***
	(0.005)	(0.001)	(0.000)	(0.000)
CEO Age	-0.013	-0.019*	-0.012	-0.018*
	(0.188)	(0.050)	(0.250)	(0.077)
Founder-heir CEO	-1.527***	-1.378***	-1.552***	-1.395***
	(0.000)	(0.000)	(0.000)	(0.000)
Log CEO tenure	0.135	0.060	0.144	0.071
	(0.230)	(0.579)	(0.194)	(0.514)
CEO-Chairman	-0.357**	-0.249	-0.405**	-0.279*
	(0.026)	(0.121)	(0.012)	(0.080)

Table 1.11, continued

CEO voting power	-0.132***	-0.102***	-0.132***	-0.104***
	(0.004)	(0.004)	(0.002)	(0.002)
Nonemployee blockholders	0.252	0.180	0.314*	0.244
	(0.118)	(0.222)	(0.054)	(0.106)
Dual class firm	0.058	0.337	0.059	0.325
	(0.839)	(0.190)	(0.836)	(0.229)
Constant	-3.666***	-3.198***	-4.101***	-3.555***
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	7709	8093	7701	8085
Chi sq	256.7	258.3	267.2	254.0
Pseudo R-squared	0.158	0.145	0.129	0.115

evidence that the change in the independence of audit (compensation) committee has any significant effect on the sensitivity of CEO turnover to firm performance, regardless of whether firm performance is measured by market-adjusted stock return or by industry-adjusted change in ROA.

4.6. Section 302 of Sarbanes-Oxley Act and independence of audit committee

The insignificant result in last section rejects my hypothesis about a potential indirect effect of change in audit committee independence on sensitivity of forced CEO turnover to firm performance. In this section, I discuss the interpretation of this result against the regulatory background around the mandatory change in audit committee composition.

First, I note that the new exchange listing rule on audit committee independence is not the first such listing requirement for firms listed on NYSE and Nasdaq. Actually, NYSE and Nasdaq issued their first listing requirement regarding audit committee independence in December 1999 in response to SEC's calls for improvement in the effectiveness of corporate audit committees. According to that listing requirement, listed firms are required to maintain audit committees with at least three directors "all of whom have no relationship to the company that may interfere with the exercise of their independence from management and the company" (NYSE Listed Company Manual 303.01 [B][2][a]). Although the statement implies that each member of the audit committee should be independent, the definition of independence was vague and, more importantly, that rule allowed stock exchanges to grant exemptions to some firms under "exceptional and limited circumstances". Compared to the listing requirement in 1999, the new exchange listing requirement in 2003 impose a much clearer and stricter definition of independent director and limit exemptions to only two special situations – newly-listed companies and overlapping board relationships. Thus, the treatment group in the sample of this paper consists mainly of firms exempt from the 1999 listing requirement and firms that complied with the 1999 listing requirement but not all audit committee members meet the stricter independent director definition of the new exchange listing rule of 2003. Given this earlier listing requirement, the number of potential treatment firms in my sample is greatly reduced and a weaker result is partially expected.

Second, Section 302 of SOX became effective on August 29, 2002, which is before changes in most audit committees took place. Section 302 requires CEO and CFO to certify that their firm's financial reports fairly represents, in all material aspects, the financial condition and results of operations of the firm, and that their internal "disclosure controls and procedures" are effective[10]. The essence of the certification requirements is to make the CEO and CFO be ultimately responsible for the quality of a firm's disclosure controls and financial reporting. Section 302 affects all publicly listed firms not just the treatment firms. Its effect on quality of reported accounting numbers and consequently the strength of the relation between forced CEO turnover and accounting performance might be quite significant such that the subsequent changes in audit committee composition only have limited incremental effect. This is another potential reason why we could fail to find significant indirect effect of audit committee independence on the sensitivity of forced CEO turnover to firm performance.

[10] Section 906 is similar to Section 302 but the scope of the latter is a little wider than the former because it also requires CEOs and CFOs to certify the effectiveness of "disclosure controls and procedures". Section 906 details criminal penalties for knowingly certifying fraudulent financial reports, while Section 302 details civil penalties for intentional false certification. Section 906 became effective on July 30, 2002, while Section 302 became effective on August 29, 2002.

Interestingly, in Table 1.11, the coefficient on the interaction of *Post3 (Post4)* and *Change in ROA* is negative and statistically significant, suggesting that during the post-SOX period forced CEO turnover is more sensitive to accounting performance in all firms in the two matched samples. If implementation of section 302 of SOX is responsible for this observed increase in sensitivity of forced CEO turnover to accounting performance in Table 1.11, then I should see a structural change in the turnover-performance relation from 2003 onward. Also, since Section 302 applies to all publicly listed firms, the structural change should be observed in all firms not just the matched treatment and control firms in Table 1.11. To shed some light on this conjecture, I estimate a logit regression where the dependent variable is the indicator for forced CEO turnover and the explanatory variables are an indicator for the post-2003 period and its interaction with *Change in ROA* using all firms in the full sample. The post-2003 indicator, *Post2003*, equals to 1 in years after 2003 (inclusive) and 0 otherwise. The regression results are reported in column 1 of Table 1.12. The coefficient on the interaction of *Post2003* and the *Change in ROA* is indeed significantly negative, suggesting an increase in the sensitivity of forced CEO turnover to accounting performance after 2003 in all firms. Since many firms also changed their board structure in year 2003 to comply with the new listing rules, the evidence in column 1 is not sufficient to establish that the change in turnover-performance sensitivity is due to the implementation of Section 302 of SOX. In column 2, I run a horse race between *Post1* to *Post3* and *Post-2003*. I find that *Post2003* appears to have the most explanatory power with a p-value for the interaction of *Post2003* and *Change in ROA* of 0.14, which is statistically significant at the 10 percent level under a one-sided test, while the other three interaction terms have p-values ranging from 0.35 to 0.64. Considering the high correlations among the four competing post indicators, which make multicolinearity a serious concern, the p-value of 0.14 is quite impressive. Hence, it seems that the implementation of Section 302 of SOX is mainly responsible for the increase in the sensitivity of forced CEO turnover to accounting performance in all firms. Since Section 302 became effective before most changes in audit committee composition took

Table 1.12: Implementation of Section 302 of SOX and the sensitivity of forced CEO turnover to accounting performance (1996-2007)

The table shows the results from logit regressions where the dependent variable is an indicator for forced CEO turnover in year t. *Post2003* is an indicator variable that equals to one in years after 2003(inclusive) and equals to zero otherwise. All other independent variables are defined in Appendix A and are measured in year t-1. The sample consists of all firms in the final sample of this paper from 1996 and 2007. Year and industry fixed effects are included in all columns. Industries are defined by the 17 industry groups[11] on Ken French's website. The numbers in parentheses are robust p-values and ***, **, and * denote significance at the 1, 5 and 10 percent levels, respectively.

VARIABLES	(1)	(2)
Change in ROA	-6.957***	-6.948***
	(0.000)	(0.000)
Post2003 × Change in ROA	-6.495***	-15.015
	(0.007)	(0.146)
Post3 × Change in ROA		5.900
		(0.545)
Post1 × Change in ROA		5.528
		(0.357)
Post2 × Change in ROA		-2.607
		(0.644)
Log of sales	0.105**	0.104**
	(0.035)	(0.035)
Stock volatility	6.831***	6.852***
	(0.000)	(0.000)
CEO Age	-0.035***	-0.035***
	(0.000)	(0.000)
Founder-heir CEO	-1.393***	-1.393***
	(0.000)	(0.000)
Log CEO tenure	0.153	0.155
	(0.122)	(0.116)
CEO-Chairman	-0.314**	-0.315**
	(0.022)	(0.022)
CEO voting	-0.107***	-0.108***
	(0.000)	(0.001)
Non-emp block	0.324**	0.320**
	(0.021)	(0.022)
Dual Class firm	0.168	0.161
	(0.454)	(0.477)
Constant	-3.098***	-3.094***
	(0.000)	(0.000)
Observations	10518	10518
Chi sq	260.1	261.6
Pseudo R-squared	0.104	0.105

[11] Stata output produced missing model fit statistics such as Chi-square when I included industry fixed effects for Fama and French (1997) 48 industry groups.

place and it has similar effect on quality of reported earnings numbers to audit committee independence, the implementation of Section 302 may have made it difficult to discover any incremental effect of changes in audit committee independence on quality of reported earnings numbers and consequently on sensitivity of forced CEO turnover to accounting performance.

Overall, further investigation is needed before concluding that change in audit committee independence does not affect the sensitivity of forced CEO turnover to accounting performance. The results in Table 1.11 only say that the incremental effect of audit committee independence following the 2003 exchange listing rules is insignificant.

5. Robustness Checks

5.1. Using involuntary CEO turnover as the dependent variable

So far, I have used forced CEO turnover as the dependent variable in all the regressions. This variable is coded using the method in Parrino (1997). The method classifies a CEO turnover as forced if news reports clearly indicate as much. If the new report is vague about the true reason for departure, the classification is based on a elaborate scheme. One concern is that, if in the post-SOX period, news reporters become more aggressive at seeking out the true reason for a CEO turnover in non-compliant firms than in compliant firms, or if non-compliant firms are more candid about CEO turnovers than compliant firms in the post-SOX period, then this could introduce a bias into my classifications that favors findings previous results. To assess whether previous results are sensitive to the specific method that I use to classify forced CEO turnovers, I define an alternative dependent variable, which I call involuntary CEO turnover, and use it in Table 1.13 to reexamine the main results in this study. The alternative involuntary CEO turnover variable is defined in Section 3.2.1. The main advantage of this alternative dependent variable is that its value does not depend on news reports.

Table 1.13 reports the logit regression results. Columns 1 and 2 are estimated using the matched sample for the rule that requires firms to have a majority of independent directors on the board and columns 3 and 4 are estimated using the matched sample for the rule that requires firms to have a fully independent nominating committee. For each listing rule, I estimate separate regressions using market-adjusted stock return (column 1 and 3) and industry-adjusted change in ROA (column 2 and 4) as the firm performance measure. I find that the results in columns 1 and 2 are very similar to those in columns 1 and 3 of Table 1.9, except that the statistical significance of the coefficient on the triple interaction of *Post1*, *Treat1* and *Performance* is somewhat weaker, though it remains significant at 10% level in one-sided tests. Similarly, the results in columns 3 and 4 are very similar to those in columns 1 and 3 of Table 1.10, with the coefficient on the triple interaction of *Post2*, *Treat2* and *Performance* being negative and highly statistically significant for both stock- and accounting-based performance measures. As expected, the pseudo R-squared is lower in Table 1.13 than in Tables 1.9 and 1.10. This may reflect some voluntary CEO turnovers being misclassified as involuntary, whose occurrence is not correlated with poor firm performance. I conclude that the forced CEO turnover definition is preferable because the logit models have higher explanatory power. Nevertheless, the results in Table 1.13 confirm that my earlier findings in Table 1.9 and 1.10 are unlikely to be driven by possible changes in media coverage.

5.2. Excluding year 2007

The last year of my sample period is 2007. Since I lag firm-level variables by a year, year 2007 is associated with CEO turnovers that occurred in the second half of 2007 and the first-half of 2008. This is the time period when the most recent Global Financial Crisis was in full swing. It is possible that treatment firms faced systematically greater challenges than control firms. One potential result is that boards of treatment firms would need to act more promptly to remove incompetent CEOs to save their firms from financial distress. If this conjecture is true, then I

Table 1.13: Robustness check: using involuntary CEO turnover as the dependent variable

The table shows coefficient estimates from logit regressions where the dependent variable is a dummy for involuntary CEO turnover in year t. A CEO turnover is defined as involuntary if the departing CEO leaves office before the age of 65 and does not take a CEO position in another S&P 1500 firm within one year of the initial CEO turnover announcement. The independent variables are defined in Appendix A and are all measured in year t-1. Column 1 and 2 are estimated using the matched sample defined by the new listing rule on board independence, and column 3 and 4 are estimated using the matched sample defined the new listing rule on nominating committee independence. I exclude forced CEO turnovers that resulted from punishment for violations because poor firm performance is normally not the main reason for these turnovers and treatment firms contain a disproportionately higher fraction of such type of forced CEO turnovers. *Performance* is measured by market-adjusted stock returns in column 1 and 3, while it is measured by industry-adjusted change in ROA in column 2 and 4, both measures are defined in Appendix A. Year and industry fixed effects are included in all columns. Industries are defined by Fama and French 48 industry groups. The numbers in parentheses are robust p-values and ***, **, and * denote significance at the 1, 5 and 10 percent levels, respectively.

VARIABLES	Majority of independent directors		Fully independent nominating committee	
	(1)	(2)	(3)	(4)
Performance	-0.676***	-3.177***	-0.984***	-7.507***
	(0.000)	(0.005)	(0.000)	(0.000)
Treat1	-0.582***	-0.592***		
	(0.000)	(0.000)		
Post1 × Treat1	0.204	0.212		
	(0.401)	(0.389)		
Treat1 × Performance	0.325	1.983		
	(0.319)	(0.440)		
Post1 × Performance	0.085	-0.803		
	(0.794)	(0.736)		
Post1 × Treat1 × Performance	-1.238	-13.681**		
	(0.126)	(0.022)		
Treat2			-0.208*	-0.204*
			(0.056)	(0.059)
Post2 × Treat2			0.005	0.028
			(0.977)	(0.871)
Treat2 × Performance			0.485*	7.001***
			(0.093)	(0.001)
Post2 × Performance			0.801*	1.939
			(0.060)	(0.555)
Post2 × Treat2 × Performance			-1.551***	-8.094*
			(0.008)	(0.078)
Log of sales	0.062*	0.070**	0.071**	0.076**
	(0.064)	(0.039)	(0.043)	(0.030)
Stock volatility	4.367***	4.284***	4.805***	4.598***
	(0.000)	(0.000)	(0.000)	(0.000)
CEO Age	0.031***	0.031***	0.032***	0.031***
	(0.000)	(0.000)	(0.000)	(0.000)
Founder-Heir CEO	-0.498***	-0.522***	-0.636***	-0.669***
	(0.001)	(0.000)	(0.000)	(0.000)
Log CEO tenure	0.377***	0.380***	0.371***	0.374***
	(0.000)	(0.000)	(0.000)	(0.000)
CEO-Chairman	-0.077	-0.088	-0.005	-0.009
	(0.443)	(0.378)	(0.957)	(0.927)

Table 1.13, continued

CEO voting power	-0.031**	-0.031**	-0.039**	-0.038**
	(0.050)	(0.039)	(0.020)	(0.018)
Nonemployee blockholders	0.104	0.122	0.060	0.068
	(0.313)	(0.239)	(0.577)	(0.533)
Dual class firm	0.122	0.108	0.018	0.013
	(0.486)	(0.538)	(0.932)	(0.950)
Constant	-5.196***	-5.232***	-5.416***	-5.377***
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	7320	7312	7095	7087
Chi sq	237.8	219.0	238.2	220.1
Pseudo R-squared	0.0558	0.0529	0.0589	0.0560

could observe a larger increase in the rate of forced CEO turnover and a larger increase in the sensitivity of forced CEO turnover to firm performance in treatment firms than in control firms.

Although this conjecture is reasonable, I do not think it is a serious concern in this study since I matched treatment firms with control firms so that they have similar characteristics, at least on several critical dimensions. For example, in the matched sample defined by the new listing rule requiring a majority of independent directors on the board, the treatment firms and control firms are not significantly different in terms of total assets, sales, Tobin's Q, earnings volatility, percentage of firms with classified boards, and CEO age in 2001.

Nevertheless, to more definitively rule out this possibility, I repeat the base regressions in Tables 1.9 and 1.10, but exclude observations in year 2007. The results are reported in the two panels of Table 1.14. The left panel checks the robustness of the results related to the new exchange rule requiring a majority of independent directors on the board; while the right panel checks the robustness of the results related to the new exchange rule requiring a fully independent nominating committee. Firm performance is measured by market-adjusted stock return in columns 1 and 3, while it is measured by industry-adjusted change in ROA in columns 2 and 4. Matched samples for the corresponding new exchange rules are used for the estimation. I find that the coefficients on the triple interaction of *Post1*, *Treat1* and *Performance* and those on the triple interaction of *Post2, Treat2* and *Performance* remain negative and statistically significant.

Table 1.14: Robustness check: excluding observations in year 2007

The table shows coefficient estimates from logit regressions where the dependent variable is an indicator for forced CEO turnover in year t. The independent variables are defined in Appendix A and are all measured in year t-1. Column 1 and 2 are estimated using the matched sample defined by the new listing rule on board independence, and column 3 and 4 are estimated using the matched sample defined by the new listing rule on nominating committee independence. Observations in year 2007 are excluded. I also exclude forced CEO turnovers that resulted from punishment for violations because poor firm performance is normally not the main reason for these turnovers and treatment firms contain a disproportionately higher fraction of such type of forced CEO turnovers. *Performance* is measured by market-adjusted stock returns in column 1 and 3, while it is measured by industry-adjusted change in ROA in column 2 and 4, both measures are defined in Appendix A. Year and industry fixed effects are included in all columns. Industries are defined by Fama and French (1997) 48 industry groups. The numbers in parentheses are robust p-values and ***, **, and * denote significance at the 1, 5 and 10 percent levels, respectively.

VARIABLES	Majority of independent directors		Fully independent nominating committee	
	(1)	(2)	(3)	(4)
Performance	-2.734***	-7.042***	-2.677***	-11.286***
	(0.000)	(0.000)	(0.000)	(0.000)
Treat1	-0.593	-0.485*		
	(0.125)	(0.072)		
Post1 × Treat1	-0.093	0.154		
	(0.862)	(0.735)		
Treat1 × Performance	-0.140	3.076		
	(0.871)	(0.531)		
Post1 × Performance	0.962	-7.388**		
	(0.220)	(0.024)		
Post1 × Treat1 × Performance	-3.252*	-20.175**		
	(0.059)	(0.043)		
Treat2			-0.148	-0.112
			(0.540)	(0.570)
Post2 × Treat2			-0.337	-0.086
			(0.357)	(0.787)
Treat2 × Performance			0.628	8.112**
			(0.337)	(0.017)
Post2 × Performance			1.502*	-6.088
			(0.074)	(0.178)
Post2 × Treat2 × Performance			-3.005**	-10.214
			(0.017)	(0.104)
Log of sales	0.050	0.088	0.041	0.083
	(0.404)	(0.142)	(0.502)	(0.182)
Stock volatility	5.145***	7.341***	7.424***	8.750***
	(0.002)	(0.000)	(0.000)	(0.000)
CEO Age	-0.018*	-0.017	-0.024**	-0.024**
	(0.096)	(0.136)	(0.037)	(0.038)
Founder-Heir CEO	-1.262***	-1.319***	-1.380***	-1.504***
	(0.001)	(0.000)	(0.002)	(0.001)
Log CEO tenure	0.131	0.149	0.198	0.208*
	(0.294)	(0.234)	(0.103)	(0.083)
CEO-Chairman	-0.229	-0.274	-0.136	-0.159
	(0.195)	(0.116)	(0.434)	(0.354)
CEO voting power	-0.135***	-0.136***	-0.145***	-0.142***
	(0.001)	(0.001)	(0.000)	(0.000)

Table 1.14, continued

Nonemployee blockholders	0.245	0.315*	0.274	0.333*
	(0.159)	*(0.071)*	*(0.114)*	*(0.058)*
Dual class firm	0.316	0.258	0.314	0.327
	(0.272)	(0.367)	(0.319)	(0.293)
Constant	-3.521***	-3.852***	-3.945***	-4.226***
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	6719	6713	6528	6522
Chi sq	240.4	240.3	245.1	243.0
Pseudo R-squared	0.158	0.119	0.163	0.137

5.3. Investor pressure

The corporate scandals in 2001 and 2002 have resulted in a dramatic change in public attitude toward corporate governance issues in U.S. firms. Investors have become more determined to have their voices heard in corporate board rooms. It is possible that treatment firms attracted more attention from activist investors during the post-SOX period than control firms had. This might reflect more past corporate governance problems in treatment firms. The greater pressure on boards of directors in treatment firms from activist investors might then force these firms take quicker actions to remove poorly-performing CEOs during the post-SOX period. However, when I read news around the CEO turnovers in my sample, I find only a couple of cases where investor pressure is mentioned and even these are not all in treatment firms. This fraction of cases appears to be too small to affect my results. Someone may argue that activist investors mainly influence boards privately so most of their actions are not reported in news. However, this seems to be unrepresentative of many well known activist investors, who usually like to publicize their agenda in order to generate shareholder support for their causes. Thus, I doubt that activist investor pressure is a likely cause of my finding.

6. Performance changes around forced CEO turnovers

In this section, I address the question of whether the documented increase in sensitivity of forced CEO turnover to firm performance can be interpreted as evidence of more effective monitoring. By more effective monitoring, I mean that the more prompt firing of poorly-performing CEOs is on average beneficial to shareholders. This necessitates two things. First, forced CEO turnovers in treatment firms in the post-SOX period should in general be followed by improvements in firm performance. Second, the quicker action of firing in the post-SOX period is more beneficial to shareholders than does the supposedly slower action in the pre-SOX period in treatment firms.

As to the first point, Denis and Denis (1995) and Huson, Malatesta and Parrino (2004) find that forced CEO turnovers are associated with subsequent improvements in operating performance. However, since these two studies use data in 1980s and early 1990s, it is unclear if their findings apply to the sample period of this paper. More importantly, it is unclear if forced CEO turnovers are still followed by improvements in firm performance in the post-SOX period when firm performance prior to the turnovers is not as bad as it is in the pre-SOX period. Presumably, it is easier to improve firm performance when it is really bad than when it is not so bad. Hence, it is important to examine this in my sample.

As to the second point, higher turnover-performance sensitivity should be beneficial to shareholders in the sense that it provides stronger incentives to CEOs to work harder, just as higher pay-performance sensitivity does. A large executive compensation literature generally concludes that higher pay-performance sensitivity is good for shareholders. Given the low turnover-performance sensitivity observed in the data, I believe that there is an improvement in CEO incentives in the post-SOX period. Besides, if forced CEO turnovers are actually followed by improvement in firm performance in the post-SOX period, then the quicker action to fire unfit CEOs is likely to reduce volatility in firm performance, which is another benefit to shareholders.

However, the more prompt firing also increases the risk of making type I errors, i.e. firing an otherwise good CEO. Given the large costs associated with CEO dismissals, such as severance pays, search costs for a new CEO, etc., making type I errors can be quite costly to the firm. Although it is difficult to compare the relative magnitude of the positive and negative effects, if I find no evidence that CEOs are prematurely fired in the post-SOX period, then quicker firing in the post-SOX period is likely to have a positive effect on firm value due to the incentive effect.

In the following, I examine abnormal stock performance and changes in operating performance around forced CEO turnover events in firms that are affected by the rule that requires listing firms to have a majority of independent directors on the board and the rule that requires listing firms to have a fully independent nominating committees to provide evidence on the two criteria of effective monitoring I discussed above.

6.1. Abnormal stock performance around forced CEO turnovers

In this section, I examine average cumulative abnormal stock return from 12 months before to 12 months after each forced CEO turnover announcement. The purpose is to see the trend in cumulative abnormal stock return around each turnover event to shed light on the question of whether forced CEO turnovers in treatment firms in the post-SOX period are followed by improvement in stock performance. I use monthly stock returns from CRSP to estimate market models and cumulate the prediction errors from the market model month by month over the 25-month period around each forced CEO turnover. Market model parameters are estimated over the 24 month period that ends 12 month before the month of CEO turnover announcement. I measure market returns by monthly returns on either the value-weighted or the equal-weighted CRSP index and plot the average cumulative abnormal stock return against event time in separate panels in Figure 1.4. In Panel A, the treatment and control firms are determined based on whether they have a majority of independent directors on the board in the pre-SOX period, while, in Panel B, the classification is based on whether a firm has a fully independent nominating committee in the pre-SOX period. The left panels in both Panel A and Panel B show the plots when value-weighted CRSP index is used as the market index in the market model regression, while the right panels show the plots when equal-weighted CRSP index is used as the market index.

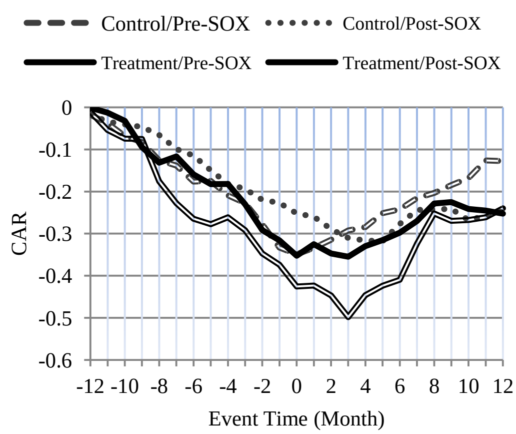
Focusing on treatment firms in the post-SOX period, I find that, in the left panel of Panel A, the average abnormal stock return falls to -35% in the month of the turnover announcement and then climbs up by 10% to -25% twelve months after the turnover. In the right panel of Panel A, the pattern is similar. The average abnormal stock return falls to -42% in the month of turnover announcement and then goes up by 12% to -30% twelve months after the turnover. Although the increase in abnormal stock return is statistically insignificant, probably due to small sample size, the plots clear show that stock performance on average improves following forced CEO turnovers. For other subgroup of firms, the pattern of the plots is similar.

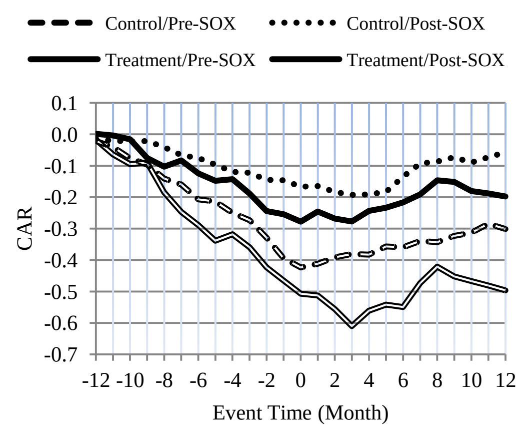
Similarly, in the left panel of Panel B where market return is measured by return on value-weighted CRSP index, I find that the mean cumulative abnormal stock return in treatment firms in the post-SOX period is -37% in the month of the turnover announcement. It falls to -44% in the three months after the turnover announcement and climbs back to -33% at the end of the 12-month period after the turnover announcement. In the right panel of Panel B where the market return is measured by return on equal-weighted CRSP index, the mean cumulative abnormal stock return declines by -28% by the time of the turnover announcement. It declines further to -33% three months after the turnover announcement but recovers by 15% to -18% twelve months after the turnover announcement. The continued decline in abnormal stock return in the first 3 months after the turnover announcement seems to be related to the specific time period because we also observe similar decline in control firms. The fact that the abnormal stock return eventually reverses course is inconsistent with the view that there is no improvement in firm performance following forced CEO turnovers in treatment firms in the post-SOX period.

Figure 1.4: Cumulative abnormal stock returns around forced CEO turnover announcements

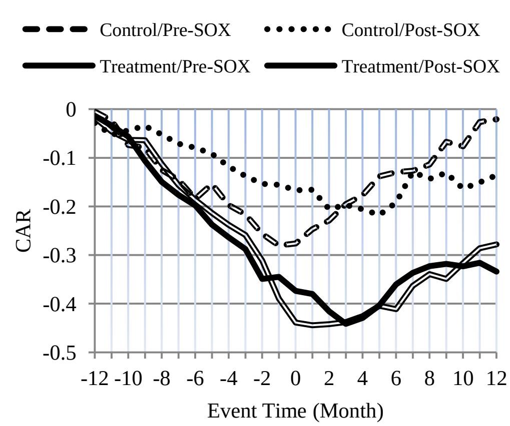
The plots show the mean cumulative abnormal stock returns (CAR) around forced CEO turnover announcements by treatment and control firms and by pre- and post-SOX period. Event time is in measured in months relative to the announcement month. The abnormal stock return is calculated using a market model where market return is measured by value-weighted CRSP index return (Left Panels) or by equal-weighted CRSP index return (Right Panels). Parameters of the market models are estimated using monthly stock returns from 36 to 13 months before each CEO turnover announcement. In Panels A, treatment and control firms are defined by whether a firm had majority of independent directors on board in the pre-SOX period. In Panel B, treatment and control firms are defined by whether a firm had a fully independent nominating committee in the pre-SOX period.

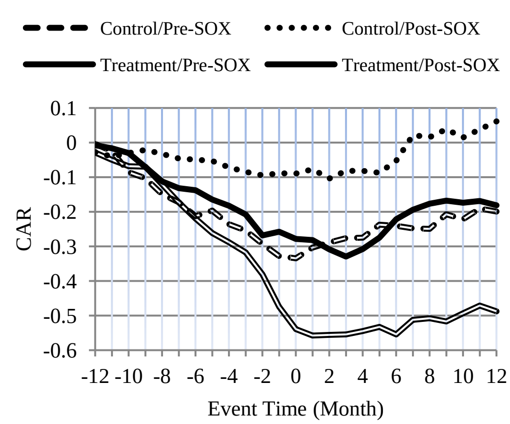
Panel A: The rule requiring majority of independent directors on board





Panel B: The rule requiring fully independent nominating committees





When I compare graphs of cumulative abnormal stock return in treatment firms between the pre- and post-SOX period in Figure 1.4, I find that, except in the left panel of Panel B, forced CEO turnovers in post-SOX period are on average associated with a smaller decline in abnormal stock performance prior to the CEO turnover events and a quicker recovery thereafter than in the pre-SOX period. This is especially noticeable when equal-weighted CRSP index return is used in the market model.

6.2. Change in operating performance around forced CEO turnovers

In this section, I examine if there are real improvements in operating performance following forced CEO turnovers. If investors are rational and the market is efficient, the observed increase in abnormal stock return following CEO turnover events in Figure 1.4 predicts improvements in operating income after CEO turnover. I examine change in ROA where ROA is calculated as the annual EBIT scaled by lagged total assets. In order to control for influence of industry factors, I adjust the change in ROA by industry median change of this ratio where industry is defined by Fama and French 48 industry classification. Annual data from Compustat database are used in the analysis and the industry median is based on all firms with valid data on Compustat. The change in ROA is computed from two years before to two years after each forced CEO turnover. I require that each firm to have five years of valid data in Compustat around the turnover event to be included in the analysis. Panel A of Table 1.15 reports the mean (median) change in ROA and its statistical significance by event time for the sample of treatment firms associated with the two listing rules. T-test is performed for the means and signed rank test is performed for the medians. The left panel is for treatment firms that did not have majority of independent directors on the board before SOX. I observe that, in the year prior to the turnover announcement, industry-adjusted change in ROA is negative and statistically significant in all but one column. In contrast, in the year after the turnover, the change in ROA is positive in all but one column. Importantly, during the post-SOX period, both the mean and median changes in

ROA in the year after the turnover are positive and the median change is also statistically significant at the 10% level. The right panel is for treatment firms that did not have a fully independent nominating committee before SOX. I observe that the mean and median change in ROA is significantly negative in the year prior to the CEO turnover events, while the mean change in ROA is positive and median change in ROA is slightly negative – neither is significantly different from zero – in the year after the turnover events.

Table 1.15: Changes in operating performance around forced CEO turnovers

This table shows the sample means and medians of industry-adjusted change in ROA around forced CEO turnovers and the change of it (i.e. second derivative) for the sample of treatment firms defined by the two listing rules. Industry is defined by Fama and French 48 industry groups. Industry-adjusted change in ROA is calculated as the change in the ratio of annual EBIT divided by beginning of year total assets minus the industry median change for this ratio. Event time is measured in years relative to the year of CEO turnover announcements. ***, ** and * represent 1, 5 and 10 percent statistical significance respectively for two-sided t-test for means and signed rank test for medians that the annual change in ROA is zero.

	A Majority Independent Board Rule				Independent Nominating Committee Rule			
Event Time	Pre-SOX (N=31)		Post-SOX (N=16)		Pre-SOX (N=101)		Post-SOX (N=37)	
	Mean	Median	Mean	Median	Mean	Median	Mean	Median
Panel A: Industry-adjusted Change in ROA (%)								
-2	-2.37	-2.41	-0.18	-0.01	-1.16	-0.32	-0.41	-0.07
-1	-7.85*	-2.04	-3.44***	-2.13***	-2.50*	-1.44***	-1.91**	-1.07**
0	-4.10*	-2.23	-1.58**	-2.16**	-1.82*	-0.72	-1.72	0.15
1	3.40**	0.78*	1.12	1.69*	0.48	0.34	1.07	-0.02
2	0.31	-0.74	-1.15	-0.38	0.01	0.24	0.84	1.19
Panel B: Change in Industry-adjusted Change in ROA (%)								
-1	-5.48	0.95	-3.27**	-1.48***	-1.34	-0.33	-1.499	-1.28**
0	3.75	1.22	1.86	0.86	0.68	0.03	0.1903	1.50
1	7.51**	4.60**	2.69**	2.11***	2.30	1.27**	2.7840*	0.34
2	-3.09	-0.06	-2.27**	-1.78**	-0.47	0.08	-0.224	-0.37

In panel B of Table 1.15, I report the change in change in ROA which equals to the change in ROA in the current year minus that in the prior year (i.e. second derivative). By this measure, I want to see if forced CEO turnovers are associated with at least a deceleration in the drop in ROA following the turnover events. For treatment firms that did not have a majority of independent directors on the board before SOX, the table shows that both the mean and median change in change in ROA are positive and statistically significant at the 5% level in the first year after the turnover. For treatment firms that did not have a fully independent nominating committee on the board before SOX, I find that the mean change in change in ROA is significantly positive at the 10% level in treatment firms in the post-SOX period, which suggests that there is a significant reversal of the momentum in change in ROA from the pre-turnover period.

Figure 1.5: Change in operating performance around forced CEO turnovers before and after SOX

The following graphs show the median (Panel A), average (Panel B) and cumulative industry-median adjusted change in ROA (Panel C) around forced CEO turnover events for the subsample of treatment firms that did not have majority of independent directors on board (Left Panels) or fully independent nominating committees (Right Panels) during the pre-SOX period. Industry is defined by the Fama and French 48 industry classification. *ROA* is calculated as annual EBIT divided by beginning of year total assets. *Change in ROA* equals to the difference between current and lagged ROA. Event time is measured in years relative to the year of CEO turnover announcements.

Panel A: Median change in ROA

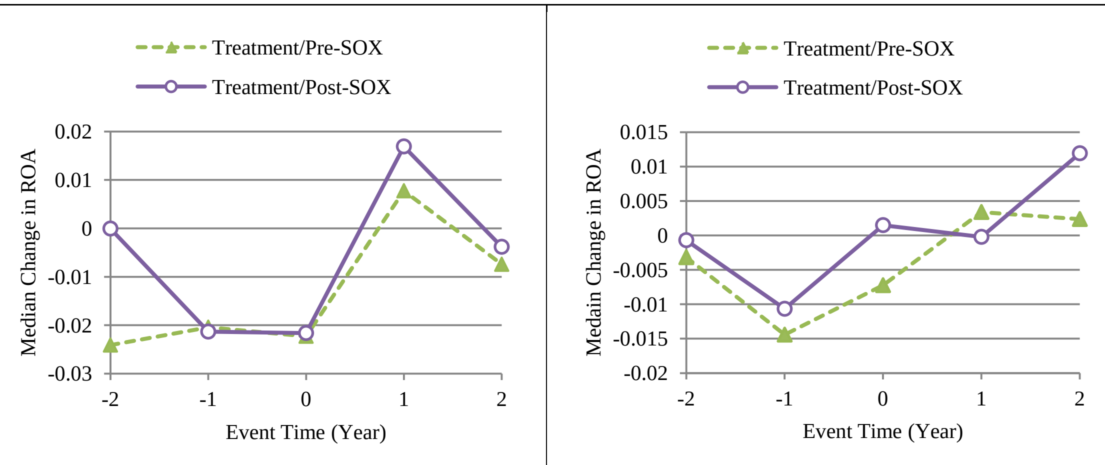
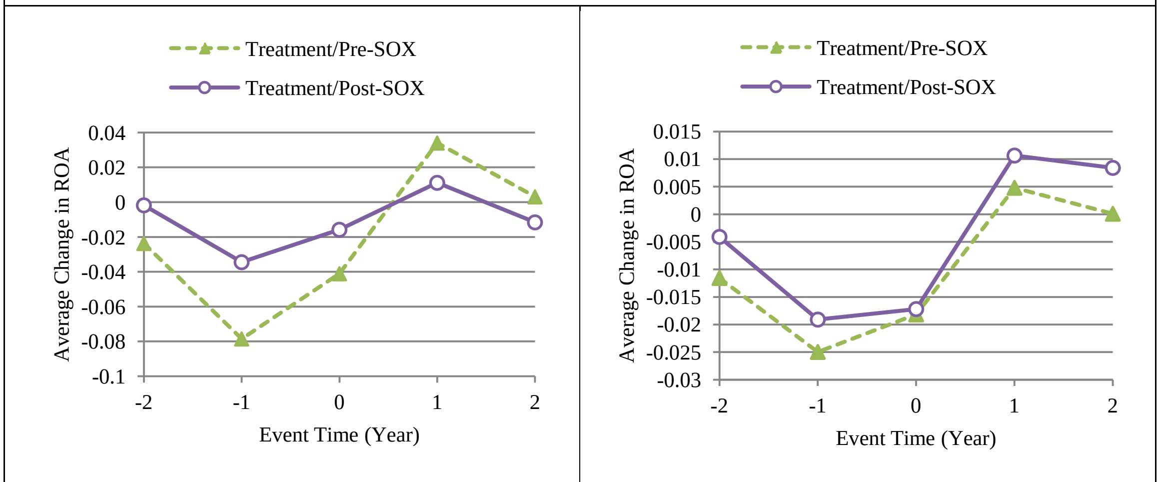
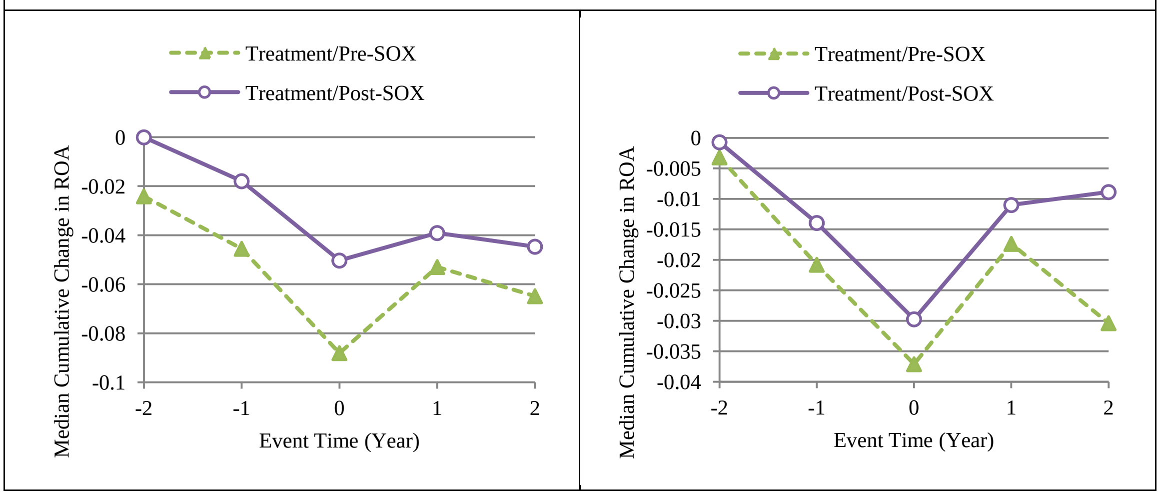


Figure 1.15, continued

Panel B: Average change in ROA



Panel C: Median cumulative change in ROA



These changes are also plotted in Figure 1.5 which show the sample median (Panel A), mean (Panel B) and cumulative (Panel C) change in ROA for the two types of treatment firms from two years before to two years after each CEO turnover event. The three left panels are for firms that were noncompliant with the rule requiring a majority independent board before SOX, while the three right panels are for firms that were noncompliant with the rule requiring a fully independent nominating committee. The plots of change in ROA in Panel A and B clearly show that operating performance improves in treatment firms after turnover events during both the preand post-SOX period. The plots of cumulative change in ROA in Panel C show that the line for cumulative change in ROA in the post-SOX period is everywhere above that in the pre-SOX period. This suggests that forced CEO turnovers in treatment firms in the post-SOX period is associated with a smaller drop in operating performance before and a quicker recover after each forced CEO turnover than they do in the pre-SOX period.

6.3. Premature firing hypothesis

To see if the premature firing effect is significant in treatment firms in the post-SOX period, I conduct event studies and compare abnormal stock return around forced CEO turnover announcements in the pre- and post-SOX periods. In an efficient market, the abnormal stock return is supposed to capture the firm value effect of the CEO turnover decisions. However, it is also influenced by other factors. Since I compare the abnormal stock return over two periods, I need to discuss how these other factors might be different over the two periods and thus affect my inference. One factor that might be systematically different between the two periods is the amount of negative information being revealed by the CEO turnover announcements. Since turnovers in post-SOX period are on average announced at better firm performance than turnovers in the pre-SOX period, I expect more negative information to be revealed in the post-SOX period. Another factor is market anticipation of the turnover events. In terms of firm performance prior to CEO firing, the turnover events are likely to be more anticipated in the pre-SOX period than in the post-SOX period. However, in terms of corporate governance, the turnover events are probably more anticipated in the post-SOX period than in the pre-SOX period. Since there is no theoretical reason to believe that the anticipation effect is going to be systematically different between the two periods, I assume that this factor has similar effect in both periods. Even if there is some systematic difference, as long as the anticipation effect is small, my inference will not be significantly affected.

I use standard event-study methodology and measure abnormal stock return by prediction errors from market models. Specifically, I compute the cumulative prediction errors over the day of and the day before the CEO turnover announcement. This event window has been used in prior studies by Denis and Denis (1995) and Huson, Malatesta and Parrino (2004). Market model parameters are estimated over the 250-day period ending 250 days before each CEO turnover announcement and value-weighted CRSP index return is used for market return. All stock return data are from the CRSP database. Since forced CEO turnovers are in general preceded by poor stock returns, especially in the year prior to the turnover announcements, I end my estimating window 250 days before the turnover announcement to avoid potential biases in market model parameter estimates. As before, I exclude forced CEO turnovers that are related to violations. Furthermore, I exclude firms that made earnings announcements during the event window to make sure that the announcement returns mainly reflect market surprise over the CEO turnover announcements.

Table 1.16 reports the cross-sectional average of cumulative prediction errors, broken down by treatment and control firms and by pre- and post-SOX period. The statistical significance of the average cumulative prediction error is assessed using cross-sectional t-test to control for any change in variance of abnormal returns induced by the turnover events. The top panel reports results for the sample of forced CEO turnovers in firms that were forced to maintain a majority of independent directors on the board after SOX.

I observe that the two-day abnormal stock return in treatment firms is significantly negative in both the pre- and post-SOX period. On the surface, the negative abnormal stock return seems to suggest that the CEO firing decision destroys firm value. But the negative sign could also be driven by more negative news being revealed by the turnover announcements. The prior evidence on improvement in firm performance following these forced CEO turnovers supports the latter explanation. Hence, one reasonable explanation for the negative abnormal stock return is that the turnover decisions themselves increase firm value but the negative information revealed more than offset the positive effect in the two-day event window.

Table 1.16: Two-day announcement period abnormal stock returns

This table shows the mean cumulative abnormal stock returns over the (-1,0) event window for forced CEO turnover announcements, broken down by treatment vs. control firms and pre- vs. post-SOX period. Abnormal stock returns are estimated using a market model where market return is measured by daily returns on value-weighted CRSP index. Parameters of the market model are estimated using daily stock returns from 500 through 251 trading days before each CEO turnover announcement. The sample consists of all forced CEO turnovers in the subsample of treatment and control firms defined by the rule that requires listed firms to have majority of independent directors (top panel) and the rule that requires firms to have fully independent nominating committee (bottom panel), respectively. *Positive* equals to the percentage of the two-day cumulative stock returns that are positive. The t-statistic from two-sided test that the average CAR across CEO turnovers is not significantly different from zero is reported beneath each average CAR. N is the number of observations.

		Treatment		Control	
CAR (-1,0)		Pre-SOX	Post-SOX	Pre-SOX	Post-SOX
Rule that requires majority of independent directors	Mean	-0.047	-0.022	0.000	0.000
	t-statistics	-2.06	-2.19	0.06	0.02
	Positive	0.37	0.36	0.47	0.49
	N	25	24	119	72
Rule that requires fully independent nominating committees	Mean	-0.005	-0.002	-0.004	-0.009
	t-statistics	-0.45	-0.36	-0.31	-0.71
	Positive	0.52	0.44	0.40	0.40
	N	81	50	60	37

As far as my test of the premature firing hypothesis is concerned, I am only interested in the relative magnitude of the abnormal stock returns in the pre- and post-SOX period. I notice that the abnormal stock return has actually become less negative in the post-SOX period than it is in the pre-SOX period.[12] This suggests that the CEO firing decision in treatment firms in the post-SOX period is at least as good as it is in the pre-SOX period if not any better, which suggests that the premature firing effect is insignificant.

The bottom panel of Table 1.16 reports results for the subsample of firms that were forced to have a fully independent nominating committee after SOX. The two-day abnormal stock

[12] The difference is not statistically significant.

return in treatment firms is negative in both the pre- and post-SOX period though neither is statistically different from zero. Again, there is no evidence that the turnover decisions in the post-SOX period is of lower quality than those in the pre-SOX period.

Since more negative information is likely to be revealed in the post-SOX period than in the pre-SOX period, my interpretation of the findings in Table 1.16 is robust to difference in amount of negative information being revealed by the turnover announcements. In control firms, the two-day abnormal stock return is not significantly different from zero in both panels.

7. Conclusion

This paper provides new evidence on the potential benefits of SOX and the ensuing new exchange listing rules and the effectiveness of monitoring by independent directors. Although many researchers, regulators and investors believe that increasing the representation of independent directors on corporate boards can improve quality of board oversight, empirical evidence has been mixed and inconclusive. Recent research even raises doubt about the effectiveness of independent directors in monitoring CEOs. Using the change in NYSE and Nasdaq listing rules following the passage of the Sarbanes-Oxley Act as a source of exogenous variation, I provide the first statistically convincing evidence on a causal relation between board and nominating committee independence and the sensitivity of forced CEO turnover to firm performance. Specifically, I find that firms that after SOX moved to a majority independent board or to a fully independent nominating committee experience increased sensitivity of forced CEO turnover to performance. This evidence suggests that quality of board monitoring is positively related to board independence and nominating committee independence and the causation goes from board structure to quality of board monitoring.

Consistent with the increase in turnover-performance sensitivity representing more effective monitoring, I find that forced CEO turnovers are followed by improvements in both stock performance and operating performance in firms that are forced to adopt a majority independent board or a fully independent nominating committee. Plots of cumulative abnormal stock returns and change in ROA around forced CEO turnovers in these firms show that these firms in general experience a smaller decline in firm performance prior to CEO turnover and a quicker recovery thereafter in the post-SOX period than they do in the pre-SOX period. Lastly, when I compare CEO turnover announcement period abnormal stock returns in the pre- and post-SOX periods, I find no evidence that the more prompt firing in the post-SOX period significantly increases the chances of making type I errors. Since the increase in sensitivity of forced CEO turnover to firm performance should in general lead to stronger incentives to CEOs, the lack of evidence of premature firing suggests that the increase in turnover-performance sensitivity is a reflection of *more* effective monitoring.

Appendix A: Definition of Variables

Variables	Definition
Dependent variables	
Forced CEO turnover	An indicator variable for forced CEO turnover classified using the method in (Parrino, 1997).
Involuntary CEO turnover	An indicator variable for involuntary CEO turnover. A CEO turnover is involuntary if the departing CEO leaves office before the age of 65 and does not take a CEO position in another S&P 1500 firm within one year of the initial CEO turnover announcement.
Firm characteristics	
Log of total assets	Natural log of book value of total assets in millions of dollars
Log of sales	Natural log of annual sales in millions of dollars
Tobin's Q	Book value of total assets minus book value of common equity plus the market value of common equity over book value of total assets.
Earnings volatility	Standard deviation of annual EBIT scaled by beginning of year total assets over the past five years.
Stock volatility	Standard deviation of monthly stock returns in the past 12 months.
Firm Performance	
Return	The firm's stock return adjusted by return on the value-weighted CRSP index cumulated over the one year period prior to the CEO turnover. This variable is winsoried at the two 1% tails.
Change in ROA	The firm's change in EBIT scaled by lagged total assets adjusted by the median of this ratio for the firm's Fama and French 48 industry over the year prior to CEO turnover. This variable is winsorized at the two 1% tails.
Governance characteristics	
CEO-Chairman	An indicator variable that equals to 1 if the CEO is also the Chairman of the board and equals to 0 otherwise.
Classified board	An indicator variable for classified board (also known as staggered board).
Dual class firm	An indicator variable for firms with more than one class of common shares.
Independent directors (%)	Total number of independent directors divided by total number of directors.
G-index	The Gomper, Ishi and Metricks (2003) index which equals to the number of anti-takeover provisions a firm has. The maximum is 24.

Nonemployee blockholders	An indicator variable for the presence of non-employee blockholders on board where a blockholder is defined as anyone holding more than 1% of the total voting power.
CEO characteristics	
CEO age	CEO age at the time of the CEO turnover announcement.
CEO voting power	The percentage of votes held by the CEO as defined by RiskMetrics.
Founder-Heir CEO	An indicator variable for CEOs who are founders or heirs of founders.
Insider-linked voting	The percentage of voting power held by insider and linked (gray) directors as defined by RiskMetrics.
Log CEO tenure	Natural log of one plus CEO tenure in years.
Difference-in-Difference Variables	
Post1	An indicator variable for post-SOX period for the new listing rule requiring firms to have a majority of independent directors on the board. For firms in the control group, it equals to 1 in years after 2003 (inclusive) and equals 0 otherwise. For firms in the treatment group, it equals 1 in years after the firm met the new exchange requirement and equals 0 otherwise. However, the last year a treatment firm can meet the requirement is 2005.
Post2	An indicator variable for post-SOX period for the new listing rule requiring firms to have a fully independent nominating committee. For firms in the control group, it equals 1 in years after 2003 (inclusive). For firms in the treatment group, it equals 1 in years after the firm met the new exchange requirement and equals 0 otherwise. However, the last year a treatment firm can meet the requirement is 2005.
Post3	An indicator variable for post-SOX period for the new listing rule requiring firms to have a fully independent audit committee. For firms in the control group, it equals 1 in years after 2003 (inclusive). For firms in the treatment group, it equals 1 in years after the firm meets the new exchange requirement and equals 0 otherwise. However, the last year a treatment firm can meet the requirement is 2005.
Treat1	An indicator variable for membership in the treatment group defined by the new listing rule requiring firms to have a majority of independent directors on the board.
Treat2	An indicator variable for membership in the treatment group defined by the new listing rule requiring firms to have a fully independent nominating committee.
Treat3	An indicator variable for membership in the treatment group defined by the new listing rule requiring firms to have a fully independent audit committee.

References

Adams, R. B., & Ferreira, D. (2007). A theory of friendly board. *Journal of Finance , 62* (1), 217-250.

Beasley, M. S. (1996). An empirical analysis of the relation between the board of director composition and financial statement fraud. *The Accounting Review , 71* (4), 443-465.

Bebchuck, L. A., Cremers, M., & Peyer, U. (2010). The CEO pay slice. *Journal of Financial Economics , Forthcoming*.

Bebchuk, L. A., & Fried, J. M. (2003). Executive compensation as an agency problem. *Journal of Economic Perspectives , 17*, 71-92.

Berle, A. A., & Means, G. C. (1932). *The modern corporattion and private property*. New York: MacMillan.

Bhagat, S., & Bolton, B. (2008). Corporate governance and firm performance. *Journal of Corporate Finance , 14*, 257-273.

Boone, A. L., Field, L. C., Karpoff, J. M., & Raheja, C. G. (2007). The determinants of corporate board size and independence: an empirical analysis. *Journal of Financial Economics , 85* (1), 66-101.

Brickley, J. A., Coles, J. L., & Terry, R. L. (1994). Outside directors and the adoption of poison pills. *Journal of Financial Economics , 35* (3), 371-90.

Byrd, J. W., & Hickman, K. A. (1992). Do outside directors monitor managers? evidence from tender offers bids. *Journal of Financial Economics , 32* (2), 195-221.

Chhaochharia, V., & Grinstein, Y. (2009). CEO compensation and board structure. *Journal of Finance , 64* (1), 231-261.

Coles, J. L., Daniel, N. D., & Naveen, L. (2008). Boards: Does one size fit all? *Journal of Financial Economics , 87*, 329-356.

Core, J. E., Holthausen, R. W., & Larcker, D. F. (1999). Corporate governance, CEO compensation, and firm performance. *Journal of Financial Economics , 51*, 371-406.

Dahya, J., McConnell, J. J., & Travlos, N. G. (2002). The Cadbury committee, corporate performance, and top management turnover. *Journal of Finance , 67*, 461-483.

Demsetz, H., & Lehn, K. (1985). The structure of corporate ownership: causes and consequences. *Journal of Political Economy , 93*, 1155-1177.

Denis, D. J., & Denis, D. K. (1995). Performane changes following top management dismissals. *Journal of Finance , 50* (4), 1029-1057.

Denis, D. J., Denis, D. K., & Sarin, A. (1997). Ownership structure and top executive turnover. *Journal of Financial Economics , 45*, 193-221.

Duchin, R., Matsusaka, J. G., & Oguzhan, O. (2010). When are outside directors effective? *Journal of Financial Economics , 96*, 195-214.

Fama, E. (1980). Agency problems and the theory of the firm. *Journal of Political Economy , 88*, 288-307.

Fama, E. F., & Jensen, M. C. (1983). Separation of ownership and control. *Journal of Law & Economics , 26*, 301-325.

Fich, E. M., & Shivdasani, A. (2006). Are busy boards effective monitors? *The Journal of Finance , 61* (2), 689-724.

Gabaix, X., & Landier, A. (2008). Why has CEO pay increased so much? *Quarterly Journal of Economics , 123,* 49-100.

Gomper, P., Ishii, J., & Metrick, A. (2003). Corporate governance and equity prices. *Quarterly Journal of Economics , 118* (1), 1007-1155.

Goyal, V. K., & Park, C. W. (2002). Board leadership structure and CEO turnvoer. *Journal of Corporate Finance , 8*, 49-66.

Guthrie, K., Sokolowsky, J., & Wan, K.-M. (2010). CEO Compensation and Board Structure Revisited. *Journal of Finance , Forthcoming*.

Hallock, K. F. (1997). Reciprocally interlocking boards of directors and executive compensation. *Journal of Financial and Quantitative Analysis , 32* (3), 331-343.

Harris, M., & Raviv, A. (2008). A Theory of Board Control and Size. *Review of Financial Studies , 21* (4), 1797-1832.

Hazarika, S., Karpoff, J. M., & Nahata, R. (2009). Internal corporate governance, CEO turnover, and earnings management. *Journal of Financial Economics , Forthcoming*.

Hermalin, B. E. (2005). Trends in Corporate Governance. *Journal of Finance , 60* (5), 2351-2384.

Hermalin, B. E., & Weisbach, M. S. (2003). Boards of directors as an endogenously determined institution: a survey of the economic literature. *FRBNY Economic Policy Review* , 7-26.

Hermalin, B. E., & Weisbach, M. S. (1998). Endogenously chosen boards of directors and their monitoring of the CEO. *American Economic Review , 88* (1), 96-118.

Hermalin, B. E., & Weisbach, M. S. (1988). The determinants of board composition. *RAND Journal of Economics , 19* (4), 589-606.

Huson, M. R., Malatesta, P. H., & Parrino, R. (2004). Managerial succession and firm performance. *Journal of Financial Economics , 74*, 237-275.

Huson, M. R., Parrino, R., & Starks, L. T. (2001). Internal monitoring mechanisms and CEO turnover: a long-term perspective. *Journal of Finance , LVI* (6), 2265-2297.

Hwang, B.-H., & Kim, S. (2009). It pays to have friends. *Journal of Financial Economics , 93* (1), 138-158.

Jensen, M. C. (1993). The modern indsutrial revolution, exit, and the failure of internal control systems. *Journal of Finance , 48*, 831-880.

Jenter, D., & Kanaan, F. (2010). CEO Turnover and Relative Performance Evaluation. *Journal of Finance* , Forthcoming.

Jenter, D., & Lewellen, K. (2010). Performance-induced CEO Turnover. *Stanford University Working Paper* .

Kaplan, S. N., & Minton, B. A. (2011). How has CEO turnover changed? *International Review of Finance* .

Klein, A. (2002). Auditing committee, board of director characteristics, and earnings management. *Journal of Accounting and Economics , 33*, 375-400.

Klein, A. (2002). Economic determinants of audit committee independence. *The Accounting Review , 77*, 435-452.

Lel, U., & Miller, D. P. (2008). International cross-listing, firm performance and top management turnover: a test of the bonding hypothesis. *Journal of Finance , 63* (4).

Linck, J. S., Netter, J. M., & Wintoki, M. B. (forthcoming). *Endogeneity and the dynamics of internal corporate governance.* Journal of Financial Economics.

Linck, J. S., Netter, J. M., & Yang, T. (2008). The determinants of board structure. *Journal of Financial Economics , 87* (2), 308-328.

Masulis, R. W., Wang, C., & Xie, F. (2009). Agency costs at dual-class companies. *Journal of Finance , 64* (4), 1697-1727.

Masulis, R. W., & Mobbs, S. (2011). Are all inside directors the same? Do they entrench CEOs or enhance board decision making? *Journal of Finance* , Forthcoming.

Parrino, R. (1997). CEO turnover and outside succession: A cross-sectional analysis. *Journal of Financial Economics , 46* (2), 165-197.

Raheja, C. (2005). Determinant of board size and composition: A theory of corporate boards. *Journal of Financial and Quantitative Analysis , 40* (2), 283-206.

Romano, R. (2005). The Sarbanes-Oxley Act and the making of quack corporate governance. *Yale Law Review , 114*, 1521-1611.

Shivdasani, A., & Yermack, D. (1999). CEO involvement in the selection of new board members: An empirical analysis. *Journal of Finance , 54,* 1829-1853.

Warner, J., Watts, J., & Wruck, K. (1988). Stock prices and top management changes. *Journal of Financial Economics , 20,* 431-460.

Weisbach, M. S. (1988). Outside directors and CEO turnover. *Journal of Financial Economics , 20*, 431-46

CHAPTER II

INFORMATION QUALITY AND CEO TURNOVER

1. Introduction

Hiring and firing CEOs is one of the most important tasks of boards of directors. Not surprisingly, understanding CEO turnover decisions is an important research agenda in corporate governance research. Most CEO turnover models assume that boards of directors learn CEO ability or matching with the firm from firm performance[13]. The learning process is typically modeled as a Bayesian updating process. One key prediction of the Bayesian updating rule is that the board should put less weight on firm performance when it contains more noise. This implies that noise in a firm performance measure should affect the boards' learning about the CEO ability from the firm performance measure and thus sensitivity of CEO turnover to the firm performance measure. However, existing CEO turnover studies have overwhelmingly focused on the effect of corporate governance on the turnover-performance relation and so far overlooked the potential effect of information quality on this relation[14]. In this paper, I extend the existing literature by showing that information quality of stock prices (earnings) has both statistically and economically significant effect on the board's assessment of CEO ability from stock returns (earnings). The impact is more significant in firms with recently hired CEOs than in firms with relatively longer-tenured CEOs. Depending on the source of the noise, this can translate into significant economic effect on the sensitivity of CEO turnover to firm performance. These new findings change the commonly-held view that sensitivity of forced CEO turnover to firm performance is positively related to the quality of internal corporate governance unconditionally. Although quality of

---

[13] The boards could learn CEO ability or matching with the firm or both. Exactly what the board learns is unimportant here. For brevity, I will only say CEO ability in the following.
[14] See Weisbach (1988), Yermack (1996), Denis, Denis and Sarin (1997), Mikkelson and Partch (1997), Perry (1999), Huson, Parrino and Starks (2001), and Goyal and Park (2002).

internal corporate governance may be more important in explaining the turnover-performance sensitivity of relatively longer-tenured CEOs, information quality seems to be more important than governance in explaining the turnover-performance sensitivity of recently hired CEOs. Overall, the results suggest that information quality puts significant constraints on boards' ability to identify bad CEOs when they make CEO retention decisions. An important policy implication of this study is that, as internal corporate governance systems improve over time in U.S. publicly listed firms due to new regulations and investor activism, return to further improvement in internal governance structure is likely to decline, more attention should probably be given to improving information flow to the stock markets and stock market liquidity, especially as average CEO tenure declines with the improvement in internal corporate governance.

In theory, boards of directors remove a CEO when her ability or matching with the firm is below some threshold. However, since CEO ability is unobservable, the board can only learn CEO ability from other observables, among which publicly observed stock returns and accounting earnings should be important sources of information. The large attention paid to turnover-performance sensitivity in the CEO turnover literature testifies to the perceived importance of firm performance to forced CEO turnover decisions. A rational approach to model the board's learning process is to assume that it follows the Bayesian updating rule. Under this setting, the board of directors updates its estimate of the CEO ability every period when new firm performance information comes in. If the updated estimate of the CEO ability is below certain threshold, the incumbent CEO is removed and a new CEO is drawn from a pool of potential CEOs, and the learning process starts anew. Otherwise, the incumbent CEO is retained and the learning continues. One prediction from these models is that the weight the board puts on firm performance in the updating process is negatively related to the noise in the firm performance measure *certeris paribus*. Here the noise refers to the component of firm performance that is not under the control of the CEO. An important implication of this prediction is that, for the same drop in firm performance, estimate of CEO ability is adjusted down by less when the firm performance measure contains more noise than when it contains less noise. If everything else is the same, this translates into differences in turnover-performance sensitivity. In this paper, I develop a framework for measuring noise in stock returns and accounting earnings and investigate the impact of noise in firm performance on the board's updating process in detail within the framework of a Bayesian learning model. The main hypothesis that I test is that noise in stock (accounting) performance reduces the sensitivity of the board's updating on CEO ability to stock (accounting) performance.

Stock returns and accounting earnings (scaled by total assets) are the two most often used measures of firm performance in existing CEO turnover studies. In this paper, I measure stock performance by industry-adjusted stock returns over the 12 months prior to CEO turnover and accounting performance by industry-adjusted return on assets (ROA) in the year prior to CEO turnover, where industry is defined by the Fama and French 48 industry classification. Based on the intuition from Bayesian learning models, I define information quality of a firm performance measure by the component of it that is not under the control of the CEO (i.e. noise in firm performance). Information quality is considered to be higher when this noise component is lower and vice versa. I identify two sources of noises that can reduce the information quality of stock returns (earnings). The first source of noise is exogenous firm-specific shocks to stock returns (earnings) that are not under the control of the CEO. For stock returns, this source of noise is measured by the standard deviation of industry-adjusted stock returns over the 12 months prior to CEO turnover. For accounting earnings, this source of noise is measured by the standard deviation of industry-adjusted ROA over the most recent 5 years. Intutitively, firms that are subject to more exogenous firm-specific shocks should have more volatile industry-adjusted stock returns (ROA). The second source of noise arise from potential difference between observed firm performance and the unobserved true underlying performance. For stock prices, this noise has to do with the process through which firm-specific information is impounded into stock prices in the stock market. I measure the magnitude of this source of noise by stock liquidity and dispersion in analysts' earnings forecasts. In the market microstructure literature, stock liquidity is found to be positively related to the information content of stock prices (See Chordia, Roll and Subraymanyam, 2008). Dispersion in analysts' earnings forecast is a widely used proxy for noise in stock prices and mispricing (Diether et al., 2002; Gilchrist et al., 2005). For accounting earnings, this second source of noise is related to estimation errors in accruals. Thus, I measure it by the accrual quality measure (AQ) developed by Dechow and Dichev (2002) (DD).

To test the main hypothesis, I run logit regressions where I regress an indicator for forced CEO turnover in year t on firm performance, information quality and an interaction between firm performance and information quality, all measured in year $t-1$. The economic interpretation of each coefficient is tied to terms in a simple one-period Bayesian learning model of CEO turnover that I develop in the paper. According to this model, the sign and statistical significance of the coefficient of the interaction term between firm performance and information quality is used to test the main hypothesis that noise in firm performance reduces the weight on firm performance in the board's updating on its estimate of CEO ability. Supporting the main hypothesis, I find that the standard deviation of industry-adjusted stock return (for brevity, also called stock volatility in the following) (RVOL) and dispersion in analysts' earnings forecasts (DISP) are negatively related to the weight on industry-adjusted stock return in the board's updating process, while stock liquidity is positively related to the weight on industry-adjusted stock return in the board's updating process. The results are statistically significant at conventional significance levels in all specifications. As for information quality of earnings, I find that standard deviation of industry-adjusted ROA (for brevity, also called earnings volatility in the following) (EVOL) and the DD accrual quality measure (AQ) are all negatively related to the weight on industry-adjusted ROA in the board's updating process[15]. The effect of earnings volatility is statistically significant at the 1% level in all specifications, while the effect of AQ is

[15] The AQ measure is inversely related to accrual quality.

weaker but is still statistically significant at above the 10% level in one-sided test in all specifications.

The accrual quality measure (AQ) I use above captures both unintentional and intentional estimation errors in accruals. The former is related to a firm's business model and operating environment so it is often unavoidable, while the latter reflects managerial discretion. The board is unlikely to know the exact magnitude of the unavoidable estimation errors in any given year except ex post. However, the board may have inside information on the magnitude of the intentional estimation errors (i.e. discretionary accruals) in any given year. This is because, as insiders, the board should be able to gain access to the internal accounting book to find out the discretionary accruals when necessary. As a matter of fact, Hazarika et al. (2009) actually find evidence that boards of directors do recognize earnings management by managers and are more likely to fire CEOs who engage in earnings management. This difference between unavoidable and intentional estimation errors offers an interesting opportunity to examine the board's use of private information and can be used to conduct an additional test on whether information quality is important to the board's CEO retention decisions. If the board has inside information on discretionary accruals, then they should be able to undo the errors when they evaluate CEO ability based on accounting earnings. In other words, intentional estimation errors are not noise to boards of directors. As a result, discretionary accruals should appear to have no systematic effect on the board's updating on CEO ability in logit regressions. To test this prediction, I decompose the AQ measure into its innate component (iAQ) and discretionary component (dAQ) following Francis, LaFond, Olsson, & Schipper (2005) and use them to re-run the logit regressions. Like AQ, the value of iAQ (dAQ) is positively related to estimation errors in accruals and thus inversely related to accrual quality. I find that the interaction between industry-adjusted ROA and the innate component of *AQ* is positive and statistically significant (p-value 0.066) but the interaction between industry-adjusted ROA and the discretionary component of AQ is not statistically significant at conventional levels. Hence, the weaker results I obtain for AQ previously can be explained by the different effect of the unavoidable and intentional estimation errors on the board's updating process. To check the robustness of the results, I repeat the analysis using three additional proxies for discretionary accruals and find that the interaction between industry-adjusted ROA and each proxy for discretionary accruals is largely statistically insignificant. These tests lend further support to my general argument that boards of directors care about information quality when making CEO retention decisions. Not only do directors care about the quality of firm performance measures that they are less likely to have private information about, they also actively use private information to get more accurate information when they are able to.

Having established the basic results that noise in stock returns (earnings) significantly affect the board's updating on CEO ability based on stock returns (earnings) in the overall sample, I next explore how the information quality effect may differ between learning the ability of relatively new CEOs and that of relatively old CEOs. Here the new and old are in inference to their tenure in the firm not their age. There are several reasons to believe that the information quality effect should be stronger for relatively new CEOs than for relatively old CEOs. First, the board in general has less private information about new CEOs than old CEOs. Thus, the need for learning the CEO ability from firm performance is bigger with new CEOs than with old CEOs. Second, the board in general has less precise information about the ability of new CEOs than old CEOs. According to the basic Bayesian learning model, the effect of firm performance on the board's update on CEO ability should be larger when the board's prior information about the CEO ability is less precise *ceteris paribus*. As the weight on firm performance gets bigger in the board's updating process when the board's prior belief is less precise, the effect of information quality on the weight on firm performance is expected to also become more easily detectable. Third, noise related to several of my information quality proxies is likely to be amplified in firm with new CEOs than with old CEOs. For example, after a new CEO joins a firm, the stock market also needs to go through a learning process. During this process, stock prices are likely to be more subject to the influence of noise traders or market sentiments when stock liquidity is low and dispersion in analysts' earnings forecasts is high. As for estimation errors in accruals, a new CEO is likely to make larger unavoidable estimation errors than an old CEO under the same circumstances because the new CEO is less expert at forecasting.

To see if the information quality effect is stronger in firms with relatively new CEOs than old CEOs, I divide my sample of CEOs into two subsamples based on the median CEO tenure in my sample. The new CEO subsample contains CEOs with tenure less than the median (about 5.5 years), while the old CEO subsample contains CEOs with tenure equal to or above the median. I estimate similar logit regressions as before within each subsample and find a stark contrast between the two subsamples. In the new CEO subsample, for each of the six information quality proxies (including iAQ), the interaction between firm performance and the information quality proxy has the predicted sign and is statistically significant. The results actually become statistically more significant (lower p-values) than they are in the overall sample for some proxies. In contrast, in the old CEO subsample, only the interaction between industry-adjusted ROA and earnings volatility is statistically significant. The consistency in results across different information proxies, especially between the stock price information quality proxies and accounting information quality proxies, is surprising. However, it lends more credibility to the interpretation that the board learns CEO ability from firm performance because the differential effect of information quality on the board's learning of ability of new and old CEOs is a reasonable implication of the board's learning of CEO ability from firm performance. It also makes any alternative explanation of my results based on the correlation between the information quality proxies and the board's use of private information less convincing because if anything the board should have more private information about old CEOs than new CEOs, which often leads to the opposite prediction on the differential information quality effect.

Obviously, some of my information quality proxies are likely to be endogenous. Among the five information quality proxies[16], three of them are especially exposed to endogeneity problems. They are the innate component of accrual quality (iAQ), stock liquidity (LIQ) and dispersion in analysts' earnings forecasts (DISP). The main concern is that innovations in them are likely to be correlated with unobservable governance variables. The innate component of accrual quality (iAQ) is calculated based on five firm characteristics over a 10-year rolling window. The endogeneity concern is that the five firm characteristics are correlated with an unobservable corporate governance variable which drives the relation we observe. However, given the long-term nature of these firm characteristics, the unobservable corporate governance variable and thus its effect should be quite stable over time. This contradicts with the previous finding that the iAQ effect is only significant in the new CEO subsample. Hence, I can refute this endogeneity concern. For stock liquidity and dispersion in analysts' earnings forecasts, to establish causality, I implement a two-step estimator for probit models with endogenous continuous regressors (Woodridge, 2002). Like Fang, Noe and Tice (2009) and Jayaraman and Milbourn (2010), I use both lagged value of each firm's stock liquidity (dispersion in analysts' earning forecasts) and the median stock liquidity (dispersion in analysts' earnings forcasts) of the firm's Fama and French 48 industry as my instruments. Both instruments are correlated with the firm's stock liquidity (dispersion in analysts earnings forecasts) but are uncorrlated with the error terms. I find that the interaction term between stock return and stock liquidity (dispersion in analysts' earnings forecasts) is negatvie (positive) and statistically significant at above 5% level in the overall sample. Furthermore, in subsample tests, I find that the stock liquidity effect is mainly driven by the subsample of new CEOs with the coefficient of the interaction term statistically significantly at the 10 percent level for new CEOs but statsitically insignificant at conventional levels for old CEOs. For dispersion in analysts' earnings forecasts, the distinction

[16] They are stock volatility (RVOL), stock liquidity (LIQ), dispersion in analysts' earnings forecasts (DISP), earnings volatility (EVOL) and innate component of accrual quality (iAQ). Since I find that the accrual quality effect is mainly driven by unavoidable estimation errors for which iAQ is a better measure, I focus on iAQ only here.

between new and old CEOs becomes less clear cut. The coefficient of the interaction term is statistically significant at the 10 percent level for both new and old CEOs with the p-value for the old CEO subsample slightly larger. The interaction term in the old CEO subsample seemes to capture some firm performance effect because the stock return is netgatvie but statistically insignificant in the old CEO subsample while it is statistically significant at the 5 percent level in the new CEO subsample. Overall, the effect still seems to be stronger for new CEOs than for old CEOs.

As for the economic significance of the results, since the board's estimate of CEO ability is unobservable, we can only assess the economic significance through its effect on the probability of CEO turnover. If everything else is the same, the effect of information quality on the board's update on estimate of CEO ability based on firm performance should translate into a same direction effect on sensitivity of CEO turnover to firm performance. This is because, for the same drop in firm performance, estimate of CEO ability is adjusted downward by a smaller amount when information quality is lower than when information quality is higher. However, as I show through the link between my empirical model and the simple one-period model of CEO turnover, a change in the value of the information quality proxy can affect both the threshold CEO ability below which to fire the CEO (main effect) and the board's update on CEO ability based on firm performance (interaction effect). In a nonlinear model like logit, this main effect on threshold CEO ability causes a non-parallel shift in the probability of CEO turnover over firm performance, so a direct comparison of change in probability of CEO turnover for a certain change in firm performance between firms with high and low information quality captures both the main effect and the interaction effect. This explains why the marginal interaction effect calculated as the cross derivative of probability of CEO turnover with respect to firm performance and information quality in logit and probit models may have different sign from the coefficient of the interaction term. This is not a problem in linear probability models because the main effect causes a parallel shift in probability of CEO turnover over firm performance. Since my hypothesis is about the effect of information quality on the board's updating process not the effect on threshold CEO ability, the economic effect pertinent to my hypothesis should be evaluated by fixing the threshold CEO ability.

Therefore, I adopt a new method to infer the economic effect that is relevant for my hypothesis. Basically, I fix the value of the information quality proxy in the main effect term but allow the value of the information quality proxy in the interaction term to vary to capture the effect of information quality on the sensitivity of forced CEO turnover to firm performance that is purely driven by the interaction term. These calculations produce very significant economic effect for new CEOs in an average firm[17]. I find that, when the information quality proxy in the main effect term is fixed at the sample median, the difference in increase in probability of forced CEO turnover when the information quality proxy is high (the 95th percentile) and when the information quality proxy is low (the 5th percentile) for a decrease in stock return from the top to bottom decile ranges from 5.6 percent to 14 percent for the three stock price information quality proxies. Similar calculations done for accounting information quality proxies and ROA produce differences in increase in probability of forced CEO turnover in the range from 0.9 percent to 1.6 percent, which is much smaller than the numbers above. However, because change in accounting performance also has a smaller effect on probability of forced CEO turnover than change in stock performance does, the relative magnitude of those differences in increases in probability of forced CEO turnover is still economically significant. They are about 45 percent to 60 percent of the increase in probability of forced CEO turnover when the accounting information quality is low (i.e. value of the information quality proxy is at the 95th percentile).

For comparison, I also calculate the combine effect of information quality on sensitivity of forced CEO turnover to firm performance, which includes the effect on both the threshold

---

[17] Since the logit regression results show that most of the information quality proxies have statistically insignificant effect on the board's updating process in the old CEO subsample. It is uninteresting to evaluate their economic effects for old CEOs.

CEO ability and the board's updating process. This combine effect corresponds to the cross-derivative of probability of forced CEO turnover with respect to firm performance and information quality in Equation (2.14) in the paper. Intuitively, for information quality proxies that are not significantly related to the threshold CEO ability, the combined effect would be similar to the pure interaction effect. However, for information quality proxies whose effect on threshold CEO ability is significant, the combined effect will be different from the pure interaction effect. In extreme cases, the direction of the combined effect can even be opposite to that of the pure interaction effect. Consistent with this intuition, I find that the estimated combined effect is close to the pure interaction effect for stock liquidity (LIQ) and innate component of accrual quality (iAQ). The combined effect is weakened to some extent relative to the pure interaction effect but is still in the same direction as the pure interaction effect for dispersion in analysts' earnings forecasts (DISP) and accrual quality (AQ). The combine effect is significantly weakened or even reversed relative to the pure interaction effect for stock volatility (RVOL) and earnings volatility (EVOL). In summary, noise arising from stock market trading or estimation errors in accruals does not significantly affect the threshold CEO ability so their effect on the board's updating process is directly translated into the same direction effect on sensitivity of CEO turnover to firm performance across firms with different levels of noise. However, noise arising from exogenous firm-specific shocks affects both the threshold CEO ability and the board's updating process so their effect on sensitivity of CEO turnover to firm performance across firms with different levels of noise may be different from their effect on the board's updating process.

This study makes the following contributions to related literatures. First, I extend the existing CEO turnover literature by showing that information quality puts significant constraints on the board's ability to quickly identify bad CEOs when they make CEO turnover decisions. In the corporate governance literature, the strength of the relation between forced CEO turnover and firm performance is often taken to be an indicator of effectiveness of internal governance. This reflects a view that is well expressed by Jensen (1993) who commented in 1993 that the removal of CEOs after poor performance came “too late” and the effect was “too small” to meet the obligations of the board. Hence, a strengthening of internal corporate governance is expected to strengthen the relation between forced CEO turnover and poor performance. The CEO turnover literature is dominated by studies that relate board or other corporate governance characteristics to the sensitivity of forced CEO turnover to firm performance. However, estimates from this literature show only modest difference in sensitivity of forced CEO turnover to firm performance between firms with supposedly good corporate governance and firms with supposedly bad corporate governance. For example, according to a well-known study by Weisbach (1988) on outside directors and CEO turnover, moving from the top to bottom decile of market-adjusted stock return increases the probability of CEO turnover by about 6% in firms with more than 60 percent of outside directors while it increases the probability of CEO turnover by 2% in firms with less than 40 percent of outside directors. The difference is only 4%. Attributing the low annual rate of forced CEO turnover in the data entirely to agency costs, Taylor (2010) estimates that it is consistent with a CEO entrenchment cost of up to 3% firm value, a surprisingly large number. These findings raise the suspicion that maybe we have overemphasized the role of corporate governance and agency problems in CEO turnover decisions. There could be other factors that are simultaneously affecting the turnover-performance relation. This paper examines one such factor that has been overlooked by the existing CEO turnover literature. This is information quality of firm performance measures. My estimate of economic effect suggests that, for removing relatively new CEOs, the cross-sectional impact of information quality on turnover-performance sensitivity is often larger than that of having a majority of independent directors on the board. Taking the information quality proxy with the strongest effect among the five proxies I examine – stock liquidity – as an example, I find that moving from the top to bottom decile of industry-adjusted stock return increases the probability of forced CEO turnover by about 14 percent in firms with high stock liquidity (the 95th percentile) while it increases the probability of forced CEO turnover by only about 4 percent in firms with low stock liquidity (the 5th percentile). The difference is about 10 percent, which is more than twice the difference found in Weisbash (1988) between firms with outsider-dominated boards and firms with insider-dominated boards. My results suggest that information quality is likely to be more important than quality of internal governance in explaining the turnover-performance sensitivity of relatively new CEOs, while quality of internal governance is likely to be more important in explaining the turnover-performance sensitivity of relatively longer-tenured CEOs.

Second, I introduce a new framework to interpret interaction terms in logit or probit models that are widely used in CEO turnover studies. To my knowledge, this study is the first to make the distinction between the effect of a key variable that interacts with firm performance on threshold CEO ability below which the board is assumed to fire the CEO and the effect of this variable on the board's updating on estimate of CEO ability based on the firm performance. Based on this distinction, I develop a new method to infer the economic effect of the interaction term in my logit regressions, which avoids the confusions associated with interpreting interaction terms in logit and probit models as pointed out by Ai and Norton (2003) and Powers (2005)[18].

Third, this study contributes to a growing corporate finance and accounting literature that links stock price informativeness to firms' investment decisions, executive compensation decisions and firm value. Using different measures of stock price informativeness, Chen, Goldstein and Jiang (2007) and Durnev, Morck and Yeung (2004) find that more informative stock prices facilitate more efficient corporate investments. Holmstrom and Tirole (1993) establish the theoretical foundation for a link between stock liquidity and managerial incentives. Jayaraman and Milbourn (2011) find that increase in stock liquidy leads to increase in proportion of equity-based pay in total compensation and higher pay-performance sensitivity in CEO compensation. Fang, Noe and Tice (2009) find that higher stock liquidity leads to higher firm

[18] I suspect that most of the papers that are said to have misinterpreted the coefficient of the interaction terms in logit and probit models by Ai and Norton (2003) and Powers (2005) probably simply misstated their hypothesis.

value as measured by Tobin's Q. This paper contributes to this literature by showing that stock price informativeness as measured by stock liquidity and dispersion in analysts' earnings forecasts facilitates quicker CEO turnover decisions. This introduces another channel through which stock liquidity may increae firm value as documented by Fang, Noe and Tice (2009).

Lastly, this paper contributes to the accounting literature that studies the consequences of earnings quality in various decision contexts. Although this literature is very large, it is relativley thin in the area of how earnings quality affects executive turnover decisions (see review by Dechow, Ge and Schrand, 2010). Existing studies mainly focus on the labor market outcomes of executives and directors under extremely poor earning quality conditions such as after a firm has restated or been found to have misrepresented earnings (Srinivasan, 2005; Desai, Hogan and Wilkins, 2006; Karpoff, Lee and Martin, 2008). Only two existing papers examine the executive turnover consequences of earnings quality in less than extreme conditions. Engel, Hayes and Xue (2003) study earnings timeliness and the relative weight on stock returns and accounting earnings in CEO turnover decisions. Hazarika, Karpoff and Nahata (2009) examine CEO turnover after less extreme earnings management than those that result in high profile restatements, shareholders lawsuits or SEC investigations. To my knowledge, this study is the first to show that accrual quality affects the usefulness of earnings in CEO turnover decisions. Furthermore, this paper shows that the effect is different for accrual quality related to unavoidable estimation errors and accrual quality related to discretionary accruals. While the former has significant impact on the usefulness of earnings in CEO turnover decisions, the latter does not.

The rest of the paper proceeds as follows. Section 2 develops a simple Bayesian learning model of CEO turnover to guide the development of main hypothesis and the empirical model. It also develops a framework to measure information quality of stock prices and accounting earnings. Section 3 summaries the sample and explains the details of variable construction. Section 4 reports the empirical results and deals with potential endogeneity issues. Section 5 develops a method for inferring the economic effect of information quality on sensitivity of CEO turnover to firm performance that purely comes from the interaction term in the logit model and reports the economics effect of various information quality proxies. Section 6 concludes.

2. Hypotheses, empirical specification, and measures of information quality

Most existing CEO turnover models that relate CEO turnover to firm performance are built on the idea that the board of directors learns CEO ability (or matching between the CEO and the firm), which is unobservable, from observable firm performance[19]. The board of directors then updates its estiamte of the CEO ability following a Bayesian updating process as new firm performance information comes in each period. When the updated estimate of the CEO ability is below certain threshold, the incumbent CEO is removed and a new CEO is drawn from a pool of potential CEOs, and the learning process starts again. When the updated estimate of the CEO's ability is above the threshold, the incumbent CEO is retained and the learning process continues but with an updated prior that incorporates what has been learned in the previous periods. One intution from these models is that noises in firm performance reduce the effect of firm performance on the updates. The second intuition is that the longer the CEO serves, the more precise information the boads of directors should have about the CEO ability. In the following, I will show a simple one-period model to illustrate these intuitions and use this simple model to motivate my hypothesis and empirical specification. I will also develop a framework for measuring information quality of stock prices and accounting earings and discuss specific information quality measures.

2.1. A simple learning model of CEO turnover and empirical specification

At time t=0, a new CEO is hired, the prior distribution of the CEO's ability is $\alpha \sim N(a_0, 1/\tau_0)$ where τ_0 is the precision of the distribution. At time t=1, firm peformance is

[19] See Hermalin and Weisbach (1998), Bushman, Dai and Wang (2010) and Taylor(2010).

realized. Firm performance x_1 is assumed to be related to the CEO ability by the following equation:

$$x_1 = \alpha + \varepsilon \tag{2.1}$$

where ε is a random noise with a distribution of $N(0, 1/\tau_\varepsilon)$. The board of directors then updates their estimate of the CEO's ability using the standard Bayesian updating rule. The posterior distribution of the CEO's ability is thus given by $\alpha \sim N(a_1, 1/\tau_1)$ where:

$$a_1 = \left(\frac{\tau_0}{\tau_0 + \tau_\varepsilon}\right) a_0 + \left(\frac{\tau_\varepsilon}{\tau_0 + \tau_\varepsilon}\right) x_1 \tag{2.2}$$

$$\tau_1 = \tau_0 + \tau_\varepsilon \tag{2.3}$$

The CEO is replaced when $a_1$is below some threshold $\bar{a}$ and retained otherwise. I define an indicatior variable Y for CEO turnover, then the CEO turnover decision can be expressed as:

$$\text{Y=1 when } y^* = \bar{a} - \left(\frac{\tau_0}{\tau_0 + \tau_\varepsilon}\right) a_0 - \left(\frac{\tau_\varepsilon}{\tau_0 + \tau_\varepsilon}\right) x_1 > 0 \tag{2.4}$$

$$\text{Y=0 when } y^* = \bar{a} - \left(\frac{\tau_0}{\tau_0 + \tau_\varepsilon}\right) a_0 - \left(\frac{\tau_\varepsilon}{\tau_0 + \tau_\varepsilon}\right) x_1 \leq 0 \tag{2.5}$$

where y^* represents the difference between the threshold ability and the estimated CEO ability. As we can see in Equation (2.2), when the board updates its estimate of the CEO's ability, the weight on firm performance x_1 decreases with noise in x_1 (i.e. $1/\tau_\varepsilon$) and increases with noise in prior belief (i.e. $1/\tau_0$) *ceteris paribus*. The first part means that for the same poor performance x_1 the posterior mean a_1 is adjusted down by less when the noise $1/\tau_\varepsilon$ is high than when the the noise $1/\tau_\varepsilon$ is low *ceteris paribus*. The second part means that if the incumbent CEO is retained in the first period, the weight on firm performance in the updating process in second period should be smaller than it is in the first period and so on *ceteris paribus* because, as equation (2.3) shows, the prior precision at the beginning of second period is higher than that at the beginning of the first period. This illustrates the intuition behind the interplay between noise in firm performance, CEO tenure and the sensitivity of CEO turnover to firm performance that motivates the research investigation of this study. However, given the simplicity of this one-period model, it is ex ante unknown if all predictions will be borne out by the data. However, this model provides a good starting point. It suggests that noise in firm performance measures can potentially have significant effect on boards of directors' assessement of CEO ability and, in turn, on the relation between CEO turnover and firm performance. The model also makes it clear that noise in firm performance for the purpose of evaluating CEO ability is the component of firm performance that does not reflects the CEO ability.

To estimate this noise effect, I parameterize the experession for y^* in Equations (2.4) and (2.5) by introducing a variable x_2 which is a proxy for noise in firm performance x_1 and interacting it with firm performance x_1. Without agency costs, the threshold CEO ability below which to replace the CEO, $\bar{a}$, should be determined by marginal benefits and costs of CEO turnover. However, agency theory also suggests that $\bar{a}$ should be affected by agency costs in the firm. So I model $\bar{a}$ by a linear function of observables that can potentially affect the threshold and an error term. This leads to the following empirical model for y^*:

$$y^* = \beta_0 + \beta_1 x_1 + \beta_2 x_2 + \beta_{12} x_1 x_2 + Z\gamma + \delta \quad (2.6)$$

where y^* represents the unobserved difference between $\bar{a}$ and the posterior mean of CEO ability a_1. Z is a vector of observables that may affect the thresh hold $\bar{a}$. β_1 is the weigh on firm performance. $\beta_2 x_2$ captures the potential effect of the information quality proxy x_2 on the threshold $\bar{a}$. $\beta_{12} x_1 x_2$ is the term that operationalizes the idea that the weight on firm performance x_1 varies with information quality x_2. I expect $\beta_1 < 0$ because poor performance should lower the posterior mean of CEO ability and thus increases the likelihood of CEO turnover. The key coefficient for this study is β_{12} which measures the effect of noise in firm performance on the sensitivity of the board's estimate of CEO ability to firm performance. According to the basic intution from the learning model of CEO turnover, I predict that $\beta_{12} > 0$ if x_2 increases with noise in firm performance x_1 and $\beta_{12} < 0$ if x_2 decreases with noise in firm performance x_1. δ is an error term with mean 0. Equation (2.6) is not estimable because y^* is unobservable. However, if we assume that δ has a standardized logistic distribution (with known variance of $\pi^2/3$), then the parameters in equation (2.6) can be estimated by a logit regression where the probability of CEO turnover is given by:

$$P(Y = 1|X) = \Lambda(\beta_0 + \beta_1 x_1 + \beta_2 x_2 + \beta_{12} x_1 x_2 + Z\gamma) \tag{2.7}$$

where $\Lambda(X\beta) = \frac{\exp(X\beta)}{1+\exp(X\beta)}$ is the cumulative distribution function for standardized logistic distribution. Here, the assumption of known variance of the logistic distribution is without loss of generality because we can always divide both sides of Equation (2.6) by the standard deviation of δ.

This derivation makes it clear that the sign and statitical significance of the coefficient β_{12} provides a test of my main hypothesis. The warnings in Ai and Norton (2003) and Powers (2005) on interpretation of interaction terms in logit and probit models do not apply because my hypothesis is stated with respect to the latent variable y^* in Equation (2.6) not the probability of CEO turnover. This is not a matter of choice of words but a matter of precisely stating what the learning model says. Writing out the model helps me to correctly state the hypothesis. The derivation also makes it clear that information quality x_2 potentially has two distinct effects on probability of CEO turnover – one through its effect on the threshold ability $\bar{a}$ (associated with the main effect term $\beta_2 x_2$) and the other through its effect on estimating CEO ability from firm performance (assocaited with the interaction term $\beta_{12} x_1 x_2$). This distinction has important implications for calculating the economic effect of information quality on the sensitivity of CEO turnover to firm performance that I will discuss later.

2.2. Measure of information quality of stock prices

According to the Bayesian learning model, noise in stock returns for the purpose of evaluating CEO ability should be the component of stock return that is not under the control of the CEO. Since I measure firm performance by industry-median-adjusted stock returns, most of the market- and industry-wide exogenous shocks have already been filtered out. However, firms can still be subject to exogenous firm-specific shocks so the industry-adjusted stock return may not completely reflect the CEO ability. To proxy for the magnitude of noise arising from exogenous firm-specific shocks, I use the standard deviation of monthly industry-median-adjusted stock returns in the 12-month period prior to CEO turnover (for brevity, I call it stock volatility in the following). The rationale is that firms that are more exposed to exogenous firm-specific shocks are likely to have more volatile industry-adjusted stock returns. I predict that stock volatility should be negatively related to the weight on stock returns in the board's updating process.

Besides firm-specific shocks, stock market trading can also generate noises in stock returns that are not related to the CEO ability. In theory, stock price reflects the market expectation of the present value of all expected future cash flows per share of stock. The market expectation is formed when informed traders, trying to profit from their information advantage, trade with noise traders and other informed traders in the stock market. In this process, information of the informed traders is being revealed and impounded into the stock prices. When trading cost is high, it may discourage information production and subdue arbitrage activities in the market, the result is less informative stock prices (See Grossman & Stiglitz, 1980; Kyle, 1985; Shleifer and Vishny, 1997). I measure this source of noise related to stock market trading and arbitrage activities by stock liquidity.

Stock liquidity is closly related to trading costs and arbitrage activiites in the stock market. Holmstrom and Tirole (1993) in particular show that greater stock liquidity attracts informed traders to the stock and in equilibrium makes the stock price more informative. Hence, information quality of stock price should be higher when stock liquidity is higher. I measure stock liquidity by the negative of the natural logarithm of the Amihud illiquidity measure. Amihud (2002) finds that his illiquidity measure is positively and strongly correlated with the Kyle's λ estimated from intrad day transcations and quotes. Hasbrouck (2009) further finds that the

Amihud illiquidity measure is most strongly correlated with the TAQ-based price impact coefficient among the daily proxies he examined. I predict that stock liquidity is positively related to the weight on stock return in the board's update on CEO ability based on stock returns.

Moreover, this source of noise in stock prices can also related to the quality of public information. When information about the firms is less precise, stock price is likely to be less informative. I measure the noise related to public information by dispersion in analysts' earnings forecasts. Larger dispersion in analysts' earnings forecast indicates larger uncertainty over a firm's future earnings among sophisticated investors and higher level of noises in public information about the firm. Hence, information quality of stock price should be lower when dispersion in analysts' earnings forecasts is higher. Dispersion in analysts' earnings forecasts is widely used in prior literature as a proxy for information quality of stock prices and stock mispricing (See Diether et al., 2002; Gilchrist et al., 2005). I predict that dispersion in analysts' earnings forecasts should be negatively related to the weight on stock returns in the board's updating on estimate of CEO ability.

2.3. Measure of information quality of accounting earnings

Prior studies have consistently found that forced CEO turnovers are negatively related to past stock and accounting performance of firms. This suggests that, beside stock performance, boards of directors also consider accounting performance in making CEO retention decisions. Since earnings are probably the most important accounting numbers in any measure of a firm's accounting profitability[20], I use measures of earnings quality to proxy for the information quality of my accounting performance measure – industry-median-adjusted return on assets (ROA).

Under the framework of this paper, I choose to examine earnings quality that is related to exogenous firm-specific shocks and estimation errors in reported earnings. For the former, I use

[20] In a survey conducted by Graham et al. (2005), they find that the majority of the firms view earnings as a key metric for external audiences.

volatility of industry-adjusted ROA (for brevity earnings volatility in the following). It is calculated as the standard deviaiton of annual earnings before interests and taxes (EBIT) deflated by lagged total assets over the most recent 5 years. Data for the calculation are from the Compustat database. Similar to volatility of industry-adjusted stock returns, I expect that earnings volatility to be positively related to exogenous firm-specific shocks to the firm. Hence, I predict that earnings volatility should be negatively related to the weight on ROA in the board's update on estimate of CEO ability based on ROA.

For the latter, I use accrual quality. Accruals are the differences between reported earnings and realized cash flows. The objective of using accruals-based accounting rather than cash-flow-based accounting is to better reflect a firm's economic condition and financial performance. However, recording accruals requries managers to make forecasts for furture cash flows. This introduces estimation errors into accruals. These estimation errors can be unavoidable, for example, because a firm's business model or operating environment makes it difficult for managers' to accurately forecast future cash flow realizations. In this case, the estimation errors represent honest reporting errors. On the other hand, managers may intentionally introduce estimation errors into accruals in order to achieve certain objectives, often for gainning private benefits at the expense of shareholders. These accruals are often called discretionary accruals (DA).

My first measure of accrual quality is based on the model of Dechow and Dichev (2002), which I call the DD meaasure of accrual quality. Dechow and Dichev (2002) model accruals as a function of past, present and future cash flows because accruals represent anticipations for future cash collection or payments and reverse when previously recoganized cash is received or paid. The quality of the matching from accruals to cash flows, which is measured by the standard deviation of the error terms from a regression of current accruals on past, current and future cash flows, thus gives an indication of the magnitude of the estimation errors in accruals. A smaller standard deviation represents higher accrual quality. I predict that the DD accrual quality measure should be negatively related to the weight on ROA in the board's updating on estimate of CEO ability based on ROA.

The DD accrual quality measure calculated above does not distinguish between unavoidable estimation errors related to a firm's business model or operating environment and intertional estimation errors related to manageiral discretion and opportunism. Since the boards of directors may have private information about discretionary accruals, they may treat the two types of estimation errors differently. My main hypothesis on boards' learning of CEO ability from firm performance clearly suggests that the unavoidable estimation errors should reduce the weight on ROA in the board's updating process; however, the effect of discretionary accruals will depend on the boards of directors' knowledge of the discretionary accruals. If the board is able to undo the intentional estimation errors based on their knowledge of them, then empirically discretionary accruals should appear to have no systemtic effect on the board's updating on CEO ability based on ROA. To test these two predictions, I decompose the AQ measure into its innate component and discretionary component following the procedure in Francis et al. (2005) and re-run the logit regressions using them as the information quality measure respectively. To check the robutness of the results, I also repeat the analysis using two additional proxies for discretionary accruals – one based on the modified Jones model and the other based on the performance-matched approach of Kothari, Leone and Wasley (2005).

3. Sample and variable construction

My CEO turnover data is constructed from the ExecuComp database and it covers CEO turnovers in S&P 1500 firms between 1992 and 2008. I exclude CEO turnovers that are due to mergers and acquistions and spin-offs as well as turnovers of interim CEOs and co-CEOs. This data is then matched with stock return data from CRSP, firms financial data from Compustat and corporate governance data from RiskMetrics, all in the year prior to the CEO turnover announcement, to obtain the base sample. The base sample has 2,936 firms with 23,536 firm-year observations from 1992 to 2007 and contains 2,471 CEO turnovers that are announced in year 1993 through 2008.

I then calculate a series of information quality measures and add them to the base sample. Stock liquidity is calculated using CRSP daily stock data, dispersion in analysts' earnings forecasts is calculated using data in the First Call database and accrual quality measures are calculated using Compustat data.

3.1. Classification of CEO turnover

CEO turnovers are classified as either voluntary or forced following the method in Parrino (1997). Specifically, if news articles mention that the CEO is fired, forced out, or departs due to unspecified policy differences, the CEO turnover is classified as forced. Otherwise, if the CEO is 60 years old or above, then the CEO turnover is voluntary. For all remaining cases, the CEO turnover is classified as voluntary if the CEO departure is due to poor health, death or acceptance of another position within the firm or outside or if the CEO is stated to retire and the retirement is announced at least 6 months before the succession. The rest of the cases are classified as forced. Finally, forced CEO turnover can be reclassified as voluntary if an extensive news search finds new information that suggests the CEO departure is unrelated to firm activities. This method is designed to sovle the problem that firms rarely admit that they fire CEOs. The method has been widely used in CEO turnover studies such as Huson, Parrino and Starks (2001), Hazarika, Karpoff, & Nahata (2009), and Jenter and Kannan (2010), etc.

The sample has 2,471 CEO turnovers announced from 1993 to 2008, of which 1,791 are classified as voluntary and 680 are classified as forced. According to this data, CEO turnovers happen in about 12 percent of the firms each year and forced CEO turnovers happen in about 3 percent of the firms each year in my sample. These values are a little higher than those found in a sample of 1,627 CEO turnovers from 1993 to 2001 by Jenter and Kannan (2010). In their sample, CEO turnovers happen in about 10 percent of the firms each year and forced CEO turnovers happen in about 2.3 percent of the firms each year. The small increase is consistent with a strengthening of internal corporate governance in U.S. firms since the passage of the Sarbanes-Oxley Act (SOX).

3.2. Firm performance

Prior studies have shown that the likelihood of forced CEO turnovers is negatively related to both stock and accounting performance of a firm. I measure stock performance by industry-median-adjusted stock return and accounting performance by industry-median-adjusted return on assets (ROA) in this study. To calculate industry-adjusted stock returns, I subtract from each sample firm's monthly stock return the median return of firms in the same Fama and French 48 industry and cumulate the difference over the 12-month period that ends one month before the CEO turnover announcement in years associated with CEO turnover and over the fiscal year in years associated with no CEO turnover. The industry-median-adjusted stock return is winsorized at the two 1% tails. To calculate industry-adjusted ROA, I first deflate each firm's annual earnings before interest and tax (EBIT) by lagged total assets and then subtract from it the median ratio of the firm's Fama and French 48 industry over the fiscal year that best reflects the information available to the board when they make the CEO turnover decision. If a CEO turnover is announced in the first half of a fiscal year, the appropriate fiscal year is the prior fiscal year. If the CEO turnover is announced in the second half of a fiscal year, the appropriate fiscal year is the current fiscal year. This measure is not affected by changes in capital structure and tax treatments of the firms. It was used in prior CEO turnover studies such as Parrino (1997) and Huson, Parrino, & Starks (2001). Another widely used measure of accounting-based performance in CEO turnover studies is change in ROA (See Weisbach, 1988; DeFond and Park, 1999; Engel et al., 2001). Weisbach (1988) argues that change in ROA should be a better performance measure because it captures the unexpected change in performance. I choose to use the ROA measure instead because my information quality measures – earnings volatility and accrual quality – directly measure the information quality of annual earnings rather than change in annual earnings. The industry-median-adjusted ROA is winsorized at the two 0.5 percent tails.

3.3. Construction of stock price information quality proxies

Stock volatility (RVOL) is calculated as the standard deviation of monthly industry-median-adjusted stock returns in the 12-month period over which the stock performance is measured.

Stock liquidity (LIQ) is calculated as the negative of the natural log of the Amihud illiquidity measure for the 12-month period over which the stock performance is measured. Following Amihud (2002), stock illiquidity in a specific firm-year in my sample is calculated as the daily ratio of absolute daily stock return over daily dollar trading volume averaged over the year. Data on stock returns and trading volumes come from CRSP database. The average ratio is then multiplied by a factor of 10^6 to give the measure an interpretation of average daily price change associated with per million dollar trading volume. Intuitively, it is a measure of the average daily price impact of trade. Higher Amihud illiquidity means higher price impact of trade and thus lower stock liquidity. The following is the formula used to calculate the Amihud illiquidity for firm i in year y:

$$ILLIQ_{iy} = 10^6 \times \frac{1}{D_{iy}} \sum_{d=1}^{D_{iy}} \frac{|R_{iyd}|}{VOL_{iyd}} \tag{2.8}$$

where R_{iyd} is the daily return of stock i in year y on day d, VOL_{iyd} is the corresponding daily trading volume in dollars, and D_{iy} is the number of trading days in the year. Following Fang, Noe and Tice (2009), I require that at least 200 days of trading data be availabel and stock price be above $5.

It is well known that trading volume for Nasdaq stocks is overstated relative to NYSE and AMEX stocks because inter-dealer trades are included in calculation of trading volume on

Nasdaq but not on NYSE and AMEX. To make the Amihud illiquidity measures for Nasdaq stocks and NYSE stocks comparable, I follow a practice widely used in the literature by cutting the Nasdaq trading volume by half when calculating Amihud illiquidity for Nasdaq stocks (See Loughran and Marietta-Westberg, 2005).

The Amihud illiquidity measure in my sample has a skewness of about 28 and a kurtosis of about 1074. To reduce the skewness of the stock illiquidity measure and the influence of extreme values, I take natural logarithm of the calculated Amihud measure and add a negative sign in front to convert it to a continuous measure of stock liquidity.

The dispersion in analysts' earnings forecasts is calculated using data from the First Call Historical Database (FCHD). FCHD contains a history of First Call's Real Time Earnings Estimates (RTEE) as far back as 1990 over 9,700 securities, among which 8,500 are U.S. securities. To construct my measure of dispersion in analysts' earnings forecasts for a firm-year in my sample, I first scale the standard deviation of analysts' one-year-ahead earnings forecasts issued in that year by the corresponding mean earnings forecasts. Then, this ratio is averaged over all one-year-ahead earnings forecasts issued in that year. Data on the mean earnings forecasts and the standard deviations of earnings forecasts are from the summary statistics file of FCHD. Unlike I/B/E/S database in which the summary statistics are calculated using all outstanding forecasts, the FCHD summary statistics are calculated using only the most recent estimate of each broker, so we do not have to worry about the stale forecasts problem that researchers have met when using I/B/E/S. All estimate and actual data are adjusted for stock splits and stock dividends. To reduce the skewness of the measure and the influence of extreme observations, I take the natural log of the dispersion in analysts' earnings forecasts calculated above and use it as the measure of information quality of stock prices (DISP).

3.4. Construction of accounting information quality proxies

I follow Francis et al. (2005) to calculate the DD accrual quality metric. Francis et al. (2005) augment the model of Dechow and Dichev (2002) by including growth in revenue and PP&E in the model of normal accruals because McNichols (2002) shows that adding these two terms significantly improves the explaining power of the model and thus reduces measurement errors. Specifically, using all firms in the Compustat database, I estimate the following cross-sectional regression for each industry-year with valid data for at least 20 firms, where industry is defined by the Fama and French 48 industry classification:

$$TCA_{i,t} = b_0 + b_1 CFO_{i,t-1} + b_2 CFO_{i,t} + b_3 CFO_{i,t+1} + b_4 \Delta Rev_{i,t} + b_5 PPE_{i,t} + \varepsilon_{i,t} \tag{2.9}$$

where $TCA_{i,t}$ is total current accrual and it equals to total accrual $TA_{i,t}$ plus depreciation and amortized expenses $DP_{i,t}$. $CFO_{i,t}$ is operating cash flow from continuing operations, $\Delta Rev_{i,t}$ is annual revenue growth and $PPE_{i,t}$ is gross property, plant and equipment. All variables are deflated by the average total assets in year t. Hribar and Collins (2002) recommend to use the statement of cash flows data to calcualte accruals to avoid measurement errors. Hence, I use the statement of cash flow approach to calculate accruals. In this approach, total accrual $TA_{i,t} = IBC_{i,t} - CFO_{i,t}$ where $IBC_{i,t}$ is earnings before exordinary items and discontinued operations and $CFO_{i,t}$ is operating cash flow from continuing operations which is calculated as the difference between $OANCF_{i,t}$ (net operating cash flow) and $XIDOC_{i,t}$ (cash flow from exordinary items and discontinued operations). All items are from the statement of cash flows.

To reduce the influence of potential outliers, I truncate my sample at the two 1% tails of $TCA_{i,t}$ before running the regression. The accrual quality metric (AQ) for firm i in year t is then calculated as the standard deviation of the residuals from the regressions over years $t-4$ through t, i.e. $AQ_{i.t} = \sigma(\upsilon_{i,s})$ where $\upsilon_{i,s}$ is the residual from the regression in year s. Since the estimation of AQ in year t requires information on cash flow in year $t+1$, I use the AQ calculated for year

$t-1$ to proxy for potential estimation errors in year t because it represents the best estimate the boards of directors can have in year t. Note that the AQ metric is negatively related to accrual quality, i.e. high AQ means low accrual quality and vice versa. I predict that AQ should be negatviely related to the weight on ROA in the board's updating on estimte of CEO ability based on ROA.

To decompose the AQ measure into its innate component and discretionary component, I follow the procedure in Francis et al. (2005) to regress the DD accrual qualit metric AQ on the five firm characteristics identified by Dechow and Dichev (2002) to affect a firm's propensity to make unavoidable estimation errors. Specifically, I run the following annual cross-sectional regressions using all firms in the Compustat database for each year in my sample period:

$$\begin{aligned} AQ_{i,t} \quad = \quad & \lambda_0 + \lambda_1 Size_{i,t} + \lambda_2 \sigma(Sales)_{i,t} + \lambda_3 \sigma(CFO)_{i,t} \\ & + \lambda_4 OperCycle_{i,t} + \lambda_5 NegEarn_{i,t} + \mu_{i,t} \end{aligned} \tag{2.10}$$

where $Size_{i,t}$ is firm size measured as the natural log of total assets, $\sigma(Sales)_{i,t}$ is the standard deviation of firm i's sales revenues. $\sigma(CFO)_{i,t}$ is the standard deviation of firm i's cash flow from operations. $OperCycle_{i,t}$ is the log of firm i's operating cycles, calcualted as 360/(Sales/Average Accounts Receivables) + 360/(Cost of goods sold)/(Average Inventory) in year t. $NegEarn_{i,t}$ is the incidence of negative earnings, i.e. income before extraordinary items < 0. Besides firm size, each variable is measured on a firm-specific basis over 10-year rolling windows (I require at least five valid observations in each window). The predicted value from Equation (2.10) is the estimate of the innate component of firm i's accrual quality in year t (iAQ):

$$\begin{aligned} iAQ_{i,t} = \hat{\lambda}_0 + \hat{\lambda}_1 Size_{i,t} + \hat{\lambda}_2 \sigma(Sales)_{i,t} + \hat{\lambda}_3 \sigma(CFO)_{i,t} \\ + \hat{\lambda}_4 OperCycle_{i,t} + \hat{\lambda}_5 NegEarn_{i,t} \end{aligned} \tag{2.11}$$

I use the value of iAQ in year $t-1$ to be my proxy for potential unavoidable estimation errors in the reported earnings in year t. Since the right-hand-side variables change slowly over time, I expect this to be a good proxy for unavoidable estimation errors in year t. The residual from Equation (2.10) is the discretionary component of AQ, denoted by dAQ in this paper, i.e. $dAQ_{i,t} = \hat{\mu}_{i,t}$.

There is a shortcoming with the discretionary accruals measure dAQ calculated above for measuring discretionary accruals in a given year. Strictly speacking, dAQ is a measure of discretionary accruals over the period it is being estiamed. It does not measure the magnitude of discretionary accruals in the year it is caclucated for. Whether high discretionary accruals in the past five years imply high discretionary accruals in the current year is uncertain. This is especially true if the current year operation is under a new CEO. To addresses this shortcoming and check the robustness of my results to different discretionary measures, I estimate two alternative measures of discretionary accruals.

The first discretionary accruals measure is based on the modified Jones model as shown in Equation (2.12) (see Dechow, Sloan and Sweeney, 1995).

$$TA_{i,t} = \phi_{0,j} + \phi_{j,1}\left(\frac{1}{Assets_{i,t}}\right) + \phi_{j,2}\left(\Delta Rev_{i,t} - \Delta AR_{i,t}\right) + \phi_{j,3}PPE_{i,t} + \upsilon_{i,t} \qquad (2.12)$$

where $TA_{i,t}$ is total accruals and is calculated using data from statement of cash flows as I discussed before. $Assets_{i,t}$ is average total assets, $\Delta AR_{i,t}$ is change in accounts receivable, $\Delta Rev_{i,t}$ is annual revenue growth and $PPE_{i,t}$ is net plant, property and equipments. All variables except the inverse of average total assets are deflated by the average total assets in year t. To reduce the influence of outliers, the sample is truncated at the two 1% tails of $TA_{i,t}$.

I run annual cross-sectional regressions of Equation (2.12) for each Fama and French 48 industry with valid data for 20 or more firms on Compustat over 1992 to 2007. The estimated discretionary accrual for firm i in year t is simply the residual $\hat{\upsilon}_{i,t}$ in the equation. Since large positive and negative $\hat{\upsilon}_{i,t}$'s all indicate high abnormal accruals and low accrual quality, I use the absoluate value of $\hat{v}_{i,t}$ as the unsigned discretionary accrual measure from the modified Jones model.

Kothari, Leone, and Wasley (2005) argue that the residuals from the Jones and modified Jones model may be correlated with firm performance so they recommend to control for the normal level of accruals conditional on firm performance where firm performance is measured by ROA (annual net income divided by average total assets). My second discretionary accruals are based on this performance-matched apporach. Specifically, I identify a firm from the same Fama and French 48 industry with the closest ROA to the sample firm as the control firm and then deduct the residual from Equation (2.12) for the control firm from that for the sample firm to get the performance-matched discretionary accruals for the sample firm. It is important to note that this measure can actually add noise and reduce the power of test when performance is not an issue (see Dechow, Ge and Schrand, 2010).

Controlling for firm performance can also be done under a linear regression approach, though according to Kothari et al. (2005) the performance-matched approach above should perform better because it allows for nonlinear effect of firm performance on discretionary accruals. However, for comparison with results in Hazarika, Karpoff and Nahata (2009) who use the linear regression approach to calculate discretionary accruals of Kothari et al. (2009), I also estimate a discretionary accruals measure where I control for firm performance by adding the current year ROA to the modified Jones model in Equation (2.12) as follows:

$$TA_{i,t} = \phi_{0,j} + \phi_{j,1}\left(\frac{1}{Assets_{i,t}}\right) + \phi_{j,2}\left(\Delta Rev_{i,t} - \Delta AR_{i,t}\right) + \phi_{j,3}PPE_{i,t} + \phi_{j,4}ROA_{i,t} + v_{i,t} \tag{2.13}$$

where $ROA_{i,t}$ is return on assets calculated as the net income in year t devided by average total assets in year t. All other variables are defined as in Equation (2.12). Unlike Hazarika et al. (2009) who match on the prior year $ROA_{i,t-1}$, I use current year $ROA_{i,t}$ because Kothari et al.

(2005) find that matching based on current year $ROA_{i,t}$ peforms better than matching on prior year $ROA_{i,t-1}$.

3.5. Descriptive statistics

Table 2.1 reports the descriptive statistics of the full sample. Panel A reports statistics of firm and corporate governance variables, while Panel B reports statistics of information quality variables. Table 2.2 reports the descriptive statistics of firm and governance characteristics by new and old CEOs, where the new CEO subsample consists of CEOs whose tenure is below the sample median (about 5.5 years) and the old CEO subsample consists of CEOs whose tenure is at or above the sample median. As we can see, firm characteristics such as total assets, sales, Tobin's Q, leverage, etc. are very similar between the two subsamples. However, there do seem to be some differences in corporate governance characteristics between the two subsamples. A higher percentage of firms in the new CEO subsample have a majority-independent board, a separate CEO and Chairman and a non-founder-related CEO. Also, new CEOs are on avearge younger and hold lower fraction of voting power. The average (median) tenure of new CEOs is 2.67 (2.58) years while that for old CEOs is 12.89 (10.50) years. Table 2.3 reports descriptive statistics of information quality variables by new and old CEOs. As we can see, they are very similar across the two subsamples. Table 2.4 reports the correlations among the information quality variables. The Spearman correlation coefficients, which measure correlations in ranks of the variables, are highly significant for all pairs of variables, while the Pearson correlation coefficients, which measure linear correlations, are highly significant for all pairs except two. Although these variables are highly correlated, I find that they have quite different economic effects on turnover-performance sensitivity based on coefficient estimates from regressions where they are included one at a time.

Table 2.1: Descriptive statistics

The sample consists of 2,936 firms on ExecuComp database from 1992 to 2007 with a total of 23,536 firm-year observations. *Total assets, Sales, Market value* and *Net PPE* are in $ millions. *N* is the number of observations with non-missing values. *Tobin's Q* is calculated as total assets minus book value of equity plus market value of equity divided by total assets. *Leverage* is calculated as the sum of debt in current liabilities plus long term debt divided by total assets. *Independent board* is a dummy variable that equals to one if more than half of the directors are independent. *CEO duality* is a dummy variable that equals to one if the CEO also serves as the Chairman of the board. *Founder* is a dummy variable that equals to one if the CEO is a founder or is from a founder's family. *CEO voting* is the percentage of voting power held by the CEO. *Hceoown* is a dummy variable that equals to one if CEO voting power is above 3 percent. *Nonemployee block* is a dummy for the presence of a nonemployee blockholder on the board. *Return* is the industry-median-adjusted stock return in the year prior to CEO turnover. *ROA* is the industry-median-adjusted ROA in the year prior to CEO turnover. Industry is defined by the Fama and French 48 industry classification. *RVOL* is the standard deviation of monthly industry-adjusted stock returns in the year prior to CEO turnover. *LIQ* is the negative of the natural logarithm of Amihud illiquidity over the year prior to CEO turnover. *DISP* is the natural logarithm of dispersion in analysts' one-year-ahead earnings forecasts in the year prior to CEO turnover. *EVOL* is the standard deviation of annual industry-adjusted ROA in the 5 years prior to CEO turnover. *AQ* is the accrual quality measure calculated based on the Dechow and Dichev (2002) (DD) model using the most recent 5 years of data. *iAQ* is the innate component of *AQ*. *dAQ* is the discretionary component of *AQ*. *MJ_DA* is the absolute value of discretionary accruals in the year prior to CEO turnover calculated based on the modified Jones model. *PM_DA* is the absolute value of discretionary accruals in the year prior to CEO turnover calculated using the performance-matched approach of Kothari et al. (2005). LN_DA is the absolute value of discretionary accruals in the year prior to CEO turnover calculated based on the linear regression approach of Kothari et al. (2005).

	Mean	Std Dev	P1	Median	P99	N
Panel A: Firm and governance variables						
Total assets	12105.04	61502.91	51.32	1541.28	194716.00	22197
Sales	4861.21	14080.56	18.52	1240.70	56434.00	22191
Market Value	6901.01	22330.25	35.20	1470.29	100240.30	22161
Net PPE	1821.19	5598.31	1.70	294.11	22801.00	21708
Tobin's Q	2.07	2.40	0.79	1.51	9.16	22158
Leverage	0.24	0.21	0.00	0.22	0.85	22111
Board size	9.64	2.85	5.00	9.00	18.00	14574
Independent Board	0.80	0.40	0	1	1	14574
CEO duality	0.64	0.48	0	1	1	14574
CEO age	55.44	7.42	39.00	56.00	75.00	21957
CEO tenure (years)	7.78	7.45	0.50	5.42	35.92	22198
Founder	0.14	0.35	0	0	1	22198
CEO voting	3.48	9.36	0.00	0.00	52.60	14161
Hceoown	0.50	0.50	0	0	1	22198
Nonemployee block	0.27	0.44	0	0	1	14574
Return	0.05	0.39	-0.71	-0.01	1.78	21258
ROA	0.07	0.14	-0.27	0.04	0.57	21577
Panel B: Information quality variables						
RVOL	0.094	0.057	0.022	0.079	0.324	21258
LIQ	5.812	2.095	0.763	5.901	10.282	20791
DISP	-3.039	1.245	-5.015	-3.295	0.862	15576
EVOL	0.054	0.060	0.003	0.034	0.347	19417
AQ	0.048	0.047	0.005	0.035	0.258	13951
iAQ	0.047	0.030	0.008	0.040	0.157	13769
dAQ	0.001	0.036	-0.073	-0.003	0.137	13769
MJ_DA	0.059	0.072	0.001	0.039	0.327	20228
PM_DA	0.080	0.089	0.001	0.054	0.425	20228
LN_DA	0.049	0.056	0.001	0.034	0.259	20228

Table 2.2: Descriptive statistics of firm and governance characteristics by new and old CEOs

This table is the same as Table 2.1 except the sample is broke up at the median CEO tenure. The new CEOs subsample contains CEOs whose tenure is below the sample median, while the old CEOs subsample contains CEOs whose tenure is equal to or above the sample median. All variables are defined in Table 2.1.

	Mean	Std Dev	P1	Median	P99	N
Panel A: New CEOs						
Total assets	13324.96	70251.49	43.11	1686.30	219232.00	11112
Sales	5332.28	14687.18	15.01	1344.10	59917.00	11107
Market Value	7064.39	22322.09	24.82	1480.29	99744.79	11088
Net PPE	1997.06	5780.97	1.50	318.08	23640.00	10890
Tobin’s Q	2.03	2.81	0.77	1.48	9.19	11088
Leverage	0.24	0.21	0.00	0.22	0.92	11071
Board size	9.79	2.72	5.00	10.00	18.00	7252
Independent Board	0.83	0.38	0	1	1	7252
CEO duality	0.51	0.50	0	1	1	7252
CEO age	53.40	6.62	38.00	54.00	69.00	10990
CEO tenure (years)	2.67	1.45	0.42	2.58	5.42	11112
Founder	0.06	0.23	0	0	1	11112
CEO voting	1.13	4.46	0.00	0.00	20.80	6941
Hceoown	0.42	0.49	0	0	1	11112
Nonemployee block	0.28	0.45	0	0	1	7252
Return	0.04	0.39	-0.71	-0.01	1.74	10468
ROA	0.06	0.14	-0.31	0.03	0.57	10782
Panel B: Old CEOs						
Total assets	10882.15	51229.78	56.38	1402.78	188874.00	11085
Sales	4389.15	13429.34	22.69	1154.83	51794.00	11084
Market Value	6737.40	22338.24	48.61	1465.26	103341.30	11073
Net PPE	1644.15	5402.69	1.91	266.32	22029.00	10818
Tobin’s Q	2.11	1.90	0.81	1.55	9.16	11070
Leverage	0.23	0.22	0.00	0.21	0.78	11040
Board size	9.50	2.96	5.00	9.00	19.00	7322
Independent Board	0.77	0.42	0	1	1	7322
CEO duality	0.77	0.42	0	1	1	7322
CEO age	57.48	7.62	40.00	58.00	79.00	10967
CEO tenure (years)	12.89	7.54	5.58	10.50	39.50	11086
Founder	0.23	0.42	0	0	1	11086
CEO voting	5.74	11.93	0.00	1.80	63.82	7220
Hceoown	0.57	0.50	0	1	1	11086
Nonemployee block	0.26	0.44	0	0	1	7322
Return	0.05	0.39	-0.70	0.00	1.79	10790
ROA	0.07	0.14	-0.22	0.04	0.58	10795

Table 2.3: Descriptive statistics of information quality variables by new and old CEOs

The new CEOs subsample contains CEOs whose tenure is below the sample median, while the old CEOs subsample contains CEOs whose tenure is equal to or above the sample median. All variables are defined in Table 2.1.

	Mean	Std Dev	P1	Median	P99	N
Panel A: New CEOs						
RVOL	0.095	0.059	0.022	0.078	0.324	10468
LIQ	5.872	2.106	0.847	5.963	10.370	10202
DISP	-2.985	1.295	-5.011	-3.259	0.961	7676
EVOL	0.054	0.060	0.004	0.034	0.347	9473
AQ	0.050	0.050	0.005	0.036	0.266	6834
iAQ	0.047	0.031	0.008	0.040	0.158	6757
dAQ	0.003	0.037	-0.072	-0.002	0.149	6757
MJ_DA	0.061	0.079	0.001	0.039	0.352	10143
PM_DA	0.080	0.092	0.001	0.053	0.437	10143
LN_DA	0.050	0.059	0.001	0.034	0.266	10143
Panel B: Old CEOs						
RVOL	0.093	0.055	0.023	0.079	0.317	10790
LIQ	5.754	2.082	0.615	5.842	10.143	10589
DISP	-3.091	1.193	-5.027	-3.317	0.760	7900
EVOL	0.055	0.060	0.003	0.034	0.347	9944
AQ	0.047	0.045	0.005	0.035	0.247	7117
iAQ	0.047	0.028	0.009	0.041	0.157	7012
dAQ	0.000	0.035	-0.074	-0.004	0.119	7012
MJ_DA	0.057	0.065	0.001	0.039	0.299	10085
PM_DA	0.080	0.086	0.001	0.055	0.407	10085
LN_DA	0.049	0.052	0.001	0.034	0.254	10085

Table 2.4: Correlation coefficients between information quality variables

All variables are defined in Table 2.1. Pearson correlations are reported above the main diagonal and Spearman correlations are reported below the main diagonal. All correlations are statistically significant at above 1% level except for the Pearson correlations between *DISP* and *dAQ* and between *iAQ* and *dAQ* which are statistically insignificant at conventional levels.

	RVOL	LIQ	DISP	EVOL	AQ	iAQ	dAQ	MJ_DA	PM_DA	LN_DA
RVOL		-0.30	0.34	0.44	0.35	0.48	0.05	0.34	0.24	0.27
LIQ	-0.36		-0.25	-0.11	-0.09	-0.29	0.12	-0.10	-0.07	-0.09
DISP	0.30	-0.28		0.26	0.23	0.36	0.00	0.18	0.13	0.14
EVOL	0.44	-0.11	0.28		0.48	0.61	0.11	0.29	0.21	0.28
AQ	0.37	-0.12	0.20	0.53		0.64	0.77	0.29	0.21	0.28
iAQ	0.51	-0.36	0.35	0.58	0.60		0.00	0.29	0.20	0.28
dAQ	-0.05	0.20	-0.08	0.07	0.57	-0.22		0.12	0.09	0.12
MJ_DA	0.26	-0.08	0.12	0.27	0.26	0.25	0.07		0.47	0.72
PM_DA	0.17	-0.05	0.06	0.16	0.18	0.17	0.05	0.27		0.52
LN_DA	0.21	-0.06	0.09	0.24	0.25	0.23	0.07	0.39	0.31	

4. Empirical Results

4.1. Stock price information quality effects

Table 2.5 presents the main results on the effect of stock price information quality on the board's updating on estimate of CEO ability based on industry-adjusted stock returns. Here, stock volatility is used to proxy for exogenous firm-specific shocks that are not under the control of the CEO, and stock liquidity and dispersion in analysts' earnings forecasts are used to proxy for noise in stock prices due to trading and imprecise information. Since board data on RiskMetrics was not available until 1996, I estimate two specifications for each information quality proxy – one without controls for board characterstics and the other with them. The former is estimated using data from 1992 to 2007, while the latter is estimated using data from 1996 to 2007. In the former specification (columns 1, 3 and 5), I only control for firm size and CEO age in order to maximize the number of observations. In the latter specification (columns 2, 4 and 6), I add controls for board independence, CEO-Chairman duality, CEO voting power, CEO founder status, nonemployee blockholders on board and the interaction between board independence and stock return[21].

In panel A of Table 2.5, I report results estimated by including year and Fama and French 48 industry fixed effects. The results show that coefficient on stock return is negative and statistically significant in all specificaiton, indicating that stock return is negatively related to the likelihood of forced CEO turnover. Consistent with my main hypotheses, the interactions between stock return and stock volatility and dispersion in analysts' earnings forecasts are positive and statistically significant at above 5% significance levels in all specifications, suggesting a negative relation between the weight on stock return in the board's updating process and stock volatility and dispersion in analyst's earnings forecasts. Similarly, the interaction between stock return and stock liquidity is negative and statistically significant at above 5% significance levels in all

[21] Adding additional control variables such as Tobin's Q, leverage, number of antitakeover provisions, and interaction terms between CEO voting right, dual CEO, founder CEO and stock returns does not change the results.

Table 2.5: Stock price information quality effects

This table shows the coefficient estimates from logit regressions where the dependent variable is an indicator for forced CEO turnover and the key explanatory variables are the information quality proxy, *Info*, and the interaction between stock return and information quality, *Return*Info*. The particular information quality proxy used is shown on top of each column. All other variables are defined in Table 2.1. For each proxy, I show two specifications with different sample sizes. Columns 1, 3 and 5 are estimated using data from 1992 to 2007, while columns 2, 4 and 6 are estimated using data from 1996 to 2007 because corporate governance data is unavailable until 1996. Year and industry fixed effects are included in all columns. In Panel A, industry fixed effects are controlled at the Fama and French 48 industry level. In panel B, industry fixed effects are controlled at the Fama and French 17 industry level and standard errors are clustered by firm. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels respectively in two-sided tests.

Panel A: Fama and French 48 industry fixed effects

	RVOL		LIQ		DISP	
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Return	-3.837***	-3.780***	-1.620***	-1.636**	-1.954***	-1.971***
	(0.000)	(0.000)	(0.000)	(0.015)	(0.000)	(0.001)
Info	7.155***	7.506***	-0.114***	-0.148***	0.271***	0.304***
	(0.000)	(0.000)	(0.007)	(0.007)	(0.000)	(0.000)
Return*Info	9.490***	7.502***	-0.188**	-0.225**	0.254**	0.341**
	(0.000)	(0.000)	(0.015)	(0.025)	(0.028)	(0.013)
Logat	0.090***	0.101***	0.153***	0.152**	0.052	0.052
	(0.002)	(0.007)	(0.005)	(0.025)	(0.152)	(0.245)
Age	-0.031***	-0.028***	-0.035***	-0.032***	-0.029***	-0.018*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.055)
Indepboard		0.145		0.108		0.137
		(0.390)		(0.545)		(0.522)
Return* Indepboard		0.257		0.079		0.078
		(0.535)		(0.875)		(0.886)
CEO duality		-0.330***		-0.354***		-0.391***
		(0.002)		(0.002)		(0.002)
Founder		-1.322***		-1.457***		-1.182***
		(0.000)		(0.000)		(0.000)
Hceoown		-0.543***		-0.562***		-0.707***
		(0.001)		(0.001)		(0.000)
Nonemployee block		0.165		0.077		0.128
		(0.154)		(0.535)		(0.343)
Constant	-3.203***	-19.462***	-2.273*	-	-1.208	-
				18.090***		18.008***
	(0.006)	(0.000)	(0.051)	(0.000)	(0.330)	(0.000)
Observations	22081	14665	21057	14141	15727	11036
Chisq	665.0	559.4	484.0	473.4	469.6	426.6
Pseudo R-squared	0.115	0.142	0.0964	0.133	0.118	0.148

Table 2.5, continued

Panel B: Fama and French 17 industry fixed effects and clustered errors by firm

	RVOL		LIQ		DISP	
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Return	-3.830***	-3.757***	-1.576***	-1.659*	-1.963***	-2.054***
	(0.000)	(0.000)	(0.007)	(0.051)	(0.000)	(0.001)
Info	7.689***	8.175***	-0.081*	-0.106*	0.275***	0.296***
	(0.000)	(0.000)	(0.055)	(0.067)	(0.000)	(0.000)
Return*Info	9.426***	7.581***	-0.206**	-0.233*	0.262**	0.335**
	(0.000)	(0.000)	(0.042)	(0.095)	(0.033)	(0.026)
Log total assets	0.097***	0.106***	0.106**	0.102	0.052	0.146
	(0.001)	(0.002)	(0.036)	(0.116)	(0.150)	(0.515)
CEO age	-0.034***	-0.032***	-0.039***	-0.038***	-0.032***	0.104
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.865)
Indepboard		0.149		0.092		0.048
		(0.390)		(0.630)		(0.261)
Return*Indepboard		0.206		0.060		-0.023***
		(0.640)		(0.920)		(0.002)
CEO duality		-0.353***		-0.384***		-0.412***
		(0.001)		(0.001)		(0.002)
Founder		-1.299***		-1.427***		-1.159***
		(0.000)		(0.000)		(0.000)
Hceoown		-0.533***		-0.551***		-0.671***
		(0.000)		(0.001)		(0.001)
Nonemployee block		0.158		0.051		0.112
		(0.160)		(0.665)		(0.385)
Constant	-3.210***	-2.962***	-2.085***	-1.990***	-1.295**	-1.767**
	(0.000)	(0.000)	(0.000)	(0.002)	(0.043)	(0.013)
Observations	22075	14686	21054	14157	15728	10962
Chisq	542.5	441.7	343.1	329.7	401.4	314.3
Pseudo R-squared	0.107	0.133	0.0874	0.121	0.108	0.135

specificaitons, suggesting a positive relation between the weight on stock returns and stock liquidity. As for the control variables, log of total assets is positively related to forced CEO turnover and is statistically significant in four out of the six specification. This is consistent with findings in Huson, Parrino, & Starks (2001) who find that larger firms tend to have more independent boards, lower CEO ownership and higher institutional ownership. Hence, the log of total assets in the parsimonious specifications in columns 1, 3 and 5 also serve as a control for corporate governance. CEO duality, Founder, and the high CEO ownership dummy are all negatively related to forced CEO turnover, suggesting that CEOs who are Chairmen of the board, who are founders or who are from the founders family, or who have larger voting powers are less likely to be forced out (Denis, Denis, & Sarin, 1997; Goyal & Park, 2002).

In unreported results, I also estimate logit regressions similar to those in Table 2.5 but use clustered standard errors by firm. I find that the coefficient estimates and their statistical significance are similar to those in Table 2.5 but for unknown reasons the model-fit Chi-square statistics are missing in Stata outputs when Fama and French 48 indusry fixed effects are included. Hence, in Table 2.6, I report results estimated using industry fixed effects at the Fama and French 17 industry level and clustered standard errors by firm. As we can see, the coefficients on the interaction between stock return and stock liquidity (dispersion in analysts' earnings forecasts) are negative (positive) and statistically significant at the 5% level across the columns except in column 4 where the use of clustered standard error reduces the statistical significance of the interation between stock return and stock liquidity from the 5% level to the 10% level.

4.2. Accounting information quality effects

Table 2.6 presents the main results on the effect of accounting information quality on the board's updating on estimate of CEO ability based on industry-adjusted ROA. Here, earnings volatility is used to proxy for exogenous firm-specific shocks that are not under the control of the CEO and the DD accrual quality measure, AQ, is used to proxy for estimation errors in accruals. Similar to Table 2.5, I estimate two specifications for each proxy – one with controls for only firm size and CEO age and the other with additional controls for board and CEO characteristics. The former is estimated using data from 1992 to 2007 while the latter is estimated using data from 1996 to 2007. Year and industry fixed effects are included in all columns where industry is defined by the Fama and French 48 industry classification.

First, we note that prior year industry-adjusted ROA is negatively related to the likelihood of forced CEO turnover in all columns. This is consistent with findings in prior studies that CEOs in firms with poor accounting performance are more likely to be removed. Turning to earnings volatility and AQ, we observe that the interaction between ROA and earnings volatility is statistically significant at the 1% level in both specifications (columns 1 and 2), the positive sign of the coefficient is consistent with a lower weight on ROA when earnings volatility is higher. The interaction between industry-adjusted ROA and AQ is statistically significant at the 5% level in column 3 but becomes statistically insignificant (p-value 0.15) in column 4 when the sample size is reduced and other control variables are included, though the sign of the interaction is consistent with my hypothesis in both columns. Overall, the results are highly significant for earnings volatility but are somewhat weaker for AQ in the full sample.

As I explained previously, the AQ measure captures both unavoidable estimation erros and intentiaonal estimation errors. If the board of directors cannot distinguish the two types of errors, I expect the two types of errors to have similar effect on boards' learning of CEO ability from accounting earnings. The weaker results for AQ above would imply that estimation errors in accruals only have weak effect on the board's learning of CEO ability from earnings. However, if the board of directors has inside information about the magnitude of intentional estimation errors, then they should be able to undo the intentional estimation errors, and as a result, intentional estimation errors should appear to have no systematic effect on the board's learning of CEO ability from accounting earnings. In this case, the weaker results for AQ above are likely to be driven by its inclusion of intentional estimation errors.

To test if the board of directors treats the two types of estimation errors differently and the differential effects of the two types of errors are driving the weaker results for AQ above, I decompose the AQ measure into its innate component and discretionary component following Francis et al. (2005) and repeat the logit regressions in columns 3 and 4 for AQ by using the two components of AQ as the information quality proxy respectively. The innate component of AQ, iAQ, is a proxy for unavoidable estimation errors and the discretionary component of AQ, dAQ, is a proxy for intentional estimation errors or discretionary accruals. The regression results are reported in columns 5 through 8 in Table 2.6. Consistent with the board treating the two types of

Table 2.6: Accounting information quality effects

This table shows the coefficient estimates from logit regressions where the dependent variable is an indicator for forced CEO turnover and the key explanatory variables are the information quality proxy, *Info*, and the interaction between ROA and information quality, *ROA*Info*. The particular information quality proxy used is shown on top of each column. All other variables are defined in Table 2.1. For each proxy, I show two specifications with different sample sizes. Columns 1, 3 and 5 are estimated using data from 1992 to 2007, while columns 2, 4 and 6 are estimated using data from 1996 to 2007 because corporate governance data is unavailable until 1996. Year and industry fixed effects are included in all columns where industry fixed effects are controlled at the Fama and French 48 industry level. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels respectively in two-sided tests.

	EVOL		AQ		iAQ		dAQ	
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ROA	-5.651***	-6.167***	-4.748***	-4.850***	-6.113***	-5.725***	-4.081***	-4.163***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Info	-0.547	-0.692	0.722	-0.271	-3.622*	-4.718*	2.240**	1.170
	(0.484)	(0.525)	(0.434)	(0.843)	(0.091)	(0.096)	(0.047)	(0.450)
ROA*Info	12.569***	18.516***	8.205**	9.118	21.335***	17.586*	2.297	2.237
	(0.000)	(0.000)	(0.037)	(0.147)	(0.007)	(0.093)	(0.682)	(0.786)
Log total assets	0.030	0.047	0.052	0.066	0.013	0.035	0.045	0.062
	(0.320)	(0.224)	(0.145)	(0.134)	(0.745)	(0.473)	(0.204)	(0.161)
CEO age	-0.038***	-0.030***	-0.044***	-0.036***	-0.046***	-0.038***	-0.045***	-0.036***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Indepboard		0.019		0.010		0.024		0.023
		(0.881)		(0.942)		(0.861)		(0.867)
ROA*Indepboard		-0.429		-0.474		-0.618		-0.592
		(0.631)		(0.622)		(0.528)		(0.537)
CEO duality		-0.245**		-0.271**		-0.263**		-0.278**
		(0.028)		(0.026)		(0.032)		(0.023)
Founder		-1.282***		-1.301***		-1.330***		-1.304***
		(0.000)		(0.000)		(0.000)		(0.000)
Hceoown		-0.479***		-0.549***		-0.504***		-0.511***
		(0.003)		(0.002)		(0.005)		(0.005)
Nonemployee block		0.170		0.108		0.131		0.133
		(0.148)		(0.406)		(0.317)		(0.311)
Constant	-1.534*	-17.181***	-1.473	-17.414***	-0.431	-16.825***	-0.925	-17.393***
	(0.096)	(0.000)	(0.198)	(0.000)	(0.715)	(0.000)	(0.420)	(0.000)
Observations	19591	13727	13907	10849	13735	10724	13735	10724
Chisq	346.4	321.0	291.3	277.8	289.9	280.7	282.8	276.5
Pseudo R-squared	0.0656	0.0864	0.0725	0.0904	0.0731	0.0920	0.0713	0.0906

estimation errors differently, I find that the interaction between ROA and the innate component of AQ is positive and statistically significant at the 10% level or better in both specifications. In contrast, the interaction between ROA and the discretionary component of AQ is statistically insignificant in either specification. Hence, the effect of AQ in columns 3 and 4 is completely driven by the innate component of AQ. It is worth to note that Hazarika, Karpoff and Nahata (2009) also find that the board has inside information about discretionary accruals.

In unreported results, I also cluster standard errors by firm and find that the statistic significance of the interaction terms actually becomes stronger. However, somehow, the reported model fit Chi-square statistic from Stata is missing when clustered errors are used together with Fama and French 48 industry fixed effects so I do not report these results. Unlike the case with stock price information quality proxies, it is important to maintain the Fama and French 48 industry fixed effects when accrual qulity is used as the information quality proxy because the AQ measure is estimated by running annual cross-sectional regressions within each Fama and French 48 industry. Some of the variations in AQ therefore reflect varying degree of model fit across industry and year rather than mismatch between accruals and cash flows so it is important to remove them from the AQ by using the right industry fixed effects.

4.3. Information quality effects in the new and old CEO subsamples

So far, I have shown that my full sample results are consistent with the predictions of Bayesian learning models of CEO turnover. In this section, I explore a further implication of the board's learning about CEO ability from firm performance to better understand the board's learning process and its implications for turnover-performance sensitivity.

If the board does learn CEO ability from firm performance as I have shown above, then there are several reasons to believe that the effect of information quality on the board's learning process should in general be stronger for newly hired CEOs (I call them new CEOs in the following) than for relatively longer-tenured CEOs (I call them old CEOs in the following). First, the board usually has less precise information about the ability of new CEOs than old CEOs. According to the intuition from the basic Bayesian learning model I present in Section 2.2, this means that the board's update on CEO ability is likely to be more sensitive to firm performance for new CEOs than for old CEOs and thus the effect of information quality on the updating process is more easily identifiable for new CEOs. Second, publicly observable stock and accounting performance is likely to be a more important source of information to the board of directors in firms with new CEOs than in firms with old CEOs because the board of directors has yet to acquire more private information about the newly hired CEO. As a result, the board is likely to pay more attention to noise in stock and accounting performance in firms with new CEOs than in firms with old CEOs. Stock performance can also be more important for evaluating new CEOs than old CEOs because stock market and boards of directors are more or less on equal footing in terms of information about newly hired CEOs. In this situation, the aggregation role of stock market gives the stock market an advantage over the boards of directors because it can aggregate information from far more sources and thus potentially produce more accurate information about the newly hired CEO. Third, the same levels of stock liquidity and dispersion in analysts' earnings forecasts are likely to be associated with larger noise in stock prices in firms with new CEOs than in firms with old CEOs because the stock market has less precise prior on the CEO ability and is thus subject to greater influence by noise traders and market sentiments in firms with new CEOs. For accounting information quality, the same firm characteristics-driven propensity for making unavoidable estimation errors is also likely to be associated with larger estimation errors in accruals in firms with new CEOs than in firms with old CEOs because the new CEO is less expert at forecasting. Overall, the arguments for differential effects for new and old CEOs are stronger for stock price information quality proxies than for accounting information quality proxies and are stronger for noise related to discrepancy between observed performance and true underlying performance than for noise related to exogenous shocks to firm performance.

To test the differential effects of information quality of stock prices in firms with relatively new and old CEOs, I separate CEOs in my sample into two groups based on their tenure. The new CEO group consists of CEOs whose tenure is below the sample median, which is about 5.5 years, while the old CEO group consists of CEOs whose tenure is equal to or above the sample median[22]. I then re-estimate the logit regressions before using each of the two subsamples of CEOs. The use of median tenure ensures that the two subsamples have similar sample sizes so that any difference in statistical significance between the two subsamples is unlikely to be driven by differences in sample size. The median CEO tenure of 5.5 years also seems to be a good cutoff point in itself because five-year intervals are often used by individuals and institutions to classify career stages. Table 2.7 reports the subsample results for stock volatility, stock liquidity and dispersion in analysts' earnings forecasts separately. The information quality variable being tested is shown on top of each column and is denoted by *info* collectively in the list of independent variables. Fama and French 17 industry and year fixed effects are included in all columns. Also, standard errors are clustered by firm in all columns[23].

A clear contrast emerges from results in Table 2.7: the coefficient estimates of the interaction between information quality and industry-adjusted stock return are statistically significant at the 5% level for *new* CEOs across the three proxies of stock price information quality but are statistically insignificant for *old* CEOs also across the three proxies of stock price information quality. The control variables in general have the expected signs and some are statistically significant. If we compare the control variables across the two subsamples of CEOs, we can observe some interesting patterns. For example, the CEO-Chairman duality indicator is statistically significant at above 5% level for new CEOs but statistically insignificant for old CEOs. This may reflect the fact that the board has more confidence in new CEOs who are given

[22] Here the term new and old are used for convenience, they are not based on age even though it is true that CEOs with longer tenure tend to be older in age.
[23] Results based on Fama and French 48 industry and year fixed effects but no clustered standard errors are stronger. To save space, I don't report them but are available upon requests.

the Chairman title so they are less likely to be fired. Or being the Chairman makes significant differences for new CEOs to entrench themselves but not so much for long-tenured CEOs whose long tenure already gives them large influence over the board. In contrast, if a CEO is a founder or comes from a founder's family, the CEO is less likely to be replaced regardless of his/her tenure.

Table 2.8 reports the subsample results for earnings volatility and accrual quality. For earnings volatility, the interaction between industry-adjusted ROA and earnings volatility is statistically significant in both the new CEO and old CEO subsamples so there is no clear indication of differential effects for new and old CEOs. Since my arguments for differential effects for new and old CEOs are about a general pattern rather than a rule that applies to all information quality proxies, and also the original arguments for differential effect of earnings volatility are not among the strongest, this finding does not refute my hypothesis. Moving to columns 2 and 3, for AQ, I find that the interaction between industry-adjusted ROA and AQ is statistically significant in the new CEO subsample but is insignificant in the old CEO subsample, which is consistent with the general pattern that the information quality effect should be stronger when the CEO is relatively new. Comparing this result with that in Table 2.6, we can see that the weak result on AQ in Table 2.6 is also driven by the fact that the full sample includes old CEOs. When I decompose the AQ into its innate and discretionary component in columns 5 through 8, I find that the interaction between industry-adjusted ROA and iAQ is significant in the new CEO subsample but is insignificant in the old CEO subsample, while the interaction between industry-adjusted ROA and dAQ is statistically insignificant in both the new and old CEO subsamples, which provides further evidence that the board treats unintentional and intentional estimation errors differently.

Interestingly, the coefficient on dAQ is statistically insignificant in the new CEO subsample but is positive and statistically significant in the old CEO subsample. The result in the

Table 2.7: Stock price information quality effects by new and old CEOs

This table shows the coefficient estimates from logit regressions as those in Table 2.5 by new and old CEO subsamples. The new CEO subsample contains CEOs whose tenure is below the sample median (about 5.5 years) and the old CEO subsample contains CEOs whose tenure is at or above the sample median. As in previous tables, *Info* is the information quality proxy and *Return*Info* represents the interaction between stock return and the information quality proxy. The particular information quality proxy used is shown on top of each column. All other variables are defined in Table 2.1. Year and industry fixed effects at the Fama and French 17 industry level are included in all columns. Standard errors are clustered by firm. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels respectively in two-sided tests.

	RVOL		LIQ		DISP	
	New CEO	Old CEO	New CEO	Old CEO	New CEO	Old CEO
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Return	-3.599***	-3.892***	-0.924	-3.524**	-2.138***	-2.349**
	(0.000)	(0.001)	(0.304)	(0.022)	(0.007)	(0.042)
Info	7.851***	6.616***	-0.041	-0.149	0.266***	0.333***
	(0.000)	(0.002)	(0.564)	(0.130)	(0.000)	(0.000)
Return*Info	8.040***	4.300	-0.328**	0.020	0.446**	0.073
	(0.001)	(0.341)	(0.036)	(0.931)	(0.021)	(0.735)
Indepboard	0.060	0.349	0.042	0.232	0.167	0.200
	(0.765)	(0.346)	(0.849)	(0.555)	(0.556)	(0.625)
Return*Indepboard	0.317	0.232	0.361	-0.225	0.749	-0.634
	(0.514)	(0.808)	(0.597)	(0.848)	(0.332)	(0.579)
Log total assets	0.046	0.184***	-0.047	0.289***	0.006	0.119
	(0.287)	(0.004)	(0.584)	(0.009)	(0.912)	(0.124)
CEO age	-0.021**	-0.048***	-0.023**	-0.055***	-0.007	-0.046***
	(0.024)	(0.000)	(0.021)	(0.000)	(0.526)	(0.000)
CEO duality	-0.412***	-0.277	-0.439***	-0.250	-0.433***	-0.388
	(0.003)	(0.177)	(0.003)	(0.247)	(0.010)	(0.102)
Founder	-1.507***	-0.994***	-1.472***	-1.130***	-1.845***	-0.814**
	(0.003)	(0.000)	(0.003)	(0.001)	(0.009)	(0.012)
Hceoown	-0.329	-0.654***	-0.233	-0.790***	-0.512	-0.670**
	(0.165)	(0.004)	(0.362)	(0.003)	(0.102)	(0.010)
Nonemployee block	0.211	0.096	0.108	0.014	0.213	-0.044
	(0.139)	(0.630)	(0.467)	(0.948)	(0.183)	(0.848)
Constant	-3.131***	-2.400**	-2.114**	-2.076*	-1.687*	0.516
	(0.000)	(0.029)	(0.013)	(0.079)	(0.067)	(0.662)
Observations	6844	7098	6560	6872	5107	5358
Chisq	261.2	191.3	196.6	162.6	174.3	156.1
Pseudo R-squared	0.116	0.150	0.102	0.147	0.124	0.155

Table 2.8: Accounting information quality effects by new and old CEOs

This table shows the coefficient estimates from logit regressions as those in Table 2.6 by new and old CEOs. The new CEO subsample contains CEOs whose tenure is below the sample median (about 5.5 years) and the old CEO subsample contains CEOs whose tenure is at or above the sample median. As in previous tables, *Info* is the information quality proxy and *ROA*Info* represents the interaction between stock return and the information quality proxy. The particular information quality proxy used is shown on top of each column. All other variables are defined in Table 2.1. Year and industry fixed effects at the Fama and French 48 industry level are included in all columns. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels respectively in two-sided tests.

	EVOL		AQ		iAQ		dAQ	
	New CEO	Old CEO	New CEO	Old CEO	New CEO	Old CEO	New CEO	Old CEO
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ROA	-5.363***	-7.391***	-5.130***	-3.718**	-6.553***	-3.774*	-4.057***	-4.151***
	(0.000)	(0.000)	(0.000)	(0.038)	(0.000)	(0.077)	(0.000)	(0.004)
Info	-0.748	-1.503	-3.345*	3.454*	-9.579**	1.463	-1.248	4.633*
	(0.591)	(0.411)	(0.096)	(0.071)	(0.012)	(0.740)	(0.545)	(0.054)
ROA*Info	16.770***	20.644**	15.616*	-9.629	23.460*	-4.141	6.647	-11.333
	(0.001)	(0.035)	(0.058)	(0.536)	(0.066)	(0.855)	(0.491)	(0.544)
Indepboard	-0.089	0.175	-0.221	0.406*	-0.247	0.457*	-0.229	0.458*
	(0.586)	(0.392)	(0.217)	(0.080)	(0.167)	(0.052)	(0.202)	(0.052)
ROA*Indepboard	0.091	-1.407	0.416	-2.669	0.522	-3.085*	0.440	-3.031*
	(0.932)	(0.400)	(0.723)	(0.129)	(0.659)	(0.081)	(0.704)	(0.091)
Log total assets	-0.007	0.089	-0.013	0.143*	-0.063	0.138*	0.004	0.112
	(0.898)	(0.155)	(0.826)	(0.051)	(0.321)	(0.084)	(0.951)	(0.128)
CEO age	-0.018*	-0.041***	-0.027**	-0.046***	-0.028**	-0.049***	-0.025**	-0.048***
	(0.098)	(0.002)	(0.031)	(0.003)	(0.024)	(0.001)	(0.046)	(0.001)
CEO duality	-0.331**	-0.073	-0.231	-0.345	-0.228	-0.337	-0.253	-0.342
	(0.024)	(0.713)	(0.150)	(0.103)	(0.157)	(0.111)	(0.116)	(0.106)
Founder	-1.524***	-0.972***	-2.102***	-0.890***	-2.174***	-0.897***	-2.078***	-0.884***
	(0.003)	(0.000)	(0.004)	(0.002)	(0.003)	(0.002)	(0.004)	(0.002)
Hceoown	-0.251	-0.655***	-0.319	-0.822***	-0.185	-0.804***	-0.234	-0.812***
	(0.310)	(0.004)	(0.261)	(0.002)	(0.509)	(0.002)	(0.408)	(0.002)
Nonemployee block	0.222	0.142	0.125	0.080	0.167	0.093	0.153	0.093
	(0.144)	(0.472)	(0.456)	(0.719)	(0.323)	(0.675)	(0.367)	(0.675)
Constant	-17.527***	-16.430***	-16.942***	-15.941	-16.101***	-15.944***	-17.269***	-15.695***
	(0.000)	(0.000)	(0.000)	(.)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	6322	6558	5028	5252	4972	5190	4972	5190
Chisq	160.8	179.5	137.7	168.3	145.0	162.4	136.9	165.9
Pseudo R-squared	0.0771	0.118	0.0798	0.134	0.0845	0.130	0.0798	0.133

old CEO subsample is consistent with Hazarika et al. (2009) who find that discretionary accruals significantly increase the likelihood of CEO turnover. One reason for the contrast between the results in the new and old CEO subsamples is that AQ (thus dAQ) is calculated using data in the previous 5 years so dAQ really measures the discretionary accruals in the past 5 years. All old CEOs can be held responsible for the discretionary accruals happened during the previous five years but not all new CEOs. A second reason may be that only old CEOs have the influence and power to successfully manipulate earnings.

As I just alluded, one problem with using the discretionary component of AQ above to proxy for discretionary accruals in the year prior to CEO turnover is that the measure is estimated using data over a 5-year period prior to the year the measure is used for. Although it is reasonable to assume that past unavoidable estimation errors related to slowly moving firm characteristics predict future unavoidable estimation errors, it is less clear if past intentional estimation errors predict future intentional errors, especially when there is a change of CEO. To further confirm the differential effects of unavoidable estimation errors and intentional estimation errors, I construct three additional measures of discretionary accruals which are calculated using just data in the year the measure is used for. With these new measures, I repeat the analysis on discretionary accruals above. The three measures of discretionary accruals are constructed using modified Jones model, the performance-matched approach of Kothari et al. (2005) and the linear regression approach of Kothari et al. (2005). They are discussed in detail in Section 3. Table 2.9 reports the coefficient estimates of logit regressions using the absolute value of these three discretionary accruals for the full sample and the new and old CEO subsamples. Within the new and old CEO subsamples, the interaction between industry-adjusted ROA and the unsigned discretionary accruals measure is statistically insignificant at conventional levels for all three discretionary accruals measures. In the full sample, only the interaction between industry-adjusted ROA and the unsigned discretionary accruals estimated by the linear regression approach of Kothari et al. (2005) is statistically significant at the 10% level. Overall, the results continue to support differential

Table 2.9: A robustness check on the effect of discretionary accruals

This table shows the coefficient estimates from logit regressions where the dependent variable is an indicator for forced CEO turnover and the key explanatory variables are the absolute value of a measure of discretionary accruals, *Abs(DA)*, and the interaction between ROA and the discretionary accruals measure, *ROA*Abs(DA)*. The particular discretionary accruals measure used is shown on top of each column. All other variables are defined in Table 2.1. The new CEO subsample contains CEOs whose tenure is below the sample median (about 5.5 years) and the old CEO subsample contains CEOs whose tenure is at or above the sample median. Year and Fama and French 48 industry fixed effects are included in all columns. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels respectively in two-sided tests.

	MJ_DA			PM_DA			LN_DA		
	All CEO	New CEO	Old CEO	All CEO	New CEO	Old CEO	All CEO	New CEO	Old CEO
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
ROA	-3.854***	-3.453***	-3.525**	-3.423***	-3.030***	-3.506**	-3.949***	-3.751***	-3.334**
	(0.000)	(0.000)	(0.017)	(0.000)	(0.002)	(0.015)	(0.000)	(0.000)	(0.030)
Abs(DA)	1.389**	0.585	2.973***	1.123**	0.983	1.928**	1.963**	1.443	3.327**
	(0.021)	(0.445)	(0.006)	(0.023)	(0.107)	(0.032)	(0.012)	(0.138)	(0.026)
ROA*Abs(DA)	4.920	3.069	2.423	1.097	-0.294	0.565	7.357*	7.631	0.742
	(0.108)	(0.441)	(0.679)	(0.661)	(0.926)	(0.911)	(0.061)	(0.139)	(0.935)
Indepboard	0.027	-0.115	0.347*	0.022	-0.124	0.352*	0.028	-0.105	0.349*
	(0.827)	(0.469)	(0.099)	(0.861)	(0.434)	(0.093)	(0.821)	(0.507)	(0.097)
ROA* Indepboard	-0.449	0.407	-3.009**	-0.463	0.462	-3.021**	-0.522	0.327	-3.122**
	(0.586)	(0.685)	(0.049)	(0.571)	(0.641)	(0.047)	(0.525)	(0.745)	(0.039)
Log total assets	0.043	-0.005	0.097	0.042	-0.004	0.092	0.049	0.002	0.101*
	(0.239)	(0.910)	(0.111)	(0.246)	(0.940)	(0.128)	(0.181)	(0.961)	(0.098)
CEO age	-0.030***	-0.023**	-0.038***	-0.031***	-0.023**	-0.040***	-0.030***	-0.022**	-0.038***
	(0.000)	(0.034)	(0.004)	(0.000)	(0.035)	(0.002)	(0.000)	(0.038)	(0.003)
CEO duality	-0.299***	-0.422***	-0.102	-0.298***	-0.422***	-0.101	-0.293***	-0.418***	-0.088
	(0.006)	(0.003)	(0.605)	(0.006)	(0.003)	(0.610)	(0.007)	(0.004)	(0.656)
Founder	-1.389***	-1.660***	-1.084***	-1.404***	-1.674***	-1.119***	-1.392***	-1.648***	-1.082***
	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.002)	(0.000)
Hceoown	-0.497***	-0.330	-0.651***	-0.494***	-0.342	-0.628***	-0.500***	-0.332	-0.643***
	(0.001)	(0.174)	(0.004)	(0.002)	(0.160)	(0.005)	(0.001)	(0.172)	(0.004)
Nonemployee block	0.120	0.167	0.156	0.120	0.165	0.145	0.124	0.171	0.159
	(0.303)	(0.260)	(0.433)	(0.302)	(0.266)	(0.467)	(0.285)	(0.247)	(0.425)
Constant	-17.284***	-17.559***	-17.665***	-17.325***	-17.693***	-17.558***	-17.414***	-17.710***	-17.712***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	13546	6423	6351	13546	6423	6351	13546	6423	6351
Chisq	322.6	159.8	181.8	321.4	161.3	179.7	325.6	163.2	180.0
Pseudo R-squared	0.0846	0.0736	0.121	0.0843	0.0743	0.119	0.0854	0.0752	0.119

effects of unavoidable estimation errors and intentional estimation errors. Besides, I find that all three unsigned measures of discretionary accruals are positively related to the likelihood of forced CEO turnover in the full sample and their coefficients are all statistically significant at 5% level, which confirms the results in Hazarika et al. (2009), Interestingly, the subsample results suggest that the statistical significance of the unsigned discretionary accrulas in the full sample is completely driven by the subsample of old CEOs. This seems to provide more support to the Hazarika et al. (2009) interpretation of their results that the board punishes CEOs for earnings management because longer-tenured CEOs are more likely to have the power and influence to manipulate earnings than newly hired CEOs.

Overall, in this section, I find a consistent pattern across my stock and accounting information quality proxies that information quality tends to have a stronger effect on the board's updating process when the CEO is relatively new than when the CEO is relatively longer-tenured.

4.4. Distinguishing the information quality effects coming from different sources of noises

Although the information quality proxies are supposed to capture different sources of noises and are motivated and calculated quite differently, they are in general highly correlated with each other (see Table 2.4). This makes it difficult to isolate their individual effect if we include them simultaneously in the same regression. This is one reason why I have chosen to include them one at a time in regressions. Keeping this difficulty in mind, in this section, I provide some evidence on to what extent the statistical significance of individual effect is affected when I include proxies for both sources of noises in stock (accounting) performance measures simultaneously in the same logit regression. For stock prices, the two sources of noises are noise related to exogenous shocks and the noise related to information content of stock prices. In Table 2.10, I report results from logit regressions where stock volatility and stock liquidity or dispersion in analysts' earnings forecasts are simultaneously included in the same logit regression for the full sample and the new and old CEO subsamples. Year and Fama and French 48 industry fixed effects are included in all columns so the full sample results can be directly compared with those in Panel A of Table 2.5. As expected, in the full sample, the statistical significance of all key interaction terms falls in Table 2.10 from those in Table 2.5. However, the interaction between stock return and stock volatility remains significant at the 5% level in columns 1 and 4. The statistical significance of interaction between stock return and stock liquidity or dispersion in analysts' earnings forecasts falls by a larger amount but remains statistically significant at the 10% level in one-sided tests. The evidence suggests that the results I obtain on stock liquidity and dispersion in analysts' earnings forecasts in previous tables are not completely driven by their correlation with stock liquidity. The stable effect of stock volatility from Table 2.5 to Table 2.10 is expected because, in theory, both exogenous firm-specific shocks and noises from trading or information uncertainty contribute to stock return volatility so stock returns volatility subsumes some of the effect of stock liquidity and dispersion in analysts' earnings forecasts. Moving to the subsample results in columns 2 and 3 for stock liquidity and columns 5 and 6 for dispersion in analysts' earnings forecasts, I find that, consistent with my previous results, the interaction between stock return and stock volatility is statistically significant only in the new CEO subsample, while the interaction between stock return and stock liquidity (dispersion in analysts' earnings forecasts) becomes more statistically significant with a p-value of 0.10 (0.11) in the new CEO subsample and statistically insignificant in the old CEO subsample.

For accounting earnings, the two sources of noise are respectively noise associated with exogenous firm-specific shocks and noise associated with accrual quality. In unreported results, I find that when earnings volatility is simultaneously included with the DD accrual quality measure, AQ, in the same logit regression, all statistical significance goes to the interaction between industry-adjusted ROA and earnings volatility. This is not completely surprising because earnings volatility is likely to subsume the effect of accrual quality when they are both included in the same regression because estimation errors tend to increase the volatility of earnings. Although this does not refute the AQ effect, because earnings volatility is the strongest instrument for AQ according to Dechow and Dichev (2002) thus a significant earnings volatility effect is also consistent with a significant AQ effect, it does testify to the difficulty in trying to disentangle the effects associated with the two sources of noises. To see if the effect of earnings volatility is broader than just that of AQ, I run an OLS regression where earnings volatility is regressed on AQ and a series of year and industry dummies. The residual from this regression is taken to be the component of earnings volatility that is uncorrelated with AQ. Then I run logit regressions of CEO turnover in which I simultaneously include this residual component and the AQ measure as well as their respective interactions with industry-adjusted ROA to see if the interaction between industry-adjusted ROA and the residual component of earnings volatility is statistically significant. If yes, then we can say that earnings volatility and AQ are not the same and that earnings volatility has an effect on the board's updating process that is independent of the effect of AQ. Table 2.11 reports the results from this logit regression for the full sample and the new and old CEO subsamples. In the full sample, the interaction between industry-adjusted ROA and the residual of earnings volatility has a p-value of close to 0.01, while the interaction between industry-adjusted ROA and AQ has a p-value of 0.13. In the new CEO subsample, the interaction between industry-adjusted ROA and the residual of earnings volatility has a p-value of 0.13 while the interaction between industry-adjusted ROA and AQ has a p-value of 0.06. In the old CEO subsample, neither interaction is statistically significant at above 10% level in one-sided tests. Hence, there is some evidence that the effect of earnings volatility is broader than that through AQ[24]. The contrast between the new and old CEO subsample results is consistent with the general pattern I find before that information quality is more important when learning the ability of new CEOs.

In this section, I have only tried to distinguish the effect of different information quality proxies by looking at their statistical correlations with the board's updating process. Obviously, given the considerable correlations among the information quality proxies, it is difficult to clearly

[24] Similar analysis is also conducted for iAQ instead of AQ in which I find similar pattern of results.

Table 2.10: Simultaneous effects of stock information quality proxies

This table shows coefficient estimates from logit regressions with two information quality proxies in the same regression. New and old CEOs subsamples are defined as in Table 2.7. In columns 1-3, stock liquidity is interacted with stock return, while in columns 5-7, dispersion in analysts' earnings forecasts is interacted with stock return. All variables are defined in Table 2.1. Year and Fama and French 48 industry fixed effects are included in all columns. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels respectively in two-sided tests. The bold fonts denote statistical significance at the 10% level in one-sided tests of interaction terms of interests.

	LIQ			DISP		
	All CEO	New CEO	Old CEO	All CEO	New CEO	Old CEO
VARIABLES	(1)	(2)	(3)	(5)	(6)	(7)
Return	-2.756***	-2.357**	-3.083*	-3.195***	-3.235***	-3.185**
	(0.002)	(0.024)	(0.062)	(0.000)	(0.003)	(0.036)
LIQ or DISP	-0.118**	-0.051	-0.257***	0.223***	0.175**	0.334***
	(0.031)	(0.471)	(0.009)	(0.000)	(0.017)	(0.001)
Return*LIQ or DISP	**-0.140**	**-0.193**	0.020	**0.196**	**0.295**	-0.018
	(0.155)	**(0.103)**	(0.914)	**(0.170)**	**(0.110)**	(0.942)
RVOL	6.797***	7.339***	2.045	5.284***	6.641***	0.928
	(0.000)	(0.000)	(0.509)	(0.000)	(0.000)	(0.766)
Return*RVOL	6.194**	6.730**	-1.183	6.721**	6.136*	4.854
	(0.022)	(0.026)	(0.849)	(0.024)	(0.079)	(0.438)
Indepboard	0.135	0.051	0.284	0.175	0.205	0.198
	(0.440)	(0.808)	(0.397)	(0.410)	(0.458)	(0.593)
Return*Indepboard	0.116	0.249	-0.358	0.168	0.748	-0.614
	(0.806)	(0.661)	(0.688)	(0.753)	(0.290)	(0.514)
Log total assets	0.186***	0.061	0.413***	0.083*	0.063	0.099
	(0.006)	(0.501)	(0.000)	(0.073)	(0.303)	(0.195)
CEO age	-0.029***	-0.016	-0.042***	-0.017*	-0.001	-0.037**
	(0.001)	(0.171)	(0.004)	(0.076)	(0.963)	(0.020)
CEO duality	-0.355***	-0.451***	-0.177	-0.395***	-0.440***	-0.357
	(0.002)	(0.003)	(0.404)	(0.002)	(0.010)	(0.119)
Founder	-1.478***	-1.413***	-1.210***	-1.192***	-1.779**	-0.876***
	(0.000)	(0.007)	(0.000)	(0.000)	(0.015)	(0.007)
Hceoown	-0.594***	-0.280	-0.813***	-0.729***	-0.647**	-0.706***
	(0.001)	(0.273)	(0.002)	(0.000)	(0.040)	(0.010)
Nonempblock block	0.098	0.159	0.024	0.147	0.294*	-0.073
	(0.432)	(0.315)	(0.911)	(0.280)	(0.090)	(0.757)
Constant	-19.220***	-18.793***	-18.461***	-18.987***	-19.801***	-13.601
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	14141	6566	6515	11036	4983	4908
Chisq	498.3	250.1	244.3	438.0	236.3	196.3
Pseudo R-squared	0.140	0.124	0.176	0.152	0.145	0.174

Table 2.11: Simultaneous effect of accrual quality and the component of earnings volatility that is independent of the accrual quality

This table shows coefficient estimates from logit regressions of forced CEO turnover on *AQ* and a residual component of earnings volatility (*EVOL*). *RSEVOL* is the residual from an OLS regression of earnings volatility (*EVOL*) on *AQ* and a series of year and Fama and French 48 industry dummies. New and old CEOs subsamples are defined as in Table 2.7. All variables are defined in Table 2.1. Year and Fama and French 48 industry fixed effects are included in all columns. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels respectively in two-sided tests. The bold fonts denote statistical significance at the 10% level in one-sided tests of key interaction terms.

VARIABLES	All CEOs (1)	New CEOs (2)	Old CEOs (3)
ROA	-5.317***	-5.455***	-4.438**
	(0.000)	(0.000)	(0.022)
RSEVOL	-0.094	0.459	-1.525
	(0.939)	(0.776)	(0.456)
ROA* RSEVOL	14.178**	**10.167**	16.216
	(0.014)	**(0.126)**	(0.228)
AQ	-0.121	-3.035	2.997
	(0.930)	(0.135)	(0.128)
ROA*AQ	**10.213**	16.630*	-6.962
	(0.131)	(0.056)	(0.667)
Indepboard	0.010	-0.215	0.393*
	(0.943)	(0.229)	(0.091)
ROA*Indepboard	-0.546	0.270	-2.512
	(0.571)	(0.817)	(0.170)
Log total assets	0.071	0.000	0.131*
	(0.111)	(0.998)	(0.077)
CEO age	-0.035***	-0.026**	-0.046***
	(0.000)	(0.036)	(0.003)
CEO duality	-0.267**	-0.235	-0.329
	(0.028)	(0.144)	(0.121)
Founder	-1.303***	-2.093***	-0.878***
	(0.000)	(0.004)	(0.003)
Hceoown	-0.558***	-0.322	-0.847***
	(0.002)	(0.256)	(0.001)
Nonemployee block	0.112	0.134	0.073
	(0.391)	(0.426)	(0.741)
Constant	-16.871***	-17.051***	-15.845
	(0.000)	(0.000)	(.)
Observations	10848	5028	5251
Chisq	284.0	140.7	170.2
Pseudo R-squared	0.0924	0.0815	0.136

disentangle them. Another way to see the differences among them is to compare the magnitude of their economic effects. The proxies may be highly correlated but the magnitude of their economic effects can still be quite different. Later, when I analyze the economic effects, we will see more differences among them.

4.5. Endogeneity

Although the results so far are consistent with information quality of stock prices and accounting earnings affecting the board of directors' learning of the ability of CEO in the direction predicted by the Bayesian learning models, whether the effect is causal is still not well established. Even though I have controlled for the more important corporate governance variables which include board independence, CEO-Chairman duality, CEO voting power, CEO founder status, and non-employee blockholders on board[25], I still need to worry about other variables that I did not control and especially those that are unobservable to the econometricians but are actually driving the documented association. The main concern here is that innovations in the information quality proxies are correlated with change in unobservable governance variables which drive the information quality effect that I have documented. In this section, I discuss this endogeneity issue in detail and provide additional evidence on the causal effect of information quality on the board's learning process. Given the number of information quality proxies in this paper, I focus on three information quality proxies because they are the most important ones in this paper and they are also most susceptible to endogeneity concerns. They are stock liquidity (LIQ) and dispersion in analysts' earnings forecasts (DISP), and the innate component of the DD accrual quality measure (iAQ).

I begin with the innate component of accrual quality (iAQ) because its endogeneity concern can be addressed by resorting to the contrast in information quality effects in the new and

[25] In unreported results, I find that interactions between these variables and stock return are largely insignificant and adding them does not change the overall results.

old CEO subsamples. As we know, iAQ is calculated by regressing the DD accrual quality measure AQ on five firm characteristics. All except firm size are calculated using a 10-year rolling window. Hence, iAQ is correlated with slowly-moving long-term firm characteristics. The endogeneity concern here arises from potential correlation between these firm characteristics and an unobservable corporate governance variable that is driving the iAQ effect I document. This is possible because many corporate governance researchers argue that corporate governance is endogeneously chosen by firms based on its characteristics and monitoring environment (e.g. Demsetz and Lehn, 1985). Since the firm characteristics at the center of the issue here are long-term characteristics, the unobservable corporate governance variable that we are concerned about should also be quite stable over time. This implies that, if the unobservable corporate governance variable is driving the results for iAQ, the results should be the same for new and old CEOs. To the extent that they are not the same, I can reject this alternative explanation.

Next, I move on to stock liquidity and dispersion in analysts' earnings forecasts. The concern is that innovations in stock liquidity (or dispersion in analysts' earnings forecasts) are correlated with innovations in an omitted variable which are driving the information quality effects that I find. For example, one likely choice for the omitted variable is the strength of internal corporate governance which can be unobservable to econometricians[26]. For example, under the prudent man rule, institutional investors may be more likely to invest in stocks of firms with strong internal governance. Their trading can make the stocks of these firms more liquidity. At the same time, firms with strong internal corporate governance may be more aggressive at adjusting down their estimate of CEO ability after poor firm performance. Hence, the positive association between stock liquidity and the sensitivity of the board's updating on CEO ability to stock return can be driven by the strength of internal governance. Similarly, for dispersion in

[26] Another possible omitted variable could be a variable that is related to how effectively institutional investors can monitor a firm. Institutional investors may choose to invest in firms that they can more effectively monitor. The increase in institutional trading may increase stock liquidity. At the same time, institutional monitoring makes CEO turnover more sensitive to prior year stock return.

analysts' earnings forecasts, it is likely that firms with strong internal governance are more transparent and thus associated with lower dispersion in analysts' earnings forecasts. This can explain the negative association between dispersion in analysts' earnings forecasts and the sensitivity of the board's updating on CEO ability to stock return.

To address this endogeneity problem, I take an instrumental variable approach and estimate a two-step estimator for probit regressions with endogenous continuous regressors (see Woodridge, 2002). I use both lagged value of each firm's stock liquidity (dispersion in analysts' earning forecasts) and the median stock liquidity (dispersion in analysts' earnings forcasts) of the firm's Fama and French 48 industry as my instruments. Similar instruments are used by Fang et al. (2009) and Jayaraman and Milbourn (2011) in their two-stage least square estimations. Specifically, I instrument for a firm's stock liquidity in year *t* with two variables: they are the firm's lagged stock liquidity in year *t-2* and the median stock liquidity in year *t* of the the firm's Fama and French 48 industry. Both instruments are correlated with the firm's stock liqudity in year *t* but are unlikely to be correlated with the error term in the latent variable equation underlying the probit model. The use of the lagged stock liquidity addresses the concern that the level of the omitted variable in year *t* is correlated with both the firm's stock liquidity in year *t* and the error term. The use of the industry median liquidity in year *t* explores the exogenous variation in the industry component of the firm's stock liquidity in year *t* that is less likely to be correlated with the error term. I choose to use lagged stock liquidity in year t-2 rather than that in t-1 because stock liquidity is highly correlated over time. The use of two lags reduces the concern that there may be no meaningful difference between the lagged stock liquidity and the current year stock liquidity.

Based on similar reasoning, I use two instruments for a firm's dispersion in analysts' earnings forecasts in year *t*. They are the dispersion in analysts earnigns forecasts in year *t-1* and the median dispersion in analysts' earnings forecasts in year *t* of the firm's Fama and French 48 industry. The use of lagged dispersion mitigates the endogeneity problem caused by a

Table 2.12: IV estimates of the effect of stock liquidity and dispersion in analysts' earnings forecasts

This table shows the coefficient estimates from a probit model where stock liquidity (dispersion in analysts' earnings forecasts) and its interaction with stock return are treated as endogenous. Stock liquidity (dispersion in analysts' earnings forecasts) is instrumented by the lagged value of the firm's stock liquidity (dispersion in analysts' earnings forecasts) and the median stock liquidity (dispersion in analysts' earnings forecasts) of the firm's Fama and French 48 industry. The new CEO subsample contains CEOs whose tenure is below the sample median (about 5.5 years) and the old CEO subsample contains CEOs whose tenure is at or above the sample median. *Info* is the information quality proxy and *Return*Info* represents the interaction between stock return and the information quality proxy. The particular information quality proxy used is shown on top of each column. All other variables are defined in Table 2.1. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels respectively in two-sided tests.

	LIQ			DISP		
	All CEO	New CEO	Old CEO	All CEO	New CEO	Old CEO
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Return	-0.662*	-0.809	-0.917*	-0.504	-1.060**	-0.099
	(0.078)	(0.112)	(0.096)	(0.170)	(0.042)	(0.848)
LIQ or DISP	0.036	0.082**	-0.009	0.048	0.012	0.090*
	(0.155)	(0.020)	(0.814)	(0.154)	(0.787)	(0.089)
Return*LIQ or DISP	-0.135**	-0.126*	-0.095	0.297***	0.260*	0.279*
	(0.014)	(0.093)	(0.238)	(0.003)	(0.051)	(0.065)
Indepboard	0.010	-0.049	0.117	0.034	0.019	0.069
	(0.895)	(0.623)	(0.377)	(0.697)	(0.876)	(0.613)
Return*Indepboard	0.199	0.291	0.228	0.084	0.556	-0.391
	(0.421)	(0.364)	(0.541)	(0.745)	(0.135)	(0.291)
Log total assets	-0.061**	-0.152***	0.049	-0.011	-0.046*	0.040
	(0.024)	(0.000)	(0.222)	(0.547)	(0.067)	(0.154)
CEO Age	-0.017***	-0.008	-0.026***	-0.014***	-0.004	-0.023***
	(0.000)	(0.126)	(0.000)	(0.001)	(0.511)	(0.001)
CEO duality	-0.154***	-0.166**	-0.097	-0.156***	-0.140*	-0.155
	(0.003)	(0.017)	(0.289)	(0.007)	(0.070)	(0.109)
Founder	-0.578***	-0.567***	-0.509***	-0.437***	-0.705**	-0.341**
	(0.000)	(0.008)	(0.000)	(0.000)	(0.015)	(0.010)
Hceoown	-0.279***	-0.098	-0.366***	-0.274***	-0.245*	-0.217*
	(0.000)	(0.411)	(0.001)	(0.001)	(0.090)	(0.051)
Nonemployee block	0.032	0.042	0.030	0.045	0.088	-0.019
	(0.580)	(0.570)	(0.756)	(0.475)	(0.277)	(0.855)
Constant	-0.648***	-0.649**	-0.860**	-0.839***	-1.208***	-0.651
	(0.007)	(0.047)	(0.033)	(0.002)	(0.001)	(0.144)
Observations	12923	5975	6419	10000	4624	4945
Chisq	264.5	143.2	112.0	209.6	116.4	80.44

contemporaenous correlation between dispersion in analysts' forecasts and the error term in the underlying latent variable model, while the use of the industry median dispersion relies on the industry variation in dispersion in analysts' earnings forecasts that is correlated with the firm's forecast dispersion but is much less likely to be correlated with the error term.

In Table 2.12, I report the coefficient estimates of these Instrumental variables (IV) probit regressions. For each proxy, I estimate three regressions that differ in sample composition. The first regression is estimated using all CEOs, the second new CEOs and the third old CEOs. The first three columns of Table 2.12 report results on stock liquidity. The coefficient estimate of the interaction of stock liquidity and industry-adjusted stock return is statistically significant at the 5% level in the full sample and at the 10% level in the new CEO subsample but is statistically insignificant in the old CEO subsample. The last three columns of Table 12 reports results on dispersion in analysts' earnings forecasts. The coefficient estimate of the interaction of the dispersion in analysts' earnings forecasts and industry-adjusted stock return is statistically significant at the 1% level in the full sample and at the 10% level in both the new and old CEO subsample. Note that stock return is statistically insignificant in the regression for the old CEO subsample (column 6) while it is negative and statistically significant at the 5% level in the regression for the new CEO subsample (column 5). Somehow the interaction between stock return and dispersion in analysts' earnings forecasts subsumes some effect of stock return. Hence, the effect of dispersion in analysts' earnings forecast on the board's updating based on stock return is still weaker in column 6 than it is in column 5, which is still consistent with our previous findings that information quality is more important for learning about new CEOs than old CEOs.

5. Calculating economic effects

In the previous section, I have found statistical support for my hypotheses. In this section, I calculate the economic magnitude of the effect of the information quality proxies on the board's updating process. Since the board of directors' internal estimate of CEO ability is unobservable, I choose to evaluate the effect of noise in firm performance on the board's updating process by examining its effect on the probability of CEO turnover. Here, one important distinction to make is that the economic effect of information quality (x_2) on the sensitivity of the board's *assessment of CEO ability* to firm performance (x_1) is not the same as its effect on the sensitivity of *CEO turnover* to firm performance. The expression for the latter is given by:

$$\frac{\partial^2 P(Y=1|X)}{\partial x_1 \partial x_2} = \beta_{12} \frac{e^{X\beta}}{(1+e^{X\beta})^2} + (\beta_1 + \beta_{12}x_2)(\beta_2 + \beta_{12}x_1) \frac{(1-e^{X\beta})}{(1+e^{X\beta})^3} \qquad (2.14)$$

where X is the vector of all explanatory variables. As we see, the marginal effect calculated by Equation (2.14) captures both the main effect from the term $\beta_2 x_2$ and the interaction effect from the term $\beta_{12}x_1x_2$ on the probability of CEO turnover. However, the economic effect that I desire to calculate is the one that is purely driven by the interaction term $\beta_{12}x_1x_2$ in Equation (2.6). To infer the economic effect on probability of CEO turnover that comes purely from the interaction term $\beta_{12}x_1x_2$, we need to keep the threshold CEO ability below which the CEO is removed constant, i.e. the main effect term $\beta_2 x_2$ constant, while changing the information quality in the interaction term. This way only the effect of information quality on the board's updating process is captured.

5.1. Inferred economic effect of information quality on the board's updating process

To implement this, I first estimate logit regressions similar to those in Tables 5-8 but without the industry and year fixed effects to simplify the calculateion of the economics effects. The coefficient estimates from these logit regressions are reported in Table 2.13.

Based on these logit models, I fix the value of the information quality proxy in the main effect term while allowing its value in the interaction term to change from the 5th percentile to the 95th percentile of the sample. Then I compare the change in estimated probability of forced CEO turnover for a drop in firm performance from the top decile to the bottom decile when the

Table 2.13: Logit regressions used for calculating the economic effects of information quality proxies

The new CEO subsample contains CEOs whose tenure is below the sample median (about 5.5 years) and the old CEO subsample contains CEOs whose tenure is at or above the sample median. As in previous tables, *Info* is the information quality proxy and *Return*Info* (*ROA*Info*) represents the interaction between stock return (ROA) and the information quality proxy. The particular information quality proxy used is shown on top of each column. All other variables are defined in Table 2.1. Standard errors are clustered by firm. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels respectively in two-sided tests.

Panel A: Stock price information quality proxies

	RVOL		LIQ		DISP	
	New CEO	Old CEO	New CEO	Old CEO	New CEO	Old CEO
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Return	-3.511***	-3.461***	-0.598	-3.978***	-1.544**	-2.863***
	(0.000)	(0.000)	(0.530)	(0.004)	(0.013)	(0.000)
Info	7.420***	4.307**	-0.019	-0.073	0.236***	0.268***
	(0.000)	(0.038)	(0.739)	(0.382)	(0.001)	(0.002)
Return*Info	8.804***	2.601	-0.361**	0.080	0.528**	0.104
	(0.000)	(0.547)	(0.020)	(0.717)	(0.022)	(0.637)
Indepboard	-0.094	0.234	-0.153	0.210	-0.134	0.329
	(0.551)	(0.272)	(0.367)	(0.388)	(0.484)	(0.210)
Log total assets	0.009	0.147***	-0.116*	0.170**	-0.031	0.113*
	(0.822)	(0.005)	(0.092)	(0.021)	(0.519)	(0.069)
CEO age	-0.020**	-0.044***	-0.023**	-0.052***	-0.008	-0.040***
	(0.023)	(0.000)	(0.018)	(0.000)	(0.409)	(0.000)
CEO duality	-0.393***	-0.175	-0.383***	-0.145	-0.351**	-0.271
	(0.003)	(0.360)	(0.006)	(0.474)	(0.025)	(0.213)
Founder	-1.451***	-0.997***	-1.378***	-1.089***	-1.736**	-0.817***
	(0.004)	(0.000)	(0.006)	(0.001)	(0.015)	(0.007)
Hceoown	-0.366	-0.623***	-0.278	-0.773***	-0.522*	-0.567**
	(0.113)	(0.004)	(0.260)	(0.002)	(0.085)	(0.026)
Nonemployee block	0.178	0.113	0.090	0.052	0.247	0.040
	(0.195)	(0.556)	(0.530)	(0.798)	(0.109)	(0.859)
Constant	-2.953***	-2.917***	-0.980	-1.883**	-1.899***	-1.776**
	(0.000)	(0.001)	(0.130)	(0.038)	(0.005)	(0.038)
Observations	6990	7147	6702	6919	5209	5436
Chisq	206.7	152.3	110.2	120.6	111.6	115.8
Pseudo R-squared	0.103	0.125	0.0821	0.122	0.102	0.119

Panel B: Accounting earnings information quality proxies

	EVOL		AQ		iAQ	
	New CEO	Old CEO	New CEO	Old CEO	New CEO	Old CEO
VARIABLES	(2)	(3)	(5)	(6)	(5)	(6)
ROA	-2.790***	-7.117***	-2.719***	-5.962***	-3.475***	-6.186***
	(0.002)	(0.000)	(0.001)	(0.000)	(0.002)	(0.001)
Info	1.513**	1.477	1.028	3.645***	1.289	5.338**
	(0.040)	(0.147)	(0.254)	(0.000)	(0.487)	(0.023)

Table 2.13, continued

ROA*Info	7.635*	11.983**	8.729*	4.872	17.137*	11.919
	(0.051)	(0.044)	(0.073)	(0.506)	(0.059)	(0.443)
Log total assets	-0.144***	0.086**	-0.139***	0.200***	-0.134***	0.216***
	(0.000)	(0.043)	(0.001)	(0.000)	(0.005)	(0.000)
CEO Age	-0.025***	-0.052***	-0.032***	-0.057***	-0.033***	-0.059***
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Constant	-0.840*	-1.314**	-0.365	-1.847***	-0.354	-1.972***
	(0.078)	(0.022)	(0.491)	(0.004)	(0.546)	(0.008)
Observations	9353	9825	6700	7004	6625	6907
Chisq	72.71	118.1	54.56	104.8	49.60	93.55
Pseudo R-squared	0.0233	0.0612	0.0242	0.0668	0.0245	0.0621

information quality proxy in the interaction term is at the 5th percentile of the sample with that when the information quality proxy in the interaction term is at the 95th percentile of the sample. The difference in the change in probability of forced CEO turnover between the two information quality levels is a measure of the inferred economic effect of the information quality proxy on the board's updating process as shown up in sensitivity of CEO turnover to firm performance.

In Table 2.14, I report the estimated probability of forced CEO turnover in the new CEO subsample when firm performance is at the middle of the top and bottom decile and when the value of information quality proxy in the interaction term is at the 5th and 95th percentile of the sample respectively[27]. The value of information quality proxy in the main effect term is fixed at 5th percentile (Panel A), median (Panel B), and 95th percentile (Panel C) of the sample respectively. All other variables are set to the means in the new CEO subsample. The table shows that the absolute magnitude of the economic effect is quite large while the relative magnitude is even bigger. For example, according to Panel B, when the information quality proxy in the main effect term is fixed at the sample median, a change in stock return from the top to bottom decile increases the estimated probability of forced CEO turnover by 14.45% when stock liquidity is

[27] Since the logit regression results show that most of the information quality proxies have statistically insignificant effect on the board's updating process in the old CEO subsample. It is uninteresting to evaluate the economic effects in the old CEO subsample.

high (the 95th percentile), while the same change in stock return only increase the estimated probability of forced CEO turnover by 4.03% when stock liquidity is low (the 5th percentile). The difference is 10.42%, which is 2.56 times the increase in probability of forced CEO turnover when stock liquidity is low. Similar difference is 5.55% for stock volatility which is about 1.28 times the increase in probability of forced CEO turnover when stock volatility is high (the 95th percentile) and is 8.93% for dispersion in analysts' earnings forecasts which is 1.90 times the increase in probability of forced CEO turnover when dispersion in analysts' earnings forecasts is high.

According to Table 2.14, the absolute magnitude of the economic effect of accounting information quality proxies is considerably smaller than that of stock price information quality proxies. However, since change in accounting performance also has a smaller effect on probability of forced CEO turnover, the relative magnitude of the economic effect is still significant. For example, Panel B of Table 2.14 shows that a change in ROA from the top to bottom decile increases the estimated probability of forced CEO turnover by 2.52% when iAQ is at the 95th percentile of the sample, while the same change in ROA increases the probability of forced CEO turnover by 4.11% when iAQ is at the 5th percentile of the sample. The difference is 1.59% which is about 60% of the change in probability of forced CEO turnover when iAQ is at the 95th percentile of the sample. Similar difference is 1.15% for AQ which is about 50% of the change in probability of forced CEO turnover when AQ is at the 95th percentile of the sample and is 0.85% for earnings volatility which is about 45% of the change in probability of forced CEO turnover when stock liquidity is at the 95th percentile of the sample. Figure 2.1 plots these estimated probabilities based on Panel B of Table 2.14.

To provide an alternative perspective to look at the economic effect, in Figure 2.2, I plot the estimated probability of forced CEO turnover against firm performance when the value of the information quality proxy in the main effect term is kept at the 95th percentile of the sample while the value of the information quality proxy in the interaction term is set at the 5th and 95th

Table 2.14: Inferred effect of information quality on probability of forced CEO turnover in the new CEO subsample

This table presents the estimated probability of forced CEO turnover when stock return (ROA) is at the middle of the top and bottom decile and when the value of the information quality proxy in the interaction term is at the 5th and 95th percentile of the sample respectively. The value of the information quality proxy in the main effect term is fixed at the 5th percentile (Panel A), median (Panel B) and 95th percentile (Panel C) respectively. All other variables are set to the means of the new CEO subsample. P_1 is the estimated probability of forced CEO turnover at the middle of bottom decile of firm performance (5th percentile) and P_2 is the estimated probability of forced CEO turnover at the middle of top decile of firm performance (95th percentile). $\Delta(P_1-P_2)$ is the difference in (P_1-P_2) between the two levels of information quality. A negative value indicates that (P_1-P_2) is smaller when the information quality is at 95th percentile than when the information quality is at the 5th percentile of the sample.

		Stock Information quality							Accounting Information quality					
		RVOL		LIQ		DISP			EVOL		AQ		iAQ	
		P95	P5	P95	P5	P95	P5		P95	P5	P95	P5	P95	P5
Effect	Return	(1)	(2)	(3)	(4)	(5)	(6)	ROA	(7)	(8)	(9)	(10)	(11)	(12)
Panel A:														
P_1	-0.48	3.60	7.35	15.50	5.30	3.96	10.49	-0.08	4.50	4.79	4.78	5.17	4.94	5.53
P_2	0.74	0.49	0.15	0.16	1.00	0.46	0.09	0.33	2.51	1.92	2.55	1.82	2.50	1.54
P_1-P_2		3.11	7.20	15.34	4.30	3.50	10.40		1.99	2.87	2.23	3.36	2.44	3.99
$\Delta(P_1-P_2)$		-4.09		11.04			-6.9		-0.88		-1.13		-1.55	
Panel B:														
P_1	-0.48	5.03	10.11	14.60	4.96	5.31	13.75	-0.08	4.31	4.59	4.90	5.30	5.09	5.70
P_2	0.74	0.69	0.22	0.15	0.93	0.62	0.12	0.33	2.40	1.83	2.62	1.87	2.57	1.59
P_1-P_2		4.34	9.89	14.45	4.03	4.69	13.62		1.91	2.76	2.28	3.43	2.52	4.11
$\Delta(P_1-P_2)$		-5.55		10.42		-8.93			-0.85		-1.15		-1.59	
Panel C:														
P_1	-0.48	12.28	22.91	13.83	4.67	9.84	23.68	-0.08	5.26	5.60	5.37	5.81	5.49	6.15
P_2	0.74	1.81	0.57	0.14	0.88	1.20	0.24	0.33	2.94	2.25	2.88	2.06	2.78	1.72
P_1-P_2		10.47	22.34	13.69	3.79	8.64	23.44		2.32	3.35	2.49	3.75	2.71	4.43
$\Delta(P_1-P_2)$		-11.87		9.9		-14.80			-1.03		-1.26		-1.72	

Figure 2.1: Comparison of inferred probability of forced CEO turnover at the top and bottom decile of firm performance

The following graphs show the estimated probability of CEO turnover when industry-adjusted stock return (industry-adjusted ROA) is in the middle of the bottom decile (Decile 1) and top decile (Decile 10) respectively for new CEOs (i.e. CEOs with tenure less than the sample median) based on the logit regressions in Table 2.13. The information quality proxy used is shown in the title of each chart. All variables are defined in Table 2.1. The estimated probability is calculated by keeping the value of the information quality proxy in the main effect term at the sample median but allowing the value of the information quality proxy in the interaction term to be at the 5th and 95th percentile of the sample respectively. All other variables are at their means in the new CEO subsample. The two bars above *P(Y=1|ret*p95)* or *P(Y=1|roa*p95)* show the implied probability of CEO turnover (values shown on top of each bar) when the information quality proxy in the interaction term is at the 95th percentile of the sample, while the two bars above *P(Y=1|ret*p5)* or *P(Y=1|roa*p5)* show the implied probability of CEO turnover when the information proxy in the interaction term is at the 5th percentile of the sample. The difference in height of any pair of adjacent bars is a measure of sensitivity of CEO turnover to firm performance, while the difference in this difference between the two groups of bars in each cell gives a measure of the economic effect of information quality on sensitivity of CEO turnover to firm performance.

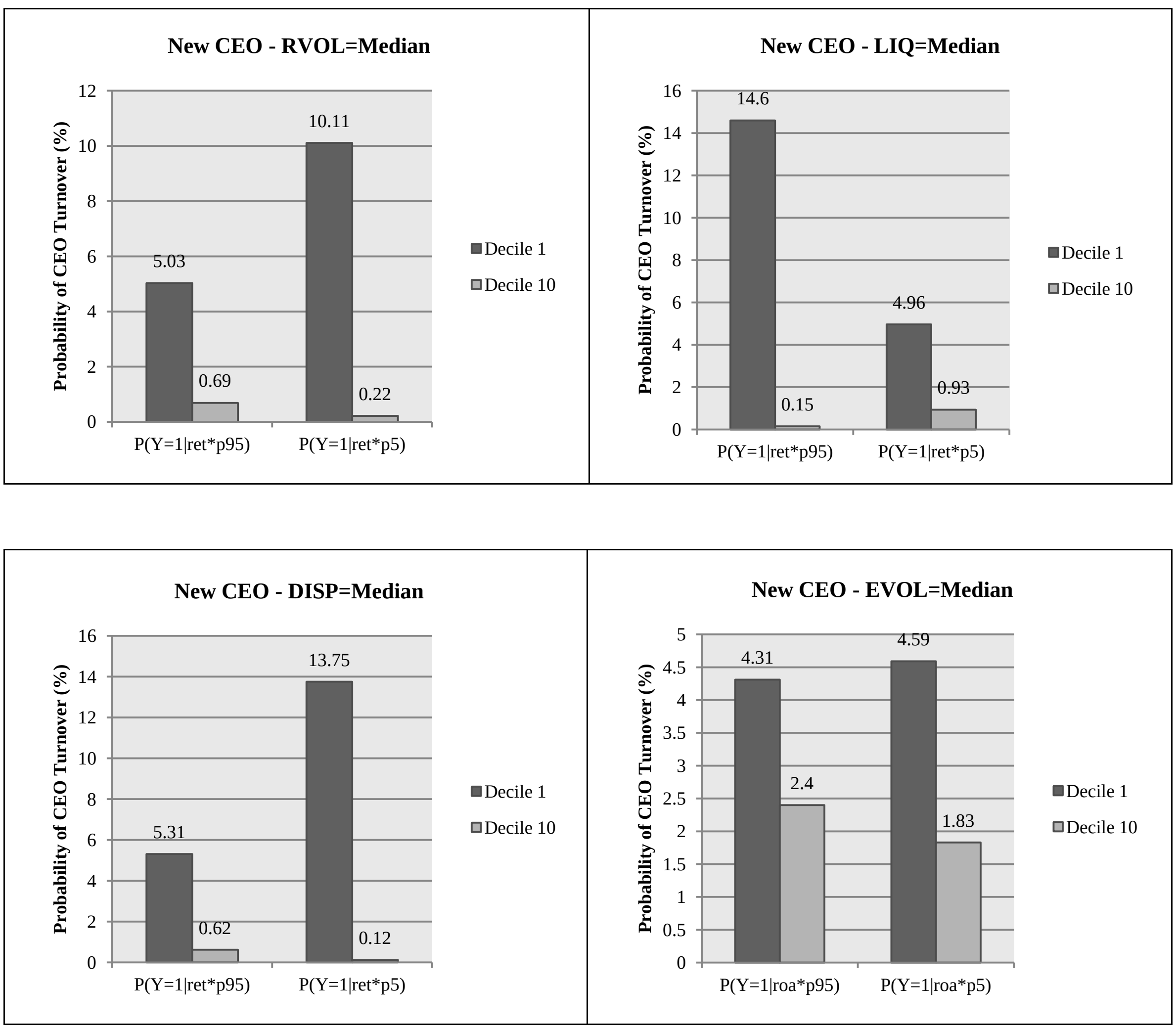
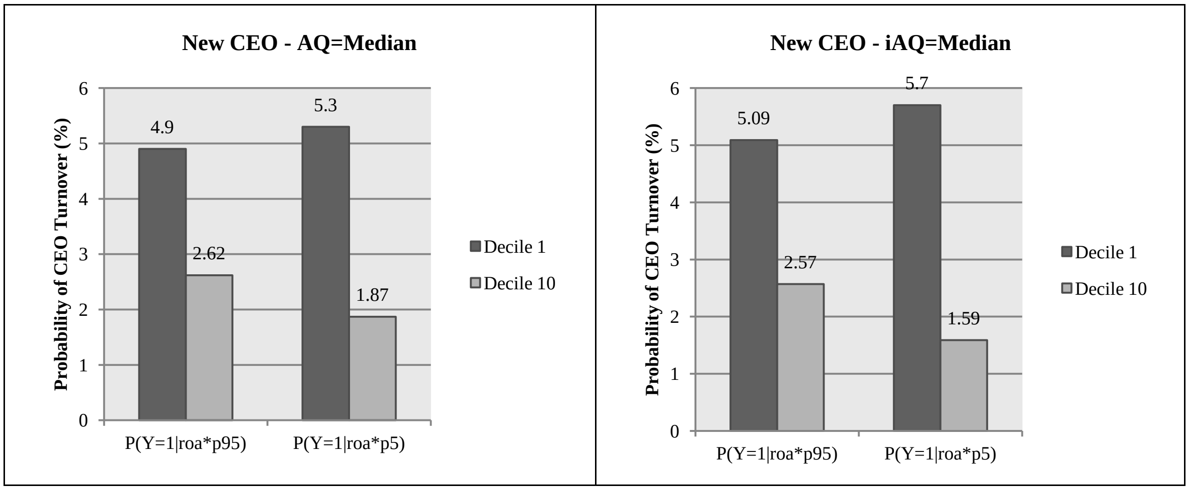


Figure 2.1 Continued



percentile of the sample respectively. I use the notation *P(Y=1|m, ret*n)* to represent the estimated probability of CEO turnover when the value of the information quality proxy in the main effect term is at the m^{th} percentile and the value of the information quality proxy that interacts with stock return is at the n^{th} percentile of the sample. *P(Y=1|m, roa*n)* is similarly defined except the firm performance measure is return on assets. Panel A shows the graphs for stock price information quality proxies. The difference between the lines for *P(Y=1|p95, ret*p95)* and *P(Y=1|p95, ret*p5)* represents the inferred economic effect of a change in the value of the information quality proxy from the 5^{th} to 95^{th} percentile on the board's updating process when the threshold CEO ability is fixed at a level corresponding to the value of the information quality proxy at the 95^{th} percentile of the sample. As we can see, the inferred sensitivity of forced CEO turnover to stock return is significantly higher when information quality is high than when information quality is low in the new CEO subsample, while the difference is very small in the old CEO subsample. Panel B shows the graphs for accounting information quality proxies. The inferred effect of information quality on sensitivity of forced CEO turnover to firm performance is much smaller than those in Panel A. However, we can still see that higher information quality (i.e. lower noise in earnings) is associated with higher inferred turnover-performance sensitivity in the new CEO subsample, while the difference is very small in the old CEO subsample.

Comparing panel A with panel B of Figure 2.2, I find an interesting difference in the boards of directors' use of stock and accounting performance when they update their estimates of CEO ability. According to Panel A, the inferred sensitivity of forced CEO turnover to stock return is higher for new CEOs than for old CEOs when information quality is high. For example, when stock liquidity is high (the 95th percentile), the inferred probability of forced CEO turnover increases by 11% for a change in stock return from 0.8 to -0.4 for new CEOs, while it increases by only 4% for old CEOs under the same condition. However, when it comes to return on assets, the opposite is observed. Here, the inferred sensitivity of forced CEO turnover to industry-adjusted ROA is lower for new CEOs than for old CEOs. This holds even when noise in accounting earnings is low in the new CEO firm. For example, when iAQ is at the 5th percentile of the sample, the inferred probability of forced CEO turnover increases by 7% for a fall in industry-adjusted ROA from 0.3 to -0.2 when the CEO is new, but it increases by 10% when the CEO is old. One explanation for the contrasts could be that the first few years of earnings after a new CEO takes office are likely to be affected by what happened under her predecessor; however, stock prices can quickly respond to new strategic moves taken by the new CEO. Thus stock return becomes more important than accounting earnings for evaluating new CEOs.

5.2. Distinction between inferred economic effects and combined economic effects

It is important to note that, since I fix the information quality variable in the main effect term at the 95th percentile of the sample, the implied probability of CEO turnover represented by the line *P(Y=1|p95, ret*p5)* is the inferred probability assuming that we could change the information quality proxy in the interaction term to the 5th percentile of the sample while keeping the threshold CEO ability below which to remove the CEO constant. The difference between *P(Y=1|p95, ret*p95)* and *P(Y=1|p95, ret*p5)* gives us a measure of the economic effect of

Figure 2.2: Inferred economic effect of information quality on sensitivity of forced CEO turnover to firm performance

The following charts show the estimated probability of forced CEO turnover in any given year vs. prior year industry-adjusted stock return or industry-adjusted ROA based on the logit regressions in Table 2.13. The information quality measures used are shown in the chart titles. All variables are defined in Table 2.1. At each row, the left panel shows the graph for the subsample of new CEOs while the right panel graph for the subsample of old CEOs. *P(Y=1|m, ret*n)* represents the implied probability when the value of the information quality measure in the main effect term is set at the m^{th} percentile of the sample while the value of the information quality measure that interacts with stock return is set at the n^{th} percentile of the sample. *P(Y=1|m, roa*n)* is similarly defined except firm performance is measured by industry-adjusted ROA. The values of all other explanatory variables in the logit regressions in Table 2.13 are kept at their means in the respective new and old CEO subsample. Panel A is for stock price information quality proxies, while Panel B is for accounting information quality proxies.

Panel A: Stock price information quality

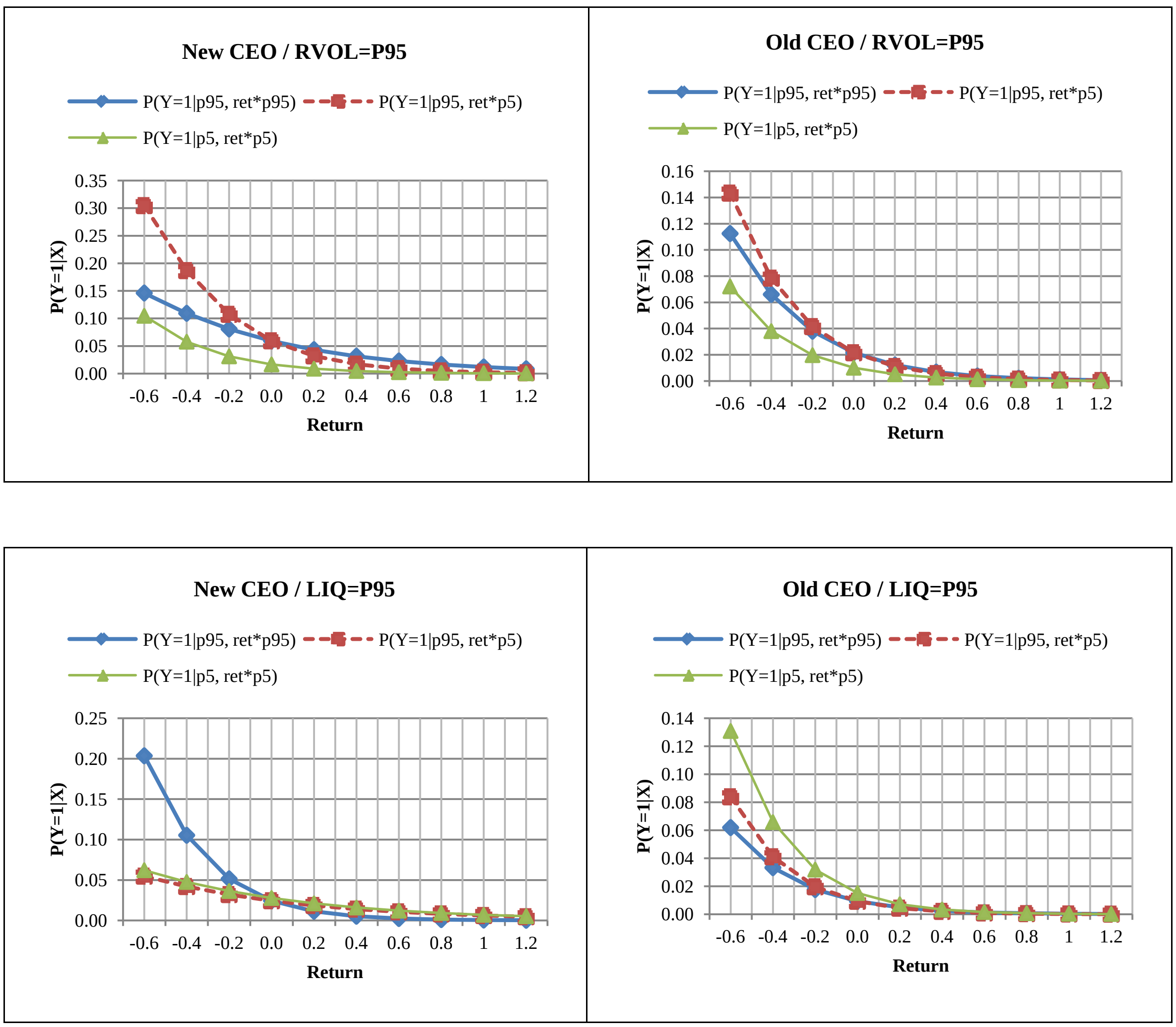
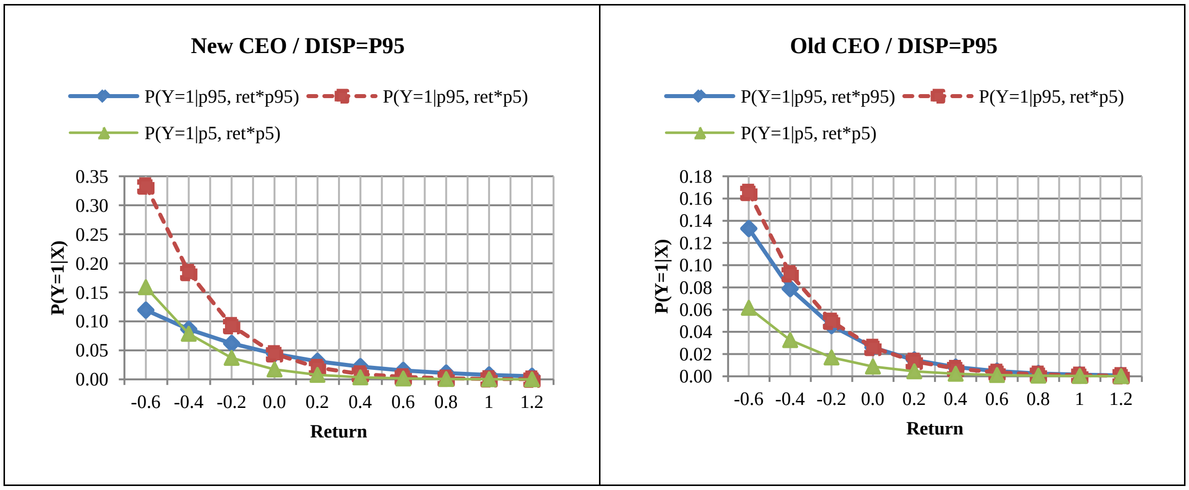
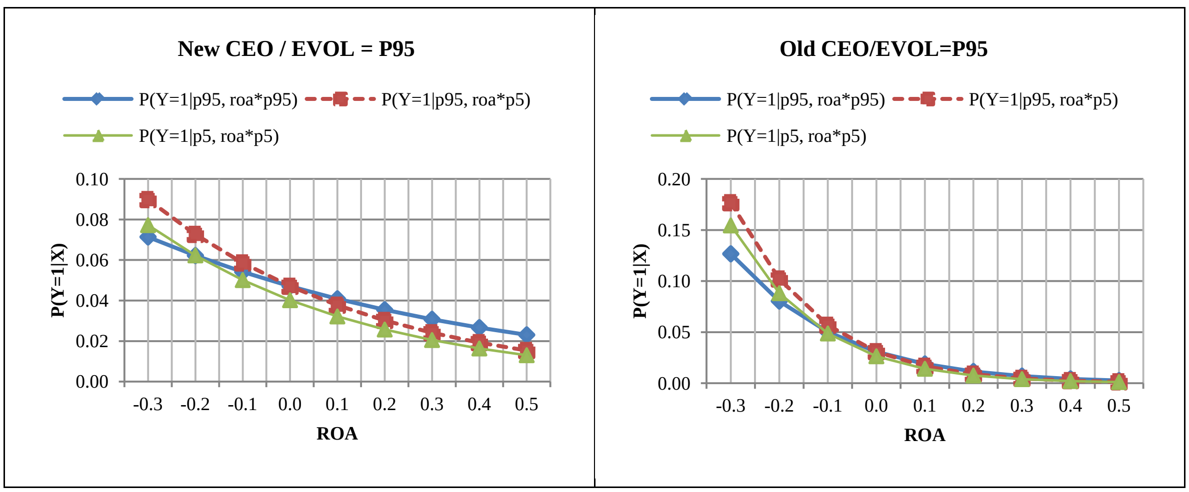


Figure 2.2, continued



Panel B: Accounting information quality



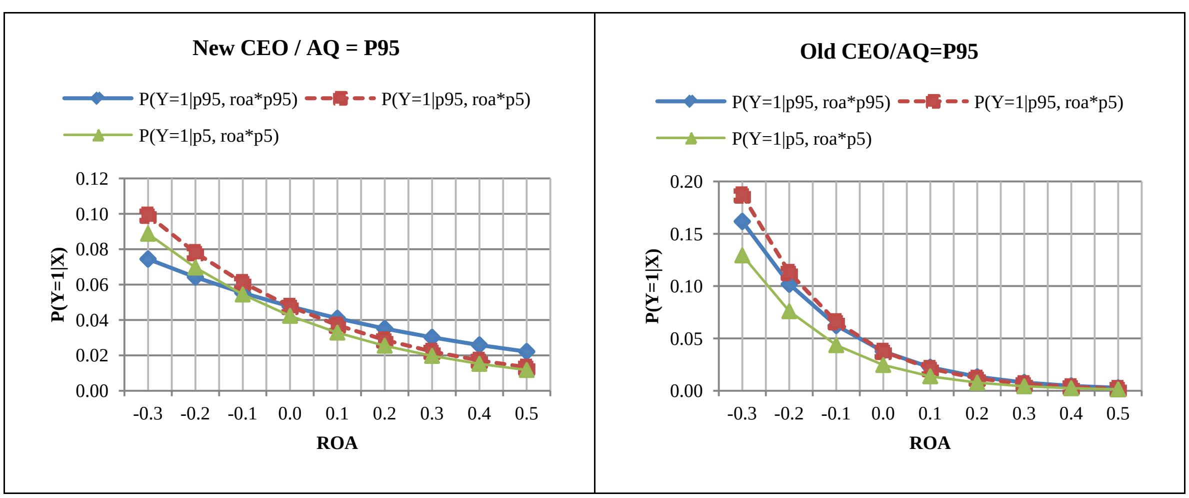
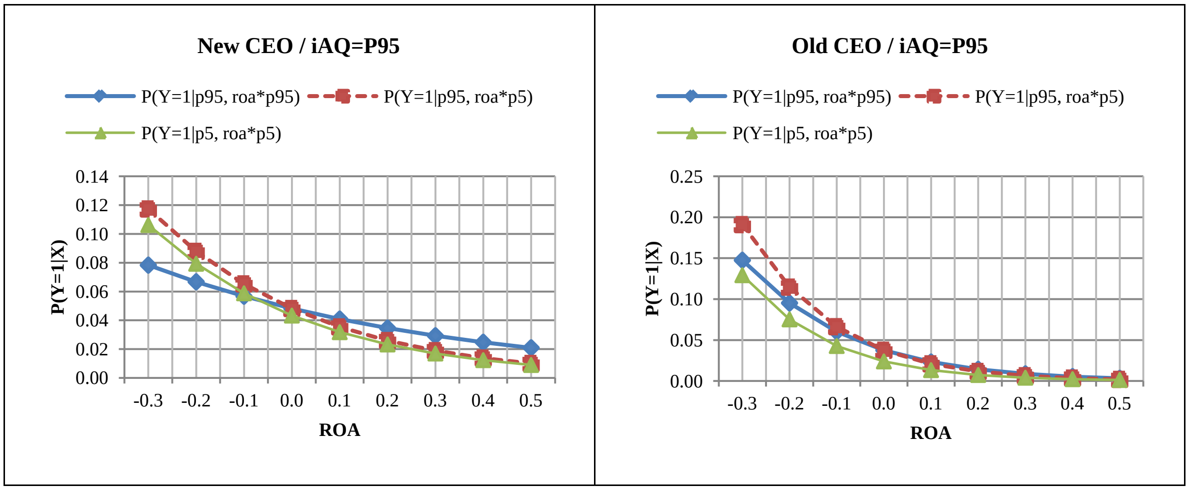


Figure 2.2, continued



information quality on the board's updating process. This is in general different from the difference between estimated probability of CEO turnover in firms with information quality at the 95th and the 5th percentile of the sample, which is directly related to the marginal interaction effect represented by Equation (2.14).

To illustrate this distinction, I overlap the plots of the estimated probability of forced CEO turnover when the value of the information quality proxy in both the main and interaction effect term is at the 5th percentile of the sample on Figure 2.2. These are the thin lines with triangel markers associated with *P(Y=1|p5, ret*p5)* or *P(Y=1|p5, roa*p5)*. The difference between the lines *P(Y=1|p95, ret*p95)* and *P(Y=1|p5, ret*p5)* or between the lines *P(Y=1|p95, roa*p5)* and *P(Y=1|p5, roa*p5)* represents the total economic effect of a change in the value of the information quality proxy from the 5th to 95th percentile of the sample. In Panel A, we can see that, for stock volatility, the total economic effect is much smaller than the inferred economic effect. This is because, according to the logit regressions in Table 2.13, stock volatility reduces the weight on stock return in the board's updating process but at the same time increases the threshold CEO ability as indicated by the significantly positive coefficient of stock volatility. The two effects offset each other to some extent when their effects are refleced on the probability of

CEO turnover. As a result, the estimated difference in probability of CEO turnover in firms with high and low stock volatility significantly underestimate the effect of information quality on the the Bayesian updating process. This example points to how directly comparing the estimated probability of CEO turnover in high and low information quality firms may miss the significant effect of information quality on the board's updating process. This is exactly why the economic effect of information quality on board's updating process should not be calculated by the cross derivative in Equation (2.14). However, when a information quality proxy does not affect the threshold CEO ability, the estimated probability of CEO turnover in high and low information quality firms should be the same as the inferred probability of CEO turnover calculated by keeping the threshold CEO ability constant. This is the case with stock liquidity. According to logit regressions in Table 2.13, the coefficient of stock liquidity is not statistically significant. Hence, in Panel A, the thin line for *P(Y=1|p5, ret*p5)* is very close to the line for *P(Y=1|p95, ret*p5)* in the new CEO panel. Hence, strictly speaking, the distinctions in interpretation I just discussed is only important when the information quality variable itself has a significant effect on the threshold CEO ability. Otherwise, the sensitivity of the board's updating process to firm performance has a one-to-one correspondence to the sensitivity of CEO turnover to firm performance.

5.3. The combined economic effects of information quality on turnover-performance sensitivity

The inferred economic effect in Section 5.1 helps us to better understand the effect of information quality on the board's updating on estiamte of CEO ability based on firm performance. However, in practice, the main effect of an information quality proxy on the threshold CEO ability and the interaction effect of it on the board's updating process change simultaneously with value of the information quality proxy. Very often, people are interested in knowing how sensitivity of forced CEO turnover to firm performance is different between firms with high and low information quality. This refers to the marginal interaction effect represented by Equation (2.14). In Table 2.15, I compare the increase in estimated probability of forced CEO turnover for a drop in firm performance from the top to bottom decile in firms with high and low information quality in the new CEO subsample. All varaibles are set at the sample means. $\Delta(P_1- P_2)$ is the difference in increase in probability of forced CEO turnover in the high and low information quality firms. This difference reflects the combined effect of an information quality proxy on both the threshold CEO ability and the board's updating process so it could have different sign than the coefficient on the interaction term. It is a measure of the marginal interaction effect of information quality on sensitivity of forced CEO turnover to firm performance. Consistent with the discussion in last section that a significant main effect term creates difference between the combined and inferred economic effects, the combined economic effect of information quality is significantly weaker than the inferred pure interaction effect for the following information quality proxies: stock volatility, dispersion in analysts' earnings forecasts and earnings volatility, while the combined economic effect of information quality is close to the inferred pure interaction effect for these information quality proxies: stock liquidity, AQ and iAQ.

I note that although the main effect of earnings volatility is positive and statistically significant in Table 2.13, it is not statistically significant in the full specification in Table 2.6 where industry and year fixed effects are included. This suggests that the number in Table 2.15 probably underestimates the combined economic effect of earnings volatility on turnover-performance sensitivity. Similarly, the number in Table 2.15 also underestimates the combined economic effect of iAQ because the main effect of iAQ is negative and statistically significant in the full specification in Table 2.6, which suggests a reduction in threshold CEO ability when iAQ increases. Figure 2.3 shows the estimated probability of forced CEO turnover for new CEOs in firms with high and low information quality. Figure 2.4 plots the estimated probability of forced CEO turnover against firm performance in high and low information quality firms for new and old CEOs separately.

Table 2.15: Estimated probability of forced CEO turnover in firms with high and low information quality for new CEOs

This table presents the estimated probability of forced CEO turnover when stock return (ROA) is at the middle of the top and bottom decile and when the value of the information quality proxy in both the main and the interaction term is at the 5th and 95th percentile of the sample respectively. All other variables are set to the means of the new CEO subsample. P_1 is the estimated probability of forced CEO turnover at the middle of bottom decile of firm performance (5th percentile) and P_2 is the estimated probability of forced CEO turnover at the middle of top decile of firm performance (95th percentile). $\Delta(P_1\text{-}P_2)$ is the difference in $(P_1\text{-}P_2)$ between the two levels of information quality. A negative value indicates that $(P_1\text{-}P_2)$ is smaller when the information quality proxy is at 95th percentile than when the information quality proxy is at the 5th percentile of the sample.

		Stock information quality							Accounting information quality					
		RVOL		LIQ		DISP			EVOL		AQ		iAQ	
		P95	P5	P95	P5	P95	P5		P95	P5	P95	P5	P95	P5
Effect	Return	(1)	(2)	(3)	(4)	(5)	(6)	ROA	(1)	(2)	(3)	(4)	(5)	(6)
P_1	-0.48	12.28	7.35	13.83	5.30	9.84	10.49	-0.08	5.26	4.79	5.37	5.17	5.49	5.53
P_2	0.74	1.81	0.15	0.14	1.00	1.20	0.09	0.33	2.94	1.92	2.88	1.82	2.78	1.54
P_1-P_2		10.47	7.2	13.69	4.30	8.64	10.40		2.32	2.87	2.49	3.35	2.71	3.99
Δ(P_1-P_2)		3.27		9.39		-1.76			-0.55		-0.86		-1.28	

Figure 2.3: Comparison of probability of forced CEO turnover at the top and bottom decile of firm performance in high and low information quality firms

The following graphs show the estimated probability of forced CEO turnover when industry-adjusted stock return (industry-adjusted ROA) is in the middle of the bottom decile (Decile 1) and top decile (Decile 10) respectively for new CEOs (i.e. CEOs with tenure less than the sample median) based on the logit regressions in Table 2.13. The information quality proxy used is shown in the title of each chart. All variables are defined in Table 2.1. The estimated probability is calculated by setting the value of the information quality at the 5th and 95th percentile of the sample respectively. The two bars above *P95* show the estimated probability of forced CEO turnover (values shown on top of each bar) when the information quality proxy is at the 95th percentile of the sample, while the two bars above *P5* show the estimated probability of forced CEO turnover when the information proxy is at the 5th percentile of the sample. The difference in height of any pair of adjacent bars is a measure of sensitivity of CEO turnover to firm performance, while the difference in this difference between the two groups of bars in each cell gives a measure of the economic effect of information quality on sensitivity of CEO turnover to firm performance.

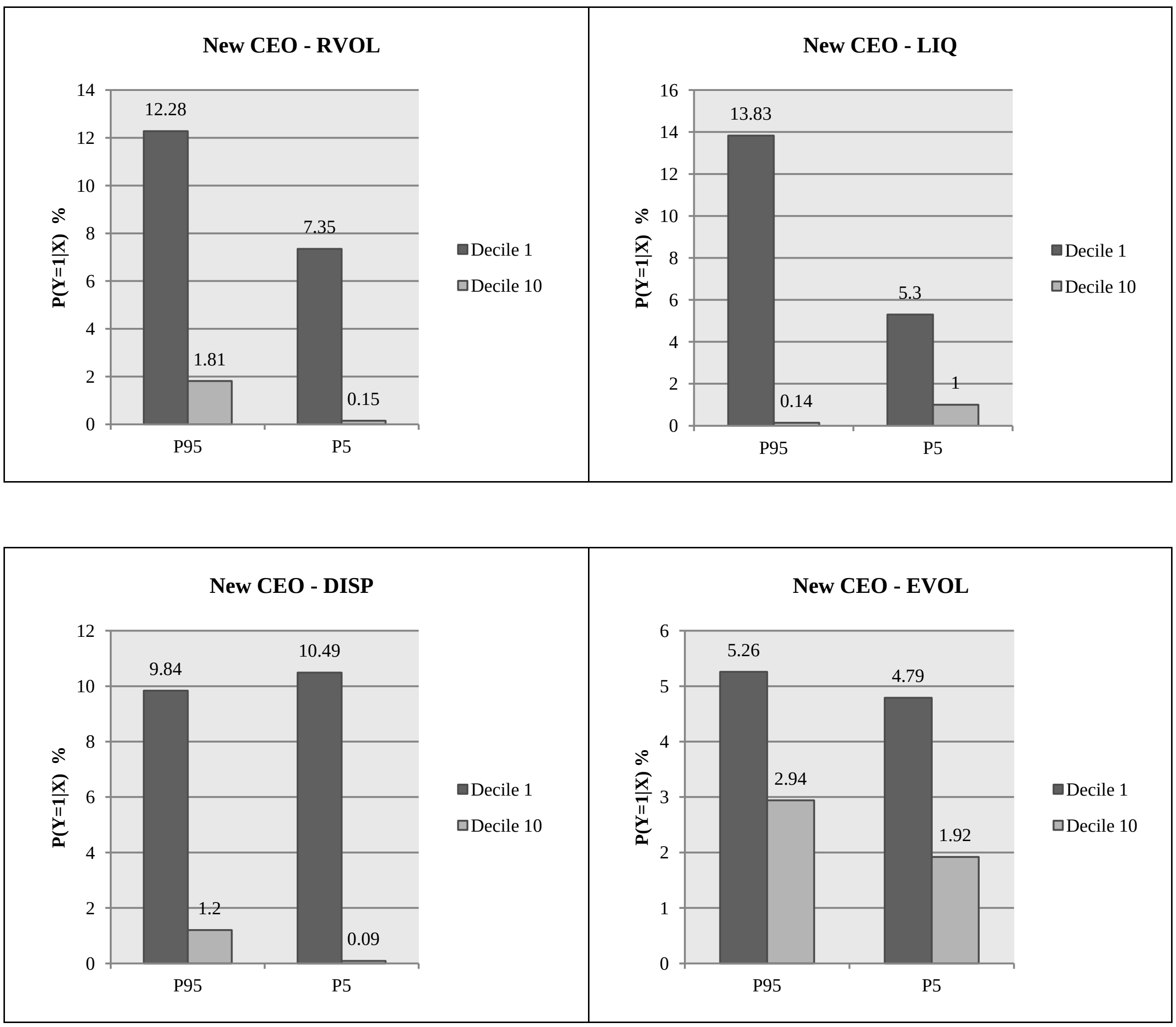


Figure 2.3, continued

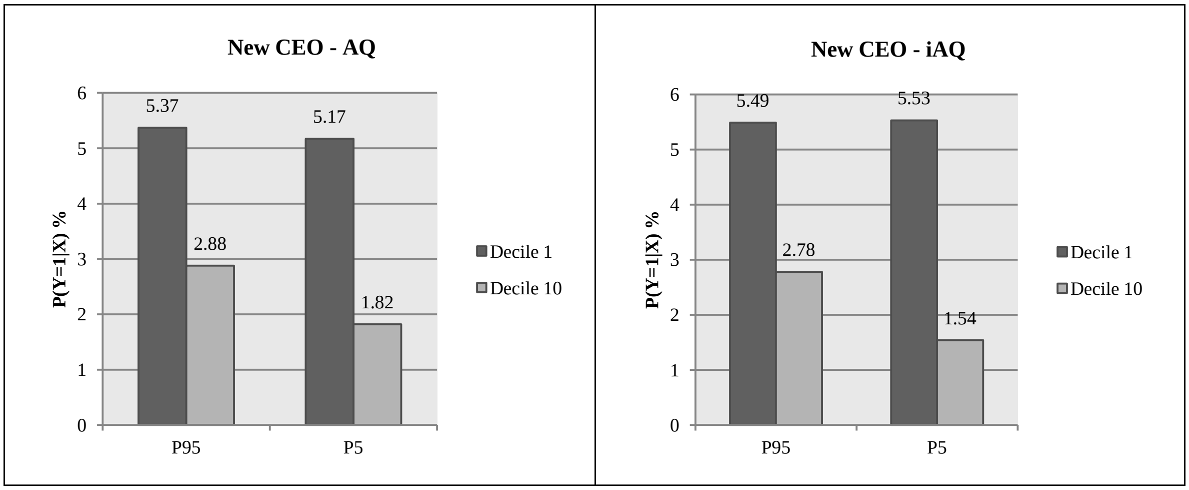
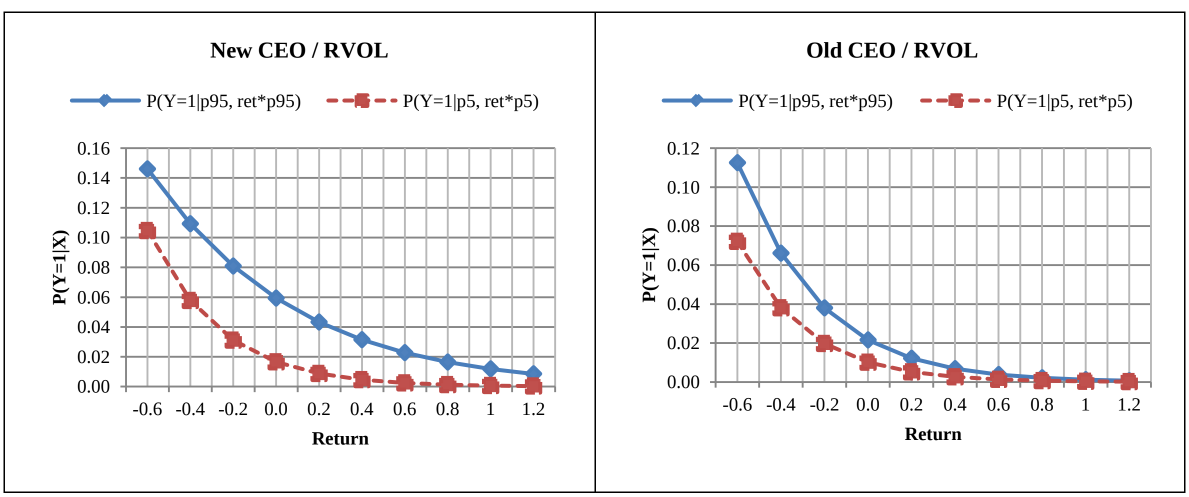


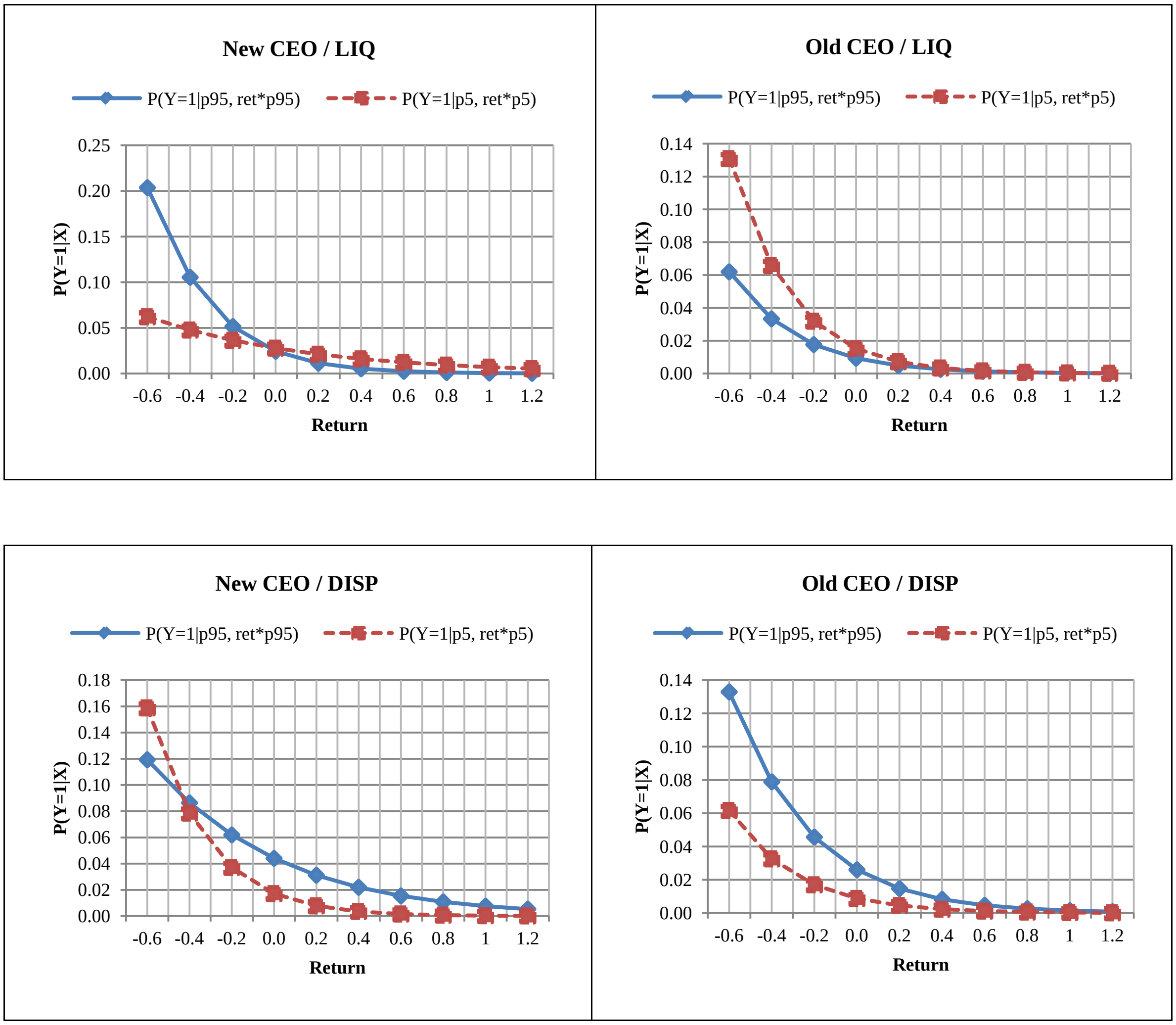
Figure 2.4: Combined economic effects of information quality on sensitivity of forced CEO turnover to firm performance

The following charts show the estimated probability of forced CEO turnover in any given year vs. prior year industry-adjusted stock return or industry-adjusted ROA based on the logit regressions in Table 2.13. The information quality measures used are shown in the chart titles. All variables are defined in Table 2.1. At each row, the left panel shows the graph for the subsample of new CEOs while the right panel graph for the subsample of old CEOs. *P(Y=1|n, ret*n)* represents the implied probability of forced CEO turnover in firms with information quality proxy in the n^{th} percentile of the sample. *P(Y=1|n, roa*n)* is similarly defined except firm performance is measured by industry-adjusted ROA. The values of all other explanatory variables in the logit regressions are kept at their means in the respective new and old CEO subsample. Panel A is for stock price information quality proxies, while Panel B is for accounting information quality proxies.

Panel A: Stock price information quality



Figured 2.4, continued



Panel B: Accounting information quality

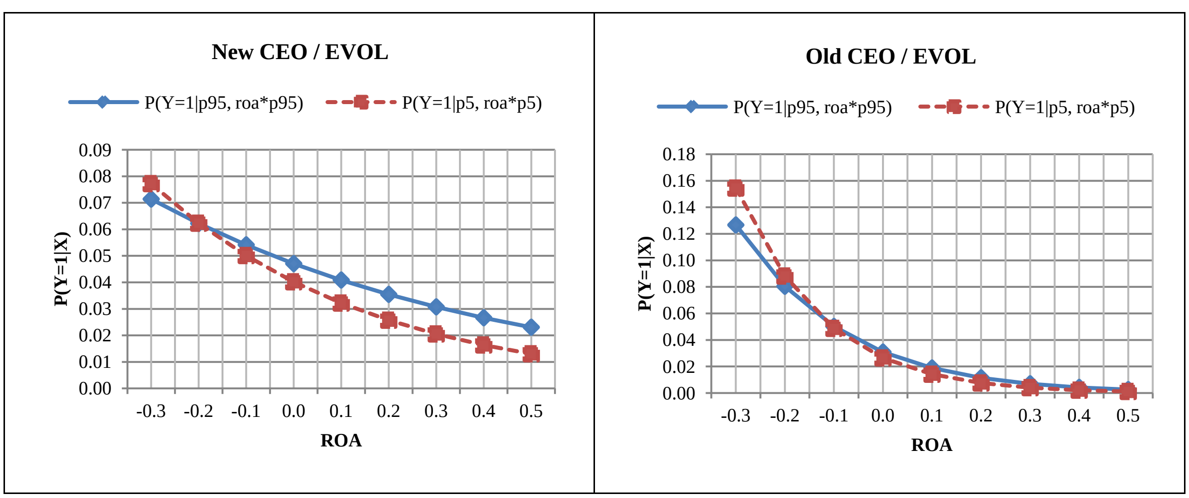
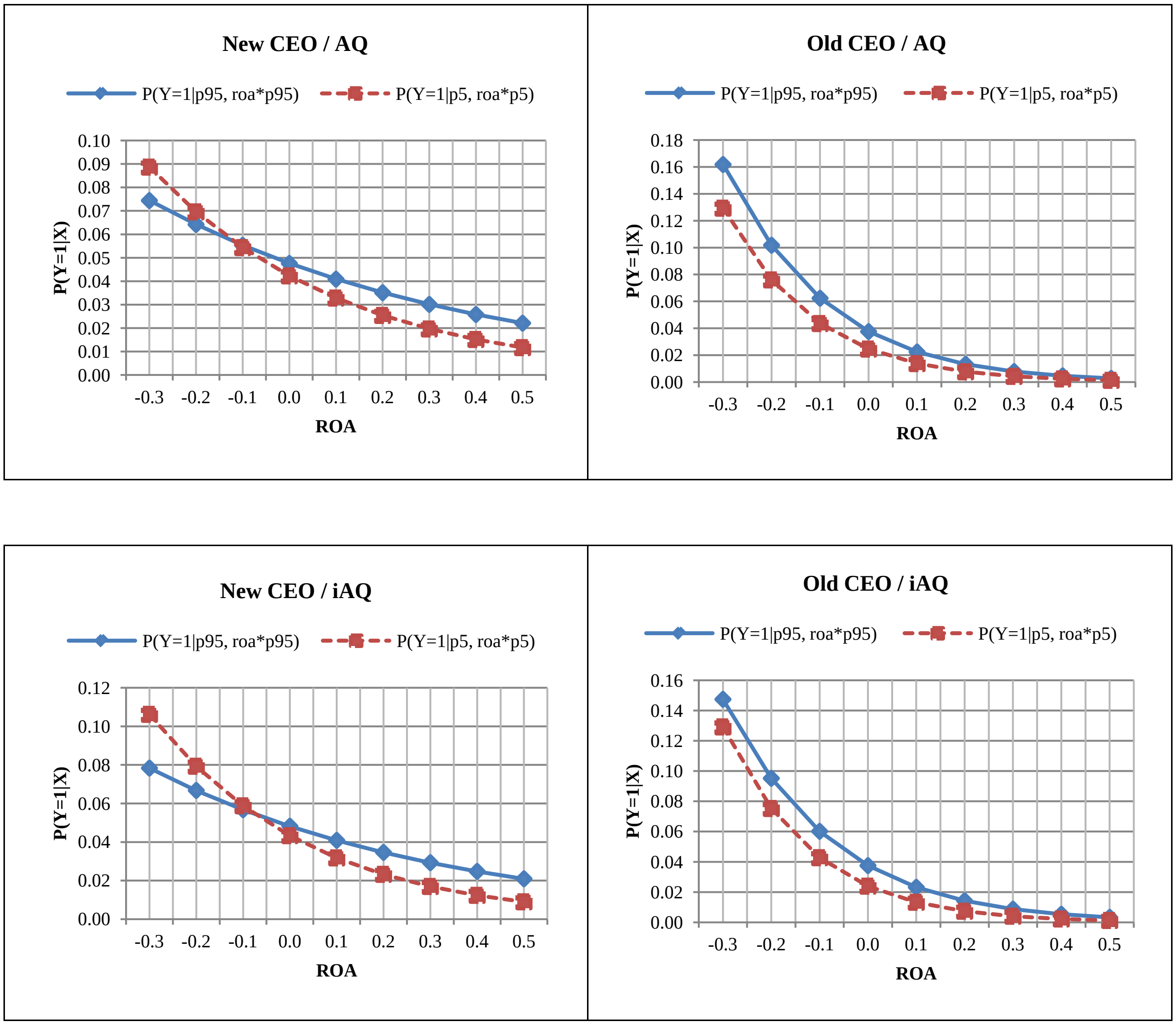


Figure 2.4, continued



An important finding in this section is that stock liquidity and iAQ both have economically significant effect on sensitivity of forced CEO turnover to firm performance. Their effects are mainly driven by their effect on the board's updating process rather than on threshold CEO ability. Table 2.15 shows that going from the top to bottom decile of stock return increases the probability of forced CEO turnover by about 14% in firms with high stock liquidity (the 95th percentile) but only by about 4% in firms with low stock liquidity (the 5th percentile). As for iAQ, going from the top to bottom decile of industry-adjusted ROA increases the probability of forced

CEO turnover by about 4% in firms with low iAQ and by 2.7% in firms with high iAQ. The difference of 1.3% is small in the absolute sense but it is about 48% of the 2.7% increase in probability of forced CEO turnover in firms with high iAQ.

These results show that even when we compare sensitivity of CEO turnover to firm performance in different types of firms, in our case, the high and low information quality firms, information quality can have significant economic effect. In summary, noises arising from stock market trading or estimation errors in accruals are not significantly correlated with the threshold CEO ability so their effect on the board's updating process is directly reflected as the same direction effect on sensitivity of CEO turnover to firm performance. On the other hand, noises arising from exogenous firm-specific shocks are correlated with threshold CEO ability so their effect on sensitivity of CEO turnover to firm performance may be different from their effect on the board's updating process.

6. Conclusion

This study develops a uniform framework for measuring information quality of stock prices and accounting earnings to study the impact of information quality of stock returns and accounting earnings on the board's updating on estimate of CEO ability from firm performance under a Bayesian learning framework. Under this framework, I classify noises in stock prices and accounting earnings into two types. The first type of noise is related to exogenous shocks to firm performance that is not under the control of the CEO. It is measured by stock volatility and earnings volatility respectively. The second type of noise is related to the potential differences between observed firm performance and the unobserved true underlying firm performance. For stock prices, I measure it by stock liquidity and dispersion in analysts' earnings forecasts. For accounting earnings, I measure it by accrual quality. I consider the information quality of a performance measure low if noise in the performance measure is high and vice versa. The main hypothesis I test is that noises in firm performance measures reduce the sensitivity of the board's update on estimate of CEO ability to these firm performance measures. Consistent with this hypothesis, I find that all my information quality proxies have significant effect on the board's updating process as predicted by the main hypothesis. The effect is both statistically and economically significant when appropriately calculated. However the effect is not uniform between stock price information quality proxies and accounting information quality proxies, and between firms with newly-hired CEOs and firms with relatively longer-tenured CEOs. The economic effect is much stronger for stock price information quality proxies than for accounting information quality proxies. Most importantly, I find that the information quality effect is mainly driven by firms with recently hired CEOs where the need for learning is likely to be greater.

Furthermore, I find that the effect of accrual quality is mainly driven by unavoidable estimation errors. Discretionary accruals (i.e. intentional estimation errors) are found to have no systematic effect on the board's updating on estimate of CEO ability based on accounting earnings, consistent with boards of directors being able to undo the intentional estimation errors when making CEO retention decisions.

In terms of economic effect, although the inferred effect on sensitivity of forced CEO turnover to firm performance is economically significant for all information quality proxies, the effect are not necessarily reflected as the same direction differences in sensitivity of CEO turnover to firm performance across firm types defined by the information quality proxy. For stock price information quality proxies, I find that sensitivity of forced CEO turnover to stock return is higher in firms with high stock liquidity (low dispersion in analysts' earnings forecasts) than in firms with low stock liquidity (high dispersion in analysts' earnings forecasts). However, the sensitivity of forced CEO turnover to firm performance is even a little bit higher in firms with high stock volatility than in firms with low stock volatility, which is opposite to the effect of stock volatility on the board's updating on estimate of CEO ability based on stock returns. For accounting information quality proxies, I find that the sensitivity of forced CEO turnover to industry-adjusted ROA is lower in firms with low accrual quality than in firms with high accrual quality. This is mainly driven by firms with intrinsic propensity to make larger unavoidable estimation errors. Although the sensitivity of forced CEO turnover to industry-adjusted ROA is lower in firms with high earnings volatility than in firms with low earnings volatility, the difference is quite small.

The results in this paper suggest that the commonly-held view that the strength of the relation between sensitivity of forced CEO turnover to firm performance is positively related to the quality of internal governance overlooks the cross-sectional variations in turnover-performance sensitivity due to information quality. The relative importance of quality of internal corporate governance and information quality in explaining turnover-performance sensitivity is likely to change with CEO tenure. For relatively new CEOs, information quality is more important; while, for relatively long-tenured CEOs, quality of internal corporate governance is more important.

The policy implication of this study is that lawmakers, regulators and large shareholders looking for ways to improve corporate governance should not focus their attention only on changing the board structure and director incentives. Rather, they should also look for ways to improve information quality of performance signals, especially through improving stock market liquidity, so that rightly-incentivized directors have the right information to do their job. This broader view on how to improve corporate governance becomes more important today as new regulations in the past decade or so in the U.S. seem to have improved board monitoring in U.S. publicly listed firms and we may be facing a decreasing return to further improvement in internal governance systems.

References

Ai, C., & Norton, E. (2003). Interaction terms in logit and probit models. *Economics Letters , 80*, 123-129.

Amihud, Y. (2002). Illiquidity and stock returns: cross-section and time-series effects. *Journal of Financial Markets , 5*, 31-56.

Brickley, J. A. (2003). Empirical research on CEO turnover and firm-performance: A discussion. *Journal of Accounting and Economics , 36*, 227-233.

Bushman, R., Dai, Z., & Wang, X. (2010). Risk and CEO turnover. *Journal of Fianncial Economics , 96* (3), 381-398.

Chen, Q., Goldstein, I., & Jiang, W. (2007). Price informativeness and investment sensitivity to stock price. *Review of Financial Studies , 20* (3), 619-650.

Chordia, T., Roll, R., & Subraymanyam, A. (2008). Liquidity and market efficiency. *Journal of Financial Economics , 87*, 249-268.

Dechow, P. M., Sloan, R. G., & Sweeney, A. P. (1995). Detecting earnings management. *The Accounting Review , 70*, 193-225.

Dechow, P., Ge, W., & Schrand, C. (2010). Understanding earnings quality: a review of the proxies, their determinants and their consequences. *50*, 344-401.

DeFond, M. L., & Park, C. W. (1999). The effect of competition on CEO turnover. *Journal of Accounting and Economics , 27*, 35-36.

Demsetz, H., & Lehn, K. (1985). The structure of corporate ownership: causes and consequences. *Journal of Political Economy , 93*, 1155-1177.

Denis, D. J., Denis, D. K., & Sarin, A. (1997). Ownership structure and top executive turnover. *Journal of Financial Economics , 45*, 193-221.

Desai, H., Hogan, C., & Wilkins, M. (2006). The reputation penalty for aggressive accounting: earnings restatements and management turnover. *The Accounting Review , 81*, 83-112.

Diether, K. B., Malloy, C. J., & Scherbina, A. (2002). Differences of opinion and cross section of stock returns. *The Journal of Finance , 57*, 2113-2141.

Durnev, A., Morck, R., & Yeung, B. (2004). Value enhancing capital budgeting and firm-specific stock return variation. *Journal of Finance , 59*, 65-105.

Engel, E., Hayes, R. M., & Xue, W. (2003). CEO turnover and properties of accounting information. *Journal of Accounting and Economics , 36*, 197-226.

Gilchrist, S., Himmelberg, C., & Huberman, G. (2005). Do stock price bubbles influence corporate investments? *Journal of Monetary Economics , 52*, 805-827.

Goyal, V. K., & Park, C. W. (2002). Board leadership structure and CEO turnvoer. *Journal of Corporate Finance , 8*, 49-66.

Graham, J. R., Campbell, H. R., & Rajgopal, S. (2005). The economic implication of corporate financial reporting. *Journal of Accounting and Economics , 40*, 3-73.

Grossman, S., & Stiglitz, J. E. (1980). On the impossibility of informationally efficient markets. *The American Economic Review , 70* (3), 393-408.

Hasbrouck, J. (2009). Trading costs and returns for US equities: estimating effective costs from daily data. *Journal of Finance , 64* (3), 1445-1477.

Hazarika, S., Karpoff, J. M., & Nahata, R. (2009). Internal corporate governance, CEO turnover, and earnings management. *Journal of Financial Economics , Forthcoming*.

Hermalin, B. E., & Weisbach, M. S. (1998). Endogenously chosen boards of directors and their monitoring of the CEO. *American Economic Review , 88* (1), 96-118.

Holmstrom, B., & Tirole, J. (1993). Market liquidity and performance monitoring. *Journal of Political Economy , 101*, 678-709.

Huson, M. R., Parrino, R., & Starks, L. T. (2001). Internal monitoring mechanisms and CEO turnover: a long-term perspective. *Journal of Finance , LVI* (6), 2265-2297.

Jayaraman, S., & Milbourn, T. T. (2011). The role of stock liquidity in executive compensation. *The Accounting Review* , Forthcoming.

Karpoff, J., Lee, D., & Martin, G. (2008). The consequences to managers for financial misrepresentation. *Journal of Financial Economics , 88*, 193-215.

Kothari, S. P., Leone, A. J., & Wasley, C. E. (2005). Performance matched discretionary accrual measures. *Journal of Accounting and Economics , 39*, 163-197.

Kyle, A. S. (1985). Continuous auctions and insider trading. *Econometrica , 53*, 1315-1335.

Loughran, T., & Marietta-Westberg, J. (2005). Divergence of opinion surrounding extreme events. *European Financial Mangement , 11* (5), 579-601.

Mikkelson, W. H., & Partch, M. M. (1997). The decline of takeovers and disciplinary managerial turnover. *Journal of Financial Economics , 44*, 205-228.

Morck, R., Shleifer, A., & Vishny, R. W. (1989). Alternative mechanism for corporate control. *American Economic Review , 79*, 842-852.

Parrino, R. (1997). CEO turnover and outside succession: A cross-sectional analysis. *Journal of Financial Economics , 46* (2), 165-197.

Perry, T. (1999). Incentive compensation for outside directors and CEO turnover. *Working Paper*, Arizona State University.

Powers, E. A. (2005). Interpreting logit regressions with interaction terms: an application to the managemetn turnover literature. *Journal of Corporate Finance , 11*, 504-552.

Shleifer, A., & Vishny, R. (1997). The limits of arbitrage. *The Journal of Finance , 52*, 35-55.

Srinivasan, S. (2005). Consequences of financial reporting failure for outside directors: evidence from accounting restatements and audit committee members. *Journal of Accounting Research , 43*, 291-334.

Taylor, L. (2010). Why CEOs are rarely fired? evidence from structural estimation. *Journal of Finance , 65* (6), 2051-2087.

CHAPTER III

INDUSTRY COMPETITION, FIRM PROFITS AND CEO TURNOVER

1. Introduction

It is widely believed that product market competition reduces managerial slack. This view can be traced back to Adam Smith, who wrote in 1776 that “monopoly … is a great enemy to good management”, and has greatly influenced economic thinking and policy making since then. When the product market is perfectly competitive, this is clearly true because all firms have to maximize profits to survive and there is no room for managerial slack. However, most product markets in real life can best be described as imperfectly competitive. Although some empirical studies find evidence that is consistent with the notion that firms in competitive industries operate more efficiently (For example, Nickell, 1996; Giroud and Mueller, 2010), theoretical studies have had a difficult time to clearly demonstrate the precise mechanism through which product market competition reduces managerial slack in an imperfectly competitive market. It is fair to say that until today economists still do not have a complete understanding of this issue despite the progresses that have being made.

Very often we hear people making the following argument: while managers in firms in non-competitive industries can afford to enjoy “quite life” (and they would do so), managers in firms in competitive industries have to continuously improve efficiency so that their firms would survive. Although there is some truth to this argument, for example, Schmidt (1997) formalizes it as the “threat-of-liquidation” effect and shows that this effect unambiguously induces managers to work harder to reduce cost in order to avoid the disutility of liquidation, this argument is incomplete. It leaves the matter of providing incentives to managers entirely to the hand of market forces while, in reality, internal governance by boards of directors also plays an important role in setting incentives to managers. Taking internal governance into account, Jensen and Meckling (1976) argue that "the owners of a firm with monopoly power have the same incentive to limit divergence of the manager from value maximization … as do the owners of competitive firms" and "both will undertake the level of monitoring which equates the marginal cost of monitoring to the marginal wealth increment from reduced consumption of perquisite by the manager." Since Jensen and Meckling (1976) do not explore how competition may affect the trade-off between marginal cost and marginal benefit of monitoring, they implicitly conclude that competition has no effect on agency costs. However, when the potential effect of competition on optimal incentive scheme is being taken into accounts, existing theoretical papers that have tried to formalize the notion that competition reduces managerial slack in general produce ambiguous predictions. Hart (1983) provides a hidden-information model and demonstrates that competition mitigates managerial slack through the price mechanism. However, Scharfstein (1988) shows that this result critically depends on the specification of managerial utility function. The effect can be completely reversed, i.e. competition can exacerbate incentive problem, when managerial preference is specified differently. Hermalin (1992) decomposes the effect of competition on executive behavior into four components and finds that each of the four components is of ambiguous sign. Schmidt (1997) shows that although the "threat-of-liquidation" effect unambiguously reduces managerial slack; an increase in competition, at the same time, change the profitability of cost reduction effort so that the total effect of competition on managerial incentives is of ambiguous sign. An interesting paper is Raith (2003). He provides a model that gives some unambiguous predictions on the relation between competition and managerial incentives. Unlike previous studies, Raith (2003) treats market structure as endogenous and allows the degree of competition to vary along three dimensions. He shows that the relation between competition and managerial incentives is unambiguous but depends on which of the three competition fundamentals included in his model drives the variations in the degree of competition.

Given the ambiguity in existing theory, it is important for empirical researchers to provide more empirical evidence that will shed light on which elements in these theories are dominant in empirical data. This should help us to better understand the underlying forces of competition and guide the development of new theories that will further our understanding of the working of competition. The purpose of this paper is to provide more empirical evidence on this issue using CEO turnover data in U.S. publicly listed firms.

I consider that this work is important for understanding the link between product market competition and managerial slack for the following reasons. First, threat of termination is an integral part of the total incentives provided to CEOs. To a CEO, the career consequence of firm liquidation is usually the same as dismissal – the loss of the CEO position. However, although people often relate product market competition to threat of liquidation, much less attention has been paid so far to the relation between competition and the threat of termination – a link that is less intuitive but has potentially just as strong an effect on managerial slack. Second, many people associate managerial slack in non-competitive industries with the enjoyment of "quite life". As Hicks (1935) put it, "the best of all monopoly profits is a quite life." It seems that a necessary condition for CEOs in non-competitive industries to enjoy "quite life" is continued employment. If boards are just as likely to fire CEOs who shirks in monopoly firms as they do in competitive firms, product market competition may have a much smaller effect on managerial slack than economists have assumed. This would cast doubt on the proposition that competition reduces managerial slack. Third, besides shirking, boards also fire CEOs for reasons related to low CEO ability or poor CEO and firm matching. According to the theory by Hermalin (1992), Schmidt (1997) and Raith (2003), competition may change the relative value of managers' cost reduction efforts and thus affect optimal incentives. In general, when an increase in competition increases the relative value of managers' cost reduction efforts, firms are expected to offer stronger incentives to managers and vice versa. It is reasonable to expect that a situation where cost-reduction effort is more important is also a situation where cost-reduction *ability* is more important. In many situations, new talent for cost reduction could well be more important than higher level of effort of the incumbents. Hence, CEO turnover studies like this may provide cleaner and stronger evidence than studies focusing on executive compensation alone. Here, cost reduction could refer to any action of the manager to increase the profitability of his firm, including but not limited to cutting cost, reorganizing his company, investing in new growth options, etc.

The second purpose of this study is to provide new empirical evidence on how product market competition affects executive turnover. In the corporate governance literature, prior evidence on this relation is very limited. DeFond and Park (1999) measure product market competition by Herfindahl-Hirschman Index (HHI). They find that the frequency of CEO turnover is higher and the relation between industry-adjusted accounting performance and CEO turnover is stronger in low concentration industries. However, Ali, Klasa and Yeung (2009) show that the DeFond and Park (1999) finding, which uses Compustat-based measure of industry concentration, is not robust to using US Census-based industry concentration measures. Fee and Hadlock (2000) test a number of theories on the relation between competition and managerial incentives using data on non-CEO executive turnover in the US newspaper industry. They measure competition by the number of newspapers in a local market. They find no support for any of the theories they examined and the only hypothesis that is supported by their data is that non-CEO executives are more likely to look for alternative jobs when market competition increases the likelihood of liquidation of their firms. Therefore, existing evidence has shown no clear relation between product market competition and the board's decision to fire managers.

This paper departs from DeFond and Park (1999) and Fee and Hadlock (2000) by treating market structure as endogenous. This departure reflects a view that is gradually getting popularity in the industrial organization literture which says that market structure is endogenous in the long run. Raith (2003) shows that, when market structure is endogenous, HHI or number of firms are poor measures of competition and their relations with managerial incentives are ambiguous. This may explain the lack of clear evidence in prior studies.

Following Raith (2003), I characterize product market competition by three variables – product substitutability, market size and entry costs – and examine how each of the three variables is related to the likelihood of forced CEO turnover and the sensitivity of forced CEO turnover to firm performance. I measure product substitutability in an industry by the median net profit margin (NPM). In the industrial organization literature, industry NPM is often used as a proxy for the Lerner index which is negatively related to price elasticity of demand facing a firm. I measure market size by the industry total sales but I control for industry concentration at the same time so that conditional on industry concentration cross-industry comparison of market size captures the differences in average firm-level output in an industry. As I will discuss in more detail later, firm-level output drives the effect of market size on optimal managerial incentives in the Raith model. Lastly, I measure entry costs by the weighted average costs of gross property, plant and equipment (PPE).

My results show that industry NPM is significantly negatively associated with the likelihood of forced CEO turnover in most specifications but is significantly positively associated with the sensitivity of forced CEO turnover to firm performance. While the former effect is consistent with the prediction of the Raith model, the latter effect does not seem to be so but, as I will discuss later, it does not completely reject the prediction of the Raith model either. Conditioning on industry concentration, industry total sales is significantly positively associated with both the likelihood of forced CEO turnover and the sensitivity of forced CEO turnover to firm performance – both are consistent with the prediction of the Raith model. On the other hand, industry PPE is significantly negatively associated with both the likelihood of forced CEO turnover and the sensitivity of forced CEO turnover to firm performance, which is opposite to the prediction of the Raith model. However, the evidence on entry cost is intuitively consistent with entry cost being negatively associated with the threat of liquidation and thus being consistent with the prediction in Schmidt (1997) that threat of liquidation is negatively related to the cost of inducing higher level of effort.

Since the Raith (2003) model is built on strategic interactions between firms in an oligopoly industry, I further examine whether the three fundamental dimensions of competition are only important in oligopoly industries. I find support for this hypothesis. Specifically, I find that all my results are driven by the subsample of industries with concentration levels above the sample median. Overall, I conclude that the theoretical model of Raith (2003) still cannot explain all important empirical regularities in the data though it does seem to capture some important dimensions of competition in oligopoly industries.

The rest of the paper is organized as follows. Section 2 reviews the theoretical literature on product market competition and managerial incentives and develops the hypothesis. Section 3 describes data and sample. Section 4 reports empirical results. Section 5 reports results from robustness tests. Section 6 concludes.

2. Literature review and hypotheses development

Despite its simplicity and intuitive appeal, the proposition that product market competition helps to reduce managerial slack has proved to be difficult to formulate theoretically. The early papers focus on the effect of competition on information structure of the agency problems. The basic idea is that when a firm's performance can be compared with that of other firms in the same industry, relative performance evaluation can be used to provide more powerful incentives (Holmstrom, 1982; Nalebuff and Stiglitz, 1983). However, as argued by Schmidt (1997), it is not sufficient to conclude from this that the optimal incentive level is higher in more competitive firms than in others, because the level of incentive also depends on the trade-off between the marginal cost and marginal benefit of providing a higher level of incentive. Hart (1983) provides a model in which competition can affect managerial incentives even when each manager's wage depends on its own firm performance. In his model, the common shocks to firms' costs are transmitted through market price. There are two types of firms – the entrepreneurial firms in which costs are always observable to owners and the managerial firms in which costs are not observable to owners. When costs are high, both entrepreneurial firms and managerial firms operate efficiently. However, when costs are low, managers in managerial firms may shirk. But if the proportion of entrepreneurial firms is increased, then industry output will increase and price will fall; as a result, managers in managerial firms will have to work harder to meet her fixed profit target. Hence, competition unambiguously reduces managerial slack. However, Scharfstein (1988) shows that this result critically depends on the assumption that managers are infinitely risk reverse to change in income so that a fixed profit target contract is optimal. When marginal utility of income is assumed to be positive and finite, Scharfstein (1988) finds that competition actually exacerbate the incentive problem. This is because managerial firms now operate inefficiently when costs are high. When the proportion of entrepreneurial firms increases, industry output increases and price falls. This further lower the profit target at high costs state for managers in managerial firms which makes shirking more attractive.

Another link between competition and managerial incentives that have been explored in the literature is that competition may change the sensitivity of firm profits to cost-reduction actions of managers. Hermalin (1992) calls this the "change-in-relative-value-of-actions" effect. Schmidt (1997) calls it the "value-of-a-cost-reduction" effect. Specifically, if an increase in competition makes cost-reduction actions of managers more profitable, the optimal level of incentives is likely to be higher. On the other hand, if an increase in competition drives the marginal cost of providing the current level of incentives above the marginal benefits, the optimal level of incentives will be lower. Hence, this effect in general has ambiguous sign. Unlike Hermalin (1992) and Schmidt (1997) who do not discuss what kinds of changes in market fundamentals will increase or decrease the profitability of cost-reduction actions, Raith (2003) explicitly models three forms of change in fundamentals of competition in an oligopolistic industry. They are product substitutability, market size and entry costs. In his model, an increase in competition due to greater product substitutability has two opposite effects on managerial incentives. First, there is a business-stealing effect which calls for greater managerial incentives. This is because a reduction in costs can steal more business from rivals as demand elasticity increases. Second, there is a scale effect which reduces managerial incentives. This is because, with greater product substitutability, a firm's rivals are more likely to reduce costs and charge lower prices, which tends to reduce the market share of the firm and makes a cost-reduction effort look less attractive. Like prior studies, Raith (2003) finds that when the number of firms is fixed, the sign of the net effect is ambiguous. However, when market structure is allowed to be endogenously determined by free entry and exit, Raith (2003) finds that change in each of the three parameters of competition in his model has unambiguous effect on managerial incentives. The key intuition of the model is that in equilibrium firm provides stronger incentives to managers when competition increases firm-level output and vice versa. The main predictions of the model are as follows: First, for any fixed number of firms, greater product substitutability leads to lower price and lower profit for every firm in the market, which encourages some firms to exit. In equilibrium, the remaining firms each produce a larger output and thus provide stronger incentives to managers. Second, an exogenous increase in market size leads to higher profit for any given number of firms and encourages entry. However, since the increase in number of entries is proportionally less than the increase in market size, the net effect is that each firm produces more output and thus provides stronger incentives. Finally, an exogenous decrease in entry cost leads to the entry of new firms, which, in equilibrium, reduces the output of each firm and thus each firm provides weaker incentives to managers. One important message sent by the Raith (2003) model is that concentration indices alone are poor measures of degree of competition with free entry and exit. An increase in concentration may be related to an increase or decrease in competition depending on what other aspects of competition have also changed. For example, when the increase in concentration is caused by an increase in product substitutability, higher concentration is related to higher degree of competition and incentives. On the other hand, if the increase in concentration is caused by a decrease in demand, higher concentration is related to lower degree of competition and incentives because, according to thee Raith (2003) model, firm-level output would decrease more than proportionally than the number of firms that exit the industry. Besides concentration indices being poor measures of competition, Raith (2003) also suggests that the relation between competition and managerial incentives is better captured by a combination of a variety of fundamental factors of competition – three of which are present in his model.

Although the idea that competition may affect returns to cost-reduction effort is present in several theory papers I have discussed, Raith (2003) is the first to explicitly demonstrate what changes in market structure increase or decrease returns to cost-reduction efforts, which allows me to empirically model these changes and be able to conduct empirical tests on the proposed mechanism. Thus, in this study, I use the predictions in the Raith (2003) model to develop my hypotheses. I measure competition by the three fundamental factors proposed in Raith (2003) – product substitutability, market size and entry costs – and examine their joint relations with CEO turnover. For each of the three competition fundamentals, I hypothesize its relation with both the likelihood of forced CEO turnover and the sensitivity of forced CEO turnover to firm performance based on its relation with value of cost reduction effort in Raith (2003). The general argument is that, in equilibrium, if a factor is positively related to the value of cost reduction, then it should be positively related to the likelihood of forced CEO turnover and the sensitivity of forced CEO turnover to firm performance. This is because, first, as the value of cost reduction increases, it is optimal for firms to provide stronger incentives to CEOs to engage in cost reduction; second, the expected gain from hiring a CEO with a higher ability of cost reduction is also more likely to exceed the costs of CEO turnover, which makes the option of replacing the incumbent CEO more attractive. Specifically, I have the following six hypotheses:

- H1a: Product substitutability is positively associated with the likelihood of forced CEO turnover.
- H1b: Product substitutability is positively associated with the sensitivity of forced CEO turnover to firm performance.
- H2a: Market size is positively associated with the likelihood of forced CEO turnover conditional on industry concentration.
- H2b: Market size is positively associated with the sensitivity of forced CEO turnover to firm performance conditional on industry concentration.
- H3a: Entry cost is negatively associated with the likelihood of forced CEO turnover.
- H3b: Entry cost is negatively associated with the sensitivity of forced CEO turnover to firm performance.

3. Sample and variables

The sample consists of all firms with data available on ExecuComp, Compustat and CRSP from 1992-2008. Both ExecuComp and Compustat cover firms in the S&P 1500 index currently or from 1992 forward but data before 1994 are mostly for S&P 500 firms. I use the ExecuComp database to obtain my CEO turnover sample. Industry competition measures are constructed from Compustat segment database and Compustat industrial annual database at the four-digit SIC code level. Firm financial data are from Compustat and stock return data from CRSP. Corporate governance variables are from the RiskMetrick (formerly IRRC) database. The coverage of RiskMetics' directors database is also S&P 1500 firms but the data is only available from 1996 onward. Hence, in specifications with corporate governance controls, the regressions are estimated using data from 1996 to 2008.

I exclude financial and utility firms (SIC codes 4900-4999 and 6000-6999) because these industries are regulated and the extent of product market competition is restricted. Furthermore, I require that each industry to include at least 4 firms so that I can study the competition among them.

3.1. CEO turnover sample

The ExecuComp database contains information on annual compensation for up to five top executives in firms that are in the S&P 1500 index. The database allows me to track the CEO's identity over time and thus, to identify a CEO turnover when the CEO's identity changes from one fiscal year to the next. I then search the *Factiva* and *Lexis-Nexis* databases to find the earliest turnover announcement date and code the stated reasons for the turnover. Since I associate each CEO turnover with the industry and firm characteristics in the year prior to the turnover announcement, the sample period is 1996-2007. Year 1996 is the first year that board data is available from RiskMetrics (formerly IRRC) and year 2007 is the last year I have information about CEO turnovers announced in the following year.

For my analysis, I am mainly interested in CEO turnovers that result from board disciplinary actions. However, many CEO turnovers occur for other reasons like retirements, health problems, deaths, departures for more attractive positions, etc. Consequently, I classify CEO turnovers into forced and voluntary. To determine whether a CEO turnover is forced or voluntary, I follow the classification method used in Parrino (1997), which is commonly used in recent CEO turnover studies (e.g., Huson, Malatesta and Parrino, 2004; Hazarika, Karpoff and Nahata, 2009). Specifically, a CEO turnover is classified as forced if (i) news articles mention that a CEO was fired, forced out or left due to unspecified policy differences; (ii) the CEO is under the age of 60 and the news did not mention death, poor health, or the acceptance of another position (within the firm or elsewhere) as the reason for the departure; (iii) a reported CEO retirement is not announced at least six months in advance. For this third group, I check a wider range of news to make sure that no other articles suggest that the turnover is voluntary in nature. These CEO turnovers are reclassified as voluntary if the incumbent takes a comparable position elsewhere or departs for reasons previously undisclosed that are unrelated to firm activities. The remaining CEO turnovers are classified as voluntary. The CEO turnover sample is then combined with the no turnover years of these firms in my sample period to produce a panel database containing CEO turnovers between1996 and 2007. I further exclude firm-years that are related to interim CEOs or co-CEOs and CEO turnovers that result from bankruptcies, spin-offs and mergers and acquisitions.

I identify 2,111 turnovers, 594 are classified as forced and 1,517 are classified as voluntary. On average, CEO turnovers happen in about 11% of the firms each year, with about 3% of the firms having forced CEO turnovers. As a percentage of all turnovers, forced turnovers account for about 28% of all turnovers in my sample. The overall CEO turnover rate is the same as the 11% rate in Huson et al. (2001) who construct their sample of CEO turnovers using *Forbes* compensation surveys from 1971 to 1994. The percentage of all CEO turnovers that are forced is comparable to those in Jenter and Kanaan (2010) and Hazarika et al. (2009). Like us, Jenter and Kanaan (2010) construct their CEO turnover sample from ExecuComp database. They find that forced CEO turnovers account for 24% of the 1,590 CEO turnovers in their sample from 1993-2003. Hazarika et al. (2009) build their CEO turnover sample from both ExecuComp and Forbes annual compensation surveys. In their sample of 1,895 CEO turnovers from 1992-2004, 21% are forced turnovers.

3.2. Measures of product market competition

Measures of product market competition are calculated at the four-digit SIC industry level using Compustat segment and Compustat industrial annual data. To account for the fact that some firms have segments operating in different industries, measures of industry competition are calculated using segment level data rather than firm level data. The segment file of Compustat reports net sales, operating income after depreciation, assets, capital expenditures and depreciation, at the segment level for all active Compustat firms other than utility subsidiaries. Compustat assigns a primary and a secondary SIC code to each business segment of the firm. I use the primary SIC code of a segment as the industry code for that segment. The segment data are then merged with firm level data from the Industrial Annual file of Compustat by firm and year. To exclude observations with potentially erroneous segment data, I require that the sum of sales in all segments of a firm be within 10% of the firm level sales from the Industrial Annual file. Firms that are in the industrial annual file but are not in the segment file are included as single-segment firms in our sample. For variables that are not reported in the segment file, I allocate the corresponding firm level value to each segment based on the sales share of each segment in the firm. For example, the segment-level property, plant and equipment (PPE) is calculated as the firm-level PPE multiplied by the sales share of the segment in the firm.

I measure product substitutability in an industry by the median net profit margin (NPM) for all segments in that industry. At the segment level, the NPM is computed as operating income before depreciation and amortization divided by sales. In the industrial organization literature, industry NPM is widely used as a proxy for the Lerner Index, which measures the market power of firms to set prices above marginal costs. The Lerner index ranges from a maximum of 1 to a minimum of 0 with higher values implying greater market power and 0 implying no market power. It is can be shown that the Lerner index equals to the negative reciprocal of price elasticity of demand facing a firm. Hence, larger values of NPM indicate less product substitutability and stronger market power and vice versa. This measure is used by Giroud and Mueller (2010) to define competitive vs. non-competitive industries. Karuna (2007) uses price-cost margin calculated as the sales divided by operating costs to proxy for product substitutability, where operating costs equal to the sum of costs of goods sold, selling, general, and administrative expenses and depreciation. Since operating costs are missing for many observations, I choose to use the net profit margin to proxy for product substitutability.

As for market size, in the original model of Raith (2003), market size equals to the density of demand on a unit circle along which competing firms are evenly distributed. An increase in demand attracts more entrants; however, since the increase in number of firms is proportionally less than the increase in demand, each firm still produces more than it produces before and thus provides stronger incentives to managers. To proxy for market size, Karuna (2007) uses industry total sales. Although this is a valid proxy for industry demand in general, what the market size parameter in Raith (2003) model tries to capture is not industry demand but firm-level demand (or output). I emphasize this point because firm-level output plays a key role in Raith (2003) model and drives the predictions of the model[28]. This point is easily missed because in the original model each firm in an industry is assumed to produce the same output, thus, industry sale has a one-to-one correspondence with firm-level output. However, when we take the model to empirical tests, we need to make this distinction and be clear about what we want to proxy for. In empirical data, besides difference in industry sales, both the number of firms and the distribution of industry sales across firms within an industry vary across industries. Direct comparison of industry sales across industries probably reveals little about cross-industry differences in average firm-level output. Even when two industries have the same industry sales and the same number of firms, differences in the distribution of the industry sales across firms in the same industry may still affect the firm-level output of the average firm in each industry and thus the average level of managerial incentives provided in the industry. Therefore, I measure market size by industry sales but at the same time I also control for the Herfindahl-Hirschman index (HHI) of the industry so that conditional on a given level of HHI, comparison of industry sales captures the cross-industry differences in average firm-level output. I expect that for a given level of industry sales larger HHI indicates higher concentration of firms in the industry and thus higher average firm-level output for the industry as a whole and vice versa. Similarly, for a given

[28] Raith (2003) p1430: "how incentives are related to competition depends on how firm-level output varies with the degree of competition."

level of HHI, larger industry sales indicate higher firm-level output for the industry as a whole and vice versa. I need to emphasize that although Karuna (2007) also uses industry sales as a proxy for market size and control for HHI in his regressions of equity incentives on proxies for industry competition, the reason he gives for including HHI is quite different from mine. I consider that the inclusion of HHI makes comparison of industry sales across industries meaningful, whereas he treats HHI as an additional industry competition measure that needs to be controlled for. Since the main point of the Raith (2003) model is that industry concentration is endogenous, interpreting it as another determinant of industry competition seems to be inconsistent with the model.

Following Karuna (2007), I measure entry costs by the weighted average costs of gross property, plant and equipment (PPE) of all segments in an industry where the weight is each segment's sales share in the industry. An alternative measure of entry costs may be the minimum segment PPE in an industry. However, I prefer the weighted average PPE measure because, when there is economy of scale, the weighted average costs of PPE probably better reflects the efficient scale of operation in an industry than the minimum costs of PPE in an industry. Moreover, the SIC industry does not coincide with actual product market. A firm with minimum total assets may be a peripheral firm which does not produce the product that most of the firms in the industry are competing on. Using the average of all firms in an industry reduces the risk of selecting an unrepresentative firm. Lastly, to make the competition measures comparable over time, all dollar value industry competition measures are adjusted for inflation to year 2000 dollars using CPI index.

I then merge CEO turnover data with industry competition data by year and the SIC code of the primary segment of each firm. The primary segment of each firm is defined as follows: if the firm has a segment with the same SIC code as the firm's, then that segment is the primary segment. Else, the segment with the largest sales is the primary segment. This merging procedure implicitly assumes that managerial incentives are only affected by product market competition in a firm's primary segment industry. For large conglomerates, a firm may have more than one primary markets and this assumption may be violated. However, for the majority of firms in my sample, I consider this to be a valid characterization. Even for conglomerates, as long as the consideration for the primary segment dominates that for other segments when setting incentives or making CEO turnover decisions, this assumption would still be valid.

3.3. Measures of firm performance

To measure stock performance, I use a firm's monthly stock return adjusted by the return on value-weighted CRSP index cumulated over the 12-month period that ends one month prior to the CEO turnover announcement date for years associated with CEO turnover events and over the prior fiscal year for years associated with no CEO turnovers. This measure is used in Weisbach (1988) and some other CEO turnover studies. The use of market adjustment is supported by results in Warner, Watts and Wruck (1988) and Barro and Barro (1990) who find that market component of stock return is at least partially filtered out in CEO turnover decisions and results in Jenter and Kanaan (2010) who find that highly-visible market benchmarks are removed from CEO turnover decisions.

I measure a firm's accounting-based performance by change in ROA in the year prior to CEO turnover, where ROA is defined as annual earnings before interest and taxes (EBIT) scaled by total assets at the beginning of the fiscal year. This performance measure is not affected by changes in capital structure and tax treatments of the firms. The median of this ratio for all Compustat firms in the firm's Fama and French 48 industry is then subtracted from the firm's change in ROA to obtain the industry-adjusted ROA. Most researchers believe that some kind of filtering of exogenous shocks is used in evaluating CEO performance though there is no agreement as to the exact comparison group being used. Industry adjustment based on two-digit SIC industry or Fama-French industry is typically used in prior CEO turnover studies such as

(Weisbach, 1988; DeFond and Park, 1999; Huson et al., 2001; Engel et al, 2003). I use the Fama-French industry though results are similar to those obtained using four-digit SIC industry.

Table 3.1 reports descriptive statistics of the final sample. Industry median NPM is winsorized at the upper and lower 5% tails, while industry sales, industry PPE and HHI are winsorize at both upper and lower 1% tails. As we can see, both industry sales and industry PPE are skewed to the right. Hence I take the natural logarithm of both variables and use them as the continuous measure of industry competition in regression analyses. Table 3.2 reports the pairwise Pearson correlation coefficients among the three competition measures and two industry concentration measures – the HHI and four-firm concentration ratio. The correlation coefficients between industry sales and industry PPE and between industry sales and four-firm concentration ratio are similar to those reported in Karuna (2007) for his sample of Compustat firms between 1992 and 2003. However, the correlation between industry PPE and four-firm concentration ratio has opposite sign to that in Karuna (2007). Also, the signs of the correlation coefficients between my proxy for product substitutability and other variables in the table are opposite to those reported in Karuna (2007). However, I find that signs of these correlations in my table are the same as those reported in Li (2010) for her sample of Compustat firms for the sample period 1977-2007. Unlike us, Li (2010) actually uses the same proxy for product substitutability as Karuna (2007). This fact suggests that the difference between the correlation between variables in my data and that in Karuna (2007) is not purely driven by my use of a different proxy for product substitutability than Karuna (2007).

4. Empirical Results

4.1. Main results

According to Raith (2003), managerial incentives are jointly determined by the three fundamental factors in his model. Hence, examining any one of the factors without controlling for

Table 3.1: Descriptive statistics

The sample consists of all firms in the CEO turnover database between 1992-2007 that also have data available on Compustat segment database and industrial annual database as well as CRSP. Firms whose sum of segment sales differs from firm level sales by more than 10% are excluded to avoid using erroneous segment data. Firms in financial and utility industries are also excluded because product market competition is restricted in these industries. Lastly, Four-digit SIC industries with less than four firms are excluded. All variables are defined in Appendix B.The variables CEO voting power, CEO-Chairman, Nonemployee blockholders on board and Dual Class firm are constructed using data on RiskMetics database which starts in 1996. Hence the summary statistics for these four variables are for the sample period 1996-2007.

Variables	Mean	Median	Q1	Q3	SD
Industry median NPM	0.12	0.10	0.06	0.16	0.10
Industry sales ($ mil)	119929.10	36709.41	11813.04	121716.10	215651.60
Industry average PPE ($ mil)	6789.35	2098.38	678.28	7536.54	12785.22
Industry HHI	0.19	0.14	0.07	0.24	0.15
Four-firm concentration ratio	0.63	0.63	0.43	0.82	0.23
Total assets ($ mil)	11575.09	1454.50	484.86	5255.21	60513.82
Sales ($ mil)	4665.03	1207.57	454.90	3649.40	13686.90
Tobin's Q	2.06	1.52	1.17	2.24	2.13
Stock volatility	0.109	0.092	0.065	0.134	0.064
Market-adjusted stock return	0.050	-0.014	-0.233	0.234	0.480
Industry-adjusted change in EBIT	0.0069	0.0020	-0.0155	0.0258	0.0646
CEO tenure	7.8	5.5	2.6	10.6	7.5
CEO age	55.6	56.0	51.0	60.0	7.5
Founder-heir CEO	0.15	0.00	0.00	0.00	0.36
CEO voting power	3.79	0.00	0.00	2.70	9.89
CEO-Chairman	0.65	1.00	0.00	1.00	0.48
Nonemployee blockholders on board	0.27	0.00	0.00	1.00	0.44
Dual class firm	0.09	0.00	0.00	0.00	0.28

Table 3.2: Correlations matrix

This table reports the pairwise Pearson correlation coefficients among the three competition measures and the two industry concentration measures in the full sample. All variables are defined in Appendix B. All correlation coefficients are statistically significant at the 1% level.

	Industry median NPM	Industry sale	Industry average PPE	HHI
Industry sales	0.3048			
Industry average PPE	0.2215	0.6123		
HHI	-0.1451	-0.3408	-0.1373	
Four-firm concentration ratio	-0.2473	-0.4717	-0.2227	0.8456

the other two factors may give misleading results due to correlations between these factors. Therefore, in all regressions, I include all three competition variables simultaneously to better isolate the individual effect of any one of them conditional on the other two. As I have discussed, I also control for industry HHI so that, conditional on HHI, differences in market size capture cross-industry differences in average firm-level output for each industry as a whole.

In Table 3.3, I examine how the likelihood of forced CEO turnover is affected by the three competition variables by estimating logit regressions in which the dependent variable is an indicator for forced CEO turnover and the three competition variables are the key explanatory variables. These logit regressions are estimated using pooled cross section time series data so the competition effects are identified from both time series and cross-sectional variations in the dependent and independent variables. Year fixed effects are included to control for changes in rate of CEO turnover over time that are unrelated to changes in industry competition. Column 1 of Table 3.3 reports the results for logit regressions that only include industry level variables. Since my proxy for product substitutability is likely to be positively correlated with industry performance, to isolate the effect that is due to competition rather than industry performance, I control for industry stock performance by industry median stock return adjusted by return on CRSP value-weighted index and control for industry accounting performance by industry median change in ROA. As expected, coefficients on both industry median stock and accounting performance are negative and highly statistically significant, suggesting that CEOs in underperforming industries are more likely to be forced out than CEOs in other industries. This is consistent with findings in Jenter and Kanaan (2010) and Kaplan and Minton (2011) who find that industry underperformance increases the likelihood of forced CEO turnover in that industry. Interestingly, I find that the coefficient on industry median NPM is negative and statistically significant even after I have controlled for industry performance. Since higher values of NPM suggests low price elasticity of demand and thus less product substitutability, the negative sign before industry median NPM indicates that CEOs are less likely to be fired in industries with less price competition and less substitutable products than in other industries, which provides support to hypothesis H1a. Besides NPM, the coefficient on market size is also statistically significant. The positive sign is consistent with my hypothesis that the likelihood of forced CEO turnover is higher in industries with larger demand because the return to cost reduction efforts is likely to be higher in such industries than in others. The coefficient on log industry PPE is marginally significant with a p-value of 0.107. The negative sign of log industry PPE suggests that CEOs in industries with higher entry costs are less likely to be forced out, which is inconsistent with hypothesis H3a. According to Raith (2003), high entry costs deter entries of new firms and thus in equilibrium each firm already in the industry produces more and provide more powerful incentives. However, if we think of competition from the point of view of threat of liquidation, the negative sign of log industry PPE is intuitively consistent with the prediction in Schmidt (1997) that it becomes cheaper to implement higher level of incentives when threat of liquidation is higher. Intuitively, when entry cost is low, competition from new entrants tends to increase the risk of liquidation of existing firms; on the other hand, when entry cost is high, existing firms earn more rents and face a smaller risk of liquidation. One explanation of the negative sign on log industry PPE is that, besides the return to cost reduction efforts effect that is modeled by Raith (2003), which suggests a positive sign, the relation between product market competition and managerial incentives is also influenced by a threat of liquidation effect (See Schmidt,1997), which suggests a positive sign, and the latter effect dominates the former.

I also notice that the coefficient on HHI is positive and highly significant. This appears to be counter intuitive because industries with high HHI indices are often considered to be concentrated and thus less competitive in prior studies. However, if market structure is endogenous, this is not as surprising. As shown by Raith (2003), depending on what causes change in competition, with free entry and exit, industries with higher concentration can be more competitive than industries with low concentration. One example is the U.S. airline industry which is highly concentrated and competitive. Actually, the positive coefficient on industry HHI appears to be consistent with the key intuition of the Raith (2003) model that higher firm-level output is associated with stronger managerial incentives. Conditional on the level of industry sales, we expect that industry HHI to be positively related to average firm-level output in the industry. To confirm this claim, in unreported results, I estimate an OLS regression of log of firm-level sales on industry sales and industry HHI using all firms in my sample. I find that both coefficients are indeed positive and highly significant. Ali et al. (2009) argue that industry HHI calculated with Compustat data, which contains only public firms, is negatively related to industry sales growth rate and hence declining industries tend to have higher industry HHI. Since I control for industry performance in my regressions, it seems less likely that the positive sign simply reflects higher rate of CEO turnover in declining industries.

In column 2, I estimate similar logit regressions but use firm level rather than industry level control variables. The firm level data allow me to control for a variety of firm and governance characteristics that may affect forced CEO turnovers. I control for firm stock performance by market-adjusted stock returns and firm accounting performance by industry-median-adjusted change in ROA where industry is defined by Fama and French 48 industry classification[29]. Both measures are widely used in prior CEO turnover studies. I also control for firm size measured by log of total assets, firm risk measured by the firm's monthly stock return volatility in the past 12 months and growth opportunities by Tobin's Q. For CEO characteristics, I include CEO tenure measured as log of one plus CEO tenure in years, CEO age and an indicator for whether the CEO is a founder or an heir of founders. Compared with column 1, the signs of the coefficients on the three competition variables and HHI remain the same but the coefficient on industry median NPM is no longer statistically significant; however, the coefficient on industry PPE becomes more significant. In column 3, I control for more corporate governance variables using data from RiskMetrics. They include CEO voting power, an indicator for CEO Chairman duality, an indicator for dual class firms, and an indicator for the presence of

[29] The results are qualitatively the same when I use 4-digit SIC industry medians.

Table 3.3: The effect of competition on rate of forced CEO turnover (1992-2007)

This table reports result of logit regressions where the dependent variable is a forced CEO turnover indicator for year t. All independent variables are measured in year t-1 and are defined in Appendix B. The sample consists of all firms in the final sample. In column 3, firms are also required to have data available on RiskMetrics database. This requirement restricts the sample period to 1996-2007. Year fixed effects are included in all columns. Standard errors are clustered by four-digit SIC codes. The numbers in parentheses are robust p-values and ***, **, and * denote significance at the 1, 5 and 10 percent levels, respectively.

VARIABLES	(1)	(2)	(3)
Industry median NPM	-1.118**	-0.141	-0.745
	(0.031)	(0.831)	(0.365)
Log of market size	0.240***	0.210***	0.228***
	(0.009)	(0.001)	(0.004)
Log of industry PPE	-0.129	-0.126**	-0.129**
	(0.107)	(0.020)	(0.048)
Industry HHI	1.307***	1.236***	1.491***
	(0.008)	(0.002)	(0.005)
Market-adjusted return		-1.500***	-1.849***
		(0.000)	(0.000)
Industry-adjusted change in ROA		-2.430***	-3.609***
		(0.000)	(0.005)
Log of total assets		-0.038	0.062
		(0.330)	(0.213)
Stock volatility		4.183***	3.574***
		(0.000)	(0.001)
Tobin's Q		-0.121*	-0.146*
		(0.051)	(0.056)
Log (1 + CEO tenure)		-0.198***	-0.014
		(0.001)	(0.868)
Founder-heir CEO		-1.574***	-1.262***
		(0.000)	(0.000)
CEO Age		-0.025***	-0.031***
		(0.000)	(0.001)
Market-adjusted industry median return	-0.961***		
	(0.000)		
Industry median change in ROA	-2.239**		
	(0.016)		
CEO voting power			-0.038*
			(0.095)
Dual class firms			0.122
			(0.595)
CEO-Chairman			-0.418***
			(0.001)
Nonemployee blockholders on board			0.201
			(0.164)
Constant	-5.095***	-4.135***	-3.746***
	(0.000)	(0.000)	(0.000)
N	12002	15289	9019
Chisq	102.9	458.3	362.7
Pseudo R-squared	0.0218	0.118	0.131

nonemployee blockholders on board. Since board data on RiskMetrics starts in 1996, adding these extra control variables reduces the sample size and limits the sample period to 1996 to 2007. However, as we can see from column 3, the signs and statistical significance of the coefficients on the three competition variables and HHI are very similar to those in column 2. The sign and statistical significance of control variables are largely consistent with results in prior CEO turnover studies and with economic intuition.

In Table 3.4, I examine how different dimensions of industry competition affect the sensitivity of forced CEO turnover to firm performance. Whether industries showing higher rate of forced CEO turnover also show higher sensitivity of forced CEO turnover to firm performance depends the equilibrium between CEO effort and sensitivity of forced CEO turnover to firm performance. In general, we expect higher turnover-performance sensitivity to be associated with higher rate of forced CEO turnover. However, knowing that the turnover-performance sensitivity is higher, a CEO is likely to exert higher effort. So, in equilibrium, the rate of forced CEO turnover does not necessarily have to be higher in industries with higher turnover-performance sensitivity. Hence, if both the rate of forced CEO turnover and sensitivity of forced CEO turnover to firm performance are high (or low), then the evidence is consistent with higher (or lower) incentives being provided to the CEOs in these firms. However, if rate of forced CEO turnover is low but turnover-performance sensitivity is high, the result can have alternative interpretations. For example, if the effort cost in the industry is the same as that in other industries, then the evidence is still consistent with higher incentives being provided to CEOs. However, if the effort cost is very low in the industry, then higher turnover-performance sensitivity in equilibrium may still induce less effort in the industry than in other industries, in which case, the evidence could still be consistent with lower incentives being provided to the CEOs.

To examine the sensitivity of forced CEO turnover to firm performance, I add to the logit models in Table 3.3 four interaction terms consisting of the interactions of the three competition variables with firm performance as well as that of industry HHI with firm performance. In columns 1 and 2, firm performance is measured by market-adjusted stock return, while in columns 3 and 4 by industry-adjusted change in ROA. Columns 1 and 3 are estimated using data from 1992 to 2007. Columns 2 and 4 are estimated using data from 1996 to 2007 and I include more corporate governance variables in the regressions. I find that the coefficient on industry median NPM is negative and highly significant in all columns. The negatives sign is consistent with our hypothesis H1a which says that CEOs are more likely to be fired in industries with greater product substitutability than in other industries *ceteris paribus*. However, the coefficient on the interaction of industry median NPM and firm performance is negative and statistically significant, suggesting that forced CEO turnover is more sensitive to firm performance in industries with higher industry NPMs, i.e. lower product substitutability, which is inconsistent with hypothesis H1b. One explanation of this seemingly inconsistency is that, CEOs in industries with lower product substitutability and thus higher net profit margin can considerably reduce their chance of being fired by exert somewhat higher effort and thus it may be optimal for these firms to have higher turnover-performance sensitivity so some minimum level of incentive is provided to the CEOs. Hence, this result offers mixed evidence but does not completely reject the hypothesis that firms in industries with lower product substitutability offers lower incentives to CEOs in equilibrium than firms in industries with higher product substitutability. Alternatively, comparing with firms in industries with lower median NPMs, firms in industries with higher median NPMs in general have relatively stable profits. As a result, the same drop in firm performance may signal greater managerial problems in firms in industries with higher NPM than in firms in industries with lower NPM.

As for the other two competition factors, the coefficients on their interactions with firm performance are statistically significant in three out of the four columns. The negative coefficient on the interaction of market size with firm performance suggests that forced CEO turnovers are more sensitive to firm performance in industries with larger market size for any given level of industry concentration. In contrast, the positive coefficient on the interaction between industry

Table 3.4: The effect of competition on the sensitivity of forced CEO turnover to firm performance (1992-2007)

This table reports result of logit regressions where the dependent variable is a forced CEO turnover indicator for year t. All independent variables are measured in year t-1 and are defined in Appendix B. The sample consists of all firms in the final sample. In columns 2 and 4, firms are also required to have data available on RiskMetrics database. This requirement restricts the sample period to 1996-2007. Firm performance is measured by market-adjusted stock return in columns 1 and 2, while by industry-adjusted change in ROA in columns 3 and 4. Year fixed effects are included in all columns. Standard errors are clustered by four-digit SIC codes. The numbers in parentheses are robust p-values and ***, **, and * denote significance at the 1, 5 and 10 percent levels, respectively.

VARIABLES	(1) Market-adjusted stock return	(2) Market-adjusted stock return	(3) Change in ROA	(4) Change in ROA
Industry median NPM	-1.756***	-2.334***	-1.275**	-2.283***
	(0.003)	(0.007)	(0.037)	(0.001)
Log of market size	0.063	0.089	0.182**	0.176**
	(0.469)	(0.540)	(0.010)	(0.031)
Log of weighted average PPE	0.046	0.036	-0.098*	-0.073
	(0.532)	(0.784)	(0.099)	(0.293)
HHI	0.596	0.882	0.732	0.920
	(0.265)	(0.240)	(0.106)	(0.139)
Firm performance	0.037	-0.652	5.538	10.118
	(0.978)	(0.773)	(0.333)	(0.284)
NPM × Firm performance	-6.035***	-5.622**	-24.587***	-42.947***
	(0.000)	(0.029)	(0.000)	(0.000)
Market Size × Firm performance	-0.549***	-0.514	-1.759***	-2.870***
	(0.006)	(0.204)	(0.008)	(0.004)
Log PPE × Firm performance	0.637***	0.620	1.697***	2.636***
	(0.001)	(0.101)	(0.008)	(0.008)
HHI × Firm performance	-2.598**	-2.376	-20.673***	-26.001***
	(0.028)	(0.196)	(0.000)	(0.001)
Log of total assets	-0.043	0.051	-0.047	0.071
	(0.262)	(0.307)	(0.232)	(0.158)
Stock volatility	3.885***	3.453***	5.032***	4.943***
	(0.000)	(0.001)	(0.000)	(0.000)
Tobin's Q	-0.156***	-0.181**	-0.286***	-0.327***
	(0.010)	(0.014)	(0.000)	(0.000)
Log (1 + CEO tenure)	-0.197***	-0.011	-0.174***	0.007
	(0.001)	(0.897)	(0.004)	(0.939)
Founder-heir CEO	-1.578***	-1.258***	-1.588***	-1.266***
	(0.000)	(0.000)	(0.000)	(0.000)
CEO Age	-0.025***	-0.030***	-0.027***	-0.031***
	(0.000)	(0.001)	(0.000)	(0.001)
CEO voting power		-0.039*		-0.037
		(0.090)		(0.102)
Dual class firm		0.090		0.157
		(0.688)		(0.495)
CEO-Chairman		-0.436***		-0.434***
		(0.001)		(0.001)
Nonemployee blockholders on board		0.203		0.212
		(0.154)		(0.141)

Table 3.4, continued

Constant	-3.522***	-3.151***	-3.462***	-3.096***
	(0.001)	(0.003)	(0.001)	(0.000)
Observations	15399	9089	15349	9025
Chisq	567.9	393.3	473.9	347.4
Pseudo R-squared	0.120	0.130	0.0955	0.106

average PPE and firm performance suggests that forced CEO turnovers are less sensitive to firm performance in industries with higher entry costs than in other industries *ceteris paribus*. Again, this result on entry costs is consistent with entry costs being positively related to threat of liquidation but is inconsistent with prediction of the Raith (2003) model as expressed in H3b. Lastly, the negative coefficient on the interaction of HHI and firm performance appear to be consistent with the intuition of Raith (2003) model in the sense that conditional on a given level of industry sales, higher concentration tends to be associated with higher average firm-level output in the industry and thus stronger incentives according to Raith (2003). But this interpretation is only suggestive as other alternative explanations are possible.

4.2. Competitive vs. oligopoly Industries

Raith (2003) models competition in an oligopolistic industry where strategic reactions of other firms in the same industry play an important role in setting managerial incentives. However, not all industries are oligopolistic. Hence, the model predictions may not apply to industries where a large number of firms compete in the same product market to the point that strategic reactions of competitors play little role in any firm’s decisions. To see if the three dimensions of competition I study are only important in oligopolistic industries, or in other words, my previous results are mostly driven by firms in oligopolistic industries, I divide the sample firms into two groups – those in oligopoly industries and those in competitive industries. The difference between the two groups is only in the degree of importance of strategic interactions in firm decisions and not in with or without strategic interactions. One concentration measure that is often used to quantitatively describe oligopoly is the four-firm concentration ratio which is calculated as the market share of the four largest firms in an industry in percentage. Usually, when this ratio is in the range of 50% to 80%, an industry is likely to be an oligopoly. The range from 80% to 100% includes oligopoly industry all the way to monopoly industry. In my sample, the four-firm concentration ratio ranges from 24% to close to 100% with a median of 67%. I use the median four-firm concentration ratio to separate the sample into two subsamples. I choose to use median instead of a value of 50% as the cut-off to divide sample firms because I want to make sure that the two subsamples are about the same size so that any differences in the significance of the coefficient estimates are unlikely to be purely driven by differences in sample size. The subsample with the ratio above the sample median is likely to contain mostly firms in oligopolistic industries, while the subsample with the ratio below the sample median contains mostly firms in competitive industries.

Table 3.5 reports the results for the two subsamples separately. Columns 1 and 2 are for the competitive industries subsample while columns 3 and 4 are for the oligopoly industries subsample. As in Table 3.3, the coefficient of industry median NPM is mostly statistically insignificant except in one specification in the competitive sample. Thus I focus on comparing the coefficients on market size and industry average PPE across the two subsamples. I find that for firms in the competitive subsample, the coefficients on both market size and industry average PPE are statistically insignificant. In contrast, for firms in the oligopoly subsample, both coefficients are statistically significant at the 1% level in column 3. In column 4, when I include more control variables for corporate governance and the sample period is reduced to 1996 to 2007, the coefficient on market size remains statistically significant at the 1% level, while the coefficient on industry average PPE is statistically significant at the 10% level in one-sided test.

In Table 3.6, I further examine the effects of competition on the sensitivity of forced CEO turnover to firm performance in the two subsamples. In columns 1 and 2, firm performance

Table 3.5: The effect of competition on rate of forced CEO turnover - competitive vs. oligopoly industries (1992-2007)

This table reports result of logit regressions where the dependent variable is a forced CEO turnover indicator for year t. All independent variables are measured in year t-1 and are defined in Appendix B. In columns 2 and 4, firms are also required to have data available on RiskMetrics database. This requirement restricts the sample period to 1996-2007. The results are reported separately for two subsamples of industries divided by the median four-firm concentration ratio in our sample. The competitive subsample contains firms in industries with four-firm concentration ratio below the sample median, while the oligopoly subsample contains firms in industries with four-firm concentration ratio above the median. The subsample used is indicated at the top of each column. Year fixed effects are included in all columns. Standard errors are clustered by four-digit SIC codes. The numbers in parentheses are robust p-values and ***, **, and * denote significance at the 1, 5 and 10 percent levels, respectively.

	Competitive		Oligopoly	
VARIABLES	(1)	(2)	(3)	(4)
Industry median NPM	-0.646	-1.964**	1.228	2.270
	(0.461)	(0.040)	(0.323)	(0.211)
Log of market size	0.093	0.163	0.327***	0.314***
	(0.370)	(0.187)	(0.000)	(0.005)
Log of weighted average PPE	-0.045	-0.130	-0.232***	-0.138
	(0.598)	(0.123)	(0.002)	(0.172)
HHI	0.767	0.326	1.447***	1.500**
	(0.671)	(0.903)	(0.005)	(0.039)
Market-adjusted return	-1.226***	-1.261***	-1.825***	-2.561***
	(0.000)	(0.002)	(0.000)	(0.000)
Industry-adjusted change in ROA	-1.867***	-2.129	-3.635***	-5.723***
	(0.007)	(0.151)	(0.010)	(0.005)
Log of total assets	-0.114**	0.097	0.048	0.023
	(0.033)	(0.146)	(0.425)	(0.758)
Stock volatility	3.773***	4.100***	4.728***	3.045*
	(0.000)	(0.001)	(0.000)	(0.058)
Tobin's Q	-0.130*	-0.218**	-0.133	-0.098
	(0.074)	(0.037)	(0.207)	(0.367)
Log (1 + CEO tenure)	-0.112	0.178	-0.286***	-0.160
	(0.198)	(0.177)	(0.001)	(0.186)
Founder-heir CEO	-1.607***	-1.252***	-1.557***	-1.371***
	(0.000)	(0.000)	(0.000)	(0.001)
CEO Age	-0.023***	-0.032**	-0.029***	-0.030**
	(0.003)	(0.011)	(0.001)	(0.029)
CEO voting power		-0.017		-0.089**
		(0.463)		(0.012)
Dual class firms		-0.129		0.263
		(0.703)		(0.332)
CEO-Chairman		-0.417**		-0.434**
		(0.024)		(0.020)
Nonemployee blockholders on board		0.212		0.218
		(0.344)		(0.260)
Constant	-2.910**	-3.281**	-5.113***	-4.304***
	(0.044)	(0.024)	(0.000)	(0.000)
N	7683	4307	7606	4712
Chisq	456.2	248.7	277.6	283.5
Pseudo R-squared	0.111	0.104	0.137	0.180

Table 3.6: The effect of competition on the sensitivity of forced CEO turnover to firm performance - competitive vs. oligopolistic industries (1992-2007)

This table reports result of logit regressions where the dependent variable is a forced CEO turnover indicator for year t. All independent variables are measured in year t-1 and are defined in Appendix B. The results are reported separately for two subsamples of industries divided by the median four-firm concentration ratio in the sample. The competitive subsample contains firms in industries with four-firm concentration ratio below the sample median, while the oligopoly subsample contains firms in industries with four-firm concentration ratio above the median. The subsample used is indicated at the top of each column. Firm performance is measured by market-adjusted stock return in columns 1 and 2, while by industry-adjusted change in ROA in columns 3 and 4. Year fixed effects are included in all columns. Standard errors are clustered by four-digit SIC codes. The numbers in parentheses are robust p-values and ***, **, and * denote significance at the 1, 5 and 10 percent levels, respectively.

	Competitive	Oligopoly	Competitive	Oligopoly
VARIABLES	Stock return	Stock return	Change in ROA	Change in ROA
	(1)	(2)	(3)	(4)
Industry median NPM	-1.751**	-1.343	-1.621**	-0.475
	(0.029)	(0.270)	(0.040)	(0.699)
Log of market size	-0.027	0.123	0.075	0.247**
	(0.850)	(0.304)	(0.525)	(0.016)
Log of weighted average PPE	0.054	0.052	-0.029	-0.154*
	(0.628)	(0.626)	(0.763)	(0.072)
HHI	0.664	0.811	0.028	0.890
	(0.778)	(0.194)	(0.989)	(0.127)
Firm performance	1.077	-0.717	8.359	10.988
	(0.671)	(0.698)	(0.374)	(0.264)
NPM × Firm performance	-4.179***	-9.146***	-18.779***	-42.822***
	(0.005)	(0.000)	(0.006)	(0.001)
Market Size × Firm performance	-0.475	-0.822***	-1.636	-3.190**
	(0.123)	(0.003)	(0.173)	(0.020)
Log PPE × Firm performance	0.376	1.083***	1.333	3.092**
	(0.124)	(0.000)	(0.197)	(0.015)
HHI × Firm performance	0.411	-2.548	-37.421*	-23.632***
	(0.952)	(0.102)	(0.094)	(0.003)
Log of total assets	-0.116**	0.040	-0.131**	0.044
	(0.030)	(0.499)	(0.019)	(0.447)
Stock volatility	3.438***	4.185***	4.429***	5.875***
	(0.000)	(0.002)	(0.000)	(0.000)
Tobin’s Q	-0.163**	-0.165	-0.280***	-0.310***
	(0.024)	(0.111)	(0.000)	(0.009)
Log (1 + CEO tenure)	-0.118	-0.276***	-0.095	-0.256***
	(0.181)	(0.001)	(0.270)	(0.002)
Founder-heir CEO	-1.596***	-1.608***	-1.613***	-1.584***
	(0.000)	(0.000)	(0.000)	(0.000)
CEO Age	-0.023***	-0.029***	-0.024***	-0.032***
	(0.002)	(0.001)	(0.001)	(0.000)
Constant	-2.146	-4.640***	-2.249	-4.212***
	(0.175)	(0.001)	(0.120)	(0.003)
Observations	7709	7690	7722	7627
Chisq	699.9	381.2	429.6	284.2
Pseudo R-squared	0.112	0.142	0.0909	0.109

is measured by market-adjusted stock return, while in columns 3 and 4 by industry-adjusted change in ROA. Columns 1 and 3 are for firms in the competitive subsample, while columns 2 and 4 are for firms in the oligopoly subsample. I find that coefficients on interactions of market size and industry average PPE with firm performance are statistically significant at the 1% level in the oligopoly subsample but are statistically insignificant in the competitive subsample. The signs of these coefficients are the same as those in Table 3.4. This is consistent with my hypothesis that these two dimensions of competition are only important when strategic interactions between firms are important. However, the coefficients on the interaction of industry median NPM with firm performance are statistically significant at the 1% level in both subsamples, suggesting that its importance does not depend on whether strategic interactions between firms play an important role in firm decisions. The sign of the interaction of industry median NPM with firm performance remains negative as in Table 3.4.

Overall, results in Table 3.5 and Table 3.6 show that market size and entry costs seem to affect forced CEO turnover decisions mainly in oligopolistic industries. This is consistent with the Raith (2003) model in the sense that these two factors are important only when strategic interactions between firms are important to firm decisions. However, industry median NPM seems to affect forced CEO turnover decisions in both competitive and oligopoly industries.

5. Robustness Checks

Until now, I have used competition measures constructed from annual data. Given the volatility in some of the variables I use, changes in these variables from year to year may overstate the actual change in fundamentals of competition. For example, change in industry median NPM may just reflect change in macroeconomic environment rather than change in product substitutability. Similarly, annual changes in sales in most cases simply reflect the volatile nature of sales rather than change in expected demands. As a robustness check, in Tables

Table 3.7: Robustness check on effect on rate of forced CEO turnover using smoothed competition measures (1992-2007)

This table reports result of logit regressions where the dependent variable is a forced CEO turnover indicator for year t. All independent variables are measured in year t-1 and are defined in Appendix B. The sample consists of all firms in the final sample. In column 3, firms are also required to have data available on RiskMetrics database. This requirement restricts the sample period to 1996-2007.Year fixed effects are included in all columns. Standard errors are clustered by four-digit SIC codes. The numbers in parentheses are robust p-values and ***, **, and * denote significance at the 1, 5 and 10 percent levels, respectively.

VARIABLES	(1)	(2)	(3)
Industry median NPM	-1.009	0.018	-0.363
	(0.128)	(0.982)	(0.743)
Log of market size	0.224**	0.205***	0.254***
	(0.018)	(0.003)	(0.003)
Log of weighted average PPE	-0.130	-0.117*	-0.142**
	(0.148)	(0.057)	(0.046)
HHI	1.016*	1.004**	1.624**
	(0.086)	(0.032)	(0.010)
Market-adjusted return		-1.509***	-1.862***
		(0.000)	(0.000)
Industry-adjusted change in ROA		-2.405***	-3.622***
		(0.000)	(0.004)
Log of total assets		-0.040	0.054
		(0.313)	(0.273)
Stock volatility		4.261***	3.830***
		(0.000)	(0.000)
Tobin's Q		-0.115*	-0.134*
		(0.059)	(0.075)
Log (1 + CEO tenure)		-0.199***	-0.028
		(0.001)	(0.748)
Founder-heir CEO		-1.608***	-1.259***
		(0.000)	(0.000)
CEO Age		-0.026***	-0.033***
		(0.000)	(0.001)
Market-adjusted industry median return	-0.987***		
	(0.000)		
Industry median change in EBIT	-2.604***		
	(0.007)		
CEO voting power			-0.037
			(0.100)
Dual class firms			0.144
			(0.536)
CEO-Chairman			-0.384***
			(0.004)
Nonemployee blockholders on board			0.203
			(0.162)
Constant	-4.863***	-4.047***	-3.884***
	(0.000)	(0.000)	(0.000)
N	11933	15114	8911
Chisq	96.00	475.6	359.9
Pseudo R-squared	0.0210	0.119	0.132

Table 3.8: Robustness check on effect on sensitivity of forced CEO turnover to firm performance using smoothed competition measures (1992-2007)

This table reports result of logit regressions where the dependent variable is a forced CEO turnover indicator for year t. All independent variables are measured in year t-1 and are defined in Appendix B. The sample consists of all firms in the final sample. In columns 2 and 4, firms are also required to have data available on RiskMetrics database. This requirement restricts the sample period to 1996-2007. Firm performance is measured by market-adjusted stock return in columns 1 and 2, while by industry-adjusted change in ROA in columns 3 and 4. Year fixed effects are included in all columns. Standard errors are clustered by four-digit SIC codes. The numbers in parentheses are robust p-values and ***, **, and * denote significance at the 1, 5 and 10 percent levels, respectively.

VARIABLES	(1) Market-adjusted stock return	(2) Market-adjusted stock return	(3) Change in ROA	(4) Change in ROA
Industry median NPM	-1.719**	-1.889	-1.064	-1.940*
	(0.025)	(0.131)	(0.207)	(0.054)
Log of market size	0.067	0.083	0.181**	0.200**
	(0.489)	(0.608)	(0.020)	(0.039)
Log of weighted average PPE	0.044	0.031	-0.096	-0.084
	(0.595)	(0.831)	(0.175)	(0.305)
HHI	0.252	0.632	0.460	1.002
	(0.700)	(0.505)	(0.389)	(0.183)
Firm performance	0.105	0.775	7.413	6.890
	(0.942)	(0.761)	(0.210)	(0.495)
NPM × Firm performance	-6.859***	-5.222*	-19.347***	-28.501**
	(0.000)	(0.085)	(0.009)	(0.012)
Market Size × Firm performance	-0.499**	-0.635	-1.678**	-2.009*
	(0.019)	(0.159)	(0.021)	(0.060)
Log PPE × Firm performance	0.577***	0.634	1.392**	1.844**
	(0.003)	(0.121)	(0.032)	(0.048)
HHI × Firm performance	-2.934**	-3.896*	-24.627***	-26.210***
	(0.040)	(0.082)	(0.000)	(0.005)
Log of total assets	-0.046	0.044	-0.053	0.057
	(0.242)	(0.377)	(0.177)	(0.259)
Stock volatility	3.916***	3.663***	5.083***	5.199***
	(0.000)	(0.000)	(0.000)	(0.000)
Tobin’s Q	-0.150**	-0.167**	-0.278***	-0.312***
	(0.012)	(0.024)	(0.000)	(0.000)
Log (1 + CEO tenure)	-0.199***	-0.024	-0.176***	-0.009
	(0.001)	(0.781)	(0.004)	(0.918)
Founder-heir CEO	-1.612***	-1.255***	-1.636***	-1.282***
	(0.000)	(0.000)	(0.000)	(0.000)
CEO Age	-0.027***	-0.032***	-0.029***	-0.034***
	(0.000)	(0.001)	(0.000)	(0.000)
CEO voting power		-0.039*		-0.036
		(0.091)		(0.104)
Dual class firms		0.109		0.185
		(0.633)		(0.427)
CEO-Chairman		-0.406***		-0.385***
		(0.002)		(0.004)
Nonemployee blockholders on board		0.213		0.219
		(0.138)		(0.132)

Table 3.8, continued

Constant	-3.370***	-2.949***	-3.335***	-3.127***
	(0.002)	(0.007)	(0.001)	(0.001)
Observations	15222	8981	15174	8917
Chisq	589.8	397.3	477.2	372.7
Pseudo R-squared	0.121	0.130	0.0951	0.102

3.7 and 3.8, I reexamine the main results in Tables 3.3 and 3.4 using smoothed competition measures respectively. Specifically, for each year, I calculate the five-year moving average of each competition measures as well as industry HHI. I require that data be available for at least three out of the past five years to be included in the sample. I find that the results are in general very similar to those in Tables 3.3 and 3.4. Except for coefficient on industry median NPM in column 1 which become statistically insignificant, the coefficient on market size and industry PPE remain statistically significant in both columns 2 and 3.

6. Conclusion

In this paper, I empirically examine the relation between product market competition and forced CEO turnover using a multi-dimensional set of competition measures motivated by recent theory by Raith (2003). Consistent with the prediction of Raith (2003), I find that market size is positively related to the likelihood of forced CEO turnover and sensitivity of forced CEO turnover to performance. Contrary to the prediction of Raith (2003), I find that entry costs are negatively related to the likelihood of forced CEO turnover as well as the sensitivity of forced CEO turnover to performance. However, this evidence is consistent with the prediction in Schmidt (1997) which says that threat of liquidatiaon lowers the cost for the owner of the firm to induce higher level of effort by the managers. It seems that the relation between entry cost and managerial incentives is driven both by the threat of liquidation effect in Schmidt (1997) and the return to cost reduction effort effect in Raith (2003) with the former dominating the latter[30]. The evidence on product substitutability is mixed. While product substitutability is positively associated with the probability of forced CEO turnover, which is consistent with the prediction based on Raith (2003); it is negatively associated with the sensitivity of forced CEO turnover to firm performance, which does not seem to be consistent with the prediction based on Raith (2003) but it does not reject the prediction either.

I further show that, consistent with the oligopoly setting of Raith (2003) model, the results are mainly driven by the subsample of firms in oligopoly industries. Overall, the results suggest that the Raith (2003) model seems to capture some important feature of competition but the mechanism through which competition affects managerial incentives proposed in the model is not fully supported by our CEO turnover data. Besides the return to cost reduction mechanism studied in Raith (2003), the threat of liquidation mechanism studied in Schmidt (1997) also plays important roles in the relation between production market competition and managerial slack. The results also suggest that strategic interactions between firms in oligopoly industry appear to play an important role in forced CEO turnover decisions. This aspect of CEO turnover decision has not been explored in the literature before.

One limitation of this study is that I have used Compustat data to construct the competition measures as well as other control variables[31]. As a result, I only include public firms in the sample. Recent study by Ali, Klasa and Yeung (2009) suggests that leaving out private firms in an industry could be a serious concern in studies of product market competition in general[32]. However, I consider it not a big concern in this study because I have shown that the

---

[30] Schmidt (1997) also models a value of cost reduction effect but in his model this effect only shows up when "the manager is paid a rent in excess of his reservation utility". It is this second effect that makes the relation between competition and managerial slack in his model ambiguous.

[31] The US Census of Manufacturers data do not have all variables that I need to construct the competition measures.

[32] Ali, Klasa and Yeung (2009) show that industry concentration measures calculated with Compustat data are poor proxies for actual industry concentration and thus results in some prior studies that used Compustat-based industry concentration to define competition are not robust when Census-based concentration measures are used.

results are mainly driven by the sample of firms in oligopoly industries where the major players are usually public firms. For example, the U.S. airline industry and automobile industry are all oligopoly industries where the major players are public. In these industries, public firms mainly compete with other public firms in the same industry, thus leaving out private firms should not bias the results; on the contrary, including private firms may actually bias the results because these small firms are less likely to be direct competitors to the oligopoly firms in their industry.

Appendix B: Definitions of variables

Variables	Definition
Dependent variables	
Forced CEO turnover	An indicator variable for forced CEO turnover classified using the method in (Parrino, 1997).
Industry competition measures	
Industry median NPM	The median net profit margin of all segments in a four-digit SIC industry where profit margin is calculated at the segment level as operating income before depreciation and amortization divided by sales.
Industry sales	Sum of sales of all segments in a four-digit SIC industry.
Industry average PPE	Weighted-average costs of gross property, plant and equipment of all segments in a four-digit SIC industry where the weight is each segment's sales share in the industry.
Industry concentration measures	
HHI	Herfindahl-Hirschman Index calculated from sales shares of all segments in a four-digit SIC industry.
Four-firm concentration ratio	Sales share accounted for by the largest four segments in a four-digit SIC industry.
Firm characteristics	
Log of total assets	Natural log of book value of total assets in millions of dollars
Log of sales	Natural log of annual sales in millions of dollars
Tobin's Q	Book value of total assets minus book value of common equity plus the market value of common equity over book value of total assets.
Stock volatility	Standard deviation of monthly stock returns in the past 12 months.
Firm Performance	
Market-adjusted return	The firm's stock return minus return on value-weighted CRSP index cumulated over the 12 months that end one month before the CEO turnover announcement or over the fiscal year when there is no CEO turnover. This variable is winsoried at 1% and 99% of the distribution.
Industry-adjusted Change in ROA	The firm's change in ROA adjusted by the median of this ratio for the firm's industry over the fiscal year prior to the CEO turnover, where ROA is defined as annual EBIT scaled by lagged total assets and industries are defined by Fama and French 48 industry classification. This variable is winsorized at 1% and 99% of the distribution.

Appendix B, continued

Governance characteristics	
CEO-Chairman	An indicator variable that equals to one if the CEO is also the Chairman of the board and equals to zero otherwise.
Dual Class firm	An indicator variable for firms with more than one class of common shares.
Nonemployee block	An indicator variable for the presence of non-employee blockholders on board where a blockholder is defined as anyone holding more than 1% of the total voting power.
CEO characteristics	
CEO Age	CEO age at the time of the CEO turnover announcement.
CEO Voting Power	The percentage of votes held by the CEO as defined by RiskMetrics.
Founder-heir CEO	An indicator variable for CEOs who are the founders or heirs of founders of their firms.
Log CEO Tenure	Natural log of one plus CEO tenure in years.

References

Aggarwal, R., & Samwick, A. (1999). Executive compensation, strategic competition, and relative performance evaluation: theory and evidence. *Journal of Finance , 54*, 1999-2043.

Ali, A., Klasa, S., & Yeung, E. (2009). The limitations of industry concentration measures constructed with compustat data: implications for finance research. *Review of Financial Studies , 22*, 3839-3872.

Barro, J. R., & Barro, R. J. (1990). Pay, performance, and turnover of bank CEOs. *Journal of Labor Economics , 8* (4), 448-481.

Cunat, V., & Guadalupe, M. (2009). Executive compensation and competition in the banking and financial sectors. *Journal of Banking & Finance , 33*, 495-504.

DeFond, M. L., & Park, C. W. (1999). The effect of competition on CEO turnover. *Journal of Accounting and Economics , 27,* 35-36.

Engel, E., Hayes, R. M., & Xue, W. (2003). CEO turnover and properties of accounting information. *Journal of Accounting and Economics , 36*, 197-226.

Fee, C. E., & Hadlock, C. J. (2000). Management turnover and product market competition: empirical evidence from the U.S. newspaper inudstry. *Journal of Business , 73* (2), 205-243.

Giroud, X., & Mueller, H. M. (2010). Does corporate governance matter in competitive industries. *Journal of Financial Economics , 95,* 312-331.

Hart, O. (1983). The market mechanism as an incentive scheme. *Bell Journal of Economics , 14*, 366-382.

Hazarika, S., Karpoff, J. M., & Nahata, R. (2009). Internal corporate governance, CEO turnover, and earnings management. *Working Paper* .

Hermalin, B. E. (1992). The effects of competition on executive behavior. *RAND Journal of Economics , 23*, 518-537.

Hicks, J. (1935). Annual survey of economic theory: the theory of monopoly. *Econometrica , 3*, 1-20.

Holmstrom, B. (1982). Moral hazard in teams. *Bell Journal of Economics , 13*, 324-340.

Hubbard, R. G., & Palia, D. (1995). Executive pay and performance: evidence from the US banking industry. *Journal of Financial Economics , 38*, 105-130.

Huson, M. R., Parrino, R., & Starks, L. T. (2001). Internal monitoring mechanisms and CEO turnover: a long-term perspective. *Journal of Finance , LVI* (6), 2265-2297.

Jensen, M. C., & Meckling, W. H. (1976). Theory of the firm: managerial behavior, agency costs and ownership structure. *Journal of Financial Economics , 3*, 305-360.

Kaplan, S. N., & Minton, B. A. (2006). How has CEO turnover changed? Increasingly performance sensitive boards and increasingly uneasy CEOs. *Working paper* .

Karuna, C. (2007). Industry product market competition and managerial incentives. *Journal of Accounting and Economics , 43*, 275-297.

Li, X. (2010). The impact of product market competition on the quantity and quality of voluntary disclosures. *Review of Accounting Studies , 15* (3), 663-711.

Nalebuff, B. J., & Stiglitz, J. E. (1983). Information, competition, and markets. *American Economic Review, Papers and Proceedings , 73*, 278-283.

Nickell, S. J. (1996). Competition and corporate performance. *Journal of Political Economy , 104*, 724-746.

Parrino, R. (1997). CEO turnover and outside succession: A cross-sectional analysis. *Journal of Financial Economics , 46* (2), 165-197.

Raith, M. (2003). Competition, risk and managerial incentives. *American Economic Review , 93*, 1425-1436.

Scharfstein, D. (1988). Product-market competition and managerial slack. *RAND Journal of Economics , 19*, 147-155.

Schmidt, K. M. (1997). Managerial incentives and product market competition. *Review of Economic Studies , 64*, 191-213.

Warner, J., Watts, J., & Wruck, K. (1988). Stock prices and top management changes. *Journal of Financial Economics , 20*, 431-460.

Weisbach, M. S. (1988). Outside directors and CEO turnover. *Journal of Financial Economics , 20*, 431-460.