# Board Structure and Monitoring: New Evidence from CEO Turnovers

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## Abstract

We use the 2003 NYSE and NASDAQ listing rules concerning board and committee independence as a quasi-natural experiment to examine the causal relations between board structure and CEO monitoring. Noncompliant firms forced to raise board independence or adopt a fully independent nominating committee significantly increase their forced CEO turnover sensitivity to performance relative to compliant firms. Nominating committee independence is important even when firms have an independent board, and the effect is stronger when the CEO was on the committee. We conclude that more independent boards and fully independent nominating committees lead to more rigorous CEO monitoring and discipline.

**Keywords:** CEO turnover; Nominating committee independence; Board Independence; Board monitoring; Independent directors; Sarbanes-Oxley Act; Endogeneity

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## 1. Introduction

Raising independent director representation is often advocated by institutional investors and regulators as a way to strengthen board oversight. By definition, independent directors are better able to challenge CEOs than gray or inside directors and they have incentives to develop a reputation as skilled decision control experts in the corporate directorship market (Fama, 1980). Yet, these directors often face limited access to firm-specific information and high costs in assessing its reliability, which reduces their monitoring incentives and effectiveness (Jensen, 1993; Raheja, 2005; Adams and Ferriera, 2007; Harris and Raviv, 2008; Masulis and Mobbs, 2011). Independent directors also typically hold small equity stakes in firms where they are board members, which limit their financial incentives to carefully monitor (Perry, 1999). Appointing more independent directors to a board can also aggravate a free-rider problem among independent directors, which can further weaken board monitoring (Harris and Raviv (2008)). These incentive problems could offset the reputation benefits of being a careful monitor (Fama (1980)). Hence, the net effect on CEO monitoring of increasing independent directors is theoretically ambiguous and ultimately an empirical question.

Although a large empirical literature exists on the effects of independent directors, endogeneity of board structure remains a major concern in interpreting the causal implications of the existing empirical evidence.[1] It is unclear whether particular board structure leads to certain decisions or whether boards that make certain decisions tend to have particular board structures. In this study, we exploit an exogenous shock to the board structure of publicly listed U.S. firms to overcome this endogeneity problem. A unique feature of our experiment is that the exogenous shock affects the degree of independence of both the board and its major committees. This allows us to provide evidence on a causal relation not only between overall board independence and board monitoring, but also between the independence of key board committees and board montoring. The latter evidence is very important because boards do most of their work in committees (Adams, Hermalin amd Weisbach, 2010) and we know much less about how committee composition affects the effectiveness of board monitoring than we know about board composition.

[1] See surveys by Bhagat and Black (2002), Hermalin and Weisbach (2003) and Adams, Hermalin and Weisbach (2010).

One committee that is likely to be particularly important in determining the quality of board oversight of the CEO is the board's nominating committee. This committee is delegated to nominate new directors for election at the annual meeting and is usually responsible for evaluating individual director performance and approving their renominations.[2] The composition of this committee could affect not only the composition of the entire board but also the actual independence of directors and ultimately the quality of board oversight (Shivdasani and Yermack, 1999).[3] Management control over the director selection process is long recognized as a major cause of weak internal governance (Berle and Means, 1932; Jensen, 1993),[4] yet, the existing corporate governance literature has largely focused on how the outcome of this selection process – e.g. the percentage of independent directors on the board – affects board oversight. With few exceptions, prior research pays little attention to the critical question of who selects directors and how this selection process affects board monitoring.[5] In this study, we fill this gap.

To provide evidence on the effectiveness of board monitoring, we focus on one of the most important decisions made by the boards of directors – the decision to fire a CEO.[6] Hermalin and Weisbach (2003) argue that "one way to evaluate the board's effectiveness is to look at the quality of these (CEO turnover) decisions." Hermalin (2005) observes that among the few corporate decisions where boards plays a significant role "are those decisions pertaining to the selection, monitoring, and retention (or dismissal) of the CEO." Similarly, Jenter and Kanaan (2014) begins their study by saying "Whether to retain or fire a CEO after bad stock price or accounting performance is one of the most important decisions made by corporate boards."

[2] According to the NYSE's new exchange listing requirements, the nominating committee must at the minimum be responsible for identifing individuals qualified to become board members; selecting, or recommending that the board select the director nominees for the next annual meeting of shareholders; developing and recommending to the board a set of corporate goverance guidelines applicable to the corporation and overseeing the evaluation of the board and management.

[3] Shivdasani and Yermack (1999) find that when the CEO is on the nominating committee, a firm appoints fewer outside directors and more gray directors.

[4] 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 existing management, they can virtually dictate their own successors."

[5] Core, Holthausen and Larcker (1999) find that CEO compensation is higher when the CEO is involved in the nomination of new directors. Grinstein and Hribar (2004) find that the merger bonus given to acquiring firm CEOs is higher when the CEO is involved in the nomination of new directors. However, the causality is not clear in these studies.

In studies of board structure and CEO turnover, unobservable factors present a serious endogeneity concern since they can potentially contaminate the causal interpretation of the statistical relationship between board composition and forced turnover sensitivity to performance. As a illustration, one can think of a situation where independent directors prefer to serve on boards that intensively monitor management, say out of concern for their reputations in the labor market for directors. We would then expect to observe a positive association between board independence and board monitoring intensity, for example, as measured by forced CEO turnover sensitivity to performance. However, this correlation is driven by omitted firm characteristics that make these firms more attractive to independent directors, rather than by board independence itself. Thus, it would be mistaken to conclude from this correlation that greater board independence necessarily leads to stricter board monitoring. In reality, endogenous relations between board (committee) independence and board decisions can be complicated. Since the omitted variables that drive these endogenous relations are usually unobservable, exogenous shocks that substantially alter board structure is one promising way to overcome this endogeneity problem.

To overcome the endogeneity problem, we utilize an exogenous shock to board structure due to new NYSE and Nasdaq listing rules issued in 2003. In the wake of major corporate scandals invoving such high profile firms as Enron, Worldcom and Tyco Internationl in 2001, the U.S. congress passed the Sarbanes-Oxley Act (SOX) in July 2002. Shortly thereafter, the NYSE and Nasdaq proposed new exchange listing rules to strengthen the internal governance of listed firms. With minor modifications, the SEC approved these proposals in late 2003 and they went into effect in 2004 and 2005. The main provisions of the new exchange listing rules require the board of each publicly listed company to have (1) a majority of independent directors, and (2) fully independent nominating, compensation, and audit committees, defined as a committee composed of only independent directors and hereafter labeled an independent committee.[7]

[7] These exchange listing rules also require non-management directors to hold regular meetings that exclude management. Nasdaq allows a majority of independent directors to nominate directors or set CEO compensation if a firm lacks a nominating or compensation committee. "Controlled" firms where an individual, group or another firm holds a majority of voting rights are exempt from the independent board, nominating and compensation committee requirements.

One major attraction of this natural experiment is that for each individual board structure requirement of the new exchange listing rules, a large number of firms are in compliance prior to the new rules. These compliant firms represent potential control firms in a difference-in-difference analysis that facilitates controlling for the temporal trends, such as SOX's effect on internal governance and the heightened public scrutiny of governance in U.S. public firms over time.[8] A second significant attraction of this natural experiment is the cross-sectional differences in firms' prior noncompliance with individual board structure rules. For example, a firm can have an independent board, but be non-compliant with one or more rules requiring full independence of major board committees or vice versa. This offers a rare opportunity to isolate the importance of board independence from that of the full independence of major board committees, particularly the nominating committee. An attractive feature of this experiment, which makes separating the nominating committee indepenence effect possible, is that a large number of firms in our sample had an independent board prior to the rule changes, but lacked a fully independent nominating committee.

We hypothesize that mandatory adoption of an independent board should substantially improve its monitoring role and lead to heightened sensitivity of forced CEO turnover to prior firm performance. This prediction follows from a large empirical literature on the board of directors, which generally finds that firms with better governance are associated with greater CEO turnover to performance sensitivity.[9] Hence, if a mandatory change in board structure improves monitoring, we expect to see greater sensitivity of forced CEO turnover to firm performance in the affected firms.

The prediction of higher turnover-performance sensitivity in firms with a more independent board is also supported by the equilibrium outcome in the theoretical model in Hermalin and

[8] The official name of Sarbanes-Oxley Act is the Public Company Accounting Reform and Investor Protection Act, whose objective is to insure the accuracy and credibility of corporate financial reports. The SOX has a single board structure requirement, i.e. all public firms must have a fully independent audit committee (Section 301). This is the only overlap between SOX and the new exchange listing rules we study. Except for Section 404, all the other provisions of SOX affect almost all U.S. public firms and must be implemented by the end of 2003. This precedes the compliance dates of the exchange listing requirements, which occur in 2004 and 2005. Section 404 (a) requires companies to include in their annual reports an assessment of internal control for financial reporting (ICFR), and Section 404(b) requires companies to provide an independent auditor's attestation. For smaller public companies defined as those with an equity market capitalization less than $75 million, the implementation of Section 404(a) was postponed until after December 15, 2007, while the implementation of Section 404(b) was initially postponed for these firms and later became an exemption under the Dodd-Frank Act. We find that our noncompliant samples are not dominated by these smaller public companies. In fact, the mean market capitalization of firms noncompliant in 2001 with any of the four rules is above $6 billion.

[9] Weisbach (1988); Huson, Parrino and Starks (2001); Lel and Miller (2008); Jenter and Lewellen (2010); Kaplan and Minton (2012); Mobbs (2013); Coles, Daniel and Naveen (2014).

Weisbach (1998). In their model, the board learns about an incumbent CEO's ability from prior firm performance and the possible later acquisition of a private signal. Monitoring in the model refers to a board's effort to acquire a private signal about CEO ability. An independent board is assumed to be more willing to exert the effort to acquire the private signal than a non-independent board (i.e. an independent board conducts more intentisve montiroing). Since the right to fire the CEO after obtaining the private signal gives the board a valuable option and a new CEO has higher option value than an incumbent CEO due to the higher uncertainty about a new CEO's ability, an independent board is more willing to replace the incumbent CEO with a new CEO after observing poor firm performance than is a non-independent board. This reflects the independent board's greater likelihood of acquiring a private signal. Thus, in equilibrium, an independent board is less tolerant of poor firm performance than an non-independent board. [10]

A fully independent nominating committee excludes the CEO and her subordinates as well as gray directors from the nominating process, which can crucially affect board monitoring for at least two reasons. First, independent directors become more independent of the CEO if there is less concern that CEO displeasure with their actions can reduce their likelihood of board re-nomination. Second, requiring nominations to come from independent directors reduces the CEO's opportunities to influence the selection of nominees to individuals who meet the regulatory definition of independence, but are connected to a CEO in other ways, e.g. through social connections (Hwang and Kim, 2009; Fracassi and Tate, 2012). Thus, making the same theoretical argument used for board independence, we hypthesize that a shift to an independent nominating committee raises forced CEO turnover sensitivity to firm performance in affected firms. Unlike the nominating committee, audit and compensation committee duties suggest their independence does not directly affect board incentives to evaluate and replace incumbent CEOs. So we expect audit and compensation committee independence to have weaker or insignificant effects on CEO turnover decisions compare to an independent board or an independent nominating committee.

[10] As noted by Hermalin and Weisbach (1998), the same prediction is generated by assuming independent directors attribute less value to retaining the incumbent CEO than do non-independent directors because of their reputation concerns, better alignment with shareholder interests, or lack of other ties to the CEO.

Our empirical analysis is based on a difference-in-difference (DiD) methodology where we exploit the cross-sectional variations in a firm's degree of compliance to these new listing rules prior to their implementation. Treatment and control firms are defined for each new exchange listing rule. To mitigate any bias introduced by firms self-selecting into compliant and noncompliant status with respect to each specific new exchange listing rule, we match treatment and control firms based on both industry and propensity scores prior to the rule change and then perform DiD analysis. Since firms can be noncompliant with one or more of these new listing rules, we exploit various sample variations to isolate the individual effects. Since our sample covers a long period of varying economic conditions and given that CEO turnover policies may change with the macro economy, we allow our baseline sensitivity of forced CEO turnover to firm performance to vary by year. We also include firm fixed effects to control for any unobservable time-invariant differences across firms.

Our main findings are that after these new rules take effect, firms previously non-compliant with either the board or nominating committee independence requirements, exhibit significantly greater rise in sensitivity of forced CEO turnover to firm performance relative to previously compliant firms. When firms are previously compliant with the board independence requirement, but not compliant with the nominating committee independence requirement, moving to a fully independent nominating committee leads to a significant rise in CEO turnover sensitivity to firm performance. In contrast, changes in board structure required to meet the independent audit and compensation committee rules have no significant effect on CEO turnover to performance sensitivity. These results hold whether we measure firm performance by market-adjusted stock return or industry-adjusted change in EBIT. When p-values are calculated from simulated distributions of test statistics for placebo board structure changes, statistical significance remains unchanged or actually rises.

We use a hypothetical fall in firm performance from the 75$^{th}$ percentile to the 25$^{th}$ percentile of the sample to assess the economic size of the two listing rules effects. Our estimated board independence effect indicates that the probability of forced CEO turnover rises by 2.4% (2%) more in the post-treatment period relative to the pre-treatment period for noncompliant firms relative to compliant firms, when performance is measured by market-adjusted stock returns (industry-adjusted

changes in EBIT). Likewise, our estimated nominating committee independence effect indicates that the probability of forced CEO turnover rises by 2% (2.1%) more in the post-treatment period than in the pre-treatment period for noncompliant relative to compliant firms. To understand the economic significance of these figures, we compare them to the average rise in the probability of forced CEO turnover for the same drop in stock (operating) performance across different samples of firms. In the pre-treatment period, the rise is only 1.5% (0.4%) for firms noncompliant with the board independence rule and 1.7% (0.65%) for firms noncompliant with nominating committee independence rule. So in both cases the incremental rise in the post-treatment period is larger than the rise in the pre-treatment period for noncompliant firms. This evidence supports the view that *ceteris paribus*, independent directors provide more intensive internal monitoring than do inside or gray directors and that an independent *nominating committee* is important to board monitoring, even for firms with a majority of independent directors.

Further supporting the conclusion that independent directors are responsible for the rise in monitoring intensity, we find that noncompliant firms further away from meeting the independent board requirement prior to the rule change exhibit a larger rise in sensitivity of forced CEO turnover to firm performance. Consistent with CEO involvement in the director nomination process being an important reason for ineffective monitoring, we find among firms without an independent nominating committee, those firms with the CEO on the nominating committee exhibit greater gains in sensitivity of forced CEO turnover to performance after the rules change than other noncompliant firms.

We investigate an array of sensitivity tests to assess the robustness of our major findings. We conduct a placebo test to assess the statistical significance of our results, allow the treatment effect to be nonlinear in firm performance and calculate the marginal effects from a logit model. We control for CEO pay-performance sensitivity and its interaction with firm performance, explore different propensity score matching model specifications as well as a simple matching model based on firm size and industry. We exclude firms that could be misclassified as noncompliant due to the stricter RiskMetrics definition of director independence. We exclude firms that fail to meet the board or nominating committee independence requirements in 2005, the final year for compliance since they

are likely to be "controlled companies" exempt from these new exchange listing requirements. Our central findings survive all these robustness tests.

This study makes three contributions to the corporate governance literature and to the policy debates on the regulation of corporate boards. First, to our knowledge, we are the first study to document a causal relation between an independent nominating committee and forced CEO turnover. We find that even in firms with independent boards, the independence of the nominating committee causes closer scrutiny of the CEO and a more expeditious board response to underperformance. This evidence fills an important gap in the existing literature on boards of directors regarding the relevance of committee independence. One policy implication of this finding is that governance reforms seeking to enhance board monitoring of CEOs should consider strengthening the nominating committee independence requirement to complement the board independence requirement.

Second, we provide the most credible statistical evidence to date on the *causal* relation between overall board independence and CEO turnover.[11] Weisbach (1988) documents a significant *association* between overall board independence and CEO turnover. He finds that CEO turnover is more sensitive to 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), where an outside director is defined as not working for or having extensive dealings with the company or its senior management. Dahya et al. (2002) examine changes in CEO turnover sensitivity to performance for U.K. firms that adopt the Cadbury Commission recommendations. They report that adopting firms experience a rise in CEO turnover sensitivity to performance. A key limitation of this experiment is the voluntary nature of this recommendation, which means that the board changes are not actually exogenous. In contrast, we study the effects of new exchange listing rules, which are mandatory for exchange listed firms.[12] We also use more rigorous econometric techniques to identify the board

[11] Chhaochharia and Grinstein (2009) exploit the same natural experiment to study the effect of board independence on CEO compensation. Guthrie, Sokolowsky and Wan (2010) revisited these results and find that board independence has no effect on CEO total pay, while firms forced to adopt an independent compensation committee *raise* CEO total pay. While higher CEO pay can indicate weaker monitoring, it can also reflect more intensive monitoring. For example, in Hermalin (2005), an rise in board monitoring reduces CEO job stability, which requires higher pay to compensate for higher CEO risk-bearing. In robustness analysis, we find that controlling for CEO pay for performance sensitivity leaves our major findings unchanged.

[12] Dahya et al. (2002) note that compliance with the Cadbury Code is not totally voluntary because London Stock Exchange requires listed companies to "comply or explain". However, to the extent that some firms did choose not to adhere to the

independence effect than in earlier studies.[13] Taking a very different approach, Knyazeva, Knyazeva and Masulis (2013) exploit the wide variation in local supplies of independent director candidates as an exogenous instrument to examine how board independence affects board monitoring and firm performance. They find board independence causes higher CEO turnover-performance sensitivity and better firm performance. However, their study excludes large cap firms (top quartile) which are better able to recruit outside directors from beyond the local labor market.

None of these earlier studies control for nominating committee independence. However, we find 95 percent of publicly listed U.S. firms that lacked a majority of independent directors also lacked a fully independent nominating committee prior to SOX. Such high correlations raise questions about the conclusions drawn in prior studies that only analyze board independence and ignore the level of nominating committees independence. Specifically, it is unclear to what extent the effects reported in prior studies of board independence are due to nominating committee independence. To our knowledge, this is the first study to disentangel the two effects and document clear causal links between forced CEO turnover and both board and nominating committee independence.

Lastly, we document an important mechanism through which stock exchange listing rules can significantly alter the level of board monitoring. While the intent of the new rules is clear, their ultimate effects are much less so. Some commentators question whether board independence can be truly increased by setting numerial targets for the percentage of independent directors on a board. After all, CEOs and board members have great latitude in the selection process to offset the benefits created by exogenously imposed 'independence' (Hermaline and Weisbach, 1998; Romano, 2005), thereby circumventing the rule's original intent. Our evidence indicates that the new exchange listing rules have placed binding constraints on the director selection process of affected firms, leading to a rise in the level of board independence. To the extent SOX via pressure from the SEC placed added pressure on stock exchanges to issue stricter corporate governance rules than otherwise, these listing rule changes can be viewed as an indirect effect of SOX.

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Code, the interpretation of the results is still contaminated by endogeneity concerns. Furthermore, it is unclear whether their evidence is applicable to firms in other countries that differ in terms of board powers and other governance mechanisms.

[13] Rather than using a DiD method, Dahya et al. (2002) run separate regressions for compliant and noncompliant firms.

## 2. Empirical methodology

### 2.1. Difference-in-Difference specification

The 2003 NYSE and Nasdaq exchange listing rules were introduced at a time when the corporate governance of U.S. public firms was undergoing intense scrutiny and experiencing substantial change. In July 2002 the U.S. Congress passed the Sarbanes-Oxley Act in response to corporate accounting scandals involving such firms as Enron, Worldcom and Tyco International. It brought sweeping reforms to financial reporting in publicly held firms and significantly raised the legal accountability of CEOs and CFOs. To benchmark this temporal effect, we adopt a DiD approach that uses firms previously in compliance with the new listing rules as control firms to estimate this temporal change. The effect of changing board structure on CEO turnover sensitivity to performance is identified by a change in forced CEO turnover to firm performance sensitivity that exceeds this common time trend.

To implement the DiD analysis, we estimate the following linear probability model (LPM) for each treatment that we study:

$$Y_{i,t+1} = \alpha + \beta(T_i P_t) + \gamma(T_i R_{it}) + \delta(T_i P_t R_{it}) + \boldsymbol{\phi}'\boldsymbol{\pi} R_{it} + \boldsymbol{\psi}'\boldsymbol{X_{it}} + \lambda_i + \lambda_t + \varepsilon_{it} \quad (1)$$

where $Y_{i,t+1}$ equals 1 if a forced CEO turnover occurs in firm $i$ in year $t+1$ and 0 otherwise. $T_i$ is an indicator for a treatment. It equals one if firm $i$ is noncompliant with the specific new exchange listing rule under study at year-end 2001 and zero otherwise. $P_t$ is an indicator for the post-treatment period. It equals one for the years 2005 and later and zero otherwise. Listed firms are required to comply with the new NYSE and Nasdaq 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. Hence, we treat fiscal year 2005 as the first year of compliance. $R_{it}$ is firm $i$'s performance in year $t$, measured by either market-adjusted stock returns or industry-adjusted changes in EBIT where industry is defined by Fama and French 48 industries. Since our sample covers a long period of varying macroeconomic conditions, we allow the baseline sensitivity of forced CEO turnover to performance to vary by year. This is accomplished by multiplying $R_{it}$ by a vector $\pi$ of year fixed effects. $X_{it}$ is the vector of firm level controls and $\lambda_i$ and $\lambda_t$ are firm and year fixed effects, respectively, and $\varepsilon_{it}$ is the error term. The

specification does not include indicators for treatment firm and post-treatment period because they are subsumed by the firm and year fixed effects.

The coefficient of main interest is $\delta$. It measures the change in sensitivity of forced CEO turnover to firm performance in treatment firms relative to control firms from before to after the treatment. Our hypothesis predicts that $\delta < 0$ for both the board and nominating committee independence rules. It is worth noting that coefficient $\beta$ also measures a difference-in-difference effect, but it is the effect on the average *rate* of forced CEO turnover, rather than CEO turnover sensitivity to performance. While an increase in CEO turnover-performance sensitivity should in general lead to higher rate of CEO turnover, the rate of CEO turnover can be affected by many other factors beyond firm performance. For example, if CEOs work harder after SOX, then even if CEO turnover-performance sensitivity rises, the average rate of CEO turnover may not increase. Hence, we do not test the hypothesis on $\beta$, even though a positive $\beta$ is suggestive of more intensive monitoring.

We report a linear probability model as opposed to a non-linear logit or probit model for two reasons. First, unobservable firm attributes are an important source of omitted variable bias in corporate governance studies. Including firm fixed effects is an effective way to control for time-invariant firm attributes. Unfortunately, firm fixed effects cannot be easily included in logit or probit models since maximum likelihood estimators are generally inconsistent with fixed effects due to the incidental parameter problem. However, they can be easily included in linear models.[14] Second, as our main interest is in the marginal effects, the linear model has the advantage that its coefficients can be directly interpreted. Assessing the statistical significance of this marginal effect in a non-linear model is less straightforward (Ai and Norton, 2003), especially when the marginal effect of interest involves a triple interaction term as in our case (i.e. $T_i P_t R_{it}$). Like us, Cornelli, Kominek and Ljungqvist (2012) use a linear probability model to estimate their CEO turnover model with a triple interaction term. Nevertheless, as a robustness check, we later estimate logit models and calculate the

[14] Our model choice is also consistent with the recommendation in Adams et al. (2010) who observe that few existing CEO turnover studies control for firm-specific heterogeneity and such an approach would provide a valuable contribution to the understanding of the CEO turnover process.

size and statistical significance of the marginal effects using the delta method. We find that the logit estimates are similar in size to our linear probability estimates and are statistically more significant.

**2.2. Propensity-Score Matching**

The DiD approach we take allows for heterogeneity in treatment and control firms, which in our case means that the treatment and control groups can have their own average rate of forced CEO turnover and sensitivity of forced CEO turnover to firm performance, both before and after the treatment. However, the DiD effect is identified by assuming a parallel temporal trend in the turnover-performance relation in the period from before to after the treatment in both the treatment and control groups in the absence of the treatment. To the extent that firm characteristics that determine the endogenous choice of board structure before the treatment also affect the time trend, the DiD estimates can be biased due to differences in the time trends of treatment and control groups.

To mitigate this potential bias, we first estimate a probit model to predict the likelihood that a firm is noncompliant with a particular new exchange listing rule at year-end 2001 using data from 1996 to 2000. The predicted likelihood is called the propensity score. We then match each treatment firm with one or more control firms drawn from the same Fama and French 48 industry and having propensity scores within a predefined radius of the treatment firm in 2001. This method is known as *Radius Matching* (Dehejia and Wahba (2002)). A benefit of radius matching is that it allows for the use of additional control firms when the matches are good, which helps reduce the variance of the estimates. The choice of radius is based on two considerations. On the one hand, a smaller radius increases the quality of the matches. On the other hand, it also increases the probability that a match cannot be found. Failure to find a match is a particular concern in CEO turnover studies since forced CEO turnovers are infrequent and thus only a limited number of forced CEO turnovers are available in our sample period. Based on this trade off, we choose a 0.15 radius for *all* our reported results. In unreported results, we find that using a radius of 0.1 or 0.2 does not change our results significantly.

We rely on recent theories of corporate boards to guide our specification of the propensity score models. Boone, Field, Karpoff, and Raheja (2007) summarize existing theories on corporate boards into three non-mutually exclusive hypotheses. The scope and complexity hypothesis predicts that

more assets and greater complexity give rise to more serious agency problems requiring more independent directors for monitoring. The monitoring hypothesis predicts that firms operating in noisy environment are more costly for outsiders to monitor and thus are associated with a less independent board. The bargaining hypothesis posits that more influencial CEOs are able to bargain for more inside and gray directors on the board.

In the propensity score model we use to predicts whether a firm was noncompliant with the independent board rule in 2001, we include measures for each of the three hypotheses. We use total assets to measure a firm's scope and complexity, the entrenchment index (E-index) of Bebchuk, Cohen, and Ferrell (2009) to measure the benefit of private control, market-to-book ratio and monthly stock return variance to measure the cost of monitoring by outside directors, indicators for dual class firms and CEO-Chairs, CEO tenure and age and non-independent director voting power to measure CEO influence on the board and an indicator for a nonemployee blockholder-director to capture limits on CEO influence. A blockholder is defined as an investor having over 1% of firm voting power.

To predict the likelihood a firm has an independent board committee, e.g. a nominating committee, we add the fraction of independent directors on the board as another predictor. Our reasoning is that, with more independent directors, the likelihood of independent board committees rises since more independent directors are available to become committee members and the independent directors have greater voting power to push for more independent board committees.

Coefficient estimates of the four propensity score models used to predict treatment group membership under the four new listing rules are reported in Appendix 2. All models are estimated using 1996-2000 data, the time period prior to when we classify firms into treatment and control groups. The sign and statistical significance of the coefficients in Appendix 2 offer partial support both at the board and board committee levels for the three hypotheses on determinants of board independence studied in Boone et al. (2007) and are generally in line with prior board studies.

We recoganize that, although matching between treatment and control units should in general mitigate the estimation bias in observational studies, we can only match firms on observable attributes. As a result, matched firms may still be different on unobservable attributes. To mitigate this

concern, we control for any unobservable time-invariant differences across firms by including firm fixed effects in all our DiD regressions.

## 3. Data and Sample

### 3.1. Data

We obtain data on boards of directors from the RiskMetrics database and data on CEO turnover from the ExecuComp database. Our sample period is from 1996 to 2009. Year 1996 is the first year that board of directors data are available from RiskMetrics, while year 2009 is the last year we have information on CEO turnovers announced in the following year. Firm financial data are from Compustat and stock return data from CRSP. Institutional holdings data are from Thomson Reuters.

Our sample criteria requires that firms must have (1) director independence data available in RiskMetrics for fiscal year 2001 and (2) their common stock is listed on the NYSE or Nasdaq from 2001 to at least 2005. The first requirement enables us to classify firms at year-end 2001 by compliant and non-compliant status with respect to each of the four new exchange listing rules. Although the deadlines for compliance with the new exchange listing rules are in 2004 and 2005, some firms began to make board structure changes as early as when the NYSE and Nasdaq proposals first became public in August and October of 2002 respectively. Thus, board data in fiscal year 2001 represent the last year board structure is not influenced by the new listing rules since compliance can occur in fiscal year 2002 if a firm's fiscal year-end is later than August.[15] The second requirement insures that sample firms are subject to regulatory shocks and our results are not simply driven by firms entering and exiting the sample before or after the adoption of the new listing rules. These criteria yield a sample of 1,231 firms in year 2001 and 17,080 firm-year observations over the years 1996-2009. Panel A of Table 1 reports summary statistics of the full sample.

For each of the four new exchange listing rules on board structure, we classify sample firms into either a treatment group or a control group based on whether their board structure in 2001 is in compliance with the specific new exchange listing rule. Panels B and C of Table 1 compare major characteristics of treatment and control firms in 2001 defined by the four listing rules. Generally

[15] As a robustness check, we also repeat our analysis on 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.

speaking, treatment and control firms differ systematically on many characteristics with respect to all four listing rules. Noncompliant firms with respect to the board independence rule or nominating committee independence rule are on average smaller and have more growth opportunities and more volatile stock returns than compliant firms. Consistent with internal and external governance mechanisms serving as substitutes, these noncompliant firms on average have fewer antitakeover provisions as measured by the E-index than compliant firms (Guo, Lach and Mobbs, 2015). By design, noncompliant firms have a significantly lower fraction of independent directors on the board than complaint firms. However, the lack of board independence seems to be somewhat offset by other governance mechanisms. For example, noncompliant firms are less likely to have a dual CEO-Chairman, but more likely to have an outside blockholder on the board.

Overall, the systematic differences in treatment and control firms suggest that compliance in 2001 with subsequent exchange listing rule changes is not random; rather these two groups of firms have systematically different characteristics. To control for these differences, we match treatment and control firms by industry and propensity scores prior to undertaking a DiD analysis.

Table 2 reports the distribution of our sample firms by compliance status with the four new exchange listing rules at year-end 2001. The table shows that most firms have a majority of independent directors at year-end 2001. Hence, only 25% of sample firms are affected by the rule requiring an independent board. At the board committee level, most firms also have independent audit (69%) and compensation (71%) committees by year-end 2001. In contrast, 69% of sample firms lacked an independent nominating committee and thus, are affected by the exchange rule on nominating committees. The table shows that board independence is highly positively correlated with full independence of key board committees. One particularly striking statistic is that 95% of firms without a majority of independent directors on the board in 2001 also lacked an independent nominating committee.

### 3.2. CEO turnover sample

CEO turnovers occur for many reasons. Besides forced departures for poor performance or violation of laws or codes of conduct, CEOs often voluntarily leave offices due to retirements, major

health problems, or appointments to more attractive positions, etc. They can also die in office. In this study, we are mainly interested in CEO turnovers that are the result of board disciplinary actions for poor performance. Consequently, we divide CEO turnovers into forced and voluntary following the classification method developed by Parrino (1997) and now commonly used in CEO turnover studies (e.g., Huson, Malatesta and Parrino, 2004; Hazarika, Karpoff and Nahata, 2009; Jenter and Kanaan, 2014). We search the *Factiva* and *Lexis-Nexis* databases to find the earliest announcement dates for CEO turnovers and other information needed to classify them.

A CEO turnover is classified as forced if (1) news articles mention that a CEO was fired, forced out or left due to unspecified policy differences; (2) 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; (3) a CEO retirement is not announced at least six months in advance. We further refine reason (3) by checking a wider range of news sources to insure that no indications of a voluntary resignation are found. If an incumbent CEO takes a comparable position elsewhere or departs for reasons unrelated to the firm or are purely personal in nature, we reclassified the turnover as voluntary. Remaining CEO turnovers not in categories (1) – (3) are classified as voluntary.

Of the 1,231 firms that meet our sample criteria in 2001, there are a total of 1,902 CEO turnovers between 1996 and 2010, of which 442 are classified as forced and 1460 are classified as voluntary. This translates into an 8.5% annual turnover rate for the full sample and a 2% rate for the forced turnover sample. These values are close to those reported by Jenter and Kannan (2014) for all firms in ExecuComp over the 1993-2001 period. In their sample, CEO turnovers occur in about 10 percent of firms annually and forced CEO turnovers occur in about 2.3 percent of firms annually. The difference between our sample and theirs is probably due to different sample criteria and sample periods.

One issue that requires special attention in the DiD analysis is that some forced CEO turnovers are not performance induced. A good example is the departure of Hewlett-Packard's CEO Mark Hurd in August 2010 for violating company business standards. In this period, H-P stock greatly outperformed the market, e.g. by 101% over his five-year tenure. Obviously, attempting to explain such a forced CEO turnover by poor firm performance would be seriously misleading. This is a not

major concern in CEO turnover studies that do not use a DiD approach because the presence of non-performance induced forced CEO turnovers only adds noise to the data, but does not affect the coefficient estimate's sign. But, in a DiD approach, a disproportional presence of non-performance induced forced CEO turnovers in the treatment group relative to the control group could bias our statistical inferences. This is a particular concern in our sample period as explained below.

In the years immediately after the passage of SOX, a number of high-profile investigations into earnings manipulation, option backdating and other inappropriate business practices by public firms occurred in U.S. Under SEC pressure, many firms conducted internal investigations into these matters. The investigations resulted in the firings of a number of top executives including CEOs. We identified CEO turnovers caused by statutory, regulatory and internal company codes of conduct violations by reading new articles on CEO turnovers. Consistent with independent directors being more effective monitors, we find a higher frequency of CEO turnovers announced post-SOX due to CEO legal or quasi-legal violations (and unrelated to poor firm performance) occurred pre-SOX in the board independence treatment group relative to the control group. Given the higher frequency of violation-related CEO turnovers in treatment firms, including turnovers due to legal violations could obscure any observed rise in forced CEO turnover sensitivities to poor performance in treatment firms relative to control firms. Thus, we exclude forced turnovers due to legal and quasi-legal violations from our analysis.

### 3.3. Change in board structure around the regulatory "shock"

The upper panel of Figure 1 reports changes in board structure for our sample firms over the 1998-2009 period. The plot begins in 1998 since board committee data in the RiskMetrics database is very spotty before 1998. We observe that the fractions of firms with a majority independent board, fully independent nominating, audit or compensation committees rise significantly between 2001 and 2005. The proportion of firms with a majority of independent directors on the board increases by 17 percent from 2001 to 2005, while the proportion of firms with fully independent nominating, audit and compensation committees rises by 43 percent, 16 percent and 12 percent, respectively.

In 2005, the year when mandatory compliance with all new exchange listing rules is required, firm compliance rates with the board, nominating, audit and compensation committee independence rules are 92%, 74%, 85% and 83%, respectively. The lack of full compliance in 2005 can be due to four reasons. First, this figure uses fiscal years, rather than calendar years. Second, the RiskMetrics definition of independent directors is more restrictive than the NYSE and Nasdaq definitions. For example, the NYSE and Nasdaq define former employees as independent if they left the firm more than three years earlier, while RiskMetrics considers all former employees non-independent. The exchanges also allow independent directors to have an "insignificant" business relation with the firm, while RiskMetrics treats any director with a business relation as non-independent. Thus, some directors that NYSE and Nasdaq consider independent, RiskMetrics classify as gray.[16] Third, closely controlled companies are exempt from the exchange rules on board, nominating and compensation committee independence. Lastly, Nasdaq exempts firms from the independent nominating and compensation committee rules provided director nominations and executive compensation decisions are determined by a majority of independent directors.[17]

The bottom panel of Figure 1 shows the mean percentage of independent directors on the board over the 1996-2009 period with firms classified into those with and without a majority of independent directors in 2001. We observe that in treatment firms the proportion of independent directors is relatively stable before 2001, but increases significantly after 2001. On the other hand, in control firms, the proportion of independent directors increases only gradually over the entire sample period. Thus, it is clear that the new exchange listing rules represent a significant shock to the board structure of many publicly listed firms that we classify as treatment firms. [18]

### 3.4. Firm performance and control variables

[16] Although noncompliant firms should be determined based on the exchange definitions, this is not practical since the size of business relations between directors and firms are generally unobservable. We conduct a robustness check on this issue later and find that our results are not affected by the misclassification of compliant and noncompliant firms.

[17] This noise in the classifications should in general bias against us finding a significant difference between treatment and control groups. To the extent that we do find differences, they do not affect our conclusions. We explicitly check the robustness of our results to these issues and find that our results are not affected by the misclassification of compliant and noncompliant firms and the inclusion of controlled companies as shown in the Internet Appendix sections E and F.

[18] For information on how our sample firms complied with the new rules, please refer to Internet Appendix section A.

We measure firm performance by both market-adjusted stock returns and industry-adjusted changes in EBIT. A market-adjusted stock return is defined as a firm's monthly stock return minus the CRSP value-weighted market return, compounded over the 12 months that ends 30 days before the CEO turnover announcement. An industry-adjusted change in EBIT is defined as the annual change in EBIT in the fiscal year scaled by beginning-of-year total assets minus the Fama-French 48 industry median of this ratio, which is used in Weisbach (1988) and other prior CEO turnover studies. We use market-adjusted stock returns, rather than industry-adjusted stock returns, as recent CEO turnover studies find that boards adjust for market benchmarks, but not industry benchmarks, when making CEO turnover decisions (Kaplan and Minton, 2011; Jenter and Kanaan, 2014).[19] To measure operating performance, we use annual changes in EBIT rather than annual EBIT (where both are scaled by total assets) since CEO turnover decisions are likely to be mainly driven by unanticipated changes in performance (Weisbach, 1988). To reduce the influence of outliers, we winsorize the performance measures at both the 1$^{st}$ and 99$^{th}$ percentiles.

In our DiD analysis, we include an array of explanatory variables to control for the effects of any remaining time-varying differences in firm and governance characteristics across treatment and control firms. Most of these control variables are used in prior CEO turnover studies. These include firm size measured by log of sales, firm risk measured by standard deviation of the prior 12 monthly stock returns, a CEO-Chair indicator, an indicator for whether the CEO is a founder or heir, CEO voting power measured by the percentage of votes under CEO control, the presence of a nonemployee blockholder-director, CEO age and a set of CEO tenure (in years) indicators. Founder and heir information is hand-collected from an extensive search of CEO biographic information in firm proxy statements, firm websites and other electronic news sources, such as *Factiva*, etc.

## 4. Empirical Results

### 4.1. Univariate analysis

[19] We also tried to decompose a firm's stock returns into an industry-induced component and an idiosyncratic component following Jenter & Kanaan (2014) and measure stock performance by its idiosyncratic component. In unreported results, we find our conclusions are robust to this alternative stock performance measure.

Before estimating multivariate regressions to test our main hypothesis, we conduct a univariate analysis to see how the rate of forced CEO turnover when sorted on firm performance has changed in our sample of treatment and control firms from before to after the issuance of new exchange listing rules and whether there is any support for our main hypothesis.

We first sort our sample firms into treatment and control groups and into pre- and post-treatment periods. Then, we compare the rate of forced CEO turnovers over time and across treatment and control groups by firm performance terciles. We exclude forced CEO turnovers that are due to serious violations of law or the company's code of conduct, which we repeat in all of our later regression analysis. Based on our main hypothesis, we expect an increased rate of forced CEO turnover in the bottom firm performance tercile after the treatment for the treatment group relative to the control group, but we expect much weaker results in the other terciles.

Table 3 presents the results.[20] Panel A is based on the matched sample of treatment and control firms for the board independence rule. We find that the rate of forced CEO turnover rises significantly in the bottom terciles of both the market-adjusted stock returns and industry-adjusted change in EBIT for the treatment firms after the rule change. On the other hand, the rate of forced CEO turnover in control firms over the same time period exhibits an insignificant decline in the bottom terciles of both stock and accounting performance. The difference between treatment and control firms suggests that the rate of forced CEO turnover in the bottom stock (accounting) performance tercile is 5.79% (4.71%) higher post-treatment for the treatment firms relative to control firms. This difference is statistically significant at the 1% (10%) level. The differences between treatment and control firms for the other two terciles are smaller in magnitude and statistically insignificant.

Panel B is based on the matched sample of treatment and control firms for the nominating committee independence rule. Again, we find that the rate of forced CEO turnover rises for the treatment firms in the bottom firm performance tercile after the rule change, but this rise is only statistically significant for the market-adjusted stock return performance measure. In control firms, the

[20] To save space, we only report the results for these two rules because we only expect them to have significant impact on turnover-performance sensitivity.

rate of forced CEO turnover in the bottom firm performance tercile declines over the same time period. The difference between treatment and control firms suggests that the rate of forced CEO turnover in the bottom stock (accounting) performance tercile is 4.45% (2.02%) higher following treatment for the treatment firms relative to control firms. This difference is statistically significant at the 1% level for stock performance, while it is statistically insignificant for accounting performance. The differences between treatment and control firms for the other two terciles are smaller in magnitude and statistically insignificant.

We note that in both panels there is an overall decline in the rate of forced CEO turnover in the post-treatment period across all performance terciles. This time trend may reflect a cautious shift in board attitudes toward firing CEOs during the 2007-2009 global financial crisis. It is beyond the scope of this paper to explain the reason for this change, but it does show the importance of using the difference-in-difference approach to filter out this time trend in order to draw accurate inferences.

**4.2. Baseline results**

In this section, we use equation (1) to test our main hypothesis that mandatory changes in board and nominating committee independence lead to greater sensitivity of forced CEO turnover to firm performance in previously noncompliant firms, while mandatory changes in audit and compensation committee independence have less significant effects.

For each board structure change, we report two regressions, one measures firm performance by market-adjusted stock returns, the other by industry-adjusted changes in EBIT. Although the two performance measures are in general positively correlated, they contain different information. Stock returns mainly reflect the effect of an incumbent CEO's decisions on future cash flows, while accounting earnings mainly reflect the effect of an incumbent CEO's decisions on past operating performance. The board is likely to use both sources of information when evaluating CEO performance, although they may weight them unequally. Hence, a priori, an improvement in board monitoring may not lead to the same rise in sensitivity of forced CEO turnover to both performance measures. In other words, evidence of increased sensitivity of forced CEO turnover to one of these measures would support our hypothesis provided that the sensitivity to the other measure is either

unchanged or also increases, while finding increases in sensitivity to both performance measures would provide even stronger support for our hypotheses.

Table 4 reports DiD estimates of the effects of the four exogenous board structural changes on forced CEO turnover sensitivity to performance. Individual effects are captured by a triple interaction of *Post*, *Treat* and *Performance*. Columns 1 and 2 of Table 4 are estimated using a matched sample of treatment and control firms defined by the board independence rule. Consistent with our hypothesis, the coefficient on the triple interaction term is negative and statistically significant at 5% level for market-adjusted stock return (column 1) and at the 10% level for industry-adjusted changes in EBIT (column 2). Columns 3 and 4 of Table 4 report DiD estimates for a matched sample of treatment and control firms defined by the independent nominating committee rule. The analysis is analogous to that in columns 1 and 2 except *Treat* now indicates whether a firm is noncompliant with the nominating committee independence rule. Consistent with our hypothesis, the coefficient on the triple interaction term is negative in both columns and statistically significant at the 1% level for market-adjusted stock returns (column 3) and at the 5% level for industry-adjusted changes in EBIT (column 4).

In columns 5 through 8 of Table 4, we examine the effects of the independent audit and compensation committee rules. *Treat* indicates noncompliance with the audit (compensation) committee independence rule in columns 5 and 6 (columns 7 and 8). In contrast to the results in columns 1 - 4, we find that coefficients of the triple interaction of *Treat*, *Post* and *Performance* are statistically insignificant at conventional levels in all four columns, which confirms our prediction that these two board structure changes are unlikely to have a significant effect on forced CEO turnover sensitivity to firm performance. The insignificant results in columns 5 - 8 are further confirmation that the significant results in columns 1 - 4 are unlikely to be driven by biases in our empirical methodology or an omitted variable correlated with board structure changes after SOX. Otherwise, we should find similar results in columns 5 - 8 since we use the same methodology and changes in audit and compensation committee independence are likely to be correlated with the same omitted variable. Very complex assumptions on biases or omitted variables are needed to produce such different results.

Beyond the triple interaction term, two double interaction terms are also worth noting. They include interactions of *Post* and *Treat* as well as *Treat* and *Performance*. Both are positive for the board and nominating committee independence rules. The positive coefficient of the first double interaction indicates that the average rate of forced CEO turnover rises in treatment firms relative to control firms after the treatment. The positive coefficient of the second double interaction indicates that the turnover-performance sensitivity is lower in treatment firms relative to control firms in the pre-treatment period. These effects are statistically significant in some columns.

Examining the control variables in Table 4, we find that they are generally consistent with prior findings and are economically reasonable. The positive coefficient on the natural log of sales suggests that CEOs in larger firms are more likely to be fired, which is consistent with evidence in Huson, Parrino, and Starks (2001). The positive coefficient on stock volatility suggests that CEOs of riskier firm are more likely to be fired. Consistent with more powerful CEOs being fired less often (Denis, Denis and Sarin, 1997; Goyal and Park, 2002), we find that CEOs with more voting power or more board power (CEO-Chairs) and CEOs who are founders or founding family members are less likely to be fired, while a nonemployee blockholder-director raises the likelihood of forced CEO turnover.

To illustrate the economic significance of the effects of the board and nominating committee independence rules, we posit a hypothetical fall in firm performance from the 75$^{th}$ percentile (market-adjusted stock return = .23 or industry-adjusted change in EBIT = .0231) to the 25$^{th}$ percentile (market-adjusted stock return = -.20 or industry-adjusted change in EBIT = -.0157) of the sample. For the board independence rule, the coefficient estimate of -0.0556 (-0.525) in column 1 (column 2) shows that the rise in probability of a forced CEO turnover is 2.4% (2%) higher in the post-treatment period for treatment firms after controlling for a time trend between the pre- to post-treatment periods. Economically, this is quite significant since the average rise in the probability of a CEO being fired for the same change in market-adjusted stock return (industry-adjusted change in EBIT) is 2.2% (1.3%) in the overall sample, and it is only 1.5% (0.4%) in the treatment sample before treatment. [21]

[21] We estimate equation (1) without the DiD terms to establish a baseline relation between forced CEO turnover and firm performance in the full sample by varying performance, while setting all other variables at their respective sample means.

For the nominating committee independence rule, the coefficient estimate of - 0.0496 (-0.557) in column 3 (column 4) suggests that a rise in probability of forced CEO turnover is 2% (2.1%) higher in the post-treatment period than in the pre-treatment period in treatment firms, after adjusting for time trends in the pre- and post-treatment periods. This compares with a rise in the probability of a CEO being fired for the same change in market-adjusted stock return (industry-adjusted change in EBIT) of 2.2% (1.3%) in the overall sample and of 1.7% (0.65%) in treatment firms in the pre-treatment period.

**4.3. Separating the board and nominating committee independence treatment effects**

In Table 4, we include one treatment in each regression. However, according to Table 2, board independence and nominating committee independence are highly positively correlated. For example, in 2001, 39% of firms with an independent board have an independent nominating committee, while only 5% of firms without an independent board have an independent nominating committee.

Prior studies on board monitoring generally ignore the correlation between board and nominating committee independence and attribute evidence of more intensive board monitoring to board independence. But, given this high correlation and the findings in Table 4, it is important to know if board independence and nominating committee independence both matter for board monitoring or if only one matters. We next conduct a series of tests to answer these questions.

To separate the treatment effect of the board independence rule, ideally, we would like to find firms that have an independent nominating committee, but lack an independent board prior to the rule changes, because then these firms are only affected by the board independence rule. However, this sample (only 15 firms) is too small to conduct any meaningful statistical analysis on. We therefore explore three different variations in the sample to show the existence of a separate board independence effect. First, we explore the cross-section variations in firm compliance with the two rules (i.e. a large number of firms prior to SOX have independent boards, but lack independent nominating committees. A small number of firms have an independent nominating committee, but lack an independent board.). We modify equation (1) by including a full set of DiD terms for both board and nominating committee independence rules in the same regression and estimate the model using the matched sample of treatment and control firms defined by the board independence rule.

Second, we explore additional time-series variation in a firm's actual compliance year with each of the two rules (i.e. a firm can comply with the two rules in two different years prior to 2005) to further separate out the board independence effect. In these regressions, we use rule-specific post-treatment indicators. Specifically, we define a separate post-treatment period indicator for each rule. In firms noncompliant with one or both rules, the post-treatment period starts in the year the firm actually becomes compliant with that rule. In firms compliant with one or both rules, the post-treatment period starts in 2004 for the rule(s) that they are compliant with. We choose 2004 since it is in the middle of the period between 2003, the year a large number of firms began complying with the new exchange listing rules, and 2005, the year when listed firms must comply. [22]

Lastly, we exclude noncompliant firms that raise board independence by a small amount. We defined a small rise in board independence as one that is below 11%, which is the 25$^{th}$ percentile of improvement among firms noncompliant with the board independence rule. By excluding these firms, the board independence effect is likely to be stronger relative to the nominating committee independence effect in the remaining sample, which should make detection easier. These results are reported in columns 1-6 of Table 5. Consistent with a separate board independence rule treatment effect, we observe that the coefficients of the triple interaction of *Post*, *TreatBod* and *Performance* are negative and statistically significant or marginally significant in all columns.

In contrast to isolating the treatment effect of the board independence rule, a sharper test is possible to isolate the treatment effect of the nominating committee independence rule. In our sample, there are 925 firms with a majority of independent directors on the board in 2001, of which 61% lack an independent nominating committee. Since these firms are not affected by the board independence rule, we can use this subsample to isolate the treatment effect of the nominating committee independence rule.[23] For this purpose, we first construct a matched sample of treatment and control firms based on nominating committee independence status in year 2001. The propensity score model

[22] Given the actual compliance date can be endogenous, one should be careful not to interpret these results independently of those in columns 1 and 2 of Table 5.

[23] We do not consider the audit and compensation committee independence rules here because we already show in Table 4 that they do not affect CEO turnover decisions.

used here is identical to the one used for testing the nominating committee independence rule in Table 4, except that the model is only estimated for firms that have an independent board by 2001.

We next estimate equation (1) for this sample of firms and report the results in columns 7 and 8 of Table 5. *TreatNom* is an indicator for noncompliance with the nominating committee independence rule in year 2001. The coefficient on the triple interaction of *Post*, *TreatNom* and *Performance* is negative and statistically significant at the 1% level when firm performance is measured by market-adjusted stock returns (column 1) and significant at the 10% level when firm performance is measured by industry-adjusted changes in EBIT (column 2) based on two-sided tests, suggesting that post-treatment forced CEO turnovers become more sensitive to performance in treatment firms relative to control firms. This result supports a separate nominating committee independence effect on forced CEO turnover sensitivity to performance and indicates that an independent nominating committee raises this sensitivity, even for firms with independent boards.

**4.4. CEOs on the nominating committee**

CEO influence over director nominations is often cited as an important reason for weak board monitoring. We expect the nominating committee independence rule to have a larger impact on firms when the CEO is directly involved in the director nominating process prior to the rule change, indicated by the CEO being a nominating committee member. In our sample of firms that lack full nominating committee independence, 68 percent have the CEO on the nominating committee, while the remaining 32 percent of the sample have inside or gray directors as nominating committee members. In this section, we test this hypothesis using only firms noncompliant with the nominating committee independence rule in 2001.

We first match firms where the CEO is on the nominating committee, to firms where the nominating committee excludes the CEO following a similar propensity score matching procedure as before. We use the same set of predictors used in the independent nominating committee analysis to predict when the CEO is on the nominating committee since the two cases only differ in the degree to which the nominating committee lacks independence. This model is estimated from the firms noncompliant with the nominating committee independence rule in 2001.

Table 6 reports coefficient estimates from equation (1) estimated using the above matched sample of firms. *CEO on Nom* is an indicator for whether the CEO is a nominating committee member in 2001. Supporting our hypothesis, the coefficient of the triple interaction term of *Post*, *CEO on Nom*, and *Performance* is negative for both stock and accounting performance. It is statistically significant at the 5% level for stock performance, although insignificant for accounting performance. As discussed before, given that the board may give different weights to stock and accounting performance measures, finding a statistically significant rise in turnover-performance sensitivity for one of the firm performance measures, while the sensitivity to the other firm performance measure does not change, is sufficient evidence of more intensive board monitoring. In addition, the coefficient of the interaction of *CEO on Nom* and *Performance* is positive and statistically significant at the 10% level for both stock and accounting performance, indicating that during the pre-treatment period forced CEO turnover is less sensitive to firm performance when the CEO sits on the nominating than when the CEO does not. Hence, Table 6 shows that CEO involvement in the director nominating process weakens board monitoring.

**4.5. Distance from the board independence requirement**

In our sample, firms noncompliant with the board independence requirement vary in their distance from meeting this requirement prior to the rule change. Intuitively, we expect noncompliant firms forced to implement greater board changes to exhibit larger gains in sensitivity of CEO forced turnover to performance. To test this proposition, we divide our sample of noncompliant firms into two subsamples based on whether the proportion of independent directors on the board is above or below the sample median of 0.43 in 2001. For each subsample of noncompliant firms, we match them with compliant firms using the same propensity score matching procedure used in columns 1-2 of Table 4. Then, we estimate equation (1) within each matched subsample. Columns 1 and 2 of Table 7 show results for noncompliant firms having a lower fraction of independent directors, while in columns 3 and 4 of Table 7 show results for noncompliant firms having a larger fraction. The coefficient of the triple interaction term that captures the board independence treatment effect is negative in all the columns. However, the coefficient estimates are only statistically significant in the

subsample of noncompliant firms with a lower fraction of independent directors. Also, the coefficient estimates in columns 1 and 2 are larger in absolute value than those in columns 3 and 4.

## 5. Robustness Analysis

### 5.1. Statistical significance based on placebo tests

Bertrand, Duflo, and Mullainathan (2004) show that DiD regressions can over reject the null hypothesis when long time series are used and observations within each unit are serially correlated. To address the problem, we follow Bertrand et al. (2004) and estimate the distribution of t-statistics of the triple interaction coefficient of *Post*, *Treat* and *Performance* in columns 1 through 4 in Table 4 from 5,000 simulated pseudo regulatory shocks to board and nominating committee independence. We then assess the statistical significance of the original coefficient estimates by locating their t-statistics in the distribution of the simualted t-statistics in Figure 3. We observe that due to mild asymmetry in the distribution of simulated t-statistics, the statistical significance of our coefficient estimates in Table 4 for the board independence rule improve. The original t-statistic of the coefficient of the triple interaction of *Post*, *Treat* and *Performance* in column 1 of Table 4 is located deep inside the 2.5% left tail of the simulated distribution, while the orignal t-statistic of the same coefficient in column 2 of Table 4 is now located almost on the border of the 2.5% left tail. For the nominating committee independence rule, the original t-statistics are also deep in the 2.5% left tails, whether firm performance is measured by market-adjusted stock return or industry-adjusted change in EBIT.

### 5.2. Nonlinear treatment effects

Equation (1) assumes the turnover-performance relation and the effect of board structure changes on turnover-performance sensitivity are the same across all firm performance levels. Yet, there is no compelling economic reason to believe this should be the case.[24] In this section, we examine the firm performance regions where changes in board and nominating committee independence exhibit the most significant effects.

[24] Warner, Watts, & Wruck (1988) find regressing firm performance on CEO turnover in a logit model does not capture the relation well, although it does better at extreme performance levels. Jenter and Lewellen (2010) note that economic theory does not predict corporate governance has the same effect on turnover-performance sensitivity across all performance levels.

According to the Hermalin and Weisbach (1998) model, firms with an independent board use higher minimum thresholds of estimated CEO ability in making their retention decisions than firms with a non-independent board. One implication of this model is that if firm performance falls below a certain minimum level, a board's assessment of CEO ability can even fall below the retention threshold of firms with a non-independent board, so that both board types fire their CEOs. Thus, a rise in board independence has no effect on CEO turnover in this region. On the other hand, when firm performance is above a certain high level, estimated CEO ability exceeds the retention threshold of firms with an independent board, so that neither board type replaces its CEO. A rise in board independence again has no effect on CEO turnover in this range. In between, independent boards fire the CEO, but non-independent boards do not. In the intermediate region, a rise in board independence strengthens the CEO turnover-performance relation. While framed in terms of board independence, this discussion also applies to nominating committee independence.

Empirically it is unclear where to draw the two boundaries of the region over which firms with an independent board fire the CEO, but firms with a non-independent board do not. We begin by using quintile measures of firm performance. Q1 indicates the bottom quintile of firm performance and Q5 the top quintile. The mean (median) market-adjusted stock return for Q1 is -.45 (-.41) and Q2 is -.17 (-.16), and the mean (median) industry-adjusted changes in EBIT for Q1 is -.062 (-.046) and Q2 is -.011 (-.010). We then modify equation (1) to allow the treatment effect on the CEO turnover-performance relation to vary with performance quintiles. Using this new specification, we re-estimate the treatment effects in column 1 - 4 of Table 4 and report them in columns 1 - 4 of Table 8. We only report results for the bottom two performance quintiles since the treatment effects in the other quintiles are statistically insignificant. The coefficients of the triple interaction of *Post*, *Treat* and *Q1* are positive and statistically significant at conventional levels in all four columns, suggesting that the adoptions of board and nominating committee independence rules lead to a rise in the likelihood of forced CEO turnover in the bottom quintile of firm performance. However, the coefficient of the triple interaction of *Post*, *Treat* and *Q2* is statistically insignificant in all four columns. Hence, the upper boundary of the intermediate performance region over which changes in board or nominating

committee independence make a difference to the board's CEO retention decision appears to be between the bottom two quintiles of performance.

To further locate the lower boundary of this intermediate performance region, we repeat the analysis above using firm performance deciles instead of quintiles, where Q1 represents the lowest performance decile. The mean (median) market-adjusted stock return for Q1 is -.56 (-.54) and Q2 is -.34 (-.33), while the mean (median) changes in EBIT for Q1 is -.092 (-.077) and Q2 is .032 (-.031) respectively. If the lower boundary is between the bottom two deciles, we expect to find more significant treatment effects in Q2 than in Q1. Columns 5 - 8 of Table 8 show the results. We find that the triple interaction coefficients of *Post*, *Treat* and *Q1* and *Q2* are positive and statistically significant at the 10% level or better in one-sided tests in all four columns, consistent with the results in columns 1-4 of Table 8. However, only in column 5, is the coefficient of the triple interaction more significant in Q2 than in Q1. Column 5 captures the board independence rule effect when firm performance is measured by market-adjusted stock returns. Hence, the lower boundary of the intermediate performance region where board or nominating committee independence can strengthen board oversight generally appears to be well within the bottom decile of firm performance.

Overall, the evidence in Table 8 suggests that the treatment effects of the board and nominating committee independence rules are nonlinear in firm performance. The rise in monitoring intensity due to the two board independence rules mainly occurs in the bottom firm performance quintile.[25]

**5.3. Marginal effects from logit model estimates**

Until now, we have presented results based on linear probability models. Although we do not choose the non-linear model as our base model for reasons we have discussed, model choice should not drive the main results, though some discrepancies between the estimates from the two types of models are expected. In this section, we check if this is the true.

We estimate logit models that are similar to the linear probability models (LPMs) in columns 1-4 of Table 4, but we replace firm fixed effects by Fama and French 48 industry fixed effects. Including firm fixed effects can lead to inconsistent coefficient estimates due to an incidental parameters

[25] This evidence is consistent with Jenter and Lewellen (2010) who find that difference in rate of CEO turnover between firms with high and low board quality is most significant in the bottom quintile of firm performance.

problem. We employ matched samples of treatment and control firms defined by the board and nominating committee independence rules respectively. Unlike linear models, the coefficient estimate of the triple interaction term in a logit models does not equal to its marginal effect. Therefore, after estimating the model coefficients, we calculate the marginal effects in two steps. First, we calculate the derivative of the likelihood of forced CEO turnover with respect to firm performance by treatment and control firms in the pre- and post-treatment periods, while setting all the other firm and CEO characteristics at their sample means. Then, we calculate the difference in the derivatives between the pre- and the post-treatment periods within treatment and control firms and finally take the difference in the two differences. This last difference is our measure of the marginal effects. Its statistical significance is assessed using the delta method.

We find that the marginal effects for the board and nominating committee independence rules have similar magnitudes to the linear probability estimates and are all statistically significant at the 1% level. For example, for the board independence rule, our logit estimate indicates that the probability of forced CEO turnover rises by an additional 6% (for a 100% fall in market-adjusted stock return) in the post-treatment period compared to pre-treatment period in treatment firms relative to control firms. The corresponding linear estimate is 5.56%. To save space, the coefficient estimates and the marginal effects are reported in the Internet Appendix.

**5.4. Further robustness**

In tables reported in the Internet Appendix, we conduct a battery of tests to address other concerns about the robustness and interpretation of our main results. First, we examine whether our results simply capture a substitution between turnover-performance sensitivity and pay-performance sensitivity, in which case, our results may not represent an increase in the overall level of board monitoring. Second, we test the robustness of our main results to several alternative propensity score matching specifications. In the simplest case, we only match firms on size and industry. Third, we investigate whether our results are driven by misclassification of some compliant firms as noncompliant due to RiskMetrics' stricter definition of independence. This should bias our results against finding significant differences in treatment and control firms. But, if a significant number of

noncompliant firms are misclassified and our results are mainly driven by these misclassifications, then our conclusions could be false.

Lastly, we investigates whether inclusion of "controlled companies" affect our results since exchange listing rules exempt controlled companies from board and nominating committee independence requirements. A controlled company is defined as having an individual, a group or another firm that holds over 50% of its voting rights. We find that none of these sensitivity tests alters our major conclusions about how either board or nominating committee independence affect CEO forced turnover sensitivity to performance. More complete details of these tests are available in the Internet Appendix (sections B-E and table I-4 through I-7).

**6. Conclusion**

Although regulators and institutional investors have pressed for more representation by independent directors on corporate boards as a way to improve board oversight, theoretical support for the effectiveness of such measures is ambiguous and empirical evidence is mixed and inconclusive. Boards also usually do most of their work in committees. Yet, our understanding of how independence of major board committees, especially the nominating committee, affects board monitoring is still very limited. Past studies of corporate boards typically ignore the nominating committee's level of independence when studying board independence. However, we find that firms with an insider-dominated board almost always lack an independent nominating committee. Given the importance of the board nominating process for attaining actual director independence and improving board incentives to monitor, an important question needs to be asked. To what extent is the existing evidence on board independence driven by the level of nominating committee independence and are both necessary for effective board monitoring? We use changes in board composition required under the 2003 NYSE and Nasdaq listing rules as a source of exogenous variation to address both questions.

Our main finding is that board and nominating committee independence *causes* more careful board monitoring. Specifically, firms that move to an independent board following the adoption of new exchange listing rules experience greater increases in sensitivity of forced CEO turnover to performance. The same is true for firms that move to a fully independent nominating committee. The

board and nominating committee independence rules have distinct, but complementary effects on board monitoring, in the sense that, firms moving to an independent nominating committee have greater increases in forced CEO turnover to firm performance sensitivity, even though they previously met the board independence requirement. For firms changing both board and nominating independence, after controlling for the effects of a change in nominating committee independence, we find that moving to an independent board still leads to greater forced CEO turnover sensitivity to firm performance. Finally, in the cross-section, we find changes in sensitivity of forced CEO turnover to firm performance are higher in noncompliant firms which prior to the new rules have less independent boards and which have the CEO on the nominating committee.

To our knowledge, we are the first study to document a relation between a fully independent nominating committee and forced CEO turnover. This evidence suggest that corporate governance reforms can achieve greater improvement in board monitoring if they target both independence of the overall board and full independence of the nominating committee.

Depending on what determines board structure prior to the issuance of the new exchange listing requirements, the increased intensity of board monitoring we document can have different implications for shareholder wealth. From an agency viewpoint, the composition of corporate boards is unduly influenced by the CEO and the main problem with corporate boards is that they are an ineffective internal control mechanism. Thus, the exogenous shock we study should benefit shareholders. However, from an optimal contracting viewpoint, the composition of corporate boards is optimally determined by a tradeoff between the monitoring and advising needs of a firm. Under this hypothesis, although adding independent directors to the board or nominating commitee raises the intensity of board monitoring, it may still be suboptimal, since the board can lose valuable advisory services for example, which could on net hurt firm value and shareholder wealth.

The board of directors litearture has yet to reach an conclusion on whether the observed board structure prior to this mandatory change in board composition was optimal or suboptimal. However, using the same natural experiment, Duchin et al (2010) finds that board independence improves firm performance for firms with low information costs, but reduces firm performance for firms with high

information costs. Using a very different approach based on the supply of local director talent, Knyazeva, Knyazeva and Masulis (2012) find that making boards independent improves firm performance. Combining these results with our findings suggest that strengthening a board's monitoring effectiveness through regulatory changes can on average benefit shareholders, and particularly those in firms with relatively low information costs. Future research should take into account, not only the degree of overall board independence, but also the level of independence on key board committees. Ideally, this assessment of director independence should also account for various types of director social connections to a firm's CEO.

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Figure 1: Changes in board structure from 1996 to 2009 of sample firms

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 2005. The data source is the RiskMetrics database. The top panel shows the percentage of sample firms that have majority of independent directors on board (ind_bod), fully independent nominating (ind_nom), audit (ind_aud) and compensation (ind_com) committees respectively from 1998 to 2009. The bottom panel shows the mean percentage of independent directors on the board from 1996 to 2009 for treatment firms and control firms defined by whether the firm had a majority of independent directors on the board in 2001. Treatment firms do not have a majority of independent directors on the board in 2001.

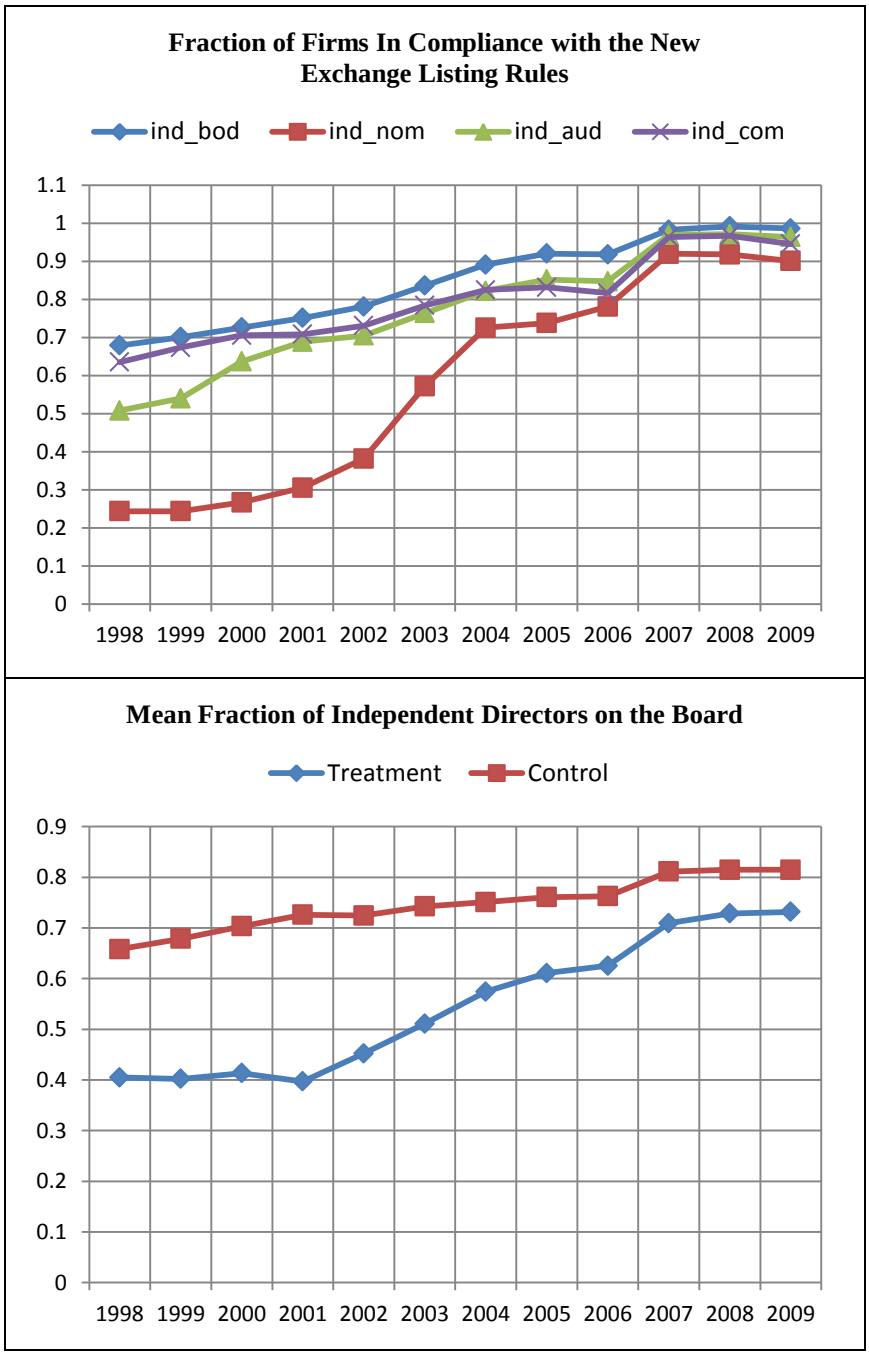

Figure 2: Distribution of t-statistics from 5000 simulations of pseudo regulatory shocks.

This figure shows the histograms of the t-statistics of the coefficient of Post × Treat × Performance in Table 4 from 5000 simulations of pseudo regulatory shocks to board or nominating committee independence. The top two graphs show the histograms for the board independence rule, while the bottom two graphs show the histograms for the nominating committee independence rule. Kernel density estimates are imposed on top of the histograms. The solid line marks the location of the original t-statistics in Table 4. The long dashed lines mark the locations of the simulated t-statistics for 5% statistical significance in two-sided tests, while the short dashed lines mark the locations of the simulated t-statistics for 10% statistical significance in two-sided tests.

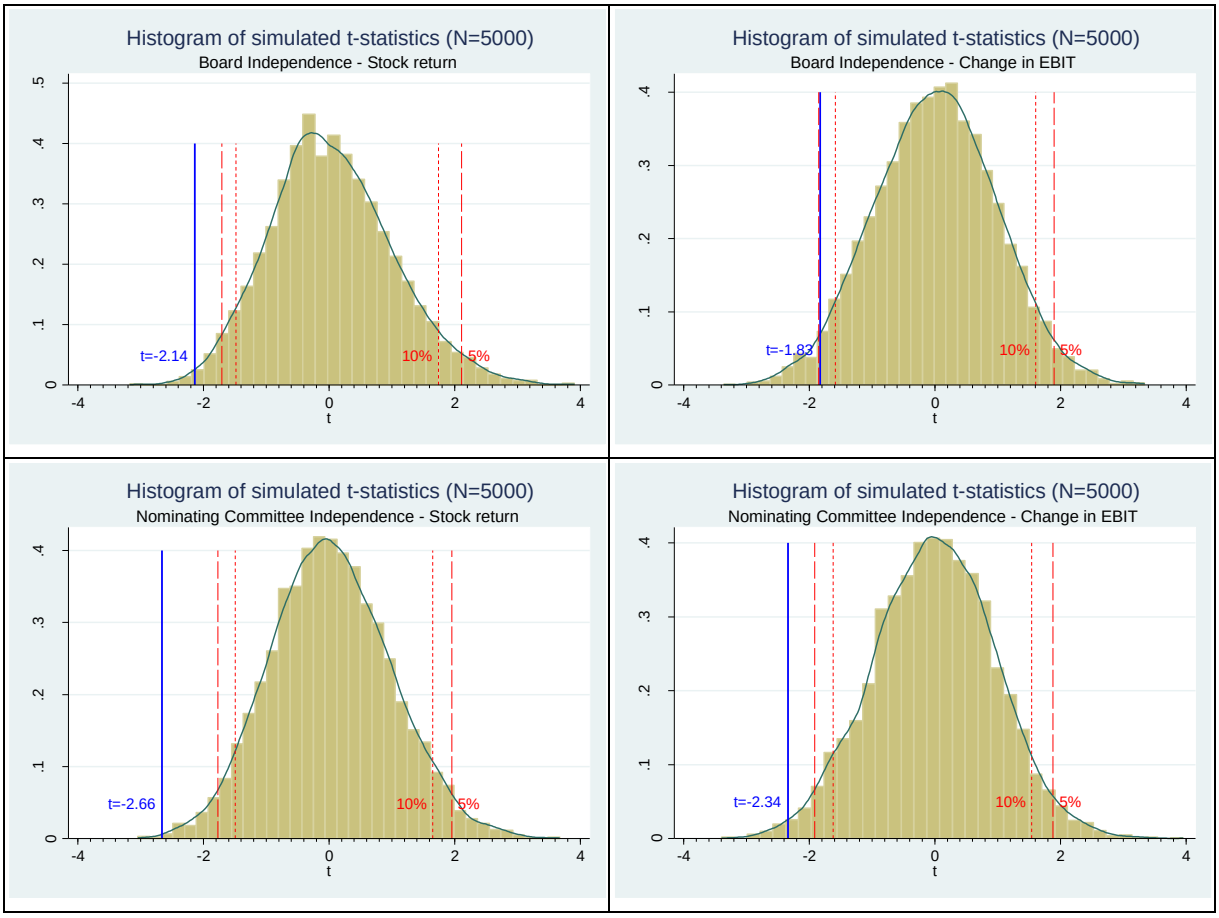

Table 1: Summary Statistics

Panel A reports summary statistics for the entire sample, which consists of all firm-year observations from 1996 to 2009 of the 1,231 firms that have board data available on RiskMetrics in 2001 and have survived from 2001 to at least 2005 on the ExecuComp database. Panel B compare firms that are and are not in compliance with the new exchange listing rules that require listed firms to have a majority of independent directors on the board and a fully independent nominating committee in year 2001, respectively. Panel C compare firms that are and are not in compliance with the new exchange listing rules that require listed firms to have a fully independent audit committee and compensation committee in 2001, respectively. All variables are defined in Appendix 1. The symbols ***, **, * represent statistical significance at the 1%, 5% and 10% levels for difference in means between the treatment and control firms.

Panel A: All firm-years (1996-2009)

| Variables | Obs | Mean | Median | SD |
|---|---|---|---|---|
| Total assets ($ mil) | 16092 | 17034.64 | 2054.77 | 87114.27 |
| Sales ($ mil) | 16086 | 6284.49 | 1597.98 | 17989.36 |
| Market capitalization ($ mil) | 15383 | 8439.53 | 1795.58 | 25176.89 |
| Book leverage | 16035 | 0.23 | 0.22 | 0.20 |
| Market-to-book | 15964 | 2.01 | 1.46 | 2.45 |
| Stock return volatility | 15908 | 0.117 | 0.098 | 0.079 |
| Market-adjusted stock return | 15924 | 0.0893 | -0.0004 | 0.6719 |
| Industry-adjusted change in EBIT | 15836 | 0.0045 | 0.0010 | 0.2152 |
| Fraction of Independent directors | 13614 | 0.68 | 0.71 | 0.17 |
| Board size | 13614 | 9.8 | 9 | 2.8 |
| Nonemployee blockholder | 13614 | 0.25 | 0 | 0.44 |
| CEO-Chairman | 13287 | 0.66 | 1 | 0.47 |
| E-index | 13861 | 2.25 | 2 | 1.47 |
| Percentage of CEO voting control (%) | 11458 | 3.36 | 0 | 9.39 |
| CEO tenure (year) | 16116 | 5.2 | 5 | 3.6 |
| CEO age | 15838 | 55 | 56 | 7 |
| Founder-Heir CEO | 16116 | 0.078 | 0 | 0.269 |
| Fraction of all CEO turnovers | 16116 | 0.103 | 0 | 0.305 |
| Fraction of forced CEO turnovers | 16116 | 0.0252 | 0 | 0.157 |
| Total compensation ($000s) | 15386 | 5692.89 | 2972.24 | 12485.5 |
| Equity to nonequity Pay | 15344 | 1.70 | 0.84 | 2.84 |
| Delta ($000s) | 14316 | 881.56 | 259.04 | 1984.74 |

Panel B: Comparison of treatment and control firms in 2001

| | Board Independence | | t-statistics | Nominating Committee Independence | | t-statistics |
|---|---|---|---|---|---|---|
| | Treatment | Control | (2)-(1) | Treatment | Control | (4)-(3) |
| | (1) | (2) | | (3) | (4) | |
| Total assets ($ mil) | 14146.40 | 14746.04 | 0.13 | 11696.22 | 21186.61 | 2.02 |
| Sales ($ mil) | 4589.72 | 6061.86 | 1.41 | 4404.61 | 8631.77 | 3.86 |
| Market capitalization ($ mil) | 7764.40 | 8338.93 | 0.27 | 6348.33 | 12505.84 | 3.07 |
| Market-to-book | 2.06 | 1.90 | -1.79 | 2.02 | 1.77 | -3.19 |
| Stock return volatility | 0.159 | 0.139 | -3.42 | 0.152 | 0.127 | -6.34 |
| E-index | 1.67 | 2.41 | 7.72 | 2.09 | 2.54 | 5.47 |
| Independent directors | 0.40 | 0.73 | 48.72 | 0.59 | 0.77 | 20.15 |
| CEO-Chairman | 0.61 | 0.72 | 3.30 | 0.66 | 0.77 | 3.90 |
| Nonemployee blockholder | 0.47 | 0.25 | -6.77 | 0.37 | 0.17 | -7.60 |
| Total compensation ($000s) | 5171.627 | 7077.778 | 2.15 | 6647.891 | 6537.621 | -0.14 |
| Equity-to-nonequity Pay | 2.09 | 2.64 | 2.11 | 2.60 | 2.29 | -1.42 |
| Delta | 1293.149 | 869.7862 | -2.58 | 1061.685 | 772.8177 | -2.27 |
| Number of firms | 306 | 925 | | 855 | 376 | |

Panel C: Comparison of treatment and control firms in 2001

| | Audit Committee Independence | | t-statistics | Compensation Committee Independence | | t-statistics |
|---|---|---|---|---|---|---|
| | Treatment | Control | (2)-(1) | Treatment | Control | (4)-(3) |
| | (1) | (2) | | (3) | (4) | |
| Total assets ($ mil) | 20873.60 | 11770.07 | -1.90 | 9692.45 | 16619.00 | 2.26 |
| Sales ($ mil) | 7207.99 | 5016.08 | -2.08 | 4602.55 | 6147.84 | 1.75 |
| Market capitalization ($ mil) | 11904.30 | 6527.79 | -2.58 | 6574.08 | 8849.38 | 1.33 |
| Market-to-book | 1.97 | 1.93 | -0.51 | 1.96 | 1.93 | -0.35 |
| Stock return volatility | 0.143 | 0.145 | 0.38 | 0.146 | 0.144 | -0.42 |
| E-index | 2.10 | 2.30 | 2.34 | 1.95 | 2.35 | 4.48 |
| Independent directors | 0.53 | 0.70 | 15.72 | 0.51 | 0.70 | 18.08 |
| CEO-Chairman | 0.68 | 0.70 | 0.54 | 0.65 | 0.71 | 1.94 |
| Nonemployee blockholder | 0.40 | 0.26 | -4.79 | 0.42 | 0.26 | -5.30 |
| Total compensation ($000s) | 8988.388 | 5553.641 | -2.49 | 6018.983 | 6853.453 | 0.91 |
| Equity-to-nonequity Pay | 2.70 | 2.42 | -1.10 | 2.23 | 2.62 | 1.57 |
| Delta | 1396.340 | 779.913 | -3.70 | 1130.052 | 909.087 | -1.48 |
| Number of firms | 383 | 848 | | 359 | 872 | |

Table 2: Distribution of firms by compliance status with the new exchange listing rules in year 2001

This table reports the number (first row), row percentage (parenthesis) and column percentage (bracket) of sample firms by compliance status with the four new exchange listing rules in year 2001. The sample consists of 1,231 firms that have board data available on RiskMetrics in 2001 and have survived from 2001 through 2005 on the ExecuComp database.

| Independence of | | Nominating Committee | | Audit Committee | | Compensation Committee | | Total |
|---|---|---|---|---|---|---|---|---|
| | Compliant? | Yes | No | Yes | No | Yes | No | |
| Board | Yes | 361 | 564 | 723 | 202 | 754 | 171 | 925 |
| | | (39%) | (61%) | (78%) | (22%) | (82%) | (18%) | |
| | | [96%] | [66%] | [85%] | [53%] | [86%] | [48%] | [75%] |
| | No | 15 | 291 | 125 | 181 | 118 | 188 | 306 |
| | | (5%) | (95%) | (41%) | (59%) | (39%) | (61%) | |
| | | [4%] | [34%] | [15%] | [47%] | [14%] | [52%] | [25%] |
| Nominating Committee | Yes | … | … | 317 | 59 | 338 | 38 | 376 |
| | | | | (84%) | (16%) | (90%) | (10%) | |
| | | | | [37%] | [15%] | [39%] | [11%] | [31%] |
| | No | … | … | 531 | 324 | 534 | 321 | 855 |
| | | | | (62%) | (38%) | (62%) | (38%) | |
| | | | | [63%] | [85%] | [61%] | [89%] | [69%] |
| Audit Committee | Yes | … | … | … | … | 675 | 173 | 848 |
| | | | | | | (80%) | (20%) | |
| | | | | | | [77%] | [48%] | [69%] |
| | No | … | … | … | … | 197 | 186 | 383 |
| | | | | | | (51%) | (49%) | |
| | | | | | | [23%] | [52%] | [31%] |
| Total | | 376 | 855 | 848 | 383 | 872 | 359 | 1,231 |
| | | (31%) | (69%) | (69%) | (31%) | (71%) | (29%) | 100% |

Table 3: Rate of forced CEO turnover by performance terciles

This table reports the rate of forced CEO turnover in treatment and control firms in the pre- and post-treatment periods by firm performance terciles. We exclude forced CEO turnovers that resulted from punishment for violations because they could introduce bias into the estimates. Panel A is based on the matched sample of treatment and control firms defined by the board independence rule, while Panel B is based on the matched sample of treatment and control firms defined by the nominating committee independence rule. Stock performance is measured by market-adjusted stock return over the 12-month period that ends one month before the CEO turnover announcement, while accounting performance is measured by industry-adjusted change in EBIT in the year prior to the CEO turnover announcement. Performance terciles are calculated based on all firm-years in the respective sample. *Pre* and *Post* indicate the pre- and post-treatment periods. ***, **, and * denote significance at the 1, 5 and 10 percent levels, respectively, for tests of difference in means

Panel A: Board Independence Rule

| | Stock Return | | | ΔEBIT | | |
|---|---|---|---|---|---|---|
| | Low | Middle | High | Low | Middle | High |
| Control firms: | | | | | | |
| Pre | 5.43% | 2.30% | 1.15% | 4.81% | 1.89% | 2.09% |
| Post | 4.66% | 1.32% | 1.20% | 4.19% | 1.04% | 2.12% |
| Diff1 | -0.77% | -0.98% | 0.05% | -0.62% | -0.86% | 0.03% |
| | | | | | | |
| Treatment firms: | | | | | | |
| Pre | 3.48% | 1.87% | 0.28% | 3.17% | 1.05% | 1.21% |
| Post | 8.50% | 1.17% | 0.60% | 7.26% | 2.25% | 0.98% |
| Diff2 | 5.02%*** | -0.70% | 0.31% | 4.09%*** | 1.20% | -0.23% |
| | | | | | | |
| Diff2-Diff1 | 5.79%*** | 0.28% | 0.26% | 4.71%*** | 2.06%* | -0.26% |

Panel B: Nominating Committee Independence Rule

| | Stock Return | | | ΔEBIT | | |
|---|---|---|---|---|---|---|
| | Low | Middle | High | Low | Middle | High |
| Control firms | | | | | | |
| Pre | 6.16% | 1.98% | 1.84% | 5.45% | 1.86% | 2.53% |
| Post | 3.57% | 1.54% | 0.74% | 3.94% | 0.70% | 2.21% |
| Diff1 | -2.58%** | -0.44% | -1.10% | -1.51% | -1.16% | -0.32% |
| | | | | | | |
| Treatment firms | | | | | | |
| Pre | 4.53% | 2.08% | 1.14% | 4.32% | 1.68% | 1.66% |
| Post | 6.39% | 1.05% | 0.59% | 4.83% | 1.77% | 1.76% |
| Diff2 | 1.87%* | -1.04%* | -0.55% | 0.50% | 0.08% | 0.10% |
| | | | | | | |
| Diff2-Diff1 | 4.45%*** | -0.60% | 0.55% | 2.02% | 1.25% | 0.41% |

Table 4: The effect of the new exchange listing rules on turnover-performance sensitivity

This table reports coefficient estimates from OLS regressions where the dependent variable is an indicator for forced CEO turnover in year t. We exclude forced CEO turnovers that resulted from punishment for violations because they could introduce bias into the estimates. Each column is estimated using the matched sample of treatment and control firms constructed for the particular new listing rule indicated at the top of the column. *Treat* and *Post* are the treatment and post-treatment period indicators for the rule targeting the board structure shown in top of the column. *Treat* equals one if the firm is noncompliant with the specific board structure rule at the year-end of 2001 and zero otherwise. *Post* equals one in years 2005 and later and zero otherwise. *Performance* is measured in year t-1 by either market-adjusted stock returns or industry-adjusted change in EBIT as indicated in each column. Industries are defined by Fama and French (1997) 48 industry groups. Unreported interactions between year fixed effects and *Performance* are included in all the columns. Each column also includes CEO tenure (in years), firm and year fixed effects. All other independent variables are defined in Appendix 1 and measured in year t-1. Standard errors are clustered at the firm level. T-statistics for two-sided tests are in parentheses and ***, **, and * denote significance at the 1, 5 and 10 percent levels, respectively.

| | Board | | Nominating Committee | | Audit Committee | | Compensation Committee | |
|---|---|---|---|---|---|---|---|---|
| | Stock Return | ΔEBIT | Stock Return | ΔEBIT | Stock Return | ΔEBIT | Stock Return | ΔEBIT |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Post × Treat | 0.0269** | 0.0270** | 0.0160* | 0.0105 | 0.00400 | 0.00475 | 0.0164* | 0.0166* |
| | (2.54) | (2.55) | (1.76) | (1.30) | (0.52) | (0.64) | (1.76) | (1.93) |
| Treat × Performance | 0.0100 | 0.191 | 0.0318*** | 0.350** | -0.00164 | 0.108 | 0.00190 | 0.208* |
| | (0.95) | (1.35) | (2.60) | (2.26) | (-0.15) | (0.78) | (0.17) | (1.66) |
| Post × Treat × Performance | -0.0556** | -0.525* | -0.0496*** | -0.557** | -0.00821 | 0.0175 | -0.0146 | -0.333 |
| | (-2.14) | (-1.83) | (-2.66) | (-2.34) | (-0.41) | (0.07) | (-0.75) | (-1.35) |
| Log of sales | 0.00615 | 0.0122* | 0.0115** | 0.0187*** | 0.00746 | 0.0126* | 0.00890 | 0.0144** |
| | (0.91) | (1.84) | (2.07) | (3.23) | (1.12) | (1.93) | (1.34) | (2.23) |
| Stock volatility | 0.167*** | 0.184*** | 0.200*** | 0.198*** | 0.157*** | 0.182*** | 0.170*** | 0.188*** |
| | (3.14) | (3.46) | (3.42) | (3.42) | (2.85) | (3.27) | (2.93) | (3.29) |
| CEO-Chairman | -0.0122* | -0.0128* | -0.0120 | -0.0138* | -0.0119* | -0.0121* | -0.0112 | -0.0116 |
| | (-1.73) | (-1.80) | (-1.64) | (-1.86) | (-1.70) | (-1.71) | (-1.59) | (-1.64) |
| CEO voting power | -0.000783* | -0.000885* | -0.000907* | -0.00100* | -0.000760** | -0.000836** | -0.000641* | -0.000726* |
| | (-1.77) | (-1.90) | (-1.77) | (-1.85) | (-2.14) | (-2.26) | (-1.77) | (-1.89) |
| CEO age | -0.00143** | -0.00147** | -0.00183*** | -0.00194*** | -0.00147** | -0.00147** | -0.00171*** | -0.00178*** |
| | (-2.16) | (-2.15) | (-2.62) | (-2.69) | (-2.28) | (-2.17) | (-2.67) | (-2.67) |
| Founder-Heir CEO | -0.0459*** | -0.0462*** | -0.0483*** | -0.0482*** | -0.0498*** | -0.0521*** | -0.0388** | -0.0405** |
| | (-2.85) | (-2.81) | (-2.71) | (-2.66) | (-3.19) | (-3.26) | (-2.36) | (-2.44) |
| Nonemployee blockholders | 0.0122* | 0.0123 | 0.0134 | 0.0133 | 0.0119* | 0.0119 | 0.00958 | 0.00967 |
| | (1.65) | (1.61) | (1.63) | (1.58) | (1.66) | (1.61) | (1.31) | (1.28) |
| Constant | 0.0116 | -0.0317 | -0.00125 | -0.0555 | 0.00644 | -0.0328 | 0.00760 | -0.0297 |
| | (0.19) | (-0.51) | (-0.02) | (-0.94) | (0.10) | (-0.53) | (0.12) | (-0.49) |
| Observations | 8756 | 8718 | 8403 | 8370 | 8807 | 8767 | 8732 | 8696 |

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| Adj. $R^2$ | 0.052 | 0.048 | 0.054 | 0.051 | 0.051 | 0.046 | 0.051 | 0.049 |

Table 5: Separating the treatment effect of the board and nominating committee independence rules

This table reports coefficient estimates from OLS regressions where the dependent variable is an indicator for forced CEO turnover in year t. We exclude forced CEO turnovers that resulted from punishment for violations because poor firm performance is not the main reason for these turnovers. Columns 1 - 4 are estimated using the matched sample of treatment and control firms defined by the board independence rule. Columns 5 and 6 are estimated with the same matched sample, but excluding treatment firms that implement small increases in board independence from before to after the treatment and their matched control firms. A small increase in board independence is defined as an increase in percentage of independent directors on the board below 11%, which is the 25th percentile of percentage gains in independent directors among treatment firms. Columns 7 and 8 are estimated using a matched sample of treatment and control firms selected from firms that already have a majority of independent directors on the board in year 2001 and thus are not affected by the board independence rule. *TreatBod* and *TreatNom* are the treatment indicators for the rule requiring an independent board and a fully independent nominating committee respectively. In columns 1, 2, 5 and 6, *Post* equals one in years 2005 and later and zero otherwise. For noncompliant firms in columns 3 and 4, *Post* is defined for each rule and it changes from zero to one in the actual year the firm become compliant with that rule. For compliant firms in columns 3 and 4, *Post* equals one for years 2004 and later and zero otherwise. *Performance* is measured in year t-1 by either market-adjusted stock returns or industry-adjusted change in EBIT as indicated in each column. Industries are defined by Fama and French (1997) 48 industry groups. Unreported interactions between year fixed effects and *Performance* are included in all the columns. Each column also includes CEO tenure (in years), firm and year fixed effects. All other independent variables are defined in Appendix 1 and measured in year t-1. Standard errors are clustered at the firm level. T-statistics for two-sided tests are in parentheses and ***, **, and * denote significance at the 1, 5 and 10 percent levels, respectively.

| | 2005 Cut-off | | Rule Specific Cut-off | | No Small Increases | | Nominating Committee | |
|---|---|---|---|---|---|---|---|---|
| | Stock Return | ΔEBIT | Stock Return | ΔEBIT | Stock Return | ΔEBIT | Stock Return | ΔEBIT |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Post × TreatBod | 0.0226** | 0.0235** | 0.0184* | 0.0169* | 0.0304** | 0.0293** | | |
| | (2.02) | (2.11) | (1.76) | (1.71) | (2.31) | (2.27) | | |
| TreatBod × Performance | -0.00519 | 0.106 | 0.000740 | 0.143 | 0.00484 | 0.0781 | | |
| | (-0.48) | (0.76) | (0.07) | (1.05) | (0.43) | (0.48) | | |
| Post × TreatBod × Performance | -0.0380 | -0.416 | -0.0578** | -0.482* | -0.0691* | -0.665* | | |
| | (-1.42) | (-1.39) | (-2.35) | (-1.89) | (-1.87) | (-1.73) | | |
| Post × TreatNom | 0.0126 | 0.0104 | 0.00714 | 0.00526 | 0.0117 | 0.00954 | 0.0106 | 0.00845 |
| | (1.48) | (1.27) | (0.95) | (0.73) | (1.33) | (1.12) | (1.23) | (1.02) |
| TreatNom × Performance | 0.0477*** | 0.288* | 0.0459*** | 0.325** | 0.0520*** | 0.305* | 0.0460*** | 0.369** |
| | (3.87) | (1.91) | (3.80) | (2.15) | (4.05) | (1.80) | (3.60) | (2.25) |
| Post × TreatNom × Performance | -0.0553*** | -0.360 | -0.0366* | -0.419** | -0.0635*** | -0.394 | -0.0598*** | -0.442* |
| | (-2.98) | (-1.48) | (-1.92) | (-2.03) | (-3.24) | (-1.50) | (-3.19) | (-1.79) |
| Log of sales | 0.00608 | 0.0125* | 0.00617 | 0.0129* | 0.00490 | 0.0118 | 0.00655 | 0.0118 |
| | (0.92) | (1.91) | (0.93) | (1.96) | (0.67) | (1.63) | (0.82) | (1.50) |
| Stock volatility | 0.171*** | 0.186*** | 0.172*** | 0.186*** | 0.171*** | 0.186*** | 0.176*** | 0.193*** |
| | (3.22) | (3.48) | (3.23) | (3.49) | (3.04) | (3.27) | (2.90) | (3.16) |
| CEO-Chairman | -0.0122* | -0.0131* | -0.0120* | -0.0131* | -0.0122* | -0.0133* | -0.0108 | -0.0114 |

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | (-1.73) | (-1.84) | (-1.70) | (-1.85) | (-1.67) | (-1.80) | (-1.44) | (-1.49) |
| CEO voting power | -0.000784* | -0.000863* | -0.000800* | -0.000873* | -0.00110* | -0.00120* | -0.000972 | -0.00109 |
| | (-1.78) | (-1.85) | (-1.80) | (-1.86) | (-1.87) | (-1.92) | (-1.57) | (-1.62) |
| CEO age | -0.00139** | -0.00147** | -0.00141** | -0.00149** | -0.00135* | -0.00136* | -0.00258*** | -0.00269*** |
| | (-2.10) | (-2.17) | (-2.13) | (-2.20) | (-1.91) | (-1.86) | (-3.30) | (-3.38) |
| Founder-Heir CEO | -0.0443*** | -0.0456*** | -0.0445*** | -0.0468*** | -0.0426** | -0.0438** | -0.0497*** | -0.0533*** |
| | (-2.73) | (-2.77) | (-2.75) | (-2.84) | (-2.41) | (-2.44) | (-2.79) | (-2.89) |
| Nonemployee blockholders | 0.0122* | 0.0122 | 0.0120 | 0.0121 | 0.0112 | 0.0111 | 0.0133 | 0.0129 |
| | (1.66) | (1.60) | (1.63) | (1.60) | (1.40) | (1.35) | (1.53) | (1.43) |
| Constant | 0.00900 | -0.0352 | 0.00949 | -0.0367 | 0.0171 | -0.0343 | 0.0696 | 0.0358 |
| | (0.15) | (-0.57) | (0.16) | (-0.59) | (0.25) | (-0.51) | (0.92) | (0.48) |
| Observations | 8756 | 8718 | 8756 | 8718 | 7874 | 7836 | 7075 | 7039 |
| Adj. $R^2$ | 0.054 | 0.049 | 0.054 | 0.050 | 0.053 | 0.049 | 0.059 | 0.056 |

Table 6: CEOs on the nominating committee and the nominating committee independence effect

This table reports coefficient estimates from OLS regressions where the dependent variable is an indicator for forced CEO turnover in year t. We exclude forced CEO turnovers that resulted from punishment for violations because poor firm performance is not the main reason for these turnovers. The sample consists of firms that do not have a fully independent nominating committee in year 2001. These firms differ in whether the CEO is on the nominating committee. Firms in which the CEO is on the nominating committee is matched with those in which the CEO is not on the nominating committee using a propensity score model similar to that for matching firms with and without a fully independent nominating committee in year 2001. *CEO on Nom* equals to one if the CEO is on the nominating committee in year 2001 and zero otherwise. *Post* is the post-treatment period indicators for the rule requiring a fully independent nominating committee. It equals one in years 2005 and later and zero otherwise. *Performance* is measured in year t-1 by either market-adjusted stock returns or industry-adjusted change in EBIT. Industries are defined by Fama and French (1997) 48 industry groups. Unreported interactions between year fixed effects and *Performance* are included in both columns. Each column also includes CEO tenure (in years), firm and year fixed effects. All other independent variables are defined in Appendix 1 and measured in year t-1. Standard errors are clustered at the firm level. T-statistics for two-sided tests are in parentheses and ***, **, and * denote significance at the 1, 5 and 10 percent levels, respectively.

| | Stock Return | ΔEBIT |
|---|---|---|
| | (1) | (2) |
| Post × CEO on Nom | 0.0122 | 0.0138 |
| | (1.10) | (1.24) |
| CEO on Nom × Performance | 0.0292* | 0.272 |
| | (1.70) | (1.20) |
| Post × CEO on Nom × Performance | -0.0706** | -0.545 |
| | (-2.00) | (-1.05) |
| Log of sales | 0.0140 | 0.0190* |
| | (1.52) | (1.86) |
| Stock volatility | 0.192** | 0.243** |
| | (2.06) | (2.53) |
| CEO-Chairman | -0.0214* | -0.0217** |
| | (-1.96) | (-1.97) |
| CEO voting power | -0.00161** | -0.00181** |
| | (-2.29) | (-2.19) |
| CEO age | -0.000951 | -0.00105 |
| | (-1.03) | (-1.07) |
| Founder-Heir CEO | -0.0495 | -0.0530 |
| | (-1.43) | (-1.49) |
| Nonemployee blockholders | 0.00936 | 0.0111 |
| | (0.82) | (0.93) |
| Constant | -0.0622 | -0.101 |
| | (-0.70) | (-1.08) |
| Observations | 3179 | 3151 |
| Adj. $R^2$ | 0.082 | 0.074 |

Table 7: Distance from the majority independent requirement and the board independence effect

This table reports coefficient estimates from OLS regressions where the dependent variable is an indicator for forced CEO turnover in year t. We exclude forced CEO turnovers that resulted from punishment for violations because poor firm performance is not the main reason for these turnovers. The sample is the matched sample of treatment and control firms defined by the board independence rule. We break the treatment firms into two subsamples based on the median percentage of independent directors on the board in 2001 of all treatment firms. Columns 1 and 2 (columns 3 and 4) are estimated using the treatment firms with below (above) the sample median percentage of independent directors on the board in 2001 and their matched control firms. *Treat* and *Post* are the treatment and post-treatment period indicators. *Treat* equals one if the firm has less than 50% independent directors on the board at the year-end of 2001 and zero otherwise. *Post* equals one in years 2005 and later and zero otherwise. *Performance* is measured in year t-1 by either market-adjusted stock returns or industry-adjusted change in EBIT as indicated in the header of each column. Industries are defined by Fama and French (1997) 48 industry groups. Unreported interactions between year fixed effects and *Performance* are included in all four columns. Each column also includes CEO tenure (in years), firm and year fixed effects. All other independent variables are defined in Appendix 1 and measured in year t-1. Standard errors are clustered at the firm level. T-statistics for two-sided tests are in parentheses and ***, **, and * denote significance at the 1, 5 and 10 percent levels, respectively.

| | Independence<0.43 in 2001 | | Independence>=0.43 in 2001 | |
|---|---|---|---|---|
| | Stock Return | ΔEBIT | Stock Return | ΔEBIT |
| | (1) | (2) | (3) | (4) |
| Post × Treat | 0.0260* | 0.0290* | 0.0108 | 0.0107 |
| | (1.71) | (1.85) | (0.67) | (0.69) |
| Treat × Performance | 0.0143 | 0.259 | 0.0191 | 0.177 |
| | (0.96) | (1.37) | (1.22) | (0.83) |
| Post × Treat × Performance | -0.0806** | -0.785* | -0.0272 | -0.181 |
| | (-1.99) | (-1.77) | (-0.88) | (-0.53) |
| Log of sales | 0.00712 | 0.0142* | 0.0179*** | 0.0218*** |
| | (0.81) | (1.71) | (2.71) | (3.22) |
| Stock volatility | 0.149** | 0.170** | 0.173** | 0.197*** |
| | (2.28) | (2.54) | (2.58) | (2.97) |
| CEO-Chairman | -0.0139 | -0.0147 | -0.0119 | -0.0120 |
| | (-1.52) | (-1.58) | (-1.48) | (-1.47) |
| CEO voting power | -0.000872 | -0.00102 | -0.00146 | -0.00163 |
| | (-1.38) | (-1.52) | (-1.56) | (-1.58) |
| CEO age | -0.000657 | -0.000638 | -0.00128* | -0.00130* |
| | (-0.75) | (-0.71) | (-1.71) | (-1.68) |
| Founder-Heir CEO | -0.0503*** | -0.0492** | -0.0569*** | -0.0568*** |
| | (-2.60) | (-2.50) | (-2.90) | (-2.81) |
| Nonemployee blockholders | 0.0172* | 0.0176* | 0.0114 | 0.0126 |
| | (1.73) | (1.72) | (1.24) | (1.32) |
| Constant | -0.0299 | -0.0839 | -0.0849 | -0.114* |
| | (-0.37) | (-1.07) | (-1.30) | (-1.71) |
| Observations | 5612 | 5577 | 5898 | 5863 |
| Adj. $R^2$ | 0.066 | 0.062 | 0.069 | 0.062 |

Table 8: Nonlinear treatment effects

This table reports coefficient estimates from OLS regressions where the dependent variable is an indicator for forced CEO turnover in year t. We exclude forced CEO turnovers that resulted from punishment for violations because poor firm performance is not the main reason for these turnovers. They are estimated using the same corresponding samples in columns 1 through 4 of Table 4. Firm performance is measured by indicators for quintiles or deciles of either market-adjusted stock returns or industry-adjusted change in EBIT in year t-1 as indicated at the top of each column. Industries are defined by Fama and French (1997) 48 industry groups. *Q1* and *Q2* are indicators for the bottom two quintiles of firm performance in columns 1 through 4 and bottom two deciles of firm performance in columns 5 through 8. To save space, we do not report terms beyond the bottom two quintiles or deciles of firm performance. *Treat* and *Post* are the treatment and post-treatment period indicators for the rule targeting the board structure shown in top of the column. *Treat* equals one if the firm is noncompliant with the specific board structure rule at year-end 2001 and zero otherwise. *Post* equals one in years 2005 and later and zero otherwise. Unreported interactions between year fixed effects and firm performance quintiles or deciles are included in all the columns. Each column also includes CEO tenure (in years), firm and year fixed effects. All other independent variables are defined in Appendix 1 and measured in year t-1. Standard errors are clustered at the firm level. T-statistics for two-sided tests are in parentheses and ***, **, and * denote significance at the 1, 5 and 10 percent levels, respectively.

| | Board | | Nom Com | | Board | | Nom Com | |
|---|---|---|---|---|---|---|---|---|
| | Stock Return | ΔEBIT | Stock Return | ΔEBIT | Stock Return | ΔEBIT | Stock Return | ΔEBIT |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Treat × Q1 | -0.0191 | -0.0243 | -0.0438** | -0.0193 | 0.00228 | -0.00610 | -0.0636** | -0.0592* |
| | (-1.02) | (-1.36) | (-2.53) | (-0.92) | (0.08) | (-0.18) | (-2.41) | (-1.67) |
| Treat × Q2 | -0.000793 | 0.000238 | 0.0143 | 0.0246 | -0.0217 | -0.0138 | -0.0333 | 0.00870 |
| | (-0.05) | (0.02) | (1.07) | (1.48) | (-0.84) | (-0.68) | (-1.51) | (0.32) |
| - | - | - | - | - | - | - | - | - |
| Post × Treat × Q1 | 0.104*** | 0.0809** | 0.0824*** | 0.0449* | 0.105 | 0.0965 | 0.114*** | 0.0803* |
| | (2.80) | (2.43) | (3.39) | (1.78) | (1.39) | (1.61) | (2.76) | (1.72) |
| Post × Treat × Q2 | 0.0103 | 0.0143 | -0.00741 | -0.00433 | 0.123*** | 0.0647 | 0.0625** | 0.0179 |
| | (0.42) | (0.67) | (-0.43) | (-0.28) | (2.73) | (1.57) | (2.19) | (0.60) |
| - | - | - | - | - | - | - | - | - |
| Log of sales | 0.00661 | 0.0105 | 0.0131** | 0.0171*** | 0.00673 | 0.0104 | 0.0134** | 0.0175*** |
| | (0.97) | (1.56) | (2.38) | (3.02) | (0.99) | (1.51) | (2.44) | (3.04) |
| Stock volatility | 0.140** | 0.158*** | 0.135** | 0.164*** | 0.129** | 0.154*** | 0.124** | 0.154*** |
| | (2.53) | (2.96) | (2.23) | (2.84) | (2.32) | (2.85) | (2.02) | (2.61) |
| CEO-Chairman | -0.0129* | -0.0119* | -0.0137* | -0.0125* | -0.0126* | -0.0118* | -0.0130* | -0.0125* |
| | (-1.85) | (-1.68) | (-1.88) | (-1.68) | (-1.80) | (-1.67) | (-1.80) | (-1.68) |
| CEO voting power | -0.000784* | -0.000953** | -0.000867* | -0.00111** | -0.000644 | -0.000970** | -0.000725 | -0.00121** |
| | (-1.81) | (-2.04) | (-1.77) | (-2.02) | (-1.45) | (-2.05) | (-1.46) | (-2.19) |
| CEO age | -0.00149** | -0.00145** | -0.00187*** | -0.00185** | -0.00157** | -0.00145** | -0.00193*** | -0.00184** |

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | (-2.22) | (-2.11) | (-2.64) | (-2.54) | (-2.34) | (-2.09) | (-2.76) | (-2.50) |
| Founder-Heir CEO | -0.0471*** | -0.0482*** | -0.0456** | -0.0498*** | -0.0503*** | -0.0475*** | -0.0467*** | -0.0477*** |
| | (-2.94) | (-2.91) | (-2.53) | (-2.72) | (-3.17) | (-2.87) | (-2.59) | (-2.60) |
| Nonemployee blockholders | 0.0100 | 0.0108 | 0.0119 | 0.0118 | 0.0105 | 0.0115 | 0.0125 | 0.0119 |
| | (1.36) | (1.43) | (1.47) | (1.42) | (1.43) | (1.50) | (1.53) | (1.42) |
| Constant | -0.00136 | -0.0235 | -0.0310 | -0.0556 | -0.0299 | -0.0319 | -0.0631 | -0.0630 |
| | (-0.02) | (-0.36) | (-0.54) | (-0.92) | (-0.46) | (-0.50) | (-1.09) | (-1.07) |
| Observations | 8756 | 8718 | 8413 | 8370 | 8756 | 8718 | 8413 | 8370 |
| Adj. $R^2$ | 0.055 | 0.048 | 0.057 | 0.051 | 0.058 | 0.048 | 0.063 | 0.055 |

**Appendix 1: Definitions of Variables**

| Variables | Definition |
|---|---|
| **Firm characteristics** | |
| Log of total asset | Natural log of book assets ($MM) |
| Log of sales | Natural log of annual sales ($MM) |
| Market-to-book | Book value of total assets minus book value of common equity plus the market value of common equity over book value of total assets. |
| Leverage | The sum of debt in current liabilities plus long term debt divided by total assets. |
| Stock volatility | Standard deviation of monthly stock returns in the past 12 months. |
| Idiosyncratic volatility | Standard deviation of the residuals from regressing a firm's monthly stock returns on the equally-weighted monthly returns of all firms in the same Fama and French 48 industry on the merged CRSP/Compustat database (excluding the sample firm) for the full fiscal year. |
| **Firm Performance** | |
| Stock Return | A firm's stock return adjusted by the value-weighted CRSP index return cumulated over the 12-month period ending one month before the CEO turnover announcement or over the fiscal year when there is no CEO turnover. This variable is winsorized at the 1st and 99th percentiles. |
| ΔEBIT | A firm's annual change in EBIT scaled by beginning of year total assets adjusted by the median of this ratio for all firms in its Fama and French 48 industry on the Compustat database over the appropriate fiscal year for a CEO turnover or over the fiscal year when there is no CEO turnover. This variable is winsorized at the 1st and 99th percentiles. |
| **Governance characteristics** | |
| Board size | Total number of directors on the board. |
| CEO-Chairman | An indicator variable that equals one if the CEO is also the Chairman of the board and equals zero otherwise. |
| Classified board | An indicator for classified board (also known as staggered board). |
| Dual class firm | An indicator for firms with more than one class of common shares. |
| Fraction of independent directors | Total number of independent directors divided by total number of directors on the board. |
| E-index | The entrenchment index of Bebchuk, Cohen, and Ferrell (2009) |

| Variables | Definition |
|---|---|
| Nonemployee blockholders | An indicator 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. |
| Inside and linked vote | Proportion of voting power held by inside and linked directors, including the CEO. |
| **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 whether the CEO is a founder or an heir of the founders of the firm. |
| Log CEO tenure | Natural log of one plus CEO tenure in years. |
| **CEO Compensation** | |
| Total Compensation | Total compensation ($000s) (Execucomp variable *tdc1*) |
| Delta | Dollar change in the CEO wealth associated with a 1% increase in the firm's stock price ($000s). This variable is winsorized at the 1st and 99th percentiles. |
| Equity-to-nonequity pay | The total ex ante value of a CEO's current stock and option grants divided by the value of total non-equity pay (i.e. total compensation – the total ex ante value of a CEO's new stock and option grants) in the fiscal year. |

## Appendix 2: Coefficient estimates of the propensity score models

The table reports the coefficient estimates of the probit models used to predict whether a firm is noncompliant with one of the four board structure requirements under the new exchange listing rules in 2001. The sample consists of all firm years observations from 1996 to 2000 in our full sample. The dependent variable *Treat* equals one if the firm is noncompliant with the board independence rule in 2001 and zero otherwise. The dependent variables *Treat_Nom*, *Treat_Aud* and *Treat_Com* are define similarly with respect to the new exchange listing requirements for fully independent nominating, audit and compensation committees respectively. All independent variables are defined in Appendix 1. The numbers in parentheses are t-statistics and ***, **, and * denote significance at the 1, 5 and 10 percent levels, respectively.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Dependent Variable | Treat | Treat_Nom | Treat_Aud | Treat_Com |
| Log of total assets | -0.118*** | -0.0872*** | 0.0824*** | -0.0549*** |
| | (-6.08) | (-4.94) | (4.73) | (-3.07) |
| Market-to-book ratio | -0.0264* | 0.0110 | 0.0104 | 0.00661 |
| | (-1.92) | (0.77) | (0.85) | (0.52) |
| Stock volatility | 1.264** | 3.141*** | -1.369** | -0.454 |
| | (2.27) | (4.74) | (-2.52) | (-0.93) |
| Fraction of independent directors | | -3.012*** | -2.556*** | -2.727*** |
| | | (-18.96) | (-17.54) | (-18.12) |
| E-index | -0.160*** | -0.0129 | 0.0190 | -0.0526*** |
| | (-7.85) | (-0.70) | (1.05) | (-2.77) |
| Dual class firms | -0.0239 | 0.358*** | -0.0904 | -0.392*** |
| | (-0.26) | (3.61) | (-1.02) | (-4.17) |
| CEO-Chairman | -0.278*** | -0.176*** | -0.0612 | -0.241*** |
| | (-4.72) | (-3.15) | (-1.14) | (-4.37) |
| Inside and linked vote | 0.0146*** | 0.00380 | -0.00271* | 0.000335 |
| | (9.03) | (1.63) | (-1.71) | (0.19) |
| Nonemployee blockholder | 0.0798 | 0.190*** | 0.311*** | 0.163*** |
| | (1.37) | (3.24) | (5.74) | (2.92) |
| Log of CEO tenure | 0.0933* | 0.130*** | 0.110** | -0.00510 |
| | (1.69) | (2.61) | (2.22) | (-0.10) |
| Founder-Heir CEO | 0.351*** | 0.157** | -0.0789 | -0.0547 |
| | (4.87) | (1.96) | (-1.07) | (-0.73) |
| CEO age | 0.0127*** | -0.00484 | -0.00975*** | 0.00470 |
| | (3.53) | (-1.34) | (-2.90) | (1.35) |
| Constant | -4.212 | 6.820 | -2.876 | -2.551 |
| | (-0.05) | (0.07) | (-0.03) | (-0.02) |
| *N* | 4022 | 4029 | 4017 | 4006 |
| pseudo $R^2$ | 0.187 | 0.180 | 0.127 | 0.149 |

## Internet Appendix

### A. Compliance between 2001 and 2005

Our sample firms that are noncompliant with the board independence requirement in 2001 became compliant by 2005 in three ways: 92% of them complied by both adding new directors and dropping existing directors, 5% complied by retaining all existing directors, but increasing the board's size to accommodate additional new independent directors and the remaining 3% complied simply by dropping some existing non-independent directors. On average, these firms add 2.8 independent directors, 1.2 linked directors and 1.1 employee directors, while they replaced 1.5 independent directors, 1.7 linked directors and 1.9 employee directors. Mean numbers of retained independent, linked and employee directors are 2.6, 2.2, and 2.2 respectively (see Table A-1). As we can see, the mean number of new independent directors is similar to the mean number of continuing independent directors and larger than the mean number of continuing linked or employee directors.

Sample firms not complying with the independent nominating committee rule in 2001 fall into two categories: 46% have no nominating committee and the remaining 54% have a nominating committee that is not fully independent. Firms in the first category complied by appointing independent directors to a newly formed nominating committee. On average, 3.6 independent directors subsequently join the nominating committee in these 312 firms. Firms in the second category complied with the rule in one of two ways, namely through: (1) departures of existing committee members (3% of all treatment firms) and (2) appointments of new committee members combined with departures of existing committee members (51% of all treatment firms).[26] The mean numbers of new and continuing committee members are 2.4 and 1.9 respectively, while the mean numbers of departing independent, linked and inside directors are 1.7, 1.2 and 1.2 respectively (see Table I-2).

### B. Substitution of forced turnover-performance for pay-performance sensitivity

[26] We use the term "new" here to refer to directors who are new to the nominating committee and the term "continuing" to refer directors who continue to serve on the nominating committee from 2001 to 2005.

In the Hermalin and Weisbach (1998) model, the objective of board monitoring is to discover the true ability of the CEO and replace her when her perceived ability is lower than some threshold related to the ability of the average replacement on the market. Here, CEO turnover is not an incentive device and thus does not substitute for CEO compensation in providing incentives. However, in reality, the threat of forced CEO turnover can potentially also serve to mitigate moral hazard problems, such as shirking, in a somewhat similar way to equity-based compensation. From this perspective, the forced CEO turnover to performance sensitivity can be a substitute for the pay-performance sensitivity in providing incentives to the CEOs. If the increase in forced CEO turnover to performance sensitivity we document is completely driven by a shift away from using pay-performance sensitivity to using turnover-performance sensitivity in providing incentives to CEOs in non-compliant firms relative to compliant firms, then it would be less clear whether we can interpret the evidence as reflecting an improvement in board monitoring. In this section, we examine this concern by controlling for change in pay-performance sensitivity in our main regressions in Table 4. Specifically, we add a measure of pay-performance sensitivity and its interaction with firm performance to the regressions in columns 1 through 4 in Table 4 and re-estimate them. If our prior results are completely driven by a shift between turnover-performance sensitivity and pay-performance sensitivity, then we expect the addition of these two terms to take away most of the statistical significance of our DiD terms.

Our first measure of pay-performance sensitivity is the delta of a CEO's equity portfolio at the end of each fiscal year. It equals to the dollar change in CEO wealth associated with a 1% change in the firms' stock price (in $000s). This measure is calculated using ExecuComp data by adding the delta of the portfolio of options and the delta of the portfolio of shares of the CEO. The delta of the portfolio of options is estimated using the formulae and approximation method detailed in Core and Guay (2002). The delta of the portfolio of shares is computed as the share owned by the CEO excluding options (i.e. SHROWN_EXCL_OPTIONS in ExecuComp) times the stock price at the fiscal year end (i.e. PRCCF in ExecuComp) times 0.01. Our second measure of pay-performance sensitivity is the ratio of equity-based pay over nonequity-based pay. ExecuComp changed its data

reporting format in 2006 to be in line with the reporting requirements in FAS123R. For pre-2006 data, we calculate the equity-based pay by summing up the value of restricted stock granted in the year (RSTKGRNT) and the value of option grants calculated using the Standard & Poor's Black-Scholes methodology (OPTION_AWARDS_BLK_VALUE). For post-2006 data, we calculate the equity-based pay by summing up the grant-date fair value of the stock awards (STOCK_AWARDS_FV) and that of the option awards (OPTION_AWARDS_FV). Nonequity-based pay equals to the total compensation minus the value of the equity-based pay. Both measures of pay-performance sensitivity are highly right skewed and contain some extreme values. Hence, we winsorize them at the two 1% tails and use the natural log of one plus the winsorized values as our measures of pay-performance sensitivity.

The regression results are shown in Table I-4. Columns 1 through 4 are for the board independence rule, while columns 5 through 8 are for the nominating committee independence rule. The coefficients of our two measures of pay-performance sensitivity are significantly negative at the 5% level in all the columns, suggesting that the probability of forced CEO turnover is lower when the CEO's equity-based incentives are higher. One explanation of this could be that boards choose to provide more equity-based incentives when it is more difficult to for them acquire private information about the CEO's ability and thus the likelihood of forced CEO turnover is lower in these firms *ceteris paribus*. The interactions of our pay-performance sensitivity measures and firm performance are positive in all columns except one, but they are statistically insignificant in all columns except in column 2 where it is positive and statistically significant at the 10% level in a two-sided test. After controlling for pay-performance sensitivity, we observe that the coefficients of the triple interaction of *Post*, *Treat* and *Performance* are little affected. They are still negative and statistically significant at conventional levels in two-sided tests in all columns. Hence, our prior results do not seem to be driven by any substitution between pay-performance sensitivity and turnover-performance sensitivity.

**C. Alternative specifications of the propensity score model**

Although our propensity score model is well motivated by existing theory and empirical evidence on boards of directors, the exact choice of variables is not as clear cut. To verify that our results are not driven by a particular specification, we evaluate the robustness of our main results to a number of alternative model specifications and report two of them here. In the first variation, we restrict the model used to predict firms noncompliant with the board independence rule to the following limited set of explanatory variables: firm size, market-to-book ratio, monthly stock return volatility, the entrenchment index (E-index) and industry. We add the fraction of independent directors on the board to the model when predicting firms that are noncompliant with the nominating committee independence rule. We exclude all governance variables except for the E-index, which tends to be quite stable prior to SOX. In the second specification, we only include firm size and industry to predict firms noncompliant with either rule. To facilitate a comparison with our main results, we continue to use a 0.15 radius for propensity score matching. Table I-5 presents the results. Columns 1 through 4 are estimated using the first propensity score model specification. Examining the coefficient of the triple interaction of *Treat*, *Post* and *Performance*, we find that it is negative and statistically significant at 10% or better in all columns. Columns 5 through 8 are estimated using size and industry matching only. The coefficient of the triple interaction is negative in all columns. It is statistically significant at 10% or better in two-sided tests in 3 out of the 4 columns. In the remaining column (column 6), it is statistically significant at the 10% level in a one-sided test.

**D. Misclassification of independent directors**

In this section, we investigate whether our results are driven by the misclassification of some noncompliant firms as compliant firms due to our use of the stricter RiskMetrics definition of independence than the exchange definition. As we discussed before, RiskMetrics classifies all former employees as non-independent while the exchanges consider a former employee independent if three or more years have passed since the end of his employment. In addition, RiskMetrics consider any business relation between a director and the firm compromises his independence while the exchanges only do so for material business relations. Since there is no detailed data on business relations, we

cannot define independent directors using the exchange definition. To address this concern, we choose to apply a stricter standard for noncompliance and re-classify our firms while using the RiskMetrics definition of independence. Specifically, we classify a firm as noncompliant with the board (nominating committee) independence rule only if the firm has less than the sample median percentage of independent directors on the board (nominating committee) among noncompliant firms in year 2001. In un-tabulated results, we find the median percentage of independence directors on the board is 43% and that on the nominating committee is 66%. This should eliminate most, if not all, of the misclassified compliant firms from our sample of noncompliant firms. We then follow the same procedure to re-estimate models 1 - 4 in Table 4 and report the results in Table I-6. Columns 1 and 2 report results for the board independence rule, while columns 3 and 4 report results from the nominating committee independence rule. We find the coefficient of the triple interaction of *Post*, *Treat* and *Performance* is significant at 10% level or better in all the columns.

**E. Closely controlled companies**

The new exchange listing rules exempt closely "controlled companies" from the board and committee independence requirements. A controlled company is defined as a firm in which an individual, a group or another firm hold more than 50% of the total voting rights of the firm. However, since we do not have detailed information on firm ownership to identify these controlled companies, they are not excluded from our sample. This in general should only bias against us finding significant difference between treatment and control firms. To make sure this is the case, in Table I-7, we exclude firms in which the percentage of independent directors on the board is below 50% and that on the nominating committee is below 100% in 2005 from our sample. Year 2005 is the final year that all firms must comply with the new exchange listing rules. Then, for the board independence rule and nominating committee independence rule, we first construct a matched sample of treatment firms and control firms from the remaining firms following the same propensity score matching procedure as before and then re-estimate equation (1). As we can see in Table I-7, both treatment effects are still

statistically significant at conventional levels after we exclude potential controlled companies from the sample.

Table I-1: Board adjustments to meet the exchange listing rule requiring an independent board between 2001 and 2005

The sample consists of 174 firms without a majority of independent directors on the board in 2001 and remains listed to 2005. Panel A presents the percentage of firms in my sample that complied with the rule by adding, dropping or replacing directors. 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 I-2: Board adjustments to meet the exchange listing rule requiring a fully independent nominating committee between 2001 and 2005

The sample consists of 693 firms without a nominating committee or without a fully independent one in 2001 and remains listed to 2005. Panel A presents the percentage of firms in this sample that complied by 2005 with the fully independent nominating committee rule by adding, dropping or replacing directors. 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 |

Table I-3: Coefficient estimates and marginal effects from logit regressions

This table reports the coefficient estimates from logit regressions where the dependent variable is an indicator for forced CEO turnover in year t. We exclude forced CEO turnovers that resulted from punishment for violations because they could introduce bias into the estimates. The samples are the same as those in columns 1-4 of Table 4. The specification of the model is similar to the linear probability models in Equation (1) but with the firm fixed effects being replaced by Fama and French 48 industry effects. *Treat* and *Post* are the treatment and post-treatment period indicators for the rule targeting the board structure shown in top of the column. *Performance* is measured in year t-1 by either market-adjusted stock returns or industry-adjusted change in EBIT as indicated in each column. Industries are defined by Fama and French (1997) 48 industry groups. Unreported interactions between year indicators and *Performance* are included in all the columns. Each column also includes CEO tenure (in years) and year indicators. All other independent variables are defined in Appendix 1 and measured in year t-1. Standard errors are clustered at the firm level. T-statistics for two-sided tests are in parentheses and ***, **, and * denote significance at the 1, 5 and 10 percent levels, respectively. The last row reports the delta method estimates of the marginal effects of the respective board structural changes on turnover-performance sensitivity when all firm and CEO characteristics are held at sample means. To facilitate interpretation, we invert the signs of these estimates so the numbers reported represent the differential changes in the probability of a CEO being fired for a 100% fall rather than rise in firm performance.

| | Board | | Nominating Committee | |
|---|---|---|---|---|
| | Stock Return | ΔEBIT | Stock Return | ΔEBIT |
| | (1) | (2) | (3) | (4) |
| Post × Treat | 0.274 | 0.707* | 0.177 | 0.424 |
| | (0.51) | (1.84) | (0.44) | (1.10) |
| Treat × Performance | 0.396 | 4.028 | 1.350*** | 8.557*** |
| | (0.92) | (1.00) | (3.33) | (2.88) |
| Post × Treat × Performance | -3.398* | -13.36 | -3.171** | -13.21** |
| | (-1.89) | (-1.61) | (-2.38) | (-1.99) |
| Log of sales | 0.0267 | 0.0722 | 0.0514 | 0.135*** |
| | (0.48) | (1.32) | (0.98) | (2.72) |
| Stock volatility | 4.001*** | 6.146*** | 3.661** | 6.859*** |
| | (2.66) | (4.71) | (2.37) | (5.90) |
| CEO-Chairman | -0.345** | -0.310* | -0.384** | -0.404** |
| | (-1.98) | (-1.77) | (-2.21) | (-2.39) |
| CEO voting power | -0.116*** | -0.116*** | -0.0840*** | -0.0698*** |
| | (-3.32) | (-3.37) | (-3.02) | (-3.12) |
| CEO age | -0.0195* | -0.0188* | -0.0244** | -0.0334*** |
| | (-1.86) | (-1.81) | (-2.28) | (-3.24) |
| Founder-Heir CEO | -0.698* | -0.810* | -0.663 | -0.614 |
| | (-1.71) | (-1.88) | (-1.62) | (-1.52) |
| Nonemployee blockholders | 0.163 | 0.241 | 0.234 | 0.274 |
| | (0.91) | (1.31) | (1.32) | (1.55) |
| Constant | -3.872*** | -4.374*** | -3.760*** | -4.640*** |
| | (-4.00) | (-4.47) | (-3.81) | (-5.07) |
| Observations | 7916 | 7889 | 7741 | 7781 |
| pseudo $R^2$ | 0.156 | 0.127 | 0.159 | 0.103 |
| Diff-in-Diff marginal effects | 0.060*** | 0.385*** | 0.047*** | 0.316*** |

Table I-4: Controlling for substitution between pay-performance and turnover-performance sensitivity

This table reports coefficient estimates from OLS regressions where the dependent variable is an indicator for forced CEO turnover in year t. We exclude forced CEO turnovers that resulted from punishment for violations because poor firm performance is not the main reason for these turnovers. The samples are the same as those in column 1-4 of Table 4. *Treat* and *Post* are the treatment and post-treatment period indicators for the rule targeting the board structure shown in top of the column. *Treat* equals one if the firm is noncompliant with the specific new listing rule at year-end 2001 and zero otherwise. *Post* equals one in years 2005 and later and zero otherwise. *Performance* is measured in year t-1 by either market-adjusted stock returns or industry-adjusted change in EBIT as indicated in each column. Industries are defined by Fama and French (1997) 48 industry groups. Unreported interactions between year fixed effects and *Performance* are included in all the columns. *PPS* is measured by either the delta of the CEO's portfolio of options and shares or the ratio of equity-based over nonequity-based pay in the fiscal year. Each column also includes CEO tenure (in years), firm and year fixed effects. All other independent variables are defined in Appendix 1 and measured in year t-1. Standard errors are clustered at the firm level. T-statistics for two-sided tests that the coefficient estimate equals zero are in parentheses and ***, **, and * denote significance at the 1, 5 and 10 percent levels, respectively.

| | Board (Delta) | | Board (Equity Ratio) | | Nom Com (Delta) | | Nom Com (Equity Ratio) | |
|---|---|---|---|---|---|---|---|---|
| | Stock Return | ΔEBIT | Stock Return | ΔEBIT | Stock Return | ΔEBIT | Stock Return | ΔEBIT |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Post × Treat | 0.0329*** | 0.0316*** | 0.0259** | 0.0263** | 0.0140 | 0.0100 | 0.0150 | 0.00942 |
| | (2.94) | (2.88) | (2.40) | (2.45) | (1.58) | (1.26) | (1.63) | (1.15) |
| Treat × Performance | 0.00999 | 0.118 | 0.00868 | 0.187 | 0.0198* | 0.270* | 0.0297** | 0.309** |
| | (1.05) | (0.86) | (0.76) | (1.32) | (1.71) | (1.80) | (2.39) | (1.99) |
| Post × Treat × Performance | -0.0742*** | -0.673** | -0.0562** | -0.519* | -0.0427** | -0.607*** | -0.0482** | -0.527** |
| | (-2.73) | (-2.31) | (-2.11) | (-1.79) | (-2.51) | (-2.86) | (-2.53) | (-2.24) |
| PPS | -0.0137*** | -0.0172*** | -0.0117** | -0.0122** | -0.0137*** | -0.0169*** | -0.0139** | -0.0138*** |
| | (-3.71) | (-4.77) | (-2.31) | (-2.42) | (-3.48) | (-4.53) | (-2.40) | (-2.62) |
| PPS × Performance | 0.00394 | 0.0603* | -0.000809 | 0.0842 | 0.00123 | 0.0306 | 0.00111 | 0.0862 |
| | (1.48) | (1.65) | (-0.11) | (1.34) | (0.40) | (0.85) | (0.15) | (1.32) |
| Log of sales | 0.0145* | 0.0203*** | 0.00813 | 0.0148** | 0.0214*** | 0.0285*** | 0.0136** | 0.0217*** |
| | (1.92) | (2.70) | (1.17) | (2.15) | (3.68) | (4.65) | (2.46) | (3.71) |
| Stock volatility | 0.102** | 0.112** | 0.165*** | 0.183*** | 0.126** | 0.122** | 0.199*** | 0.198*** |
| | (1.98) | (2.22) | (3.15) | (3.49) | (2.21) | (2.20) | (3.46) | (3.47) |
| CEO-Chairman | -0.00562 | -0.00540 | -0.0118 | -0.0125* | -0.00605 | -0.00661 | -0.0117 | -0.0134* |
| | (-0.83) | (-0.79) | (-1.64) | (-1.72) | (-0.87) | (-0.94) | (-1.58) | (-1.78) |
| CEO voting power | -0.000465 | -0.000404 | -0.000842* | -0.000933* | -0.000544 | -0.000509 | -0.000949* | -0.00105* |
| | (-0.91) | (-0.77) | (-1.73) | (-1.82) | (-0.88) | (-0.78) | (-1.68) | (-1.75) |
| CEO age | -0.00103 | -0.00105 | -0.00155** | -0.00154** | -0.00119* | -0.00128* | -0.00194*** | -0.00203*** |
| | (-1.58) | (-1.54) | (-2.29) | (-2.23) | (-1.75) | (-1.80) | (-2.74) | (-2.77) |
| Founder-Heir CEO | -0.0288* | -0.0277* | -0.0468*** | -0.0466*** | -0.0311* | -0.0293 | -0.0492*** | -0.0485*** |
| | (-1.74) | (-1.69) | (-2.87) | (-2.81) | (-1.71) | (-1.61) | (-2.72) | (-2.65) |

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| Nonemployee blockholders | 0.0140* | 0.0138* | 0.0125* | 0.0124 | 0.0168** | 0.0166** | 0.0139* | 0.0136 |
| | (1.93) | (1.86) | (1.67) | (1.60) | (2.10) | (2.03) | (1.67) | (1.58) |
| Constant | -0.0138 | -0.0407 | 0.00973 | -0.0396 | -0.0537 | -0.0913 | -0.00416 | -0.0645 |
| | (-0.22) | (-0.63) | (0.15) | (-0.62) | (-0.98) | (-1.56) | (-0.07) | (-1.08) |
| Observations | 8169 | 8136 | 8647 | 8610 | 7860 | 7831 | 8308 | 8276 |
| Adj. $R^2$ | 0.065 | 0.064 | 0.054 | 0.051 | 0.066 | 0.064 | 0.057 | 0.054 |

Table I-5: Robustness to the specification of the propensity score model

This table reports coefficient estimates from OLS regressions where the dependent variable is an indicator for forced CEO turnover in year t. We exclude forced CEO turnovers that resulted from punishment for violations because poor firm performance is not the main reason for these turnovers. The matched samples of treatment and control firms are constructed as follows: treatment and control firms are matched on firm size, market-to-book ratio, stock return volatility, E-index and industry in columns 1 and 2. They are matched on above dimensions plus the fraction of independent directors in columns 3 and 4. They are matched on firm size and industry only in columns 5 through 8. *Treat* and *Post* are the treatment and post-treatment period indicators for the rule targeting the board structure shown in top of the column. *Treat* equals one if the firm is noncompliant with the specific new listing rule at year-end 2001 and zero otherwise. *Post* equals one in years 2005 and later and zero otherwise. *Performance* is measured in year t-1 by either market-adjusted stock returns or industry-adjusted changes in EBIT as indicated in each column. Industries are defined by Fama and French (1997) 48 industry groups. Unreported interactions between year fixed effects and *Performance* are included in all the columns. Each column also includes CEO tenure (in years), firm and year fixed effects. All other independent variables are defined in Appendix 1 and measured in year t-1. Standard errors are clustered at the firm level. T-statistics for two-sided tests are in parentheses and ***, **, and * denote significance at the 1, 5 and 10 percent levels, respectively.

| | Board | | Nom Com | | Board | | Nom Com | |
|---|---|---|---|---|---|---|---|---|
| | Stock Return | ΔEBIT | Stock Return | ΔEBIT | Stock Return | ΔEBIT | Stock Return | ΔEBIT |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Post × Treat | 0.0145 | 0.0153 | 0.0191* | 0.0136 | 0.00688 | 0.00565 | 0.0171* | 0.0116 |
| | (1.26) | (1.34) | (1.95) | (1.57) | (0.64) | (0.52) | (1.81) | (1.40) |
| Treat × Performance | 0.00861 | 0.202 | 0.0327** | 0.440*** | 0.00408 | 0.0301 | 0.0333*** | 0.394*** |
| | (0.74) | (1.33) | (2.47) | (2.65) | (0.38) | (0.21) | (2.78) | (2.79) |
| Post × Treat × Performance | -0.0456* | -0.488* | -0.0523*** | -0.585** | -0.0381* | -0.355 | -0.0473*** | -0.476** |
| | (-1.83) | (-1.78) | (-2.93) | (-2.49) | (-1.83) | (-1.54) | (-2.76) | (-2.39) |
| Log of sales | 0.0108 | 0.0160** | 0.00869 | 0.0153** | 0.0135** | 0.0194*** | 0.0124* | 0.0192*** |
| | (1.56) | (2.35) | (1.22) | (2.22) | (2.00) | (2.89) | (1.83) | (2.89) |
| Stock volatility | 0.148*** | 0.170*** | 0.163*** | 0.162*** | 0.147*** | 0.174*** | 0.163*** | 0.165*** |
| | (2.60) | (2.98) | (2.72) | (2.73) | (2.85) | (3.35) | (3.20) | (3.25) |
| CEO-Chairman | -0.0126* | -0.0127* | -0.0122 | -0.0133* | -0.0127* | -0.0127* | -0.0116 | -0.0126* |
| | (-1.69) | (-1.68) | (-1.61) | (-1.73) | (-1.80) | (-1.77) | (-1.64) | (-1.76) |
| CEO voting power | -0.000642 | -0.000729* | -0.000639 | -0.000750 | -0.000590 | -0.000663* | -0.000544 | -0.000650* |
| | (-1.58) | (-1.69) | (-1.27) | (-1.40) | (-1.65) | (-1.76) | (-1.50) | (-1.70) |
| CEO age | -0.000773 | -0.000807 | -0.000723 | -0.000808 | -0.00102* | -0.00107* | -0.00101* | -0.00108* |
| | (-1.18) | (-1.19) | (-1.11) | (-1.20) | (-1.68) | (-1.70) | (-1.67) | (-1.73) |
| Founder-Heir CEO | -0.0565*** | -0.0559*** | -0.0542*** | -0.0531*** | -0.0490*** | -0.0482*** | -0.0510*** | -0.0493*** |
| | (-3.39) | (-3.28) | (-3.18) | (-3.05) | (-3.33) | (-3.22) | (-3.39) | (-3.23) |
| Nonemployee blockholders | 0.0135* | 0.0138* | 0.0134 | 0.0141 | 0.0109 | 0.0112 | 0.0111 | 0.0111 |
| | (1.74) | (1.74) | (1.59) | (1.63) | (1.49) | (1.49) | (1.51) | (1.48) |
| Constant | -0.0516 | -0.0901 | -0.0302 | -0.0817 | -0.0567 | -0.0983* | -0.0449 | -0.0966* |
| | (-0.83) | (-1.45) | (-0.47) | (-1.30) | (-0.96) | (-1.66) | (-0.76) | (-1.65) |
| Observations | 8006 | 7966 | 7436 | 7403 | 8882 | 8833 | 8853 | 8813 |

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| Adj. $R^2$ | 0.065 | 0.059 | 0.065 | 0.060 | 0.066 | 0.061 | 0.067 | 0.063 |

Table I-6: Using stricter standards of noncompliance

This table reports coefficient estimates from OLS regressions where the dependent variable is an indicator for forced CEO turnover in year t. We exclude forced CEO turnovers that resulted from punishment for violations because poor firm performance is not the main reason for these turnovers. We define firms with less than the sample median percentage (43%) of independent directors on the board in 2001 as noncompliant with the board independence rule. Similarly, we define firms with less than the sample median percentage (66%) of independent directors on the nominating committee in 2001 as noncompliant with the full nominating committee independence rule. We exclude observations in the transition years of 2003 and 2004 to reduce noise in the sample. *Treat* and *Post* are the treatment and post-treatment period indicators for the rule targeting the board structure shown in top of the column. *Treat* equals one if the firm is noncompliant with the specific new listing rule at year-end 2001 and zero otherwise. *Post* equals one in years 2005 and later and zero otherwise. *Performance* is measured in year t-1 by either market-adjusted stock returns or industry-adjusted changes in EBIT as indicated in each column. Industries are defined by Fama and French (1997) 48 industry groups. Unreported interactions between year fixed effects and *Performance* are included in all four columns. Each column also includes CEO tenure (in years), firm and year fixed effects. All other independent variables are defined in Appendix 1 and measured in year t-1. Standard errors are clustered at the firm level. T-statistics for two-sided tests that the coefficient estimate equals zero are in parentheses and ***, **, and * denote significance at the 1, 5 and 10 percent levels, respectively.

| | Board (43% cut-off) | | Nom Com (66% cut-off) | |
|---|---|---|---|---|
| | Stock Return | ΔEBIT | Stock Return | ΔEBIT |
| | (1) | (2) | (3) | (4) |
| Post × Treat | 0.0256* | 0.0276* | 0.0158 | 0.00867 |
| | (1.78) | (1.87) | (1.60) | (0.99) |
| Treat × Performance | 0.0128 | 0.203 | 0.0428*** | 0.488*** |
| | (0.93) | (1.11) | (3.53) | (3.26) |
| Post × Treat × Performance | -0.0794** | -0.791* | -0.0684*** | -0.493* |
| | (-2.07) | (-1.89) | (-3.68) | (-1.94) |
| Log of sales | 0.00701 | 0.0139* | 0.0143** | 0.0210*** |
| | (0.95) | (1.95) | (2.38) | (3.43) |
| Stock volatility | 0.169*** | 0.190*** | 0.202*** | 0.195*** |
| | (2.85) | (3.18) | (3.20) | (3.09) |
| CEO-Chairman | -0.0119 | -0.0120 | -0.0118 | -0.0128 |
| | (-1.49) | (-1.49) | (-1.45) | (-1.54) |
| CEO voting power | -0.000938 | -0.00107 | -0.000696 | -0.000806 |
| | (-1.48) | (-1.59) | (-1.39) | (-1.49) |
| CEO age | -0.000630 | -0.000676 | -0.00114 | -0.00124 |
| | (-0.88) | (-0.92) | (-1.50) | (-1.58) |
| Founder-Heir CEO | -0.0527*** | -0.0517*** | -0.0475*** | -0.0453** |
| | (-3.04) | (-2.93) | (-2.59) | (-2.44) |
| Nonemployee blockholders | 0.0171** | 0.0175** | 0.0170* | 0.0172* |
| | (2.00) | (1.99) | (1.92) | (1.91) |
| Constant | -0.0342 | -0.0841 | -0.0605 | -0.111* |
| | (-0.50) | (-1.25) | (-1.02) | (-1.85) |
| Observations | 7038 | 7002 | 6587 | 6556 |
| Adj. $R^2$ | 0.063 | 0.058 | 0.070 | 0.064 |

Table I-7: Excluding controlled companies

This table reports coefficient estimates from OLS regressions where the dependent variable is an indicator for forced CEO turnover in year t. We exclude forced CEO turnovers that resulted from punishment for violations because poor firm performance is not the main reason for these turnovers. The sample includes only firms that complied with the exchange listing requirements by 2005. Firms that were still noncompliant in 2005 are likely to be controlled companies so we exclude them from the sample. Columns 1 and 2 are estimated for the board independence rule, while columns 3 and 4 are estimated for the nominating committee independence rule. *Treat* and *Post* are the treatment and post-treatment period indicators for the rule targeting the board structure shown in top of the column. *Treat* equals one if the firm is noncompliant with the specific new listing rule at year-end 2001 and zero otherwise. *Post* equals one in years 2005 and later and zero otherwise. *Performance* is measured in year t-1 by either market-adjusted stock returns or industry-adjusted changes in EBIT as indicated in each column. Industries are defined by Fama and French (1997) 48 industry groups. Unreported interactions between year fixed effects and *Performance* are included in all four columns. Each column also includes CEO tenure (in years), firm and year fixed effects. All other independent variables are defined in Appendix 1 and measured in year t-1. Standard errors are clustered at the firm level. T-statistics for two-sided tests that the coefficient estimate equals zero are in parentheses and ***, **, and * denote significance at the 1, 5 and 10 percent levels, respectively.

| | Board | | Nominating Committee | |
|---|---|---|---|---|
| | Stock Return | ΔEBIT | Stock Return | ΔEBIT |
| | (1) | (2) | (3) | (4) |
| Post × Treat | 0.0292** | 0.0295** | 0.0194* | 0.0127 |
| | (2.54) | (2.58) | (1.72) | (1.25) |
| Treat × Performance | 0.00762 | 0.165 | 0.0372*** | 0.360** |
| | (0.66) | (1.12) | (2.64) | (1.99) |
| Post × Treat × Performance | -0.0639** | -0.533* | -0.0582*** | -0.617** |
| | (-2.14) | (-1.77) | (-2.62) | (-2.20) |
| Log of sales | 0.00445 | 0.0106 | 0.0118* | 0.0201*** |
| | (0.63) | (1.52) | (1.74) | (2.90) |
| Stock volatility | 0.158*** | 0.177*** | 0.246*** | 0.243*** |
| | (2.85) | (3.18) | (3.57) | (3.54) |
| CEO-Chairman | -0.0127* | -0.0134* | -0.0164* | -0.0192** |
| | (-1.72) | (-1.79) | (-1.82) | (-2.10) |
| CEO voting power | -0.00107* | -0.00121* | -0.000372 | -0.000379 |
| | (-1.80) | (-1.93) | (-0.49) | (-0.45) |
| CEO age | -0.00146** | -0.00145** | -0.00248*** | -0.00271*** |
| | (-2.07) | (-1.99) | (-2.94) | (-3.14) |
| Founder-Heir CEO | -0.0463*** | -0.0466*** | -0.0411** | -0.0406** |
| | (-2.66) | (-2.63) | (-2.15) | (-2.05) |
| Nonemployee blockholders | 0.0131* | 0.0133 | 0.0147 | 0.0149 |
| | (1.65) | (1.63) | (1.46) | (1.45) |
| Constant | 0.0251 | -0.0223 | 0.0263 | -0.0294 |
| | (0.38) | (-0.34) | (0.36) | (-0.40) |
| Observations | 8136 | 8098 | 5976 | 5946 |
| Adj. $R^2$ | 0.053 | 0.048 | 0.053 | 0.054 |