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Are Executive Suite Independence and Board Independence Interrelated?

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Abstract

We find a regulatory shock that increases board independence spills over to executive suites, increasing CEO connectedness with top executives through appointments and pre-existing social ties. Spillover does not occur, however, when treated firms increase CEO-independent director social ties, suggesting CEO-executive connections and CEO-director connections are substitutes. How changes in CEO-executive connections affect firm performance depends on the information environment: When outsiders' cost to acquire information is high (low), increases in CEO-executive connections are (not) associated with better operating performance and higher shareholder value. Our findings are not driven by the Sarbanes-Oxley Act or other confounding effects, and are robust to a battery of other tests. We conclude that independence in the two main governing bodies, the board and executive suite, are inversely related. Inferring the overall independence from board independence alone can be misleading.

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1. Introduction

Two main governing bodies of corporations are the board of directors and the executive suite of the CEO and his top lieutenants. While much research has been devoted to studying board independence and its effectiveness in protecting shareholder interest from CEOs' self-serving behavior,[1] research on executive suite independence has been limited. Fama (1980) points out the importance of independent executives when he states, "Less well appreciated, however, is the monitoring that takes place from bottom to top" (p. 293). In a more recent contribution, Landier, Sraer, and Thesmar (2009) argue that the presence of more top executives with different preferences and dissenting views—independent executives—strengthens governance and steers CEOs toward more shareholder-friendly decisions. Acharya, Myers, and Rajan (2011) also analyze internal monitoring of CEOs by other top executives. These theoretical considerations suggest independence in the executive suite matters.

In this paper we investigate whether independence in the two main governing bodies is related; and if it is, whether the relation is positive or negative. These are important governance issues. If the relation is negative, for example, inferring the overall governance independence from board independence alone will be misleading.

The relation can be positive, however, if an independent board wants independent top executives and has sufficient influence over their personnel decisions. A positive relation is also plausible in the Hermalin and Weisbach model (1998), wherein board independence is determined by a bargaining process; CEOs with greater bargaining power end up with more dependent boards. Since the bargaining power comes from perceived ability arising from good performance, CEOs with dependent boards may have more freedom to pick and choose their top lieutenants, which allows more dependent executive suites.

[1] An incomplete list of studies examining the relation between director independence and the strength of board oversight and/or firm performance includes Brickley and James (1987); Weisbach (1988); Rosenstein and Wyatt (1990); Byrd and Hickman (1992); Brickley, Coles, and Terry (1994); Cotter and Zenner (1994); Borokhovich, Parrino, and Trapani (1996); Mayers, Shivdasani, and Smith (1997); Dahya, McConnell, and Travlos (2002); Huson, Malatesta, and Parrino (2004); Dahya and McConnell (2007); Chhaochharia and Grinstein (2009); Nguyen and Nielsen (2010); Wintoki, Linck, and Netter (2012); Knyazeva, Knyazeva, and Masulis (2013); and Coles, Daniel, and Naveen (2014).

Our data, however, tells a different story. Because both board and executive suite independence are endogenous, we study the interrelation between the two governing bodies using an exogenous shock that increases board independence (the independent board requirement for NYSE- and NASDAQ listed firms). We find the shock decreases executive suite independence, as measured by CEO connectedness with top executives through appointments and pre-existing network ties.

We attribute the weakening of executive suite independence to information and entrenchment related responses to the shock. Adams and Ferreira (2007) argue CEOs value board advising but resent monitoring from boards they consider too independent. When the shock forces a dependent board to become independent, the CEO is likely to consider the board too independent and may attempt to weaken its monitoring by withholding more information than before from independent directors (Adams and Ferreira, 2007). Independent directors' accessibility to information and its accuracy can be obstructed through a CEO's control of information flows and his strategic behavior to increase information asymmetries for entrenchment (Shleifer and Vishny, 1989; Edlin and Stiglitz, 1995). CEOs have at their disposal full-time employees to gather, and screen information they do not wish to share with the board. Independent directors, by contrast, work part time, meeting a few times a year, so they have limited access to pertinent information, relying heavily on management as their primary source of information (Dominguez-Martinez, Swank, and Visser, 2008; Adams, Hermalin, and Weisbach, 2010).

Effective control of information flow to the board, however, is not automatic. It requires coordination with, and cooperation from other executives who have access to inside information, which can be leaked to the board. To erect a more united front against the board's monitoring, a CEO hit by the shock on board independence may strengthen her control by increasing connectedness in executive suites and thereby her soft influence over top executives.

How increasing CEO connectedness affects firm performance depends on why a firm chose a dependent board prior to the shock. Harris and Raviv (2008) show an insider controlled board is optimal when insiders' information is crucial for firm profitability relative to agency costs—e.g., startups in high-tech industries. Duchin, Matsusaka, and Ozbas (2010) argue, with supporting evidence, that when

outsiders' cost to acquire information is high, an insider controlled board is optimal for shareholders, and the board regulation—the shock employed in this study—decreases shareholder value. When firms are faced with such an optimality-distorting regulation, increasing CEO connectedness in executive suites may help counter the negative impact. A more closely knit group can better insulate itself from outsiders, allowing insiders to freely share information to exploit their collective inside information for profitability with more freedom to make decisions. We find that when outsiders' cost to acquire information is high, increases in CEO connectedness have positive marginal effects on both operating performance and shareholder value.

Duchin et al. (2010) also show that when outsiders' cost to acquire information is low, an outsider controlled board is optimal, but entrenched CEOs may choose suboptimal insider-controlled boards for private benefits. For such a firm, forcing its board to become independent will be value-enhancing, but it may also reduce the CEO's private benefits. To protect private benefits, the CEO may increase his connectedness in executive suites. More closely connected executives may be more cooperative, and acquiesce to withholding information useful for the board's monitoring of private benefits.[2] We find when outsiders' cost to acquire information is low, increases in CEO connectedness have no favorable effects on firm performance.

We proxy executive suite *dependence* by the fraction of top-four non-CEO executives appointed (FTA) during a CEO's tenure and the CEO's pre-existing social ties with the appointees. Higher FTA helps increase what social psychologists refer to as "social influence," which relies on norms of reciprocity, liking, and social consensus to shape management's decision making (Cialdini, 1984). CEOs are heavily involved in recruiting, nominating, and appointing their top lieutenants, so their appointees are more likely to share similar briefs and preferences with, and may be more beholden to, the CEO in comparison to those appointed by a previous CEO (Landier, Sauvagnat, Sraer, and Thesmar, 2013). Morse, Nanda, and Seru (2011) and Coles et al. (2014) rely on a similar notion of reciprocity between

[2] Consistent with this conjecture, Armstrong, Core, and Guay (2014) find that more entrenched managements are more effective in withholding information demands of independent directors.

directors and the CEO when they measure how "co-opted" a board is by the fraction of directors appointed during a CEO's tenure.[3]

We estimate differences-in-differences in FTA using the independent board requirement for NYSE- and NASDAQ-listed firms as an exogenous shock. The estimation relies on variation in the pre-regulation board composition. The treatment (control) group is firms without (with) a majority of independent directors prior to the shock. Treatment and control groups show parallel trends in FTA prior to the shock. Nevertheless, we construct another control group using propensity-scores to improve the comparability. Reported estimates are based on both unmatched and propensity-score matched samples.

Regardless of which sample is used, we find treated firms significantly increase FTA, suggesting a shakeup in executive suites. Most treated firms achieve higher FTA without costly severance packages or losing firm-specific knowledge. Most executives who lost positions on the top-four list remain with the firm following the regulation. The higher FTA seems largely a result of appointing new executives to positions of higher rank than previous CEOs' appointees.

A closer look at executive turnovers during the board transition reveals that the higher FTA is not driven by replacement of inside-executive directors, CEO turnovers, or longer tenure of previous CEOs' appointees. It shows CEOs' strong influence on who stays in the executive suite and who remains as inside director—their own appointees. For example, while longer tenure works against previous CEOs' appointees, it helps current CEOs' appointees to retain their positions on the top-four list. In addition, inside directors who are current CEOs' appointees emerge from the board restructuring largely intact. The strong CEO influence may arise from the ability to control information flow to the board concerning their top lieutenants' qualifications, talent, and fit in the executive suite.

In addition, we find newly appointed executives of treated firms are more socially pre-connected to the CEO, as measured by network ties formed prior to their appointments through past employment,

[3] New independent directors brought in to meet the independent board requirement are appointed during the tenure of CEOs at the time of the shock. Thus, they might be more co-opted, weakening the real effect the shock has on the board independence. This, in turn, may lead to weaker spillover to the executive suite; that is, increases in FTA and CEOs' prior social connections with their appointees will be smaller, leading our estimates in favor of the null hypothesis of no shock effect.

education, and membership to social organizations during overlapping years. Prior social connections play a role in appointment decisions because they provide valuable information about personal characteristics unavailable in the public domain. However, this applies to both treated and control groups and thus does not explain why the network ties increase more at treated firms.

Closer CEO connectedness with top executives may not be the only means with which treated firms can counter the shock. If regulation forces a board to appoint more independent directors than endogenously determined, the firm may recruit independent directors with more "disutility for monitoring" (Hermalin and Weisbach, 1998). Fracassi and Tate (2012) find that powerful CEOs are more likely to appoint directors with whom they have social ties and that CEO-director social ties weaken the intensity of board monitoring. We find weak evidence that treated firms increase CEO-independent director social ties, which indicates that some CEOs influence director appointment decisions even when nominating committees consist entirely of independent directors. Surprisingly, when CEO-director ties increase, we find no changes in FTA among treated firms. Only when CEO-director ties do not increase do we observe significant increases in FTA. CEO-director ties and FTA appear substitutes. Perhaps CEOs increase FTA only when they are unable to increase social dependency of the board.

Our findings of higher FTA are not driven by the enactment of Sarbanes-Oxley Act in 2002 or by other confounding events. This conclusion is reached after conducting (1) placebo tests using "controlled" firms, which are exempt from the independent board requirement but subject to the SOX requirements and other confounding events; (2) reestimation of the baseline regression with a sample of firms that are exempt from Section 404 of the SOX; and (3) an examination of whether the financial transparency of treatment and control groups is affected differently by the SOX. The higher FTA among treated firms are also robust to a compensation-weighted FTA allowing for differences in influence across rank among top-four non-CEO executives, abnormal measures of FTA (residuals of regression relating FTA to CEO tenure and other factors mechanically correlated to FTA). We also check robustness to an alternative measure of treatment effects, major structural changes within the firm, and an alternative sample construction. None alters our conclusion.

This paper contributes to the corporate governance literature by investigating dynamics between the executive suite and the board, an important issue overlooked by previous researchers. Our findings suggest independence in the board and executive suite is inversely related; thus, inferring the overall governance independence from board independence alone can be misleading. In addition, we illustrate that weakening executive suite independence is not necessarily bad; it could help or hurt firm performance, depending on the firm-specific information environment.

This study also fills a void in the literature on CEO influence and involvement in the selection of top echelon players governing the firm. Previous studies examine CEO influence on selecting board members (e.g., Shivdasani and Yermack, 1999; Hwang and Kim, 2009; Fracassi and Tate, 2012; Coles et al., 2014). We add to this literature by studying CEO influence on the appointment and composition of the other corporate governing body—the executive suite.

In addition, our estimates reveal that when one aspect of governance is regulated, some firms shift other aspects of governance. Therefore, when regulators target a specific governance mechanism, they must carefully consider possible spillovers to other governing mechanisms, as well as to other aspects of the targeted mechanism.

The next section develops our hypotheses and describes empirical design and data. Section 3 presents main findings on CEO-executive connections. Section 4 investigates how the shock affects CEO-independent director social ties and how their changes CEO-director ties are related to changes in FTA. Section 5 presents firm performance analysis. Section 6 presents various robustness tests. Section 7 concludes.

2. Predictions, Empirical Design, and Data

2.1. Theoretical Considerations and Predictions

To investigate the interrelationship between board and executive suite independence, we consider possible changes in executive suites that might take place in response to a regulatory shock requiring board independence. Then we rely on existing theories on the choice of insider- versus outsider controlled board to make predictions on how the responses impact firm performance.

It is often assumed that board independence, as defined by the percentage of independent directors, is associated with stronger board monitoring. However, the strength of monitoring does not depend only on the fraction of independent directors. Effective monitoring also requires that independent directors have access to relevant firm-specific information (Adams and Ferreira, 2007; Harris and Raviv, 2005, 2008; Duchin et al., 2010). The accessibility, in turn, is not exogenous. Adams and Ferreira (2007) show how much information CEOs reveal to the board involves a tradeoff between benefits of better advising and the risk of the board's interference in CEOs' decision making; hence, CEOs "will not communicate firm-specific information to a board that is too independent" (p. 218).[4] When a CEO with a dependent board is hit by a shock requiring board independence, he might consider the newly independent board "too independent," and may attempt to withhold more information from the board than before the shock.

Withholding information from the board requires cooperation and acquiescence from other members of the executive suite. The CEO is not the only person with access to inside information. Others in the executive suite can leak information that the CEO does not wish to share with independent directors; but they might be less inclined to do so if closely connected to the CEO through appointments and social ties. CEOs are heavily involved in the appointment decisions of their top lieutenants, so their own appointees are more likely to share similar preferences with, and may be beholden to, the CEO in comparison to those appointed by a previous CEO.[5] Thus, the more top executives appointed during a CEO's tenure, the greater the CEO's soft influence, which is likely to be further enhanced if the appointees are socially pre-connected to the CEO. The familiarity acquired through prior social interactions helps a CEO select individuals who are more closely aligned and less likely to dissent, as well as those who are a better fit to the executive team.

[4] In a different context, Shleifer and Vishny (1989) show that managers can counter monitoring and disciplinary forces by strategically choosing the information level available to outsiders through manager-specific investments. Edlin and Stiglitz (1995) go a step further: to counter monitoring, managers invest in activities for which information asymmetries are particularly large.

[5] A previous CEO's appointee also may feel grateful if a CEO decides to retain her. However, new hiring and promotion requires greater engagement and commitment on the part of the CEO than retaining someone from the previous team; hence, the connectedness will be closer with one's own appointees than with others' appointees..

Thus, when a CEO has more of her own appointees with prior social ties in the executive suite, the CEO is able to exert stronger control over information she does not wish to share with the board. Consistent with this conjecture, Armstrong, Core, and Guay (2014) find that "when management is more likely to be entrenched, management forecasts become less precise, and to some extent less frequent." (p. 385). These considerations lead us to make:

Prediction 1: When a dependent board is forced by regulation to become independent, the CEO's connectedness in the executive suite will increase through appointments and greater social ties.

Such responses to the shock will make the overall governance independence from CEO influence weaker than the post-shock board independence alone indicates.

If CEO connectedness in executive suites reduces the effectiveness of monitoring, why would shareholders allow it? From the shareholder perspective, there are benefits as well as costs. The main benefit is better coordination between the CEO and connected executives, which helps make timely reactions and pro-actions to internal and external challenges facing the firm. The benefits, however, should be balanced against costs associated with giving a higher priority to CEO connectedness. The costs are the risk of inadequate checks and balances in executive suites [6] and the opportunity costs associated with compromising the best possible combination of talent, experience, and fit in forming the top executive team. An optimal degree of CEO connectedness involves trading off these costs against the need for efficient decision making. Li, Lu, and Phillips (2015) provide evidence that the effect of CEO connectedness on firm performance is positively related to the urgency of timely decision making, which is proxied by product market competitiveness, instability, and dynamism.

How increasing CEO connectedness (in response to the shock) affects firm performance depends on the reasons some firms chose dependent boards prior to the shock, while others chose independent boards. Recent theoretical contributions to the understanding of how board independence is determined

[6] The risk of inadequate checks and balances due to close CEO connectedness with top executives is illuminated in Khanna, Kim and Lu (2015), who show that appointment-based CEO connectedness increases the likelihood of committing fraud, decreases the likelihood of detection, makes CEO dismissal less likely upon discovery, and lowers the coordination costs of carrying out illegal activities.

include Hermalin and Weisbach (1998); Raheja (2005); Adams and Ferreira (2007); Harris and Raviv (2008); Duchin, Matsusaka, and Ozbas (2010); and Ferreira, Ferreira, and Raposo (2011).

Adams and Ferreira (2007) show that potential conflicts between the board's advisory and monitoring roles may lead shareholders to optimally elect a less independent board that does not monitor the CEO too intensely. Harris and Raviv (2008) show an insider controlled board, a dependent board for our analysis, is optimal when insiders' information is crucial for firm profitability relative to agency costs. Incorporating key insights from these two studies, Duchin et al. (2010) argue that when outsiders' cost of acquiring information is high, an outsider-controlled board is ineffective and an insider-controlled board is optimal; a regulatory shock forcing an optimal-dependent board to become independent will decrease shareholder value.

When firms are faced with such an optimality-distorting regulation, we hypothesize that increasing CEO connectedness with top executives will soften the negative impact of the regulation. A tightly-knit, top executive team can be better at insulating itself from outsiders, which helps insiders freely share information to exploit their collective inside information for profitability. Effective utilization of inside information by an executive team with more freedom to make decisions is especially important for firms that chose a dependent board for optimality reasons, which according to the equilibrium model in Harris and Raviv (2008) occurs when insiders' information is crucial for firm profitability. If increasing CEO connectedness indeed helps reduce the negative effect of the regulation, it should have a positive marginal effect on firm performance.

Duchin et al. (2010) also argue that when outsiders' cost of acquiring information is low, an outsider controlled board is optimal, but entrenched CEOs may choose suboptimal-insider controlled boards for private benefits. For this type of firm, forcing the boards to become independent will improve shareholder value, but the newly independent board's monitoring may also reduce the CEO's private benefits. To protect private benefits, the entrenched CEO may increase his connectedness to put up a more united front against the newly independent board's monitoring on private benefits. Such responses are likely to dampen the positive effect the shock could have had on firm performance. Thus, we make:

Prediction 2: When outsiders' cost of acquiring information is high (low), increases in CEO connectedness in response to the regulatory shock mandating board independence are (not) associated with favorable effects on firm performance.

To test these predictions, we first investigate whether or not a regulatory shock mandating board independence increases CEO connectedness in executive suites. Then, we relate the regulation-related changes in CEO connectedness to operating performance and shareholder value under different information environments.

2.2. Empirical Design

2.2.1. Proxies for CEO connectedness

Our primary proxy for CEO connectedness in executive suites is connections a current CEO has built through appointment decisions of top-four non-CEO executives. It is the same as the appointment-based CEO connectedness measure used in Khanna, Kim, and Lu (2015), the fraction of top-four non-CEO executives appointed (FTA) during the current CEO's tenure. FTA_{it} is the number of executives hired or promoted from within the firm to the top-four non-CEO positions during the tenure of firm *i*'s CEO as of year *t* divided by four. It ranges from zero to one in increments of 0.25. Top four non-CEO executives are identified from ExecuComp, which ranks executives by the sum of salaries and bonuses. We assume the year an executive first appears on the top-four list is the year she obtained the position. We compare this year with the year a current CEO took office to determine whether the executive is appointed during the CEO's tenure. To prevent changes in the reported number of executives from affecting within-firm variation in *FTA*, we drop firm-year observations when ExecuComp reports less than four non-CEO executives.[7]

We also use CEOs' pre-exiting network ties with their appointees on the top-four list as another measure of CEO connectedness. We count the total number of network ties current CEOs and their

[7]Kim and Lu (2011) illustrate the importance of keeping the number of executives constant when constructing executive variables for panel regressions with firm fixed effects. Cross-checking against proxy statements shows that missing executives in ExecuComp are due to omission rather than to dismissal; hence, the restriction is unlikely to introduce a selection bias.

appointees have, *Exe_Tie*, through past employment (either working as an employee or serving on the board), educational institutions, and *past* membership to social and professional organizations. Similar measures have been used in previous papers (e.g., Cohen, Frazzini, and Malloy, 2008; Engelberg, Gao, and Parsons, 2013; Fracassi and Tate, 2012; Duchin and Sosyura, 2012). To avoid reverse causality, we include only network ties formed prior to joining the company. We also require that network ties are established during overlapping years for each category of network ties; in the education category, for example, the years a CEO and an executive attended the same school must overlap. We capture the depth of past connections by counting the number of network ties for each category. Then we sum the three types of ties to arrive at the total number of ties.

2.2.2. Difference-in-differences Estimation

Because both board and executive suite compositions are endogenous, we use an exogenous shock on board independence to investigate the interrelationship between board and executive suite independence. The shock is the requirement for a majority of independent directors for firms listed on NYSE and NASDAQ. The deadline for compliance was October 31, 2004; however, many firms that lacked a majority of independent directors began to change their board composition when the recommendations were promulgated by NYSE and NASDAQ in 2002. The largest changes occurred in 2002 and 2003 (see Chhaochharia and Grinstein, 2009, Table 1, Panel A; and Duchin et al., 2010, Figure 1.) Thus, we use 2001 as the base-year to define which firms are affected by the regulation and 2003 as the first year of the post-regulation period. We treat 2002 as the transition period and exclude observations during that year in estimating difference-in-differences.[8] The baseline specification is:

$$Y_{it} = a_i + a_t + \beta_1 Affected_i * Post_t + \beta_2 X_{it} + \varepsilon_{it} \qquad (1)$$

Y_{it} is a measure of firm i's current CEO's connectedness as of year t, as measured by either *FTA* or pre-existing social ties. $Affected_i$ is the treatment indicator, equal to one if firm i does not have a majority of independent directors in 2001, and zero otherwise. This indicator is interacted with $Post_t$, the post-regulation indicator, equal to one if year t is 2003 or later. The regression includes firm- and year fixed

[8] The results are robust to including 2002 observations in the pre-regulation sample.

effects, a_i and a_t. Because of these fixed effects, the specification does not contain a separate term for $Affected_i$ or $Post_t$. X is time-varying control variables. When estimation is based on an unmatched sample, standard errors are clustered at the firm level. The Appendix contains definitions of all variables.

In a difference-in-differences estimation, the outcome variable of the control group is used to calculate the expected counterfactual, assuming that the treatment and control groups have the same time trend if there are no regulatory changes. Thus, it is important to check whether FTAs of treated and control firms were following similar trends before the treatment. Figure 1 plots FTAs separately for our sample of treated and control firms from 1996 to 2006. Both groups show increasing FTAs, with remarkably parallel trends until 2002, when treated firms start to show sharper increases in FTA, narrowing the gap with control firms. The pre-regulation trends indicate that the parallel trends assumption is valid. Also noteworthy, control firms show higher FTAs than treated firms throughout the sample period, suggesting a negative correlation between board and executive suite independence.

2.2.3. Propensity-Score Matching

Although the parallel trends during the pre-shock period justify our difference-in-differences approach, to improve comparability with the treated firms, we construct a propensity-score matched control group following Rosenbaum and Rubin (1983). It is based on information in the base-year 2001. A probit model is estimated relying mostly on previous studies. Linck, Netter, and Yang (2008) argue board independence is related to firm complexity, costs of monitoring, ownership incentive, and CEO characteristics. We capture firm complexity by firm size, firm age,[9] and the number of segments within a firm; costs of monitoring by EBITDA/TA, Tobin's Q, and board size; ownership incentive by the percentage shares held by a CEO; and CEO characteristics by age, gender, founder status, and board chair status. In addition, we include the number of financial analysts covering the firm. Duchin et al. (2010) find an analyst-based variable is related to the likelihood of having a dependent board. We also include industry fixed effects to control for industry level unobserved factors.

[9] Boone, Field, Karpoff, and Raheja (2007) suggest that complexity increases with firm age, and Duchin et al. (2010) find that firm age is significantly related to the likelihood of having a dependent board.

Landier et al. (2013) show that a measure similar to FTA based on the fraction hired (rather than appointed) is related to CEO tenure,[10] an indicator for CEOs hired from outside, the average tenure of top-four non-CEO executives (*EXECSEN*), and the fraction of top-four executives appointed during a CEO's first year in office (*FTA_1Y*). We include these variables. We also include the fraction of executives whose first year as a top-four non-CEO executive can be identified from ExecuComp (*KNOWN*). This controls noise in *FTA* and *EXECSEN* arising from the ambiguity about the precise year of some of the top-four executive appointments.[11] Likewise, we add the fraction of top executives whom we cannot determine whether they are appointed during a CEO's first year in office (*FTA_1Y _Unknown*). This variable helps control noise in *FTA_1Y*.

Table A-1 in the Appendix reports estimation results of the Probit model used to compute the propensity score. Consistent with the estimates reported in Duchin et al. (2010) (Table 9, the second column), firm age, the number of segments, and the number of analysts all show negative relations to the likelihood of having a dependent board. Our estimation results also suggest that the likelihood of having a dependent board is higher when CEOs own more shares, top executives' average tenure is longer, and CEOs do not chair the board. Log likelihood, Prob > Chi^2, and Pseudo R^2 for the propensity score estimating regression are -282.31, 0.00, and 0.14, respectively. Propensity scores calculated from the estimates are then used to match one affected firm to the three nearest unaffected firms based on the Mahalanobis distance metric. We exclude all observations that do not satisfy the common support condition. We bootstrap 200 times to correct standard errors for all OLS regressions estimated with matched samples.

2.3. Sample Construction

[10] If a CEO leaves the position and returns later, ExecuComp reports only the latest appointment date. Thus simply comparing the CEO appointment date reported by ExecuComp with the current year may generate negative CEO tenure. We correct for this problem by backtracking the previous appointment year using the CEO and company names.

[11] If an executive is already one of the top four non-CEO executives at the firm's first appearance in ExecuComp, we cannot determine the year of her appointment. For such an executive, we use the year the executive joined the company as the year she was appointed as a top-four executive. This understates *FTA* and overstates *EXECSEN*, which is why *we* include *KNOWN* as a control variable.

Our sample is constructed with NYSE- and NASDAQ-listed firms for which we have information on board composition in 2001 from RiskMetrics and on executives from ExecuComp. Other data sources include Compustat for accounting data; CRSP for stock return data; and BoardEx for pre-existing social ties. To avoid ambiguity about who constitutes the current CEO, we drop firm-year observations when a new CEO's first year overlaps with the last year of the previous CEO.

The sample period covers 1996 through 2006, excluding 2002. We begin with 1996 to include sufficient pre-regulation observations. We stop after 2006 because RiskMetrics modified the definition of independent directors in 2007 to conform to the exchanges' definition, making it difficult to compare the level of board independence before and after 2007. In addition, 2008 was the beginning of the financial crisis, a rare event that led to unusual changes in the executive suite unrelated to the regulation.

Table 1 lists, by year, the number of firms in the full sample, which contains 8,975 firm-year observations associated with 1,035 unique firms. Columns (3) and (4) report the number of firms in the treatment and control group for the unmatched full sample. The number of firms increases over time due to greater firm coverage by ExecuComp in later years. Columns (5) - (7) show the propensity score matched sample. The number of affected firms in the matched sample is slightly smaller than that in the unmatched sample because some affected firms do not satisfy the common support condition. The number of unaffected firms is substantially fewer than three times the number of affected firms because of multiple matches to the same unaffected firms. Both unmatched and matched samples are not balanced. Reestimation results based on a balanced sample are similar.

2.4. *Descriptive Statistics*

Table 2, Panel A provides summary statistics for the unmatched full sample. The indicator *Affected* has a mean of 0.167, indicating 17% of our sample firms had a dependent board in 2001. The post regulation period indicator, *Post*, has a mean of 0.44, indicating fairly evenly distributed observations between pre- and post-regulation periods. The mean and median *FTA* are 0.44 and 0.50, implying about half of the top-four non-CEO executives are appointed during the current CEO's tenure.

Panel B compares affected and unaffected firms for both the unmatched and propensity-score matched samples at the time of matching, the base-year 2001. Both samples show significantly smaller FTA for firms with a dependent board than those with independent board in 2001, suggesting an inverse relation between board and executive suite independence. For most of the other variables, the difference between treated and control firms is smaller and less significant for the propensity score matched sample (Columns (8) and (9)) than for the unmatched sample (Columns (4) and (5)), indicating the propensity score matching improves the comparability between the treatment and control groups. The propensity score matched sample also shows insignificant differences between the treatment and control groups for the majority of firm and CEO characteristic variables, with a few exceptions that arise because the propensity score matching is based on the overall similarity.

3. CEO Connectedness in Executive Suites

In this section we estimate how CEOs' appointment-based connectedness in executive suites responds to the shock on board independence. Specifically, we examine how the regulation affects FTAs and pre-existing network ties CEOs have with their appointees on the top-four list. We also explore other possible explanations for our estimation results by taking a closer look at changes that took place in the executive suite during the mandated board transition.

3.1. Changes in FTA

We begin by estimating the impact of the independent board requirement on FTA. Control variables include time-varying firm and CEO characteristics that may influence FTA; hence, they are similar to those in the probit model used to construct the propensity score. Table 3 presents the estimation results. Odd-numbered columns report OLS estimates. The coefficient on *Affected*Post*, the estimated regulatory shock effect, is positive and significant at 1%, irrespective of whether the sample is unmatched or propensity-score matched. Ordered logistic regression estimates, reported in even-numbered columns, are consistent with the OLS results. All estimates imply mandating an independent board significantly increases FTA. The coefficient on *Affected*Post* in Column (1) implies that the regulation increased FTA

by 0.089. This is economically meaningful, when considering the treated firms' mean FTA in 2001 was 0.381 (See Table 2, Panel B).[12]

Most firms achieved higher FTA without forcing previous CEOs' appointees to leave the firm. If firms force out executives, they have to provide severance packages and may also lose valuable firm-specific knowledge. These costs can be avoided by increasing FTA without forcing costly resignations, for example, by promoting or hiring an executive to a position higher than previous CEOs' appointees who occupy the top-four list. A closer look at executives dropped from the top-four list during the mandated board restructuring reveals that the vast majority remained with the firm post-regulation.[13]

Coefficients on control variables are largely consistent across the four regressions. As expected, FTA is positively (negatively) related to the length of the CEO (the average non-CEO executives) tenure. The fraction of top executives appointed during a CEO's first year in office shows positive but mostly insignificant coefficients. Interestingly, founder CEOs and CEOs with greater share ownership are associated with lower FTA, while older CEOs tend to have higher FTA. Larger and older firms tend to have a higher FTA.

3.2. Other Explanations for Higher FTA

This evidence of a higher FTA indicates a shakeup in executive suites amid the mandated board restructuring. In this section we investigate whether the higher FTA can be explained by other stories related to turnovers in board rooms and executive suites.

3.2.1. Executive Inside Director Turnovers

When a dependent board becomes independent, some executive inside directors may lose board seats and leave the firm for better opportunities elsewhere. If they are previous CEOs' appointees on the

[12] Since there might be some time-variant omitted variables which are correlated with the treatment, we re-estimate the baseline regression in Table 3 while controlling for both firm- and year fixed effects and firm-specific time trend effects. Firm-specific time trend effects, constructed based on interaction terms between firm fixed effects and the time trend variable, can control for time-variant firm level omitted factors and allow each firm to have a different time trend. The results are robust.

[13] We identify 455 executives of the treated firms who were dropped from the top-four list between 2001 (pre-regulation) and 2004 (post-regulation). Of the 455, 327 executives remained with the firm, one died, 39 executives retired, and only 88 executives resigned from the firm as of 2004.

top-four list, FTA will increase. However, close examination of the mandated board restructuring by treated firms reveals that inside directors come out of the restructuring largely intact. On average, treated firms in our sample increased the fraction of independent directors from 0.36 in 2001 to 0.56 in 2004, but kept the board size more or less the same—9.06 directors in 2001 and 9.28 in 2004. Importantly, the higher fraction of independent directors is achieved mostly by replacing "affiliated" directors, outside directors with a material relationship with the firm.[14] Between 2001 and 2004, treated firms' average fraction of affiliated directors declined from 0.33 to 0.20, a reduction of 0.13. By contrast, the fraction of inside directors declined by only 0.07, from 0.32 in 2001 to 0.25 in 2004.

Among treated firms covered by both ExecuComp and RiskMetrics in 2001 and 2004, we identify 103 top-four non-CEO executives who sat on the board in 2001 but no longer served on the board in 2004. Of the 103, 84 are previous CEOs' appointees. However, the majority (47) remained with the firm as of 2004, with the rest leaving the firm due to death (1), retirement (22), and resignation (14). Notice that only 19 of 103 who lost board seats are current CEO's appointees, indicating that inside directors connected to the CEO though appointment decisions remain largely intact during the mandated board restructuring.

3.2.2. CEO Turnovers

If treated firms experience greater CEO turnovers and new CEOs bring their own team of top executives, their FTAs are likely to increase. Although this possibility is the reason all regressions control for *FTA_1Y*, the fraction of top-four non-CEO executives appointed within the year of a new CEO appointment, we estimate the effect of the board regulation on CEO turnovers with firm-level conditional logistic regressions with year dummies. The dependent variable is an indicator for CEO turnover, equal to one if a CEO in year *t* is not the same as the CEO in year *t-1*. Control variables include *Return*, one year

[14] Affiliated directors are non-independent outside directors, such as a provider of professional services (legal, consulting, or financial services) to the company; a customer of, or supplier to the company; a designee, such as a significant shareholder, under a documented agreement between the company and a group; a director who controls more than 50% of the company's voting power; a family member of an employee; a former employee of the company or of a majority-owned subsidiary; or an employee of an organization or institution that receives charitable gifts from the company.

buy-and-hold stock returns because CEO turnovers are likely to be related to firm performance. We also control for indicators for CEO_Chair and CEO-founder because CEOs' influence over the board and a founder status may enhance their ability to remain in the job. Other control variables include firm size, and CEOs' tenure and share ownership. All control variables are lagged by one year. The estimation results are reported in Table A-2 in the Appendix. The coefficient on *Affected*Post*, the estimated shock effect, is insignificant. The shakeup in the executive suite seems to be limited to non-CEO top executives. The higher FTA among treated firms cannot be explained by the CEO turnover story.[15]

3.2.3. Executive Tenure Effects

Previous CEOs' appointees tend to have longer tenure. If longer tenure makes executives more vulnerable during a shakeup in executive suites, it will lead to a higher FTA. To investigate this tenure-based explanation, we separate top-four non-CEO executives into current CEO appointees and previous CEO appointees. For each group, we estimate firm level conditional logistic regressions using the executive level data. The dependent variable is an indicator equal to one if an executive on the top-four list in year *t* is dropped from the list in year *t+1*. Control variables include firm size, *Return*, the CEO-founder indicator, the length of an executive's tenure on the top-four list, and her share ownership.

The estimation results are reported in Table 4.[16] Coefficients on the standalone executive tenure variable, *EXETEN*, in the first two columns imply that for previous CEOs' appointees, the longer the tenure, the more likely they will be dropped from the list. Surprisingly, the opposite holds for current CEOs' appointees; the longer the tenure, the more likely they will stay on the list. That is, whereas longer tenure works against previous CEOs' appointees, it helps current CEOs' appointees to maintain their position as one of the CEO's top lieutenants. The greater turnovers among previous CEOs' appointees could be due to their longer tenure relative to the current CEO's appointees. Thus, we restrict each

[15] In unreported regressions, we include a triple interaction, $Affected*Post*Return_{t-1}$, to control for possible changes in turnover-performance sensitivity for CEOs. The coefficients on the triple interaction term are significantly negative, indicating newly independent boards increase CEO turnover-performance sensitivity. More important, the coefficient on *Affected*Post* remains insignificant.

[16] Because the propensity score is matched at the firm level, not at the executive level, our estimation is based only on the unmatched sample.

subsample to only those executives whose tenure in the top-four list is longer than the sample median and reestimate the regressions. The results, reported in the last two columns in Table 4, are robust.

Furthermore, the interaction term, *Affected*Post*, shows positive and significant coefficients for previous CEOs' appointees, but insignificant (negative) coefficients for the current CEO's appointees, implying that the regulation increased the likelihood of turnovers for previous CEOs' appointees but not for the current CEO's appointees. This result holds even when we control for differences in tenure in the last two columns. The tenure-based story cannot explain the higher FTA. If anything, the opposite effects of tenure between previous and current CEOs' appointees demonstrate CEOs' strong influence on who occupies executive suites.

3.3. Changes in CEOs' Social Connections with Top Executives Appointed during their Tenure

If the higher FTA is a result of CEOs' attempts to increase their connectedness within executive suites, CEOs may opt for individuals with whom they are socially pre-connected. Prior social connection per se may not necessarily help obtain a position on the top-four list, but the CEO's familiarity acquired through prior social interactions helps to select individuals more talented and fit better to the executive team, more closely aligned, and less likely to dissent. The latter two attributes will help increase CEOs' influence over executives they appoint. To test this prediction, we estimate the regulatory impact on CEO-executive ties by estimating the baseline regressions for the total number of pre-existing network ties a CEO has with his appointees.

The dependent variable is log of one plus *Exe_Tie*, the total number of network ties current CEOs have with their appointees on the top-four list. CEO-executive ties are obtained by manually matching individual names in ExecuComp with those in BoardEx. BoardEx provides information for past employment, education background, and membership in social organizations (e.g., philanthropic and religious organizations, social clubs, and professional organizations). Coverage by BoardEx prior to 2000 is quite limited; however, the social ties for years 1996 to 1999 for individuals covered by BoardEX in later years can be obtained from BoardEx because it collects information about individuals by looking into the past. If a person is first covered in 2002, for example, her education, employment, and

organization membership history before 2002 is included in the database. Nevertheless, we reestimate the baseline regressions using network tie data only from 2000 and find the results are robust.

The control variables are the same as in Table 3, except we add *FTA* as a control because the number of network ties a CEO has with his appointees is likely to be greater the more executives appointed during his tenure. Information on network ties between an executive and the CEO is sometimes missing or incomplete because some relevant individuals are not covered by BoardEx. Even when both individuals are covered, the information provided in BoardEx could be insufficient to determine whether the connections occurred during overlapping years. To avoid reducing the sample size, we assign zero connection when the information is missing or incomplete. This leads to underestimation of network ties. To counter the underestimation problem, we include *Pct_Miss_FTA_Tie*, the percent of executives appointed by the current CEO for whom we have missing or incomplete information on their pre-existing network ties to their CEOs. This variable is set to zero when a firm-year observation shows no executives are appointed during a current CEO's tenure.

Table 5 reports estimation results. Both unmatched and matched samples show significant increases in CEOs' network ties with top executives appointed during their tenure. Social connections play a role in appointment decisions because they provide valuable information about personal abilities and character unavailable in the public domain. However, this should apply to both treatment and control groups and thus cannot explain why the network ties increase more at treated firms post-regulation.

4. CEOs' Social Connections in the Board

Increasing CEO influence in executive suites is not the only way treated firms counter the board regulation. They may circumvent the legal requirement by recruiting legally independent but socially dependent directors. In this section we investigate how the shock affects CEO-independent director social ties and how the changes interact with appointment-based CEO connectedness in executive suites.

4.1. CEOs' Social Connections with Independent Directors

We estimate the shock effect on CEO-director ties with the same baseline regression, with the dependent variable equal to the log of one plus total number of pre-existing network ties a CEO has with

independent directors, *Dir_Tie*, calculated following a procedure similar to that used in measuring CEO-executive ties. The control variables are the same as in Table 5, except for modifications necessary for switching from CEO-executive ties to CEO-director ties.[17]

The estimation results are reported in the first two columns of Table 6. Coefficients on *Affected*Post* are positive for both unmatched and matched samples, but significant only for the unmatched sample at the ten percent level. Estimators based on propensity score often generate more precise estimates in finite samples (Angrist and Hahn, 2004). Hence, we interpret the results providing weak evidence indicating some CEOs have sufficient influence on the appointments of independent directors even when nominating committees consist entirely of independent directors, and they are able to recruit more socially dependent independent directors in response to the regulation.[18]

4.2. CEO-director Social Connections vs. CEO-Executive Appointment-based Connections

If treated firms are able to circumvent the independent board requirement by recruiting socially-dependent independent directors, would they still increase the appointment-based CEO connectedness in executive suites? To answer this question, we compare CEO-independent director social ties between 2001 and 2004 for each firm to determine whether CEO-director ties increased after the regulation. Then we divide treated firms into those with increased CEO-director ties, *Increase_Tie*, and those with no increase in the ties, *No_Increase_Tie*, and reestimate the baseline regression for FTA while interacting these indicators with *Affected*Post*.

The estimation results are reported in the last two columns of Table 6. The effect of the regulation on FTA is positive and significant only when CEO-director ties do not increase. When CEO-director ties

[17] Specifically, we control for the number of independent directors, *Num_Ind_Dir*, instead of *FTA* because the number of pre-existing network ties is likely to increase with more independent directors. We also exclude variables specifically related to FTA; *FTA_1Y, FTA_1Y_Unknown, KNOWN, EXECSEN.*, and *Pct_Miss_FTA_1Y_Tie*. And instead of *Pct_Miss_FTA_Tie*, we include *Dir_Tie_Unknown*, the percent of independent directors whose pre-existing network ties to their CEOs are either missing or incomplete.

[18] Shivdasani and Yermack (1999) provide evidence of strong CEO involvement in director selection prior to the regulation. After the regulation, the nomination process of directors for shareholder approval became more independent, as NYSE and NASDAQ listing standards require that nominations for new independent directors be submitted solely by independent directors. Although this requirement encumbers CEOs' influence on the nomination process, CEOs may still exert influence on independent director selection because the nominating committee often needs help from management to identify potential candidates.

increase, the regulation has no effect on FTA. Firms do not seem to increase appointment-based CEO connectedness in executive suites when they are able to recruit socially-dependent independent directors to meet the regulatory requirement. This substitution effect might arise because some CEOs have insufficient influence on the nominating committee or cannot find qualified socially connected candidates.

To summarize, CEOs' social ties with independent directors also increase post-regulation, weakening the intensity of board monitoring. When CEOs can increase social ties with independent directors, they do not increase appointment-based connections with top executives. Interplays between board and executive suite independence take place not only through top executive appointment decisions but also through social dependency of independent directors.

5. Firm Performance Analyses

Do changes in executive suite independence in response to the regulation affect firm performance? To answer this question, we analyze how changes in FTA and the shock interactively affect firm performance. We measure operating performance by EBITDA divided by the book value of total assets, *EBITDA/TA*. Shareholder value is proxied by Tobin's Q, as measured by the sum of the market value of common stocks plus the book value of total liabilities divided by the book value of total assets. We compute their changes from pre-regulation to post-regulation periods, $\Delta EBITDA/TA$ and ΔQ, by taking difference in their averages for up to four years over 1998-2001 and 2003-2006. Changes in FTA, ΔFTA_i, are computed the same way. Only observations in which a CEO was the CEO in 2001 are included to avoid having different CEOs of a same firm in the estimation. To construct the changes, the panel data is collapsed to cross sectional data such that each observation is associated with one firm.

Control variables include changes in firm size, $\Delta Ln(TotalAssets)$; changes in firm risk as measured by idiosyncratic risk, $\Delta Risk$; and changes in board size, $\Delta Boardsize$. To avoid reducing the sample size due to missing observations, we set $\Delta Risk$ equal to zero if they are missing, and use a dummy variable, $\Delta RiskD$, which is set to one if $\Delta Risk$ is available and zero otherwise. We do not control for CEO characteristics because we have the same CEO for each firm. Because the regressions are cross-sectional, standard errors are clustered at the industry level based on one-digit SIC code.

Table 7 reports estimation results for the full sample in Columns (1) and (4). Coefficients on the standalone Δ*FTA* are significantly negative for Δ*EBITDA/TA* and negative but only marginally significant for Δ*Q*. These results are consistent with the finding in Landier et al. (2013) that the fraction of top executives hired by current CEOs is negatively related to firm performance.

The coefficients on the interaction term, Δ*FTA***Affected*, are insignificant for the full sample, which masks important differences in information environment across firms. Since our Prediction 2 posits firm performance implications of changes in FTA critically depend on outsiders' cost of acquiring information, we construct an information cost index in the base year 2001. It relies on three widely-used proxies for the difficulty in acquiring information: (1) The number of analysts in 2001 (source: I/B/E/S). Analyst coverage is widely considered an important form of external monitoring because it reduces information asymmetry (e.g., Hong, Lim, and Stein (2000); Brav and Lehavy (2003); Chang, Dasgupta, and Hilary (2006); Das, Guo, and Zhang (2006); and Kelly and Ljungqvist (2012)). (2) Asset intangibility in 2001, as measured by intangible assets divided by total assets. Intangible assets are considered to be more difficult to evaluate than tangible assets (e.g., Harris and Raviv, 1991). (3) Stock return volatility, as measured by the standard deviation of daily stock returns in 2001. Stock return volatility is commonly used to measure fundamental uncertainty (e.g., Boone et al., 2007), because assessing relevant firm-specific information is difficult when firm's performance is fundamentally uncertain.

Because these three factors have different scales, we normalize them by assigning a value of one to three based on which third a firm belongs to in each factor. Higher values indicate higher information costs (e.g., we assign three to the bottom-third in the number of analysts and to the top-third in the fraction of intangible assets and in stock return volatility). Since some of these factors may be correlated, we follow Armstrong et al. (2014) and employ the principle component analysis to construct the information cost index, *InfoCost_Index*, for all firms in 2001.

We use this information cost index to divide the sample into high- and low information cost subsamples using the sample median. Table 7, Columns (2) and (5) report reestimation results for the high information cost subsample. The coefficient on the standalone *Affected* is negative for both

Δ*EBITDA/TA* and Δ*Q* but significant only for Δ*EBITDA/TA*. These results are consistent with the findings in Duchin et al. (2010) that for firms in high information cost environments, forcing dependent boards to become independent hurts firm performance.

The variable of main interest, Δ*FTA*Affected,* shows positive and significant coefficients for both Δ*EBITDA/TA* and Δ*Q*, supporting Prediction 2 that increases in CEO connectedness with top executives help negate the negative shock effect on firm performance in a high information cost environment. For the low information cost subsample, by contrast, neither the standalone *Affected* nor Δ*FTA*Affected* shows a significant coefficient on either performance measure.

6. Confounding Effects and Other Robustness Tests

In this section we examine possible confounding effects of the enactment of the Sarbanes–Oxley Act (SOX) and other events which occurred around 2002. We also check robustness to an alternative measure of treatment effects, alternative definitions of FTA, organizational structure-changing events, and an alternative sample construction.

6.1. Sarbanes-Oxley Act of 2002

Our main concern is the Sarbanes-Oxley Act, which was enacted in 2002 while the independent board requirement proposal was under consideration by the SEC. If the SOX also contributed to the higher FTA, then we over-estimate the effects of the shock. To check whether and how our results are affected by the SOX, we conduct three tests: (1) placebo tests using firms exempted from the independent board requirement but subject to other SOX requirements; (2) reestimation of the baseline regression with a sample of firms exempted from Section 404 of the SOX but subject to the independent board requirement; and (3) an analysis of whether the treatment and control groups were affected differently by the SOX by comparing their financial transparency in the base year 2001.

6.1.1. Placebo Tests

To conduct placebo tests, we follow Armstrong et al. (2014) and use a sample of "controlled" firms. Firms are defined as controlled when more than 50% of the voting power in electing directors is held by an individual, a group, or another company. These firms are exempted from the independent

board requirement but are required to comply with other governance requirements including SOX (Armstrong et al., 2014); that is, they are not treated by the board regulation but were treated by the SOX and other possible confounding effects. We identify controlled firms by virtue of the 50% ownership rule as those in which directors and officers own more than 50% of the shares. As in Armstrong, Core, and Guay (2014), firms with dual class shares are also considered controlled firms. The D&O share ownership is obtained from Compact Disclosure, and firms with dual class shares are identified using the dataset provided in Andrew Metrick's website and described in Gompers, Ishii, and Metrick (2008).

Table 8, Panel A, reports placebo test results. The first column reports reestimation of Table 3 using only controlled firms. Here treated (control) firms are controlled firms with a non-majority (majority) of independent directors in 2001. Control variables are the same as in Table 3 but not reported. The coefficient on *Affected*Post* is virtually zero.[19] In the second column, we utilize the full sample, including controlled firms and non-controlled firms matched to controlled firms using the propensity-score matching process described earlier. We then add a triple interaction term, *Affected*Post*Exempt*, where *Exempt* is an indicator for controlled firms. The coefficient on the triple interaction term is negative and significant, indicating significantly lower FTA for controlled firms relative to non-controlled firms, which show significantly positive increases in FTA. The magnitudes of the negative and positive coefficients are similar, indicating the net effect on controlled firms is zero. That is, firms treated by the SOX but untreated by the board regulation show no increase in FTA, refuting the possibility that our results are affected by the SOX.

6.1.2. Reestimation with a Sample of Firms Exempted from Section 404 of the SOX

Another way to distinguish the effect of the board regulation from that of the SOX is to examine firms treated by the regulation but untreated by the most important component of the SOX, Section 404. Firms with public floats less than $75 million are exempt from Section 404, which is considered so rigorous (onerous) that Gao, Wu, and Zimmerman (2009) argue it provides an unintended incentive for

[19] Our sample of controlled firms is smaller than that in Armstrong et al. (2014) because we require coverage by both ExecuComp and RiskMetrics.

small firms to stay small. If the higher FTA was driven by the SOX, this subsample of firms should exhibit little or no treatment effects. We define public float as the market value of equity held by non-affiliates of the issuer, estimating it by the market value of common equity multiplied by (1 - D&O share ownership). We use the market cap as of the end of the second quarter of the fiscal year of 2002. [20] When D&O ownership is missing, we use the 2002 sample mean of 14%. This process yields 30 firms with estimated public floats less than $75 million.

The reestimation results of the baseline regression for this small subsample are reported in Panel B of Table 8. To make full use of the limited sample, estimation in Column (1) is done without any control variables other than firm and year fixed effects. Column (2) is estimated with the same set of control variables as before. We use only the unmatched subsample because the propensity-score matched subsample contains only six firms. In spite of the small sample size, difference-in-differences estimates for FTA remain positive and significant in both specifications.

6.1.3. Pre-SOX Financial Transparency

Finally, we check whether the accuracy of earnings reports differs between firms with dependent and independent boards in the base year 2001. Because the main focus of the SOX is to improve transparency through more accurate financial disclosure, for our difference-in-differences estimates to be driven by the SOX effects, in 2001 treated firms had to be more opaque with less accurate earnings reports than the control group. So we check whether firms with dependent boards had less accurate earnings reports than the control group in 2001 by comparing estimates of earnings management done by the treatment and control groups. A commonly-used measure of earnings management is discretionary accruals (DAC), those parts of total accruals over which management has discretion. Total accruals are computed as the difference between earnings and operating cash flows.[21] To estimate the discretionary

[20] On September 4, 2002, the SEC adopted final rules regarding the acceleration of filing deadlines, wherein the definition of non-accelerated filers is an issuer with a public float less than $75 million on the last business day of the most recent second fiscal quarter.

[21] Specifically, they are the change between non-cash current assets minus the change in current liabilities, excluding those due to the maturation of the firm's long-term debt, minus depreciation and amortization, scaled by total assets in the previous fiscal year.

components of total accruals, we follow Dechow, Sloan, and Sweeney (1995) by regressing total accruals on the inverse of total assets in the previous fiscal year; the change in sales less the change in accounts receivable; and property, plant, and equipment. Discretionary accruals (DAC) are the regression residuals.

For each firm in our sample, we calculate this measure of DAC in 2001 and compute its correlation with the dependent board indicator in 2001. The idea is to determine whether treated firms engaged in more earnings management in 2001 than the control group. We find no such evidence: The correlation between the dependent board indicator and DAC in 2001 is 0.023 with *P*-value equal to 0.512, indicating no correlation.

6.2. Other Possible Confounding Events

Other events occurred around the time the board regulation was promulgated and they may have affected the market for top executives. For example, the dotcom bubble burst in 2000 and the 9/11 attack in 2001 may have led to fewer top executive hires and promotions during 2000-2002, lowering FTA during 2000-2002, in turn leading to the appearance of higher FTA in later years. If treated firms are more affected by these confounding effects, then our results will be overestimated.

We check this possibility by following the approach used in Bertrand and Mullainathan (2003), replacing the post-regulation indicator, *Post*, with year dummies *2000*, *2001*, *2003*, *2004*, and *2005 and after*. Panel C of Table 8 reports the reestimation results. The coefficients on the interaction of *Affected* and year dummies are all insignificant for pre-regulation years. For post-regulation years, in contrast, the interaction terms show positive and mostly significant coefficients.

The coefficients for post-regulation years show an increasing trend over time, consistent with Figure 1, which shows that the gap in FTA between the control and treated groups narrows gradually over time. These trends reflect the time it takes to change the composition of executives on the top-four list through promotions and new hires.

6.3. Different Degrees of Treatment

Our difference-in-differences estimates are based on a treatment indicator, $Affected_i$, which may be too crude. Consider two treated firms, one with 40% and another with 10% of independent directors in

2001. Clearly, the latter is more affected by the regulation and may react more strongly. We reestimate the baseline regression for FTA with the interaction of the percentage of non-independent directors in 2001, $Pct\_Affected_i$, and the post-regulation indicator, *Post*. Table 9, Panel A, reports the results; they indicate that the more affected by the regulation, the greater the increase in FTA, irrespective of whether the sample is unmatched or matched.

6.4. Alternative Definitions of FTA

Our measure of FTA treats all top-four non-CEO executives equally. However, executives with higher salaries and bonuses tend to be higher ranked and more influential. Since CEO connections with more influential executives may matter more, we calculate a compensation-weighted FTA,

$$WFTA_{it} = \sum_{k=1}^{k=n} Exe\_Com_{kit} / \sum_{j=1}^{j=4} Exe\_Com_{jit}. \quad (2)$$

Exe_Com_{kit} is the sum of salaries and bonuses of executive *k* appointed during the tenure of firm *i*'s CEO as of year *t*, and *n* is the number of top executives appointed during the CEO's tenure.

In addition, we follow Landier et al. (2013) and estimate residuals of a regression relating *FTA* to *CEOTEN, OUTSIDE, EXECSEN, KNOWN, FTA_1Y,* and *FTA_1Y_Unknown* with year fixed effects. The residuals are used as a measure of the abnormal fraction of top executives appointed, *AFTA*, during a CEO's tenure. We also calculate *AFTA* weighted by executives' salaries and bonuses, *WAFTA*, by estimating the same regression with *WFTA* as the dependent variable. When these abnormal measures of *FTA* are used as dependent variables, we do not include independent variables used to estimate the residuals as control variables. Reestimation results based on these three alternative measures of *FTA* are reported in Panel B of Table 9. The regulatory impact variable shows a highly significant coefficient regardless of which alternative measure is used.

6.5. Are Results Driven by Corporate Organizational Structure-Changing Events?

Organizational structure-changing events such as mergers and acquisitions, divestitures, and spinoffs are often accompanied by changes in the executive suite, which is one reason we control for the number of segments in all regressions. As a further robustness check, we control for the number of

mergers and acquisitions, MA_{it-1}, and divestitures and spinoffs, DS_{it-1}, completed in the prior year. The data for *MA* and *DS* are obtained from SDC. The results (unreported) are robust.

6.6. Alternative Sample Construction

Finally, we check the robustness to possible biases arising from an unbalanced sample by reestimating regressions with a balanced sample of 830 firms that exist over the period 1999 through 2006. We exclude observations in 2002 and 2003 so that the pre- and post-regulation periods comprise the same number of years (three years in each). In this sample, 2004 is the first year fully affected by the regulation. The rest is the same. The reestimation results (unreported) are robust.

7. Conclusions

We find that when firms experience an external shock to increase board independence, they respond by increasing CEO connectedness in the other governing body, the executive suite. The spillover is not limited to the executive suite. The shock also seems to weaken social independence of the board. Interestingly, these spillovers do not occur at the same time. When treated firms increase CEO-independent director social ties, they do not increase CEO connectedness in executive suites, suggesting they are substitutes.

Our findings illustrate how the information environment influences the interplay between the two governing bodies. Harris and Raviv (2008) and Duchin et al. (2010) show that an insider-controlled board is optimal when insider information is crucial for firm profitability and when outsiders' cost of acquiring information is high. When firms are subject to such information environments and choose a dependent board, forcing it to become independent via regulation can damage firm performance and shareholder value. The good news, however, is that targeted firms neutralize the harmful effects by making executive suites more closely connected to the CEO. Indeed, our performance analyses indicate that when information cost is high, increasing CEO connectedness in executive suites helps negate the negative effect of the regulation. In general, whether weakening executive suite independence is good or bad for shareholders depends on whether a dependent board was optimal or sub-optimal prior to the shock, which in turn depends on its information environment.

These findings have clear implications for investors, corporate governance specialists, and policy makers: Inferring the overall independence of a firm's governing process by board independence alone is misleading. In addition, when policy makers target a specific governance mechanism for improvement, they must be aware their regulatory actions could spill over to other governing bodies and other aspects of governance within the targeted governance mechanism. Such spillovers are often difficult to predict and some could be harmful. Thus, when it comes to governance reforms, the "comply or explain" approach taken in many European, and some Asian, countries presents an appealing feature absent in the legally binding regulation approach typically taken in the U.S.[22]

Finally, a caveat is in order. We consider only top echelon employees of the firm—top executives and board members. The board and the executive suite are not the only governing bodies. Non-managerial employees also influence the governing process through their working relationships with management (Bertrand and Mullainathan, 2003), the collective bargaining process (Atanassov and Kim, 2009), and worker productivity and involvement in anti-takeover activities (Kim and Ouimet, 2014). The possible dynamics among the three governing bodies, with the board representing shareholders, employees representing their own collective interest, and the executive suite managing often conflicting interests between capital providers and labor suppliers, are intriguing and complex. More research is needed on the three-way dynamics for a more comprehensive understanding of how the interdependence between the three groups affect a firm's overall governing process.

[22] See Kim and Lu (2013) for detailed documentation of major corporate governance reforms undertaken by 26 advanced and emerging economies.

Appendix:

Variable Descriptions.

Board Composition Variables	
Affected	Dependent board indicator equal to one if a firm does not have a majority of independent directors in 2001; zero, otherwise.
Pct_Dep_Dir	The percentage of non-independent directors on the board in 2001.
Executive Suite Composition Variables	
FTA	Fraction of top-four non-CEO executives appointed during a current CEO's tenure.
AFTA	Abnormal fraction of top-four non-CEO executives appointed during a current CEO's tenure.
WFTA	Fraction of top-four non-CEO executives appointed during a current CEO's tenure, weighted by the sum of executives' salaries and bonuses.
WAFTA	Abnormal fraction of top-four non-CEO executives appointed during a current CEO's tenure, weighted by the sum of executives' salaries and bonuses.
Ln(Exe_Tie+1)	Logged value of *Exe_Tie* plus one. *Exe_Tie* is the total number of pre-existing network ties a CEO has with top-four non-CEO executives appointed during his tenure through past employment (either working as an employee or serving on the board), educational institutions, and *past* membership to social and professional organizations. Only network ties established during overlapping years are included.
Ln(Dir_Tie+1)	Logged value of *Dir_Tie* plus one. *Dir_Tie* is the total number of pre-existing network ties a CEO has with his firm's independent directors through past employment (either working as an employee or serving on the board), educational institutions, and *past* membership to social and professional organizations. Only network ties established during overlapping years are included.
Other Variables	
Post	Post-regulation indicator, equal to one if year *t* is 2003 or thereafter; zero, otherwise.
InfoCost_Index	An index for the initial level of outsiders' cost of acquiring firm specific information in 2001. It consists of three factors related to the difficulty in acquiring firm-specific information: (1) the number of analysts covering the firm; (2) intangible assets divided by total assets; and (3) stock return volatility. Each factor is normalized to the value of one to three based on which third it belongs (e.g., the maximum value of three is assigned to the bottom third in the number of analysts and to the top third in the fraction of intangible assets and in stock return volatility). Then we employ the Principle Component Analysis to construct InfoCost_Index for all firms in 2001. Higher index indicates higher cost for outsiders to acquire firm-specific information.
OUTSIDE	Outsider indicator equal to one, if a CEO comes from outside the firm; zero, otherwise.
FTA_1Y	Fraction of top-four non-CEO executives appointed within the year of a new CEO appointment.
FTA_1Y_Unknown	Fraction of top-four non-CEO executives whose information on whether they are appointed within the year of a new CEO appointment is unknown.
KNOWN	Fraction of executives whose first year on the list of the top-four non-CEO executives can be identified with data in ExecuComp.
EXECSEN	Average tenure of top-four non-CEO executives.
Female	Indicator equal to one if a CEO is female; zero, otherwise.
Ln(CEOAge)	Logged value of CEO age.
CEO_OWN	Percentage of outstanding common shares held by a CEO.

CEO_Chair	Indicator equal to one if a CEO chairs the board; zero, otherwise.
CEO_Founder	Indicator equal to one if a CEO is a founder; zero, otherwise.
Ln(TotalAssets)	Logged book value of total assets in 2000 US million dollars.
FirmAge	One plus the number of years from the firm's IPO or the number of years since its first appearance in CRSP.
Segment	Number of business segments a firm has in a given year as reported by Compustat/Segment.
Pct_Miss_FTA_Tie	Percent of top-four non-CEO executives appointed during a CEO's tenure whose network ties to their CEO are missing or incomplete.
Tobin's Q	The market value of common equity plus the book value of total liabilities divided by the book value of total assets.
EBITDA/TA	Earnings before interest, tax, depreciation, and amortization divided by the book value of total assets.
Risk	The standard error of the residuals from a CAPM model estimated using daily data over the concurrent year. It is equal to zero, if data to construct *Risk* are unavailable.
RiskD	Dummy variable equal to one if the data required to estimate *Risk* are available; zero, otherwise.
Boardsize	Total number of directors on the board.

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Figure 1: Time Trends of FTAs of Firms Affected and Unaffected by the Independent Board Regulation.

This figure plots the yearly mean FTAs of firm affected and unaffected during the sample period of 1996 through 2006.

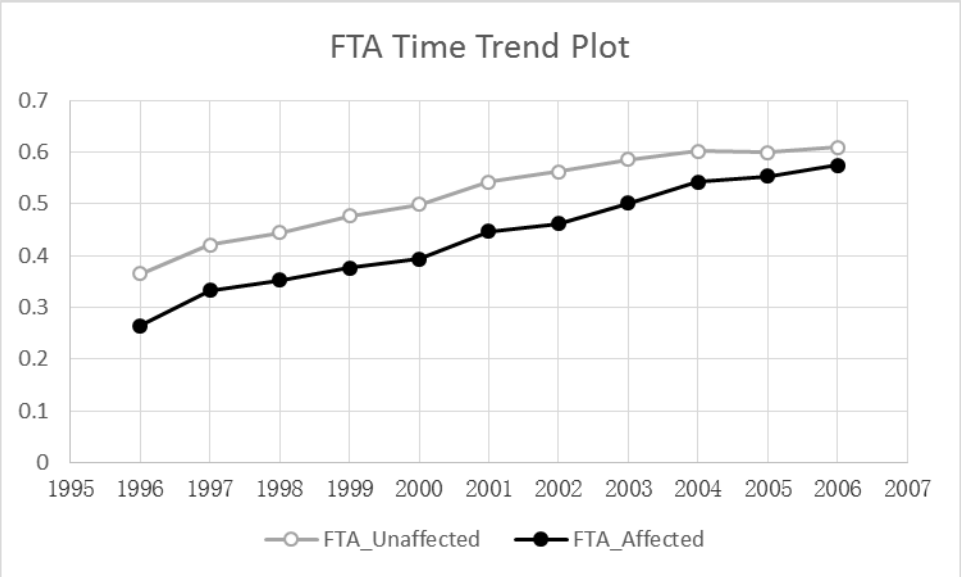

Table 1: Sample Description.

This table shows, by year, the number of observations for unmatched and propensity-score matched samples. Columns (2) and (5) report the number of firms for which we have information on both board composition in 2001 and the fraction of top-four non-CEO executives appointed (FTA) during a CEO's tenure. Columns (3) and (6) report the number of firms without a majority of independent directors in 2001 and, hence, affected by the regulation. Columns (4) and (7) show the number of firms unaffected by the regulation.

	Unmatched Sample			Propensity-score Matched Sample		
Year	**Full**	**Affected**	**Unaffected**	**Full**	**Affected**	**Unaffected**
(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
1996	691	106	585	267	102	165
1997	755	113	642	292	108	184
1998	825	128	697	332	123	209
1999	913	147	766	368	143	225
2000	946	156	790	382	151	231
2001	944	157	787	387	155	232
2003	958	164	794	378	150	228
2004	969	171	798	382	152	230
2005	978	171	807	378	150	228
2006	996	182	814	374	152	222
Total	8,975	1,495	7,480	3,540	1,386	2,154

Table 2: Descriptive Statistics.

Panel A reports summary statistics of key variables for the full sample. Panel B compares affected firms with unaffected firms in the unmatched and the propensity-score (PS) matched sample, respectively. The comparison is based on the mean values of variables in the base-year 2001. Definitions of the variables are provided in the Appendix.

Panel A: Summary Statistics for the Full Sample

Variables	Mean	Median	Std. Dev.	Min	Max
(1)	(2)	(3)	(4)	(5)	(6)
Board Composition Variables					
Affected	0.167	0.000	0.373	0.000	1.000
Pct_Dep_Dir	0.351	0.333	0.176	0.063	1.000
Executive Suite Composition Variables					
FTA	0.437	0.500	0.334	0.000	1.000
Ln(Exe_Tie+1)	0.064	0.000	0.237	0.000	2.639
Pct_Miss_FTA_Tie	0.041	0.000	0.181	0.000	1.000
Other Variables					
Post	0.435	0.000	0.496	0.000	1.000
CEOTEN	6.920	5.000	7.334	0.000	55.000
OUTSIDE	0.142	0.000	0.349	0.000	1.000
CEO_Founder	0.090	0.000	0.286	0.000	1.000
FTA_1Y	0.538	0.500	0.399	0.000	1.000
KNOWN	0.973	1.000	0.093	0.000	1.000
EXECSEN	4.350	4.000	1.872	0.000	14.750
FTA_1Y_Unknown	0.001	0.000	0.021	0.000	0.500
Female	0.013	0.000	0.113	0.000	1.000
Ln(CEOAge)	4.015	4.025	0.135	3.466	4.511
CEO_OWN	0.023	0.003	0.060	0.000	0.638
CEO_Chair	0.657	1.000	0.475	0.000	1.000
Ln(TotalAssets)	7.765	7.599	1.657	2.227	14.291
FirmAge	26.830	23.000	19.571	1.000	82.000
Segment	15.036	14.000	9.695	1.000	87.000
Tobin's Q	2.195	1.564	2.899	0.475	105.090
Risk	0.022	0.019	0.013	0.000	0.196
RiskD	0.936	1.000	0.245	0.000	1.000
Boardsize	9.795	9.000	2.842	4.000	30.000
Ln(Dir_Tie+1)	0.481	0.000	0.702	0.000	3.332
Dir_Tie_Unknow	0.048	0.000	0.158	0.000	1.000

Panel B: Comparison between Affected firms and Unaffected firms in Unmatched and PS-matched Samples.

	Unmatched Sample				PS-matched Sample			
	Affected	**Unaffected**	**(2)-(3)**	**P-Value**	**Affected**	**Unaffected**	**(6)-(7)**	**P-Value**
(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**
FTA	0.381	0.470	-0.090	0.002	0.381	0.457	-0.076	0.019
Ln(Exe_Tie+1)	0.070	0.061	0.009	0.673	0.070	0.069	0.002	0.952
Pct_Miss_FTA_Tie	0.017	0.029	-0.012	0.334	0.017	0.023	-0.006	0.649
CEOTEN	9.847	6.050	3.798	0.000	9.594	6.784	2.809	0.002
OUTSIDE	0.191	0.141	0.050	0.108	0.181	0.129	0.051	0.166
CEO_Founder	0.205	0.070	0.136	0.000	0.195	0.121	0.074	0.048
FTA_1Y	0.572	0.512	0.059	0.083	0.573	0.524	0.049	0.235
KNOWN	0.971	0.976	-0.004	0.589	0.973	0.986	-0.013	0.089
EXECSEN	4.909	4.193	0.716	0.000	4.880	4.348	0.532	0.004
FTA_1Y_Unknown	0.002	0.002	0.000	0.999	0.002	0.000	0.002	0.222
Female	0.006	0.014	-0.008	0.438	0.006	0.013	-0.006	0.538
Ln(CEOAge)	4.034	4.002	0.032	0.011	4.032	3.997	0.035	0.019
CEO_OWN	0.052	0.019	0.033	0.000	0.048	0.021	0.027	0.000
CEO_Chair	0.605	0.685	-0.080	0.052	0.600	0.655	-0.055	0.271
Ln(TotalAssets)	7.400	7.813	-0.414	0.004	7.420	7.443	-0.023	0.881
FirmAge	19.541	27.314	-7.773	0.000	19.594	21.940	-2.346	0.128
Segment	14.929	17.949	-3.020	0.000	15.046	16.767	-1.721	0.036
Tobin's Q	2.127	2.034	0.093	0.510	2.131	1.882	0.249	0.074
EBITDA/TA	0.133	0.122	0.012	0.248	0.134	0.135	-0.001	0.897
Risk	0.032	0.030	0.002	0.123	0.032	0.032	-0.001	0.657
RiskD	0.936	0.939	-0.003	0.898	0.935	0.991	-0.056	0.002
Boardsize	9.599	9.668	-0.070	0.787	9.626	9.392	0.234	0.411
Ln(Dir_Tie+1)	0.193	0.557	-0.364	0.000	0.195	0.428	-0.232	0.000
Dir_Tie_Unknow	0.005	0.019	-0.014	0.131	0.005	0.019	-0.014	0.128

Table 3: Impact of the Independent Board Requirement on the Fraction of Top Executives Appointed (FTA) during a CEO's Tenure.

This table reports estimates of the impact of the independent board requirement on the fraction of top-four non-CEO executives appointed (FTA) during a CEO's tenure. Columns (1)-(2) and (3)-(4) report estimation results with the unmatched and propensity-score (PS) matched sample, respectively. The sample period is 1996 – 2006, excluding 2002. Columns (1) and (3) are OLS estimates; Columns (2) and (4), estimates by ordered logistic regressions. Definitions of all variables are provided in the Appendix. Regressions in Columns (1) and (3) control for year- and firm fixed effects and regressions in Columns (2) and (4) control for year- and firm dummies. The regression does not include Affected and Post as separate controls because of firm- and year fixed effects. Robust standard errors reported in parentheses are clustered at the firm level in Columns (1)-(2) and are corrected by bootstrapping 200 times in Columns (3)-(4). Coefficients marked with *, **, and *** are significant at 10%, 5%, and 1%, respectively.

	FTA			
	Unmatched		**PS-Matched**	
	OLS	**Ologit**	**OLS**	**Ologit**
VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**
Affected*Post	0.089***	1.049***	0.043***	0.444**
	(0.022)	(0.307)	(0.014)	(0.222)
CEOTEN	0.023***	0.390***	0.020***	0.375***
	(0.003)	(0.055)	(0.002)	(0.052)
OUTSIDE	0.012	-0.053	-0.069**	-1.506***
	(0.033)	(0.479)	(0.035)	(0.511)
CEO_Founder	-0.313***	-4.642***	-0.184***	-2.946***
	(0.064)	(0.997)	(0.044)	(0.739)
FTA_1Y	0.051	0.468	0.066*	0.842
	(0.037)	(0.577)	(0.037)	(0.571)
KNOWN	-0.306***	-4.395***	-0.462***	-7.878***
	(0.059)	(0.806)	(0.058)	(0.899)
EXECSEN	-0.092***	-1.375***	-0.096***	-1.619***
	(0.003)	(0.063)	(0.003)	(0.071)
FTA_1Y_Unknown	0.818	5.944	-0.617***	-13.317***
	(0.594)	(19.557)	(0.230)	(3.494)
Female	-0.092	-1.411	0.018	-0.131
	(0.094)	(1.302)	(0.115)	(1.920)
Ln(CEOAge)	0.268***	3.896***	0.234***	2.512**
	(0.095)	(1.318)	(0.080)	(1.191)
CEO_OWN_{t-1}	-0.277*	-3.658	-0.541***	-7.738***
	(0.161)	(2.273)	(0.158)	(2.619)
CEO_Chair	0.023	0.193	0.023	0.034
	(0.028)	(0.406)	(0.025)	(0.457)
$Ln(TotalAsset)_{t-1}$	0.045**	0.754***	0.049***	1.001***
	(0.018)	(0.202)	(0.014)	(0.192)
FirmAge	0.003**	0.033**	0.003*	0.049**
	(0.001)	(0.016)	(0.002)	(0.024)
$Segment_{t-1}$	-0.001	-0.016	0.000	0.004
	(0.001)	(0.011)	(0.001)	(0.012)
Constant1	-0.288	4.063	0.013	-5.552
	(0.427)	(5.449)	(0.316)	(5.010)
Constant2		7.084		-2.299
		(5.458)		(5.000)
Constant3		10.212*		1.412
		(5.462)		(4.981)
Constant4		13.857**		5.631
		(5.476)		(4.966)
Firm FE & Year FE (Dummies)	Y	Y	Y	Y
Observations	6,304	6,304	2,695	2,695
Adjusted R^2	0.701	(0.4663)	0.742	(0.5166)

Table 4: The Likelihood an Executive Will Be Dropped from the Top-Four List: Current CEOs' Appointees vs. Previous CEOs' Appointees.

This table estimates the impact of the independent board requirement on the likelihood a top-four non-CEO executive will be dropped from the top-four list, separately for current CEOs' appointees, AppointCurCEO, and previous CEOs' appointees, AppointPrevCEO. The dependent variable is an indicator equal to one if an executive on the top-four list in year t is not on the list in year t+1. Columns (1) and (2) utilize the full executive panel data, while Columns (3) and (4) include only executives whose tenure on the list of top-four non-CEO executives are above the sample median, EXETEN > Median. The sample period is 1996 – 2006, excluding 2002. Definitions of all variables are provided in the Appendix. All regressions are estimated by the firm level conditional logistic regressions and control for year dummies. Robust standard errors reported in parentheses are clustered at the firm level. Coefficients marked with *, **, and *** are significant at 10%, 5%, and 1%, respectively.

	Exe_Turnover			
	AppointCurCEO	**AppointPrevCEO**	**AppointCurCEO EXETEN > Median**	**AppointPrevCEO EXETEN > Median**
VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**
Affected*Post	-0.009	0.249**	-0.008	0.363***
	(0.132)	(0.111)	(0.281)	(0.124)
EXETEN	-0.012**	0.027***	-0.001	0.032***
	(0.005)	(0.004)	(0.009)	(0.004)
EXE_OWN	1.521	-1.798	-0.185	-1.413
	(1.070)	(1.185)	(0.973)	(1.178)
Return	-0.150***	-0.087***	-0.273***	-0.109***
	(0.049)	(0.025)	(0.096)	(0.037)
Ln(TotalAsset)	0.140**	0.160***	0.085	0.096
	(0.063)	(0.059)	(0.162)	(0.074)
CEO_Founder	0.552***	0.556***	0.317	0.365**
	(0.194)	(0.126)	(0.440)	(0.170)
Year Dummies	Y	Y	Y	Y
Observations	15,100	19,816	4,590	14,258
Prob> Chi^2	0	0	0	0
Wald	58.99	152.5	40.53	127.0
pseudo-R^2	0.00509	0.00977	0.0120	0.0125

Table 5: Impact of the Independent Board Requirement on the Social Ties between a CEO and Top-four Non-CEO Executives Appointed during the CEO's Tenure.

This table estimates the impact of the independent board requirement on the social ties between a CEO and top-four non-CEO executives appointed during the CEO's tenure. Columns (1) and (2) report estimation results with the unmatched and propensity-score (PS) matched sample, respectively. The sample period is 1996 – 2006, excluding 2002. Definitions of all variables are provided in the Appendix. All regressions control for firm- and year fixed effects. The regression does not include Affected and Post as separate controls because of firm- and year fixed effects. Robust standard errors reported in parentheses are clustered at the firm level in Columns (1) and are corrected by bootstrapping 200 times in Columns (2). Coefficients marked with *, **, and *** are significant at 10%, 5%, and 1%, respectively.

VARIABLES	**Ln(Exe_Tie+1)**	
	Unmatched	**PS-Matched**
	(1)	**(2)**
Affected*Post	0.038*	0.051***
	(0.020)	(0.017)
CEOTEN	0.006***	0.007***
	(0.002)	(0.002)
OUTSIDE	0.053**	0.046
	(0.023)	(0.029)
CEO_Founder	0.043	0.026
	(0.033)	(0.035)
FTA_1Y	-0.001	0.026
	(0.025)	(0.023)
FTA_1Y_Unknown	-0.272	-0.046
	(0.253)	(0.180)
KNOWN	-0.058	-0.150**
	(0.053)	(0.071)
EXECSEN	-0.004	-0.006
	(0.003)	(0.005)
Female	0.085***	-0.015
	(0.031)	(0.015)
Ln(CEOAge)	-0.042	-0.068
	(0.063)	(0.083)
CEO_OWN_{t-1}	-0.070	-0.150
	(0.107)	(0.146)
CEO_Chair	-0.022	-0.016
	(0.013)	(0.018)
$Ln(TotalAsset)_{t-1}$	0.018**	0.017
	(0.009)	(0.014)
FirmAge	-0.000	-0.003
	(0.002)	(0.002)
$Segment_{t-1}$	-0.001	-0.000
	(0.001)	(0.001)
Pct_Miss_FTA_Tie	-0.023	-0.031
	(0.021)	(0.029)
FTA	0.034*	-0.001
	(0.021)	(0.025)
Constant	0.125	0.403
	(0.282)	(0.383)
Firm FE & Year FE	Y	Y
Observations	6,304	2,695
Adjusted R^2	0.436	0.471

Table 6: CEO-Independent Director Social Ties, the Independent Board Requirement, and the Fraction of Top Executives Appointed (FTA) during the CEO's Tenure.

This table estimates the impact of the independent board requirement on pre-existing network ties between the CEO and independent directors and how the impact on FTA is related to the changes in CEO-director ties. The first two columns estimate the impact of the regulation on CEO-independent director social ties. The last two columns estimate of the interactive effects of the regulation and changes in CEO-director ties on FTA. Increase_Tie is an indicator equal to one, if CEO-director ties increased from 2001 to 2004; No_Increase_Tie is an indicator equal to one, if there is no increase in CEO-director ties over the same period. The sample period is 1996 – 2006, excluding 2002. Definitions of all variables are provided in the Appendix. All regressions control for firm- and year fixed effects. The regression does not include Dep_Board2001 and Post as separate controls because of firm- and year fixed effects. Robust standard errors reported in parentheses are clustered at the firm level in Columns (1) and (3) and are corrected by bootstrapping 200 times in Columns (2) and (4). Coefficients marked with *, **, and *** are significant at 10%, 5%, and 1%, respectively.

	Ln(Dir_Tie+1)		**FTA**	
	Unmatched	**PS-matched**	**Unmatched**	**PS-matched**
VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**
Affected*Post	0.021	0.041*		
	(0.042)	(0.024)		
Increase_Tie*Affected*Post			0.044	-0.012
			(0.089)	(0.047)
No_Increase_Tie*Affected*Post			0.090***	0.040**
			(0.022)	(0.016)
Increase_Tie*Post			-0.002	0.036
			(0.036)	(0.034)
CEOTEN	0.005	0.006***	0.023***	0.019***
	(0.004)	(0.002)	(0.003)	(0.002)
OUTSIDE	0.035	0.058	-0.004	-0.105***
	(0.063)	(0.037)	(0.035)	(0.032)
CEO_Founder	-0.190**	-0.198***	-0.299***	-0.148***
	(0.096)	(0.050)	(0.069)	(0.044)
FTA_1Y			0.023	0.054
			(0.038)	(0.038)
KNOWN			-0.319***	-0.484***
			(0.064)	(0.051)
EXECSEN			-0.090***	-0.095***
			(0.003)	(0.003)
FTA_1Y_Unknown			-0.103	-0.828***
			(0.591)	(0.256)
Female	0.136	0.449***	-0.032	0.236
	(0.119)	(0.145)	(0.134)	(0.144)
Ln(CEOAge)	0.621***	0.552***	0.311***	0.346***
	(0.179)	(0.087)	(0.103)	(0.081)
CEO_OWN_{t-1}	0.475**	0.670***	-0.294	-0.649***
	(0.237)	(0.211)	(0.194)	(0.200)
CEO_Chair	-0.034	-0.038	0.014	0.014
	(0.051)	(0.027)	(0.029)	(0.027)
$Ln(TotalAsset)_{t-1}$	0.009	0.011	0.038*	0.037***
	(0.028)	(0.020)	(0.020)	(0.014)
FirmAge	0.004	0.004	0.003**	0.003*
	(0.003)	(0.003)	(0.001)	(0.002)
$Segment_{t-1}$	0.001	0.000	-0.001	0.000
	(0.002)	(0.001)	(0.001)	(0.001)
Dir_Tie_Unknow	-0.461***	-0.355***		
	(0.127)	(0.058)		
Num_IndDir	0.048***	0.061***		
	(0.008)	(0.005)		
Constant	-2.647***	-2.547***	-0.395	-0.301
	(0.741)	(0.372)	(0.469)	(0.312)
Firm FE & Year FE	Y	Y	Y	Y
Observations	5,739	2449	5,589	2,448
Adjusted R^2	0.795	0.786	0.698	0.745

Table 7: Performance Analyses.

This table estimates how changes in FTA, ΔFTA, attributable to the independent board requirement affect firm performance, separately for high- and low information cost environment. The dependent variables are changes in EBTDA/TA in Columns (1)-(3); Tobin's Q in Columns (4)-(6). The sample is cross-sectional and each observation is the difference in the averages of up to four years over 1998-2001 and 2003-2006 that overlap with the tenure of each CEO who was the CEO in 2001. Regressions in Columns (1) and (4) are estimated on the full sample. Columns (2)-(3) and (5)-(6) are estimated on subsamples, separated into firms with high and low information costs by the sample median of InfoCost_index, which measures the initial level of outsiders' cost of acquiring firm specific information in 2001. Definitions of all variables are provided in the Appendix. Robust standard errors reported in parentheses are clustered at the industry level, as defined by one-digit SIC code. Coefficients marked with *, **, and *** are significant at 10%, 5%, and 1%, respectively.

	ΔEBITDA/TA			**ΔTobin's Q**		
	Full	**High_InfoCost**	**Low_InfoCost**	**Full**	**High_InfoCost**	**Low_InfoCost**
VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
ΔFTA*Affected	0.052	0.085**	-0.056	1.062	2.172***	1.043
	(0.031)	(0.032)	(0.038)	(0.662)	(0.595)	(1.287)
Affected	-0.012	-0.032*	0.022	-0.212	-0.395	-0.116
	(0.009)	(0.017)	(0.019)	(0.150)	(0.232)	(0.244)
ΔFTA	-0.022***	-0.036	-0.015	-1.234	-0.816*	-1.995*
	(0.007)	(0.019)	(0.010)	(0.699)	(0.411)	(1.141)
Δn(TotalAsset)	-0.012	-0.011	-0.008	-1.517***	-1.493***	-1.648**
	(0.014)	(0.024)	(0.012)	(0.387)	(0.236)	(0.626)
ΔRisk	-0.677	-0.487	-0.615	-19.029	21.877	-91.648
	(0.383)	(0.889)	(0.396)	(32.507)	(34.940)	(98.993)
ΔRiskD	0.027	0.032	0.026**	-1.019	-2.240	0.497
	(0.019)	(0.052)	(0.009)	(0.978)	(1.874)	(0.679)
ΔBoardsize	-0.000	-0.000	-0.000	-0.005	-0.008	0.046
	(0.001)	(0.002)	(0.002)	(0.074)	(0.091)	(0.048)
Constant	-0.010	-0.010	-0.009	0.254*	0.279**	0.162
	(0.009)	(0.013)	(0.005)	(0.113)	(0.111)	(0.261)
Observations	739	313	317	644	293	256
Adjusted R^2	0.000	-0.012	-0.007	0.097	0.112	0.116

Table 8: Placebo Tests, Sarbanes-Oxley Act, and Other Confounding Effects.

Panel A reports results of placebo tests using "controlled" firms, which are exempt from the independent board requirement but are required to comply with other governance requirements of the Sarbanes-Oxley Act. Firms are considered "controlled" if directors and officers own more than 50% of outstanding shares or if a firm has ducal class shares. Column (1) reestimates the baseline regression in Table 3 using only firms exempted from the board regulation. Column (2) utilizes the full sample, including exempt firms and non-exempt firms matched to the exempt firms by propensity scores. Exempted is an indicator for firms classified as "controlled" in 2001. The regressions contain the same control variables as in Table 3 but not reported.
Panel B reestimates the impact of the independent board requirement on FTA using a subsample of firms with public float less than $75 Million in 2002. These firms are exempted from Section 404 of the SOX. Column (1) does not include any control variables except fixed effects, while Column (2) includes the same control variables as in Table 3 (unreported).
Panel C reestimates the regulatory effects using a series of year indicator variables for pre- and post-regulation periods. Dummy variables 2000, 2001, 2003, 2004, and 2005 and after are equal to one if the observation is in 2000, 2001, 2003, 2004, and 2005-2006, respectively. The regressions include the same control variables as in Table 3 (unreported). Coefficients marked with *, **, and *** are significant at 10%, 5%, and 1%, respectively.

Panel A: Placebo Tests Using Controlled Firms exempt from the Independent Board Requirement.

	FTA	
	Controlled Firms Only	**Controlled_Matched Sample**
VARIABLES	**(1)**	**(2)**
Affected*Post	-0.001	0.102***
	(0.044)	(0.031)
Affected*Post*Exempted		-0.083**
		(0.041)
Post*Exempted		0.024
		(0.020)
Firm FE & Year FE	Y	Y
Observations	647	1,810
Adjusted R^2	0.803	0.763

Panel B: Reestimation of the Baseline Model for Firms Exempt from Section 404 of the Sarbanes-Oxley Act.

	FTA	
VARIABLES	**(1)**	**(2)**
Affected*Post	0.426**	0.136*
	(0.161)	(0.081)
Firm FE & Year FE	Y	Y
Observations	120	95
Adjusted R^2	0.274	0.859

Panel C: Testing Other Possible Confounding Effects.

	FTA	
	Unmatched	**PS-Matched**
	(1)	**(2)**
Affected*2000	-0.000	-0.008
	(0.022)	(0.020)
Affected*2001	0.024	0.012
	(0.024)	(0.021)
Affected*2003	0.066**	0.021
	(0.026)	(0.021)
Affected*2004	0.088***	0.036*
	(0.029)	(0.020)
Affected*2005andafter	0.113***	0.061***
	(0.030)	(0.020)
Firm FE & Year FE	Y	Y
Observations	6,304	2,695
Adjusted R^2	0.701	0.742

Table 9: Other Robustness Checks.

Panel A estimates how the degree to which a firm is affected by the independent board requirement is related to changes in the fraction of top-four non-CEO executives appointed (FTA) during a CEO's tenure. The key independent variable is Pct_Dep_Dir*Post, the percentage of non-independent directors in 2001 interacted with the post-regulation indicator. The regression does not include Pct_Dep_Dir and Post as separate controls because of firm- and year fixed effects.
Panel B reestimates the OLS regressions in Table 3 with three alternative measures of FTA. WFTA is FTA weighted by the sum of executives' salaries and bonuses; AFTA, an abnormal measure of FTA; and WAFTA, an abnormal measure of WFTA. Columns (1)-(3) and Columns (4)-(6) report results estimated with the unmatched sample and the propensity-score (PS) matched sample, respectively.
The sample period is 1996 – 2006, excluding 2002. Coefficients marked with *, **, and *** are significant at 10%, 5%, and 1%, respectively.

Panel A: Heterogeneity in the Treatment Effects.

	FTA	
	Unmatched	**PS-Matched**
VARIABLES	**(1)**	**(2)**
Pct_Dep_Dir*Post	0.243***	0.136***
	(0.052)	(0.039)
Observations	6,304	2,695
Adjusted R^2	0.703	0.743

Panel B: Alternative Definitions of FTA.

	Unmatched			**PS-Matched**		
	WFTA	**AFTA**	**WAFTA**	**WFTA**	**AFTA**	**WAFTA**
VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Affected*Post	0.081***	0.071***	0.065***	0.029**	0.044***	0.039***
	(0.022)	(0.021)	(0.021)	(0.015)	(0.013)	(0.015)
Firm FE	Y	Y	Y	Y	Y	Y
Year FE	Y	N	N	Y	N	N
Observations	6,304	6,304	6,304	2,695	2,695	2,695
Adjusted R^2	0.702	0.512	0.521	0.741	0.555	0.559

The following tables belong to the Appendix:

Table A-1: The Matching Regression

This table estimates the regression for constructing the propensity score matched sample based on observations in 2001. The dependent variable, Affected, is an indicator equal to one if a firm does not have a majority of independent directors in 2001; zero otherwise. The regression controls for industry fixed effects at the one-digit SIC code level. Definitions of all variables are provided in the Appendix. Robust standard errors are reported in parentheses. Coefficients marked with *, **, and *** are significant at 10%, 5%, and 1%, respectively.

	Affected
VARIABLES	**(1)**
CEOTEN	0.019
	(0.012)
OUTSIDE	-0.073
	(0.191)
CEO_Founder	-0.005
	(0.265)
FTA_1Y	-0.222
	(0.184)
KNOWN	0.896
	(0.846)
EXECSEN	0.118***
	(0.036)
FTA_1Y_Unknown	0.466
	(2.347)
Female	-0.418
	(0.599)
Ln(CEOAge)	0.279
	(0.479)
CEO_OWN_{t-1}	3.796***
	(1.079)
CEO_Chair	-0.315**
	(0.132)
$Ln(TotalAsset)_{t-1}$	-0.031
	(0.057)
FirmAge	-0.013***
	(0.004)
$Segment_{t-1}$	-0.014*
	(0.008)
EBITDA/TA	0.312
	(0.632)
TobinQ	-0.008
	(0.051)
Boardsize	0.041
	(0.028)
Ln(Num_Analyst)	-0.062
	(0.071)
Constant	-3.083
	(2.074)
Industry FE	Y
Log likelihood	-282.314
Prob > chi2	0.0000
Observations	718

Table A-2: CEO Turnovers and the Independent Board Requirement.

This table estimates the impact of the independent board requirement on CEO turnovers. The dependent variable is an indicator for CEO turnover, equal to one if a CEO in year *t* is not the same as the CEO in year *t-1*. Columns (1) and (2) report estimation results with the unmatched and propensity-score (PS) matched sample, respectively. The sample period is 1996 – 2006, excluding 2002. Definitions of all variables are provided in the Appendix. Regressions are estimated by the firm level conditional logit model with year dummies. Robust standard errors reported in parentheses are clustered at the firm level. Coefficients marked with *, **, and *** are significant at 10%, 5%, and 1%, respectively.

	CEO_Turnover	
	Unmatched	**PS-matched**
VARIABLES	**(1)**	**(2)**
Affected*Post	0.495	0.377
	(0.327)	(0.332)
$Return_{t-1}$	-0.307***	-0.363**
	(0.100)	(0.179)
$Ln(TotalAsset)_{t-1}$	-0.003	-0.059
	(0.136)	(0.219)
$Founder_{t-1}$	-1.638***	-1.176*
	(0.632)	(0.644)
$CEOTEN_{t-1}$	0.296***	0.226***
	(0.031)	(0.040)
CEO_OWN_{t-1}	-2.568	-5.253*
	(2.517)	(2.988)
CEO_Chair_{t-1}	-0.119	0.214
	(0.235)	(0.391)
Year Dummies	Y	Y
Observations	5,203	2,102
Prob> Chi^2	0	0
Wald	177.3	75.40
pseudo-R^2	0.190	0.166