

The Executive Suite and Board Independence

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Ross School of Business Working Paper
Working Paper No. 1246
December 2014

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Abstract

We find mandating board independence leads to closer CEO connectedness with top executives through new appointments and pre-existing social ties. In addition, a closer look at turnovers in executive suites amid the mandated board transition suggests that CEOs strongly influence who stays in the executive suite and who remains as inside directors. These findings imply a positive relation between board independence and CEO connectedness with executives, which we attribute to two dynamics between the board and executive suite: the bargaining process between the board and the CEO in determining board independence as in Hermalin and Weisbach (1998) and the endogenous nature of information revelation as in Harris and Raviv (2008). To the extent that connected executives are more vulnerable to CEO influence, independence in the two main governing bodies, the board and executive suit, appears inversely related; thus, inferring the overall independence from board independence alone can be misleading.

December 14, 2014

JEL classification: G34, G38, K22

Keywords: Corporate Governance, Executive Suite Independence, Board Independence, Unintended Consequences of Corporate Governance Regulation.

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1. Introduction

The two main governing bodies of corporations are the board of directors and the executive suite of the CEO and his top lieutenants. Much research has been devoted to studying board independence, examining the importance of director independence in protecting shareholder interest against CEOs' self-serving behavior.¹ But an independent board alone does not necessarily ensure an overall independent governance process if governance in the executive suite lacks independence from CEO influence.

Fama (1980) points out the importance of independent non-CEO executives in reducing agency problems, "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) show 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 highlight the importance of independence in the executive suite when they analyze how governance is shaped by internal monitoring of CEOs by other top executives.

As the two main governing bodies, the board and the executive suite may display levels of independence that are closely interrelated. For example, a highly independent board may ensure executive independence through close oversight of their appointment process. The board, at the top of the organization chart, has the authority to appoint or dismiss CEOs; hence by extension, it may influence personnel decisions of CEOs' top key lieutenants. If the board effectively exercises this authority, board independence and executive suit independence will be positively related.

Our data, however, suggests a negative relation between board and executive suite independence. When an external shock (the independent board requirement for NYSE- and NASDAQ-listed firms) forces

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).

a firm with a dependent board to make it independent, our difference-in-differences estimates indicate that the executive suite becomes less independent according to our measures of executive suite independence.

We proxy (the inverse of) executive suite independence by two measures of appointment-based CEO connectedness with top executives; the fraction of top-four non-CEO executives appointed (FTA) during a CEO's tenure and the CEO's prior social connections with the appointees. We use FTA because CEOs are heavily involved in appointment decisions of their top lieutenants, so their 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 (Landier, Sauvagnat, Sraer, and Thesmar, 2013).² Thus, when more top executives are appointed during a CEO's tenure, the CEO's internal influence in the executive suite increases through 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).³ FTA is similar to the measure used in Landier et al. (2013), the fraction of top-four non-CEO executives *hired* by a CEO, which they show is negatively related to firm profitability and shareholder returns following large acquisitions.⁴ In a related study, Khanna, Kim, and Lu (2014) argue higher FTA is indicative of weak checks and balances in executive suites and document another negative effect of FTA—higher FTA facilitates management wrongdoing with CEO involvement and helps evade the detection.

The second measure, CEOs' prior social connections with top executives appointed during their tenure, is measured by network ties formed prior to the appointments through past employment, education, and membership to social organizations during overlapping years. 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). Both FTA and social ties can also be considered informal,

² 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 top management team. In addition, data show that those retained from the previous team tend to be transitory.

³ Morse, Nanda and Seru (2011) and Coles et al. (2014) rely on a similar notion of reciprocity between directors and the CEO when they measure how "co-opted" a board is by the fraction of directors appointed during the current CEO's tenure.

⁴ Our FTA includes top-four non-CEO executives promoted from within the firm as well as those hired during a current CEO's tenure.

unofficial sources of CEO power. Adams, Almeida, and Ferreira (2005) and Morse et al. (2011) document CEO power is related to stock return volatility, CEO compensation, and rigging the incentive part of CEO pay. However, there are important differences; Adams et al. and Morse et al. measure CEO power over the board and other top executives based mostly on formal positions and titles, whereas our measures are only about CEO influence in the executive suite through informal channels.

We begin by estimating difference-in-differences in FTA using an exogenous shock triggered by the mandate for NYSE- and NASDAQ-listed firms to have a majority of independent directors by October 31, 2004. The estimation relies on variation in the pre-regulation board composition. The treatment group is firms without a majority of independent directors prior to the regulation. Since firms affected and unaffected by the regulation may not be comparable, we use propensity-scores to construct the control group. Reported estimates are based on both propensity-score matched and unmatched samples.

Regardless of which sample is used, we find treated firms significantly increase their FTA post-regulation. The higher FTA suggests a shakeup in executive suites, which could be a result of attempts by newly independent boards to improve governance. However, a closer look at the board transition and changes in executive suites suggests different stories. Treated firms have achieved the required majority of independent directors mostly by replacing affiliated outside directors—rather than inside directors—with independent directors.⁵ In instances where executives lost their inside directorship, the vast majority are previous CEOs' appointees.⁶ As for the executive suite, CEO turnovers are unaffected by the regulation, as are turnovers of current CEOs' appointees. Executive turnovers during the board transition are concentrated on appointees of the previous CEOs. The difference in turnovers between current and previous CEOs' appointees is not due to differences in the length of their tenure. These findings indicate CEOs have strong

⁵ 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.

⁶ The majority of previous CEOs' appointees who lost board seats during the board transition remained with the firm after the board transition took place.

influence on who stays in the executive suite and who remains as inside director—their own appointees. If the turnovers were the making of newly independent boards, there is no reason to expect such bias against previous CEOs' appointees.

CEOs' influence over top executive personnel decisions stems from their ability to control the information channel to private information necessary for top executive personnel decisions—not just about individual qualifications and talents but also about perceived synergies individual candidates can bring to the management team. Boards meet only a few times a year, and independent directors work part time. They also 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). In contrast, CEOs have full-time employees at their disposal to perform the necessary footwork to make a case to the board. These advantages over independent directors allow CEOs to favor executives they feel more connected to when potential candidates display similar qualifications and perceived synergies to the executive team.

Furthermore, difference-in-differences estimates in pre-existing social ties between CEOs and top-four non-CEO executives appointed during their tenure also indicate significant increases in CEO social connectivity. We control for FTA in these estimates, so our estimates of social tie increases are not due to increases in FTA. Social connections may play a role in appointment decisions because they provide valuable information about personal abilities and character unavailable in the public domain. However, this consideration should apply to both treated and control groups and thus cannot explain why the network ties increase more at treated firms after the regulation. The increase in social ties, therefore, further buttresses our assertion that treated firms' CEOs increase their internal connectedness in the executive suite.

The findings on FTA and social connections are not driven by the Sarbanes-Oxley Act of 2002, other major events in 2000 and 2001,⁷ or major structural changes within the firm. They are also robust to a

⁷ Although the board regulation was promulgated around the same time as the enactment of the SOX, it is distinct from the SOX and under purview of different organizations.

compensation-weighted FTA allowing for difference 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), and an alternative sample construction.

Why does CEO connectedness in the executive suite increase when a firm is required to make its dependent board independent? There are two plausible complementary dynamics between the board and the executive suite that may explain our findings. The first is the Hermalin and Weisbach model (1998), wherein absent regulation, board independence is determined by a bargaining process between CEOs and boards, resulting in CEOs with greater bargaining power having more dependent boards. The regulation nullifies the bargaining outcome for CEOs with a dependent board. To recoup the loss of influence in the boardroom, the affected CEOs—who have more bargaining power according to the Hermalin and Weisbach model—strengthen their influence in the executive suite by increasing internal connectedness.

The increase in CEO connectedness may also be necessitated by the endogenous nature of information revelation by insiders on the board, as articulated in Harris and Raviv's (2008) equilibrium analysis.⁸ Harris and Raviv show that when the board is controlled by insiders—a dependent board—they are less reluctant to share negative information about the firm. But if the board becomes independent, insiders will release less information, which may require tighter control of information flow from the executive suite to the board. Control is easier when CEOs are closely connected to their top executives.

This paper contributes to the literature by investigating dynamics between the executive suite and the board, an important issue overlooked by previous researchers on corporate governance. 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.

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.,

⁸ Raheja (2005) also allows endogenous information revelation in modeling how CEO succession decisions affect inside directors' incentives to reveal information, showing that the optimal level of board independence depends on the level of difficulty with which independent directors can verify projects.

2014). We add to this literature by studying CEO influence on the appointment and composition of the other governing body—the executive suite.

In addition, our difference-in-differences estimation reveals 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.

The next section describes our empirical design and data. Section 3 presents our main findings on FTA, social connections, and changes taking place in executive suites amid the mandated board transition. They are followed by a battery of robustness tests in Section 4. Section 5 concludes.

2. Empirical Design and Data

2.1. Proxies for Independence in the Executive Suite

Our main proxy for (the inverse of) executive suite independence is CEO connectedness with top-four non-CEO executives. The first measure is FTA_{it} , the number of executives hired or promoted 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. 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.⁹ We assume the year a non-CEO 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.

The second measure is pre-existing social ties a CEO has with top-four executives appointed during his tenure. To avoid reverse causality, we include only network ties formed prior to the CEO and the executives joining the company. The pre-existing network ties are obtained by manually matching individual names in ExecuComp with those in BoardEx. BoardEx provides information for past

⁹Kim 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 does not seem to introduce a selection bias.

employment, education background, and membership in social organizations (e.g., philanthropic and religious organizations, social clubs, and professional organizations). We count the number of network ties established during overlapping years for each category of network ties (through past employment, education, or membership in social organizations) to capture the depth of past connections. Then we sum the three types of ties to arrive at the total number of ties.

2.2. Empirical Design

Because board composition is endogenous, we use an exogenous shock on board independence to estimate difference-in-differences in FTA and social ties. 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 lacking 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, Matsusaka, and Ozbas, 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. The baseline specification is:

$$Y_{it} = a_i + a_t + \beta_1 Dep_Board2001_i * Post_t + \beta_2 X_{it} + \varepsilon_{it} \quad (1)$$

Y_{it} is a measure of firm i 's current CEO's connectedness in the executive suite as of year t , as measured by either FTA or pre-existing social ties. $Dep_Board2001_i$ is the treated firm 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 effects, a_i and a_t . Because of these fixed effects, the specification does not contain a separate term for $Dep_Board2001_i$ or $Post_t$. X is a vector of 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, we construct a propensity-score matched control group following Rosenbaum and Rubin (1983) based on information in the base-year 2001. Ideally, the variables used to estimate the probit model must include all factors affecting both the likelihood of being affected by the regulation (board independence) and regulation outcome (FTA or social ties).

Linck, Netter, and Yang (2008) show that board independence is affected by firm complexity, costs of monitoring, ownership incentive, and CEO characteristics. Our matching criteria incorporate all these factors. Firm complexity is captured by firm size, firm age,¹⁰ and the number of business segments within a firm; costs of monitoring by EBITDA/TA, Tobin's Q, and board size; ownership incentive by the percentage share ownership held by a CEO; and CEO characteristics by log of CEO age, an indicator for a CEO chairing the board, and CEO gender. These factors may also affect FTA and social ties. We also include variables similar to those used in Landier et al. (2013) that are directly related to FTA: CEO tenure,¹¹ 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_IY*). We also add 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.¹² 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_IY_Unknown*). This variable helps control noise in *FTA_IY*.

¹⁰ Boone, Field, Karpoff, and Raheja (2007) suggest that complexity increases with firm age.

¹¹ 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.

¹² 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.

One affected firm is matched to the three nearest unaffected firms using the Mahalanobis distance metric. We exclude all observations that do not satisfy the common support condition. Log likelihood, $\text{Prob} > \text{Chi}^2$, and Pseudo R^2 for estimating the propensity scores are -293.14, 0.00, and 0.13. We bootstrap 200 times to correct standard errors for all OLS regressions estimated with matched samples.

2.3. Sample Construction

Our sample is constructed with NYSE- and NASDAQ-listed firms for which we have information on board composition in 2001 from RiskMetrics and executive data from ExecuComp. Other data sources include BoardEx for pre-existing social ties; Compustat for accounting data; and CRSP for stock return data. 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.¹³ 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 board independence before and after 2007. In addition, early 2008 was the beginning of the financial crisis, a rare event that led to unusual changes in the executive suite unrelated to the regulation.

Our sample firms affected by the regulation increased their average fraction of independent directors from 0.36 in 2001 to 0.56 in 2004. The average board size remained more or less the same—9.06 directors in 2001 and 9.28 in 2004. The higher fraction is achieved mostly by replacing affiliated directors, outside directors with material relationship with the firm, with independent directors. Between 2001 and 2004, affected firms' average fraction of affiliated directors declined from 0.33 to 0.20, a reduction by 0.13. In contrast, the fraction of inside directors declined by only 0.07, from 0.32 in 2001 to 0.25 in 2004.

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. Panel A reports the number of firms in the treatment and

¹³ Although firm coverage prior to 2000 by BoardEx is quite limited, the social ties for years 1996 to 1999 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.

control group for the unmatched full sample. The number of firms increases over time due to greater firm coverage by ExecuComp in later years. Panel B shows the propensity score matched sample. The number of unaffected firms is substantially fewer than three times the number of affected firms because of multiple matches to 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 for dependent board in 2001, *Dep_Board2001*, 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* is 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 in the matched sample at the time of matching, the base-year 2001. Most firm and CEO characteristic variables in Panel B show insignificant differences between the treatment and control group, with a few exceptions that arise because the propensity score matching is based on the overall similarity.

3. CEO Connectedness in Executive Suites and the Independent Board Requirement

In this section we estimate how CEO connectedness in executive suites changes in response to the independent board requirement. Specifically, we estimate difference-in-differences in *FTA* and prior social connections CEOs have with their appointees. We also take a closer look at the changes taking place in the executive suite during the board transition and how the affected firms have achieved a majority of independent directors.

3.1. Fraction of Top-Four Non-CEO Executives Appointed during a Current CEO's Tenure

We begin by estimating difference-in-differences in *FTA* with firm- and year fixed effects. Control variables include time-varying firm and CEO characteristics that may influence *FTA* and board independence. Firm characteristics include $\ln(\text{TotalAssets})$, log of the book value of total assets; *FirmAge*,

one plus the number of years from the firm's IPO or the number of years since its first appearance in CRSP; and *Segment*, the number of business segments as reported by Compustat Segments. CEO characteristics include $\ln(\text{CEOAge})$, log of a CEO's age; *CEO_OWN*, the percentage of outstanding shares a CEO owns; *CEO_Chair*, an indicator for a CEO chairing the board; and *Female*, an indicator for a CEO's gender. Firm size, the number of segments, and CEO share ownership are lagged by one year. We also control for the variables mechanically correlated to FTA: *CEOTEN*, CEO tenure; *OUTSIDE*, an indicator for CEOs hired from outside; *EXECSEN*, the average tenure of top-four non-CEO executives; and *FTA_1Y*, the fraction of top-four executives appointed during a CEO's first year in office. *KNOWN* and *FTA_1Y_Unknown* are also added to control noise in *FTA*, *EXECSEN*, and *FTA_1Y*.

Table 3 presents the estimation results. Odd-numbered columns report OLS estimates. The coefficient on *Dep_Board2001*Post*, the estimated regulatory 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 increases FTA. The coefficient on *Dep_Board2001*Post* in Column (1) implies that the regulation leads to 0.41 (0.102/0.25) more top executive appointments by the current CEO replacing previous CEOs' appointees (one new appointment increases FTA by 0.25). This impact is economically meaningful. The treated firms' mean FTA in 2001 was 0.381, which means the average number of top-four executives appointed by previous CEOs was 2.476 $((1 - 0.381)/0.25)$. Thus, the point estimate implies about 16.6% $(0.41/2.476)$ more previous CEOs' appointees were replaced by the current CEO's appointees.

Coefficients on the 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, and positively related to the fraction of top executives appointed during a CEO's first year in office. Interestingly, older CEOs appoint more executives during their tenure, whereas CEOs with greater share ownership and female CEOs are less likely to do so. Larger and older firms with fewer business segments tend to have a higher FTA.

3.2. Closer Look at Changes in the Executive Suite

The higher FTA suggests a shakeup in executive suites amid the mandated board restructuring. Is the shakeup a result of the newly independent board's attempts to improve governance, or is it a result of CEOs' desires to have their own appointees? To shed light on this issue, we investigate who bears the brunt of the shakeup.

3.2.1. Executive and CEO Turnovers

Table 4 estimates the effect of the board regulation on top executive and CEO turnovers. As before, the main variable of interest is the interaction term $Dep_Board2001*Post$. Because executive turnovers are likely to be related to firm performance, we control for *Return*, one year buy-and-hold stock returns. Other control variables include firm size, and executive or CEO tenure and share ownership.

The first two columns report estimates of top-four non-CEO executive turnovers with firm- and year-fixed effects. The dependent variable is the percentage of new top-four non-CEO executives who were not on the top-four list in the previous year. All control variables are lagged by one year. The coefficient on $Dep_Board2001*Post$ is positive and significant, implying that treated firms experience greater top-four non-CEO executive turnovers following the regulation.

The last two columns report estimates for 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 $t-1$. Because CEOs' influence over the board may affect their job security, we add the CEO/Chair indicator as a control. In contrast to non-CEO top executives, the coefficient on $Dep_Board2001*Post$ is insignificant; CEO turnovers seem largely unaffected by the regulation. It appears the shakeup in the executive suite is limited to non-CEO top executives.¹⁴

3.2.2. Top Executives Appointed during Current versus Previous CEOs' Tenure

¹⁴ In unreported regressions, we include a triple interaction, $Dep_Board2001*Post*Return_{t-1}$, to control for possible changes in turnover-performance sensitivity for CEOs and top executives. The coefficients on the triple interaction term are significantly negative for both CEOs and top executives, indicating newly independent boards increase CEO and top executives turnover-performance sensitivity. More important, the coefficients on $Dep_Board2001*Post$ remain positive and significant for top executives but insignificant for CEOs.

Who are more affected by the shakeup, the current or previous CEOs' appointees? We first compare all top-four non-CEO executives in the base-year 2001 with those in 2004, the deadline for compliance with the regulation. Among our sample of treated firms covered by ExecuComp in both 2001 and 2004, we identify 339 executives who are dropped from the list of top-four non-CEO executives between the pre- and the post-regulation year. Of the 339, 64% (217) are previous CEOs' appointees, about 60% (129) of whom remained with the firm as of 2004. Thus, the higher FTA is a result of both the departure and demotion of previous CEOs' appointees to make room for new appointees. Previous CEOs' appointees appear to bear the brunt of the executive shakeup.

To verify this inference, we separate top-four non-CEO executives into subsamples of current CEO appointees, *Cur_CEO_Appt*, and previous CEO appointees, *Pre_CEO_Appt*. For each subsample, 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 are the same as those in Table 4, except that the tenure and share ownership variables are now at the individual executive level.

The estimation results are reported in Table 5.¹⁵ The difference between the first two columns is striking. The likelihood of being dropped from the top-four list increases significantly for previous CEOs' appointees, but not for current CEOs' appointees.

Comparison of coefficients on executive tenure, *EXETEN*, is particularly revealing. For previous CEOs' appointees, the longer the tenure, the more likely they will be dropped from the list. The opposite holds for current CEOs' appointees; the longer their 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 turnover among previous CEOs' appointees could be due to their longer tenure relative to the current CEO's appointees. Longer tenure may indicate more entrenchment; hence, the newly

¹⁵ Because the propensity score is matched at the firm level, not at the executive level, our estimation is based only on the unmatched sample.

independent board may target executives with longer tenure. To address this possible tenure bias, we restrict each subsample to only those executives with tenure (in the top-four list) longer than the sample median and reestimate the regressions. The results, reported in the last two columns in Table 5, are robust to this alternative sample construction.

3.2.3. Executive (Inside) Director Turnovers

The bias against previous CEOs' appointees is also prevalent among top executives who sat on the board in the base year 2001. Although inside directors are less affected by the mandated board restructuring than affiliated directors, there are 103 top-four non-CEO executives who sat on the board in 2001 no longer serving on the board in 2004. (They are identified by matching ExecuComp and RiskMetrics for treated firms covered by both data sources in 2001 and 2004.) Of the 103, 84 are previous CEOs' appointees, the majority of which (47) remained with the firm as of 2004, with the rest leaving the firm due to death (1), retirement (22), and resignation (14).

3.2.4. Summary

These findings indicate that previous CEOs' appointees did bear the brunt of shakeups in executive suites triggered by the board regulation. There is no evidence of greater turnovers among CEOs or their own appointees, demonstrating CEOs' staying power during the board restructuring and their influence on who occupies executive suites and who sits on the board as inside director. Although this bias in favor of CEOs' own appointees is not surprising, it is revealing that the bias becomes greater amid the external shock making a dependent board independent. There is no reason to expect such an increase in the bias against previous CEOs' appointees if the shakeup is a result of newly independent boards' efforts to improve governance. The higher FTA following the board regulation seems to be a result of current CEOs exerting strong influence on top executive personnel decisions during the board transition.

3.3. *Social Connections*

If the increase in FTA is a result of CEOs' desires to increase appointment-based connectedness with top executives, they may opt for individuals with whom they are socially pre-connected. CEOs may not necessarily prefer executives with prior social connections, but the familiarity acquired through prior

social interactions helps select more talented individuals and also those more closely aligned with the CEOs and less likely to dissent. We estimate the degree of social connection by estimating the baseline regressions for the number of pre-existing network ties a CEO has with executives appointed during his tenure.

The dependent variable is log of one plus *Exe_Tie*, the total number of network ties a CEO and newly appointed executives have through past employment (either working as an employee or serving on the board), educational institutions, and *past* membership to social and professional organizations. To be included, network ties must be established during overlapping years. For example, in the case of education, the years a CEO and an executive attended the same school must overlap. The key independent variables and 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 higher the fraction of executives appointed during his tenure.

Information on network ties between an executive and the CEO is often missing or incomplete because the relevant individual is 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 for whom we have missing or incomplete information to precisely identify 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 6 reports estimation results for the unmatched and matched samples. Both samples show increases in CEOs' network ties with top executives appointed during their tenure following the regulation. Although the significance level for the unmatched sample is at only 10%, it is at 5% for the matched sample. Estimators based on the propensity score often generate more precise estimates in finite samples (Angrist and Hahn, 2004).

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 treated and control groups and thus cannot explain why the network ties increase more at treated firms after the regulation. This difference, therefore, further buttresses our argument that the higher FTA following the board regulation is a result of affected CEOs' desire to increase internal connectedness with their top lieutenants.

4. Confounding Effects and Other Robustness Tests

In this section we examine possible confounding effects, including the enactment of the Sarbanes–Oxley Act in 2002. We also conduct a battery of robustness tests concerning the heterogeneity in treatment effects and the sensitivity of our main results to alternative definitions of FTA, organizational structure-changing events, and an alternative sample construction.

4.1. Sarbanes-Oxley Act of 2002

While the independent board requirement proposal was under consideration by the SEC, the Sarbanes–Oxley Act was enacted in 2002. If the SOX affected firms with dependent boards in 2001 differently from those with independent boards, then the treatment effects we observe in FTA and social ties can be attributed not only to the board regulation but also to the SOX. To check this possibility, we conduct two tests: First, we reestimate regressions for FTA and social ties with a subsample of firms least affected by the SOX. If the SOX were the main driving force affecting our estimation, we should observe little or no treatment effects for this subsample. Second, 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, treated firms in the base-year 2001 had to be more opaque, with less accurate earnings reports, than the control group for our difference-in-difference estimates to be affected by the SOX.

The subsample of firms least affected by the SOX are those with public float less than \$75 million. These firms are exempted from Section 404, the provision considered so rigorous (onerous) that Gao, Wu, and Zimmerman (2009) argue it provided an unintended incentive for small firms to stay small.

Public float is the market value of equity held by non-affiliates of the issuer. We obtain data on director and officer (D&O) share ownership from Compact Disclosure and estimate public float of each firm as the market value of common equity multiplied by $(1 - \text{D\&O share ownership})$, using the market cap as of the end of the second quarter of the fiscal year of 2002.¹⁶ When D&O ownership is missing, we use the 2002 sample mean of 14%. This process yields 30 firms with estimated public float less than \$75 million.

The reestimation results for FTA and social ties for this small subsample are reported in Table 7. To make full use of the limited sample, our first estimation is done without any control variables, except for firm and year fixed effects. Then we reestimate with the same set of control variables as before. We use only the unmatched subsample because the PS-matched subsample contains only six firms. In spite of the small sample size, difference-in-differences estimates for FTA remain positive and significant. The results on social ties also show positive coefficients, which are significant only without control variables. Although statistical significance is weaker, the magnitudes of difference-in-differences estimates are larger than their counterparts in Table 3 (FTA) and Table 6 (social ties).

We also check whether firms with dependent boards had less accurate earnings reports than the control group in 2001, a necessary condition for our difference-in-difference estimates to be affected by the SOX. Specifically, we estimate earnings management in the base year and compare the estimates for the treatment and control groups. A commonly-used measure of earnings management is discretionary accruals (DAC), those parts of total accruals over which management have discretion. Total accruals are computed as the difference between earnings and operating cash flows.¹⁷ To estimate the discretionary components of total accruals, we follow Dechow, Sloan, and Sweeney (1995) by regressing total accruals on the inverse of

¹⁶ 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.

¹⁷ 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.

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 see 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. This lack of correlation in the base year, together with reestimation results with the subsample of firms least affected by the SOX, indicate that our difference-in-differences estimates are not driven by the SOX.

4.2. *Other Possible Confounding Events*

Our results may be confounded by other events affecting the market for top executives that occurred around the time the board regulation was promulgated. 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.

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*. Table 8 reports the reestimation results for both FTA and social ties. If our results are driven by the events in 2000 and/or 2001, the coefficients on the interaction of *Dep_Board2001* and year dummy in the regressions for FTA should be negative for 2000 or 2001; however, they are all insignificant with mostly positive signs. For post-regulation years, by contrast, the interaction terms show all positive and significant coefficients. The reestimation results for social ties are also consistent, albeit with considerably weaker statistical significance.

Interestingly, coefficients on the interaction of the dependent board indicator and post-regulation year dummies show an increasing trend over time. Because it takes time to replace top executives through new hires and promotions, this time trend corroborates our conclusion that the regulation led treated firms to increase their appointment-based CEO connectedness in the executive suite.

4.3. *The Degree of Treatment Effects*

Our difference-in-differences estimates are based on a treatment indicator, $Dep_Board2001_i$, 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 regressions for both FTA and social ties with the interaction of the percentage of non-independent directors in 2001, $Pct_Dep_Board2001_i$, and the post-regulation indicator. Table 9 reports the results, which indicate that the more affected by the regulation a firm is, the greater the increase in both FTA and social ties, irrespective of whether the sample is unmatched or matched.

4.4. *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. (ExecuComp defines top five executives based on their salaries and bonuses.) Since CEO connections with more influential executives 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 , $FTA_1Y_Unknown$ 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, the regressions do not include independent variables used to estimate the residuals.

Reestimation results based on these three alternative measures of FTA are reported in Table 7. The results are robust regardless of which alternative measure is used.

4.5. *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.

4.6. *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.

5. **Conclusion**

Our difference-in-differences estimates using an external shock suggest that board independence is negatively related to executive suite independence as measured by CEO connectedness. Thus, inferring the overall independence of a firm's governing process by board independence alone may be misleading. According to our estimates, when board independence is strong (weak), the overall independence is likely to be weaker (stronger) than board independence alone indicates.

Close examination of changes in executive suites amid the board restructuring mandated by regulation reveals surprisingly strong CEO influence on who stays in the executive suite and who remains as inside director on the board. Whereas turnovers among CEOs or their appointees are largely unaffected, turnovers among executives appointed by previous CEOs increase significantly when the board is restructured. The vast majority of inside directors who lost board seats to make room for more independent directors are also previous CEOs' appointees. This difference in turnovers between current and previous CEOs' appointees indicates strong CEO influence over top executive personnel decisions, which helps increase current CEOs' appointment-based connectedness within the executive suite. The increase in CEO connectedness, in turn, is likely to weaken executive suite independence because executives favorably connected to their CEOs through appointment decisions are more vulnerable to CEO influence.

Our findings also have a message for policy makers. When they target a specific governance mechanism, they must carefully evaluate how their regulatory actions spill over to other governing bodies.

Finally, a caveat is in order. Our analysis considers only one specific category of employees—top executives—ignoring other employees. Non-executive employees also influence the governing process through their working relationships with the management (Bertrand and Mullainathan, 2003) and through the collective bargaining process (Atanassov and Kim, 2009). 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 group affect a firm's overall governing process.

Appendix: Variable Descriptions.

| Board Composition Variables | |
|--|--|
| <i>Dep_Board2001</i> | Dependent board indicator equal to one if a firm does not have a majority of independent directors in 2001; zero, otherwise. |
| <i>Pct_Dep_Board2001</i> | The percentage of non-independent directors on the board in 2001. |
| Executive Suite Composition Variables | |
| <i>FTA</i> | Fraction of top-four non-CEO executives appointed during a current CEO's tenure. |
| <i>AFTA</i> | Abnormal fraction of top-four non-CEO executives appointed during a current CEO's tenure. |
| <i>WFTA</i> | Fraction of top-four non-CEO executives appointed during a current CEO's tenure, weighted by the sum of executives' salaries and bonuses. |
| <i>WAFTA</i> | Abnormal fraction of top-four non-CEO executives appointed during a current CEO's tenure, weighted by the sum of executives' salaries and bonuses. |
| <i>Ln(Exe_Tie+1)</i> | Logged value of <i>Exe_Tie</i> plus one. <i>Exe_Tie</i> 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 <i>past</i> membership to social and professional organizations. Only network ties established during overlapping years are included. |
| Other Variables | |
| <i>Post</i> | Post-regulation indicator, equal to one if year <i>t</i> is 2003 or thereafter; zero, otherwise. |
| <i>CEOTEN</i> | Number of years a CEO has been in office. |
| <i>OUTSIDE</i> | Outsider indicator equal to one, if a CEO comes from outside the firm; zero, otherwise. |
| <i>FTA_1Y</i> | Fraction of top-four non-CEO executives appointed within the year of a new CEO appointment. |
| <i>FTA_1Y_Unknown</i> | Fraction of top-four non-CEO executives whose information on whether they are appointed within the year of a new CEO appointment is unknown. |
| <i>KNOWN</i> | Fraction of executives whose first year on the list of the top four non-CEO executives can be identified with data in ExecuComp. |
| <i>EXECSEN</i> | Average number of years of top-four non-CEO executives' tenure. |
| <i>Female</i> | Indicator equal to one for female CEO; zero, otherwise. |
| <i>Ln(CEOAge)</i> | Logged value of CEO age. |
| <i>CEO_OWN</i> | Percentage of outstanding common shares held by a CEO. |
| <i>CEO_Chair</i> | Indicator equal to one for CEOs chairing the board; zero, otherwise. |
| <i>Ln(TotalAssets)</i> | Logged book value of total assets in 2000 US million dollars. |
| <i>FirmAge</i> | One plus the number of years from the firm's IPO or the number of years since its first appearance in CRSP. |
| <i>Segment</i> | Number of business segments a firm has in a given year as reported by Compustat/Segment. |
| <i>Pct_Exe_Turnover</i> | Percentage of new top-four non-CEO executives who were not on the list of top-four non-CEO executive positions in the previous year. |
| <i>CEO_Turnover</i> | CEO turnover indicator equal to one, if the CEO in year <i>t-1</i> is different from the CEO in year <i>t</i> ; zero, otherwise. |
| <i>Avg_EXE_OWN</i> | Average percentage of shares held by top-four non-CEO executives. |
| <i>Exe_Turnover</i> | Executive turnover indicator equal to one if an executive on the list of top four non-CEO executives in year <i>t</i> is not on the list in year <i>t+1</i> . |
| <i>EXE_TEN</i> | Number of years an executive has been on the list of top-four non-CEO executives. |
| <i>EXE_OWN</i> | Percentage share ownership held by a top-four non-CEO executive. |
| <i>Return</i> | One year buy-and-hold stock returns. |
| <i>Pct_Miss_FTA_Tie</i> | Percent of top-four non-CEO executives appointed during a CEO's tenure whose network ties to their CEO are missing or incomplete. |

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Table 1: Sample Description.

This table shows the number of observations for unmatched and matched samples, by year, in Panels A and B, respectively. Columns (1) and (4) 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 (2) and (5) report the number of firms without a majority of independent directors in 2001 and, hence, affected by the regulation. Columns (3) and (6) show the number of firms unaffected by the regulation.

| Year | Panel A: Unmatched Sample | | | Panel B: PS-matched Sample | | |
|-------------|---------------------------|----------|------------|----------------------------|----------|------------|
| | Full | Affected | Unaffected | Full | Affected | Unaffected |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| 1996 | 691 | 106 | 585 | 296 | 104 | 192 |
| 1997 | 755 | 113 | 642 | 318 | 110 | 208 |
| 1998 | 825 | 128 | 697 | 355 | 125 | 230 |
| 1999 | 913 | 147 | 766 | 390 | 145 | 245 |
| 2000 | 946 | 156 | 790 | 402 | 153 | 249 |
| 2001 | 944 | 157 | 787 | 410 | 157 | 253 |
| 2003 | 958 | 164 | 794 | 403 | 152 | 251 |
| 2004 | 969 | 171 | 798 | 405 | 154 | 251 |
| 2005 | 978 | 171 | 807 | 400 | 152 | 248 |
| 2006 | 996 | 182 | 814 | 400 | 154 | 246 |
| Total Obs. | 8,975 | 1,495 | 7,480 | 3,779 | 1,406 | 2,373 |
| Total Firms | 1,035 | 185 | 850 | 410 | 157 | 253 |

Table 2: Descriptive Statistics.

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

| | Panel A: Unmatched Sample | | | | | Panel B: PS-matched Sample | | | |
|--|---------------------------|--------|-----------|--------|--------|----------------------------|------------|--------|---------|
| | Mean | Median | Std. Dev. | Min | Max | Mean | | Diff | P-Value |
| | | | | | | Affected | Unaffected | | |
| (1) | (2) | (3) | (4) | (5) | (6) | (7) | (6)-(7) | | |
| Board Composition Variables | | | | | | | | | |
| Dep_Board2001 | 0.167 | 0.000 | 0.373 | 0.000 | 1.000 | 1.000 | 0.000 | | |
| Pct_Dep_Board2001 | 0.351 | 0.333 | 0.176 | 0.063 | 1.000 | 0.646 | 0.305 | | |
| Pct_Ind_Dir | 0.674 | 0.700 | 0.169 | 0.000 | 1.000 | 0.362 | 0.701 | | |
| Executive Suite Composition Variables | | | | | | | | | |
| FTA | 0.437 | 0.500 | 0.334 | 0.000 | 1.000 | 0.381 | 0.450 | -0.069 | 0.034 |
| AFTA | 0.034 | 0.043 | 0.284 | -1.039 | 0.752 | -0.047 | 0.022 | -0.069 | 0.013 |
| WFTA | 0.415 | 0.412 | 0.335 | 0.000 | 1.000 | 0.353 | 0.430 | -0.078 | 0.017 |
| WAFTA | 0.033 | 0.025 | 0.284 | -1.016 | 0.774 | -0.054 | 0.021 | -0.075 | 0.007 |
| Ln(Exe_Tie+1) | 0.064 | 0.000 | 0.237 | 0.000 | 2.639 | 0.070 | 0.067 | 0.003 | 0.917 |
| Other Variables | | | | | | | | | |
| Post | 0.435 | 0.000 | 0.496 | 0.000 | 1.000 | | | | |
| CEOTEN | 6.920 | 5.000 | 7.334 | 0.000 | 55.000 | 9.847 | 6.901 | 2.946 | 0.001 |
| OUTSIDE | 0.142 | 0.000 | 0.349 | 0.000 | 1.000 | 0.191 | 0.134 | 0.057 | 0.125 |
| FTA_1Y | 0.538 | 0.500 | 0.399 | 0.000 | 1.000 | 0.572 | 0.519 | 0.053 | 0.202 |
| KNOWN | 0.973 | 1.000 | 0.093 | 0.000 | 1.000 | 0.971 | 0.983 | -0.012 | 0.136 |
| EXECSEN | 4.350 | 4.000 | 1.872 | 0.000 | 14.750 | 4.909 | 4.530 | 0.378 | 0.033 |
| FTA_1Y_Unknown | 0.001 | 0.000 | 0.021 | 0.000 | 0.500 | 0.002 | 0.000 | 0.002 | 0.205 |
| Female | 0.013 | 0.000 | 0.113 | 0.000 | 1.000 | 0.006 | 0.012 | -0.005 | 0.584 |
| Ln(CEOAge) | 4.015 | 4.025 | 0.135 | 3.466 | 4.511 | 4.034 | 3.999 | 0.034 | 0.019 |
| CEO_OWN | 0.023 | 0.003 | 0.060 | 0.000 | 0.638 | 0.052 | 0.023 | 0.029 | 0.000 |
| CEO_Chair | 0.657 | 1.000 | 0.475 | 0.000 | 1.000 | 0.605 | 0.652 | -0.047 | 0.337 |
| Ln(TotalAssets) | 7.765 | 7.599 | 1.657 | 2.227 | 14.291 | 7.400 | 7.431 | -0.031 | 0.837 |
| FirmAge | 26.830 | 23.000 | 19.571 | 1.000 | 82.000 | 19.541 | 23.601 | -4.059 | 0.010 |
| Segment | 15.036 | 14.000 | 9.695 | 1.000 | 87.000 | 14.929 | 16.119 | -1.190 | 0.129 |
| CEO_Turnover | 0.122 | 0.000 | 0.327 | 0.000 | 1.000 | 0.123 | 0.142 | -0.019 | 0.573 |
| Return | 0.233 | 0.145 | 0.659 | -0.972 | 17.726 | 0.108 | 0.185 | -0.077 | 0.181 |
| Pct_Exe_Turnover | 0.255 | 0.25 | 0.214 | 0.000 | 1.000 | 0.228 | 0.253 | -0.025 | 0.220 |
| Avg_EXE_OWN | 0.003 | 0.001 | 0.011 | 0.000 | 0.178 | 0.006 | 0.004 | 0.002 | 0.127 |
| Exe_Turnover | 0.259 | 0.000 | 0.438 | 0.000 | 1.000 | 0.202 | 0.242 | -0.040 | 0.077 |
| EXE_TEN | 5.166 | 5.000 | 2.956 | 1.000 | 15.000 | 5.636 | 5.519 | 0.117 | 0.449 |
| EXE_OWN | 0.003 | 0.000 | 0.019 | 0.000 | 0.577 | 0.006 | 0.003 | 0.003 | 0.040 |
| Pct_Miss_FTA_Tie | 0.038 | 0.000 | 0.173 | 0.000 | 1.000 | 0.017 | 0.028 | -0.011 | 0.428 |

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 *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)-(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 (1) | Ologit (2) | OLS (3) | Ologit (4) |
| Dep_Board2001*Post | 0.102*** (0.020) | 1.272*** (0.284) | 0.070*** (0.013) | 0.998*** (0.205) |
| CEOTEN | 0.019*** (0.003) | 0.305*** (0.047) | 0.015*** (0.002) | 0.286*** (0.036) |
| OUTSIDE | 0.004 (0.033) | -0.134 (0.466) | -0.067** (0.032) | -1.384*** (0.484) |
| FTA_1Y | 0.091*** (0.035) | 1.151** (0.534) | 0.138*** (0.029) | 2.343*** (0.455) |
| KNOWN | -0.308*** (0.055) | -4.262*** (0.772) | -0.453*** (0.042) | -7.174*** (0.770) |
| EXECSEN | -0.087*** (0.003) | -1.265*** (0.056) | -0.091*** (0.002) | -1.482*** (0.063) |
| FTA_1Y_Unknown | 0.985** (0.449) | 9.882 (10.145) | -0.078 (0.231) | -3.078 (3.169) |
| Female | -0.106 (0.097) | -1.572 (1.268) | -0.207*** (0.075) | -3.237*** (0.844) |
| Ln(CEOAge) | 0.216** (0.088) | 2.880** (1.213) | 0.139** (0.064) | 1.032 (1.040) |
| CEO_OWN _{t-1} | -0.453*** (0.163) | -6.087** (2.382) | -0.416*** (0.136) | -5.965*** (2.026) |
| CEO_Chair | 0.013 (0.026) | -0.009 (0.368) | 0.023 (0.024) | -0.095 (0.391) |
| Ln(TotalAsset) _{t-1} | 0.048*** (0.017) | 0.760*** (0.193) | 0.058*** (0.011) | 0.857*** (0.166) |
| FirmAge | 0.003*** (0.001) | 0.032*** (0.012) | 0.001 (0.001) | 0.017 (0.012) |
| Segment _{t-1} | -0.001* (0.001) | -0.022** (0.011) | -0.001 (0.001) | -0.020* (0.011) |
| Constant | -0.608 (0.390) | | 0.356 (0.254) | |
| Firm FE & Year FE (Dummies) | Y | Y | Y | Y |
| Observations | 6,581 | 6,581 | 2,998 | 2,998 |
| Adjusted-R ² (Pseudo-R ²) | 0.703 | (0.5377) | 0.745 | (0.5115) |

Table 4: Executive Turnovers and the Independent Board Requirement.

This table estimates the impact of the independent board requirement on executive and CEO turnovers. The dependent variable in Columns (1) and (2) is the percentage of new top-four non-CEO executives who were not on the list of top-four in the previous year. The dependent variable in Columns (3) and (4) is an indicator of CEO turnover. Columns (1) and (3) and Columns (2) and (4) 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 in Columns (1) and (2) are estimated by the OLS with firm- and year fixed effects; regressions in Columns (3) and (4) are estimated by the firm level conditional logit model with year dummies. Robust standard errors reported in parentheses are clustered at the firm level in Columns (1) and (2) and are corrected by bootstrapping 200 times in Columns (3) and (4). Coefficients marked with *, **, and *** are significant at 10%, 5%, and 1%, respectively.

| | Pct_Exec_Turnover | | CEO_Turnover | |
|-------------------------------|----------------------|---------------------|----------------------|---------------------|
| | Unmatched (1) | PS-matched (2) | Unmatched (3) | PS-matched (4) |
| Dep_Board2001*Post | 0.042*** (0.015) | 0.022* (0.013) | 0.365 (0.292) | 0.123 (0.309) |
| Return _{t-1} | -0.011*** (0.004) | -0.013** (0.006) | -0.299*** (0.094) | -0.324** (0.159) |
| Ln(TotalAsset) _{t-1} | 0.034*** (0.009) | 0.031*** (0.010) | 0.026 (0.123) | 0.148 (0.212) |
| EXECSEN _{t-1} | 0.061*** (0.002) | 0.057*** (0.003) | | |
| Avg_EXE_OWN _{t-1} | 0.269 (0.267) | 0.216 (0.368) | | |
| CEOTEN _{t-1} | | | 0.267*** (0.031) | 0.219*** (0.042) |
| CEO_OWN _{t-1} | | | -3.378 (2.445) | -5.170 (3.446) |
| CEO_Chair _{t-1} | | | -0.001 (0.212) | 0.193 (0.333) |
| Constant | 0.183*** (0.065) | 0.192*** (0.068) | | |
| Firm FE & Year FE | Y | Y | N | N |
| Year Dummies | N | N | Y | Y |
| Observations | 8,155 | 3,500 | 5,670 | 2,383 |
| Adjusted R-squared | 0.261 | 0.243 | | |
| pseudo-R-squared | | | 0.170 | 0.162 |

Table 5: Types of Executives Dropped from the Top-Four Non-CEO Executive List and the Independent Board Requirement.

This table estimates the impact of the independent board requirement on the likelihood of a top-four non-CEO executive to be dropped from the list of top-four non-CEO executives, separately for current CEOs' appointees, *Cur_CEO_Appt*, and previous CEOs' appointees, *Pre_CEO_Appt*. 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, $L-EXETEN = 1$. 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 | | | |
|--------------------|----------------------|----------------------|--|--|
| | Cur_CEO_Appt | Pre_CEO_Appt | Cur_CEO_Appt & L_EXETEN=1 | Pre_CEO_Appt & L_EXETEN=1 |
| | (1) | (2) | (3) | (4) |
| Dep_Board2001*Post | -0.029 (0.130) | 0.231** (0.102) | -0.010 (0.282) | 0.343*** (0.121) |
| EXETEN | -0.011** (0.005) | 0.026*** (0.004) | -0.000 (0.009) | 0.032*** (0.004) |
| EXE_OWN | 1.388 (1.135) | -1.239 (0.998) | -0.132 (0.994) | -1.034 (1.023) |
| Return | -0.147*** (0.048) | -0.084*** (0.024) | -0.259*** (0.095) | -0.111*** (0.037) |
| Ln(TotalAsset) | 0.136** (0.062) | 0.143*** (0.055) | 0.068 (0.161) | 0.069 (0.071) |
| Year Dummies | Y | Y | Y | Y |
| Observations | 15,368 | 21,583 | 4,668 | 15,323 |
| pseudo-R-squared | 0.00437 | 0.00754 | 0.0123 | 0.0121 |

Table 6: 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 current 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 year- and firm 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 are corrected by bootstrapping 200 times in Columns (2). Coefficients marked with *, **, and *** are significant at 10%, 5%, and 1%, respectively.

| | Ln(Exe_Tie+1) | |
|-------------------------------|---------------------|---------------------|
| | Unmatched | PS- matched |
| | (1) | (2) |
| Dep_Board2001*Post | 0.036* (0.019) | 0.037** (0.016) |
| CEOTEN | 0.007*** (0.002) | 0.009*** (0.002) |
| OUTSIDE | 0.052** (0.022) | 0.058** (0.030) |
| FTA_1Y | -0.002 (0.023) | -0.027 (0.025) |
| FTA_1Y_Unknown | -0.275 (0.244) | -0.042 (0.161) |
| KNOWN | -0.037 (0.051) | -0.104* (0.057) |
| EXECSEN | -0.005 (0.003) | -0.007* (0.004) |
| Female | 0.083*** (0.031) | 0.024 (0.034) |
| Ln(CEOAge) | -0.063 (0.060) | -0.124** (0.060) |
| CEO_OWNT _{t-1} | -0.056 (0.097) | -0.097 (0.119) |
| CEO_Chair | -0.017 (0.012) | -0.020 (0.015) |
| Ln(TotalAsset) _{t-1} | 0.016* (0.008) | 0.007 (0.009) |
| FirmAge | -0.000 (0.001) | -0.001 (0.001) |
| Segment _{t-1} | -0.001 (0.001) | -0.001 (0.001) |
| Pct_Miss_FTA_Tie | -0.022 (0.021) | -0.045* (0.025) |
| FTA | 0.034* (0.020) | 0.014 (0.025) |
| Constant | 0.211 (0.262) | 0.618** (0.280) |
| Firm FE & Year FE | Y | Y |
| Observations | 6,581 | 2,998 |
| Adjusted R-squared | 0.433 | 0.443 |

Table 7: Reestimation for Firms Exempted from Section 404 of the Sarbanes-Oxley Act.

This table reestimates difference-in-differences in the fraction of top executives appointed (FTA) during a CEO's tenure and their prior social ties to the CEO with a subsample of firms with public float less than \$75 Million in 2002, which exempt them from Section 404 of the SOX. The dependent variable is FTA in Columns (1) and (2) and Ln(Exe_Tie+1) in Columns (3) and (4). The regressions do not include Dep_Board2001 and Post as separate controls because of firm- and year fixed effects. All regressions are estimated with the unmatched sample. The sample period is 1996 – 2006, excluding 2002. Definitions of all variables are provided in Appendix 1. All regressions control for year- and firm fixed effects. Robust standard errors reported in parentheses are clustered at the firm level. Coefficients marked with *, **, and *** are significant at 10%, 5%, and 1%, respectively.

| | FTA | | Ln(Exe_Tie+1) | |
|-------------------------------|--------------------|----------------------|-------------------|----------------------|
| | (1) | (2) | (3) | (4) |
| Dep_Board2001*Post | 0.457** (0.170) | 0.189* (0.110) | 0.143* (0.081) | 0.079 (0.070) |
| CEOTEN | | 0.025 (0.026) | | 0.014 (0.010) |
| OUTSIDE | | 0.265 (0.267) | | -0.143 (0.142) |
| FTA_1Y | | 0.418** (0.171) | | -0.151 (0.127) |
| KNOWN | | 0.115 (0.138) | | 0.576*** (0.149) |
| EXECSSEN | | -0.111*** (0.022) | | 0.003 (0.025) |
| Female | | -0.177 (0.106) | | 0.218*** (0.060) |
| Ln(CEOAge) | | 1.449*** (0.346) | | -0.838** (0.295) |
| CEO_OWN _{t-1} | | -0.591 (0.518) | | -0.703*** (0.160) |
| CEO_Chair | | 0.016 (0.083) | | -0.116* (0.062) |
| Ln(TotalAsset) _{t-1} | | -0.075 (0.059) | | 0.030 (0.055) |
| FirmAge | | 0.019 (0.019) | | -0.026** (0.009) |
| Segment _{t-1} | | 0.009* (0.005) | | 0.006 (0.004) |
| Pct_Miss_FTA_Tie | | | | -0.168** (0.076) |
| FTA | | | | 0.316** (0.133) |
| Constant | 0.343** (0.116) | -5.359*** (1.494) | -0.026 (0.060) | 3.026** (1.108) |
| Firm FE & Year FE | Y | Y | Y | Y |
| Observations | 106 | 85 | 106 | 85 |
| Adjusted R-squared | 0.237 | 0.851 | 0.369 | 0.483 |

Table 8: Confounding Effects.

This table estimates confounding effects of pre-regulation events on the fraction of top-four non-CEO executives appointed during a current CEO's tenure (*FTA*) and social ties between the CEO and top-four non-CEO executives appointed during his tenure ($\ln(\text{Exe_Tie}+1)$). 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. Columns (1)-(2) and (3)-(4) report results estimated with the unmatched and the propensity-score (PS) sample. The sample period is 1996 – 2006, excluding 2002. Definitions of variables are provided in the Appendix. All regressions control for year- and firm fixed effects. Robust standard errors reported in parentheses are clustered at the firm level in Columns (1) and (2) and are corrected by bootstrapping 200 times in Columns (3) and (4). Coefficients marked with *, **, and *** are significant at 10%, 5%, and 1%, respectively.

| | Unmatched | | PS-matched | |
|-------------------------------|----------------------|----------------------|----------------------|----------------------|
| | FTA (1) | Ln(Exe_Tie+1) (2) | FTA (3) | Ln(Exe_Tie+1) (4) |
| Dep_Board2001*2000 | 0.012 (0.020) | -0.003 (0.024) | -0.000 (0.022) | -0.009 (0.026) |
| Dep_Board2001*2001 | 0.032 (0.024) | -0.027 (0.026) | 0.019 (0.021) | -0.031 (0.026) |
| Dep_Board2001*2003 | 0.080*** (0.024) | 0.009 (0.024) | 0.055*** (0.020) | 0.010 (0.026) |
| Dep_Board2001*2004 | 0.102*** (0.027) | 0.022 (0.026) | 0.069*** (0.021) | 0.022 (0.025) |
| Dep_Board2001*2005 and after | 0.133*** (0.028) | 0.046* (0.026) | 0.087*** (0.022) | 0.042** (0.021) |
| CEOTEN | 0.019*** (0.003) | 0.007*** (0.002) | 0.015*** (0.002) | 0.009*** (0.001) |
| OUTSIDE | 0.004 (0.033) | 0.052** (0.022) | -0.067** (0.033) | 0.060** (0.024) |
| FTA_1Y | 0.091*** (0.035) | -0.002 (0.023) | 0.137*** (0.029) | -0.026 (0.023) |
| FTA_1Y_Unknown | 0.974** (0.450) | -0.275 (0.243) | -0.084 (0.222) | -0.024 (0.598) |
| KNOWN | -0.308*** (0.055) | -0.038 (0.051) | -0.454*** (0.049) | -0.107* (0.056) |
| EXECSSEN | -0.087*** (0.003) | -0.005 (0.003) | -0.091*** (0.003) | -0.007* (0.004) |
| Female | -0.107 (0.097) | 0.083*** (0.031) | -0.209*** (0.066) | 0.024 (0.081) |
| Ln(CEOAge) | 0.215** (0.088) | -0.062 (0.060) | 0.139** (0.063) | -0.120** (0.052) |
| CEO_OWNI _{t-1} | -0.447*** (0.163) | -0.054 (0.097) | -0.410*** (0.134) | -0.095 (0.121) |
| CEO_Chair | 0.013 (0.026) | -0.017 (0.012) | 0.023 (0.026) | -0.020 (0.017) |
| Ln(TotalAsset) _{t-1} | 0.048*** (0.017) | 0.016* (0.008) | 0.058*** (0.011) | 0.008 (0.011) |
| FirmAge | 0.003*** (0.001) | -0.000 (0.001) | 0.001 (0.001) | -0.001 (0.001) |
| Segment _{t-1} | -0.001* (0.001) | -0.001 (0.001) | -0.001* (0.001) | -0.001 (0.001) |
| Pct_Miss_FTA_Tie | | -0.022 (0.021) | | -0.046* (0.027) |
| FTA | | 0.033 (0.020) | | 0.013 (0.024) |
| Constant | -0.600 (0.389) | 0.205 (0.261) | 0.351 (0.268) | 0.602*** (0.232) |
| Firm FE & Year FE | Y | Y | Y | Y |
| Observations | 6,581 | 6,581 | 2,998 | 2,998 |
| Adjusted R-squared | 0.703 | 0.433 | 0.745 | 0.442 |

Table 9: Heterogeneity in the Treatment Effects.

This table 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 and social ties between the CEO and the executives appointed during his tenure ($\text{Ln}(\text{Exe_Tie}+1)$). The key independent variable is $\text{Pct_Dep_Board2001*Post}$, the percentage of non-independent directors in 2001 interacted with the post-regulation indicator. The regression does not include Pct_Dep_Board2001 and Post as separate controls because of firm- and year fixed effects. Columns (1)-(2) and Columns (3)-(4) report estimation results with the unmatched sample and the 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 year- and firm fixed effects. Robust standard errors reported in parentheses are clustered at the firm level in Columns (1) and (2) are corrected by bootstrapping 200 times in Columns (3) and (4). Coefficients marked with *, **, and *** are significant at 10%, 5%, and 1%, respectively.

| | Unmatched | | PS-matched | |
|--------------------------------------|----------------------|--------------------------------|----------------------|--------------------------------|
| | FTA | $\text{Ln}(\text{Exe_Tie}+1)$ | FTA | $\text{Ln}(\text{Exe_Tie}+1)$ |
| | (1) | (2) | (3) | (4) |
| $\text{Pct_Dep_Board2001*Post}$ | 0.279*** (0.048) | 0.046* (0.027) | 0.199*** (0.036) | 0.044* (0.023) |
| CEOTEN | 0.018*** (0.003) | 0.007*** (0.002) | 0.015*** (0.002) | 0.009*** (0.002) |
| OUTSIDE | 0.005 (0.033) | 0.052** (0.022) | -0.068** (0.031) | 0.058** (0.027) |
| FTA_1Y | 0.087** (0.034) | -0.002 (0.023) | 0.140*** (0.029) | -0.027 (0.024) |
| FTA_1Y_Unknown | 1.016** (0.456) | -0.280 (0.244) | -0.065 (0.233) | -0.060 (0.156) |
| KNOWN | -0.321*** (0.054) | -0.037 (0.051) | -0.459*** (0.054) | -0.104* (0.063) |
| EXECSEN | -0.088*** (0.003) | -0.005 (0.003) | -0.091*** (0.002) | -0.007* (0.004) |
| Female | -0.118 (0.098) | 0.082*** (0.031) | -0.224*** (0.067) | 0.022 (0.031) |
| $\text{Ln}(\text{CEOAge})$ | 0.211** (0.089) | -0.063 (0.060) | 0.139** (0.063) | -0.124** (0.061) |
| CEO_OWN_{t-1} | -0.411** (0.159) | -0.058 (0.097) | -0.366** (0.151) | -0.101 (0.109) |
| CEO_Chair | 0.014 (0.026) | -0.017 (0.012) | 0.025 (0.022) | -0.020 (0.016) |
| $\text{Ln}(\text{TotalAsset})_{t-1}$ | 0.048*** (0.017) | 0.016* (0.008) | 0.061*** (0.011) | 0.008 (0.010) |
| FirmAge | 0.003*** (0.001) | -0.000 (0.001) | 0.001 (0.001) | -0.001 (0.002) |
| Segment_{t-1} | -0.002* (0.001) | -0.001 (0.001) | -0.001* (0.001) | -0.001 (0.001) |
| Pct_Miss_FTA_Tie | | -0.022 (0.021) | | -0.045 (0.029) |
| FTA | | 0.034* (0.020) | | 0.015 (0.027) |
| Constant | -0.571 (0.394) | 0.207 (0.262) | 0.277 (0.252) | 0.618** (0.275) |
| Firm FE & Year FE | Y | Y | Y | Y |
| Observations | 6,581 | 6,581 | 2,998 | 2,998 |
| Adjusted R-squared | 0.705 | 0.433 | 0.746 | 0.442 |

Table 10: Alternative Definitions of FTA.

This table re-estimates 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*. Definitions of all variables are provided in the Appendix. Panels A and B report results estimated with the unmatched sample and the propensity-score (PS) matched sample, respectively. The sample period is 1996 – 2006, excluding 2002. 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 Panel A and are corrected by bootstrapping 200 times in Panel B. Coefficients marked with *, **, and *** are significant at 10%, 5%, and 1%, respectively.

| | Panel A: Unmatched | | | Panel B: PS-matched | | |
|-------------------------------|----------------------|----------------------|----------------------|----------------------|----------------------|----------------------|
| | WFTA (1) | AFTA (2) | WAFTA (3) | WFTA (4) | AFTA (5) | WAFTA (6) |
| Pct_Dep_Board2001*Post | 0.095*** (0.021) | 0.088*** (0.019) | 0.083*** (0.020) | 0.057*** (0.015) | 0.074*** (0.013) | 0.067*** (0.014) |
| CEOTEN | 0.019*** (0.003) | | | 0.016*** (0.002) | | |
| OUTSIDE | -0.001 (0.034) | | | -0.083** (0.037) | | |
| FTA_1Y | 0.078** (0.034) | | | 0.117*** (0.031) | | |
| FTA_1Y_Unknown | 1.027** (0.491) | | | -0.108 (0.336) | | |
| KNOWN | -0.289*** (0.057) | | | -0.456*** (0.055) | | |
| EXECSEN | -0.086*** (0.003) | | | -0.090*** (0.002) | | |
| Female | -0.145 (0.099) | -0.081 (0.108) | -0.120 (0.109) | -0.259*** (0.089) | -0.157* (0.088) | -0.201* (0.106) |
| Ln(CEOAge) | 0.203** (0.091) | 0.364*** (0.069) | 0.360*** (0.070) | 0.141** (0.067) | 0.251*** (0.051) | 0.268*** (0.048) |
| CEO_OWNT _{t-1} | -0.433*** (0.161) | -0.307* (0.166) | -0.287* (0.161) | -0.426*** (0.125) | -0.238* (0.137) | -0.246* (0.142) |
| CEO_Chair | 0.010 (0.026) | 0.013 (0.024) | 0.011 (0.025) | 0.013 (0.023) | 0.020 (0.023) | 0.009 (0.021) |
| Ln(TotalAsset) _{t-1} | 0.049*** (0.018) | 0.057*** (0.016) | 0.058*** (0.017) | 0.058*** (0.013) | 0.081*** (0.011) | 0.083*** (0.013) |
| FirmAge | 0.003** (0.001) | 0.005*** (0.001) | 0.005*** (0.001) | 0.001 (0.001) | 0.005*** (0.002) | 0.005*** (0.002) |
| Segment _{t-1} | -0.001 (0.001) | 0.001 (0.001) | 0.001 (0.001) | -0.001 (0.001) | 0.002** (0.001) | 0.002** (0.001) |
| Constant | -0.598 (0.400) | -1.986*** (0.293) | -1.976*** (0.300) | 0.353 (0.292) | -1.710*** (0.195) | -1.788*** (0.177) |
| Firm FE | Y | Y | Y | Y | Y | Y |
| Year FE | Y | N | N | Y | N | N |
| Observations | 6,581 | 6,581 | 6,581 | 2,998 | 2,998 | 2,998 |
| Adjusted-R ² | 0.703 | 0.517 | 0.523 | 0.743 | 0.551 | 0.553 |