

Managerial Power and CEO Compensation in Financially Distressed Firms*

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Abstract

We study whether financial distress affects managerial power and Chief Executive Officers' (CEOs) compensation in publicly traded non-financial firms. Using a bias-corrected matching estimator to identify suitable controls, we find that both governance structure and CEO compensation change when firms enter into financial distress. CEOs are less likely to be the board chairperson, and fewer executives sit on the board or on the compensation committee. CEO compensation decreases considerably for both incumbents and successors, and the decrease is driven by declines in stock-based pay. There are also fewer cases of opportunistic timing of stock option awards.

JEL Classification: G30, J33, M52

Keywords: Managerial influence, CEO compensation, financial distress, lucky grants, bias-corrected matching estimators

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Abstract

We study whether financial distress affects managerial power and Chief Executive Officers' (CEOs) compensation in publicly traded non-financial firms. Using a bias-corrected matching estimator to identify suitable controls, we find that both governance structure and CEO compensation change when firms enter into financial distress. CEOs are less likely to be the board chairperson, and fewer executives sit on the board or on the compensation committee. CEO compensation decreases considerably for both incumbents and successors, and the decrease is driven by declines in stock-based pay. There are also fewer cases of opportunistic timing of stock option awards.

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1 Introduction

Executive pay in U.S. public companies has been rising steadily since the 1990s. The trend persisted until the 2007-2009 recession, during which paychecks for top American executives first shrank but quickly resumed significant growth afterwards (Costello, 2011). Several high-publicity controversies around Wall Street pay packages in the middle of the financial crisis have triggered public outrage about executive compensation. In response, the Dodd-Frank Wall Street Reform and Consumer Protection Act was passed into law in 2010. Among several other rules related to corporate governance, the law provides shareholders with a say on executive pay with a non-binding vote on executive compensation and golden parachutes, requires compensation committee independence, and includes requirements that public companies set policies to take back executive compensation based on inaccurate financial statements (Zaunbrecher, Schweitzer and Hagler, 2010).

Implicit in these regulations is the view that a large fraction of CEO compensation reflects rent extraction by powerful CEOs and is thus deemed inefficient (for supporting evidence see, e.g., Bertrand and Mullainathan, 2001, and Bebchuk and Fried, 2004; for a review see, e.g., Weisbach, 2007). In contrast, a body of the executive pay literature posits that compensation, equity-based compensation in particular, aligns managerial interests with shareholder interests and thus ameliorates agency problems (see, e.g., the surveys by Murphy, 1999, and Core, Guay and Larcker, 2003). Because many observed executive pay patterns appear inconsistent with the predictions of standard optimal contracting models, researchers have conducted studies to extend these standard models (see the survey by Edmans and Gabaix, 2009). Nevertheless, no consensus view has emerged, and there is still much to learn about the determinants of CEO compensation.

This paper contributes to understanding the determinants of CEO pay by studying the dynamics of the changes in managerial power and CEO compensation when public firms go through financial distress. We apply Abadie and Imbens' (2011) bias-corrected matching estimator to find the group of suitable controls. This group consists of firms that are not only never financially distressed but also are, at least in a statistical sense, similar in observed characteristics to the financially distressed firms (i.e., the "treated") *before* they become distressed. Because pay practices and regulations change over time, our use of control firms for constructing counterfactual outcomes (i.e., what the outcomes would have been for the distressed companies if they had not been in financial distress)

filters out those changes and properly identifies the effect of financial distress. Specifically, by contrasting the comparable firms with those firms under financial distress, we estimate the effects of financial distress on managerial influence over the board, CEO compensation packages, and opportunistic timing of option grants over the period from 1992 to 2005.¹ Our study shows that financial distress curtails managerial power as measured by managerial influence over the board, reduces stock-based CEO pay (both for incumbent and successors), and decreases the occurrences of opportunistic timing of stock option awards.

Our study is naturally linked to the literature on the relation between corporate governance and firm performance. Both managerial power and executive compensation packages belong to a broader set of corporate governance mechanisms. Many prior papers have concentrated on the effects of governance on performance (see, e.g., Hermalin and Weisbach, 1991; Gompers, Ishii, and Metrick, 2003; Adams, Almeida, and Ferreira, 2005; Bebchuk, Cohen, and Ferrell, 2009), without paying particular attention to the case of distressed firms. We investigate the other direction of this relation, i.e., the impact of firm performance on corporate governance, in the context of financial distress. It has been argued that falling into financial distress can affect various aspects of a firm’s governance such as the realignment of fiduciary duties among stakeholders, shakeups in the management team and/or the board, restructuring of management compensation, and changes in ownership and corporate controls (see, e.g., the survey in Hotchkiss et al., 2008). Our empirical strategy differs markedly from the majority of the studies in this literature because we construct a control group. As shown in Sections 4 and 5, if we do not take into account the counterfactual outcome provided by the control group, we would either overestimate or underestimate the effects of financial distress, depending on the particular outcome.

Corporate governance is a complex construct that involves many dimensions. Our analysis focuses on a set of corporate governance variables that measure managerial influence over the board. The metrics include number of executives serving as board directors, number of executives

¹As explained in Sections 2 and 3, we select the group of appropriate controls by matching firms in the same calendar periods, on size and industry indicators, and on a rich set of firm characteristics measuring financial health, CEO compensation, and managerial influence over the board three years *prior to* the advent of financial distress. The key assumption of this approach is that, for firms that have the same value of the matching variables, the event of financial distress is “as if” it had been random. This allows for answering the question: “how does financial distress affect a firm’s corporate governance, CEO compensation, and opportunistic timing of stock option awards?” See Section 3 and Appendix for further details.

serving in the compensation committee of the board, whether the CEO serves as the board chair, whether at least one executive serves as a board director, and whether at least one executive serves in the compensation committee.² We report a noticeable loss of managerial influence over the board among the treated firms after they enter into financial distress. In particular, following the incidence of financial distress, the proportion of CEOs holding the chairmanship, and the proportion and the number of executives serving as directors or sitting in the compensation committee of the board all decrease significantly.

Our paper also contributes to the under-studied empirical literature on executive compensation for financially distressed firms. Gilson and Vetsuypens (1993) examine executive compensation in financially distressed firms during the 1980s and find that compensation becomes more sensitive to performance after distress. Henderson (2007) analyzes a similar period to the one in our paper, and concludes that compensation practices remain largely unchanged after financial distress. Our study differs from these two studies in two fundamental aspects. First, the two studies employ a similar empirical strategy that compares executive pay before and after distress among financially distressed firms only, while we use a different strategy by contrasting the changes in executive pay before and after distress for the treated firms with the changes for an appropriately chosen control group. As discussed above, using controls is the only way to account for the impact of market-wide shifts in compensation practices and to obtain a proper assessment of the effect of financial distress on executive compensation. Second, while the two prior studies focus on executive compensation, our analysis also explores the dynamics of changes in both executive compensation and managerial influence over the board. Our study of this dynamics can potentially provide new insights into the relationship between these two dimensions.

Our paper analyzes various components of CEO compensation such as total cash compensation, stock grants, stock option grants, and total flow compensation. We document that CEO turnover rates increase markedly in the distressed firms. Surviving CEOs experience significant reductions in total cash compensation and total flow compensation after the firms fall into financial

²There are two other popular corporate governance measures in the literature, namely, Gompers, Ishii, and Metrick's (2003) G-index and Bebchuk, Cohen, and Ferrell's (2009) E-index. These two indexes are only available every two to three years and are quite persistent across years. As the overlap between the firms in our sample and the firms for which those indexes have been calculated is quite low, it is not feasible to use those indexes in our study. We face the same issue that there are very few treated firms for which the data is available for other corporate governance measures such as board size, number of blockholders, and fraction of independent directors, etc.

distress. Replacement CEOs at financially distressed firms are also paid significantly less than their predecessors and than replacement CEOs at non-distressed firms. For both incumbent and replacement CEOs, the bulk of their total compensation reduction derives from the decline in value of new grants of stock options. In contrast, Gilson and Vetsuypens (1993) report that, when firms fall into financial distress, the reduction in cash-based compensation (salary and bonus) drives the decline in executive compensation. This difference between their finding and ours reflects the shift of executive compensation practices in the U.S. from bonus-based pay in the 1980s to stock-based pay in the 1990s and beyond. Nevertheless, the bulk of our results are in line qualitatively with Gilson and Vetsuypens' (1993) findings. This evidence suggests that, even though the compensation practices have changed significantly over time, the effects of financial distress on *total* executive compensation are robust to different time periods under study.

Our paper also relates to the growing literature on managerial opportunistic timing of option grants. For example, Lie (2005), Heron and Lie (2006), and Narayanan and Seyhun (2008) all find that firms' abnormal stock returns are negative before executive option grants and positive afterward and attribute the stock return pattern to managerial backdating of option grants. Collins, Gong, and Li (2009) report that weaker corporate governance contributes to the episode of backdating executive stock option grants. Bebchuk, Grinstein, and Peyer (2010) examine the ranking of a grant price in the distribution of stock prices during the month of the grant and show that "lucky" grants, i.e., the options issued at or below the minimum observed stock price in the grant month, provide a useful tool for studying such managerial behavior. Our paper contributes to this literature by studying such managerial opportunistic behavior in firms under financial distress.

We follow Bebchuk, Grinstein and Peyer's (2010) approach and focus on how grant prices rank within the price distribution of the grant month. We find that the proportion of lucky grants for the treated firms is higher before falling into financial distress and lower upon and after becoming distressed, while the proportion of lucky grants for the controls remains fairly stable throughout the same window under study. We also present evidence of the weakening of such managerial behavior after the enacting of the Sarbanes-Oxley Act of 2002 for both healthy and financially distressed firms, corroborating the findings of Heron and Lie (2006).

In summary, we find that the occurrence of financial distress weakens managerial power, likely

due to a greater scrutiny from stake-holders such as creditors and shareholders as well as from the bankruptcy court judge if Chapter 11 protection is sought. While we observe diminishing managerial influence over the board in distressed firms, we also find significant declines in CEOs' stock-based pay (for both incumbent and replacement CEOs) and in opportunistic timing of option grants. Taken together, the results are suggestive of a link between managerial power and executive compensation.

Two caveats are in order. First, it is very difficult to gather data once a firm goes into distress. Most distressed companies stop making regular proxy and 10-K filings, and collecting executive pay data from monthly 8-K filings or bankruptcy court dockets can be very difficult and time consuming. Thus, any study in this area, including ours, has to deal with the challenge of working with small samples.³ We face precision issues for some of our results, but in general, even with small samples our main findings are statistically significant. Second, our compensation data is based on the S&P 1500 sample of firms. Standard & Poor's, which collects the data, actively screens and deletes distressed companies from the sample. As a result, the troubled firms we ultimately include in our study may represent the least-distressed companies, potentially creating a survivorship bias. Given that non-surviving firms likely experience larger declines in managerial power than surviving firms, our findings are probably conservative estimates of the effects of financial distress.

The paper proceeds as follows. Section 2 describes the data and the sample for our study. Section 3 discusses details of the empirical strategy. Section 4 examines the dynamics of managerial influence over the board of directors before and after financial distress. Section 5 presents the effects of financial distress on CEO compensation, CEO turnover, and opportunistic option timing practices. Section 6 concludes. The Appendix explains the matching estimator.

³Gilson and Vetsuypens (1993), and Henderson (2007) use samples of only 77 and 76 firms, respectively. Other studies on different aspects of financial distress face similar constraints. For example, Bernstein (2006), in a study of CEO turnover in financially distressed firms, compares 79 bankrupt firms to 1,288 firms "suffering from poor financial circumstances", as determined by their three-year cumulative stock returns. As a comparison, we study 99 financially distressed firms, and select our controls from a group of 1,205 non-distressed firms. Some recent studies on the opportunistic timing of option grants face similar constraints as well; their samples typically range from 50 to 150 firms (see, e.g., Walker, 2007; and Narayanan, Schipani, and Seyhun, 2007).

2 Data and Sample Construction

Data for this study comes from several sources. We use executive compensation data from Standard and Poor’s (S&P) ExecuComp database. The database reports annual compensation flows as well as information related to changes in the value of stock and stock option holdings for the five highest paid executives, including the CEO, for each firm appearing in the S&P500 Index, S&P MidCap 400 Index, and the S&P SmallCap 600 Index. The database also contains some information about these executives’ positions in the board. Firms’ annual accounting data comes from S&P’s Compustat database. We obtain stock return data from the Center for Research in Security Prices (CRSP) Monthly Stock File. We take bankruptcy filing information from Professor Lynn LoPucki’s Bankruptcy Research Database (BRD). Throughout our empirical analysis, we measure all monetary values in 2005 constant dollars, and we adjust nominal stock returns by the Consumer Price Index (CPI) from the Bureau of Labor Statistics to obtain real returns.

2.1 Variables

Three sets of variables are key to this empirical study. The first set is the measure of financial distress with which we classify firms into financially distressed or healthy. The financially distressed firms in our sample consist of two (in some cases overlapping) groups. One group is formed by the firms that have filed for Chapter 11 bankruptcy and are covered in the BRD. The other group of financially distressed firms are identified based on a combination of the past three-year cumulative stock returns and Ohlson’s (1980) O-scores.⁴ Specifically, at each year-end and from the universe of all Compustat firms having non-missing information for both the O-scores and the prior three-year stock returns, we rank firms into percentiles based separately on their O-scores and three-year cumulative returns; we exclude financial firms (SIC between 6000 and 6999) from the rankings. We then classify firms as financially distressed if the following two conditions are satisfied *simultaneously*: their O-scores are in the top quintile of the O-score distribution and their

⁴To calculate the past three-year cumulative stock returns, we require at least 18 months of valid data within the three-year period. Ohlson’s (1980) O-score is a widely used measure for a firm’s financial status, and it is obtained from a probabilistic prediction of bankruptcy with a set of financial ratios including the logarithm value of total assets, the ratio of total liabilities to total assets, the ratio of working capital to total assets, the ratio of current liabilities to current assets, the ratio of net income to total assets, the ratio of funds from operation to total liabilities, the growth rate in net income, the dummy for total liabilities exceeding total assets, and the dummy for negative net income for the last two years. Typically, the higher is the value of the O-score, the more likely is the firm to go bankrupt.

past three-year cumulative returns are in the bottom quintile of the cumulative return distribution.⁵

The second set of variables consists of the covariates we use to *directly* match distressed firms with control firms. To identify the impact of financial distress we resort to the Abadie and Imbens' (2011) matching estimator. (See Section 3 for a detailed discussion of the estimator.) The matching covariates include measures of a firm's financial health, measures of CEO compensation, measures of managerial influence over the board, industry dummies, and year dummies. All the matching covariates are evaluated three years prior to the occurrence of financial distress. Specifically, to measure a firm's financial health, we use the raw values of O-scores, the past three-year cumulative stock returns, and firm size calculated as the log value of market capitalization. We also define three dummy variables based on the three raw measures to characterize the relative position of a firm in the respective cross-sectional distributions: the O-score-based dummy is equal to one if a firm's O-score is at or above the top quintile of all firms in Compustat, and zero otherwise; the cumulative-return-based dummy is equal to one if a firm's past three-year cumulative return is at or below the bottom quintile of all firms in Compustat, and zero otherwise; and the size-based dummy is equal to one if a firm's market capitalization is at or above the top 30 percentile of all firms in Compustat, and zero otherwise. Because the literature documents that leverage is an important determinant of a firm's financial status we include the debt-to-asset ratio. To achieve good comparability between the firms in the treated and control groups, we also match on the level and structure of CEO compensation packages; we thus include both total current compensation and total direct compensation as matching covariates. We further match on three managerial influence measures because CEO power could affect both firm performance and CEO compensation practices. The executives-on-compensation-committee dummy is equal to one if one of the top five executives sit on the board's compensation committee and zero otherwise. The executives-on-board dummy is equal to one if one of the top five executives serve as the board directors and zero otherwise. The CEO-chair dummy is equal to one if the CEO is also the board's chairperson and zero otherwise. Finally, to control for industry-wide differences we adopt Fama and French's five-industry definition based on the four-digit SIC code and create five industry dummies on which we also match, and we use fiscal year dummies to control for business-cycle-related effects.

⁵We also identify financially distressed firms with alternative combinations of those percentile cutoffs such as the top decile on O-scores and the bottom decile on cumulative stock returns; the results under those alternative classifications are qualitatively similar and are available upon request.

The third set of key variables is comprised of three groups of outcome variables: managerial influence over the board of directors, CEO compensation, and opportunistic timing of option grants. We defer the definitions of these outcome variables to Sections 4 and 5 when we discuss the treatment effects of financial distress.

2.2 Sample Construction

Our sample spans the period from 1992 to 2005. Moreover, because we are interested in the evolution of outcome variables over time, we choose an analysis window spanning from two years before distress through three years after distress, a total of six years. We include the two pre-distress years to capture the cases where timing might be off if our definition of financial distress does not perfectly identify the timing of distress. Accordingly, we match the treated group to similar firms based on the matching covariates in the year immediately prior to the analysis window, i.e., three years before a firm becomes financially distressed. As a result, our analysis focuses on the firms that we identify as financially distressed between 1995 and 2002.

The treatment in our study is defined as the “event” that a firm falls into financial distress. We use the term financial distress broadly to include both bankruptcies and our measure of financial distress as described above. Most firms are in either of those two groups, but some qualify as financially distressed under both definitions. Because a firm can become financially distressed more than once over time, we restrict the treatment to be the *first time* that a firm becomes financially distressed (by either definition). In only a few cases we include a second spell in distress for the same firm, if at least seven years have passed since the end of the first distress spell. In order to maintain a clean potential pool of controls for the treated group, we drop from the pool of potential controls any solvent firm which has ever been financially distressed before.

Further, we apply the following two criteria to select firms into our analysis sample: 1) Firms do not have missing information for either the matching covariates in the matching year (i.e., three years before the treatment); and 2) firms have at most one missing variable in all compensation variables in the pre-treatment window (i.e., from two years before treatment to the treatment year) and at most one missing variable in all compensation variables in the post-treatment window (i.e., from one year after the treatment to three years after the treatment). As a result, our analysis

sample contains 99 firms that were ever in financial distress and 1,205 firms that have never been in financial distress during the 1995-2002 period.⁶

Table 1 breaks down the distribution of the treated group and the pool of potential controls of the analysis sample across years. To avoid introducing serial correlation in the estimation of the standard errors of our matching estimator, we assign each of the potential control firms to only one particular year. We defer to Section 3.1 the detailed explanation of how we determine the year for each potential control firm.

3 Empirical Strategy

We consider financial distress as a treatment, and following the terminology of the program evaluation literature, we construct a control group to estimate the effect of financial distress. The basic intuition is that the control group allows us to determine the *counterfactual*, i.e., what the outcome variables of the distressed firms would have been if they had not suffered financial distress. In a standard regression framework, all the firms that do not suffer financial distress form the control group; however, as shown in the program evaluation literature, using such a control group could lead to biased inference. An alternative way is to only select as controls those non-distressed firms that are statistically similar to the treated firms in observable characteristics *before* the episode of financial distress.

In this paper we estimate the *Average Treatment Effect for the Treated (ATT)* of financial distress, using a matching estimator proposed by Abadie and Imbens (2011). This estimator allows matching directly on both continuous and discrete covariates and has several attractive features. It is simple to use, it implements a correction for potential biases generated by non-exact matching on (mostly) continuous variables, and it has well defined asymptotic variance.⁷ Most importantly, it is well suited and has desirable properties, compared for example to matching on the propensity

⁶Although we find 155 ExecuComp firms that file for Chapter 11 protection (i.e., covered in the BRD) in the 1995-2002 period, only 43 of those firms have enough valid information to be included in our analysis sample. Moreover, of those 43 firms, only 10 are classified as distressed based solely on their appearance in the BRD (i.e., because they filed for Chapter 11 protection).

⁷Being able to analytically calculate the asymptotic variance saves computing time with respect to estimating it by bootstrapping. Moreover, Abadie and Imbens (2008) show that bootstrapping fails for matching estimators. Recently, Abadie and Imbens (2009) have derived the large sample distribution of matching estimators, when matching on the estimated propensity score.

score (i.e., probability of treatment), in a case like the one under study — when the number of treated observations is small relative to the number of control observations, estimating a propensity score model with the usual logit or probit method can generate undesirable results.

Note that the key assumption for our econometric strategy to work is that, for firms that have the same value of the matching variables, the advent of financial distress is “as if” it had been a random event. That is, matching on a rich enough set of variables is enough to identify the appropriate counterfactual. In addition, as we discuss below, by relying on a difference-in-differences (DID) version of our estimator, we can further control for any unobservable characteristics of the firms that remain constant over time (e.g. “corporate culture”). This allows us to answer the question: “for firms which went into financial distress, what was the *causal* effect of financial distress on corporate governance, CEO compensation, and opportunistic timing of stock option awards?” The remainder of this section and the Appendix provide further details.

3.1 Issues in Estimating the ATT of Financial Distress

One difficulty in estimating the ATT of financial distress is that although we have properly defined the treated and control groups, i.e., the firms that ever go into financial distress versus the firms that never do in our analysis period, the *timing* of treatment (fiscal year in which the firm goes into distress) is only properly defined for the treated firms. This is relevant because the standard practice in estimating treatment effects is to define as time $t=0$ the time of the treatment and express all variables (both outcomes and covariates) with respect to time 0. The matching is performed on covariates that are *not* affected by the treatment (i.e. before $t=0$) and possibly include pre-treatment outcomes. The outcomes are the values of the post-treatment variables of interest.

In our case though, it is not properly defined what $t=0$ means for non-distressed firms because we observe most of the firms every year and in none of those years they go into financial distress. One solution to this problem would be to generate a dataset of “potential controls” in which any firm with valid information is “recentered” at time $t=0$ *every period* and is potentially included several times.⁸ For example, if firm XYZ appears in all the years from 1995 to 2002, then XYZ

⁸Because the matching estimator consists of selecting the best possible M matches (with replacement) per treated firm, some of these firms may never be good enough matches to be actually used as controls in the matching estimator.

would be included in the dataset eight times (once every year); and for each instance we could center each particular fiscal year as time $t=0$. In this way XYZ would appear once in the dataset where $t=0$ corresponds to the year 1995, $t=-1$ to 1994 and $t=+1$ to 1996; and appear at another time where $t=0$ corresponds to the year 1996, $t=-1$ to 1995 and $t=+1$ to 1997; etc.

However, a problem with that solution is that it would incorrectly assume that each instance (in different years) of the firm XYZ is independent of each other. The matching method deals effectively with sampling with replacement within the same period but cannot deal with the potential serial correlation introduced by the above exposed approach. To avoid that problem, we use each potential control firm *only once*. This means that we use firm XYZ as a potential control by considering $t=0$, say, either as 1995 or as 1996, but not both. One way to implement this restriction would be to just randomly select the year for which firms will be potential controls; such randomization avoids the serial correlation problem but might not use the available information efficiently.

An alternative and much better way is to assign each of these non-distressed firms to the year in which they could potentially be most useful. That is, we want to find among all possible years in which a non-distressed firm could be used as a control, the year in which the firm could be the best possible match for a treated firm. To implement this idea, we essentially apply our matching estimator twice. Specifically, in the first round, for each year we take all the financially distressed firms in that particular year, match them against all the non-distressed firms, and calculate the *Euclidean distance* between each treated firm and each non-distressed firm. Then for each non-distressed firm, we rank these distances *across all possible years* and pick out the treated firm with which the particular non-distressed firm has the smallest distance. Thus, the year in which that particular firm went into distress becomes the best possible year in which we can use the non-distressed firm as a control, across all possible years in which it could have been used as a control. Repeating this procedure for each non-distressed firm we essentially find the year in which each non-distressed firm could potentially be the best possible match for the treated, and we use each non-distressed firm as potential control in that year only. This is how we assign the potential controls to different years as presented in Table 1. Note that even in this situation all these chosen non-distressed firms are still *potential* controls (i.e., some of the non-distressed firms will never be

This is why we call these firms “potential controls”.

used in the matching estimation). In the second round, given that we have assigned each non-distressed firm to one and only one year, we pool together all the years and use the matching estimator again to find the best M matches for each treated firm in each year. We set the number of matches to $M = 4$, which Abadie and Imbens’ (2011) simulation analysis shows to minimize the root of the mean squared error.⁹

Also note that $t=0$ could potentially refer to different *calendar* periods for the treated and the controls in a matched pair. For example, a control firm in a “good” year for the overall market could be matched with a treated firm which goes into distress in a “bad” year for the market. To avoid this problem, we include the fiscal years of the treatment as matching covariates to force that the comparisons of the treated firms and the control firms are done within the same calendar period.

A second issue of importance is that our criteria for identifying a financially distressed firm includes the cumulative stock returns over the prior three years. It means that whenever we set time $t=0$ for a treated firm, the determination of $t=0$ is actually based (partly) on the stock performance not only in $t=0$, but also in $t=-1$ and $t=-2$. This potentially “contaminates” all the pre-treatment variables in $t=-1$ and $t=-2$, suggesting that the Conditional Independence Assumption (CIA), which is needed for our estimator to be valid (see the Appendix), may not hold in $t=-1$ and $t=-2$. The CIA implies that pre-treatment outcomes are not affected by the treatment. However, we know that by construction at least stock returns in each of these two pre-treatment years are low for firms going into distress at $t=0$. This can further affect other variables including those related to CEO compensation. Therefore, we perform the matching on covariates measured at $t=-3$, which should not be affected by the determination of the treatment status.¹⁰

3.2 Assessment of Matching Quality

Before discussing empirical results we assess the quality of matching which is critical to the success of properly identifying the treatment effect. Table 2 presents the summary statistics of the matching

⁹In our estimations we also tried alternative values of M , from one to four, and the results are similar and are available upon request.

¹⁰Also, note that for several of the compensation variables we observe outliers that could potentially affect the results. We thus drop from our analysis, for estimation, the observations with the lowest and highest values for every outcome variable, for the treated and the controls separately. Using different “trimming” rules, we obtain similar results which are available upon request.

covariates for the treated and the (potential) controls before and after the matching. As explained above, matching is done at period $t=-3$ (i.e. three years before financial distress).

The first panel of the table shows the mean and standard deviation of the matching covariates for the 99 treated firms, while the second panel shows the same information for the pool of 1,205 *potential* control firms. The third panel calculates the average difference and standard error in each matching covariate between the treated group and the potential controls. It appears that the two sets of firms are quite different in several matching covariates, particularly the year and industry distribution, O-score and the past three-year cumulative stock return. Specifically, relative to the pool of potential controls, 8% more of the treated firms are concentrated in the fiscal year 1998 but 16% less of the treated firms are concentrated in the year 2001; 11% less of the treated firms belong to the industry with SIC=2, but 12% more of the treated firms are in the industry with SIC=3; the average O-scores of the treated firms are significantly higher and 7% more of the treated are in the top quintile of the O-score distribution. Interestingly, the average past three-year (i.e. from $t=-3$ to $t=-5$) cumulative returns of the treated firms (i.e. the firms that will be financially distressed in three years) exceed that of the potential controls by 24% and the gap is significantly different from zero.

The fourth panel of Table 2 shows the mean and standard error of the *within-match* differences in the covariates between the treated firms and the *actually used* controls when we apply the matching estimator. Analyzing the within-match difference allows us to evaluate the quality of the matching and shows how the matching improves balancing of the covariates between the treated and the controls. Note that, because we match each treated firm with four controls ($M = 4$), the number of observations represents the number of within-match differences used in the calculations and, of course, equals the number of treated firms multiplied by four.

Clearly, once the matching procedure selects the best controls for the treated firms, the two groups are much more similar to each other. The matching succeeds on almost all the dummy variables. In particular, the year and industry classifications are now balanced. The balancing of covariates is very good for the continuous variables too, although not perfect. The difference in the O-scores between the two groups is cut in half, decreasing from 6% to 3%, but remains statistically significant after matching. The cumulative stock return shows the largest improvement, with the

difference between the treated and the actual controls dropping sharply from a significant 24% gap to a minimal and insignificant 4% gap. One exception is market capitalization in that the difference in firm size between the treated firms and the controls becomes significant after the matching. The difference in firm size does decrease from 0.25 to 0.24 with the matching, but the significant reduction in the standard error of the difference, shrinking from 0.15 before the matching to 0.08 after the matching explains the increase in statistical significance. This suggests that firms that go into financial distress are on average smaller than firms that do not, and we have a hard time finding firms of equivalent size as controls. However, for our additional size measure, the large-market-capitalization dummy, the difference between the two groups is reduced and is only marginally significant. This evidence shows that the comparisons are mostly not being made between small capitalization and large capitalization companies. Note also that the managerial influence measures are largely the same across the treated and the potential controls before and after matching, although the proportion of executives sitting on the compensation committee increases from 4% before matching to 7% after matching.

To sum up, the overall quality of the matching appears to be quite good and the matching works well in balancing differences in observed characteristics between the financially distressed firms and the comparable non-distressed firms. Note that we even match on the level and composition of CEO compensation as well as the managerial influence over the board at $t=-3$, which implies that the treated and the controls are similar not only in their characteristics and financial situations but also in their compensation policies and managerial power. Finally, we expect that any potential bias that may remain due to non-exact matching in the covariates will be eliminated by the bias adjustment step of our matching estimator.

3.3 Assessment of Identifying Assumptions and Timing of Financial Distress

For each outcome variable we estimate the Average Treatment on the Treated (ATT) effect over the period from two years before the firm goes into financial distress ($t=-2$) up to three years after ($t=+3$). There are two reasons to include the pre-treatment years in the window of analysis. On one hand, the event year ($t=0$), in which the firm goes into financial distress, is determined by us based on the rules explained in Section 2, so one may question the precision of the timing.

By examining whether there is any treatment effect before $t=0$, we also implicitly check whether the timing of our treatment variable is correct or not. Second, and more importantly, estimating the ATT on pre-treatment outcomes provides an indirect test on the feasibility of the Conditional Independence Assumption (CIA). The CIA is not testable, but as first noted by Heckman and Hotz (1989), one can estimate the treatment effect over a period during which there should be no treatment effect and test whether that estimated effect is zero. If the hypothesis of zero treatment effect is rejected, then it is much harder to argue that the CIA holds. Therefore, for the sake of our study, we should expect the ATT on the pre-treatment outcomes to be zero.

Another concern regarding the CIA is that there might exist unobserved heterogeneity that differs across the treated and the actual controls even after matching. The matching method deals with differences in observed covariates, and it is also possible to control for certain types of unobserved heterogeneity. If the unobserved heterogeneity generates systematic differences in the outcome variables and the heterogeneity is *fixed over time* (say, the firm's location, the type of products the firm sells, the characteristics of the industry the firm belongs to, etc.), then we subtract from each outcome variable the value of the same variable in a pre-treatment base year so that this invariant unobserved heterogeneity is removed. This *difference-in-differences (DID)* type of estimator may be more robust than the levels estimator because the *DID* method removes any time-invariant heterogeneity not taken care of by the matching on observable characteristics (Smith and Todd, 2005). Moreover, the *DID* estimator plays a role in eliminating any biases that could be generated by differences in (mostly) continuous matching covariates during the matching period. As we show in the next two sections, in most cases we do not find important qualitative differences between the results using the outcome variables in levels or in differences.

4 Effects of Distress on Managerial Influence Over the Board

It is widely believed that the occurrence of financial distress weakens managerial power due to increased scrutiny from stakeholders such as creditors and shareholders as well as from the bankruptcy court judge if Chapter 11 protection is sought. There also exists an opposite view that managerial influence among the financially distressed firms may strengthen because firm stakeholders rely more on their managers' efforts to go through the process. Consistent with

the latter view, it is a quite popular practice for distressed firms to grant generous retention plans to certain executives and key employees during the process. We assess the effect of financial distress on managerial influence over the firms' board of directors in this section.

Using the information available in the ExecuComp database we construct five measures for corporate governance. They are number of executives serving as board's directors (*EXECDIR_N*), number of executives listed in the compensation committee interlocks section of the firm's proxy (*INTERLOCK_N*), a CEO-chair dummy that equals one if the CEO also serves as the firm's board chairperson and zero otherwise (*CEOCHAIR*), an executive-director dummy that equals one if at least one executive serves as a board director and zero otherwise (*EXECDIR_D*), and an interlock dummy that equals one if at least one executive serves in the compensation committee and zero otherwise (*INTERLOCK_D*). These measures are all related to whether insiders are on the board and they jointly characterize potential influence of management over the board of directors and over the board's compensation committee. Higher values of these measures signal that managers are more powerful and corporate governance is poorer. The compensation committee measures, in particular, are clear indicators of potential influence of the executives in setting their own pay.

Table 3 presents the descriptive statistics of the five measures for the treated and the potential controls. Notably, for each of the five variables, there are no significant differences between the treated and the potential controls in the pre-treatment years or in the year of treatment (i.e., $t = 0$ or prior). However, in the post-treatment years (i.e., $t > 0$), the treated firms have lower values than the potential controls in each of the five measures, suggesting that the power of managers of the treated firms diminishes after the firms fall into financial distress. For example, in the pre-treatment years, the proportions of CEOs that serve as board chairs for the treated group and the potential controls are both about 0.6; in the post-treatment years, this proportion remains about the same for the potential controls, but declines to about 0.4 for the treated, a drop of about 30% with respect to the non-distressed firms. Similar patterns apply to executives serving as board directors. The proportion of treated firms in which at least one executive serves in the board of directors decreases from 0.96 before financial distress to 0.88 after the onset of financial distress, while the same proportion remains at 0.95 for the potential controls throughout the analysis window.

However, for the proportion of firms in which at least one executive sits in the board's compensation committee, the pattern is decreasing for both treated and potential control firms (from around 0.15 to much lower values for both groups).

A careful inspection of the statistics in Table 3 highlights the advantage of using a control group, versus an analysis that only looks at the measures before and after financial distress, for the distressed firms. A before-and-after analysis, analyzing the descriptive statistics for the treated firms only, does not take into account the counterfactual (i.e., what the corporate governance measures would have been under no distress). For example, from Table 3 we observe an overall trend that the proportion of firms with executives in the compensation committee declines for all firms, independently of financial distress. A before-and-after analysis would attribute the fall in this measure to financial distress, thereby overestimating the effect of financial distress, because it would not take into account the overall changes in the non-distressed firms. The information on potential controls as presented in Table 3, however, has one drawback — these firms may not be comparable to those affected by financial distress. This is why our empirical strategy relies on a matching estimator to identify, among the potential controls, which firms are a proper comparison for the financially distressed firms. Below we discuss the results of applying such matching estimator.

Table 4 reports the matching estimate results both in levels (Panel A) and in differences (Panel B, setting $t=-3$ as the base period), with robust standard errors reported in parentheses. Overall, the matching estimates for the treatment effects of financial distress on managerial influence over the board confirm the patterns observed from the descriptive statistics.

We first look at the results for the level estimates (Panel A). It is clear that in the pre-treatment years the treated and the controls do not differ significantly in term of their managerial influence, and the treatment effects on the five measures are close to zero. However, in the post-treatment years all the five measures show significant reductions and almost all the treatment effects are significantly negative. For example, in the year immediately after the distress, i.e, in $t=+1$, the number of executives serving as board directors shrinks by 0.32, the number of executives sitting in the board's compensation committee goes down by 0.13, the proportion of CEOs holding the board's chairmanship decreases by 15 percentage points, and the proportions of executives sitting in the board or in the board's compensation committee decrease by 7 and 8 percentage points,

respectively. In other words, after entering into financial distress fewer distressed firms allow their managers to serve in the board or in the compensation committee, let alone as a board chairperson, than their solvent counterparts.

The *DID* estimates in Panel B show similar patterns, although in general the treatment effects are more imprecisely estimated. Even accounting for any differences in the managerial influence measures between the treated and the controls in $t=-3$, we still find that the treatment effects are negative and mostly significant for the treated firms in the post-treatment years. Noticeably, in neither Panel A nor Panel B, we find any statistically significant differences in the five managerial influence measures in the periods prior to financial distress. This evidence is very reassuring because it bodes well for the validity of the Conditional Independence Assumption. We return to this issue below.

In summary, both the descriptive statistics and the matching estimators show clear evidence of diminishing managerial influence over the board among the financially distressed firms. Less executives serve in the board or in the compensation committee, and fewer CEOs hold the board chairmanship after those firms fall into financial distress. The evidence supports the conventional wisdom that financial distress causes firms and the boards of directors to reign in the CEO's and other executives' power. Given the reduced managerial power due to the incidence of financial distress, it is natural to study whether the compensation and the status of CEOs change. Below we investigate the impact of financial distress on CEO compensation, CEO turnover, and one particular form of CEO compensation practice — the opportunistic timing of stock option grants.

5 Effects of Distress on CEO Compensation, CEO Turnover, and Opportunistic Timing of Option Grants

CEO compensation consists of several components. Total current compensation (*TCC*) is the sum of salary and bonus. Total direct compensation (*TDC1*) is the sum of total current compensation, the value of restricted stock grants (*RSTGRNT*), the Black-Scholes value of stock option grants (*BLKV*), and others, where the “others” item includes other annual short-term compensation, payouts from long-term incentive plans, and all other long-term compensation. Besides *TDC1*, we

use $TDC2$ as another measure of the total flow compensation to a CEO within one fiscal year, which is the sum of TCC , $RSTGRNT$, value realized from option exercises ($EXER$), and others. For equity-based compensation, we calculate two measures of CEO ownership: the stock ownership excluding option grants ($SHOWN$), which is the total number of stock shares (options excluded) held by a CEO scaled by the total number of firm shares outstanding, and the stock ownership represented by option grants ($OPGRNT$), which is the total number of stock shares represented by options granted to a CEO divided by the total number of firm shares outstanding.

Table 5 summarizes these CEO compensation components for the treatment and potential controls groups. A few interesting patterns stand out. First, the level of the total flow compensation $TDC1$ is mainly determined by the Black-Scholes value of stock option grants $BLKV$, while the total current compensation TCC and the value of restricted stock grants $RSTGRNT$ account for a much smaller share of this total flow compensation. Second, both the Black-Scholes value of stock option grants ($BLKV$) and the value realized from options exercised ($EXER$) vary widely over time; there are much smaller fluctuations in bonus, the value of restricted stock grants, and stock ownership through option grants; and the salary portion of compensation is quite stable, consistent with the notion that it is more or less a fixed pay. As a result, we can infer that the time variations in $TDC1$ and, to some extent, $TDC2$ are mainly caused by the time variations in the two stock-option-related compensation components, $BLKV$ and $EXER$, respectively.

We observe other interesting patterns by comparing the descriptive statistics of the treated group with those of the potential controls. While $BLKV$ of the treated group shows a generally decreasing trend from the pre-treatment years to the post-treatment years, the potential controls exhibit an opposite trend of increase over time; a similar pattern is followed by the compensation measures $TDC1$, $TDC2$, and $RSTGRNT$. In addition, note that except for salary and stock ownership through option grants, each of the compensation components for the treated group is significantly smaller than the corresponding compensation components for the potential controls in almost all of the post-treatment years.

As discussed in Section 4, analyzing the descriptive statistics for only the treated firms is akin to a before-and-after analysis of the effects of financial distress, which is the backbone of the analysis in Gilson and Vetsuypens (1993). This type of analysis not taking into account the

counterfactual can lead to an overestimation or underestimation of the effects of financial distress on CEO compensation. Take *TDC1* for example. Compensation measured by *TDC1* is greatly reduced for distressed firms from before to after distress. However, the descriptive statistics for the potential controls suggest that there is an increasing market trend in *TDC1*. Therefore, the before-and-after analysis not controlling for the market trend here would lead to *underestimating* the effects of financial distress on *TDC1*. Of course, as discussed above, we need to be careful in making inference based on the potential control firms, which may not be comparable to the treated firms. In the remainder of this section we concentrate our analysis on the results obtained from using the matching estimator to select the appropriate control firms.

5.1 Effects of Distress on CEO Compensation for All Treated Firms

Table 6 presents the treatment effects on CEO compensation for all the treated firms using the outcomes in levels (Panel A) and in differences (Panel B, setting $t=-3$ as the base period). Robust standard errors are reported in parentheses. The treatment effects identified using the outcomes in levels or differences do not differ much qualitatively. So, for brevity, we will concentrate the discussion on the results in Panel B.

The first thing to notice in Table 6 is that the treatment effects for the CEO compensation components in the $t=-2$ period are virtually zero with the only exceptions being *EXER* and *TDC2* (which is driven by the difference in *EXER*). It shows that the compensation policies of the treated and control groups are essentially the same up to two years before financial distress, although CEOs of soon-to-be distressed firms exercise more of their options owned. In the year before distress (i.e., $t=-1$), we see that some components of CEO compensation, namely, *Bonus*, *TCC*, and *RSTGRNT*, become significantly different between the treated and control groups, which seems to indicate that the effects of financial distress appear earlier than the advent of the distress. We expect these types of results, in particular for $t=-1$, given that our criteria for determining the first period of financial distress is somewhat arbitrary. Overall, the evidence that there are no significant differences between treated and controls in the pre-treatment years for the majority of the CEO compensation components, suggests that we have done reasonably well in defining the treatment status and the timing of financial distress. Together with similar patterns

we observed in Table 4 for the managerial influence variables, our results bode well for the validity of the Conditional Independence Assumption.¹¹

Table 6 shows that the total cash compensation, TCC , of the distressed firms' CEOs decreases after $t=0$ in the order of \$400 thousands per year. This is a large effect in percentage terms with respect to the total cash compensation in either the pre-treatment years or the treatment year. Notably, the change in TCC is explained almost entirely by an increase in the gap in bonus between the treated and control firms after the treatment year. Moreover, Table 6 clearly illustrates, relative to the cash compensation, a much larger negative effect on stock-based compensation, particularly the Black-Scholes value of option grants, $BLKV$. The value of $BLKV$ drops by \$2.4 million immediately in the distress year ($t=0$), and continues to decline by \$4.9 million in the first year after distress ($t=+1$), followed by a reduction of \$2.6 million in the second year after distress ($t=+2$) and by a smaller, and statistically insignificant, decrease of \$0.5 million in the third year after financial distress ($t=+3$). The value of stock grants, $RSTGRNT$, declines slightly in the treatment year and the post-treatment period, but the magnitude is much smaller and the treatment effect on $RSTGRNT$ becomes trivial compared to the effect on $BLKV$. As a result, the effect on the total flow compensation to CEOs, $TDC1$, is overwhelmingly driven by the effect on $BLKV$. The value of $TDC1$ drops by \$3.2 million immediately in the year of financial distress ($t=0$), and continues to decrease by \$5.6 million in the first year after distress ($t=+1$), followed by a decline of \$3.2 million in the second year after distress ($t=+2$) and a statistically insignificant decline of \$1.2 million in the third year after financial distress ($t=+3$).

Table 6 also shows that the value realized from exercising options, $EXER$, decreases by about \$2 million per year after financial distress. The treatment effect on $EXER$ is not surprising as the stock prices of the distressed firms decline significantly after falling into financial distress, thereby reducing the value of previously-granted stock options. Consequently, the treatment effect on the alternative measure of total flow compensation to CEOs, $TDC2$, which is driven by the negative treatment effect on $EXER$, shows a similar pattern as $EXER$. Table 6 further evidences the impact of financial distress on the practices of option grants in that the stock ownership through

¹¹As mentioned before, the CIA is not testable. Nevertheless, if we can show that the outcome variables in the pre-treatment period are not statistically different across the treated and control groups, it is easier to claim that the assumption holds.

stock option grants increases in the post-treatment years. That is, the companies under financial distress appear to grant more options as CEO compensation in lieu of cash compensation, probably because these firms lack cash/liquidity due to the distress.

In summary, the results show that financial distress significantly impacts the level and structure of CEO compensation and that the most significant effect is on the Black-Scholes value of new grants of stock options, *BLKV*. As a consequence of a decrease in *BLKV* for the treated firms and an increase in *BLKV* for the control firms, the effect of financial distress on stock-based compensation is large.

5.2 Effects of Financial Distress on CEO turnover

One way a firm deals with a CEO who has not performed as desired by the board (and the shareholders) is to replace the CEO with a new one. Table 7 reports the average CEO turnover rates in various subperiods for the treated and potential controls firms. It shows that the average turnover rate is significantly higher in the treated group than in the potential controls. For example, in the $[-1, +1]$ subperiod the average CEO turnover rate is 52% for the treated group and 33% for the potential controls. The average CEO turnover rates for the treated group increase to 63% and 69% for the two subperiods $[-1, +2]$ and $[-1, +3]$, respectively. In contrast, the corresponding CEO turnover rates for the potential controls are 41% and 48%, respectively. Clearly, for financially distressed firms, most of the CEO turnover occurs in the year of distress (time 0) and in the following year (time +1).

Table 8 reports the treatment effects of financial distress on CEO turnover using the bias-corrected matching estimator (matching on the same covariates as in Table 2). In panel A, we group the years before and after the time of financial distress ($t=0$) in alternative windows. We find that turnover rates are significantly higher for treated firms than for control firms. For example, in the $[-1, +1]$ window, the CEO turnover rate is 19 percentage points higher for treated firms, which is a very large effect taking into account that the CEO turnover rate for the control firms is 33%; if we extend the window by including $t=+2$ or $t=+3$, the CEO turnover rate further rises by 4 and 9 percentage points, respectively.

We also estimate the treatment effects on the CEO turnover rate on a year-by-year basis and

report the results in Panel B. Like our results on compensation, there is no differential effect on CEO turnover in $t=-1$, or $t=-2$, or $t=0$. The single most important effect on CEO turnover occurs in the year immediately after the treatment year (i.e., $t=+1$), when we observe a significant treatment effect of 21 percentage points. That is, the CEO turnover rate of the distressed firms is 21 percentage points higher than the turnover rate of the non-distressed firms after the firms fall into distress. The treatment effect on CEO turnover is also present in $t=+2$ (6 percentage points) and $t=+3$ (8 percentage points), although in a comparatively smaller magnitude than in $t=+1$.

The result on the increased CEO turnover rate mirrors our above evidence that managerial influences declines in financially distressed firms. The more likely a CEO is to lose his job, the less influence he is to exert over the board. Also, to the extent that the estimator matches and thus implicitly controls for the covariates characterizing industry and market performance, our results can be interpreted as an estimator of relative performance. Two recent studies by Jenter and Kanaan (2010) and Kaplan and Minton (2006) both show that CEO turnover rates have increased substantially in periods roughly similar to the one we analyze. In particular, Jenter and Kanaan (2010) explicitly study the effects of relative performance on the probability of CEO turnover and find that, contrary to the findings of studies for earlier periods, CEOs are significantly more likely to be dismissed from their jobs after bad industry and market performance. Like in the two studies, we find that firm performance has a very significant impact on CEO turnover in our sample.

5.3 Effects of Financial Distress on Compensation by CEO Turnover Status

Given the significant treatment effect on CEO turnover, a reasonable concern is that the effects of financial distress on CEO compensation we document are driven or confounded by the effects on CEO turnover. To separate the two treatment effects, we estimate the treatment effects of financial distress on CEO compensation for two subsamples. The first subsample is comprised of all firms that experience CEO turnover, and the second is comprised of firms that do not experience CEO turnover.¹²

¹²We also conduct this analysis on the five corporate governance measures discussed in Section 4. Even though the results are somewhat imprecisely estimated (due to small sample size), we do not find any qualitative difference between the two subsamples as compared to the results for the full sample. We do not report these results for space considerations, but they are available upon request.

A caveat is in order. Given that the year $t=+1$ is the first and the single most important period with a significant difference in CEO turnover rates between the treated and control firms, we consider in the turnover subsample those firms that replace their CEOs only during the period $[-1, +1]$. This restriction guarantees that those firms do *not* replace their CEOs in the periods $t=+2$ and $t=+3$, which yields a clear interpretation of the treatment effects of distress in those two periods. It also allows for the comparison, in these two periods, with the results for the no-CEO-turnover subsample. For the no-CEO-turnover subsample we select all firms that retain the same (incumbent) CEOs over the entire $[-3, +3]$ period.¹³

Table 9 presents the results for each of the two subsamples using the *DID* specification of the bias-corrected matching estimator. Panel A shows the treatment effects on CEO compensation for the no-turnover firms, which are very similar to the ones estimated for all the firms in Table 6. Not surprisingly, the salary, bonus, and total cash compensation of incumbent CEOs are significantly lower in and after the distress year; the most significant and dominant effect is on stock-based compensation, particularly the Black-Scholes value of option grants. Like in the full sample, the treatment effect on the total flow compensation to incumbent CEOs is overwhelmingly driven by the treatment effect on the Black-Scholes value of option grants *BLKV*. On the other hand, firms under financial distress tend to grant their CEOs more stock options in the post-treatment years, as evidenced by the small but statistically significant treatment effects on *OPGRNT*.

Table 9, Panel B reports the treatment effects on CEO compensation for the firms that experience CEO turnover in the period from -1 to $+1$. Financial distress appears to have a smaller and oftentimes insignificant effect on the new CEO's salary, bonus and total cash compensation. This suggests that the distressed firms award their new CEOs *cash compensation* similar to that received by the new CEOs of non-distressed firms. Interestingly, the new CEOs of distressed firms do not seem to be paid market-value (i.e. the same as the new CEOs of non-distressed firms), in terms of *stock-based compensation*. For the new CEOs who replace the CEOs of distressed firms, the treatment effects of distress on *BLKV*, and thus on *TDC1*, are significantly negative.

The results for the turnover and the non-turnover subsamples imply that CEOs of distressed firms, regardless of whether they are incumbents or successors, suffer large reductions in their

¹³For robustness, we try alternative ways of forming turnover versus no-turnover subsamples using different windows. The results are similar and are available upon request.

compensation after the firms fall into financial distress and that this reduction derives mainly from the drop in the value of their stock option grants. This is similar to the patterns we observe in our analysis of the full sample. Consequently, we conclude that the treatment effect on the stock-based compensation is not dominated by the treatment effect on CEO turnover.

It is interesting to document that the new CEOs of distressed firms that experience CEO turnover are paid significantly less, in terms of stock compensation, compared both to their predecessors and to the CEOs of control firms. Noticeably, we also observe that the new CEO's power as measured by their influence over the board reduces relative to their predecessors and to the CEOs of control firms. One might argue that paying less to CEOs of poorly-performing firms is consistent with the use of benchmarking, or relative performance evaluation, by boards of directors when setting CEO compensation. However, it is hard, *a priori*, to argue that *new* CEOs should be penalized for poor performance resulting from their predecessors' tenure. Moreover, when we analyze one potential "benchmark", i.e., the performance of the firm's stock prices after a new CEO takes over, we find that in years $t=+2$ and $t=+3$ distressed firms outperform control firms (conditioning only on firms that replaced their CEOs in the $[-1, +1]$ period). Specifically, in results we do not present in tables for brevity, and using period $t=+1$ as a reference point, we find that stock prices for the treated firms increase by 32% and 38% more than those for the control firms in periods $t=+2$ and $t=+3$, respectively. Thus, at least in terms of stock price changes, new CEOs of the treated firms seem to be performing well relative to the benchmark, which makes it even harder to argue that financially distressed firms use benchmarking in setting CEO compensation.¹⁴

5.4 Effects of Distress on Opportunistic Timing of Option Grants

In this subsection we proceed to study the impact of financial distress on the opportunistic timing of option grants. Collins, Gong, and Li (2009) find that weak corporate governance mechanism is a contributing factor to the occurrence of option backdating. Our analysis in Section 4 shows

¹⁴An alternative explanation for the lower equity-based compensation for new CEOs of distressed firms, is that they are just of "worse quality" than the new CEOs of non-distressed firms. It is not easy to find good measures of quality, and we analyze two characteristics that we believe could be good proxies. First, we look at the percentage of new CEOs that have prior experience as CEOs (i.e., having appeared as CEO of another firm in the ExecuComp dataset) or at least some prior executive experience (i.e., having appeared as one of the top five executives in another firm in the ExecuComp dataset). Second, we study internal promotions, i.e. new CEOs that have appeared as executives in the same firm before becoming CEOs. With either measure of quality, we do not observe any significant difference between the treated and the potential control firms. These additional results are available upon request.

that, following financial distress, managerial influence over the board decreases. To the extent that managerial influence proxies for corporate governance, we expect that option backdating practices should reduce after a firm falls into financial distress.

We follow Bebchuk, Grinstein and Peyer (2010) and measure this opportunistic timing by identifying “lucky” grants, that is, the options that are given at or below the lowest stock price of the grant month. We set the grant price, if unavailable, to the closing price of the underlying stock on the same grant day because the common practice of option grants is to issue them at-the-money. We construct two such measures based on unscheduled option grants.¹⁵ We calculate the *raw* measure as the average proportion, weighted by the size of each option grant, of unscheduled grants that are lucky grants in a fiscal year. The *net* measure subtracts from the raw measure the average proportion of trading days in which the stock price is at the lowest stock price of the grant month. The net measure takes into account the fact that the prices of a financially distressed stock might be stale, meaning that there are few trades of such distressed stocks in the market. Finally, Heron and Lie (2006) report that the abnormal stock return pattern consistent with option backdating behavior has been much weaker after the adoption of the Sarbanes-Oxley Act (SOX). The regulation, which requires option grants to be reported within two business days of granting, took effect on August 29, 2002. To isolate the treatment effect of financial distress from the effect of the SOX regulation, we focus on the pre-SOX period.¹⁶

Table 10 reports the descriptive statistics of the two measures of lucky grants for the treated and the potential control firms, both before and after SOX. For all the firms entering into distress in 1999 and after, period $t=+3$ occurs after SOX. In the same way, for firms experiencing distress in 2000 and after, period $t=+2$ also takes place after SOX, and so on. This explains why, in the before-SOX portion of the table, the number of observations (both for treated and potential controls) decreases monotonically starting from period $t=0$. It also explains why the number of observations increases over time in the after-SOX portion of the table. Note that we only show periods $t=+2$ and $t=+3$

¹⁵Focusing on unscheduled grants is the standard approach to studying managerial opportunistic timing behavior, but this restriction might make the evidence presented in this section not perfectly compatible with the evidence documented in Section 5.1. The Black-Scholes value of option grants reported in the ExecuComp database includes both scheduled and unscheduled grants. Unfortunately, the data does not contain sufficient information to calculate the Black-Scholes value for unscheduled grants only.

¹⁶We find similar pattern of lucky grants if we do not exclude the post-SOX period from our sample. The results are available upon request.

after SOX, because sample sizes are just too small prior to $t=+2$. Unfortunately, these issues have consequences on the power of all the analyses we conduct in this subsection.

We first analyze the before-SOX statistics based on the raw proportion of lucky grants as reported in the left half of Table 10. Several patterns are worth mentioning. First, the proportions in $t=-3$ and $t=-2$ are slightly higher for the distressed firms than for the control firms. Second, the proportions of lucky grants for the controls are quite stable during the period from $t=-1$ to $t=+3$, fluctuating around 0.12. This evidence is consistent with Bebchuk, Grinstein and Peyer's (2010) estimate of the percentage of lucky grants from 1996 to 2005. Third, the proportion of lucky grants for the financially distressed firms drops noticeably from over 0.15 before $t=-1$ to below 0.10 and even to 0.05 afterwards.¹⁷ Taken together, these patterns show clearly that the proportions of lucky grants are higher for the treated than for the controls prior to financial distress and become lower from $t=-1$ onwards. If we take into account the effective trading of stocks and analyze the net measure of the proportion of lucky grants, the above patterns become stronger. As shown in the right half of Table 10, the proportion of lucky grants for the distressed firms exceeds the proportion for the controls by at least two percentage points in either $t=-3$ or $t=-2$, but the proportion for the treated is at least two percentage points lower than the proportion for the controls starting from $t=-1$ and up to $t=+3$.

Table 10 also reports the descriptive statistics of the two measures of opportunistic timing of option grants in the post-SOX period, for $t=+2$ and $t=+3$. Interestingly, the raw proportions of lucky grants among the controls in $t=+2$ and $t=+3$ drop significantly to 0.05 and 0.09, respectively. Similarly, the net proportions decline to 0.01 and 0.05. In contrast, the raw proportions for the controls in the pre-SOX period are 0.11 and 0.12 (0.07 and 0.08 for the net proportion) at $t=+2$ and $t=+3$, respectively. This result suggests that the SOX regulations have curtailed opportunistic timing of option grants for all firms, and is consistent with the findings by Heron and Lie (2006).

Table 11 reports the results from applying the matching estimators to the measures of lucky grants in the pre-SOX period, both in levels (Panel A) and in differences (Panel B). We first look at the results for the level estimates. Although many of the estimates are not statistically different from

¹⁷Note that, as explained in footnote 15, the calculation of the Black-Scholes value of option grants includes both scheduled and unscheduled grants, while the analysis of lucky grants is based only on unscheduled grants. This may explain why the proportion of lucky grants starts decreasing in $t=-1$ and the Black-Scholes value of option grants starts decreasing in $t=0$.

zero, the level results exhibit a pattern similar to the one obtained from the descriptive statistics. Using either the raw measure or the net measure, the estimated treatment effect on the proportion of lucky grants is positive in $t=-2$, suggesting that before distress, treated firms tend to award their CEOs more lucky grants than the control firms. However, the treatment effects become negative starting at $t=-1$ and remain negative through $t=+3$, indicating that the treated firms significantly cut back the lucky grants to their CEOs. Panel B presents the results for the *DID* estimates, with the difference taken relative to the proportion of lucky grants in $t=-3$. Compared to the level estimates, the *DID* estimates display a largely similar pattern, although with lower statistical significance. As discussed above, the overall lack of precision in this part of our analysis is driven by the relatively smaller sample sizes we work with when analyzing the pre-SOX period.

In summary, both the descriptive statistics and the matching estimators show some evidence that there is a change in the behavior of awarding lucky grants to CEOs of firms under financial distress before and after falling into distress. The soon-to-be insolvent firms tend to award their CEOs more lucky grants than comparable solvent firms two years prior to insolvency, but those firms significantly scale down the lucky grants to their CEOs after they become financially distressed while the comparable solvent firms maintain about the same level of lucky option grants.

6 Concluding Remarks

In this paper we study the dynamics of the changes in managerial power and CEO compensation when firms go through financial distress. We use a bias-corrected matching estimator to find appropriate comparable firms to those in financial distress and to estimate the effects of financial distress on managerial influence over the board, CEO compensation, and opportunistic timing of option grants. We find that financial distress has important consequences on corporate governance, decreasing managerial influence over the board in particular. Among distressed firms, there is a significant decrease in the proportion of CEOs holding the board chairmanship, and in the frequency of executives serving as directors or in the compensation committee of the board. The CEOs of distressed firms experience significant reductions in total compensation, the bulk of which derives from the decline in value of new grants of stock options. These results hold not only for incumbent CEOs but also for replacement CEOs. We also show that periods of financial distress are associated

with a decrease in opportunistic timing behavior of stock option awards.

Our findings are subject to caveats related to the nature of the data we rely on, but they represent three separate pieces of evidence which point in the same direction. Taken together, the results are suggestive of a link between the changes in managerial influence and the changes in CEO compensation (including the changes in opportunistic option timing). The occurrence of financial distress weakens managerial power, likely due to greater scrutiny from other stake-holders like banks or courts. The reduction in managerial influence occurs concurrently when executive compensation in financially distressed firms shrinks. The simultaneous declines in CEOs' stock-based pay (both for incumbents and replacement CEOs) and in opportunistic timing of option grants lend support to an interpretation that managerial power affects CEO pay.

We interpret our results as consistent with the view that executive pay reflects a certain degree of rent extraction due to managerial power. However, we do not regard them as evidence that the optimal contracting view is necessarily irrelevant to the pay setting process. It could be the case that financial distress triggers a restructuring of executive compensation due to both the optimal contracting mechanism and the managerial power reduction. Empirically it is very difficult to completely disentangle the managerial-power effect from the incentive-pay effect, and we believe that the pay-setting process includes elements associated with both optimal contracting and rent extraction. Nevertheless, our results imply that incorporating managerial rent-extraction motives more explicitly into theoretical models might improve our understanding of the determinants of executive compensation.

Appendix: Matching Estimator

Following the usual notation in the program evaluation literature, let $Y_i(0)$ and $Y_i(1)$ denote the *potential outcomes* of unit i under control and treatment status respectively, for $i = 1, \dots, N$. For each unit i we observe the treatment received T_i for $T_i \in \{0, 1\}$ and the outcome for each treatment, $Y_i = Y_i(0)$ if $T_i = 0$ and $Y_i = Y_i(1)$ if $T_i = 1$, as well as a vector of pre-treatment variables or covariates X_i . There are N_0 control units and N_1 treated units, $N = N_0 + N_1$. We are interested in estimating the *Average Treatment Effect for the Treated (ATT)*

$$\tau^t = E[Y_i(1) - Y_i(0) | T_i = 1].$$

The main identifying assumption necessary to estimate the above ATT is known as *unconfoundedness* or the *Conditional Independence Assumption (CIA)*. It assumes that $(Y_i(1), Y_i(0)) \perp T_i | X_i$. This implies that after controlling for observable characteristics the potential outcomes are independent of the treatment status. Actually, following Abadie and Imbens (2006, 2011) only a weaker version of the CIA is needed for estimating the ATT by matching; we only need $Y_i(0) \perp T_i | X_i$, that is, T_i is independent of $Y_i(0)$ conditional on X_i .¹⁸ The intuition is that, after we control for all potential confounders X_i we can assume that the treatment is as good as a randomized treatment.

Abadie and Imbens (2006, 2011) consider the case of matching with replacement, allowing each unit to be used as a match more than once. This procedure has the advantage of improving the average match quality with respect to the case of matching without replacement, but implies that special attention must be paid to the number of times a unit is used as a match. Following Abadie and Imbens' (2006) notation, let $j_m(i)$ be the index of the m -th match to unit i (i.e. $j_m(i)$ is the m -th closest unit to unit i in terms of the covariate values, measured by the Euclidean distance between the two vectors). Let $\mathcal{J}_M(i) = \{j_1(i), \dots, j_M(i)\}$ denote the set of indices for the first M matches for unit i , and let $K_M(i)$ denote the number of times unit i is used as a match if M matches are done per unit, $K_M(i) = \sum_{l=1}^N 1\{i \in \mathcal{J}_M(l)\}$, where $1\{\cdot\}$ is the indicator function. Now,

¹⁸In addition to the CIA, we need to satisfy an overlap condition and other regularity conditions. See Abadie and Imbens (2006, 2011) for details.

for $i = 1, \dots, N$ define the imputed potential outcome under the control status as

$$\hat{Y}_i(0) = \begin{cases} Y_i & \text{if } T_i = 0 \\ \frac{1}{M} \sum_{j \in \mathcal{J}_M(i)} Y_j & \text{if } T_i = 1. \end{cases}$$

Then, Abadie and Imbens (2006) write the matching estimator for the ATT that uses M matches per unit with replacement as

$$\begin{aligned} \hat{\tau}_M^{m,t} &= \frac{1}{N_1} \sum_{j \in \mathcal{J}_M(i)} (Y_i - \hat{Y}_i(0)) \\ &= \frac{1}{N_1} \sum_{T_i=1} \left(T_i - (1 - T_i) \frac{K_M(i)}{M} \right) Y_i. \end{aligned}$$

This is called the *simple matching estimator*. Abadie and Imbens (2006) show that this estimator is not $N^{1/2}$ -consistent in general, because it includes a conditional bias term that may be of order larger than $N^{-1/2}$, unless the matching variables include at most one continuous variable. An attractive property of this estimator is that the estimator for the asymptotic variance proposed by Abadie and Imbens does not rely on bootstrapping (contrary to other matching methods).

Abadie and Imbens (2011) propose a *bias-corrected* matching estimator where the difference within the matches is regression-adjusted for the difference in covariate values:

$$\tilde{Y}_i(0) = \begin{cases} Y_i & \text{if } T_i = 0 \\ \frac{1}{M} \sum_{j \in \mathcal{J}_M(i)} (Y_j + \hat{\mu}_0(X_i) - \hat{\mu}_0(X_j)) & \text{if } T_i = 1. \end{cases}$$

where $\hat{\mu}_0$ is a consistent estimator of $\mu_0 = E[Y(t)|X = x]$. The bias-corrected matching estimator that uses M matches per unit with replacement is then

$$\hat{\tau}_M^{bcm,t} = \frac{1}{N_1} \sum_{j \in \mathcal{J}_M(i)} (Y_i - \tilde{Y}_i(0)).$$

Contrary to the simple matching estimator, Abadie and Imbens (2011) show that this bias-corrected matching estimator is $N^{1/2}$ -consistent and asymptotically normal. In this paper we use the bias-corrected matching estimator, in light of the desirable properties described by Abadie and Imbens (2011). We carry out our estimation using the Stata command `nnmatch` which is discussed in details in Abadie et al. (2004).

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Table 1. Number of Observations for the Treated and Potential Controls across Years

This table reports the number of observations for the treated (i.e., financially distressed firms) and potential controls (i.e., non-distressed firms) for each fiscal year. We require a firm included in the analysis sample to have no missing information in the matching covariates in period -3 , and at most one missing variable among all compensation variables in periods -2 to 0 and at most one missing variable in all compensation variables in periods $+1$ to $+3$. The benchmark period 0 refers to the year when a firm falls into financial distress. The matching covariates are summarized in Table 2.

Fiscal Year	Analysis sample	
	Treated	Potential Controls
1995	11	173
1996	7	92
1997	8	71
1998	15	84
1999	15	200
2000	11	72
2001	15	371
2002	17	142
Total	99	1,205

Table 2. Balancing of Matching Covariates

This table shows balancing of the matching covariates in period -3 , where the benchmark period 0 refers to the year when a firm falls into financial distress. Column 1 lists the matching covariates including year and industry dummies, firm size measured by the log value of market capitalization, size dummy that equals one if size is in the top 30 percentile and zero otherwise, leverage measured by debt-to-asset ratio, Olson's (1980) O-score, past three-year cumulative stock returns, O-score dummy that equals one if O-score is in the top 20 percentile and zero otherwise, return dummy that equals one if cumulative return is in the bottom 20 percentile and zero otherwise, total current compensation (TCC), total direct compensation (TDC1), and three dummies measure managerial influence in the board. Columns 2 and 3 respectively summarize the covariates for the treated and the potential controls. Column 4 presents the raw difference in the covariates between the treated and potential controls, and Column 5 reports the within matches difference in the covariates between the treated and actual controls. To identify the actual controls, we apply Abadie and Imbens' (2011) matching estimator, setting the number of matches per treated firm to four. * and ** denote significance at 5% and 1%, respectively.

Variable	Treated		Potential Controls		Raw Difference		Within Matches Difference	
	Mean	S.D.	Mean	S.D.	Mean	S.E.	Mean	S.E.
Fiscal Year 1995	0.11	0.32	0.14	0.35	-0.03	0.03	0.01	0.01
Fiscal Year 1996	0.07	0.26	0.08	0.27	-0.01	0.03	0.00	0.00
Fiscal Year 1997	0.08	0.27	0.06	0.24	0.02	0.03	0.00	0.00
Fiscal Year 1998	0.15	0.36	0.07	0.25	0.08*	0.04	0.00	0.00
Fiscal Year 1999	0.15	0.36	0.17	0.37	-0.01	0.04	0.01	0.01
Fiscal Year 2000	0.11	0.32	0.06	0.24	0.05	0.03	0.00	0.00
Fiscal Year 2001	0.15	0.36	0.31	0.46	-0.16**	0.04	0.00	0.01
Fiscal Year 2002	0.17	0.38	0.12	0.32	0.05	0.04	-0.02	0.02
Dummy SIC = 1 - Consumer Industries/Services	0.19	0.40	0.26	0.44	-0.06	0.04	0.01	0.01
Dummy SIC = 2 - Manufacturing, Energy & Utilities	0.23	0.42	0.34	0.47	-0.11*	0.04	0.00	0.01
Dummy SIC = 3 - Business Equipment, Telecomm, TV	0.34	0.48	0.22	0.41	0.12*	0.05	0.02	0.02
Dummy SIC = 4 - Healthcare, Medical Equip & Drugs	0.07	0.26	0.07	0.25	0.00	0.03	0.00	0.01
Dummy SIC = 5 - Other Industries	0.16	0.37	0.11	0.32	0.05	0.04	0.03	0.03
Log of Market Capitalization	6.99	1.39	7.24	1.44	-0.25	0.15	-0.24**	0.08
Dummy Large Market Cap (70th percentile)	0.56	0.50	0.63	0.48	-0.08	0.05	-0.06*	0.03
Total Liabilities/Total Assets	0.48	0.24	0.49	0.19	0.00	0.02	-0.01	0.01
O-Score	0.20	0.21	0.15	0.16	0.06**	0.02	0.03**	0.01
Cumulative Stock Return over $[-3, -5]$	0.48	1.07	0.24	0.51	0.24*	0.11	0.04	0.02
Dummy Top Quintile O-Score Distribution	0.09	0.29	0.02	0.15	0.07*	0.03	0.00	0.00
Dummy Bottom Quintile Cum Stock Return Distribution	0.06	0.24	0.04	0.20	0.02	0.02	0.01	0.01
Total Current Compensation (TCC)	1.01	1.07	1.09	0.76	-0.08	0.11	-0.06	0.05
Total Direct Compensation 1 (TDC1)	3.56	6.47	2.75	3.28	0.81	0.66	0.40	0.33
Dummy Executive(s) in the Compensation Committee	0.18	0.39	0.14	0.35	0.04	0.04	0.07**	0.02
Dummy Some Executive(s) Serve as Director(s)	0.96	0.20	0.94	0.23	0.02	0.02	-0.01	0.01
Dummy CEO also Serves as the Board's Chair	0.49	0.50	0.51	0.50	-0.02	0.05	-0.04	0.03
Number of Observations	99		1,205		-		396	

Table 3. Descriptive Statistics for Measures of Managerial Influence over Board of Directors

This table reports the descriptive statistics of five measures of managerial influence over the board for the treated and the potential controls during the event window from period -3 to period +3, where the benchmark period 0 refers to the year when a firm falls into financial distress. The measures are: number of executives that serve as board directors (EXECDIR_N); number of executives that are listed in the compensation committee interlocks section of the firm's proxy (INTERLOCK_N); CEO-chair dummy that equals one if the CEO also serves as the firm's board chairperson and zero otherwise (CEOCHAIR); executive-director dummy that equals one if at least one executive serves as director and zero otherwise (EXECDIR_D); and interlock-dummy (INTERLOCK_D) that equals one if at least one executive serves in the compensation committee and zero otherwise.

Variable	Period	Treated			Potential Controls		
		Mean	S.D.	N	Mean	S.D.	N
EXECDIR_N = No. of executives that serve as directors	-3	1.82	0.98	99	1.87	1.07	1,205
	-2	1.83	1.00	99	1.86	1.04	1,205
	-1	1.80	0.90	99	1.82	1.01	1,205
	0	1.75	0.91	99	1.77	1.02	1,203
	+1	1.39	0.83	99	1.68	1.00	1,204
	+2	1.32	0.87	98	1.58	0.96	1,204
	+3	1.28	0.87	96	1.51	0.93	1,077
INTERLOCK_N = No. of executives that serve in the compensation committee	-3	0.27	0.73	99	0.19	0.56	1,205
	-2	0.22	0.62	99	0.20	0.55	1,205
	-1	0.21	0.61	99	0.17	0.53	1,205
	0	0.14	0.52	99	0.14	0.49	1,203
	+1	0.10	0.48	99	0.14	0.51	1,204
	+2	0.03	0.22	98	0.11	0.44	1,204
	+3	0.04	0.25	96	0.08	0.39	1,077
CEOCHAIR = Dummy =1 if CEO also serves as the board's chair	-3	0.49	0.50	99	0.51	0.50	1,205
	-2	0.60	0.49	99	0.59	0.49	1,205
	-1	0.64	0.48	99	0.64	0.48	1,205
	0	0.56	0.50	99	0.62	0.48	1,203
	+1	0.42	0.50	99	0.60	0.49	1,204
	+2	0.44	0.50	98	0.59	0.49	1,204
	+3	0.42	0.50	96	0.58	0.49	1,077
EXECDIR_D = Dummy =1 if at least one executive serves as director	-3	0.96	0.20	99	0.94	0.23	1,205
	-2	0.96	0.20	99	0.96	0.20	1,205
	-1	0.97	0.17	99	0.96	0.20	1,205
	0	0.97	0.17	99	0.95	0.22	1,203
	+1	0.88	0.33	99	0.94	0.23	1,204
	+2	0.88	0.33	98	0.93	0.25	1,204
	+3	0.88	0.33	96	0.92	0.27	1,077
INTERLOCK_D = Dummy=1 if at least one executive serves in the compensation committee	-3	0.18	0.39	99	0.14	0.35	1,205
	-2	0.15	0.36	99	0.14	0.35	1,205
	-1	0.13	0.34	99	0.12	0.33	1,205
	0	0.09	0.29	99	0.10	0.30	1,203
	+1	0.05	0.22	99	0.09	0.29	1,204
	+2	0.02	0.14	98	0.07	0.26	1,204
	+3	0.03	0.17	96	0.05	0.23	1,077

Table 4. Average Treatment Effect for the Treated (ATT) of Financial Distress on Managerial Influence Measures

This table reports the average treatment effect for the treated (ATT) of financial distress on managerial influence over the board, using Abadie and Imbens' (2011) bias-corrected matching estimator. We measure managerial influence by five variables: number of executives that serve as board directors (EXECDIR); number of executives that are listed in the compensation committee interlocks section of the firm's proxy (INTERLOCK); CEO-chair dummy that equals one if the CEO also serves as the firm's board chairperson and zero otherwise (CEOCHAIR); executive-director dummy that equals one if at least one executive serves as director and zero otherwise (EXECDIR_D); and interlock-dummy (INTERLOCK_D) that equals one if at least one executive serves in the compensation committee and zero otherwise. Panel A contains the estimates using variables in levels, and Panel B the difference-in-difference estimates. Robust standard errors are reported in parentheses. *, ** and *** denote significance at 10%, 5% and 1%, respectively.

Panel A. Variables in Levels

Period	EXECDIR_N	INTERLOCK_N	CEOCHAIR	EXECDIR_D	INTERLOCK_D
-2	0.06 (0.11)	-0.06 (0.06)	0.04 (0.05)	0.00 (0.02)	-0.03 (0.03)
-1	0.07 (0.10)	-0.01 (0.06)	0.05 (0.05)	0.02 (0.02)	-0.02 (0.03)
0	0.00 (0.11)	-0.10* (0.05)	-0.04 (0.06)	0.03* (0.02)	-0.05* (0.03)
+1	-0.32*** (0.09)	-0.13*** (0.05)	-0.15** (0.06)	-0.07** (0.03)	-0.08*** (0.02)
+2	-0.27*** (0.09)	-0.16*** (0.03)	-0.13** (0.06)	-0.06* (0.03)	-0.09*** (0.02)
+3	-0.18* (0.09)	-0.10*** (0.03)	-0.14** (0.06)	-0.03 (0.03)	-0.06*** (0.02)

Panel B. Variables in Differences with Respect to their Values in Period -3

Period	EXECDIR_N	INTERLOCK_N	CEOCHAIR	EXECDIR_D	INTERLOCK_D
-2	0.05 (0.07)	-0.03 (0.02)	0.04 (0.04)	-0.01 (0.01)	-0.02 (0.02)
-1	0.00 (0.09)	-0.03 (0.04)	0.04 (0.06)	0.01 (0.01)	-0.02 (0.03)
0	-0.04 (0.10)	-0.04 (0.05)	-0.04 (0.06)	0.02 (0.02)	-0.05 (0.03)
+1	-0.34*** (0.11)	-0.09 (0.05)	-0.14** (0.06)	-0.07** (0.03)	-0.08** (0.04)
+2	-0.29** (0.12)	-0.11** (0.05)	-0.14** (0.07)	-0.06* (0.03)	-0.09*** (0.03)
+3	-0.16 (0.12)	-0.02 (0.06)	-0.13* (0.07)	-0.04 (0.03)	-0.05 (0.03)

Notes:

Robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 5. Descriptive Statistics for Components of CEO Compensation

This table summarizes components of CEO compensation for the treated and the potential controls across years inside the event window from period -3 to period +3, where the benchmark period 0 refers to the year when a firm falls into financial distress. Total current compensation (TCC) is the sum of Salary and Bonus. Total direct compensation (TDC1) is the sum of total current compensation, the value of restricted stock grants (RSTGRNT), the Black-Scholes value of stock option grants (BLKV), and others. The "others" item includes other annual short-term compensation, payouts from long-term incentive plans, and all other long-term compensation. Besides TDC1, we use another measure of total flow compensation (TDC2) to a CEO within one fiscal year, which is the sum of TCC, RSTGRNT, value realized from option exercises (EXER), and others. All monetary variables are in millions of 2005 dollars.

Variable	Period	Treated			Potential Controls		
		Mean	S.D.	N	Mean	S.D.	N
Salary	-3	0.5	0.3	97	0.6	0.3	1,203
	-2	0.6	0.3	97	0.7	0.3	1,195
	-1	0.6	0.3	97	0.7	0.3	1,203
	0	0.6	0.4	97	0.7	0.4	1,201
	+1	0.5	0.3	96	0.7	0.4	1,202
	+2	0.6	0.3	96	0.7	0.4	1,201
	+3	0.6	0.3	94	0.8	0.4	1,075
Bonus	-3	0.4	0.5	97	0.5	0.5	1,203
	-2	0.4	0.6	97	0.6	0.9	1,195
	-1	0.2	0.6	97	0.6	0.8	1,203
	0	0.1	0.3	97	0.6	0.9	1,201
	+1	0.3	0.5	96	0.7	1.0	1,202
	+2	0.4	0.5	96	0.8	1.1	1,201
	+3	0.5	0.6	94	0.9	1.1	1,075
TCC = Total Current Compensation (Salary + Bonus)	-3	0.9	0.7	97	1.1	0.8	1,203
	-2	1.0	0.8	97	1.2	1.1	1,195
	-1	0.9	0.8	97	1.3	1.0	1,203
	0	0.7	0.6	97	1.3	1.2	1,201
	+1	0.9	0.7	96	1.4	1.2	1,202
	+2	1.0	0.7	96	1.5	1.3	1,201
	+3	1.1	0.8	94	1.6	1.4	1,075
BLKV = Black-Scholes Value of Option Grants	-3	1.9	3.6	97	1.2	2.4	1,203
	-2	3.0	9.0	97	2.5	8.1	1,191
	-1	4.8	15.4	97	2.3	5.7	1,195
	0	1.1	2.5	97	2.6	5.5	1,192
	+1	0.9	1.1	95	2.8	10.0	1,193
	+2	1.1	1.8	95	2.8	6.6	1,193
	+3	1.9	5.0	91	2.6	6.8	1,068
RSTGRNT = Restricted Stock Grants Value	-3	0.2	0.8	97	0.1	0.5	1,203
	-2	0.1	0.8	97	0.2	1.0	1,195
	-1	0.1	0.7	97	0.3	1.0	1,203
	0	0.1	0.4	97	0.4	1.5	1,201
	+1	0.2	0.6	96	0.4	1.6	1,202
	+2	0.2	0.6	96	0.4	1.5	1,201
	+3	0.3	1.0	94	0.7	2.3	1,075
TDC1 = Total Direct Compensation 1 (TCC + BLKV + RSTGRNT + Others)	-3	3.2	4.9	97	2.7	3.1	1,203
	-2	4.4	9.8	97	4.3	8.9	1,191
	-1	6.3	17.2	97	4.3	6.7	1,195
	0	2.4	3.5	97	4.7	7.0	1,192
	+1	2.3	2.5	95	5.1	11.0	1,193
	+2	2.6	3.1	95	5.2	8.2	1,193
	+3	3.9	6.4	91	5.6	9.6	1,068
EXER = Value Realized from Options Exercises	-3	1.1	4.2	96	1.4	5.9	1,190
	-2	5.5	18.5	97	1.8	6.6	1,194
	-1	2.7	14.7	97	1.8	6.6	1,202
	0	0.1	0.3	97	1.6	5.8	1,197
	+1	0.1	0.3	96	1.9	8.4	1,201
	+2	0.4	1.5	95	1.8	6.6	1,199
	+3	0.3	1.3	93	2.7	10.7	1,072
TDC2 = Total Direct Compensation 2 (TCC + EXER + RSTGRNT + Others)	-3	2.6	4.8	97	2.8	6.4	1,203
	-2	7.2	19.8	97	3.6	7.8	1,195
	-1	4.3	16.7	97	3.8	7.4	1,203
	0	1.2	1.6	97	3.7	6.8	1,201
	+1	1.6	1.9	96	4.2	9.4	1,202
	+2	2.0	2.7	96	4.3	8.0	1,201
	+3	2.2	3.1	94	5.7	13.5	1,075

Table 6. Average Treatment Effect for the Treated (ATT) of Financial Distress on CEO Compensation

This table reports the average treatment effect for the treated (ATT) of financial distress on CEO compensation, using Abadie and Imbens' (2011) bias-corrected matching estimator. All the outcome variables for CEO compensation components are defined in Table 5, except the last one (OPGRNT) which is defined as the logarithm value of the ratio of the total number of option shares granted to the total number of firm shares outstanding. The matching estimator sets the number of matches per treated firm to four and takes the difference with respect to the values of CEO compensation components in period -3. Panel A contains the estimates using variables in levels, and Panel B the difference-in-difference estimates. Robust standard errors are reported in parentheses. *, ** and *** denote significance at 10%, 5% and 1%, respectively.

Panel A. Variables in Levels

Period	Salary	Bonus	TCC	BLKV	RSTGRNT	TDC1	EXER	TDC2	OPGRNT
-2	0.0 (0.0)	-0.1 (0.1)	-0.1 (0.1)	0.1 (0.9)	-0.1 (0.2)	0.0 (1.1)	3.3* (1.9)	3.2 (2.1)	0.1* (0.0)
-1	0.0 (0.0)	-0.3*** (0.1)	-0.4*** (0.1)	1.1 (1.2)	-0.1 (0.1)	0.9 (1.4)	0.5 (1.2)	0.2 (1.5)	0.1* (0.0)
0	-0.1* (0.0)	-0.5*** (0.1)	-0.6*** (0.1)	-2.8*** (0.4)	-0.2* (0.1)	-3.3*** (0.7)	-1.9*** (0.4)	-2.6*** (0.5)	0.0 (0.0)
+1	-0.1** (0.0)	-0.4*** (0.1)	-0.5*** (0.1)	-4.3*** (0.7)	0.0 (0.1)	-4.1*** (0.8)	-2.5*** (0.5)	-3.2*** (0.7)	0.1*** (0.0)
+2	-0.1*** (0.0)	-0.3*** (0.1)	-0.5*** (0.1)	-2.7*** (0.5)	-0.2 (0.1)	-3.4*** (0.6)	-1.0*** (0.3)	-1.9*** (0.5)	0.0 (0.0)
+3	-0.1*** (0.0)	-0.2** (0.1)	-0.4*** (0.1)	-0.8 (0.6)	-0.5** (0.3)	-1.4* (0.9)	-2.2*** (0.6)	-2.9*** (0.8)	0.1*** (0.0)

Panel B. Variables in Differences with Respect to their Values in Period -3

Period	Salary	Bonus	TCC	BLKV	RSTGRNT	TDC1	EXER	TDC2	OPGRNT
-2	0.0 (0.0)	-0.1* (0.1)	-0.1* (0.1)	0.3 (0.8)	-0.2 (0.2)	0.3 (1.0)	2.7* (1.6)	2.7* (1.6)	0.0 (0.0)
-1	0.0 (0.0)	-0.3*** (0.1)	-0.3*** (0.1)	0.5 (1.7)	-0.2** (0.1)	0.0 (1.8)	0.9 (1.9)	0.2 (1.6)	0.0 (0.0)
0	-0.1*** (0.0)	-0.5*** (0.1)	-0.5*** (0.1)	-2.4*** (0.4)	-0.3** (0.1)	-3.2*** (0.6)	-1.6 (1.2)	-2.7*** (1.0)	0.0 (0.0)
+1	-0.1*** (0.0)	-0.4*** (0.1)	-0.5*** (0.1)	-4.9*** (0.8)	-0.1 (0.1)	-5.6*** (1.0)	-2.1* (1.2)	-3.3*** (0.9)	0.1** (0.0)
+2	-0.1*** (0.0)	-0.3*** (0.1)	-0.4*** (0.1)	-2.6*** (0.6)	-0.2* (0.1)	-3.2*** (0.7)	-0.5 (1.2)	-1.9 (1.3)	0.0 (0.0)
+3	-0.1*** (0.0)	-0.2** (0.1)	-0.3*** (0.1)	-0.5 (0.7)	-0.5** (0.2)	-1.2 (0.9)	-2.5* (1.4)	-3.9*** (1.5)	0.1** (0.0)

Notes:

Robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

All variables are expressed in \$ millions, except for OPGRNT

TCC = Total Current Compensation (Salary + Bonus)

BLKV = Black-Scholes Value of Option Grants

RSTGRNT = Restricted Stock Grants Value

TDC1 = Total Direct Compensation 1 (TCC + BLKV + RSTGRNT + Others)

EXER = Value Realized from Option Exercises

TDC2 = Total Direct Compensation 2 (TCC + EXER + RSTGRNT + Others)

OPGRNT = Log(Option Shares Granted / Total Shares Outstanding)

Table 7. CEO Turnover Rates

This table reports the CEO turnover rates for the treated and the potential controls in various sub-windows of the event window from period -3 to period +3, where the benchmark period 0 refers to the year when a firm falls into financial distress. Note that the sum of the number of observations in the no-turnover and turnover groups does not equal the total number of observations because for some firms turnover information is missing in some years.

Turnover in Period	Turnover Rate		Treated (# Obs.)		Potential Controls (# Obs.)	
	Treated	Potential Controls	No Turnover	Turnover	No Turnover	Turnover
[-1,+1]	52%	33%	47	51	812	391
[-1,+2]	63%	41%	36	61	711	490
[-1,+3]	69%	48%	29	65	559	514
[-2,+2]	66%	47%	33	64	636	557
[-2,+3]	69%	53%	29	65	496	570
[-3,+3]	69%	56%	29	65	467	599

Table 8. Average Treatment Effect for the Treated (ATT) of Financial Distress on CEO Turnover Rates

This table reports the average treatment effect for the treated (ATT) of financial distress on CEO compensation, using Abadie and Imbens' (2011) bias-corrected matching estimator. Panel A and Panel B contain results by various multi-year windows and year-by-year, respectively. Robust standard errors are reported in parentheses. *, ** and *** denote significance at 10%, 5% and 1%, respectively.

Panel A. Turnover Rates by Multi-Year Windows

Period	[-1,+1]	[-1,+2]	[-1,+3]	[-2,+2]	[-2,+3]	[-3,+3]
ATT	0.19***	0.23***	0.28***	0.20***	0.22***	0.21***
(S.E.)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)

Panel B. Turnover Rates Year-by-Year

Period	-2	-1	0	+1	+2	+3
ATT	-0.01	0.05	-0.02	0.21***	0.06	0.08*
(S.E.)	(0.03)	(0.04)	(0.04)	(0.05)	(0.04)	(0.04)

Notes:

Robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 9. Average Treatment Effect for the Treated (ATT) of Financial Distress on CEO Compensation by CEO Turnover: Difference-in-Difference Estimates

This table reports the average treatment effect for the treated (ATT) of financial distress on CEO compensation for the no-turnover group (in Panel A) and the turnover group (in Panel B), using Abadie and Imbens' (2011) bias-corrected matching estimator. All the outcome variables for CEO compensation components are defined in Table 3, except the last one (OPGRNT) which is defined as the logarithm value of the ratio of the total number of option shares granted to the total number of firm shares outstanding. The matching estimator sets the number of matches per treated firm to four and takes the difference with respect to the values of CEO compensation components in period -3. Robust standard errors are reported in parentheses. *, ** and *** denote significance at 10%, 5% and 1%, respectively.

Panel A. Firms with No CEO Turnover in [-3,+3]

Period	Salary	Bonus	TCC	BLKV	RSTGRNT	TDC1	EXER	TDC2	OPGRNT
-2	0.0 (0.0)	-0.1 (0.1)	-0.1 (0.1)	0.1 (0.8)	-0.1* (0.0)	-0.1 (1.1)	0.3 (0.6)	0.7 (0.8)	0.2*** (0.1)
-1	0.0 (0.0)	-0.3*** (0.1)	-0.3*** (0.1)	-1.7*** (0.5)	0.0 (0.0)	-2.1*** (0.7)	-0.4 (0.3)	-1.1** (0.4)	0.3*** (0.1)
0	0.0* (0.0)	-0.3*** (0.1)	-0.4*** (0.1)	-1.3** (0.6)	-0.2** (0.1)	-1.7** (0.7)	-1.0 (1.7)	-3.4** (1.7)	0.6*** (0.1)
+1	-0.1*** (0.0)	-0.4*** (0.1)	-0.5*** (0.1)	-2.4*** (0.5)	-0.2* (0.1)	-3.2*** (0.7)	-3.7*** (0.8)	-4.8*** (0.9)	0.1* (0.1)
+2	-0.1*** (0.0)	-0.3** (0.1)	-0.4*** (0.1)	-2.1*** (0.8)	-0.3* (0.1)	-2.6** (1.0)	-1.8* (1.1)	-2.0* (1.1)	-0.1 (0.1)
+3	-0.1*** (0.0)	-0.3** (0.1)	-0.3** (0.1)	-0.8* (0.4)	-0.2 (0.2)	-0.6 (0.8)	0.1 (1.6)	0.3 (1.8)	0.1 (0.1)

Panel B. Firms with CEO Turnover only in [-1,+1]

Period	Salary	Bonus	TCC	BLKV	RSTGRNT	TDC1	EXER	TDC2	OPGRNT
-2	0.0 (0.0)	-0.1*** (0.0)	-0.1** (0.1)	-0.8 (0.5)	-0.1 (0.1)	-1.0* (0.6)	-0.5 (0.4)	-0.8* (0.5)	0.0 (0.1)
-1	0.0 (0.0)	-0.2** (0.1)	-0.2 (0.1)	4.8** (2.0)	0.0 (0.1)	4.7** (2.1)	0.1 (0.3)	0.1 (0.6)	0.1 (0.1)
0	-0.1** (0.0)	-0.3*** (0.1)	-0.3*** (0.1)	-1.0** (0.4)	-0.1 (0.1)	-1.3*** (0.5)	0.1 (0.6)	-0.3 (0.6)	0.3*** (0.1)
+1	0.0 (0.0)	-0.1 (0.1)	-0.1 (0.1)	-2.5** (1.2)	0.0 (0.4)	-3.0* (1.5)	0.4 (0.6)	0.4 (0.8)	0.0 (0.1)
+2	0.0 (0.0)	-0.2** (0.1)	-0.2** (0.1)	-1.6*** (0.6)	0.1 (0.1)	-1.2** (0.6)	0.5 (0.7)	0.4 (0.8)	0.0 (0.1)
+3	0.0 (0.0)	-0.1 (0.1)	-0.2 (0.2)	-1.6** (0.7)	0.0 (0.1)	-2.1*** (0.8)	-1.0 (0.7)	-1.4* (0.8)	-0.1 (0.1)

Notes:

Robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

All variables are expressed in \$ millions, except for OPGRNT

TCC = Total Current Compensation (Salary + Bonus)

BLKV = Black-Scholes Value of Option Grants

RSTGRNT = Restricted Stock Grants Value

TDC1 = Total Direct Compensation 1 (TCC + BLKV + RSTGRNT + Others)

EXER = Value Realized from Option Exercises

TDC2 = Total Direct Compensation 2 (TCC + EXER + RSTGRNT + Others)

OPGRNT = Log(Option Shares Granted / Total Shares Outstanding)

Table 10. Descriptive Statistics for the Proportion of Lucky Grants: Before and After SOX

This table reports the descriptive statistics for the proportion of lucky grants for the treated and the potential controls before and after the adoption of the Sarbanes-Oxley Act (SOX) during the event window from period -3 to period +3, where the benchmark period 0 refers to the year when a firm falls into financial distress. We follow Bebchuk, Grinstein and Peyer (2010) and define lucky grants as the options that are given at or below the lowest stock price of the grant month. We calculate the raw measure as the (weighted by grant size) proportion, in the fiscal year, of unscheduled grants that are lucky grants. The net measure subtracts from the raw measure the average proportion of trading days in which the stock price was at the lowest stock price of the grant month.

Proportion of lucky grants												
Period	Raw						Net					
	Mean	Treated S.D.	N	Mean	Potential Controls S.D.	N	Mean	Treated S.D.	N	Mean	Potential Controls S.D.	N
Before SOX						Before SOX						
-3	0.15	0.35	99	0.14	0.33	1,205	0.11	0.34	99	0.09	0.33	1,195
-2	0.19	0.38	99	0.14	0.34	1,200	0.14	0.37	99	0.10	0.34	1,200
-1	0.09	0.28	99	0.13	0.32	1,200	0.05	0.28	99	0.08	0.31	1,198
0	0.08	0.27	94	0.11	0.31	1,174	0.03	0.26	94	0.07	0.30	1,174
+1	0.09	0.28	80	0.12	0.31	1,012	0.04	0.29	80	0.08	0.31	1,012
+2	0.10	0.29	62	0.11	0.30	688	0.06	0.27	62	0.07	0.30	688
+3	0.05	0.21	52	0.12	0.32	494	0.01	0.21	52	0.08	0.32	494
After SOX						After SOX						
+2	0.03	0.17	35	0.05	0.21	521	-0.01	0.17	35	0.01	0.21	521
+3	0.07	0.24	41	0.09	0.27	586	0.04	0.24	41	0.05	0.27	586

Table 11. Average Treatment Effect for the Treated (ATT) of Financial Distress on the Proportion of Lucky Grants

This table reports the average treatment effect for the treated (ATT) of financial distress on the proportion of lucky grants (only for grants before the adoption of the Sarbanes-Oxley Act, SOX), using Abadie and Imbens' (2011) bias-corrected matching estimator. We follow Bebchuk, Grinstein and Peyer (2010) and define lucky grants as the options that are given at or below the lowest stock price of the grant month. We calculate the raw measure as the (weighted by grant size) proportion, in the fiscal year, of unscheduled grants that are lucky grants. The net measure subtracts from the raw measure the average proportion of trading days in which the stock price was at the lowest stock price of the grant month. Panel A contains the estimates using variables in levels, and Panel B the difference-in-difference estimates. Robust standard errors are reported in parentheses. *, ** and *** denote significance at 10%, 5% and 1%, respectively.

Panel A. Variables in Levels

Period	Proportion of lucky grants	
	Raw	Net
-2	0.01 (0.04)	0.01 (0.04)
-1	-0.06* (0.04)	-0.06* (0.04)
0	-0.06* (0.03)	-0.07** (0.03)
+1	-0.07* (0.04)	-0.08** (0.04)
+2	-0.04 (0.04)	-0.05 (0.04)
+3	-0.03 (0.04)	-0.05 (0.04)

Panel B. Variables in Differences with Respect to their Values in Period -3

Period	Proportion of lucky grants	
	Raw	Net
-2	0.06 (0.06)	0.06 (0.06)
-1	-0.01 (0.06)	-0.02 (0.05)
0	-0.03 (0.05)	-0.03 (0.05)
+1	-0.04 (0.06)	-0.05 (0.06)
+2	0.05 (0.06)	0.02 (0.05)
+3	-0.05 (0.07)	-0.05 (0.06)

Notes:

Robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%