

CEO Power and Compensation in Financially Distressed Firms*

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Abstract

We study the changes in CEO power and compensation that arise when firms go through financial distress. We use a matching estimator to identify suitable controls and estimate the *causal effects* of financial distress for a sample of U.S. public companies from 1992 to 2005. We document that, relative to those in control firms, the CEOs of distressed firms experience significant reductions in total compensation; the bulk of this reduction derives from the decline in value of new grants of stock options. These results hold not only for incumbent CEOs but also, surprisingly, for newly hired CEOs. Financial distress has important consequences on corporate governance, decreasing managerial influence over the board. We find that, among distressed firms, there is a significant decrease in the proportion of CEOs holding board chairmanship, and in the fractions of executives serving as directors or in the compensation committee of the board. We also show that periods of financial distress are associated with a decrease in opportunistic timing behavior of stock option awards. The results are suggestive of a link between managerial power and executive compensation.

JEL Classification: G30, J33, M52

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1 Introduction

Executive pay in U.S. public companies has been rising steadily since the 1990s. The trend has persisted throughout this decade, even through the current financial crisis and economic recession. Several high-publicity controversies around Wall Street pay packages in the middle of the financial crisis have triggered a sharp escalation in the public's outrage about executive compensation. In response, the U.S. House of Representatives passed legislation in July 2009 imposing new constraints on executive pay, such as giving shareholders the right to cast annual non-binding votes on executive pay and giving government regulators the ability to curb some forms of compensation they see as harmful (Fuller, 2009). In addition, to rein in risk-taking at financial institutions, the Federal Reserve proposed new rules in September 2009, requiring bank employee pay policies to receive its approval (Paletta and Hilsenrath, 2009).¹

Implicit in these proposed regulations is the view that a large fraction of CEO compensation reflects rent extraction by powerful CEOs (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 large part of the executive pay literature posits that compensation, in particular equity-based compensation, aligns managerial interests with shareholder interests, ameliorating agency problems (see, e.g., the surveys by Murphy, 1999; and by Core, Guay and Larcker, 2003). Many observed executive pay patterns appear as inconsistent with the predictions of standard optimal contracting models, which has spurred a series of studies extending 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.

In this paper we contribute to understanding the determinants of CEO pay by studying the changes in CEO power and compensation that arise when firms go through financial distress, for a sample of U.S. public companies from 1992 to 2005. By finding suitable comparable firms to those in financial distress, we estimate the *causal effects* of financial distress on CEO compensation, managerial influence over the board, and opportunistic timing of option grants. Overall, the findings

¹Some rules on executive compensation have already been in place. For example, the U.S. Securities and Exchange Commission passed new rules in 2006 requiring companies to more fully and clearly disclose what they are paying to top executives and the total compensation figure. The Emergency Economic Stabilization Act of 2008 directs the Treasury to request firms participating in the Troubled Assets Relief Program (TARP) to “meet appropriate standards for executive compensation and corporate governance” (GPO, 2008, p. 12).

from our study are suggestive of a link between managerial power and executive compensation.

We define a firm as financially distressed if it either files for Chapter 11 bankruptcy protection or satisfies both of the following conditions: i) its Ohlson’s (1980) *O-score*, a widely used measure of financial health, is in the top quintile of the O-score distribution, and ii) its past three-year cumulative stock return is in the lowest quintile of the cumulative-return distribution, among all non-financial publicly traded firms. For our variables of interest we contrast the changes for financially distressed firms (i.e., the “treated”) over a six-year window (from two years before to three years after the onset of financial distress) with the changes among *comparable* firms (i.e., the “controls”) that do not suffer financial distress.

We apply a bias-corrected matching estimator proposed by Abadie and Imbens (2006, 2007) to find the group of suitable controls.² This group consists of firms that are not only never financially distressed but also are (in a statistical sense) similar in observed characteristics to the treated firms *before* financial distress. We use the controls to construct the *counterfactual* outcomes of interest for the treated (i.e., what the outcomes would have been for the distressed companies if they had not been in financial distress). As pay practices and regulations have changed over time, our use of control firms for constructing counterfactual outcomes filters out those changes and properly identifies the effect of financial distress on outcome variables of interest. As a result, we can interpret our results as representing the *causal effects* of financial distress on CEO compensation, managerial influence over the board, and opportunistic timing of option grants.

Our paper contributes to the under-studied empirical literature on executive compensation for financially distressed firms. The original and probably the most cited study in this field is by Gilson and Vetsuypens (1993), who examine executive compensation in financially distressed firms during the 1980s, and find that compensation becomes more sensitive to performance after distress. In a recent study, Henderson (2007) analyzes a similar period to the one in our paper, and concludes that compensation practices remain largely unchanged after financial distress. These two studies

²The Abadie and Imbens’ (2006, 2007) estimator allows matching directly on covariates, instead of requiring matching on the propensity score (i.e. the probability of treatment) as is the case with other matching estimators. The estimator we consider 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 a well defined asymptotic variance. In addition, this estimator has desirable properties in situations like the one we face in our study — estimating a propensity score model can be problematic when the number of treated observations is small relative to the number of control observations.

employ a similar empirical strategy that compares executive pay only in financially distressed firms before and after distress. A fundamental difference of our study is that we also contrast the changes in executive pay before and after distress for the treated with the changes experienced by an appropriately chosen control group. As mentioned above, using controls is the only way to account for market-wide shifts in compensation practices.

In our analysis of CEO compensation, we examine various components such as total cash compensation, stock grants, stock option grants, and total flow compensation. We document that, not surprisingly, CEO turnover rates increase markedly in the distressed firms, and that surviving CEOs experience significant reductions in total cash compensation and total flow compensation upon and after the firms fall into financial distress. The bulk of this decline in total compensation derives from the reduction in value of new grants of stock options. This reduction in value is not explained by changes in stock volatility, stock price, option maturity, or the method used to calculate the Black-Scholes value.

Surprisingly, we find that newly hired CEOs at financially distressed firms are paid significantly less than their predecessors and than newly hired CEOs at non-distressed firms. Similar to our findings for incumbent CEOs, this result is driven by lower stock based compensation. We do not find any evidence that differences in stock performance between distressed and non-distressed firms can explain this lower compensation. Nor do we find any evidence that there are “quality” differentials between the two groups of new CEOs.

Our study is also relevant to the literature on the relation between corporate governance and firm performance. Most prior papers have concentrated on the effects of governance on performance (see, e.g., Hermalin and Weisbach, 1991; Gompers, Ishii, and Metrick, 2003; Bebchuk, Cohen, and Ferrell, 2009), without paying particular attention to the case of distressed firms (except for Fich and Slezak, 2008, who analyze that relation within distressed firms). We investigate the other direction of this relation, i.e., the impact of firm performance on governance, which has been largely ignored in this literature. An exception is the paper by Gilson (1990), who examines the effect of financial distress on corporate boards and finds that following a bankruptcy or private restructuring, banks take an active role in the firms governance, including appointing a number of directors. Our empirical strategy differs in that we construct a control group, and Gilson does not.

In our analysis of the effects of financial distress on corporate governance, we focus on managerial influence over the board. We measure managerial influence by several metrics: number of executives serving as board directors, number of executives 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. We report a noticeable loss of managerial influence over the board among the treated firms after they enter into financial distress, as compared to the evolution for the control firms. In particular, following the incidence of financial distress, there is a significant decrease in the proportion of CEOs holding the chairmanship, and in the proportion and number of executives serving as directors or in the compensation committee of the board.

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. Bebchuk, Grinstein, and Peyer (2009) 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 (2009) 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).

Overall, our results are suggestive of a link between managerial power and executive compensation. The key insight of our study is that, *a priori*, the occurrence of financial distress weakens managerial power 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. The hypothesis that managerial influence diminishes after financial distress is corroborated by our findings. The simultaneous declines in CEOs' stock-related pay (for both incumbent and new CEOs) and in opportunistic timing of option grants lend support to our interpretation that managerial power affects CEO pay.

When evaluating our findings, 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 explains in detail the econometric methodology and the empirical strategy. Section 4 reports the results on the effects of financial distress on CEO compensation and turnover. Section 5 examines the dynamics of managerial influence over the board of directors and of opportunistic option timing practices before and after financial distress. Section 6 concludes.

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, 2006; Narayanan, Schipani, and Seyhun, 2007; Bernile and Jarrell, 2009).

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 spanning the period from 1992 to 2005. 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

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.

satisfied *simultaneously*: their O-scores are in the top quintile of the O-score distribution and their 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' (2006, 2007) matching estimator. (See Section 3 for a detailed discussion of the estimator.) The matching covariates include O-score, past three-year cumulative stock return, firm size measured by the log value of market capitalization, leverage measured by debt-to-asset ratio, one O-score-based dummy, one cumulative-return-based dummy, one size-based dummy, industry dummies, fiscal year dummies, total current compensation, and total direct compensation. All the matching covariates are measured three years prior to the occurrence of financial distress. Specifically, we use the raw values of the O-score, the past three-year cumulative stock return, and firm size as measures of a firm's financial health. 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. We include the debt-to-asset ratio because the literature documents that leverage is an important determinant of a firm's financial status. It is also very important to properly control for industry-wide differences. Thus, we adopt Fama and French's 5-industry definition based on the 4-digit SIC code and create five industry dummies on which we also match. To control for business-cycle-related effects, we use fiscal year dummies. Finally, to achieve further 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.

The third set of key variables is comprised of three groups of outcome variables: CEO

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.

compensation, managerial influence over the board of directors, 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

Due to the data coverage of ExecuComp, our sample starts in 1992 and ends in 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 or compensation variables 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.⁷ To avoid introducing serial correlation issues in the estimation of the standard errors of our matching estimators, we assign each of the potential control firms to only one particular year. We defer to Section 3.2 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 *causal 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 select as controls solely those non-distressed firms that are statistically similar to the treated firms in observable characteristics and compensation policies *before* the episode of financial distress. Below we explain the details of our matching approach.

3.1 Matching Estimators

There are different methods that can be used to implement our approach. In this paper we use the Abadie and Imbens' (2006, 2007, AI hereafter) matching estimators which allow matching directly on covariates (both continuous and discrete). These estimators have several attractive

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).

⁷For several of the compensation variables we observe outliers that could potentially affect the results. We thus drop from our analysis the observations with the lowest and highest values for every outcome variable and for the treated and the controls separately. This is why the sample sizes of the treated and controls appear as being at most 97 and 1,203 in Table 1. We apply the same "trimming" rule when calculating the matching estimators. Using different "trimming" rules, we obtain similar results which are available upon request.

features. They are simple to use, they implement a correction for potential biases generated by non-exact matching on (mostly) continuous variables, and they have well defined asymptotic variance.⁸ Most importantly, they are well suited and have desirable properties, compared for example to matching on the propensity score, 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 (i.e., probability of treatment) model with the usual logit or probit method can generate undesirable results.

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, 2007) 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 .⁹ 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, 2007) 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

⁸Being 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.

⁹In addition to the CIA, we need to satisfy an overlap condition and other regularity conditions. See Abadie and Imbens (2006, 2007) for details.

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 AI's 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 $\mathcal{J}_M(i) = \{j_i(1), \dots, j_M(i)\}$ 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, $K_M(i) = \sum_{l=1}^M 1\{i \in \mathcal{J}_M(l)\}$, where $1\{\cdot\}$ is the indicator function. Now, 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, AI 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 AI does not rely on bootstrapping (contrary to other matching methods).

Abadie and Imbens (2007) also 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 (2007) show that this bias-corrected matching estimator is $N^{1/2}$ -consistent and asymptotically normal. In this paper we estimate both the simple matching estimator and the bias-corrected matching estimator, and the latter is our preferred estimator. We carry out such estimations using the Stata command `nnmatch` which is discussed in details in Abadie et al. (2004).

3.2 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.¹⁰ For example, if firm XYZ appears in all the years from 1995 to 2002, then XYZ 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

¹⁰Because 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. This is why we call these firms “potential controls”.

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 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' (2007) simulation analysis shows to minimize the root of the mean squared error.¹¹

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 past 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 $t=-2$. As explained in the previous section, this potentially “contaminates” all the pre-treatment variables in $t=-1$ and $t=-2$; it may not satisfy the CIA condition that outcomes are not affected by the treatment because at least stock returns in each of these two pre-treatment years are mechanically 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.

3.3 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 covariates for the treated and the (potential) controls before and after the matching.

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

¹¹In our estimations we also tried alternative values of M , from one to four, and the results are similar and are available upon request.

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. More problematically, the average past three-year cumulative returns of the treated firms 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 for 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 0.22 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 not statistically significant. This evidence shows that the comparisons are mostly not being made between small capitalization and large capitalization companies.

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 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.

3.4 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 *differences-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. 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. Therefore, we focus on the *DID* estimator in the next two sections, even though in most cases there are no qualitative differences between the results using the outcome variables in levels or in differences.

4 Effects of Distress on CEO Compensation and CEO Turnover

In this section we investigate the effects of financial distress on CEO compensation and CEO turnover. 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 3 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 and the value realized from options exercised 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 *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.

4.1 Effects of Distress on CEO Compensation for All Treated Firms

Table 4 presents the treatment effects on CEO compensation for all the treated firms using the *DID* method and setting $t=-3$ as the base period. (For brevity, we do not report the *ATT* effects based on the level estimators; these results are available upon request.) Panels A and B contain the results from estimating a simple matching estimator and a bias-corrected matching estimator, respectively, with robust standard errors reported in parentheses. As expected, given the quality of the matching, the treatment effects identified by the simple matching estimators and the bias-corrected matching estimators do not differ much qualitatively. Still, because the bias-corrected estimator deals with biases caused by non-perfect matching on covariates, we prefer the treatment effects estimated by the bias-corrected matching estimation and concentrate our analysis on those results.

The first thing to notice in Table 4 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. These results bode well for the validity of the Conditional Independence Assumption.¹²

Table 4 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 (30% to 45%, see Table 3). 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 4 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 \$2.3 million immediately in the distress year ($t=0$), and continues to decline by \$5 million in the first year after distress ($t=+1$), followed by a reduction of \$2.7 million in the second year after distress ($t=+2$) and by a smaller, and statistically insignificant, decrease of \$0.7 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 \$5.8 million in the first year after distress ($t=+1$), followed by a decline of \$3.5 million in the second year after distress ($t=+2$) and a statistically insignificant decline of \$1.3 million in the third year after financial distress ($t=+3$).

¹²As 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.

Table 4 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 4 also evidences the impact of financial distress on the practices of option grants in that the stock ownership through 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.

4.2 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 5 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 6 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 20 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 6 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 23 percentage points. That is, the CEO turnover rate of the distressed firms is 23 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 $t=+2$ (8%) and $t=+3$ (7%), although in a comparatively smaller magnitude than in $t=+1$.

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 (2006) 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 (2006) 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.

4.3 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 is comprised of all firms

that experience CEO turnover, and the second is comprised of firms that do not experience CEO turnover. We apply the bias-corrected matching estimator to each of the two subsamples.

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 in 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 7 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 4. 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 7, 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. However, a surprising result is that 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*. The treatment effects of distress on *BLKV* (and thus on *TDC1*) are as negative on the new CEOs as they are on incumbent CEOs.

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

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 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 dominant treatment effect on the stock-based compensation is not caused by the treatment effect on CEO turnover.

The results for the new CEOs of distressed firms that experience CEO turnover are very intriguing. In those firms, new CEOs are paid significantly less, in terms of stock compensation, compared both 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. That is, 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

4.4 Discussion

As shown above, the Black-Scholes value of *new* grants of stock options declines substantially for the treated firms in and after the year of financial distress. This is also true when we analyze

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.

the two subsamples of firms that do not experience CEO turnover in the period $[-3, +3]$ and that experience CEO turnover in the period $[-1, +1]$. This pattern of value changes is unlikely to be driven by changes in stock prices because the market capitalization of distressed firms, which changes virtually one-to-one with stock prices, exhibits little variation from year to year *after* $t=0$. Nor do we observe significant reductions in the number of shares of options granted to CEOs in each post-treatment year; in fact, we find evidence that the distressed firms appear to grant more stock options to their CEOs after falling into distress. In addition, this pattern of value changes cannot be explained by changes in stock volatility or option maturity that are known to affect the calculation of the Black-Scholes value; none of these two variables experience significant changes across the post-treatment years. Further, we follow the method used by Wharton Research Data Services (WRDS) to calibrate the Black-Scholes value of stock option grants and find that the change in the option grant value cannot be attributed to the method used in the ExecuComp database. For brevity, we do not report the results confirming the above assertions; they are available upon request.

The significant decline in stock-based compensation in distressed firms may happen for other reasons. For instance, CEOs may shun options in distressed firms because they expect a downward stock price path to persist.¹⁵ Alternatively, boards may use options as a reward for past performance, and if a firm becomes distressed, no award may be in order for the CEO. With either of these two explanations, we should expect that the number of shares of option grants is reduced significantly after the firm becomes distressed. In fact, Table 4 shows evidence to the contrary — relative to their solvent counterparts, the distressed companies tend to award more shares of stock options after falling into distress.

Given that the above various explanations seem to lack empirical support, we believe that the observed reductions in the Black-Scholes value of stock options, including the somewhat surprising decreases for newly hired CEOs, might be linked to decreases in managerial power among the financial distressed firms. We investigate this issue in the next section.

¹⁵Kadan and Swinkels (2008) show theoretically that to incentivize managers in firms under financial distress, restricted stock grants dominate stock option grants. They also provide empirical evidence that higher bankruptcy risk is indeed associated with a lower use of stock options. Note that their model gives predictions on how financial distress affects the *composition* of equity-based pay, but not the level of that pay. In a sense, our results are consistent with the predictions of their model, because the reduction we observe in the value of stock-option-based compensation is much higher than the reduction in restricted-stock-based compensation, once a firm goes into distress.

5 Effects of Financial Distress on Managerial Influence and Opportunistic Timing of Option Grants

A key hypothesis of our study is that the occurrence of financial distress tightens an implicit “outrage” constraint that limits CEO compensation (Bebchuk and Fried, 2004). This weakens managerial power 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. In this section we assess this hypothesis by studying the effects of financial distress on two dimensions of managerial power: managerial influence over the board of directors and opportunistic timing of option grants.

5.1 Treatment Effects of Distress on Managerial Influence Over the Board

In this subsection we examine changes in managerial influence over the firm’s board of directors. Using the information available in the ExecuComp database we construct five measures for assessing the quality of corporate governance. These measures jointly characterize potential managerial influence over the board of directors and over the board’s compensation committee.¹⁶ The measures we consider 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*). 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 8 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

¹⁶There are two 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 firms in our sample and firms for which the indexes have been calculated is quite low, it is not feasible to use those indexes in our study. We face the same problem 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.

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 much 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 or sitting in the compensation committee. 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. Similarly, the proportion of treated firms in which at least one executive sits in the board's compensation committee decreases sharply from 0.15 before financial distress to below 0.05 after the occurrence of financial distress; in contrast, the same proportion for the potential controls remains relatively stable around 0.10 throughout the analysis window.

The estimates of the treatment effects confirm the patterns observed from the descriptive statistics. Using the same set of matching covariates as in the analysis of CEO compensation, we estimate the average treatment effects of financial distress on managerial influence over the board of directors. Table 9 reports the results based on the matching estimators both in levels (Panel A) and in differences (Panel B).

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 power, and the treatment effects on the five measures are close to zero. However, all five measures show significant reductions and their treatment effects are all (mostly) significantly negative in the post-treatment years. 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. 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 in the post-treatment years. That is, less executives serve in the board or in the compensation committee or hold the chairmanship in the financially distressed firms after the firms fall into distress.

In summary, both the descriptive statistics and the matching estimators show clear evidence that financial distress causes firms (or boards) to reign in the CEO's and other executives' power. In particular, the significant decrease in the proportion of firms whose executives serve in the compensation committee, and thus can possibly affect their own pay, is consistent with the hypothesis that financial distress leads to a tightening of the outrage constraint.

5.2 Treatment 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. We follow Bebchuk, Grinstein and Peyer (2009) 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.¹⁷ 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 in 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

¹⁷Focusing 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 4. 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.

of the SOX regulation, we focus on the pre-SOX period.¹⁸

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 analyze periods $t=+2$ and $t=+3$ 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 (2009) 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.¹⁹ 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

¹⁸We find similar pattern of lucky grants if we do not exclude the post-SOX period from our sample. The results are available upon request.

¹⁹Note that, as explained in footnote 17, 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 $t=0$.

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 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 changes in CEO power and compensation that arise when firms go through financial distress. We use a matching estimator to find suitable comparable firms to those in financial distress and to estimate the *causal effects* of financial distress on CEO compensation, managerial influence over the board, and opportunistic timing of option grants. We document that, relative to those in control firms, the CEOs of distressed firms experience significant reductions in total compensation; the bulk of this reduction derives from the decline in value of new grants of stock options. These results hold not only for incumbent CEOs but also, surprisingly, for newly hired CEOs. Financial distress has important consequences on corporate governance, decreasing managerial influence over the board. We find that, among distressed firms, there is a significant decrease in the proportion of CEOs holding the chairmanship, and in the frequency of executives serving as directors or in the compensation committee of the board. We also show that periods of financial distress are associated with a decrease in opportunistic timing behavior of stock option awards.

Although we do not have enough data to establish *directly* that the changes in compensation are *caused* by the changes in managerial influence and in opportunistic option timing, the results are suggestive of such a link. It is well known that the Black-Scholes value of a stock option decreases with respect to the grant price of the option, holding everything else fixed. Thus, the observed pattern of decreases in lucky grants for the treated firms after financial distress seems to be consistent with the declines in the Black-Scholes value of stock option grants for those firms. Moreover, both the decrease in managerial influence over the board and the decrease in the awarding of lucky grants to CEOs can explain why even the newly hired CEOs of distressed firms receive lower stock-based compensation than their predecessors and than the newly hired CEOs of non-distressed firms.

Our findings are subject to caveats related to the nature of the data we rely on, but represent three separate pieces of evidence which point in the same direction. The key insight of our study is that, *a priori*, the occurrence of financial distress weakens managerial power due to a greater scrutiny from stake-holders. The hypothesis that managerial influence diminishes after financial distress is corroborated by our findings. The simultaneous declines in CEOs' stock-related pay

(both for incumbents and newly hired CEOs) and in opportunistic timing of option grants lend support to our interpretation that managerial power affects CEO pay. Taken together, these pieces of evidence are suggestive of a link between managerial power and executive compensation.

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. Empirically it is very difficult to disentangle completely the managerial power view from the incentive-pay view, and we believe that the pay-setting process probably includes elements associated with both optimal contracting and rent extraction. Thus, incorporating managerial rent-extraction motives more explicitly into theoretical models might improve our understanding of the determinants of executive compensation.

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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 or compensation variables 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 and compensation variables are summarized in Tables 2 and 3 respectively.

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), and total direct compensation (TDC1). 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' (2006, 2007) 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	-0.22**	0.08
Dummy Large Market Cap (70th percentile)	0.56	0.50	0.63	0.48	-0.08	0.05	-0.05	0.03
Total Liabilities/Total Assets	0.48	0.24	0.49	0.19	0.00	0.02	0.00	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	0.41	0.33
Number of Observations	99		1,205		-		396	

Table 3. 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 4. Average Treatment Effect for the Treated (ATT) of Financial Distress on CEO Compensation: Difference-in-Difference Estimates

This table reports the average treatment effect for the treated (ATT) of financial distress on CEO compensation, using Abadie and Imbens' (2006, 2007) 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. Panel A and Panel B present the simple matching estimator results and the bias-corrected matching estimator results, respectively. Robust standard errors are reported in parentheses. *, ** and *** denote significance at 10%, 5% and 1%, respectively.

Panel A. Simple Matching Estimator

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)	-0.3* (0.2)	0.2 (1.0)	2.9* (1.6)	3.4** (1.6)	0.0 (0.0)
-1	0.0 (0.0)	-0.3*** (0.1)	-0.3*** (0.1)	1.1 (1.7)	-0.2** (0.1)	0.6 (1.8)	2.2 (1.9)	1.6 (1.6)	0.0 (0.0)
0	-0.1*** (0.0)	-0.5*** (0.1)	-0.5*** (0.1)	-2.3*** (0.4)	-0.3*** (0.1)	-3.3*** (0.6)	-0.4 (1.2)	-1.7* (1.0)	0.0 (0.0)
+1	-0.1*** (0.0)	-0.4*** (0.1)	-0.4*** (0.1)	-4.2*** (0.8)	-0.1 (0.1)	-5.3*** (1.0)	-0.7 (1.2)	-2.0** (0.9)	0.1* (0.0)
+2	-0.1*** (0.0)	-0.3*** (0.1)	-0.4*** (0.1)	-3.3*** (0.6)	-0.2* (0.1)	-4.0*** (0.7)	-0.1 (1.2)	-1.1 (1.3)	0.0 (0.0)
+3	-0.1*** (0.0)	-0.2*** (0.1)	-0.3*** (0.1)	-1.3* (0.7)	-0.5** (0.2)	-2.1** (0.9)	-1.3 (1.3)	-2.8* (1.4)	0.1** (0.0)

Panel B. Bias-Corrected Matching Estimator

Period	Salary	Bonus	TCC	BLKV	RSTGRNT	TDC1	EXER	TDC2	OPGRNT
-2	0.0 (0.0)	-0.1 (0.1)	-0.1 (0.1)	0.5 (0.8)	-0.2 (0.2)	0.4 (1.0)	2.9* (1.6)	3.1* (1.6)	0.0 (0.0)
-1	0.0 (0.0)	-0.3*** (0.1)	-0.3*** (0.1)	1.0 (1.7)	-0.2** (0.1)	0.6 (1.8)	1.2 (1.9)	0.6 (1.6)	0.0 (0.0)
0	-0.1*** (0.0)	-0.5*** (0.1)	-0.5*** (0.1)	-2.3*** (0.4)	-0.2** (0.1)	-3.2*** (0.6)	-1.6 (1.2)	-2.6** (1.0)	0.0 (0.0)
+1	-0.1*** (0.0)	-0.4*** (0.1)	-0.4*** (0.1)	-5.0*** (0.8)	-0.1 (0.1)	-5.8*** (1.0)	-2.0* (1.2)	-3.1*** (0.9)	0.1** (0.0)
+2	-0.1*** (0.0)	-0.3*** (0.1)	-0.4*** (0.1)	-2.7*** (0.6)	-0.2* (0.1)	-3.5*** (0.7)	-0.5 (1.2)	-1.8 (1.3)	0.0 (0.0)
+3	-0.1*** (0.0)	-0.2*** (0.1)	-0.3*** (0.1)	-0.7 (0.7)	-0.5** (0.2)	-1.3 (0.9)	-2.4* (1.3)	-3.6** (1.4)	0.1** (0.0)

Table 5. 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 6. 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' (2006, 2007) 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	0.20***	0.26***	0.29***	0.23***	0.22***	0.22***
(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	0.06	-0.02	0.23***	0.08**	0.07*
(S.E.)	(0.03)	(0.04)	(0.04)	(0.05)	(0.04)	(0.04)

Table 7. 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' (2006, 2007) 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)	-0.2** (0.1)	-0.1 (0.1)	0.1 (0.8)	0.0 (0.0)	0.0 (1.1)	0.5 (0.6)	1.1 (0.8)	0.2*** (0.1)
-1	0.0 (0.0)	-0.4*** (0.1)	-0.3*** (0.1)	-1.8*** (0.5)	0.0 (0.0)	-2.0*** (0.7)	-0.3 (0.3)	-0.7 (0.4)	0.3*** (0.1)
0	-0.1** (0.0)	-0.4*** (0.1)	-0.4*** (0.1)	-1.6*** (0.6)	-0.2** (0.1)	-1.9*** (0.7)	-1.0 (1.9)	-2.9 (1.9)	0.5*** (0.1)
+1	-0.1*** (0.0)	-0.4*** (0.1)	-0.5*** (0.1)	-3.0*** (0.5)	-0.1 (0.1)	-3.2*** (0.7)	-3.4*** (0.9)	-3.8*** (1.0)	0.1* (0.1)
+2	-0.1*** (0.0)	-0.3** (0.1)	-0.4*** (0.1)	-2.5*** (0.8)	-0.1 (0.1)	-2.7*** (1.0)	-2.0* (1.2)	-1.6 (1.2)	-0.1 (0.1)
+3	-0.1*** (0.0)	-0.2* (0.1)	-0.4** (0.1)	-1.1** (0.5)	-0.1 (0.2)	-0.8 (0.8)	-0.1 (1.8)	0.3 (1.9)	0.2** (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)	-0.2*** (0.0)	-0.2*** (0.1)	-0.9 (0.8)	0.0 (0.1)	-1.3 (0.8)	-0.1 (0.7)	-0.3 (0.7)	0.0 (0.1)
-1	0.0 (0.0)	-0.2*** (0.1)	-0.2* (0.1)	5.1*** (2.0)	0.0 (0.1)	4.9* (2.8)	-0.2 (0.4)	-0.3 (0.6)	0.1 (0.1)
0	-0.1** (0.0)	-0.3*** (0.1)	-0.4*** (0.1)	-1.0** (0.5)	0.0 (0.1)	-1.5** (0.6)	-0.1 (0.6)	-0.7 (0.6)	0.3*** (0.1)
+1	0.0 (0.0)	-0.1 (0.1)	-0.1 (0.1)	-2.6*** (1.0)	-0.2 (0.2)	-2.9*** (1.1)	0.2 (0.8)	0.1 (0.8)	-0.1 (0.1)
+2	0.0 (0.0)	-0.2* (0.1)	-0.2** (0.1)	-1.7*** (0.6)	0.1 (0.1)	-1.2** (0.5)	0.3 (0.7)	0.4 (0.8)	0.0 (0.1)
+3	0.0 (0.0)	-0.2 (0.1)	-0.2 (0.1)	-1.5** (0.6)	0.1 (0.1)	-2.3*** (0.6)	-0.7 (0.7)	-1.2 (0.8)	-0.1* (0.1)

Table 8. 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 9. 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' (2006, 2007) 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	0.04 (0.11)	0.01 (0.07)	-0.01 (0.06)	0.00 (0.02)	0.00 (0.04)
-1	0.05 (0.10)	0.03 (0.07)	0.01 (0.06)	0.02 (0.02)	0.00 (0.04)
0	0.00 (0.11)	-0.02 (0.05)	-0.06 (0.06)	0.02 (0.02)	-0.01 (0.03)
+1	-0.30*** (0.10)	-0.05 (0.04)	-0.16*** (0.06)	-0.08** (0.03)	-0.05* (0.02)
+2	-0.28*** (0.09)	-0.11*** (0.03)	-0.14** (0.06)	-0.05 (0.03)	-0.07*** (0.02)
+3	-0.22** (0.09)	-0.05** (0.02)	-0.16*** (0.06)	-0.05 (0.03)	-0.03* (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)	-0.04 (0.03)	0.01 (0.04)	-0.01 (0.01)	-0.03 (0.02)
-1	0.03 (0.10)	-0.04 (0.04)	0.04 (0.06)	0.01 (0.02)	-0.03 (0.03)
0	-0.01 (0.11)	-0.04 (0.06)	-0.03 (0.06)	0.01 (0.02)	-0.05 (0.04)
+1	-0.29** (0.11)	-0.08 (0.07)	-0.12* (0.07)	-0.08** (0.03)	-0.08* (0.04)
+2	-0.26** (0.12)	-0.12* (0.08)	-0.11 (0.07)	-0.07* (0.03)	-0.10** (0.04)
+3	-0.17 (0.13)	-0.05 (0.06)	-0.15** (0.07)	-0.07** (0.04)	-0.05 (0.04)

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

This table reports the descriptive statistics for the proportion of luck 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 (2009) 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					
	Treated			Potential Controls			Treated			Potential Controls		
	Mean	S.D.	N	Mean	S.D.	N	Mean	S.D.	N	Mean	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' (2006, 2007) bias-corrected matching estimator. We follow Bebchuk, Grinstein and Peyer (2009) 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	0.02 (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	-0.09** (0.04)	-0.09** (0.04)
+2	-0.04 (0.04)	-0.05 (0.04)
+3	-0.04 (0.04)	-0.06 (0.04)

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

Period	Proportion of lucky grants	
	Raw	Net
-2	0.08 (0.06)	0.06 (0.06)
-1	0.00 (0.06)	-0.01 (0.06)
0	-0.03 (0.05)	-0.04 (0.05)
+1	-0.03 (0.06)	-0.05 (0.06)
+2	0.03 (0.06)	0.02 (0.05)
+3	-0.05 (0.06)	-0.07 (0.06)