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Abstract

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JEL Classification: G33, G34, J33, J44, M50
Keywords: Corporate distress, managerial influence, CEO compensation, CEO turnover, bias-corrected matching estimator

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Abstract

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1 Introduction

As much anecdotal evidence shows, corporate distress inflicts substantial costs on firms and brings about all-around changes in corporate structure and decision making. Addressing poorly designed incentives and ineffective monitoring of managers is key to the revival of distressed firms. How these changes evolve and interact with each other during the distress process, however, is relatively understudied (see, e.g., the surveys of Hotchkiss, et al. (2008) and Senbet and Wang (2012)). Earlier works on this front include Gilson (1989, 1990) and Gilson and Vetsuypens (1993) that are based on data from the 1980s. Given the dramatic evolution in governance mechanisms, capital markets, as well as regulatory reforms in the past few decades, it is of natural interest to revisit this topic for the recent era.[1]

This paper examines the dynamics of two governance constructs, namely, managerial influence over the board of directors and CEO compensation, in the course of corporate distress from 1992 to 2019. As Figure 1 of this paper shows, there is a strong time trend in governance variables: governance has generally improved over time, especially since the early 2000s. For example, the average value of *Corporate Governance Score*, the standardized governance index as defined in Section 2, starts at -0.62 in 1995 and steadily rises to -0.05 in 2000, 0.10 in 2005, and 0.30 in 2010, and climbs further to 0.46 in 2016. The existence of a time trend in governance can confound a study of the effects of corporate distress. If the trend is not appropriately controlled for, the estimated effects of corporate distress on governance may largely reflect the effects of the time trend, leading to biased inference. This may not be a severe concern for a study of a short time span. However, it is a particularly serious concern in a study over a long horizon like ours. Indeed, Table 3 and Section 4.2.1 of the paper showcase this point.

A matching estimator helps address this concern. A generic matching estimator comprises two inherent empirical procedures: first constructing a suitable control group to the group perceived to be affected by the event (the so-called "treated" group) based on certain observed pre-event characteristics, then calculating the average differences in outcome variables of interest between

[1]Several recent major regulatory changes related to executive compensation in the U.S. are: the Sarbanes–Oxley Act (SOX) of 2002 that overhauled corporate governance, the Securities and Exchange Commission's (SEC) 2006 mandate to increase disclosure of compensation, the say-on-pay clause in the Dodd–Frank Act of 2010, and the SEC's 2013 mandate to disclose the ratio of CEO pay to median employee pay.

the treated group and the control group as the estimated effects of the event. Therefore, if an outcome variable has a time trend, an adequate control group identified in the first procedure can pick up the trend; by taking the difference between the treated group and the control group in the second procedure, a matching estimator filters out the effect of the trend and properly estimates the event's effect on the outcome variable. Besides controlling for the effects of time trends, a matching estimator also helps address the issue that firms going into distress may differ fundamentally from non-distressed firms. In this paper, we employ Abadie and Imbens' (2011) bias-corrected matching estimator to identify a suitable control group from non-distressed firms and conduct matching estimations of the effects of distress on governance. Our study thus represents a significant improvement in methodology over previous studies on this topic.[2]

We define corporate distress as the event in which a company either goes through chapter 11 bankruptcy or shows three consecutive years of extremely poor performance along with high likelihood of bankruptcy as measured by stock returns and various accounting ratios. We find that corporate distress has significant effects on managerial influence over the board, CEO compensation, and CEO turnover. Relative to non-distressed firms that are comparable in a variety of economic, financial, and governance measures, firms under corporate distress reduce managerial appointments to the board, intensify managerial incentive alignment, cut CEO pay, and increase CEO turnover. The effects of corporate distress vary across section and over time. Initially poorly-governed firms experience more significant reductions in managerial power and CEO compensation than otherwise upon and after the advent of distress. We also find that managerial incentive alignment strengthens after firms fall into distress, particularly in the pre-2004 subperiod. Moreover, the changes in managerial influence and the changes in CEO compensation do not occur in lockstep during the process of corporate distress: firms reduce CEO compensation before corporate distress but adjust managerial board appointments only after falling into distress.

[2]Section 3 and Appendix give details of this bias-corrected matching estimator. Using an appropriately constructed control group in executive compensation studies is of great importance that goes beyond the case of corporate distress. For example, Albuquerque (2009) shows evidence on the use of relative performance evaluation in CEO compensation if peers are composed of similar industry-size firms, affirming that a misspecified peer group is responsible for prior mixed evidence. Conyon, Core, and Guay (2011) find that adjusting for risk, the level of US CEO pay is not larger than that of UK CEOs and the apparent higher US CEO pay is cut by half if compared to a matched sample of non-UK European CEOs. Faulkender and Yang (2010) and Bizjak, Lemmon, and Nguyen (2011) find evidence that companies strategically construct compensation peer groups to inflate CEO pay and justify their CEO compensation. Albuquerque, DeFranco, and Verdi (2013) counter that the choice of highly paid peers represents a reward for unobserved CEO talent in CEO compensation.

We conduct analyses to gain insights into what forces are likely responsible for the dynamics of CEO compensation during the process of corporate distress. Based on model parameters estimated for non-distressed firms, we decompose CEO compensation for distressed firms into two parts: one related to firm performance (expected compensation) and the other not (unexpected compensation). Assuming that performance is related to compensation in the same way for distressed and non-distressed firms, the effects of distress on unexpected compensation are much weaker than those on total compensation or expected compensation. This result suggests that a significant portion of the effects of distress on compensation are related to changes in firm performance.

Our paper adds to the under-studied empirical literature on executive compensation for distressed firms. Gilson and Vetsuypens (1993) and Henderson (2007) examine executive compensation in distressed firms during 1981-1987 and 1992-2003, respectively. Recent works in this area include Eckbo, Thorburn, and Wang (2016) and Goyal and Wang (2017), who respectively investigate CEO compensation changes and the use of key employee retention and incentive plans in bankrupt firms over the 1996-2007 period. These four studies compare executive pay before and after distress among distressed firms only. Our study extends the sample period to 2019, and more importantly, contrasts the changes in executive pay before and after distress for the treated firms with the changes for an appropriately chosen control group. Our results in Table 3 clearly show that, compared to the matching estimator that selects and enlists an adequate control group to filter out the effects of the time trend, focusing only on the distressed firms tends to considerably overstate the effects of corporate distress.[3]

Our study also contributes to the literature on the relation between corporate governance, especially the role of the board in monitoring/disciplining CEOs, and firm performance. Prior papers have largely concentrated on the effects of the board-centric governance structure on firm performance without paying particular attention to the case of distressed firms (e.g., Coughlan and Schmidt (1985), Hermalin and Weisbach (1991), Core, Holthausen, and Larcker (1999), Gompers, Ishii, and Metrick (2003), and Bebchuk, Cohen, and Ferrell (2009)). We investigate the other

[3]We thank the referee for suggesting this analysis. The importance of controlling for the time trend is also highlighted by the difference in the unreported findings on outside replacement CEOs' compensation. We find that outside replacement CEOs in the distressed firms are paid significantly higher than inside replacement CEOs, a result similar to the one reported by Gilson and Vetsuypens (1993). However, they are actually not paid significantly higher, and oftentimes lower instead, relative to the outside replacement CEOs in non-distressed firms.

direction of this relation, i.e., the impact of firm performance on corporate governance, in the context of corporate distress. In particular, declines in CEO compensation occur before changes in board compositions, suggesting that the reshuffling of the board occurs in a progressive way and is most likely not the driver of compensation changes in distressed firms.

Our paper is indirectly related to the ongoing debate on the forces underlying executive compensation. We find that a significant portion of CEO compensation is tied to firm performance over the course of corporate distress. The results are largely consistent with the "shareholder value" view of CEO pay (see, e.g., the surveys by Edmans and Gabaix (2009, 2016), as well as the references therein). Our analysis shows much weaker evidence that corporate distress has impacts on the portion of compensation unrelated to firm performance, which otherwise could lend support to the "rent extraction" view of CEO pay (e.g., Bertrand and Mullainathan (2001) and Bebchuk and Fried (2004)). It is thus necessary to exercise caution in interpreting our results, given that the decomposition of compensation is based on a standard, albeit quite simplistic, parametric model to characterize the relation between pay and performance.

Two caveats are in order. First, it is very difficult to gather data, especially the CEO compensation data, once a firm goes into distress. Most distressed companies stop filing regular proxy and 10k statements, so 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.[4] Our main findings stand out statistically significant even with these small samples. 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 ones in the universe of distressed companies, potentially creating a survivorship bias. Given that non-surviving firms usually experience larger declines in managerial power than surviving firms, our analysis likely provides conservative estimates of the effects of corporate distress.

The paper proceeds as follows. Section 2 describes the data, variables, and the sample for

[4]Gilson and Vetsuypens (1993), and Henderson (2007) use samples of only 77 and 76 firms, respectively. Bernstein (2006), in a study of CEO turnover in distressed firms, compares 79 bankrupt firms with 1,288 firms "suffering from poor financial circumstances". In contrast, we study 297 distressed firms and select their comparison group from a pool of 2,259 non-distressed firms.

our study. Section 3 discusses the empirical strategy. Section 4 presents the effects of corporate distress on managerial influence over the board, CEO compensation, and CEO turnover. Section 5 concludes.

2 Data and Sample Construction

Data for this study come from several sources. The Standard and Poor's (S&P) ExecuComp 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 information about these executives' positions in the board: serving as board directors, sitting in the board's compensation committee, and holding the dual titles of CEO and board chairperson. Firms' annual accounting data come 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 2018 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 corporate distress with which we classify firms into distressed firms and non-distressed firms. The 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 is identified based on a combination of the past three-year cumulative stock returns and Ohlson's (1980) O-scores.[5] Specifically, at each year-end and from the universe of all Compustat

[5]To 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 fall into distress and go bankrupt.

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 distressed if the following two conditions are 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.[6]

The second set of key variables comprises three groups of outcome variables: managerial influence over the board of directors, CEO compensation, and CEO turnover. Managerial influence over the board pertains to their affiliations with the board, that is, whether the CEO serves as the board chair (*CEO Duality*, equal to one if true and zero otherwise), whether at least one executive serves as a board director (*Inside Directors*, equal to one if true and zero otherwise), and whether at least one executive serves in the compensation committee (*Executives in Compensation Committee*, equal to one if true and zero otherwise). In similar spirits to Bertrand and Mullainathan (2001), we construct the *Corporate Governance Score* as an average of the following three standardized variables: *CEO Duality*, number of executives serving as board directors, and number of executives serving in the compensation committee, each demeaned and divided by its standard deviation for standardization. We multiply *Corporate Governance Score* by minus one so that a higher *Corporate Governance Score* corresponds to better governance or, equivalently, weaker managerial influence over the board.[7] *Corporate Governance Score* measures the influence managers exercise on the board via their board appointments. It also implicitly measures board composition changes because a change in executive affiliations with the board translates into a reshuffling of the board.

We define two levels of CEO compensation. *Total Current Compensation* is the sum of salary and bonus. *Total Direct Compensation* is the sum of *Total Current Compensation*, the value of restricted stock grants, the Black-Scholes value of stock option grants, other annual short-term compensation, payouts from long-term incentive plans, and all other long-term compensation. Most

[6]We also identify 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.

[7]There 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. It is not feasible to use those indexes in our study because a large proportion of our sample firms are not covered by these two indices.

of our discussions focus on *Total Direct Compensation* as it measures the total *estimated* pay the board has awarded the CEO that year, reflecting the board's annual decision. In addition, we define a dummy variable, *CEOTURN*, to indicate the CEO turnover status — it equals one if a CEO replacement occurs within that year and zero otherwise.

The third set of variables consists of the covariates we use to directly match distressed firms with non-distressed firms. To identify the impact of corporate distress we resort to the Abadie and Imbens' (2011) bias-corrected matching estimator. The matching covariates include various measures of a firm's fundamentals and financial health, measures of CEO compensation, measures of managerial influence over the board, industry dummies, and year dummies. Except for the year dummies all the other matching covariates are evaluated three years prior to the onset of corporate distress. We match on those variables so that distressed firms and control firms are similar in t=-3 in the following aspects: industry affiliation, likelihood of falling into distress, CEO compensation structure, board composition, and board monitoring of managers. See Table 2 for details of the matching covariates.

2.2 Sample Construction

We choose an analysis window spanning from two years before distress through three years after distress, a total of six years, to discern the evolution of outcome variables during the course of corporate distress. We normalize the year of distress occurrence as t=0. Accordingly, we have t<0 and t>0 respectively for the pre- and post-distress periods. We conduct matching based on the matching covariates three years prior to corporate distress (i.e, t=–3). Our data cover the period from 1992 to 2019. As a result, the years for t=0 in our analysis are from 1995 to 2016.

The treatment in our study is defined as the "event" that a firm falls into corporate distress. Because a firm can become distressed more than once over time, we restrict the treatment to be the first time that a firm becomes distressed. In very few cases we include a second spell in distress for the same firm if and only if at least seven years have passed since the firm encountered the first episode of corporate distress.

We construct a control group to estimate the effect of financial distress. The control group consists of those non-distressed firms that are statistically similar to the treated firms in

observable characteristics before the episode of corporate distress. This allows us to determine the counterfactual, i.e., what the outcome variables of the distressed firms would have been if they had not suffered corporate distress. In order to maintain a clean potential pool of controls for the treated group, we exclude from the pool any solvent firm which has ever been distressed before.

Further, we apply the following two criteria to select firms into our analysis sample: 1) Firms do not have missing information for either the matching covariates in the matching year (i.e, three years before the treatment); and 2) firms have at most one missing variable in all compensation variables in the pre-treatment window (i.e., from two years before treatment to the treatment year) and at most one missing variable in all compensation variables in the post-treatment window (i.e., from one year after the treatment to three years after the treatment). As a result, our analysis sample contains 297 firms that were ever in corporate distress and 24,645 firm-year observations for 2,259 firms that have never been in corporate distress during the 1995-2016 period.[8]

Table 1 breaks down the distribution of the analysis sample across years. To avoid introducing serial correlation in the estimation, we assign each potential control firm to only one particular year. We defer the detailed explanation of the year assignments to Section 3.3.

3 Empirical Strategy

We start this section with a discussion of the motivation for our empirical strategy. We then explain in details the empirical strategy adopted.

3.1 Motivation: Time Trends in Outcome Variables

In a long-run study like this one, if an outcome variable exhibits a time trend and if the trend is not properly controlled for in the analysis, the ensuing inference about the effect of corporate distress on this outcome variable may be distorted. Such results may mostly capture the time trend rather

[8]Among the 297 distressed firms, 47 of them ever filed for Chapter 11 protection (i.e., covered in BRD) and the other 250 firms did not. The small number of BRD observations in our sample is due to that the majority of the BRD firms are not covered in the ExecuComp database. Moreover, among those BRD firms covered in ExecuComp, quite a few do not satisfy the two selection criteria to be included in our analysis sample. We also separately replicate our matching estimations over the two subgroups of distressed firms, i.e., those filed for Chapter 11 protections and those had poor performance but did not file for the protection. Not surprisingly, due to the small sample size, the matching estimates for the subgroup of Chapter 11 firms largely lack statistical significance. Nevertheless, the results for the two subgroups of firms are qualitatively similar to the main results for the whole sample as presented in the paper. To conserve space, detailed results for the two subgroups are available upon request.

than the effect of corporate distress. We thus investigate the evolution of each outcome variable over time. We conduct this exercise using our analysis sample that includes both the distressed firms and the non-distressed firms. Figure 1 graphs the yearly averages of each outcome variable for either group of firms during the 1995-2016 period.

We first examine the set of non-distressed firms, which serves to characterize time trends, if any. Figure 1 shows unambiguously that there are time trends in the outcome variables. First, as time goes by, managerial influence declines consistently and corporate governance keeps improving; both changes are more pronounced after 2002-2004. This pattern is consistent with the ample evidence that the SOX legislation in 2002, going into full effect by 2004, improves corporate governance and curtails managerial power. Second, both levels of CEO compensation are on the rise in the 1990s, but the trend reverses around 2003 and 2004 and continues a declining trend afterwards. This pattern mirrors Kaplan and Minton's (2012) finding that average CEO compensation awarded by the board declines after 2002, subsequent to a sharp increase in the 1990s. Third, CEO turnover rates fluctuate over time but generally remain around 10% a year throughout the entire sample period.[9]

We then inspect the set of distressed firms. They largely show the same patterns as the non-distressed firms in almost every outcome variable. Compared to the non-distressed firms, distressed firms appear to exhibit slightly steeper decreases in managerial influence measures and a sharper increase in the corporate governance score over time. The two layers of CEO compensation for distressed firms evolve in relatively lower levels than those for non-distressed firms, but the dynamics for distressed firms track those of non-distressed firms. Not surprisingly, the CEO turnover rates in the distressed firms are considerably higher than the CEO turnover rates in non-distressed firms in most years.

The evidence of strong time trends in governance variables calls for identifying an adequate control group. Otherwise, if we focus on the set of distressed firms only, the estimated effects of corporate distress may largely reflect the effects of this time trend, thereby leading to biased inference. For example, we observe an overall trend that the proportion of firms with executives

[9]Kaplan and Minton (2012) estimate that board-driven CEO turnover rates in Fortune 500 companies are 10.9% from 1992 to 1999 and 12.4% from 2000 to 2007. Figure 14 in Kaplan (2012) shows similar dynamics in the board-driven CEO turnover during 1992 and 2010.

serving in the compensation committee declines over time for all firms, regardless of falling into corporate distress. A before-and-after analysis, which looks at the outcome variables before and after the distress occurrence over the distressed firms only, would attribute the decline in this outcome variable entirely to corporate distress, thereby overestimating the effect of corporate distress. We present relevant results in greater details in Table 3 and defer the discussion to Section 4.2.1.

3.2 Empirical Strategy: Matching Estimator

Given the apparent time trends in the outcome variables, we use a matching estimator to construct an appropriate control group for the distressed firms. The control group serves the purpose of filtering out the confounding effects of time trends. Moreover, the control group helps address the issue that non-distressed firms may be fundamentally different from distressed firms. With the chosen control group, we then conduct matching estimations to obtain proper estimates of the effects of corporate distress. The basic intuition of a matching estimator 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 distress. If an outcome variable of the treated firms has a time trend, then the same outcome variable of the carefully chosen counterfactual also exhibits the trend. Therefore, taking a difference in the outcome variable between the treated and the counterfactual removes the effect of the trend from the matching estimate of the treatment effect.[10] Our approach is thus distinct from early works such as Gilson (1989), Gilson and Vetsuypens (1993), Eckbo, Thorburn, and Wang (2016), and Goyal and Wang (2017), all of which are based only on the set of distressed firms.

Specifically, we use Abadie and Imbens' (2011) bias-corrected matching estimator to assess the *Average Treatment Effect for the Treated (ATT)* of corporate distress on managerial influence and CEO compensation. As shown in Abadie and Imbens (2006, 2008), this matching estimator allows matching directly on both continuous and discrete covariates and has several attractive features: it is simple to use, flexible enough to implement a correction for potential biases generated by non-

[10] In a standard regression framework, all the firms that do not suffer distress form the control group; however, as shown in the program evaluation literature, using such a control group could lead to biased inference. An alternative way is to only select as control firms those non-distressed firms that are statistically similar to the treated firms in observable characteristics *before* the advent of corporate distress.

exact matching on (mostly) continuous variables, and possesses a well defined asymptotic variance.[11] More importantly, compared to other matching estimators such as propensity score matching, this estimator is better suited and has more desirable properties in a case like the one under study — when the number of treated observations is small relative to the number of control observations, estimating a propensity score model with the usual logit or probit method can generate undesirable results. Appendix gives further details of this matching estimator.

Even after the matching on observed covariates, there may exist unobserved heterogeneity that distinguishes the treated from the actual controls. Per Smith and Todd (2005), we apply a *difference-in-differences* (DID) type of matching estimator by subtracting from each outcome variable its corresponding value in t=–3 to remove time-invariant unobserved heterogeneity.

3.3 Determining Event Years for (Potential) Controls

One practical challenge in a matching estimator is to determine event years for the control firms as the timing of events is only available for the treated firms, i.e., the firms that ever went into corporate distress, but is not available for the potential controls, i.e., the firms that never did so in our analysis period.

We adopt a strategy, essentially applying the matching estimator twice, to assign each of these non-distressed firms to the year in which they could be the best possible match for a treated firm in that year. Specifically, in the first round, for each year we take all the 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 particular year in which that treated firm went into distress becomes the best possible year which we can assign to the non-distressed firm. After the assignment of "distress" year in this way, we use the non-distressed firm as a potential control in that year only. These non-distressed firms are merely potential controls because not all of them are used in the subsequent actual matching estimation. In the next round,

[11] The analytical solution of the asymptotic variance saves much computing time that would otherwise arise by bootstrapping estimation. Moreover, Abadie and Imbens (2008) show that bootstrapping fails for matching estimators.

given that we have assigned each non-distressed firm to one and only one year, we pool together observations from all the years and use the matching estimator again to find the best M matches for each treated firm in each year. Per Abadie and Imbens' (2011) simulation analysis, we set M to four.[12]

Note also that, without further restrictions, time t=0 refers to different calendar years 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 going into distress in a "bad" year for the overall market. We thus include the treatment years as matching covariates in the second round of matching to ensure that the treated firms and the controls are compared in the same calendar period.

4 Matching Estimation Results: Effects of Corporate Distress

In this section, we apply Abadie and Imbens' (2011) bias-corrected matching estimator to assess the effect of corporate distress on outcome variables of interest. We first discuss the matching quality to ensure that we construct an appropriate control group. We then analyze in detail the effects of corporate distress on managerial influence over the board, CEO compensation, and CEO turnover.

4.1 Assessment of Matching Quality

It is standard in the causal inference literature to show balancing in the covariates used for matching, which is also a useful way to justify that the matching works (Rubin, 2008; Imbens and Wooldridge, 2009). Table 2, Panel A presents the summary statistics of the matching covariates for the treated and the (potential) controls before and after the matching, whereas matching is done at t=–3.[13]

[12]The choice of the number of matches involves the standard bias-variance tradeoff (Imbens and Rubin, 2015). In general, lower values of M help reduce potential bias associated with non-exact matching, and higher values of M help improve estimation precision. In this application, our results are not highly affected by using alternative values of M. We estimate our main results with M values varying from 1 to 6. The results are qualitatively similar and largely consistent across the different values of M. While as expected the estimation precision, thus statistical significance, is somewhat affected by the choice of $M = 1$, the overall story does not change. Also, the estimated standard errors tend to be minimized at $M = 4$, echoing Abadie and Imbens' (2011) simulation analysis. These additional results are not reported in the paper to conserve space and are available upon request.

[13]For compensation variables, we observe outliers that could potentially affect the results, so we drop the observations with the lowest and highest values for the treated and the controls separately. We obtain similar results with different "trimming" rules.

The first two columns in Panel A list the mean and standard deviation of the matching covariates for the 297 treated firms. Columns (3)-(4) show the same information for the pool of 2,259 potential control firms obtained from the first-round matching. Columns (5)-(6) present the average difference and standard error in each matching covariate between the treated group and the potential controls formed after the first-round matching. Although the matching reduces a great amount of heterogeneity between the treated firms and the potential controls, the two sets of firms still have considerable differences in various matching covariates such as industry affiliation, market capitalization, financial leverage, three-year cumulative stock return, relative positions in the O-score distribution and three-year-return distribution, total flow compensation, and executive directorships in compensation committees; quite a few year assignments are misaligned between the treated and the potential controls too.

Columns (7)-(8) of Table 2, Panel A report the means and standard errors of the within-match differences in the covariates between the treated firms and the actually used controls after we apply the second-round matching. It is clear that, once the matching procedure selects the best controls for each treated firm, the two groups are much more similar to each other. The matching succeeds on all the year dummies and three out of five industry dummies. Although the within-match differences in the other two industry dummies remain statistically significant, they are much smaller in magnitude than the respective raw differences.

The balancing of covariates is pretty good for the remaining variables too: although not as perfectly balanced as matching on the dummy variables, most of the differences in the remaining covariates become markedly smaller in magnitude and not statistically significant, even with reduced standard errors after the matching. Below we discuss only the within-match differences that remain to be statistically significant. Specifically, financial leverage, O-score, relative position in the stock-return distribution, and total current compensation all retain significant within-match differences. It is largely driven by their much smaller standard errors despite their similar average values to the raw differences. The cumulative three-year stock return shows the largest improvement, with the difference between the treated and the actual controls dropping sharply from a 11% gap to a 2% gap. The total flow compensation gap reduces by 57%, going from 2.10 million dollars to 0.90 million dollars.

As further evidence of the quality of the matching, we also report in Table 2, Panel B the summary information of some covariates that are not used by the matching procedure before and after the matching for the treated and the (potential) controls. Such non-matched covariates span various dimensions of firm fundamentals and market performance: alternative measures of firm size (total assets, total sales, and number of employees), growth (market-to-book asset ratio), profitability (profit margin, return on equity, return on assets), financial health (cashflow-to-asset ratio, book leverage ratio, Altman's (1968) z-score, credit ratings, investment-grade ratings) and stock performance (one-year stock return, one-year stock return volatility, and share turnover). As Panel B clearly shows, the raw differences in those non-matched covariates are quite large and almost all strongly significant, but after we apply the second-round matching, the gaps in these characteristics are all significantly reduced in size and most of the differences become statistically insignificant.

To sum up, the matching has decent quality overall and works well in balancing differences in observed characteristics, regardless of being used in the matching procedure, between the distressed firms and the comparable non-distressed firms. The fact that the level and composition of CEO compensation as well as the managerial board affiliations are matched suggests that the treated and the controls are similar not only in their characteristics and financial situations but also in their compensation policies and managerial power at t=–3. Moreover, we expect that the bias adjustment step of our matching estimator helps to eliminate most of the bias that may arise from non-exact matching.

4.2 Effects of Distress on Managerial Influence and CEO Compensation

4.2.1 Full-sample Results: With versus Without Matching

Table 3, Panel A reports the DID matching estimation results, with robust standard errors listed in parentheses. The sample period is from 1992 to 2019, with corporate distress occurring during 1995-2016.

In the pre-treatment years (i.e., t<0), the effects of corporate distress on *CEO Duality*, *Inside Directors*, *Executives in Compensation Committee*, and *Corporate Governance Score* are virtually nil; the effects on *Total Current Compensation* and *Total Direct Compensation* are highly significant

and negative in both t=–2 and t=–1. The results suggest that CEO compensation changes precede board composition changes even before firms fall into distress. The treatment effects on the outcome variables are mostly significant in the treatment year (i.e., t=0): 8% less of CEOs serve as the board chairs, 4% fewer of executives sit on the board's compensation committee; *Corporate Governance Score* increases in value; both *Total Current Compensation* and *Total Direct Compensation* shrink.

After firms fall into corporate distress (t>0), the treatment effects on outcome variables generally remain to be strong and significant. The estimates on measures of managerial affiliations with the board are all negative and quite a few are statistically significant, especially for *CEO Duality*. The corporate governance index gains in value significantly, and CEO compensation diminishes significantly. For example, in the year immediately after the distress (i.e, t=+1), the proportion of CEOs holding the board's chairmanship decreases by 11 percentage points, and the proportions of executives sitting in the board's compensation committee decrease by 4 percentage points; the corporate governance index value rises by 0.19; total cash compensation falls by 0.33 million dollars, and total estimated pay shrinks by 3.02 million dollars. In the three years after the onset of distress (i.e, t=+3), *CEO Duality* and *Executives in Compensation Committee* respectively decrease by 13 and 2 percentage points, *Corporate Governance Score* value increases by 0.13; *Total Current Compensation* and *Total Direct Compensation* reduce by 0.41 and 2.10 million dollars, respectively.[14]

Figure 1 features a clear time trend in the outcome variables over the full sample period. The time trend confounds the study and, if not appropriately controlled for, can lead to biased inference. For comparison, we also conduct estimations without matching, i.e., using the set of distressed firms only. Specifically, to be compatible with the matching estimation, for each event period we regress each outcome variable, differenced with respect to its corresponding values in t=–3, against the set of matching covariates in t=–3 as used in the matching estimation.

Table 3, Panel B presents the estimated margins of such regressions. Although similar in dynamics, almost all the estimated effects of corporate distress on the outcome variables in Panel

[14]We also use the logarithm of CEO compensation as outcome variables. With this log specification, our matching estimations give the difference-in-differences estimates of *percentage* changes in CEO compensation over time. Specifically, for t=-2, t=-1, t=0, t=1, t=2, and t=3, the estimated percentage changes in *Total Current Compensation* are respectively -10%, -7%, -20%, -16%, -20%, and -27%, and the estimated percentage changes in *Total Direct Compensation* are respectively -13%, -12%, -38%, -44%, -51%, and -42%. We thank the referee for suggesting this specification.

B are considerably larger in magnitude than the corresponding ones in Panel A, especially for the post-distress years. For example, the estimates on *CEO Duality* with matching are respectively -0.11, -0.10, and -0.13 in t=+1, t=+2, and t=+3; the corresponding estimates without matching are respectively -0.12, -0.14, and -0.21. The estimates on *Executives in Compensation Committee* with matching are respectively -0.04, -0.02, and -0.02 in t=+1, t=+2, and t=+3 while the corresponding estimates without matching are respectively -0.08, -0.07, and -0.06. Not surprisingly, the estimates on *Corporate Governance Score* without matching are significantly higher than the corresponding estimates with matching. The former equal 0.24, 0.30, and 0.35 for t=+1, t=+2, and t=+3, respectively. In contrast, the latter are in more mild magnitude, with values equal to 0.19, 0.14, and 0.13 for t=+1, t=+2, and t=+3, respectively.

The overestimation of the distress effect is also evident in the estimates on CEO compensation. Without matching, i.e., for the distressed firms only, we respectively obtain the estimated effects on *Total Current Compensation* in t=+1, t=+2, and t=+3 as -0.51, -0.57, and -0.68, and the estimated effects on *Total Direct Compensation* in t=+1, t=+2, and t=+3 are -4.04, -3.66, and -2.90. With matching, i.e., using the control group to filter out of the effects of the time trend, our respective estimates on *Total Current Compensation* in t=+1, t=+2, and t=+3 are -0.33, -0.40, and -0.41, and the estimates on *Total Direct Compensation* equal -3.02, -2.86, and -2.10 for t=+1, t=+2, and t=+3, respectively.

In summary, comparing the estimation results with versus without matching exposes the pitfall of ignoring the roles of time trends. Focusing only on the distressed firms in estimations tends to over-estimate the effects of corporate distress in that they may merely capture the time trends, leading to biased inference about the effects of corporate distress. It is thus imperative to appropriately control for the time trends in estimations. The matching estimator provides us a tool to construct an adequate control group and obtain proper estimates of the distress effects.

Moreover, regardless of the use of matching, our estimation results confirm the patterns observed from Figure 1. There is clear evidence of diminishing managerial influence over the board among the distressed firms. Fewer CEOs hold the board chairmanship, and less executives serve in the board or in the compensation committee after firms fall into distress. The evidence supports the conventional wisdom that corporate distress triggers firms and the boards of directors to reign

in the CEO's and other executives' power. Naturally, the corporate governance of affected firms improves. The estimation results also show that corporate distress significantly reduces the level of CEO compensation. This evidence corroborates the finding of Gilson and Vetsuypens (1993) who examine the 1980s data over the set of distressed firms only and report significant declines in CEO compensation upon or after the distress occurrence. Moreover, given that the bulk of CEO compensation is equity-based pay, the results suggest that corporate distress has a materially negative effect on CEO stock-based compensation. (Our unreported results bear this out.) In addition, the matching estimates provide evidence that reduced CEO compensation precedes board composition changes into corporate distress.

4.2.2 Cross-sectional Results: High- versus Low-Influence Firms

To examine whether the treatment effects of corporate distress exhibit cross-sectional variations, we separate the firms into two groups based on their *Corporate Governance Score* values in t=–3, i.e., three years before the hit of corporate distress. Note that, by construction the score is inversely related to managerial influence via managerial affiliations with the board. Consequently, we form a poorly-governed group, i.e., firms with high managerial influence, if a firm's *Corporate Governance Score* is below the median level in t=–3; we classify the other firms into a well-governed group, i.e., firms with low managerial influence.

Table 4, Panels A and B report the DID matching estimates of the treatment effects of corporate distress on managerial influence and CEO compensation for the low-*Score* subsample and the high-*Score* subsample, respectively. Two sets of results emerge in Panel A for the poorly-governed subsample. First, corporate governance improves in the distressed firms upon and after they fall into corporate distress. The estimated treatment effects on *Corporate Governance Score* are all positive and significant in the window t$\geq$ 0. The estimated effects on *CEO Duality* are all negative and significant for t$\geq$ 0. Second, upon or after the advent of corporate distress, CEO compensation, *Total Current Compensation* and *Total Direct Compensation* alike, declines significantly. Panel B presents similar but markedly weaker results for the group of firms with initially good governance, i.e., low managerial influence at t=–3.

We draw several inferences from comparing the results of Panel A and Panel B. First, although

qualitatively similar, the improvements in corporate governance when and after firms enter into distress are substantially greater in magnitude in Panel A than the improvements in Panel B. This evidence suggests that corporate distress triggers more dramatic reductions in the overall managerial influence, and hence more considerable improvements in corporate governance in initially poorly-governed firms. In other words, when corporate governance is poor, there is much larger room for improvements, so we see more prominent treatment effects on corporate governance measures.

Second, the treatment effects on *Total Current Compensation* and *Total Direct Compensation* in the pre-treatment years are mostly significantly negative in Panel A but not significant in Panel B. The treatment effects on managerial influence over the board in the pre-treatment years are largely not significant in either panel. The two pieces of evidence combined suggest that the relation between CEO compensation and managerial influence is weak in the pre-distress years.

Third, like in the full-sample results in Table 3, for both groups of distressed firms, we see different timing between CEO compensation changes and managerial influence changes, especially in the pre-distress years. Those years do not show significant board composition changes as reflected in *Corporate Governance Score* but show some significant changes in CEO compensation, especially for the poorly-governed group. This evidence suggests that there are intertemporal interactions between CEO compensation and managerial influence.

4.2.3 Time-varying Effects: Subperiod Results

To examine whether the treatment effects of corporate distress exhibit time variation, we apply the bias-corrected matching estimator to two subperiods. We choose 2004 as the cutoff year for two practical reasons. 1) SOX, with the aim to improve corporate governance in response to a series of corporate scandals, was enacted in 2002 and went into full effect by 2004. Figure 1 shows that the dynamics in the outcome variables appear to exhibit some structural change around 2002-2004. 2) The split around 2004 creates slightly more balanced subperiod observations than the split around 2002 or earlier.

Table 5, Panels A and B, report the DID matching estimation results for the earlier and later subperiods, respectively. There exist variations in terms of how corporate distress affects outcome variables before and after 2004. Corporate distress is associated with slightly more significant

board composition changes and governance improvements in the later subperiod than in the earlier subperiod. To the contrary, the reduction in CEO compensation related to corporate distress is more pronounced in the earlier subperiod.

To offer further evidence on the time-varying effects of corporate distress, we follow the literature and estimate pay-for-performance sensitivity (PPS) as a proxy for managerial incentive alignment (e.g. Jensen and Murphy, 1990; Murphy, 2000). Specifically, we run a panel regression, controlling for both firm-fixed effects and year-fixed effects, of CEO compensation on performance and its interactions with various dummy variables measuring distress status, event periods and calendar years over the set of distressed firms and their controls.[15] Table 6 reports the panel regression results, with Columns (1), (2) and (3) corresponding to the whole period, the pre-2004 subperiod and the post-2004 subperiod, respectively. The coefficient on the triple interaction term, $performance \times postdistress \times distressfirm$, gives the DID estimate of the effect of distress on the *sensitivity* of CEO flow compensation (*Total Direct Compensation* here) to firm performance. The PPS coefficient estimate is 0.39 and significant at the 5% level in the full sample (Column (1)). This result, obtained with a DID regression, is qualitatively similar to Gilson and Vetsuypens' (1993) finding that the PPS for distressed firms increases after distress.

There exists significant time variation in the effects of corporate distress on PPS. The PPS coefficient estimate is 1.02 and significant at the 5% level in the pre-2004 subperiod (Column (2)), and it is 0.29 but not statistically significant in the post-2004 subperiod (Column (3)). In Column (4), we use a slightly different model specification that introduces a post-2004 dummy variable and interact it with the other control variables of the model. In this specification, the same triple interaction term gives the DID estimate of the distress effect on PPS in the pre-2004 subperiod, while the quadruple interaction term, i.e., the triple interaction term times the post-2004 dummy, gives the DID estimate of the *incremental* distress effect on PPS in the post-2004 subperiod relative to the pre-2004 subperiod. The two coefficient estimates are respectively 0.90, significant at the 1% level (standard error=0.32), and -0.88, significant at the 10% level (standard

[15] In our panel regressions, the dependent variables are the flow compensation measures rather than the changes in CEO wealth as often used in the literature. We use flow compensation in the regressions in order to maintain consistency with our discussions in the paper and to show the dynamics of CEO compensation (relative to corporate governance) over time. Using changes in CEO wealth as the dependent variable yields qualitatively similar results. We also obtain qualitatively similar results if we follow Core and Guay's (1999) approach to estimate the pay-for-performance sensitivity.

error=0.47). The result in Column (4) echoes the subperiod results in Columns (2) and (3): the effects of corporate distress on PPS is significant in the pre-2004 subperiod but not significantly different from zero in the post-2004 subperiod. Overall, the results in Table 6 are consistent with the evidence in Table 5 on seemingly greater reductions in CEO compensation in the pre-2004 subperiod than in the post-2004 subperiod. The bifurcating significance of the PPS estimates across the two subperiods may pertain to the following economic force — the regulatory reforms in the early 2000s improve corporate governance and better align managerial incentives, likely reducing the space for post-distress improvements in managerial incentive after 2004.

4.3 Effects of Corporate Distress on CEO Turnover

One way a firm deals with an under-performing CEO is to replace him with a new one. Table 7 reports the DID estimates of the treatment effects of corporate distress on CEO turnover. In Panel A, we group the years before and after the time of financial distress (t=0) in alternative windows. The CEO turnover rates are significantly higher for the treated firms than for the actual controls. For example, in the full sample, the treatment effect on the CEO turnover rate in the $[-1, +1]$ window is 17%, meaning that 17% more of CEOs in distressed firms lose their jobs relative to those CEOs in non-distressed firms. This panel also shows that there is a regime shift in the treatment effects over time: the effect of corporate distress on the CEO turnover rate after 2004 is generally smaller than before 2004. The treatment effects in various windows hover around 21%-24% before 2004 and range between 15%-18% after 2004.

We also estimate the treatment effects on the CEO turnover rate on a year-by-year basis and report the results in Panel B. The single most important effect on CEO turnover occurs in the treatment year (i.e., t=0), when we observe a significant treatment effect of 15% in the full sample, 14% in the pre-2004 subperiod, and 15% in the post-2004 subperiod. The treatment effects on CEO turnover in the full sample are also present in the post-treatment years: 2% in t=+1, 7% in t=+2, and 10% in t=+3, respectively.

The result of increased CEO turnover rates in distressed firms is consistent with the extant findings on the effect of firm performance on CEO turnover (e.g., Coughlan and Schmidt, 1985; Gilson, 1989; Huson, Parrino, and Starks, 2001; Kaplan and Minton, 2012; Jenter and

Kanaan, 2015). This result also mirrors our above evidence that managerial influences decline after firms become distressed. The more likely a CEO is to lose his job, the less influence he is to exert over the board.

4.4 Effects of Distress on Performance-related and Performance-unrelated CEO Compensation

Following prior studies (e.g., Jensen and Murphy, 1990; Bertrand and Mullainathan, 2001), we decompose CEO compensation into two parts, one related to performance and the other not related to performance. Specifically, using *only* non-distressed firms we regress each measure of CEO compensation, *Total Current Compensation* and *Total Direct Compensation*, against the following four firm performance measures: ROA, one-year stock return, log value of market capitalization, and log value of total assets, as well as industry dummies and year dummies. We then use the obtained parameter estimates to calculate the predicted values and residuals of the respective compensation for *both* distressed and non-distressed firms.[16] For distressed firms, the expected compensation characterizes performance-related compensation if the model parameters linking compensation to performance are the same for distressed and non-distressed firms; the unexpected compensation comprises two portions: one due to potential model misspecification and the other truly unrelated to performance, i.e., the "abnormal" compensation. We thus interpret the regression residuals for the distressed firms only as a coarse proxy for the abnormal compensation. We respectively estimate the ATT effects of distress on the two parts of compensation. Table 8 reports the results, with Panel A for expected compensation and Panel B for unexpected compensation.

Panel A contrasts the expected compensation of distressed firms with the expected compensation of control firms for the full sample as well as its two subperiods. As can be seen clearly, corporate distress significantly reduces expected compensation in all the event years, irrespective of the periods we choose to analyze. In Panel B, we compare the unexpected compensation of the distressed firms with the unexpected compensation of the control firms. Overall, the results show that corporate

[16]We tried alternative specifications for compensation decompositions. For example, to address the potential concern that size could be a proxy for managerial power, we do not include market capitalization or total assets in the regression. We also produce two dollar measures of firm performance, respectively by multiplying market capitalization with one-year stock return and total assets with ROA, and regress each level of compensation with the two dollar performance measures to obtain regression residuals. The results obtained using alternative compensation decompositions in the analysis are similar to the ones presented in this section.

distress does not affect much unexpected compensation. Almost all of the estimated treatment effects from t=–1 to t=+3 are not significantly different from zero in the full sample. The only exception is: the estimated effect on unexpected *Total Current Compensation* in t=0 is positive and significant at the 1% level. The subperiod results are largely similar to the full-sample results.

In summary, the results in Panels A and B suggest that corporate distress reduces performance-related compensation but not residual compensation. With the caveat that the compensation decomposition is based on a standard, albeit quite simplistic, parametric model for non-distressed firms and may not be completely appropriate for distressed firms, we take our results as indicating that the majority of the effects of corporate distress on CEO pay is driven by performance-related compensation.

5 Conclusions

In this paper we study the dynamics of two constructs of corporate governance, managerial power and CEO compensation, over the course of corporate distress during the 1992-2019 period. Data show an apparent trend that governance improves over time, which confounds and biases the long-run study of effects of corporate distress on governance if the trend is not properly controlled for. To address this concern, we apply a bias-corrected matching estimator to identify an appropriate control group for filtering out the effects of the trend and to estimate the true effects of corporate distress on managerial influence over the board, CEO compensation, and CEO turnover.

Corporate distress has material consequences on corporate governance and CEO compensation. Upon and after the onset of corporate distress, affected firms substantially reduce managerial appointments to the board, intensify managerial incentive alignment, cut CEO compensation, and increase CEO turnover. As a result, corporate governance improves. The effects of corporate distress on governance vary across section and over time. In particular, initially poorly-governed affected firms experience more significant reductions in managerial power and CEO compensation than initially well-governed firms. We also find evidence that managerial incentive alignments tighten after firms fall into distress, particularly in the pre-2004 subperiod.

Our results suggest that the bulk of the effects of corporate distress on CEO compensation

derives from the part of compensation tied to firm performance, consistent with the "shareholder view" of CEO compensation. Moreover, firms reduce CEO compensation before corporate distress but adjust managerial board appointments only after becoming distressed. These results suggest that the CEO compensation changes are not entirely driven by the board structure changes. Other forces are at work during the course of corporate distress. A thorough understanding of those forces warrants further research.

Appendix: Matching Estimator — Some Technical Details

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

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

The main identifying assumption necessary to estimate the above ATT is known as unconfoundedness or the Conditional Independence Assumption (CIA). It assumes that $(Y_i(1), Y_i(0)) \perp T_i|X_i$. This implies that after controlling for observable characteristics the potential outcomes are independent of the treatment status. Actually, following Abadie and Imbens (2006, 2011) only a weaker version of the CIA is needed for estimating the ATT by matching; we only need $Y_i(0) \perp T_i|X_i$, that is, T_i is independent of $Y_i(0)$ conditional on X_i.[17] The intuition is that, after we control for all potential confounders X_i we can assume that the treatment is as good as a randomized treatment.

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

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

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

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

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

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

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

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

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

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

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

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

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Table 1: Number of Observations across Years

This table reports the number of observations for distressed firms and non-distressed firms across years. We require a firm included in the analysis sample to have no missing information in the matching covariates in period -3, 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, where period 0 refers to the year in which a firm falls into distress. See Table 2 for details of the matching covariates and the compensation variables. The sample period is 1992-2019 and period 0 spans from 1995 through 2016.

Fiscal Year	Distressed Firms	Non-distressed Firms
1995	10	814
1996	9	990
1997	2	952
1998	9	934
1999	8	984
2000	6	1,090
2001	11	1,148
2002	23	1,128
2003	25	1,101
2004	13	1,102
2005	16	1,065
2006	6	1,091
2007	12	1,068
2008	20	1,042
2009	18	1,104
2010	14	1,356
2011	17	1,337
2012	24	1,322
2013	14	1,294
2014	10	1,261
2015	22	1,236
2016	8	1,226
Total	297	24,645

Table 2: Balancing: Matching Covariates and Non-Matched Covariates

This table shows balancing of the matching covariates (in Panel A) and some non-matched covariates (in Panel B) in period -3, where the benchmark period 0 refers to the year when a firm falls into distress. The sample period spans from 1992 to 2019, with corporate distress occurring in 1995-2016. Matching covariates include dummies for each fiscal year of distress, dummies for each Fama-French 5-industry classification, firm size measured by natural logarithm of market capitalization, size dummy that equals one if market capitalization is in the 70th percentile and zero otherwise, leverage measured by total debt-to-asset ratio, Ohlson's (1980) O-score, past three-year cumulative stock returns, O-score dummy that equals one if the O-score is in the 80th percentile and zero otherwise, stock return dummy that equals one if the three-year cumulative return is in the 20th percentile and zero otherwise, total current compensation (equal to the sum of salary and bonus), total direct compensation (equal to the sum of total current compensation, value of restricted stock grants, Black-Scholes value of stock option grants, other annual short-term compensation, payouts from long-term incentive plans, and all other long-term compensation), and three dummy variables measuring managerial service in the board: Executive in Compensation Committee, Insider Directors, and CEO-Chair Duality. Both Total Current Compensation and Total Direct Compensation are quoted in millions of 2018 constant dollars. Non-matched covariates include natural logarithm of total assets, natural logarithm of total sales, and natural logarithm of number of employees, market-to-book asset ratio (market value of equity plus book value of assets minus the sum of book value of common equity and deferred taxes, scaled by book value of assets), profit margin (income before extraordinary items divided by total sales), return on equity (income before extraordinary items divided by total stockholders' equity), return on assets (income before extraordinary items divided by total assets), cashflow-to-asset ratio (operating activities net cash flow divided by total assets), book leverage ratio (the sum of long-term debts and short-term debts divided by total assets), Altman's (1968) z-score (a lower value corresponding to more severe financial distress), a company's S&P credit ratings (with the ratings of "AAA", "AA+" converted to the values of 1, 2, and so on), Dummy for investment-grade rating (equal to one if a company receives a rating of "BBB-" or better, and zero otherwise), one-year stock return and one-year stock return volatility (respectively the cumulative stock return and the stock return volatility in the past one year), and share turnover (the number of common shares traded divided by the number of common shares outstanding). Columns (1)-(2) and Columns (3)-(4) respectively summarize the covariates for the treated and the potential controls. Columns (5)-(6) present the raw difference in the covariates between the treated and potential controls, and Columns (7)-(8) report the within-match difference in the covariates between the treated and actual controls. "S.D." and "S.E." stand for standard deviation and standard error, respectively. We apply Abadie and Imbens' (2011) matching estimator to identify the actual controls. *, ** and *** denote significance at 10%, 5% and 1%, respectively.

	Treated		Potential Controls		Raw Difference		Within-Match Difference	
	Mean	S.D.	Mean	S.D.	Mean	S.E.	Mean	S.E.
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Matching Covariates								
Fiscal Year 1995	0.04	0.19	0.06	0.24	-0.02	0.01	0.00	0.00
Fiscal Year 1996	0.03	0.17	0.08	0.26	-0.05**	0.01	0.00	0.00
Fiscal Year 1997	0.01	0.08	0.03	0.16	-0.02**	0.01	0.00	0.00
Fiscal Year 1998	0.03	0.18	0.02	0.15	0.01	0.01	0.00	0.00
Fiscal Year 1999	0.03	0.17	0.05	0.21	-0.02	0.01	0.00	0.00
Fiscal Year 2000	0.02	0.13	0.02	0.13	0.00	0.01	0.00	0.00
Fiscal Year 2001	0.04	0.19	0.10	0.29	-0.06**	0.01	0.00	0.00
Fiscal Year 2002	0.08	0.27	0.06	0.24	0.02	0.02	0.00	0.00
Fiscal Year 2003	0.09	0.29	0.07	0.25	0.02	0.02	0.00	0.00
Fiscal Year 2004	0.05	0.21	0.04	0.21	0.00	0.01	0.00	0.00

	Treated		Potential Controls		Raw Difference		Within-Match Difference	
	Mean	S.D.	Mean	S.D.	Mean	S.E.	Mean	S.E.
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fiscal Year 2005	0.06	0.23	0.02	0.14	0.04**	0.01	0.00	0.00
Fiscal Year 2006	0.02	0.15	0.01	0.11	0.01	0.01	0.00	0.00
Fiscal Year 2007	0.04	0.20	0.02	0.12	0.03*	0.01	0.00	0.00
Fiscal Year 2008	0.07	0.25	0.03	0.18	0.04*	0.02	0.00	0.00
Fiscal Year 2009	0.06	0.23	0.04	0.19	0.02	0.01	0.00	0.00
Fiscal Year 2010	0.05	0.21	0.09	0.29	-0.05**	0.01	0.00	0.00
Fiscal Year 2011	0.05	0.22	0.07	0.25	-0.02	0.01	0.00	0.00
Fiscal Year 2012	0.08	0.26	0.03	0.17	0.04**	0.02	0.00	0.00
Fiscal Year 2013	0.04	0.20	0.02	0.14	0.02	0.01	0.00	0.00
Fiscal Year 2014	0.03	0.16	0.05	0.22	-0.03*	0.01	0.00	0.00
Fiscal Year 2015	0.08	0.27	0.03	0.16	0.05**	0.02	0.00	0.00
Fiscal Year 2016	0.03	0.17	0.08	0.27	-0.05**	0.01	0.00	0.00
Dummy SIC = 1 – Consumer Industries/Services	0.19	0.39	0.19	0.39	0.00	0.02	0.00	0.00
Dummy SIC = 2 – Manufacturing, Energy & Utilities	0.23	0.42	0.23	0.42	0.00	0.03	-0.01	0.00
Dummy SIC = 3 – Business Equipment, Telecomm & TV	0.32	0.47	0.18	0.39	0.13**	0.03	-0.01*	0.00
Dummy SIC = 4 – Healthcare, Medical Equp & Drugs	0.11	0.32	0.07	0.26	0.04*	0.02	0.01**	0.00
Dummy SIC = 5 – Other Industries	0.16	0.37	0.33	0.47	-0.17**	0.02	0.00	0.00
Log of Market Capitalization	7.22	1.35	7.47	1.48	-0.25**	0.09	-0.08	0.04
Dummy for Large Market Cap (70th percentile)	0.19	0.40	0.25	0.43	-0.06*	0.03	-0.01	0.01
Total Liabilities/Total Assets	0.51	0.27	0.55	0.25	-0.03*	0.02	0.02	0.01
O-Score	-2.07	3.21	-2.33	2.31	0.26	0.20	0.21**	0.08
Cumulative Stock Return over [-5, -3]	0.26	0.83	0.15	0.38	0.11**	0.05	0.02**	0.01
Dummy for Top Quintile O-Score Distribution	0.08	0.28	0.02	0.14	0.06**	0.02	0.00	0.00
Dummy for Bottom Quintile 3-year Stock Return Distribution	0.08	0.28	0.04	0.19	0.05**	0.02	0.04**	0.01
Total Current Compensation ($M)	1.47	2.14	1.27	1.21	0.20	0.13	0.18**	0.06
Total Direct Compensation ($M)	6.44	13.35	4.34	5.88	2.10**	0.81	0.90**	0.24
Dummy for Executive(s) in the Compensation Committee	0.06	0.23	0.07	0.26	-0.02*	0.01	0.01	0.01
Dummy for Insider Director(s)	0.99	0.10	0.98	0.15	0.01	0.01	0.01	0.01
Dummy for CEO-Chair Duality	0.40	0.49	0.42	0.49	-0.02	0.03	0.01	0.02
Panel B. Non-Matched Covariates								
Log of Total Assets	7.01	1.55	7.59	1.74	-0.58***	0.10	0.15*	0.07
Log of Total Sales	6.70	1.86	7.11	1.60	-0.42***	0.12	0.06	0.07
Log of Number of Employees	0.95	1.88	1.18	1.73	-0.23*	0.12	-0.02	0.08
Market-to-Book Asset Ratio	3.13	5.19	2.02	1.75	1.11***	0.31	-0.16*	0.07
Profit Margin	-4.11	47.05	0.05	0.41	-4.16	2.82	0.41	0.36
Return on Equity	0.15	1.97	0.13	0.88	0.02	0.12	-0.01	0.01
Return on Asset	0.01	0.17	0.05	0.09	-0.04***	0.01	-0.01**	0.00
Cashflow-to-Asset Ratio	0.06	0.16	0.10	0.09	-0.04***	0.01	-0.01**	0.00
Book Leverage Ratio	0.24	0.22	0.22	0.20	0.02**	0.01	-0.01	0.01
Z-Score	-10.86	34.18	-4.98	8.03	-5.88***	2.05	1.72**	0.59
Credit Rating	12.15	2.56	8.88	3.18	3.27***	0.25	-0.17	0.32
Dummy for Investment-Grade Rating	0.25	0.43	0.71	0.45	-0.46***	0.04	-0.01	0.05
One-year Stock Return	0.56	1.69	0.17	0.59	0.40***	0.10	-0.08*	0.04
One-year Stock Return Volatility	0.54	0.21	0.39	0.18	0.16***	0.01	0.00	0.01
Share Turnover	3.26	2.94	1.82	1.78	1.44***	0.18	0.06	0.08

Table 3: Effects of Corporate Distress on Managerial Influence and CEO Compensation: With and Without Control Group

This table reports the estimated effects of corporate distress on managerial influence and CEO compensation during the 1992-2019 period, whereas corporate distress occurred in 1995-2016. Panel A contains both treated and control firms and presents the difference-in-differences estimates of the average treatment effect for the treated (ATT) of corporate distress on managerial influence over the board and CEO compensation for each event period. We use Abadie and Imbens' (2011) bias-corrected matching estimator to select control firms and conduct estimations. Event periods are measured relative to period 0, the year of distress occurrence. Panel B includes treated firms only and lists the margins of the following model estimated for each period $t = -2, -1, ..., +3$: $y_{i,t} = \alpha_t + X_{i,-3}\beta_t + u_{i,t}$, where X represents a set of control variables whose $t = -3$ observations are used as matching covariates of the matching estimator and each y is differenced with respect to its corresponding value in period –3. Managerial influence measures include whether the CEO serves as the board chair (*CEO Duality*; equal to one if true and zero otherwise), whether at least one executive serves as a board director (*Inside Directors*; equal to one if true and zero otherwise), and whether at least one executive serves in the compensation committee of the board (*Executives in Compensation Committee*; equal to one if true and zero otherwise). *Corporate Governance Score* is average of the following three standardized variables: *CEO Duality*, number of executives serving as board directors, and number of executives serving in the compensation committee, each demeaned and divided by its standard deviation. We multiply *Corporate Governance Score* by minus one so that a higher *Corporate Governance Score* value corresponds to better governance. CEO compensations, in 2018 constant dollars, include total current compensation (*Total Current Compensation*; equal to the sum of salary and bonus) and total direct compensation (*Total Direct Compensation*; equal to the sum of *Total Current Compensation*, value of restricted stock grants, Black-Scholes value of stock option grants, other annual short-term compensation, payouts from long-term incentive plans, and all other long-term compensation). Robust standard errors are reported in parentheses. *, ** and *** denote significance at 10%, 5% and 1%, respectively.

Panel A. Matching Estimator: Sample of Treated and Control Firms

Period	–2	–1	0	+1	+2	+3
CEO Duality	0.00	-0.01	-0.08**	-0.11***	-0.10***	-0.13***
	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)
Inside Directors	-0.01	-0.01	-0.01	-0.01	-0.01	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Executives in Compensation Committee	-0.01	-0.02*	-0.04**	-0.04**	-0.02	-0.02
	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)
Corporate Governance Score	0.03	0.02	0.11*	0.19***	0.14**	0.13**
	(0.04)	(0.05)	(0.06)	(0.06)	(0.06)	(0.07)
Total Current Compensation	-0.24***	-0.27***	-0.41***	-0.33***	-0.40***	-0.41***
	(0.07)	(0.10)	(0.09)	(0.11)	(0.11)	(0.12)
Total Direct Compensation	-2.07**	-1.87***	-2.81***	-3.02***	-2.86***	-2.10***
	(0.83)	(0.71)	(0.88)	(0.73)	(0.71)	(0.76)
Number of Observations	2,537	2,536	2,534	2,533	2,534	2,195

Panel B. Margins: Sample of Treated Firms Only

Period	–2	–1	0	+1	+2	+3
CEO Duality	-0.02	-0.02	-0.06	-0.12***	-0.14***	-0.21***
	(0.03)	(0.04)	(0.05)	(0.04)	(0.05)	(0.05)
Inside Directors	-0.00	0.00	0.01	0.00	-0.00	-0.03*
	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.02)
Executives in Compensation Committee	-0.03	-0.04*	-0.07***	-0.08***	-0.07***	-0.06***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Corporate Governance Score	0.07	0.01	0.11*	0.24***	0.30***	0.35***
	(0.04)	(0.05)	(0.07)	(0.06)	(0.06)	(0.07)
Total Current Compensation	-0.27*	-0.53**	-0.73***	-0.51***	-0.57***	-0.68***
	(0.15)	(0.21)	(0.18)	(0.17)	(0.18)	(0.19)
Total Direct Compensation	-0.10	-1.20	-2.74***	-4.04***	-3.66***	-2.90***
	(2.40)	(1.05)	(0.99)	(0.98)	(0.95)	(1.08)
Number of Observations	277	276	277	275	277	261

Table 4: Average Treatment Effect for the Treated (ATT) of Corporate Distress on Managerial Influence and CEO Compensation: High- versus Low-Influence Firms

This table respectively reports the difference-in-differences estimates of the average treatment effect for the treated (ATT) of corporate distress on managerial influence over the board and CEO compensation for the high-influence firms (Panel A) and low-influence firms (Panel B), using Abadie and Imbens' (2011) bias-corrected matching estimator. Event periods are measured relative to period 0, the year of distress occurrence. Managerial influence measures include whether the CEO serves as the board chair (*CEO Duality*; equal to one if true and zero otherwise), whether at least one executive serves as a board director (*Inside Directors*; equal to one if true and zero otherwise), and whether at least one executive serves in the compensation committee of the board (*Executives in Compensation Committee*; equal to one if true and zero otherwise). *Corporate Governance Score* is average of the following three standardized variables: *CEO Duality*, number of executives serving as board directors, and number of executives serving in the compensation committee, each demeaned and divided by its standard deviation for standardization. We multiply *Corporate Governance Score* by minus one so that a higher *Corporate Governance Score* value corresponds to better governance. CEO compensations, in 2018 constant dollars, include total current compensation (*Total Current Compensation*; equal to the sum of salary and bonus) and total direct compensation (*Total Direct Compensation*; equal to the sum of *Total Current Compensation*, value of restricted stock grants, Black-Scholes value of stock option grants, other annual short-term compensation, payouts from long-term incentive plans, and all other long-term compensation). High-influence (low-influence) firms are those that have their *Corporate Governance Score* values below (above) the median value in t=–3. Robust standard errors are reported in parentheses. *, ** and *** denote significance at 10%, 5% and 1%, respectively.

Panel A. Firms with High Managerial Influence (or Low *Corporate Governance Score*) in Period -3

Period	–2	–1	0	+1	+2	+3
CEO Duality	0.03	-0.02	-0.12**	-0.15***	-0.16***	-0.24***
	(0.03)	(0.05)	(0.06)	(0.06)	(0.06)	(0.07)
Inside Directors	0.01**	0.00	0.00	0.00	0.00	-0.04*
	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.02)
Executives in Compensation Committee	-0.01	-0.04	-0.06*	-0.04	-0.04	-0.05*
	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)
Corporate Governance Score	0.00	-0.01	0.17**	0.21***	0.21**	0.29***
	(0.06)	(0.07)	(0.08)	(0.08)	(0.09)	(0.09)
Total Current Compensation	-0.31**	-0.26	-0.55***	-0.53***	-0.48***	-0.55***
	(0.15)	(0.17)	(0.14)	(0.16)	(0.17)	(0.20)
Total Direct Compensation	-3.50**	-1.91*	-3.35**	-3.52***	-3.10***	-3.24***
	(1.59)	(1.02)	(1.44)	(1.09)	(1.01)	(1.12)
Number of Observations	1,357	1,358	1,358	1,356	1,356	1,195

Panel B. Firms with Low Managerial Influence (or High *Corporate Governance Score*) in Period -3

Period	–2	–1	0	+1	+2	+3
CEO Duality	-0.03	-0.03	-0.08**	-0.10***	-0.08**	-0.06
	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Inside Directors	-0.02	-0.02	-0.02	-0.01	-0.01	0.03
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Executives in Compensation Committee	0.00	0.00	-0.01	-0.01***	0.00	0.01
	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)
Corporate Governance Score	0.04	0.07	0.10*	0.18***	0.14**	0.07
	(0.05)	(0.06)	(0.06)	(0.05)	(0.05)	(0.06)
Total Current Compensation	-0.04	0.17	-0.24**	-0.07	-0.21*	-0.19
	(0.07)	(0.20)	(0.10)	(0.13)	(0.11)	(0.13)
Total Direct Compensation	4.29*	-0.65	-0.96	-1.31*	-2.95***	-0.41
	(2.32)	(0.87)	(0.79)	(0.79)	(0.80)	(0.91)
Number of Observations	1,170	1,168	1,166	1,167	1,168	991

Table 5: Average Treatment Effect for the Treated (ATT) of Corporate Distress on Managerial Influence and CEO Compensation: Subperiod Results

This table respectively reports the difference-in-differences estimates of the average treatment effect for the treated (ATT) of corporate distress on managerial influence over the board and CEO compensation for the pre-2004 subperiod (Panel A) and the post-2004 subperiod (Panel B), using Abadie and Imbens' (2011) bias-corrected matching estimator. Event periods are measured relative to period 0, the year of distress occurrence. Managerial influence measures include whether the CEO serves as the board chair (*CEO Duality*; equal to one if true and zero otherwise), whether at least one executive serves as a board director (*Inside Directors*; equal to one if true and zero otherwise), and whether at least one executive serves in the compensation committee of the board (*Executives in Compensation Committee*; equal to one if true and zero otherwise). *Corporate Governance Score* is average of the following three standardized variables: *CEO Duality*, number of executives serving as board directors, and number of executives serving in the compensation committee, each demeaned and divided by its standard deviation for standardization. We multiply *Corporate Governance Score* by minus one so that a higher *Corporate Governance Score* value corresponds to better governance. CEO compensations, in 2018 constant dollars, include total current compensation (*Total Current Compensation*; equal to the sum of salary and bonus) and total direct compensation (*Total Direct Compensation*; equal to the sum of *Total Current Compensation*, value of restricted stock grants, Black-Scholes value of stock option grants, other annual short-term compensation, payouts from long-term incentive plans, and all other long-term compensation). Robust standard errors are reported in parentheses. *, ** and *** denote significance at 10%, 5% and 1%, respectively.

Panel A. Pre-2004 Subperiod

Period	–2	–1	0	+1	+2	+3
CEO Duality	0.01	0.02	-0.07	-0.04	-0.11*	-0.16**
	(0.04)	(0.05)	(0.06)	(0.06)	(0.06)	(0.07)
Inside Directors	0.00	0.00	0.00	0.01	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Executives in Compensation Committee	-0.04	-0.05*	-0.09**	-0.09**	-0.05	-0.06
	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)	(0.05)
Corporate Governance Score	-0.02	-0.05	0.12	0.18*	0.12	0.17
	(0.07)	(0.09)	(0.12)	(0.11)	(0.12)	(0.13)
Total Current Compensation	-0.39***	-0.30*	-0.80***	-0.78***	-0.83***	-0.64***
	(0.13)	(0.17)	(0.13)	(0.15)	(0.15)	(0.18)
Total Direct Compensation	-5.14***	-2.28	-3.09	-4.34***	-3.70**	-2.86
	(1.96)	(1.43)	(1.91)	(1.50)	(1.47)	(1.84)
Number of Observations	1,273	1,273	1,272	1,270	1,271	1,019

Panel B. Post-2004 Subperiod

Period	–2	–1	0	+1	+2	+3
CEO Duality	0.00	-0.02	-0.09*	-0.15***	-0.09*	-0.09*
	(0.03)	(0.04)	(0.05)	(0.05)	(0.05)	(0.06)
Inside Directors	-0.02	-0.02	-0.02	-0.02	-0.01	-0.03
	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)
Executives in Compensation Committee	0.01	0.01***	0.00	0.00	0.00	-0.01
	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
Corporate Governance Score	0.07	0.06	0.12**	0.20***	0.16**	0.15**
	(0.05)	(0.05)	(0.06)	(0.06)	(0.07)	(0.08)
Total Current Compensation	-0.13	-0.18	-0.15	-0.02	-0.06	-0.21
	(0.09)	(0.14)	(0.14)	(0.15)	(0.14)	(0.17)
Total Direct Compensation	-0.71*	-1.34***	-2.13***	-2.15***	-2.39***	-2.09***
	(0.42)	(0.48)	(0.48)	(0.47)	(0.46)	(0.55)
Number of Observations	1,254	1,253	1,252	1,253	1,253	1,167

Table 6: Sensitivity of CEO (Flow) Compensation to Firm Performance for Distressed and Non-distressed Firms

This table presents the fixed-effect regression results of CEO (flow) compensation on firm performance and its interactions with various calendar and distress-related period indicators among distressed firms and their controls in our sample. Firm performance (*Performance*, in millions of dollars) is defined as a firm's market capitalization at last year-end times its annual stock return for this year. CEO compensations, in 2018 constant dollars, include total current compensation (*Total Current Compensation*; equal to the sum of salary and bonus) and total direct compensation (*Total Direct Compensation*; equal to the sum of *Total Current Compensation*, value of restricted stock grants, Black-Scholes value of stock option grants, other annual short-term compensation, payouts from long-term incentive plans, and all other long-term compensation). For brevity the regression results for *Total Current Compensation* are not reported. *Distressfirm* is a dummy variable that equals one for firms that ever fell into distress, and zero otherwise. *Postdistress* is a dummy variable that equals one if event period $t > 0$ and zero otherwise, whereas period 0 is the year of distress occurrence. *Post2004* is a dummy variable that equals one for years after 2004 and zero otherwise. All the regressions control for both firm-fixed effects and year-fixed effects. Robust standard errors as reported in parentheses adjust for firm-level clustering. *, ** and *** denote significance at 10%, 5% and 1%, respectively.

	Full Sample	Pre-2004	Post-2004	Full Sample
Performance	0.07***	0.07***	0.05***	0.07***
	(0.01)	(0.01)	(0.01)	(0.01)
Performance × *Postdistress*	-0.04**	0.09***	0.01	0.09***
	(0.02)	(0.03)	(0.02)	(0.02)
Performance× *Distressfirm*	0.05**	0.03	0.19	0.04*
	(0.02)	(0.04)	(0.14)	(0.02)
Performance× *Postdistress*× *Distressfirm*	0.39**	1.02**	0.29	0.90***
	(0.18)	(0.46)	(0.19)	(0.32)
Postdistress× *Distressfirm*	-2,063.81***	-2,081.16*	-1,455.43***	-2,013.38***
	(402.10)	(1,108.88)	(274.91)	(775.70)
Postdistress	-415.84	-1,468.49**	30.97	-1,101.41***
	(266.51)	(573.29)	(182.88)	(366.91)
Performance× *Post2004*				-0.05*
				(0.03)
Performance × *Postdistress*× *Post2004*				-0.15***
				(0.04)
Performance× *Distressfirm*× *Post2004*				0.19
				(0.29)
Performance× *Postdistress*× *Distressfirm*× *Post2004*				-0.88*
				(0.47)
Distressfirm× *Post2004*				-1,508.88*
				(833.60)
Postdistress× *Post2004*				1,091.69**
				(482.62)
Postdistress× *Distressfirm*× *Post2004*				442.32
				(945.93)
Post2004				9,090.39***
				(1,439.42)
Constant	315.58	1,229.66	5,568.87***	63.93
	(822.78)	(1,028.03)	(196.97)	(829.66)
Firm-fixed Effects	Yes	Yes	Yes	Yes
Year-fixed Effects	Yes	Yes	Yes	Yes
Number of Observations	21,793	9,056	12,737	21,793
Overall-R^2	0.018	0.040	0.026	0.019

Table 7: Average Treatment Effect for the Treated (ATT) of Corporate Distress on CEO Turnover Rates

This table reports the difference-in-differences estimates of the average treatment effect for the treated (ATT) of financial distress on CEO turnover rates, using Abadie and Imbens' (2011) bias-corrected matching estimator. Event periods are measured relative to period 0, the year of distress occurrence. 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. CEO Turnover Rates by Multi-Year Windows

Period	[-1,+1]	[-1,+2]	[-1,+3]	[-2,+2]	[-2,+3]	[-3,+3]
			Full Sample			
CEO Turnover Rate	0.17***	0.18***	0.20***	0.19***	0.20***	0.19***
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Number of Observations	2,528	2,528	2,528	2,528	2,528	2,528
			Pre-2004 Subperiod			
CEO Turnover Rate	0.16***	0.21***	0.23***	0.23***	0.24***	0.24***
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.05)
Number of Observations	1,273	1,273	1,273	1,273	1,273	1,273
			Post-2004 Subperiod			
CEO Turnover Rate	0.18***	0.15***	0.17***	0.15***	0.16***	0.16***
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Number of Observations	1,255	1,255	1,255	1,255	1,255	1,255

Panel B. CEO Turnover Rates Year-by-Year

Period	-2	-1	0	+1	+2	+3
			Full Sample			
CEO Turnover Rate	0.01	0.05*	0.15***	0.02	0.07***	0.10***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)
Number of Observations	2,526	2,525	2,523	2,512	2,520	2,174
			Pre-2004 Subperiod			
CEO Turnover Rate	0.02	0.01	0.14***	0.06	0.12***	0.12***
	(0.04)	(0.04)	(0.04)	(0.05)	(0.04)	(0.04)
Number of Observations	1,272	1,273	1,272	1,267	1,269	1,010
			Post-2004 Subperiod			
CEO Turnover Rate	0.01	0.06*	0.15***	0.01	0.03	0.08***
	(0.03)	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)
Number of Observations	1,254	1,252	1,251	1,245	1,251	1,164

Table 8: Average Treatment Effect for the Treated (ATT) of Corporate Distress on Different Parts of CEO Compensation

Panels A and B of this table report the difference-in-differences matching estimates of the average treatment effects for the treated (ATT) of corporate distress on predicted compensation and residual compensation, respectively. For each level of CEO compensation, *Total Current Compensation* and *Total Direct Compensation*, we regress it against the following four firm performance measures: return on assets (ROA), one-year stock return, natural logarithm of market capitalization, and natural logarithm of total assets, as well as industry dummies and year dummies, over the set of control firms to obtain parameter estimates, then we apply the parameter estimates to both the controls and the distressed firms to calculate predicted values and residuals. We name their respective predicted values as *Expected Total Current Compensation* and *Expected Total Direct Compensation*, and their respective residuals as *Unexpected Total Current Compensation* and *Unexpected Total Direct Compensation*. *Total Current Compensation* is the sum of salary and bonus; *Total Direct Compensation* is the sum of *Total Current Compensation*, value of restricted stock grants, Black-Scholes value of stock option grants, other annual short-term compensation, payouts from long-term incentive plans, and all other long-term compensation. Event periods are defined relative to Period 0, the year of distress occurrence. Robust standard errors are reported in parentheses. *, ** and *** denote significance at 10%, 5% and 1%, respectively.

Panel A. Estimates on Predicted Compensation

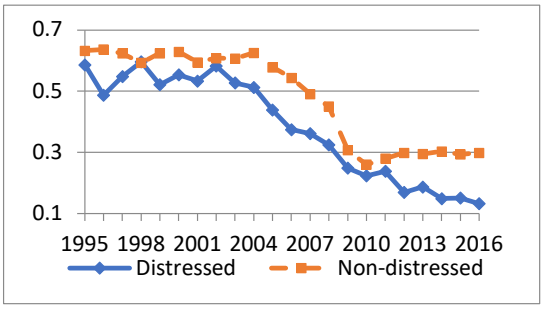
Period	-2	-1	0	+1	+2	+3
			Full Sample			
Expected Total Current Compensation	-0.16***	-0.29***	-0.66***	-0.46***	-0.53***	-0.58***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)
Expected Total Direct Compensation	-1.11***	-1.75***	-3.59***	-2.29***	-2.84***	-3.06***
	(0.17)	(0.17)	(0.19)	(0.21)	(0.20)	(0.24)
Number of Observations	2,535	2,534	2,531	2,519	2,525	2,182
			Pre-2004 Subperiod			
Expected Total Current Compensation	-0.18***	-0.37***	-0.70***	-0.52***	-0.59***	-0.72***
	(0.06)	(0.06)	(0.06)	(0.07)	(0.07)	(0.08)
Expected Total Direct Compensation	-1.43***	-2.25***	-3.99***	-2.77***	-3.32***	-4.00***
	(0.38)	(0.40)	(0.38)	(0.42)	(0.40)	(0.48)
Number of Observations	1,278	1,279	1,278	1,271	1,272	1,016
			Post-2004 Subperiod			
Expected Total Current Compensation	-0.12***	-0.23***	-0.64***	-0.43***	-0.50***	-0.52***
	(0.02)	(0.02)	(0.04)	(0.04)	(0.04)	(0.05)
Expected Total Direct Compensation	-0.77***	-1.34***	-3.35***	-2.04***	-2.53***	-2.61***
	(0.10)	(0.11)	(0.20)	(0.23)	(0.22)	(0.28)
Number of Observations	1,257	1,255	1,253	1,248	1,253	1,166

Panel B. Estimates on Residual Compensation

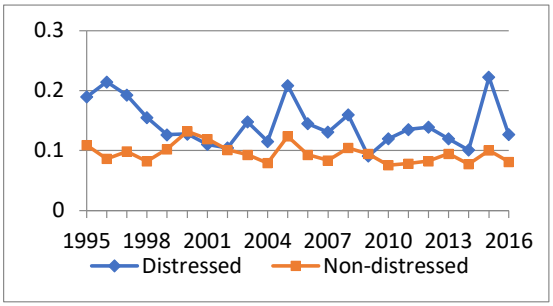
Period	-2	-1	0	+1	+2	+3
			Full Sample			
Unexpected Total Current Compensation	-0.08	0.02	0.26***	0.12	0.14	0.17
	(0.07)	(0.10)	(0.09)	(0.11)	(0.11)	(0.12)
Unexpected Total Direct Compensation	-0.97	-0.14	0.88	-0.76	0.00	0.95
	(0.85)	(0.71)	(0.89)	(0.71)	(0.69)	(0.74)
Number of Observations	2,535	2,534	2,531	2,519	2,525	2,182
			Pre-2004 Subperiod			
Unexpected Total Current Compensation	-0.20*	0.07	-0.09	-0.27*	-0.23	0.07
	(0.12)	(0.16)	(0.13)	(0.15)	(0.14)	(0.17)
Unexpected Total Direct Compensation	-3.73*	-0.09	0.87	-1.65	-0.36	1.17
	(2.03)	(1.45)	(1.94)	(1.45)	(1.43)	(1.73)
Number of Observations	1,278	1,279	1,278	1,271	1,272	1,016
			Post-2004 Subperiod			
Unexpected Total Current Compensation	-0.01	0.04	0.49***	0.40***	0.43***	0.31*
	(0.09)	(0.14)	(0.14)	(0.15)	(0.14)	(0.17)
Unexpected Total Direct Compensation	0.06	-0.03	1.22**	-0.11	0.14	0.52
	(0.42)	(0.49)	(0.51)	(0.48)	(0.48)	(0.57)
Number of Observations	1,257	1,255	1,253	1,248	1,253	1,166

Figure 1: Evolution of Outcome Variables over Calendar Years: Treated versus Potential Controls

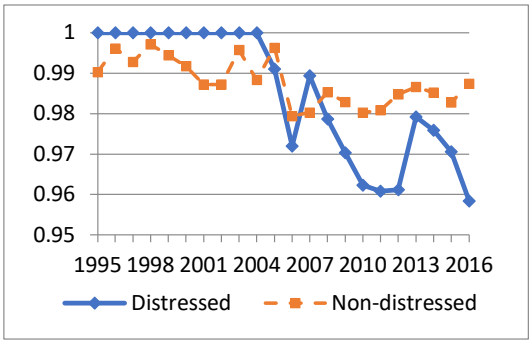
This figure plots the means of outcome variables measuring managerial influence over the board and CEO compensation over the calendar years. Firms are grouped by their treatment status. See Table 3 for definitions of the outcome variables. The sample period is 1992-2019, and the year of distress occurrence is from 1995 through 2016.



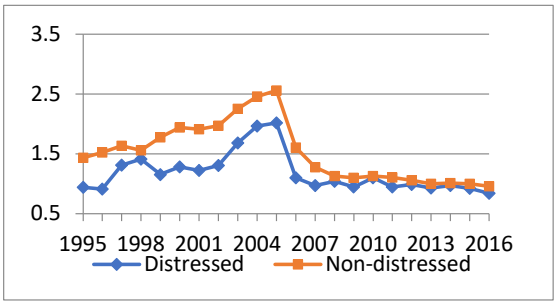
CEO Duality (in proportions)



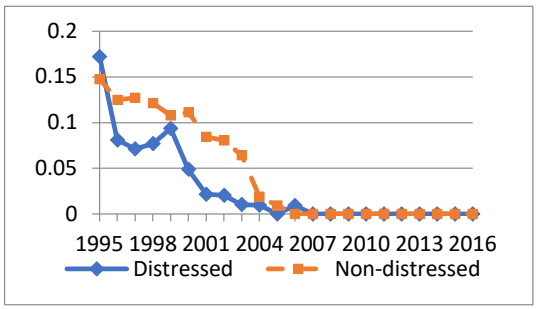
CEO Turover (in proportions)



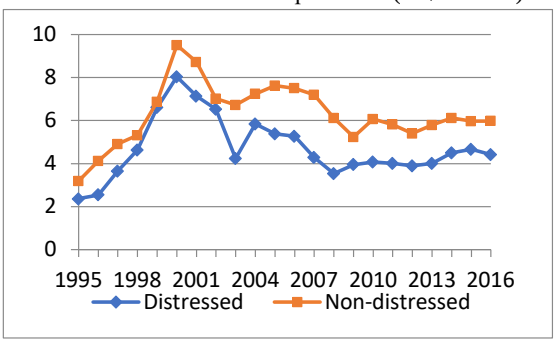
Inside Directors (in proportions)



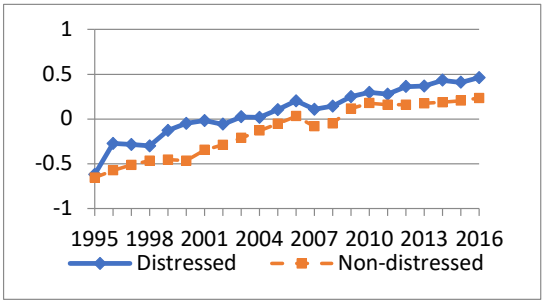
Total Current Compensation (in $millions)



Executives in Compensation Committee (in proportions)



Total Direct Compensation (in $millions)



Corporate Governance Score (in standardized units)