Busy Directors and Firm Performance: Evidence from Mergers

Roie Hauser

JOB MARKET PAPER

Nov 15, 2013

[Link to the latest version]

This paper studies whether director appointments to multiple boards impact firm outcomes. To overcome the endogeneity of the number of board appointments, I exploit variation generated by mergers that terminate entire boards. These mergers reduce board appointments of the dismissed directors and are thus used as a negative shock to their workload. I find that a reduction in director workload is associated with higher earnings, higher market to book ratios, and higher pay-performance sensitivity in CEO compensation contracts. Consistent with the hypothesis that director workload matters, the performance gains are particularly stark when directors are geographically far from firm headquarters, and when marginal value of directors' time and effort is high. In addition, directors are more likely to join board committees after losing seats on other boards, suggesting that they are able to devote more time to remaining boards. Finally, I find a similar effect on incumbent CEOs who hold additional board appointments.

Job market paper at the University of Chicago Booth School of Business. I am grateful to my dissertation committee: Steve Kaplan, Amit Seru, Kelly Shue, and Luigi Zingales for many helpful comments and discussions. I also benefited from the comments and suggestions of Yian Liu, Gregor Matvos, Margarita Tsoutsoura, and seminar participants at the University of Chicago. Email: roie.hauser@chicagobooth.edu

1. Introduction

The effectiveness of corporate boards as an internal mechanism of governance is questioned by skeptics. One common critique is that directors may be overcommitted and too busy to effectively fulfill their duties. A director's role of monitoring and advising management requires devoting substantial time and effort to gather information and make deliberate decisions. The criticism escalates when directors serve simultaneously on boards of multiple companies and their workload compounds. The National Association of Corporate Directors recommends that directors devote at least 160 hours per year for every board appointment. Lipton and Lorsch (1992) argue that the duties of a director demand at least 100 yearly hours per board appointment, exluding travel time[1]. Yet multiple directorships are not uncommon: more than 20% of directors in S&P1500 companies hold multiple board seats and more than 80% of S&P1500 firms share at least one director with other S&P1500 firms. The high workload and prevalence of multiple directorships spark a debate over the concern that busy board structures are inefficient and destroy value.

However, empirical identification of the effect of board "busyness" on firm performance is challenging since board busyness is endogenously determined. Firms carefully select directors to their boards, and thus firms who appoint busy directors may differ from firms who appoint less busy directors in many aspects that correlate to performance. Specifically, the firms that select busy directors may need particular director expertise more than director time (Field, Lowry, and Mkrtchyan, 2013)[2]. In addition, an omitted variable problem arises since complete details of a director's time-consuming activities are unobservable. While director busyness is typically proxied by the number of board seats (or some function of that number), directors may choose to reject board appointments based on prior commitments. Therefore, it is not obvious that directors with more board seats have less time to devote than directors with fewer board seats (Adams, Hermalin and Weisbach, 2010).

Given the challenges, the empirical evidence on the effect of board busyness is mixed and often contradictory. The endogenous selection of board appointments implies that the

[1] Yet since their study, the workload of directors has increased dramatically, especially post Sarbanes-Oxley (Linck, Netter and Yang, 2009). Due to these dramatic changes, the issue of the busy boards flared has in recent years.
[2] Coles, Daniel and Naveen (2008) and Linck, Netter and Yang (2008) emphasize that the effect of board structures is unlikely to be homogenous across firms.

busyness effect is entangled with the effect of director skill since most busy directors are predictably more qualified than less busy directors. Fama and Jensen (1983) and Kaplan and Reishus (1990) find that qualified directors are in high demand; they are pursued by many firms precisely for their high qualifications. A negative relation between board busyness and firm performance is documented by Fich and Shivdansani (2006), Shivdasani and Yermack (1999), Core et al. (1999), and Ahn, Jiraporn, and Kim (2010), while positive aspects of multiple directorship are documented by Field, Lowry, and Mkrtchyan (2013), Loderer and Peyer (2002), and Masulis and Mobbs (2011). In many of these studies, the relationship between board busyness and firm performance can be interpreted as a test for whether the busyness effect dominates the director quality effect (Adams et al., 2010). In this paper, I focus only on the busyness aspect. Instead of asking which effect dominates, I ask a complementary question: whether director busyness directly affects firm performance in a meaningful and central way.

The purpose of this paper is to identify the direct effect of director busyness on firm performance. To address the endogeneity issues, I exploit variation in director busyness induced by mergers. The empirical strategy uses mergers as a natural experiment that terminates directorships, in view of the fact that when two companies with two boards merge to one company, the vast majority of directorships in the acquired firm are terminated. Aside from rare cases[3], directors of the target firm "lose" their appointment (Harford, 2003). The merger presumably shocks those directors with extra time to devote to their other remaining directorships. The underlying premise is that directors are agents that optimize the time and effort they devote to their various commitments. If one commitment was to be exogenously removed, they are shocked with extra time and thus the marginal cost of exerting effort in all remaining commitments declines. As a result, they exert more effort in all directions. In particular, the extra time is spent on remaining directorships (which may add value to those firms). Following such shocks, I examine the performance of the *other* firms which continue to employ the affected directors. Econometrically, the advantage of this source of variation is that it allows for examining changes in outcomes as busyness varies, while absorbing firm and director characteristics.

[3] Harford (2003) finds that acquiring firms rarely appoint directors of the acquired firm to the merged firm's board. These rare cases usually have special circumstances such as a director who is a founder, and as such is very unlikely to be a multiple director. This finding is confirmed in my data sample as well.

I apply an instrumental variable approach that exploits the variation explained above. Specifically, I instrument for busyness with an indicator for employing a director which also served on a board of a firm that was acquired in a merger[4]. The identifying assumption is that the merger of two firms is independent of the prospects of a "third party" firm which is only involved in the merger through the fact that it shares directors with the acquisition target (other than through the channel of directors' busyness), conditional on any time invariant characteristics. A merger shock generates variation in busyness over time rather than cross section variation in board personnel[5]. In that sense, the regressions absorb director characteristics and isolate the effect of the shock. Under the identifying assumption, the tests difference out potentially confounding factors and common trends by using the firms in the sample that are not shocked as a control group[6] (though I also consider finer control groups in the robustness section[7]). In spirit, this setting compares changes in firm performance around the merger-induced reduction in board busyness, of firms with a shocked director on board (the treated group for this purpose) relative to firms (operating in the same industry at the same time) without shocked directors. Under the identifying assumption, that difference can be attributed to the effect of director workload.

Evidence that links board busyness and firm performance also speaks to the broader question of whether boards as a whole have a first-order causal effect on firms. While boards are modeled as important internal governance mechanisms to monitor management and align shareholder and manager interests[8], there is a lot of skepticism on whether the effect of boards is first-order (Yermack, 2006, Adams et al., 2010). A finding that board busyness hinders firm performance suggests that director do indeed add value and that board structures are more than just window dressing. This broader question is controversial since monitoring needs differ among companies, and board structures may already optimize firm value, making causal effects

[4] Specifically, the instrumental variable for director busyness is a dummy that indicates a merger-shock: it equals one if the firm employs on its board a director who served on a board of a different acquired firm, in the year of the merger.

[5] This approach is fundamentally different from prevailing studies which look at within-firm variation in busyness, since most of that within-firm variation reflects changes in board personnel (busy director replaced by less busy or vice versa), which bears both the effects of busyness and qualifications.

[6] That is, the control group is the S&P1500 firm years in which no board director has been involved in a merger.

[7] In the robustness section, I consider two alternative sets of control firms: (i) using only firms that are ever shocked, such that the firm-years before the shock are the control group; and (ii) I use a "nearest neighbor" propensity score matching method to match each treated firm-year to a control firm-year that is closest based on observables.

[8] For example, in Adams and Ferreira (2007) and Fama and Jensen (1983) and many others.

of boards difficult to identify and hinging on natural experiments. For that purpose, this paper turns to director busyness as a setting in which the natural experiment can be exploited to test whether boards matter.

I find that a reduction in directors' commitments is associated with improved performance of the companies they continue to serve. The effect appears whether performance is measured by market based measures (e.g. Tobin's q) or by accounting profitability measures (e.g. ROA), and after controlling for cross industry effects and time invariant firm and director characteristics. The findings are also robust to whether busyness is measured by board seats or by board committee memberships. The economic magnitude of the IV estimates is more conservative than documented in most previous studies[9], but is non-negligible: a reduction of one board appointment is associated with a 1.8% increase in Tobin's q, and a 0.5% increase in ROA. For the median S&P1500 company, these estimates suggest a removed board seat is worth roughly $10M in operating earnings or $30M in market value.

Moreover, I find a pronounced effect of a reduction in directorships when the terminated directorship is geographically distant from the individual's other directorships. This finding is important because of the direct link between geographical distance and a director's devotion of *time*, thus emphasizing the effect of workload and busyness. The importance of geographical distance to board monitoring has been documented by Alam et al. (2009). Busy directors who are also distant from company headquarters not only become even busier due to time spent on traveling, but also are less likely to interact between board meetings.

The instrumental variable setting allows me to address several identification concerns. Clearly, boards play an important role in approving mergers and the fact that directors approve a merger may signal something about the quality of those directors, implying a correlation between the instrument and director quality[10]. However, this correlation alone does not violate the identifying assumption, as long as director quality is time invariant. The regressions control for differences in director quality that are time variant or that do not systematically coincide with the

[9] For example, Field et al. (2013) estimate an effect of their busy board dummy on Tobin's q of up to 25% (of assets) and Masulis and Mobbs (2011) estimate a 9% effect for the presence of a "CID" multiple directorship.

[10] A takeover bid may be more likely to be approved by the target firm's board if that board consists of proper directors who are willing to put shareholder interests before their own. Conversely, a company may be more likely to be an appealing takeover target if the board is entrenched and acquirers can gain more value by taking over the assets and replacing the entire management.

timing of mergers. In addition, the instrument variable approach also addresses the omitted variable problem that arises since the number of board seats is an imperfect proxy for workload.

The instrumental variable setting also allows controlling for the possibility that the timing of mergers reflects merger waves in specific industries. Specifically, the concern is that the observed changes in firm performance are driven by merger waves rather than changes in board busyness. I rule out this concern in two ways. First, by controlling for industry-by-year fixed effects in every regression, the possibility that directors tend to serve firms in the same industry does not violate the identifying assumption[11]. Second, results are thoroughly similar in a robustness test that omits cases in which the acquired firm is in the same industry as the other firm, suggesting that the results are not driven by industry merger waves.

Overall, the framework allows controlling for any time invariant competing effect. Nevertheless, I consider the circumstances in which the exclusion restriction can be violated. These circumstances require that firms, which is only involved in the merger is through sharing directors with the acquired firm, systematically change exactly at the time of the merger.

An important concern which I address is that the estimates may capture the effect of takeover procedures rather than a pure causal effect of workload. In particular, I consider the plausibility of a "direct takeover effect" as a potential alternative explanation for the findings. Such an explanation would require that directors' part in a merger transaction directly leads to timely changes in their behavior. That could be the case if a director's skill set or incentives change with the takeover bid. A director's skill set might improve due to "learning by doing" and gaining managerial experience throughout the takeover process and negotiations. A director's incentives might also change due to a takeover disciplining effect[12]. Arguably, evidence on these direct takeover effects would be equally interesting from an empirical point of view, since even if takeover effects exist, this paper makes an important contribution: showing that directors do indeed matter for firm outcomes. However, I find little evidence to support direct takeover effects, while the additional evidence that I show supports the busyness interpretation.

[11] In addition, I show that there is very little industry overlap between directorships of one individual. For example, two firms which share a director are not more likely to be in the same industry (possibly due to competition).

[12] Takeovers have been argued to have disciplining effect on managerial behavior (e.g. Jensen and Ruback, 1983) which may apply to directors. Potentially, once a takeover bid takes place, directors become aware of future takeover threats and that awareness might directly affect their behavior.

I address the issue of direct takeover effect by exploiting geographical distances between directorships. Geographical distance is naturally linked to the time directors spend, but it is not as obviously linked to potential direct effects of takeover bids. Neither learning effects nor disciplining effects provide a good hypothesis for why a direct takeover effect would influence remote directors more than nearer directors. In addition, I address this concern in the robustness section by running a falsification test that exploits takeover bids that ultimately do not lead to the termination of the target board[13]. This falsification test shows that "placebo acquisitions" which do not impact directorship appointments (because the bid is withdrawn or the transaction is defined as an acquisition of partial interest in which the target firm continues to operate as a subsidiary of the acquiring firm), do not seem correlated with firm performance. Consistent with the busyness narrative, I find no evidence that takeover negotiations relate to performance in absence of changes in workload.

To further stress the empirical link between director busyness and performance, I study the interaction between changes in the number of board seats and factors that are plausibly linked to the time demand of a directorship. If shocks to workload affect performance, that effect should be pronounced when the marginal value of a director's time and effort is high. I rely on both theoretical and empirical literature to find proxies for these situations. I find that the negative relation between busyness and performance is particularly true in two subsamples in which more time and effort are likely required from directors: (i) directorships in firms with high analyst disagreement and (ii) directorships in their early years since appointment. To test the first, I study the interaction of changes in board appointments and the dispersion of forecasts made by analysts covering the firm. Disagreement among analysts indicates lack of available information about the firm, and suggests directors must work harder to become informed and evaluate the company's management and projects (Krishnaswami and Subramaniam, 1999). Therefore, analyst uncertainty and accuracy can proxy for the effort and time required by directors (Duchin et al., 2010). Consistent with this view, I find a strong relation between busyness and performance in firms with high analyst disagreement as measured by dispersion of analyst forecasts. To test the second, I study the interaction of changes in busyness with directorship tenure. This interaction is motivated by the premise that the role of a director is more time

[13] Seru (2013) and Li (2011) also exploit merger that ultimately failed or were withdrawn to better understand the effect of completed mergers.

consuming for newly appointed directors than for tenured directors. Yermack (2004) finds that most newly appointed outside directors join the board with limited knowledge of the company's operations or the issues it faces, and gain it within the first few years on the board. Consistent with the main findings, the hypothesis is that the effect of busyness is stronger when directorships are in their early years, and weaker after several years of service. I find evidence to support this hypothesis: the relationship between busyness and firm performance is especially stark in untenured directorships.

Next, I explore two potential mechanisms through which busyness can affect firm performance: director's ability and willingness to participate in committee assignments, and sensitivity of CEO compensation to stock performance. First, I explore the relationship between director busyness and the likelihood of committee membership. I find that busier directors are less likely to be assigned to board committees. Specifically, following a reduction of one board seat, directors are substantially more likely to join committees in each board they remain on (increase from 12% to roughly 16%). This evidence is important since a director's ability and willingness to serve on committees has been shown to be a mechanism in which busyness can affect firm performance (Jiraporn et al., 2009). Directors may be more influential as members of committees, since many monitoring related decisions such as auditing, governance, and executive compensation decisions are made in the board committee level (Adams and Ferreira, 2009). Second, I investigate the relation between director busyness and the sensitivity of CEO compensation to stock performance. Determining CEO compensation and negotiating CEO contract structures are some of the most important roles of boards. A strong tie between CEO compensation and firm performance suggests good governance and board effectiveness in aligning shareholder and CEO incentives (Jensen and Murphy, 1990, Kaplan, 1994)[14]. I find that a reduction in board appointments following a merger-shock is associated with greater pay-performance sensitivity, consistent with the baseline results of improved overall firm performance.

An interesting implication of the findings emanates from the fact mergers impose negative shocks to busyness, making directors *less* busy. A priori, the effect of positive and

[14] A strong sensitivity of CEO compensation to performance is widely considered a desirable feature for mitigating conflicts of interest between the CEO and shareholders. Since boards negotiate CEO compensation, high pay-performance sensitivity reflects favorable board effectiveness.

negative shocks to busyness need not be symmetric, since if directors were able to devote the maximum required effort to all their boards, a negative shock to busyness should not have an effect. My findings suggest that this is not the case: the strong response to negative shocks suggests directors are optimizing their efforts given binding time constraints. My main result is most related to recent findings of Falato, Kadyrzhanova and Lel (2013), who use a different natural experiment (death of board colleagues) to plausibly shock a director's attention away from one company[15]. In contrast, I study firm outcomes in circumstances when a director becomes substantially *less* busy: after a merger which completely terminates that director's other appointment. Although I study a different source of variation, my results complement the evidence of Falato, Kadyrzhanova and Lel. Their finding, that stock markets plunge when a director is predicted to become distracted, is consistent with my findings that a reduction in a director's workload can benefit firms operating earnings and CEO incentive contracts in the years that follow. In addition, my setting allows me to expand the analysis from director busyness to the effect of "executive busyness", by studying CEOs who lose board appointments in other firms.

Finally, if board appointments burden director workload, they may also impact other corporate executives who hold board appointments in additional companies. Perry and Peyer (2005) find that investors react negatively to the announcement of incumbent CEO appointments to boards of other firms. To expand the analysis of workload effects to corporate executives, I collect data on the smaller number of individuals who experienced merger-shocks while holding additional positions – not as directors – but rather as CEOs. I use a reduced form approach with this sample and find that performance of the firms whose CEOs are shocked by a terminated directorship significantly improved compared to firms that are not shocked. As might be expected, the magnitude of the effect on CEOs is much larger than the estimated effect of similar shocks to board directors.

The next section describes merger shocks and the sample. Section 3 presents the empirical results, and Section 4 presents additional robustness tests focusing on identification. Section 5 concludes.

[15] Falato, Kadyrzhanova and Lel (2013) use deaths of board colleagues as the source of variation in the attention a director gives his other appointments.

2. Sample Selection and Data Description

2.1 Directors and Boards Sample

I obtain data on boards and directors from RiskMetrics (formerly IRRC). RiskMetrics provides annual board snapshots of the companies that comprise the S&P1500[16] index. The database years are 1996 to 2011, in which the sample includes 212,918 directorship-year observations in 22,465 firm-years.

The main measure for director busyness is the number of board seats held by the director in that year. I obtain this variable by counting each director's appearances within the sample. Thus, the variable is limited to the firms in the sample and does not include director positions outside the S&P1500, such as directorships in private companies and smaller public companies[17]. The number of board seats variable needs to be corrected for the fact that firms' coverage by RiskMetrics may change as firms enter and exit the S&P1500 index. To correct for those cases, this variable is only defined in differences, and I follow the following procedure. For each director *j* on board *i*, the differenced variable, $\Delta\#Boards_{ijt}$, is the number of boards that appear in the sample at year t, with director *j*, and appear in year *t-1*, without director *j*, minus the number of boards that appear in the sample in year *t-1* with director *j*, and in year t without director j. In the robustness section, I construct a second proxy for busyness, $\#Committees_{ijt}$, which is defined as the total number of committee memberships within the directorships held by director *j*, (excluding committees in board *i* itself). This variable does not include membership in other committees other than the audit, compensation, and governance committees, since data is only available for these committees. I use a similar process to correct for firms' entrance and exit of the S&P1500 index, and this variable too is only defined as a difference ($\Delta\#Committees_{ijt}$).

The top panel of Table 3 presents the frequency of multiple directorships within the S&P1500 sample. The table shows the distribution of the number of board seats variable as well as the total number of board committee memberships in those boards. Multiple directorship is not uncommon, yet roughly four fifths of directors do not hold more than a single seat on S&P1500 boards, and holding more than three seats is rare.

[16] Approximately 1500 firms which consist of roughly 90% of US market cap, according to S&P website. Related, Ferris et al. (2003) find that multiple directorships are primarily a large firm phenomenon.

[17] An alternative to this measure is a variable provided by RiskMetrics but appears to be less accurate. I use that variable in some tests in the robustness section

I then aggregate the change in board seats to the firm level. For each firm-year, the aggregated firm level change in board appointments, $\Delta Boards_{it}$, is defined as the sum of $\Delta \#Boards_{ijt}$ over the directors of that board.

2.2 Identification of Merger-shocks

I obtain merger and acquisition data from the Thomson One (formerly SDC) Mergers and Acquisition database. I obtain all takeover bids applying the standard filters used in the literature, which exclude share repurchases, recapitalizations, exchange offers, owned portfolio companies, privatizations, and takeover bids that were withdrawn or whose status is not completed[18]. I use this list to identify the firm-years in which firms covered by RiskMetrics are a target of successful mergers. Following Jenter and Lewellen (2013), a merger is defined as a firm-year in which a takeover bid is completed and the fiscal year is the target firm's final year on Compustat and on RiskMetrics[19]. This procedure identifies 318 mergers of target firms in the RiskMetrics sample. Within the 318 target firms, 2928 directors served on the board in the last year before the merger, most of which did not hold additional directorships and thus are not part of the treated sample. In order to observe differences in outcome variables, I require that directorships persist through the year of the merger to the year after the merger. The final sample includes 1115 directorships with complete data, which are defined as the first year in which the director is free from a previously held directorship that was terminated due to a merger. I refer to these directorships as "shocked", and define the variable $Treat_{ijt}$ to be an indicator equal to one in these directorship-years and zero otherwise. Finally, the shock variable is aggregated to the firm level such that $Treat_{it}$ indicates firm-years in which one or more directors on the board has been shocked.

The bottom panel of Table 3 shows the distribution of changes in the number of board seats held, for the shocked directors (treated) and for the non-shocked directors. In most years, directors maintain the board seats the hold (85% of the cases for non-treated directorship-years).

[18] Additional filters used in the literature based on the value of the transaction are not needed for this purpose because any target firm in RiskMetrics would be of sufficient value.

[19] Specifically, similar to Jenter and Lewellen (2013) merger bid may be received during that year or within the next fiscal year, to ensure including cases in which the bid occurred after the end of the firm's last reported fiscal year.

In most cases of merger, the treated directors do lose a board seats within the fiscal year of the shock. Overall, a merger-shock shifts the distribution of the change in the number of board seats by roughly minus one compared to directorship-years that are not shocked.

Some shocked directors may decide to immediately accept a new directorship appointment to replace the terminated directorship. Even in those cases, they may have an extended period of reduced board duties during that year until the new appointment begins. Overall, the instrument is relevant since with high probability, the workload of shocked directors in acquired firms is higher before the merger takes place relative to after it takes place.

Figure 1 shows the average cumulative changes in the number of directorships held by shocked directors in event years surrounding merger-shocks. The reduction in the number of board seats held shows the variation generated by mergers as a shock to a director's busyness. Indeed, the number of board seat held by the shocked directors drops by 0.95 board seats, on average, in within the fiscal year the merger takes place ("year 0" in the figure), due to the termination of the board. In the subsequent years, the average number of board seats recovers only moderately suggesting only limited replacement of the terminated directorships. The figure also plots the average cumulative change in the number of committee memberships within the directorships held by shocked directors. Within the fiscal year that the merger takes place, the total number of committees drops, since committee memberships are clearly terminated when the board is terminated. However, in subsequent years the total number of committees does recover considerably. Interestingly, in the years following the loss of one board appointment, many directors may be taking on new committee appointments within their standing directorships. This trend is consistent with the premise that board appointments, as well as committee appointments, are linked to time allocation.

2.3 Firm-Level Data and Summary Statistics

I obtain the majority of firm-level financial characteristics from Compustat. I measure firm performance using two measures: Return on Assets (ROA), defined as operating income before depreciation divided by total assets, and Tobin's q, defined as the market-to-book ratio calculated as the market value of equity plus the difference between book value of assets and the book value of equity, divided by the book value of assets. For Tobin's q, I take logs so that the

estimated regression coefficients have a percentage interpretation. Both measures are reported as percentages throughout the paper. For the regressions, I winsorize the dependent variables at the 1st and 99th percentiles (the results are similar if I do not winsorize the variables), to ensure the results are not driven by a few extreme observations. Industries are classified by the Fama-French 49 industry classification. Regression control variables include factors previously found to correlate with performance, namely firm size (measured by the log of total assets), the number of directors on the board, leverage (measured by book value of debt divided by assets), the fraction of independent[20] directors on the board, volatility (measured over five years of monthly returns from CRSP), as well as investment in research and development in case firms with many investment opportunities exhibit larger Tobin's q.

Table 1 shows firm-level and directorship-level summary statistics in directorships-years. The empirical analysis relies on the comparison between firms with directors that are shocked (the "treated group" for this purpose) and firms whose directors are not shocked. A typical board in the sample includes 9.5 directors on average, 69% percent of which are classified as independent. The average tenure of directorship is 8.6 years with a median of 6 years.

Table 2 shows summary statistics for the firms that were acquired and the directorship that were shocked as a result, both at the last year before the merger. Ex ante, the treated and non-treated groups do not differ in terms of firm performance measures. On average, return on assets is 13% in both groups. Average Tobin's q is 1.89 and 1.82 among non-treated and treated firms (before the shock).

For the empirical tests that use geographical distances, I obtain distances between locations of firm headquarters. Since location of each director's residence is unavailable, for each shocked director (the treatment group only), I compute the distance between the headquarters of the firm whose board was dismissed (the acquired firm) and the other boards which that director continues to serve. By construction, this distance is defined only for the treatment group, since it requires a dismissed board. To compute the distances, I first obtain the zip codes of the headquarters of each firm from Compustat. Using the zip codes, I obtain the distances from the "Google Distance Matrix Api" which provides the distance between each pair

[20] Independent directors are defined as those classified as Independent in RiskMetrics' classification.

of relevant zip codes[21]. I then split the distances to define each shock as either "near" (below the median distance) or "far" (above the median distance). If the distance could not be computed by Google Api (e.g. there is no driving path between two headquarters) the shock was defined as a "far" shock, since these cases typically reflect headquarters outside of continental US. I define the variables $Treat^{far}$ and $Treat^{near}$ to indicate far and near shocks accordingly.

In the empirical investigation conditional on director tenure I define early years of directorships as those which the director has been on the board of the company for six years (the median tenure in the sample) or less. In the regressions, this definition is captured in the variable $I^{high\ tenure}$, an indicator that equals one for directorships with more than six years of tenure.

I obtain data on dispersion of analyst forecasts from "IBES" database summary file. Consistent with the literature, forecast dispersion is defined as the standard deviation of forecasts for upcoming quarterly earnings per share, scaled by the absolute value of the mean forecast. For each firm-year, I average that ratio for all quarterly summaries within the year. The indicator $I^{low\ disper}$ equals one if the dispersion is below the median in the directorship sample.

I obtain information on CEO compensation from ExecuComp. As a measure of CEO compensation I use the total compensation ("TDC1") which includes salary, bonuses, value of stock and option grants at the time they are awarded, and long term incentive payouts. It indicates values awarded to the CEO (not necessarily realized) and reflects board decisions rather than realized CEO wealth. I also obtain the age of the CEO from ExecuComp.

2.4 Shocked CEOs sample

To explore "CEO-busyness", I identify 177 "shocked" CEOs using the following procedure. I start with all (2928) directors who served on the boards that were terminated when the company was acquired. I use the firms listed in the RiskMetrics variable "Primary Employer" as candidate firm whose CEO could be shocked[22]. When available, I then also use RiskMetrics variable "Primary Title" to restrict the remaining cases to those in the "Executives" category or if

[21] The units provided by Google Distance matrix API are of "driving miles", rather than aerial distances or direct routes.

[22] I drop cases where Primary Employer is missing or not a corporation and cases where Primary Employer is the firm itself that was acquired in the merger.

the description contains the word "CEO". 486 individuals match these criteria, some of which are incumbent CEO in publicly traded firms. I then turn to hand collect information on these 486 individuals, in the following order. For firm-years covered by ExecuComp, I obtain the identity of the CEO from ExecuComp. For firms not included in ExecuComp, I obtain the individuals' biography from "Capital IQ" database[23]. Finally, I manually search for the remaining individuals (who cannot be found in ExecuComp or Capital IQ) in SEC filing in the "Edgar" system. These exact steps identify 177 CEOs of publicly traded companies who were shocked with directorship termination due to a merger from 1998 to 2011. Merging with Compustat/CRSP leaves 143 "shocked" firm-years with complete data. The dummy variable $Treat_{it}^{CEO}$ indicates a firm-year in this group.

These firms are generally not covered by RiskMetrics, and therefore board and director information is not available for this test. Since the "shocked" firms may be any firm in compustat, I estimate this part of the analysis in the universe of all compustat firm-years in 1998-2011.

3. Empirical Findings

3.1. Multiple Board Appointments and Firm Performance

The empirical design corresponds to the identifying assumption that a merger is independent of a firm which shares directors with the acquired firm, other than through the channel of directors' busyness, conditional on the factors we can control for. The instrumental variable approach limits the investigation to variation in board busyness due to merger-shocks. I control for time invariant unobserved effects in the firm level using first-differenced regression models. In addition, first-differencing effectively controls also for director-level time invariant unobservables, since each first-differenced observation requires valid data on two consecutive directorship-years. Therefore, the regressions by construction do not use variation created by changes in board personnel (directors joining or leaving the board). Additional regressors control for the possibility that these time varying factors may correlate with firm performance. All

[23] Capital IQ is used to determine if the individual was or was not the CEO of the company specified as the "Primary Employer" at the year of the event.

regressions include industry-by-year dummies to allow me to compare shocked and unshocked firms within the same industry at the same time. They control for the possibility that mergers occur in industries or years that firms perform well.

For the baseline empirical test, I estimate the following first-differenced instrumental variables specification:

$$\Delta Performance_{it} = \beta_1 \Delta \widehat{Boards}_{it} + \Delta Controls_{it} + f_t \times f_{industry} + \epsilon_{it} \quad (3.1)$$

Where:

$\Delta \widehat{Boards}_{it}$ are predicted values from first stage regressions on $Treat_{it}$ and a full set of controls, where the instrumental variable, $Treat_{it}$ indicates that a director on board i was on a board that was acquired and terminated between year *t-1* and year *t*. Firm Performance is measured by ROA and log of Tobin's q. Year and Fama-French industry classification effects (f_t and $f_{industry}$) as well as cross products are included to control for the possibility that events incidentally occurred in industries or years that outperformed during the sample period. Firm level controls include firm size, board size, the fraction of independent directors on the board, financial leverage, volatility and R&D expenses[24].

The first stage for the IV methodology is a regression of the change in number of directorships on an indicator for a merger (the instrumental variable). First stage regression results are presented in Column 5 of Table 4. As expected, this regression shows that the merger-shock predicts changes in directorships held. While the number of directorships drops on average by 0.9 directorships[25] for shocked directors, the conditional average is a drop of 1.02 directorships. The tabulated first stage regression estimates show how other factors predict changes in board seats. For example, directors in larger firms are more likely to accept new directorships than directors in smaller firm.

Columns 1 and 2 present "ols" estimates of the first differenced specification that can be compared to the instrument variable regression results. In these regressions, the change in performance measure is regressed on the change in directorships held, controlling for time invariant characteristics as well as factors that predict changes in directorships. Here, the source

[24] These are included in "differences", yet the results change very little if some or all the control variables are included in "levels".

[25] This can be seen in Figure 1, on the year of the shock (year 0).

of variation in multiple directorships is not limited to changes imposed by mergers. Estimates show the association between an unconditional change in directorship appointments and firm performance. Directorships end for various reasons, including liquidation or delisting of firms, replacement of unwanted directors, and resignation of directors who do not want to continue. These endogenous events bias the coefficients in columns 1-2, which correspond to the effect of busyness as well as various contaminating effects. The interpretation is difficult since the reason for termination (or appointment) of the directorship is unobserved, and prone to many plausible biases. In the sample, there is little evidence that relates unconditional variation in directorships held to firm performance measures. The "ols" estimates are small compared to the IV estimates suggesting the response to a reduced appointment due to merger is greater than the response to a reduction due to an unknown reason.

Columns 3 and 4 present the reduced form relationship between the outcome variables and the instrumental variable. I find that firms' profits and market to book ratios increase following a merger in which a director loses a seat on an acquired board, relative to other control firms operating in the same industry at the same time but whose directors did not experience such a merger event. The merger-shocks are associated with increases of 0.50 percentage points in return on assets and 1.76% in market to book ratio.

Columns 6 and 7 of Table 4 show the main results of the paper. The IV estimates for the coefficients on the change in number of board seats are negative, suggesting that increased board commitments are associated with lower performance. A reduction of one board seat is associated with 0.52% increase in ROA and a 1.82% increase in Tobin's q. In Column 8 firm performance is proxied by abnormal stock return. With abnormal stock return as a dependent variable, the coefficients estimates have the correct signs but are not statistically significant[26].

In terms of magnitude, these IV estimates are economically meaningful. For the median S&P1500 company, these estimates suggest a removed board seat is worth roughly $10M in operating earnings or $30M in market value. Interestingly, the estimates obtained from using variation around mergers are more conservative than the estimates of most previous studies on board busyness and firm performance, which exploit other sources of variation. A few notable examples, although not directly comparable, are Field et al. (2013), which find that S&P1500

[26] This is persistent throughout the paper. Stock returns appear to be a noisier series. I therefore focus on ROA and Tobin's q as firm performance measures.

boards with busy majorities (not just a single director) are associated with higher Tobin's q by 25% of assets. That effect is intended to include the advantages busy directors have to offer. Fich and Shivdasani (2006), find that Tobin's q is lower in busy boards by about 4% (of assets). Masulis and Mobbs (2011) study a very specific type of multiple directorship, and find that the presence of an outside directorship in another firm held by an inside director is, on average, associated with higher Tobin's q by 8.8% and higher ROA by 1.32%. The paper closest in spirit to mine is Falato, Kadyrzhanova and Lel (2013), where the treatment group in their identification experiment averaged an abnormal return of roughly -6% (depending on the specification) in the year following a shock to a director's attention. While variables are defined differently and thus not directly comparable, these examples suggest that the different aspects of multiple-directorship may relate to very large trends in ROA and Tobin's q. In that sense, my estimates are possibly conservative yet economically meaningful.

3.2 Director Busyness Conditional on Geographical Distances

I exploit variation in geographical distances between company headquarters to test more directly the busyness aspect of multiple directorship. Traveling distances are linked to director's time expenditure because of the travel time directors incur and because distant directors are less likely to interact between board meetings (Alam, Chen, Ciccotello, and Ryan, 2009). Obtaining soft information is more difficult for distant directors and they are likely to be more sensitive to excess workloads than directors who are nearer to their firms. I use the instrumental variable setting to examine the interaction of busyness with geographical distance from the terminated directorship.

Table 5 reports the results for these tests using data on location of company headquarters. For every merger-shock (the "treatment" group), I compute the geographical distance between the headquarters of the acquired firm (where a directorship was terminated) to the headquarters of firm i. I then define two subsample of the treatment group with respects to the median distance. For each subsample separately[27], I re-estimate the baseline specification (Eq. (3.1))

[27] The geographical distance is only defined for the "treated" group since it requires a terminated directorship. Thus, I split the sample into two subsamples instead of estimating a specification similar to Eq. (3.2).

where $Treat_{it}$ is defined to equal one only for directorships with below median distance (Columns 1-3) or only for those with above median distance (Columns 4-6). The point estimates suggest a stronger effect for changes in board appointments (0.78% in ROA, 3.6% in Tobin's q) when the source of variation is the acquisition of a relatively far firm, and a weaker effect when the source of variation is the acquisition of a relatively near firm (0.26% in ROA, 0.44% in Tobin's q). Yet the differences between far and near shocks are not quite statistically significant, as can be seen in Columns 7 and 8. Columns 7 and 8 show the reduced form specification including both the far and near treatment dummies.

3.3 Importance of Firm and Director Characteristics: Value of Director Time

To further study the busyness effect, I study the interaction between changes in the number of board seats and plausible factors associated with the marginal value of a director's time. If the improved performance is due to a shock that reduces workload and assigns extra time, then the effect of such a shock should be pronounced when the marginal value of a director's time is high. I rely on the literature to find proxies for these situations. I hypothesize that the negative relation between busyness and performance is particularly true in two subsamples in which more time and effort are likely required from directors: directors in the early years of directorship; and directors in firms with high analyst disagreement.

First, I study changes in busyness interacted with director tenure. This interaction is motivated by theoretical and empirical results that suggest that the role of a director is more time consuming for newly appointed directors than for tenured directors. It is widely recognized that directors' effectiveness strongly depends on the information that is available to them. A director's need for acquiring information in order to be effective is highlighted in theory (Harris and Raviv, 2008, Hermalin and Weisbach, 1998 among others) and empirical work (Duchin, Matsusaka, and Ozbas, 2010). However, Yermack (2004) finds that most newly appointed outside directors join the board with limited knowledge of the company's operations or the issues it faces, and gain it within the first few years on the board. In order to examine the intuition that the effect of busyness on performance depends on tenure on the board, I estimate the relation conditional on director tenure. Consistent with the main results, the hypothesis is that the effect of busyness is stronger when directorships are in their early years, and weaker after several years

of service. I find evidence to support this hypothesis: the relationship between busyness and firm performance is especially stark in untenured directorships while for longstanding directorships the effect is small in magnitude and statistically indistinguishable from zero. This finding is important also because it relates to the controversy regarding policies to restrict multiple directorships. The weak and insignificant effect of busyness on longstanding directorships suggests that, while busyness matters, it matters only to an extent, as there are ways directors can make up for it.

Second, I study changes in busyness interacted with dispersion of analyst forecasts. Duchin, Matsusaka, and Ozbas (2010) find that cost of information, as proxied by analyst uncertainty and accuracy, matters a great deal for effectiveness of directors. Presumably, when obtaining information is relatively difficult, it requires more time from directors, and may make those who cannot devote time less effective. If so, the effect of busyness should be strong in firms with relatively high difficulty of gathering information. I test this hypothesis by using disagreement and dispersion of analyst forecasts to proxy for director difficulty to gather information.

In the instrumental variable framework, the specification I estimate is:

$$\Delta Performance_{it} = \Delta \widehat{Boards}_{it} + I_{it} + (\Delta \widehat{Boards} * I)_{it} + \Delta Controls_{it} + f_t \times f_{industry} + \epsilon_{it} \quad (3.2)$$

where I_{it} is either $I_{it}^{high\ tenure}$ or $I_{it}^{low\ dispers}$, and the rest of the notation is as before.

Table 6 displays the results. In both models, I split the sample to high and low groups with respect to the median. That is, I define the indicator variables $I_{ijt}^{high\ tenure}$ to equal one if the director j has been on board i for longer than the median tenure (six years) and aggregate it to form $I_{it}^{high\ tenure}$, and $I_{it}^{low\ dispers}$ to equal one if dispersion of analyst forecasts for earnings of firm i are less than the median dispersion.

In the regressions with an interaction with the directorship tenure indicator (Columns 1 and 2), the coefficient on $\Delta Borads$ represent the effect of busyness during the early years of directorships, and the coefficient on the interaction, $\Delta Boards * I^{high\ tenure}$, represents the effect of busyness for tenured directorships. The estimates are consistent with the predictions and significantly higher than unconditional effects (shown in Table 4). Additionally, the sum of coefficients on $\Delta Boards$ and $\Delta Boards * I^{high\ tenure}$ represents the effect of busyness for tenured

directorships. Estimates for this effect are significantly smaller, both in magnitude and are statistically indistinguishable from zero. This finding may be important for the policy controversy regarding restrictions on number of boards, since it suggests the negative effect of busyness can be compensated for, and are possibly temporary. Also in Table 6, Columns 3 and 4 show the interaction of busyness and analyst forecast dispersion. Consistent with the hypothesis, the point estimates for the effect of busyness are greater in firms with high dispersion than for firms with low dispersion, although statistically I cannot reject their equality.

3.4 Changes in Board Inputs and Behavior

In this section I explore two mechanisms through which directors can affect firm performance: director's ability and willingness to participate in committee assignments, and sensitivity of CEO compensation to stock performance. One mechanism that has been shown to relate to firm performance is participation of directors in the important board committees (Jiraporn et al, 2009). But committee membership requires directors to devote substantial time and effort. Another way boards can contribute to firm performance is by setting CEO compensation to effectively match CEO's performance, since a sensitive CEO contract can help align manager and shareholder incentives (Jensen and Murphy, 1990). But it requires directors to monitor the CEO very closely so they know and understand the CEO decisions – monitoring which requires a lot of time and effort from directors. The following tests provide evidence on *how* director busyness might impact board behavior and the quality of corporate governance. This section traces down improved director behavior in these two aspects, consistent with the finding that shocked firms perform better, and consistent with the hypothesis that the shock frees up some time for directors.

3.4.1 Subsequent Committee Membership

I examine a possible channel through which directors can affect firm performance: their ability and willingness to participate in committee assignments (Jiraporn et al., 2009). Directors may be more influential as members of committees, since many monitoring related decisions

such as auditing, governance, and executive compensation decisions are made in the board committee level (Adams and Ferreira, 2009). To examine the effect of busyness on committee membership I estimate the following instrumental variables specification:

$$I_{ijt}^{join\_commit} = \Delta \widehat{Boards}_{ijt} + \Delta Controls_{it} + f_t \times f_{industry} + \epsilon_{ijt} \quad (3.3)$$

Where *i* indexes firms, *j* indexes directors and $\Delta \widehat{Boards}_{ijt}$ are predicted values from first stage regressions on $Treat_{ijt}$ and a full set of controls, where the instrumental variable, $Treat_{ijt}$ is a dummy indicating that director *j* was on a board that was acquired and terminated between year *t-1* and year *t*, and the rest of the notation is similar to Eq. (3.1). The dependent variable, $I_{ijt}^{join\_commit}$, is an indicator equal to one if director *j* obtained new membership[28] in board *i*'s audit, compensation, or governance committees. The dependent variable is in the directorship-year level; it indicates that the very director that was shocked joins a committee (not just any firm-level committee membership change).

Table 7 shows results for estimation of Eq. (3.3) and the corresponding reduced form specification[29]. The table documents increased likelihood of directors joining committees in the directorships they still hold following the merger that shocks their board appointments. Specifically, reduction of one directorship appointment is associated with roughly 4% higher likelihood for directors to join a committee of the boards they continue to serve. That increased likelihood is economically meaningful compared to the unconditional probability of a director (shocked or not shocked) joining one or more of the committees. That unconditional probability is approximately 12 percent[30]. Moreover, the studies that show a positive relationship between committee membership and firm performance raise the question of why firms do not assign more memberships. The finding that less busy directors are more inclined to accept memberships suggests that one reason might be the workload that directors face.

[28] Omitting cases in which the director both joins a committee and leaves another committee.
[29] Namely, $I_{ijt}^{join\_commit} = \beta_1 Treat_{ijt} + f_t \times f_{industry} + \epsilon_{ijt}$, including a full set of controls.
[30] For a director who is on the board in both year t-1 and year t.

3.4.2 Sensitivity of CEO Compensation to Stock Performance

A strong tie between CEO compensation and firm performance suggests good governance and board effectiveness (Jensen and Murphy, 1990, Kaplan, 1994). Ideally, vigilant directors are well positioned to observe and evaluate managerial conduct early on, rather than relying on hard information and lagged outcomes. Accordingly, if director busyness impedes monitoring effectiveness by the board, then CEO compensation should be less sensitive to performance when directors are busy. I investigate the relation between director busyness and the sensitivity of CEO compensation to stock performance using the following empirical specification:

$$\begin{aligned} \Delta CEOcompen_{it} = {} & \beta_1 \Delta \widehat{Boards}_{it} + \beta_2 StockPerformance_{it} + \beta_3 (\Delta Boards * \widehat{StockPerformance})_{it} \\ & + \beta_4 StockVolatility_{it} + \beta_5 \Delta RoA_{it} + \beta_6 \Delta logQ_{it} + \Delta Controls_{it} + f_t \times f_{industry} + \epsilon_{it} \end{aligned} \quad (3.4)$$

Where similar to before, $\Delta \widehat{Boards}_{it}$ and $(\Delta Boards * \widehat{StockPerformance})_{it}$ are predicted values[31] from first stage regressions on $Treat_{it}$ and $Treat_{it} * StockPerformance$ and a full set of controls. $CEOcompen$ is the value (at the time it was awarded[32]) of the total compensation received by the CEO of firm i during year t. Stock performance is measured by the stock return adjusted to industry similar to Kaplan and Minton (2012), yet the results are robust to not applying the industry adjustment[33]. In addition to all control variables included in Eq. (3.1), I control for the outcome variables in Eq. (3.1), namely changes in operations performance and market to book. The coefficient β_2 represents sensitivity of CEO compensation to stock performance, such that the coefficient β_3 represents the effect of busyness on that sensitivity. I interact the treatment specifically with stock performance since it appears to be the measure that CEO compensation responds strongest to. Result are essentially the same if interactions with ΔRoA, $\Delta logQ$ are added. Following Masulis and Mobbs (2011) and others I include an interaction with stock return and include ΔRoA, $\Delta logQ$ as controls.

[31] Note that this is not a "forbidden regression", but rather the appropriate alternative (Wooldridge, 2002).
[32] In particular, it is the value the board gives to the CEO, not the realized value.
[33] Unlike earlier regressions, here the industry adjustment might be important (rather than just relying on the industry dummies) because the stock performance variable appears with an interaction. The industry adjustment applies before the variable is interacted with the treatment.

Table 8 shows results for estimation of Eq. (3.4) and the corresponding reduced form specification[34]. Not surprisingly, CEO compensation is positively sensitive to all three performance measures. Among the three, the sensitivity to stock return is the strongest both in economic magnitudes and statistical significance. That sensitivity is higher when board appointments decrease. In terms of magnitude, a CEOs additional reward for improved performance is the inner product of the three components of performance. For example, to gauge the economic magnitude of the difference, the following is a very rough calculation. If every 1% increase in stock return would (for simplicity) covary with, say, 1% increase in market to book ratio and operating income, the average CEO would be paid approximately an additional 40 thousand to the CEO's total compensation. After a board director is shocked with one terminated external directorship, that sensitivity is on average higher, approximately 51 thousand per each 1%.

3.5 Busyness of CEOs

To broaden the central finding on the effect of director busyness, in this section I ask if "CEO busyness" (external appointments of CEOs to boards of other companies) relates to performance of CEOs. I exploit the fact that some directors who experienced directorships termination due to a merger are CEOs of publicly traded companies. I assume the merger also shocks the busyness of those CEOs and apply a reduced form approach by estimating the following specification:

$$\Delta Performance_{it} = \beta_1 Treat_{it}^{CEO} + \Delta Controls_{it} + f_t \times f_{industry} + \epsilon_{it} \quad (3.5)$$

Table 9 presents the results. The firms of the 143 shocked CEOs significantly improved in both operating profitability and market to book ratios compared to firms of CEOs who were not shocked. Economically, the effect of CEO busyness is much stronger than director busyness. The larger magnitudes are sensible in terms of the fact that CEO is a more influential role in a company. Point estimates in Table 9 are well above twice the magnitudes of the corresponding reduced form estimates in the director sample.

[34] Namely, $\Delta CEOcompen_{it} = Treat_{it} + StockPerformance_{it} + Treat_{it} * StockPerformance_{it} + \Delta Controls_{it} + \Delta RoA_{it} + \Delta logQ_{it} + \epsilon_{ijt}$

4. Robustness Checks

I conduct three additional tests to check the robustness of the findings and their interpretation as a workload effect. First, I study the performance of firms which share directors with firms that were a target of an acquisition bid that did not lead to termination of the target board, either because the bid is withdrawn or the company is acquired and continues to operate as an entity with a board. The purpose of this analysis is to serve as a useful "placebo" test with potential to capture a direct takeover effect. Second, I use the total number of committee memberships within the directorships held as an alternative proxy for busyness. This proxy stresses the link between the merger-shocks and workload since committee memberships are time consuming and much of a director's work is done in committees. Third, I consider specifications in which the treatment group is compared to finer control groups: restricted to firms that are ever shocked and looking only at firm-years before the shock; and firm-years that are closest to the treatment group based on the "nearest neighbor" propensity score matching method.

4.1 Direct Takeover Effect and Withdrawn Takeover Bids: A Falsification Test

This section addresses the potential direct effect of takeover bids on directors of acquired firms. Such an effect may apply if the takeover proceedings (or just a bid for takeover) provide the directors of the acquired firm with new incentives or with learning opportunities that improves their managerial decision making on other boards. A direct takeover effect is relevant to the study of busyness because it suggests a confounding channel through which merger-shocks may relate to the operation of other firms which share directors with an acquired firm. The purpose of this test is to assess if directors gain valuable experience or incentives from being on a board of a company dealing with takeover negotiations and the proceedings that lead to merger. To proxy for a direct effect of takeover, I use merger bids that do not lead to the termination of the acquired firm's board. These include bids which were ultimately withdrawn,

or acquisitions in which the acquired firm continues to exist post acquisition as an entity that has its own board[35].

These bids are interesting since they provide a "falsification test" for the busyness interpretation of the main results. If my key findings were driven by factors specific to mergers and acquisitions, we might expect a comparable effect for merger which do not reduce directorships. Yet I do not find evidence for such an effect.

To test this hypothesis, I use merger bids that generally do not lead to the termination of target boards in lieu of the usual merger-shocks. The apparatus is otherwise similar to the baseline reduced form regressions[36]: I estimate the relation between acquisitions and performance of firms who share directors with acquisition targets. First, I obtain all bids to acquire firms in the director database. Unlike the construction of the merger-shocks, here I do not require that the bid is categorized as completed, and allow it to be either "completed" or "withdrawn". Second, instead of requiring that following the bid the target firms cease to exist as an entity with a board, I require the opposite: the target firm does not cease to have its own board. This might occur if the transaction agreed by the target and acquirer is withdrawn, or if the transaction is completed and the acquired firm continues to operate as a subsidiary of the acquiring firm[37]. Despite no immediate board termination, these targeted firms may be prone to a merger at a later occasion, and their directors are prone to be discharged shortly after. To avoid these cases, I exclude directors whose number of directorships held changes in the year of the takeover bid. These steps define 51 bids in which 618 directors were on boards. Those of them with multiple board seats define 222 firm-years which are shocked with a "placebo shock".

I define a dummy variable $NonTreat_{it}$ to indicate that one or more directors of firm i were on the board of a firm which received such an acquisition bid during the year that ends at time t. Using the $NonTreat_{it}$ variable in place of the usual merger-shock dummy variable $Treat_{it}$, I re-estimate the baseline reduced form regressions. The specification is

$$\Delta Performance_{it} = NonTreat_{it} + \Delta Controls_{it} + f_t \times f_{industry} + \epsilon_{it} \quad (4.1)$$

[35] Seru (2013) and Li (2011) also exploit merger that ultimately failed or were withdrawn to better understand the effect of completed mergers.
[36] Table 4, columns (3) and (4).
[37] In particular, the firm continues to appear after the transaction in Compustat and its board continues to appear in RiskMetrics.

This test provides a falsification test in the sense that it examines directors who are involved in merger bids but whose busyness is not shocked. For this purpose, the experience (or incentives) gained through proceedings of an acquisition that does not terminate the target's board proxies for the experience (or incentives) gained through an acquisition that does terminate the target's board. To the extent that the experiences are comparable[38] in both types of events – $Treat$ and $NonTreat$ – this test may capture the direct takeover effect net of the busyness effect.

If the busyness effect is driving the results rather than a direct takeover effect, I should not find evidence of a relationship between the "placebo-shocks" and performance of firms that share directors. Columns 1 and 2 in Table 10 show exactly that: I find no evidence of such a relationship in neither operating profits nor market to book ratios. The estimated relationship between firm performance and "non-shocks" is small. Statistically, it is indistinguishable from zero. Clearly, this is a falsification test in that the interpretation is merely a failure to reject the irrelevance of placebo shock, rather than statistically proving that placebo shocks have no effect.

To that extent, the identification concern of direct takeover effects seems less plausible. If the placebo shocks would predict changes in performance of firms who share directors with the acquisition target, it could be seen as evidence consistent with the hypothesis that directors gain experience through the acquisition process and apply their improved experience in other firm – an interesting result for its own sake. However, I find no evidence to support this alternative story.

4.2 Measurement of Busyness

Columns 3-5 of Table 10 show results for the baseline IV regressions using an alternative proxy for director busyness. Instead of the usual measure, the number of directorships, I use the number of committee memberships within the directorships held. Specifically, I use the number of memberships in audit, governance, and compensation committees (the committees for which data is available). The motivation for the additional measure is that committee membership correlates to a director's involvement in the board, which is not necessarily uniform across

[38] However, it is possible that directors obtain different skills from the negotiations of the incomplete mergers.

directorships. Committee members carry more liabilities and have more time-demanding roles, which include more responsibilities and more meetings to attend. It is not uncommon that the majority of a director's workload is done in committees. To capture the director's workload and time consuming commitments, I use both busyness measures to proxy for busyness. Beyond a robustness check, an advantage of the committee membership proxy is the stronger link to time and effort, in order to stress the relation between the merger-shocks and workload. The results in Table 10 support this interpretation.

4.3 Alternative Sets of Control Firms

The empirical strategy relies on companies whose directors are not shocked by merger events as a control group. The control group allows differencing out factors that potentially predict changes in firm performance, as well as common trends in time and industry. To test the robustness of the results, in this section I consider alternative sets of control firms. First, I limit the sample to those firms that are ever shocked, and use the firm-years before the shock as a control group. Second, I use a "nearest neighbor" propensity score matching method to match each treated firm-year to a control firm-year that has the closest propensity scores.

Table 11 present the results of these tests. Columns 1 and 2 present regression result for the baseline reduced form specification of change in firm performance on the indicator for a merger shock (and the usual control variables and fixed effects). The specification is similar to the baseline reduced form regressions in Table 4, but with a limited sample as follows. All firms that were not shocked at any year through the sample are not used in this regression. For the firms that are shocked, I omit all firm-year observations after the shock (if firms are shocked more than once through the period, I omit them as well). With the remaining observations, the treatment group is firms in the year they are shocked by a merger, and the control group is the firm-years of those shocked firms before the shock. Regression coefficients show that, on average, change in performance was higher for shocked firm-years relative to non-shocked firm-years of those companies, although the standard errors of the estimates are, as expected, large such that differences are not statistically significant at a 5% level[39]. Similar to the main results of

[39] Which can be expected given the lower number of observations and lower power.

the paper, the point estimates produce positive numbers, suggesting an improvement in performance following a merger-shock relative to other years of the same firm.

Columns 3 and 4 in Table 11 present the "Average Treatment Effect on the Treated" (ATT) using the nearest neighbor matching estimator. Comparing the treatment group to a control group of matching firm-years based on observables produces positive estimates, suggesting an average improvement in performance of shocked firm relative to non-shocked firm with similar observable characteristics (although the differences are only marginally significant statistically). The propensity scores are computed using firm level observable including industry and year, and the control variables used in the regressions throughout the paper40. The reported ATT estimates are based on a single closest neighbor matching (n=1) but are robust to using more than one neighbors per treated firm-year.

5. Conclusion

This paper exploits variation in board appointments due to mergers to examine the direct impact of multiple directorship. The main finding is that a reduction in board appointments is associated with improved operating profits and higher market to book ratios. Estimates indicate an average increase of 0.5 percentage points in return on assets and an average increase of 1.8% in market to book ratios with one terminated directorship. For the median S&P1500 company, these estimates suggest a removed board seat is worth roughly $10M in operating earnings or $30M in market value. I ask not only whether multiple directorship matters, but also when a reduction in a director's board appointments can benefit firm performance. Reductions in board appointments are particularly beneficial when directorships are geographically remote, when directorships are in their early years, and for firms whose analyst forecasts are relatively dispersed. Around merger-shocks, reductions in board appointments are also associated with increased director involvement through board committee participation and with CEO compensation decisions that better match CEOs performance.

---

[40] Industry-by-year interactions cannot be included in this specification. The matching is based on the control variables in levels rather than differences.

More broadly, the finding that firm performance responds to merger-shocks speaks to the broader question of whether boards matter at all. I interpret the findings as an effect of director busyness and workload, for the following reasons. First, the effect is particularly stark when interacted with geographical distance, a factor that has a clear link to time expenditure by directors. Second, the corresponding improvements in CEO pay-performance sensitivity and in board committee participation are channels through which directors can devote effort and time to contribute to their companies. Third, the effect of reduced board appointments is high when marginal value of directors' extra time is expected to be high, as proxied by high analyst disagreement and newly appointed directorships. Fourth, similar results appear when busyness is measured by membership in board committees – a proxy designed to capture the fact that a majority of directors' workload is done in committees. Fifth, a falsification test designed to capture potential direct effects of merger bids (net of the effect of busyness) does not produce evidence that supports an alternative direct takeover effect. I investigate the empirical relevance of alternative explanations such as a direct takeover effect, but find that the collective evidence best supports the busyness interpretation. The results are further supported by the finding that among CEOs who hold board appointments in other firms, reduced board appointments are also associated with performance gains, and the effect is much stronger for CEOs.

This paper focuses on the workload and busyness aspects of multiple board appointments, and does not intend to compare these effects to the potential welfare benefits of unrestricted utilization of popular directors. Yet the literature on multiple directorship has identified several benefits, such as denser networks[41], enhanced cross firm experience, and repeated use of a director's skills and expertise. Under the identifying assumption that the acquisition of one firm is independent of the prospects of other firms with which it shares directors (other than through the busyness channel), this paper isolates the direct effect of multiple board appointments from the entangled director characteristics. The instrumental variable approach is fundamentally different than the prevailing literature on board busyness[42].

[41] Andres and Lehmann (2010), Cai and Sevilir (2012), among others.
[42] Therefore, this paper is not at odds with the previous papers that find positive effects of multiple directorships or papers that study the impact of director networks.

As such, this paper does not directly address the debate on restricting multiple board appointments[43]. However, some of the new findings may provide useful insight that relates to the controversy. The findings that a busyness effect applies particularly to directorships in their early years and to directorships that are geographically remote, suggest useful guideposts which the policy debate can take into account, as well as companies considering board candidates. To the best of my knowledge, these aspects of director busyness have not been previously documented. These findings suggest that although the effect of busyness is meaningful, it is not universally irrecoverable: other factors can make up for board busyness. This interpretation is consistent with many theories[44] that associate directors' effectiveness with information available to them. Intuitively, directors can be more informed by: spending more time and effort, being near company headquarters, or relying on a long tenure of continuous service. If so, these factors might be interchangeable (and perhaps temporary given the latter). The time and effort spent by directors are means to obtain the information that makes directors effective.

[43] Therefore, the results do not imply that intervening with firms decisions by limiting the number of directorships allowed is optimal.
[44] Such as Harris and Raviv (2008), Hermalin and Weisbach (1998), Rehaja (2005) and many others.

References

Adams, R.B., and D. Ferreira (2007). A theory of friendly boards. *Journal of Finance* 62, 217–250.

Adams, R.B., and D. Ferreira (2009). Women in the board room and their impact on governance and performance. *Journal of Financial Economics* 94, 291–309.

Adams, R.B., B.E. Hermalin, and M.S. Weisbach (2010). The role of boards of directors in corporate governance: A conceptual framework and survey. *Journal of Economic Literature* 48, 58–107.

Ahn, S., P. Jiraporn, and Y.S. Kim (2010). Multiple directorship and acquirer returns. *Journal of Banking and Finance* 34, 2011-2026.

Alam, Z., M. Chen, C. Ciccotello, and H.E. Ryan Jr. (2009). Distance, information acquisition, and monitoring by the board of directors. Working paper, Georgia State University.

Andres, C., and M. Lehmann (2010). Is busy really busy? Board governance revisited. Working paper, University of Mannheim.

Cai, Y., and M. Sevilir (2012). Board connections and M&A transactions. *Journal of Financial Economics* 103, 327–349.

Carpenter, M.A., and J.D. Westphal (2001). The strategic context of external network ties: examining the impact of director appointments on board involvement in strategic decision making. *Academy of Management* 44, 639-660.

Coles, J.L., N.D. Daniel, and L. Naveen (2008). Boards: does one size fit all? *Journal of Financial Economics* 87, 329–356.

Core, J.E., R.W. Holthausen, and D.F. Larcker (1999). Corporate governance, chief executive officer compensation, and firm performance. *Journal of Financial Economics* 51, 371-406.

Duchin, R., J.G. Matsusaka, and O. Ozbas (2010). When are outside directors effective? *Journal of Financial Economics* 96, 195–214.

Falato, A., D. Kadyrzhanova, and U. Lel (2013). Distracted directors: does board busyness hurt shareholder value?. *Journal of Financial Economics*, Forthcoming.

Fama, E.F., and M. C. Jensen (1983). Separation of ownership and control. *Journal of Law and Economics* 26, 301-325.

Ferris, S.P., M. Jagannathan, and A.C. Pritchard (2003). Too busy to mind the business? Monitoring by directors with multiple board appointments. *Journal of Finance* 58, 1087–1111.

Field, L., M. Lowry, and A. Mkrtchyan (2013). Are busy boards detrimental? *Journal of Financial Economics* 109, 63–82.

Fich, E.M., and A. Shivdasani (2006). Are busy boards effective monitors? *Journal of Finance* 61, 689-724.

Harford, J. (2003). Takeover bids and target directors' incentives: the impact of a bid on directors' wealth and board seats. *Journal of Financial Economics* 69, 51–83.

Harris, M., and A. Raviv (2008). A theory of board control and size. *Review of Financial Studies* 21, 1797-1832.

Hermalin, B.E., and M.S. Weisbach (1998). Endogenously chosen boards of directors and their monitoring of the CEO. *American Economic Review* 88, 96-118.

Jensen, M.C., and K.J. Murphy (1990). Performance Pay and Top-Management Incentives. *Journal of Political Economy* 98, 225-264.

Jenter, D., and K. Lewellen (2012). CEO preferences and acquisitions. Working paper, Stanford University.

Jiraporn, P., M. Singh, and C.I. Lee (2009). Ineffective corporate governance: director busyness and board committee memberships. *Journal of Banking and Finance* 33, 819–828.

Kaplan, S.N. (1994). Top Executive Rewards and Firm Performance: A Comparison of Japan and the United States Source. *Journal of Political Economy* 102, 510-546.

Kaplan, S.N., and B.A. Minton (2012). How has CEO turnover changed? *International Review of Finance* 12, 57–87.

Kaplan, S.N., and D. Reishus (1990). Outside directorships and corporate performance. *Journal of Financial Economics* 27, 389-410.

Krishnaswami, S., and V. Subramaniam (1999). Information asymmetry, valuation, and the corporate spin-off decision. *Journal of Financial Economics* 53, 73–112.

Li, X. (2011). Productivity, restructuring, and the gains from takeovers. Working paper, University of Michigan.

Linck, J.S., J.M. Netter, and T. Yang (2008). The determinants of board structure. *Journal of Financial Economics* 87, 308–328.

Linck, J.S., J.M. Netter, and T. Yang (2009). The effects and unintended consequences of the Sarbanes-Oxley Act on the supply and demand for directors. *Review of Financial Studies* 22, 3287–3328.

Lipton, M., and J. W. Lorsch (1992). A modest proposal for improved corporate governance. *The Business Lawyer* 1, 59-77.

Loderer, C., and U. Peyer (2002). Board overlap, seat accumulation and share prices. *European Financial Management* 8, 165-192.

Masulis, R.W., and S. Mobbs (2011). Are all inside directors the Same? Evidence from the external directorship market. *Journal of Finance* 66, 823-872.

Perry, T., and U. Peyer (2005). Board seat accumulation by executives: A shareholder's perspective. *Journal of Finance* 60, 2083-2123.

Raheja, C.G. (2005). Determinants of board size and composition: A theory of corporate boards. *Journal of Financial and Quantitative Analysis* 40, 283-306.

Seru, A. (2013). Firm boundaries matter: evidence from conglomerates and R&D activity. *Journal of Financial Economics*, Forthcoming.

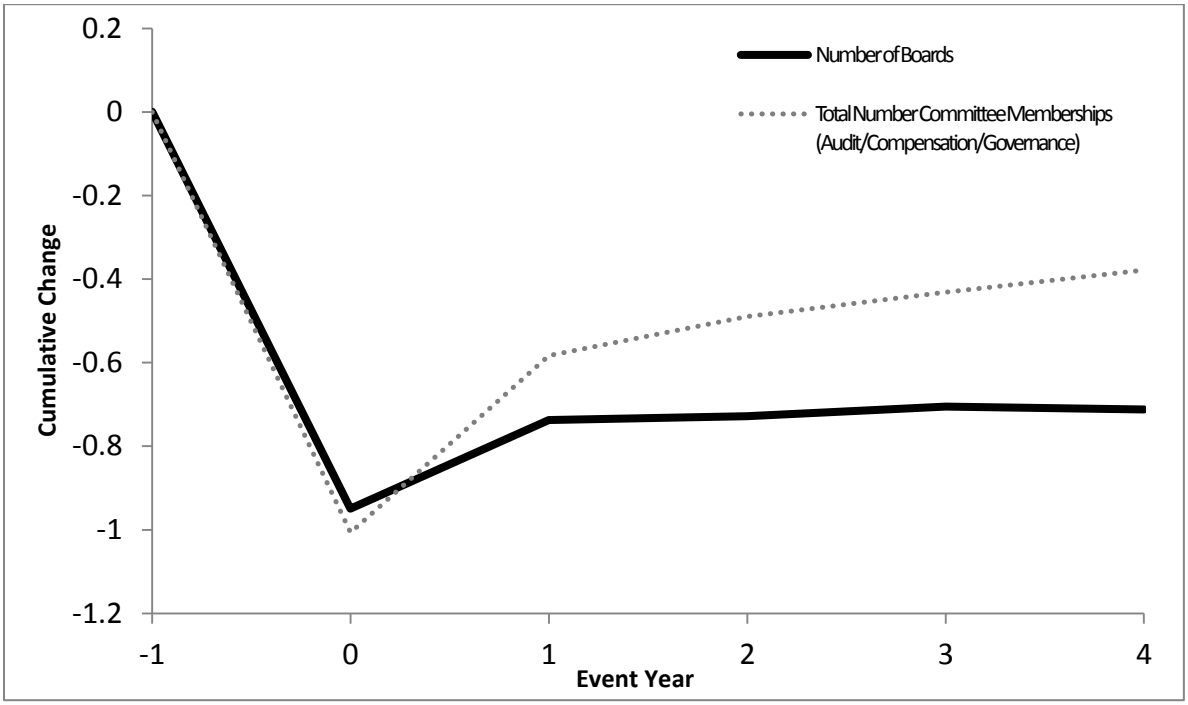
Shivdasani, A., and D. Yermack (1999). CEO involvement in the selection of new board members: An empirical analysis. *Journal of Finance* 54, 1829-1853.

Wooldridge, J.M. (2002). Econometric analysis of cross section and panel data. MIT Press.

Yermack, D. (2004). Remuneration, retention, and reputation incentives for Outside Directors. *Journal of Finance* 5, 2281-2308.

Yermack, D. (2006). Board members and company value. *Financial Markets and Portfolio Management* 20, 33-47.

FIGURE 1 – Cumulative Change in Total Directorships and Committees Held by Event Year



This figure shows average cumulative changes in the number of directorships held and the total number of board committee memberships within all directorships held. The cumulative changes are averaged over directors who served on a board of a company that was target of a successful merger (the "treated" group). Year 0 is defined as the first directorship-year after the completion of the merger, such that year -1 is that last year in which the target firm appears in the sample of boards.

TABLE 1 – Summary Statistics

This table present means, standard deviation, and medians for firm and director characteristics in the sample. The sample is based on the RiskMetrics (formerly IRRC) database, which consists of all S&P1500 companies between 1996 and 2011. The number of directorships held by directors is calculated using only directorships held in this sample of firms. Variable definitions are described in the appendix.

Firm Characteristics	N	Mean	Std. Dev.	Median
Return on Assets (ROA)	21929	0.13	0.11	0.12
Tobins's q	22430	1.89	1.56	1.44
Total assets (bil)	22465	14.8	82.3	1.86
Firm age	22465	35.6	16.6	31
Leverage	22372	0.23	0.19	0.22
Volatility	22028	0.12	0.06	0.10
R&D (% of assets)	22465	2.62	5.54	0
Board size	22465	9.49	2.82	9
Fraction Independent	22465	0.69	0.18	0.71
Total CEO compensation (mil)	20827	5.38	10.9	2.94

Directorship Characteristics	N	Mean	Std. Dev.	Median
Tenure of directorship	206371	8.62	8.02	6
High tenure dummy	206371	0.49	0.50	0
Independent dummy	212918	0.69	0.46	1
Director age	212779	60.1	8.79	61
Total # committee memberships (on multiple boards)	208028	1.70	1.67	1
Female dummy	199750	0.10	0.31	0
Attendance Violation dummy	212916	0.02	0.13	0
Audit committee membership dummy	185844	0.39	0.49	0
Compensation committee membership dummy	185844	0.38	0.49	0
Governance committee membership dummy	185844	0.28	0.45	0
Number of directorships held	212918	1.58	0.98	1
Total committee memberships in directorships held	206432	1.04	0.87	1

TABLE 2 – Summary Statistics before Merger Shocks

This table present means of firm and director characteristics for firms and directors involved in a merger events. The left column describes the target firms (and their directors) in the merger. The right column describes firms (and their directors) that share directors with the target firm. All variable means represent the end of the last fiscal year before the merger takes place. Variable definitions are described in the appendix.

Firm Characteristics	Acquired Firms (last firm-year)	Firms with Shocked Directors
	Mean	Mean
Return on Assets (ROA)	0.12	0.13
Tobins's q	1.92	1.82
Total assets (bil)	9.38	31.1
Firm age	32.7	42.5
Leverage	0.22	0.27
Volatility	0.12	0.10
R&D (% of assets)	3.59	2.15
Board size	9.28	10.7
Fraction Independent	0.70	0.70
Total CEO compensation (mil)	4.11	6.83
N	318	975

Directorship Characteristics	Boards of Acquired Firms	Shocked Directors (Treated Group)
Tenure of directorship	7.89	7.35
High tenure dummy	0.47	0.45
Independent dummy	0.69	0.80
Director age	59.4	60.3
Number of directorships held	1.51	3.19
Total committee memberships in directorships held	1.60	3.83
Female dummy	0.09	0.14
Attendance Violation dummy	0.02	0.02
Audit committee membership dummy	0.39	0.45
Compensation committee membership dummy	0.37	0.45
Governance committee membership dummy	0.24	0.31
N	2928	1115

TABLE 3 – Frequency of Multiple Directorship

This table presents the frequency of multiple directorships within the boards of sample firms. The sample is based on the RiskMetrics (formerly IRRC) database, which covers all S&P1500 companies between 1996 and 2011. The top panel shows the distribution of the number of directorships variable and the total board committee membership variable in the sample. The bottom panel shows the changes in the number of board seats variable, for the directors that were dismissed from a board of an acquired firm (treated) and the director that were not (not treated).

Frequency of Multiple Directorship and Committee Membership

	% of Directors
Total Number of Directorships Held	
1 directorship	80.9
2 directorships	13.6
3 directorships	4.0
4 or more	1.5
Total Board Committee Memberships	
0 memberships	29.9
1 membership	30.5
2 memberships	25.9
3 or more	8.0

Changes in the Number of Board Seats Held (S&P 1500)

Δ#Boards	Treated	Not Treated
-3	1.1%	0.0%
-2	11.4%	0.4%
-1	70.4%	4.4%
0	15.3%	84.7%
+1	1.7%	9.4%
+2	0.0%	1.0%
N	1,115	174,044

TABLE 4 – Board Appointments and Firm Performance

IV Regression results for

$$\Delta Performance_{it} = \beta_1 \Delta \widehat{Boards}_{it} + \Delta Controls_{it} + f_t \times f_{industry} + \epsilon_{it}$$

where $\Delta \widehat{Boards}_{it}$ are predicted changes in the number of directorships held from a first stage regression of the change in the number of boards (Δboards) on the instrumental variable, $Treat_{it}$, a dummy indicating that a director on board i was at time *t-1* on a board that was acquired between year *t-1* and year *t*. Firm performance is measured by ROA (columns (1),(3),(6)) or tobin's q (columns (2),(4),(7)). The dependent variables measured in percent. Definitions for all control variables are described in the data section. All regressions include dummies for each industry-year combination. The reported first stage regression (column (5)) is for changes in the number of board seats where the dependent variable is ΔROA. Columns (3), (4) show the reduced form regressions of Δperformance on the instrumental variables. Column (8) shows IV estimates when the dependent variable is the abnormal stock returns during the fiscal year, defined as the raw return minus the return predicted by the market model. Standard errors clustered by firm appear in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level.

	(1) OLS (within)	(2) OLS (within)	(3) Reduced Form	(4) Reduced Form	(5) IV 1st Stage	(6) IV 2nd Stage	(7) IV 2nd Stage	(8) IV 2nd Stage
Dep. Var.	ΔROA	ΔlogQ	ΔROA	ΔlogQ		ΔROA	ΔlogQ	Abnormal Stock Return
Treat			.50***	1.76***	-0.96***			
			(.18)	(.68)	(.055)			
change in…								
Number of Boards	-.0057	-.19				-.52***	-1.82***	-2.41
	(.03)	(.13)				(.18)	(.7)	(1.5)
Firm Size	-1.2***	-20***	-1.2***	-20***	.049	-1.1***	-20***	-17***
	(.42)	(1.3)	(.42)	(1.3)	(.051)	(.41)	(1.2)	(1.6)
Leverage	-12***	-26***	-12***	-26***	.063	-12***	-26***	-38***
	(1.6)	(4.1)	(1.6)	(4.1)	(.104)	(1.6)	(4)	(5.2)
R&D	-54***	-33*	-54***	-33*	-.022	-54***	-33*	-119***
	(8.1)	(18)	(8.1)	(18)	(.324)	(7.9)	(17)	(25)
Volatility	-5.3*	-19	-5.4*	-19	-.639	-5.7*	-20*	38*
	(3.2)	(12)	(3.2)	(12)	(.439)	(3.2)	(12)	(20)
Board Size	.058	.09	.059	.084	.058***	.089*	.19	-.39
	(.043)	(.16)	(.043)	(.16)	(.012)	(.046)	(.17)	(.32)
Fraction Independent	-.28	5.5**	-.29	5.5**	-.0007	-.29	5.5**	10**
	(.6)	(2.7)	(.6)	(2.7)	(.117)	(.59)	(2.7)	(4.4)
R-squared	.21	.3	.21	.3	.099	.2	.3	.049
N Observations	20,037	20,551	20,037	20,551	20,037	20,037	20,551	20,554

TABLE 5 – Director Busyness and Firm Performance Conditional on Geographical Distance

This table shows IV regression results for Eq. (3.1) when the "treatment" variable that defines the instrument is separated to long and short geographical distances. This table exploits data on location of company headquarters. For every merger-shock (the "treated" group), "Near" and "Far" shocks are defined as above or below the median distance between the headquarters of the acquired firm (where a directorship was terminated) to the headquarters of firm i. The indicator $Treat^{far}/Treat^{near}$ equals one only for "shocked" directorships with above/below median distance. Columns (7)-(8) include both treatment indicators in a reduced form regression. Performance is measured by ROA and Tobin's q, and Busyness is measured by the number of directorships held. The reported first stage regression (columns (1),(4)) are for changes in number of directorships where the dependent variable is ΔROA. Control variables are included but not tabulated as well as dummies for each industry-year combination. Standard errors clustered by firm appear in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level.

	Near Shocks only			Far Shocks only			Reduced Form	
	(1) IV 1st Stage***	(2) IV 2nd Stage	(3) IV 2nd Stage	(4) IV 1st Stage***	(5) IV 2nd Stage	(6) IV 2nd Stage	(7)	(8)
Dep. Var.		ΔROA	ΔlogQ		ΔROA	ΔlogQ	ΔROA	ΔlogQ
ΔNumber of Boards		-.26	-.44		-.78***	-3.6***		
		(.23)	(.81)		(.3)	(1.2)		
$Treat$							.66**	3.1***
							(.26)	(1.1)
$Treat^{far}$				-0.85***				
				(.075)				
$Treat^{near}$	-1.04***						-.39	-2.6*
	(.078)						(.36)	(1.4)
R-squared	.09	.20	.30	.09	.18	.28	.21	.30
N Observations	20,037	20,037	20,551	20,037	20,037	20,551	20,037	20,551

TABLE 6 – Director Busyness and Firm Performance Conditional on Directorship Tenure and Forecast Dispersion

IV Results for estimation of Eq. (3.2):

$$\Delta Performance_{it} = \Delta \widehat{Boards}_{it} + I_{it} + (\Delta \widehat{Boards} * I)_{it} + \Delta Controls_{it} + f_t \times f_{industry} + \epsilon_{it}$$

where I is either $I_{it}^{high\ tenure}$ (columns 1, 2) or $I_{it}^{low\ dispers}$ (columns 3, 4). The performance is measured by ROA (columns (1),(3)) or Tobin's q (columns 2, 4). The dependent variables are industry adjusted, and measured in percent. Busyness is measured by the number of directorships held. Firm and director level controls are included but not tabulated. The dummy $I_{ijt}^{high\ tenure}$ indicates that director j has been on the board of firm i for longer than the sample median (6 years), $I_{it}^{high\ tenure}$ is the aggregate. The dummy $I_{it}^{low\ dispersion}$ indicates that disagreement in analyst forecasts for firm i is less than the sample median. Control variables are included but not tabulated as well as dummies for each industry-year combination. Standard errors clustered by firm appear in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level.

	(1) IV 2nd Stage	(2) IV 2nd Stage	(3) IV 2nd Stage	(4) IV 2nd Stage
Dep. Var.	ΔROA	ΔlogQ	ΔROA	ΔlogQ
ΔNumber of Boards	-.75**	-4.8***	-.6**	-2.1**
	(.34)	(1.4)	(.3)	(1.1)
ΔNumber of Boards $* I^{high\ tenure}$	.39	5.1***		
	(.43)	(1.8)		
ΔNumber of Boards $* I^{low\ dispers}$			.21	.57
			(.36)	(1.4)
R-squared	.19	.28	.2	.3
N Observations	20,037	20,551	20,037	20,551

TABLE 7 – Propensity to Join Board Committee

Results for IV regression of Eq. (3.3) and Probit regressions for the "Reduced Form" regression of (3.3). The instrumental variable $Treat_{ijt}$ is a dummy indicating that director *j* was at time *t-1* on a board that was acquired between year *t-1* and year *t*. The dependent variable is an indicator that equals one if director *j* joined firm *i*'s board committees (audit, compensation, or governance committees - at least one of the three). Column (1) shows the probit regressions for the reduced form specification, namely the indicators for joining committees on the instrumental variables. The reported coefficients of the probit regression (column (1)) are the estimated marginal probabilities. For the IV regressions, busyness is measured by the number of directorships held. Control variables are included but not tabulated as well as dummies for each industry-year combination. Column (2) shows first stage regression for changes in the number of boards. Standard errors clustered by directorship appear in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level.

	Probit (Reduced Form)	IV (1st stage***)	IV (2nd Stage)
	(1)	(2)	(3)
Dep. Var.	Joined Any Committee		Joined Any Committee
$Treat$	.053***	-1.01***	
	(.011)	(.02)	
Δ Number of Boards			-.037***
			(.011)
R-squared	.07	.05	.07
N Observations	123,038	135,469	135,469

TABLE 8 – Sensitivity of CEO Compensation to Stock Performance

IV and reduced form regression results for Eq. (3.4):

$$\Delta CEOcompen_{it} = \beta_1 \Delta \widehat{Boards}_{it} + \beta_2 StockPerformance_{it} + \beta_3 (\Delta Boards * \widehat{StockPerformance})_{it}$$
$$+\beta_4 StockVolatility_{it} + \beta_5 \Delta RoA_{it} + \beta_6 \Delta logQ_{it} + \Delta Controls_{it} + f_t \times f_{industry} + \epsilon_{it}$$

where Boards is measured by the total number of board seats held. Where similar to before, $\Delta\widehat{Boards}$ and $\Delta Boards * \widehat{StockPerformance}$ are predicted values from first stage regressions on $Treat_{it}$ and $Treat_{it} * StockPerformance$ and controls. Column (1) shows the reduced form regressions of the change in compensation on the instrumental variable and controls. The dependent variable is the change in total value of the compensation awarded to the CEO, in thousands of dollars. Stock Return, ΔROA and ΔlogQ are measured in percent. Control variables are included even if not tabulated, as well as dummies for each industry-year combination.. Standard errors clustered by firm appear in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level.

	(1)	(2)	(3)
	Reduced Form	IV 1st Stage***	IV 2nd Stage
Dep. Var.	$\Delta CEOcompen$		$\Delta CEOcompen$
$Treat * StockRetrun$	10***	.0017	
	(4)	(.0012)	
ΔNumber of Boards $* StockRetrun$			-11.1**
			(5.7)
$StockReturn$	8.3***	-.0003	17.0***
	(1.6)	(.0003)	(5.0)
$Treat$	35	-0.97***	
	(161)	(.055)	
ΔNumber of Boards			-120
			(170)
ΔROA	11	.0019	7.5
	(6.9)	(.0015)	(7.3)
ΔlogQ	8.2***	-.0001	4.3
	(2.9)	(.0007)	(4.8)
ΔVolatility	526	-.483	-617
	(1451)	(.463)	(1567)
R-squared	.13	.10	.076
N Observations	17,230	17,230	17,230

TABLE 9 – Busyness of CEOs

This table presents regression results for estimation of Eq. (3.5):

$$\Delta Performance_{it} = \beta_1 Treat_{it}^{CEO} + \Delta Controls_{it} + f_t \times f_{industry} + \epsilon_{it}$$

The variable $Treat_{it}^{CEO}$ is a dummy equal to one in the 143 cases (between 1998 to 2011) in which CEO of firm i was a director at time *t-1* on a board that was acquired and terminated between year *t-1* and year *t*, and equal to zero for all other compustat firm-years. Firm performance is measured by ROA and Tobin's q, and measured in percent. Regressions include dummies for each industry-year combination. Standard errors clustered at the firm level appear in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level.

	(1)	(2)
Dep. Var.	ΔROA	ΔlogQ
$Treat^{CEO}$	2.1***	4.7**
	(.8)	(2.4)
ΔFirm Size	8.9***	-19***
	(.38)	(.5)
ΔLeverage	-3.8***	7.2***
	(.83)	(1.7)
ΔR&D	-27***	11***
	(4.8)	(1.8)
R-squared	.22	.24
N Observations	97,785	89,690

TABLE 10 – Robustness Tests: Withdrawn Mergers and Non-Terminating Acquisitions ("Placebo" Test) and Committee Membership proxy for Busyness

Regression results for Eq. (4.1) (columns 1,2) and for IV regression for Eq. (3.1) where busyness is proxied by the total number of committee membership (columns 3-5). The dummy variable $NonTreat_{it}$ indicates that a director on board i was also on the board of a firm which received a merger bid (during the year that ends at time t) which was ultimately withdrawn or in which the acquired firm continues to exist post acquisition as a subsidiary of the acquirer (and as an entity that has its own board). The usual instrumental variable $Treat_{it}$ indicates the usual merger-shock. In columns (3)-(5) busyness is measured by the total number of committee memberships of firm *i*'s directors on boards other than *i*. Firm performance is measured by ROA (columns 1,4) or Tobin's q (columns 2,5). The dependent variables are measured in percent. Definitions for all control variables (not tabulated) are described in the data section. All regressions include dummies for each industry-year combination. The reported first stage regression (column 3) is for changes in number of committees where the dependent variable is ΔROA. Standard errors clustered by firm appear in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level.

	(1)	(2)	(3) IV 1st Stage***	(4) IV 2nd Stage	(5) IV 2nd Stage
Dep. Var.	ΔROA	ΔlogQ		ΔROA	ΔlogQ
$NonTreat$	.053	.23			
	(.17)	(.55)			
$Treat$			-.90***		
			(.099)		
Δ# of Committees				-.55***	-1.9***
				(.19)	(.74)
R-squared	.21	.30	0.10	0.18	0.29
N Observations	20,037	20,551	20,037	20,037	20,551

TABLE 11 – Robustness Tests: Alternative Sets of Control Firms

This table presents reduced form tests for the effect of merger-shocks to director workload using alternative sets of control firms. Columns 1 and 2 report reduced form regression estimates (change in firm performance on the indicator for a merger shock) when the sample is limit to those firms that the firm-years defined as shocked (treatment) and the firm-years of those shocked firm that came before the shock as a control group. The specification includes the control variables and fixed effects similar to Table 4. Columns 3 and 4 report "Average Treatment Effect on the Treated" (ATT) estimates using the "nearest neighbor" propensity score matching method to match each treated firm-year to a control firm-year that has the closest propensity scores. The observables used for the matching are the control variables in the regressions including industry and year dummies. The reported ATT estimates are based on a single closest neighbor matching (n=1) but are robust to using more than one neighbors per treated firm-year. The dependent variables are changes in firm performance measured by ROA or Tobin's q The dependent variables measured in percent. Definitions for all control variables (not tabulated) are described in the data section. The standard errors for the regressions are clustered by firm. ***, **, and * denote significance at the 1%, 5%, and 10% level.

	(1)	(2)	(3)	(4)
	Only Firms that are ever shocked		Nearest Neighbor Matching	
Dep. Var.	ΔROA	ΔlogQ	ΔROA	ΔlogQ
$Treat$	0.70*	2.22		
	(0.42)	(1.94)		
ATT Estimate			0.63*	2.58*
			(0.33)	(1.51)
R-squared	.38	.45		
N Observations	2,753	2,807	19,138	19,592