



# Busy directors and firm performance: Evidence from mergers<sup>☆</sup>

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## ABSTRACT

This paper studies whether director appointments to multiple boards impact firm outcomes. To overcome endogeneity of board appointments, I exploit variation generated by mergers that terminate entire boards and thus shock the appointments of those terminated directors. Reductions of board appointments are associated with higher profitability, market-to-book, and likelihood of directors joining board committees. The performance gains are particularly stark when directors are geographically far from firm headquarters. I conclude that the effect of the shocks to board appointments is: (i) evidence that boards matter; and (ii) plausibly explained by a workload channel: when directors work less elsewhere, their companies benefit.

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## 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 concurrently on boards of multiple companies and their workloads compound. The National Association of Corporate Directors recommends that di-

rectors 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, excluding travel time.<sup>1</sup> Yet concurrent board appointments are not uncommon: more than 20% of directors in Standard and Poor's (S&P) 1500 companies hold multiple board seats and nearly 85% of S&P1500 firms share at least one director with other S&P1500 firms. The heavy workload and prevalence of concurrent directorships spark a debate over the concern that busy board structures are inefficient.

Others, however, question if corporate boards matter at all. Although modeled as a mechanism to monitor and align manager and shareholder interests, much skepticism surrounds whether boards' impact is real and first-order (Yermack, 2006; Adams et al., 2010). Testing boards' relevance boils down to estimating causal effects of board structures. This approach hinges on natural experiments

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<sup>1</sup> Yet since their study, the workload of directors has increased dramatically, especially post Sarbanes-Oxley (Linck, Netter, and Yang, 2009). The issue of busy boards has flared in recent years due to these changes.

since observed board structures are chosen endogenously by firms facing different needs. One such characteristic of board structure is the service of directors holding multiple concurrent board seats. Identifying its effect is an empirical challenge since appointments to boards are endogenously determined. Firms carefully select directors who match their needs, and those firms appointing directors who serve on multiple boards likely differ from firms appointing directors who serve on fewer boards, in aspects that confound firm outcomes.

This paper aims to identify the direct effect of concurrent board appointments on firm performance. To overcome the endogeneity issues, I exploit variation in board appointments 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,<sup>2</sup> directors of the target firm “lose” their appointments (Harford, 2003). Econometrically, the advantage of this source of variation is that it allows for examining changes in outcomes as board appointments vary, while absorbing firm and director characteristics. Following such mergers, I examine the performance of the *other* firms which continue to employ the affected directors, and find that their performance improves.

The first contribution of this paper is to provide an identification strategy to test the relevance of boards. I argue that a link between exogenous changes in directors’ appointments and their firms’ performance is evidence that directors matter. The finding that performance improves following mergers suggests that directors can indeed add or destroy value and hence board structures are more than just window dressing. Secondly, I ask *why* the affected firms may be performing better and turn to investigate potential mechanisms in which concurrent board appointments impact firms.

Specifically, I examine the workload aspect of concurrent board appointments. A merger which terminates directorships presumably shocks those directors with extra time to devote to their other *remaining* directorships. The underlying premise views directors as agents who optimize the time and effort they devote to their various commitments. If one commitment is to be exogenously removed, they are shocked with extra time and thus the marginal cost of exerting effort to all remaining commitments declines. As a result, they spend the extra time on all remaining directorships and in turn add value to those firms.

Given the identification challenges, the empirical evidence on the effect of director workload and busy boards is mixed and often contradictory. The endogenous selection of board appointments implies that the effect of heavy workload 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. Moreover, firms that select busy directors may need particular director expertise more than director time (Field et al., 2013).<sup>3</sup> In addition to selection, 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 due to 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 et al., 2010). A negative relation between board busyness and firm performance is documented by Core et al. (1999), Shivdasani and Yermack (1999), Fich and Shivdasani (2006), and Ahn et al. (2010), while positive aspects of multiple appointments are documented by Loderer and Peyer (2002), Ferris et al. (2003), Masulis and Mobbs (2011), and Field et al. (2013). In many of these studies, the relationship between board busyness and firm performance can be interpreted as a test for whether the workload effect outweighs the director quality effect (Adams et al., 2010). In this paper, I focus on the workload aspect. Instead of asking which effect is stronger, I ask a complementary question: whether director workload directly affects firm performance in a meaningful and central way.

An alternative explanation for the effect of merger-shocks is that the estimates may capture the effect of takeover procedures rather than a pure effect of workload. In particular, I consider the plausibility of a “direct takeover effect” as a potential mechanism driving the findings. Such an alternative mechanism would require that a director’s role in a merger transaction directly leads to timely changes in her or his 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’s disciplining effect.<sup>4</sup> Arguably, evidence on these direct takeover effects would be equally interesting from an empirical point of view. However, I find little evidence to support direct takeover effects, while the collective evidence that I show supports the workload effect narrative.

I investigate the direct takeover effect vis-a-vis the director workload channel by exploiting geographical distances between directorships. I find a pronounced effect of a merger-shock when the merger terminates a directorship that 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. The link between distance and board monitoring has been

<sup>2</sup> Harford (2003) finds that acquiring firms rarely appoint directors of the acquired firm to the merged firm’s board. These rare cases usually involve special circumstances such as a director who is a founder—and as such is unlikely to be a multiple director. This finding is confirmed in my data sample as well.

<sup>3</sup> Coles et al. (2008) and Linck et al. (2008) emphasize the heterogeneity across firms in the effect of board structures.

<sup>4</sup> Takeovers have been argued to have a disciplining effect on managerial behavior (e.g., Jensen and Ruback, 1983). Similar forces may apply to directors if, for example, one takeover bid increases directors’ awareness of future takeover threats and that awareness affects their behavior.

documented by Alam et al. (2014). Directors who are distant from company headquarters not only become busier due to time spent traveling, but also may face additional obstacles to monitor given the distance. In contrast, geographical distance 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 potential direct takeover effects as an alternative to workload by running a falsification test that exploits withdrawn takeover bids that do not lead to termination of the target board. Consistent with the workload narrative, this falsification test shows no evidence that “placebo mergers”—merger bids which are eventually withdrawn—relate to performance. Despite possible merger proceedings and negotiations, “placebo mergers” do not seem to correlate with firm performance when bids are withdrawn without subsequent impact on board appointments.

If reduced director workload benefits firms, each concurrent board appointment imposes a cost to firms sharing the director. I estimate the effect of a board appointment with an instrumental variable (IV) approach exploiting the variation that mergers generate in board seats. Specifically, I instrument for board appointments with a variable for employing a director who concurrently served on a board of a firm that was acquired in a merger. The identifying assumption for the IV approach 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 a director with the acquisition target (other than through the channel of that director’s workload), conditional on any time-invariant characteristics. Such merger-shocks generate variation in directors’ appointments over time rather than variation due to changes in board personnel.<sup>5</sup> 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 (though I also consider finer control groups in the robustness section). In spirit, this setting compares firms with a shocked director on the board (the “treated” group) to firms without shocked directors, operating in the same industry at the same time (the control group), in terms of changes in firm performance around a merger-induced reduction in board appointments. Under the identifying assumption, that difference can be attributed to the effect of directors’ concurrent appointments.

I find that a reduction in directors’ board seats 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., Return on Assets), and after controlling for cross-industry effects and

time-invariant firm and director characteristics. The economic magnitude of the IV estimates is more conservative than documented in most previous studies,<sup>6</sup> but is not negligible: a reduction of one board seat is associated with a 1.3% increase in Tobin’s  $q$ , and a 0.3% increase in Return on Assets (ROA). For the median S&P1500 company, these estimates suggest a removed board seat is worth roughly \$6M in operating earnings or \$30M in market value.

If board appointments impact directors, they may also impact other corporate executives who hold board appointments in additional companies. Perry and Peyer (2005) find that investors react negatively to announcements of incumbent Chief Executive Officer (CEO) appointments to boards of other firms. To expand the analysis of the effect of concurrent board appointments to corporate executives, I collect data on a smaller number of individuals who experienced merger-shocks while holding additional positions—not as directors—but rather as incumbent CEOs. I find that performance of the firms whose CEOs are shocked with a terminated board seat substantially improves compared to firms that are not shocked. As might be expected, the magnitude of a shock’s effect on CEOs is much larger than the effect of a similar shock on board directors.

The empirical setting allows me to address several identification concerns. First, the setting accounts for the possibility that an approved merger reveals a signal about the quality of the directors who approved it. Indeed, the board voted in favor of a merger at least on one occasion—a fact that may correlate with the instrument and director quality.<sup>7</sup> However, this correlation alone does not undermine the identification strategy. The regressions control for any differences in director qualities that are time-invariant. Therefore, a viable alternative explanation would have to justify comovement that coincides with merger timing, between two firms connected merely by the fact that they shared a director. In addition, the instrumental variable approach addresses the omitted variable problem that arises since the number of board seats is an imperfect proxy for workload.

An identification concern I address is that the timing of mergers reflects merger waves within thriving industries. Specifically, the concern is that the observed changes in firm performance are driven by merger waves rather than by directors responding to the shock. I rule out this concern in two ways. First, controls for industry-by-year fixed effects in every regression account for periods in which a certain industry flourishes. Thus, the possibility that directors tend to serve firms in the same industry does not disrupt the identification strategy. In addition, industry overlap between directorships of one individual is not common, possibly due to competition among firms in

<sup>6</sup> For instance, 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” directorship.

<sup>7</sup> The signal about those directors may be positive or negative. A takeover bid may be more likely approved by a target firm’s board consisting of proper directors who put shareholder interests above their own. Conversely, a firm may be a more appealing takeover target if its board is entrenched and acquirers can gain more by taking over and replacing the management.

<sup>5</sup> This approach is fundamentally different from prevailing studies using within-firm variation in board seats, since most of the within-firm variation in board seats reflects changes in board personnel (busy director replaced by less busy or vice versa), which bears both the effects of workload and qualifications.

the same industry. Second, results are thoroughly similar in a robustness test that omits cases in which the acquired firm is in the same industry as the treated firm, suggesting that the results are not driven by industry merger waves.

Finally, I explore potential mechanisms through which workload can affect firm performance: directors' ability and willingness to participate in committee assignments and meetings. I first explore the relationship between director appointments and the likelihood of becoming a member or chair of board committees. Directors holding many board seats are less likely to be assigned to new board committees and less likely to be assigned as committee chairs. Specifically, following a reduction of one board seat, directors are substantially more likely to become chair of a board committee in each board they remain on (likelihood increases from 7% to roughly 10%) and more likely to join committees (roughly increases from 17% to 21%). This evidence is important since a director's ability to serve on committees has been shown to be a mechanism in which director workload can affect firm performance (Jiraporn et al., 2009). Directors who participate in committees can be more influential, but have more meetings to attend and more business and legal responsibilities.

An interesting implication of the findings emanates from the fact that mergers impose negative shocks to workload, making directors *less* busy. A priori, the effect of positive and negative shocks to workload need not be symmetric, since if directors were able to devote the maximum required effort to all their boards, a negative shock to workload should not have an effect.<sup>8</sup> My findings indicate that this is not the case: the strong response to negative shocks suggests directors are optimizing their efforts given a binding time constraint. My main result is most related to recent findings of Falato et al. (2014), who use a different natural experiment (death of board colleagues) to plausibly shock a director's attention away from one company. 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 et al. Their finding, that stock prices plunge when a director is predicted to become distracted, is consistent with my findings that reduction in a director's workload can benefit firms' operating earnings and market value in the years that follow. In addition, my setting allows me to expand the analysis from director workload to the effect of "executive workload" by studying CEOs who lose board appointments in other firms.

The next section describes the empirical framework. Section 3 describes the data sample and presents summary statistics. Section 4 presents the results, and Section 5 presents additional robustness tests focusing on identification. Section 6 concludes.

## 2. Empirical framework

### 2.1. Multiple concurrent board appointments

The first objective of this paper is to provide evidence that boards matter and have a first-order causal effect on companies' operations and in turn company performance. I provide this evidence by focusing on appointments of directors to multiple boards at the same time. The advantage of studying board appointments is that it allows me to exploit a quasi-natural experiment necessary to make causal inferences. By showing that board appointments affect performance, it follows that the board of directors matters as a governing body in the firm. The central hypothesis—that concurrent board appointments affect firm performance—can be expressed in the equation:

$$Performance = b_1 \text{Number of Boards} + B_2' \text{Controls} + u, \quad (1)$$

where Controls include all determinants of firm performance. However, the coefficient  $b_1$  is not identified since the vector of controls omits unobserved factors (such as director ability) which confound firm performance and board appointments. An identification strategy calls for exogenous variation in board appointments that does not affect firm performance conditional on observable control variables. I use mergers in which one firm acquires another as the source of variation in the board appointments of a third firm whose directors were also board members of the acquired firm. I refer to these events as "merger-shocks", and to any director who does not experience such an event as "unshocked". To exploit the exogenous variation, I capture merger events in the variable  $Treat_{it}$ , defined simply to count the merger-shocks for the directors of firm  $i$  at year  $t$  (aggregating shocks at the director level  $Treat_{ijt}$ ), and equal to zero when no board member is shocked by a merger.<sup>9</sup> Econometrically, this variable is aggregated to the firm level since that is where outcomes can be observed. The interpretation is the same; the coefficient  $b_1$  in Eq. (1) reflects the average effect of a board seat to a director.

Since some board seats are eliminated with the merger,  $Treat$  should be negatively correlated to a change in the number of board seats. This negative correlation is with a change in board seats rather than with board seats in levels, for which the relationship is indirect and more complicated. Therefore, in order to exploit the variation originated by mergers, I use first-differenced specifications for the empirical tests. Written in terms of differences, the corresponding relationship becomes:

$$\Delta Performance = b_1 \Delta \text{Number of Boards} + B_3' \text{Controls} + v, \quad (2)$$

<sup>8</sup> In particular, a negative shock to workload should not have any effect in a world where directors' time constraints are not binding, and directors can devote to each board appointment all the time it requires. My setting can reject this null hypothesis due to the negative shocks.

<sup>9</sup> Where  $i$  indexes firms,  $j$  directors, and  $t$  years. The aggregated variable can take values greater than one if directors on a board coincidentally experienced merger (each in their respective other firm) within a fiscal year. However, these cases are not common, such that the distribution of treatment is close to a dummy variable (results are robust to truncating this variable into binary values).

where *Controls* include only time-varying performance determinants that are related to board seats.

Two additional advantages of a first-differenced specification over an analogous specification with firm fixed effects go beyond controlling for time-invariant firm characteristics. First, due to data: the number of board seats variable is defined more accurately in differences than in levels (as explained in the data section). Second, a first-differenced specification imposes consistency in board personnel since each first-differenced observation requires valid data of two consecutive directorship-year observations. The requirement of two consecutive directorship-year observations means directors who join (leave) a board do not breach the board seats measure in the year they join (leave). In particular, when one director replaces another on a board, differences in their board seats are not reflected in my measure.

This second advantage is a key difference between this paper and previous papers studying busy boards. This is because most of the variation in the number of board seats held by board members occurs when one busy director replaces another less busy director or vice versa. As a result, most of the variation in the number of board seats coincides with changes in board personnel—a source of endogeneity if a director's ability is correlated with board seats. In contrast, my first-differenced measure of board seats captures variation over time in appointments of incumbent directors rather than variation due to change in board personnel.

As a result, the first-differenced regressions provide a within-estimator that is not only within-firm, but effectively within-directorship. It effectively controls also for director-level time-invariant characteristics such as directors' ability.

## 2.2. Reduced-form effect of merger-shocks

Disentangling director personnel and board appointments is not sufficient to identify the effect of appointments, since even within-directorship variation in appointments can stem from endogenous dynamics. The first-differenced model is useful in conjunction with merger-shocks as a natural experiment, creating a setting that limits the investigation to variation in board appointments originating from merger-shocks.

I implement this strategy with a reduced-form approach in which I regress firm performance on the variable *Treat* that captures merger-shocks to a director. The identifying assumption requires that acquisition of a target firm is independent of the prospects of other firms with which the target shares directors (other than the effect of those directors themselves), conditional on the controls in the regressions. This assumption is admissible because the treated firms are not the target nor the acquirer in the mergers. Certainly, the fact that the treated firm shared a director with an acquired firm may carry a signal about the quality of the shared director. For instance, such a signal is possible if directors of high quality are more likely to approve mergers. But taking first-differences controls for this possibility, and a signal does not violate the identifying assumption. In order to violate the identifying assumption,

any competing hypothesis would have to explain comovement that systematically coincides with the timing of mergers.

The timing of a merger-shock is defined as the first fiscal year when the acquired firm no longer exists. I define this as year  $t$ : the year that the acquired board disappears from the board database. Throughout the previous fiscal year (year  $t-1$ ), the board members are still responsible for their board seat. The merger takes place at any point during the fiscal year, thus the board seat responsibilities apply only for part of the year—depending on the month of completion. In the next fiscal year, the acquired board members are entirely free of the board seat, incurring no workload. For the specifications in first-differences, I use the last year when the board seat fully applies as the “pre” year, and the first year when it fully does not apply as the “post” year.<sup>10</sup> This approach compares a shocked firm pre- and post-shock to control firms in the corresponding years, net of year and industry trends. The effect of the merger-shock is the average difference between the pre- to post-trends of the two groups.

In all regressions, I include industry-by-year dummies to control for the possibility that mergers incidentally occurred in industries or years that outperformed during the sample period. Additional control variables include factors previously found to correlate with performance, namely, firm size, board size, board independence, financial leverage, volatility, as well as investment in research and development (R&D) in case firms with many investment opportunities exhibit larger Tobin's  $q$ .<sup>11</sup> As the first-differenced specification prescribes, I include the control variables in “differences”, yet the results are robust to including some or all the control variables in “levels”. Finally, in line with the literature, I use two measures for firm performance: the accounting-based Return on Assets (ROA) and the market-based Tobin's  $q$  (proxied by the market-to-book ratio). For Tobin's  $q$ , I take logs so that regression coefficients have a percentage interpretation. Full details of the construction of all variables are provided in [Appendix A](#).

## 2.3. Director workload

Under the identifying assumption, the estimated response of firm outcomes to a merger-shock is attributed to the effect of the director that was shocked. I interpret this effect as evidence that boards can impact the way firms are managed and firm value. Beyond this baseline application of the empirical framework, the setting is also useful to investigate underlying reasons *why* board appointments impact firm performance. In particular, I consider whether director workload affects firm performance, but also consider the extent of evidence in favor of alternative aspects

<sup>10</sup> The difference skips the year in between since trying to classify it as busy or not busy would be complicated and risk contamination. In that year, the affected board seat is still partially present: it imposes workload during the months until the board is terminated. In addition, that year may be contaminated if some mergers impose heightened effort shortly before the deal is agreed.

<sup>11</sup> I winsorize the regression 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.



of a change in board appointments (for example, impact of director incentives or director learning).

A shock that eliminates a board appointment is primarily a shock that is likely to reduce the director's workload. To explore the role of time, I introduce interactions with proxies for the time intensity of the shock treatment (geographical distance). I also study behavioral responses in uses of director time (such as committee membership and attendance in meetings).

In particular, the notion that director workload impacts firm performance predicts that the average response to merger-shocks will increase with the distance between directors and their firm that was acquired. The link between distance and the time directors need to devote is natural, and goes beyond extra traveling time. First, the required traveling includes trips for official board meetings as well as trips on other occasions necessary to monitor firm management outside of board meetings. Second, if directors make fewer trips over the course of a fiscal year, extra work may be required to make up for the trips not made. [Knyazeva et al. \(2013\)](#) note that beyond formal board meetings, overseeing firm developments requires less time and energy from directors that are local.

In contrast to the natural link between distance and the time required of directors, the other channels that can link board appointments to firm performance may not provide clear predictions on the intensity of distant merger-shocks. I use proxies for geographical distances to evaluate the plausibility of the most common channels—namely, workload, ability, incentives, and social ties—which the literature identifies as potential underlying mechanisms for an effect of board appointments. The interaction is useful to discern between the different channels because each alternative explanation would have to explain performance gains while simultaneously accounting for amplifying or attenuating factors (far or near distance) of the performance gains.

#### 2.4. Instrumental variable estimation of a workload effect

To estimate the effect of a board seat held concurrently by a director, I use an instrumental variable approach in which the merger-shock treatment variable ( $Treat_{it}$ ) is an instrument for the endogenous variable  $\Delta Boards_{it}$  (the change in the total number of board seats held by directors of firm  $i$  at year  $t$ ). The exclusion restriction for the IV estimate requires that acquisition of a target firm is independent of the prospects of other firms with which the target shares a director, other than through the channel of that director's workload (conditional on the controls in the regressions). As such, the IV approach requires a stronger assumption than the reduced-form approach. I interpret the reduced-form effect of merger-shocks as a causal effect of the shocked director without any assumptions on its underlying mechanisms. However, the IV approach requires the added assumption of irrelevance of mechanisms other than workload for directors to channel merger-shocks. Under the assumption that a board-terminating merger is a shock to a director's workload, I interpret the IV estimate for the effect of one board seat as a causal effect of workload that a concurrent board seat imposes on directors.

The plausibility of the exclusion restriction for the IV approach is discussed in detail in later sections, and is supported by the collective evidence presented in this paper.

### 3. Sample selection and data description

#### 3.1. Directors and boards sample

I obtain data on boards and directors from Institutional Shareholder Services (ISS, formerly RiskMetrics). ISS provides annual board snapshots of the companies that comprise the S&P1500 index from 1996 to 2014. I follow the step-by-step procedure of [Coles et al. \(2014\)](#) to merge ISS with Compustat and CRSP.<sup>12</sup> The final sample for the study includes 20,718 firm-year observations.<sup>13</sup>

I measure director workload as the number of board seats held by the director in that year. This variable is corrected for changes in the set of firms covered by the ISS database—as firms enter and exit the S&P1500 index. To correct for those cases, this variable is only defined in differences. It counts changes in a director's board seats if the firm is covered by ISS in the consecutive years necessary to observe joining or leaving a board. For each firm  $i$  in year  $t$ , the change in board appointments,  $\Delta Boards_{it}$ , is defined as the sum  $\sum_{j \in i} \Delta Boards_{ijt}$  aggregated over the directors ( $j$ ) of its board.

[Table 1](#) presents the frequency of concurrent board seats on S&P1500 boards. The top panel shows the distribution of the number of board seats variable as well as the total number of board committee memberships in those boards. Concurrent board seats are fairly common, yet roughly two-thirds of directors do not hold more than a single seat on S&P1500 boards. Holding more than four board seats is rare.

#### 3.2. Merger-shock events

I identify 1013 cases in which a firm in the ISS database is a target of a successful merger. A detailed description of the exact steps to identify merger-shocks is elaborated in [Appendix B](#). These steps identify 3083 directorships that are retained by the directors shocked by a merger.

The bottom panel of [Table 1](#) 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 they hold (92% of the cases for untreated directorship-years). In contrast, the treated directors typically lose a board seat within the fiscal year of the merger. Overall, a merger-shock shifts the distribution of the change in the number of board seats by roughly minus one compared

<sup>12</sup> Including their detailed corrections to data entry issues such as mis-specified dates and directors' identifiers. This allows matching a CRSP identifier to all firm-year observations in the ISS panel. However, substantially less observations are available for regressions specified in differences, as they require variables from all three databases (ISS, Compustat, and CRSP) both in the pre- and post- year. Identical data requirements apply to treated and control groups.

<sup>13</sup> The sample for later tests with director-level outcome variables is all board members of these 20,718 firm-years, and consists of 199,005 directorship-year observations representing 27,786 unique individuals.

**Table 1**

Frequency of concurrent board seats.

This table presents the frequency of concurrent board seats held by directors. The sample is based on the ISS (formerly RiskMetrics) database, which covers all S&P1500 companies between 1996 and 2014. The number of board seats presented here counts only appointments at firms in the ISS panel dataset. Panel A shows the distribution of the number of boards variable and the total board committee memberships within those boards. Panel B shows the changes in the number of board seats variable, distinguishing between the directors that were dismissed from a board of an acquired firm (treated) and the directors that were not (not treated).

<i>Panel A: Frequency of concurrent directorships and committee memberships</i>		
		% of directors
Total number of board seats (S&P1500)		
1 board		64.4
2 boards		21.3
3 boards		9.3
4 boards		3.3
5 or more		1.7
Total board committee memberships across board seats held		
0 memberships		26.7
1 membership		19.9
2 memberships		15.8
3 memberships		16.8
4 memberships		8.2
5 memberships		4.0
6 or more		8.6
N		199,005

<i>Panel B: Changes in the number of board seats held (S&amp;P 1500)</i>		
$\Delta$ Boards	Treated	Not treated
−3	0.6%	0.0%
−2	8.1%	0.3%
−1	69.9%	3.7%
0	19.2%	91.9%
+1	1.7%	3.7%
+2	0.3%	0.3%
N	3083	195,922

to directorship-years that are not shocked. The instrument is relevant since with high probability, the total appointments of a shocked director is lower after the merger than it was before.

Fig. 1 shows the average cumulative changes in the number of directorships held by shocked directors in event years surrounding merger-shocks. Indeed, a steep drop in number of board seats occurs within the fiscal year that the merger takes place (“year 0” in the figure). The number of board seats held by shocked directors drops by an average of 0.93 board seats. The drop of 0.93 (rather than 1.0) may be a result of some shocked directors replacing the lost board seat with a new appointment. In the subsequent years, the average number of board seats recovers only moderately suggesting only limited replacement of the terminated directorships (for example, after two years the cumulative change recovers to  $-0.86$ ).

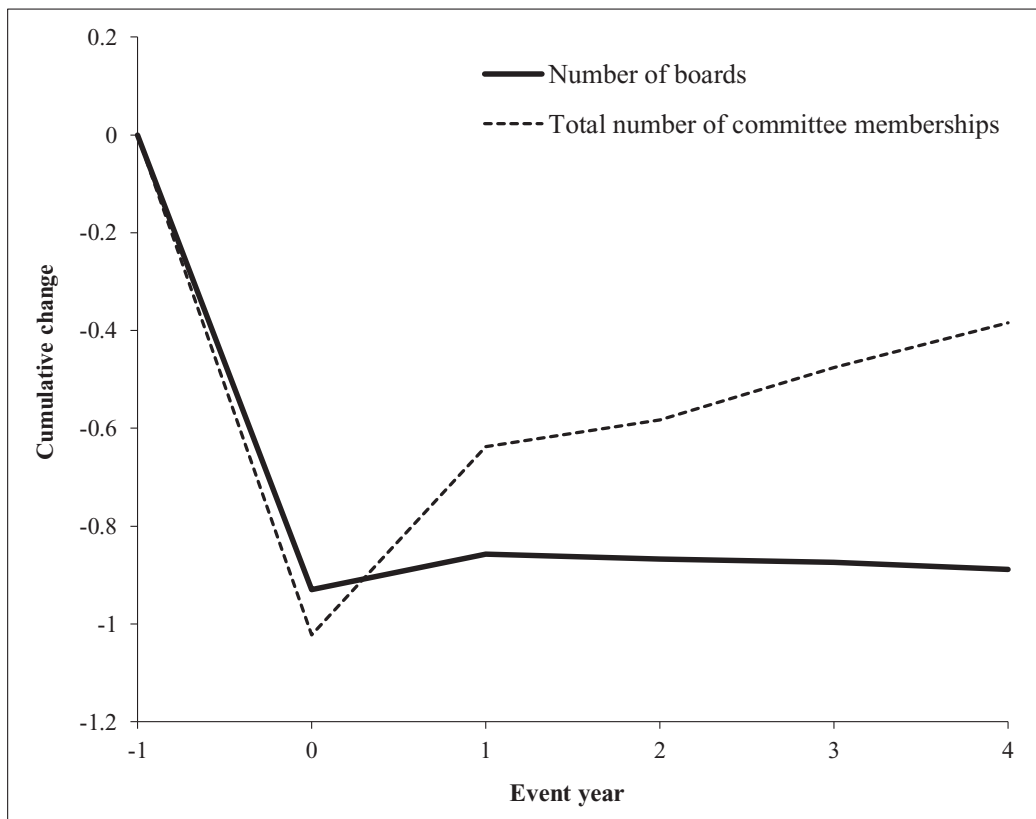
Fig. 1 also plots the average cumulative change in the number of committee memberships within the directorships held by shocked directors. In the event year (“year 0”), the number of committees drops since committee memberships are surely terminated when the board is terminated. The number of committees, however, does appear to recover considerably in subsequent years. Interestingly, many directors seem to be taking upon themselves new committee appointments within their standing directorships. This trend is consistent with the premise that

board appointments (as well as committee appointments as member or chair) are linked to time allocation.

### 3.3. Descriptive statistics – treatment and control

Table 2 shows firm-level and directorship-level descriptive statistics. Panel A provides firm characteristics for all the firm-year observations that (after taking first-differences) are in the sample for the empirical analysis. Covering S&P1500 firms, the sample includes relatively large publicly traded firms; the median firm has total assets of \$2.1 billion. The sample is also characterized by relatively high return on assets. The average ROA in the sample is 0.13, which is substantially higher than the average ROA in the full Compustat-CRSP universe of publicly traded firms. The other outcome variable, Tobin's  $q$ , is 1.89 on average. An average board in the sample includes 9.5 directors, 71% of which are classified as independent. Panel B provides directorship characteristics of the directors serving the boards in Panel A. The average director has tenure of 8.7 years and is a member of 1.3 committees.

The empirical analysis relies on the comparison between a treated group of firms with shocked directors and a control group of firms with no shocked directors. Table 3 focuses on the pre-merger year to assess these groups prior to the treatment. Column 1 of Table 3 shows descriptive statistics for the 1013 firms that were acquired—in



**Fig. 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 these directorships held, in event years surrounding merger-shocks. The cumulative changes are averaged over directors who served on a board of a company that was target of a successful merger (the “shocked” directors). Year 0 is defined as the first directorship-year after the completion of the merger, such that year -1 is the last year in which the target firm appears in the sample of boards. The committees included in this measure are the audit, compensation, governance, and nominating committees.

their last year before the merger. These acquired firms are neither treated nor control; in fact, they are not part of the final sample since by construction they are not present in year  $t$ . However, descriptive statistics of these firms may be of interest since they play an important role in facilitating a quasi-experiment. Panel B shows directorship characteristics for members of these firms’ boards. Column 4 describes the directorships on boards of acquired firms—just before the merger. Column 5 describes directorships held by the same directors, but on boards of the other firms which continue to employ them. These directorships which they retained define the treated group for later tests with director-level outcome variables such as committee participation.

Although the acquired firms described in Column 1 are not part of the regression sample, they define the treated group for the study through concurrent board seats. Column 2 shows firm characteristics for this treated group, in the last year prior to being shocked. There are noticeable differences when comparing Columns 1 and 2. This can be expected since the former describes firms on the verge of being acquired. For instance, the acquired firms seem less profitable (average of 0.118 in ROA) and smaller on average (\$5.84 billion in assets). Still, the differences are not as drastic in terms of board characteristics such as

board independence (0.68 on average target). Finally, Column 3 describes the control group for the study: the firms with no shocked directors. Before the treatment, comparing the treated and control groups shows little difference in terms of firm performance measures. Ex ante, return on assets averages 13.4% and 13.9% among untreated and treated firms, and Tobin’s  $q$  averages 1.90 and 2.01 in these groups.

## 4. Empirical findings

### 4.1. Board appointments and firm performance

The empirical design corresponds to the identifying assumption that an acquisition of one firm is independent of the prospects of other firms with which it shares directors (other than through the channel of those directors themselves), conditional on the factors regressions can control for. I control for time-invariant unobserved effects using first-differenced regression models.

Columns 1 through 4 of Table 4 present ordinary least squares (OLS) estimates of the relationship between any changes in number of board seats held by the board members, and changes in firm performance as measured by ROA and Tobin’s  $q$ . The OLS estimates can be compared



**Table 2**

Full sample descriptive statistics.

This table presents means, standard deviations, and medians for firm and director characteristics. The sample is based on the ISS (formerly RiskMetrics) database, which consists of all S&P1500 companies. The sample period is from 1996 to 2014. Panel B shows director and directorship characteristics for all the board members of the firms in Panel A. Details on variable definition and construction appear in [Appendix A](#).

<i>Panel A: Firm characteristics</i>	<i>N</i>	<i>Mean</i>	<i>Std. dev.</i>	<i>Median</i>
Return on assets (ROA)	25,958	0.13	0.10	0.12
Tobins's <i>q</i>	25,958	1.89	1.44	1.46
Total assets (\$bil)	25,958	16.7	91.9	2.11
Firm age	25,958	27.5	16.6	23
Leverage	25,861	0.23	0.18	0.21
Volatility	25,935	0.41	0.21	0.36
R&D (% of assets)	25,958	4.01	10.3	1.62
Board size	25,958	9.54	2.72	9
Board independence	25,958	0.71	0.17	0.75
<i>Panel B: Director and directorship characteristics</i>	<i>N</i>	<i>Mean</i>	<i>Std. dev.</i>	<i>Median</i>
Tenure of directorship	223,696	8.73	7.98	7
Independent dummy	247,590	0.71	0.45	1
Audit committee membership dummy	223,861	0.40	0.49	0
Compensation committee membership dummy	223,861	0.38	0.49	0
Governance committee membership dummy	223,854	0.30	0.46	0
Nominating committee membership dummy	223,861	0.35	0.48	0
Total committee memberships (on this board)	247,590	1.30	1.23	1
Number of committee chairs (on this board)	247,590	0.24	0.52	0
Director age	247,337	60.7	9.53	61
Female dummy	236,142	0.11	0.32	0
Attendance violation dummy	247,588	0.015	0.12	0
Number of directorships held (across multiple boards)	247,483	1.57	0.96	1
Total committee memberships (across multiple boards)	247,483	2.09	2.26	2

**Table 3**

Descriptive statistics before merger-shocks.

This table presents descriptive statistics for firms and directors involved in merger events. Column 1 describes the acquired firms in their last year, and Column 4 describes the board members of these firms in that year. Column 2 describes firms that share directors with the acquired firm also in the same year (last year prior to the merger), and Column 3 describes firms that do not share these directors (the remaining firm-years). Column 5 describes other directorships which the board members of acquired firms held in the last year prior to the merger (and retained after the merger). Details on variable definition and construction appear in [Appendix A](#).

<i>Panel A: Firm characteristics</i>	Acquired firms (Last fiscal year) (1)		Firms employing shocked directors (Treated group) (2)		Firms not employing shocked directors (Control group) (3)	
	<i>N</i>	<i>Mean</i>	<i>N</i>	<i>Mean</i>	<i>N</i>	<i>Mean</i>
Return on assets (ROA)	741	0.118	2529	0.139	18,189	0.134
Tobins's <i>q</i>	751	1.74	2529	2.01	18,189	1.90
Total assets (\$bil)	764	5.84	2529	27.0	18,189	14.8
Firm age	774	22.7	2529	31.0	18,189	26.8
Leverage	751	0.24	2528	0.25	18,120	0.22
Volatility	765	0.45	2526	0.38	18,174	0.42
R&D (% of assets)	1013	3.59	2529	4.34	18,189	3.99
Board size	1013	9.05	2529	10.6	18,189	9.46
Board independence	1013	0.68	2529	0.71	18,189	0.70
<i>N</i>		1013		2529		18,189
<i>Panel B: Directorship characteristics</i>	Board members of acquired firms (4)		Shocked directors (5)			
	<i>N</i>	<i>Mean</i>	<i>N</i>	<i>Mean</i>		
Tenure of directorship	7941	7.99	2612	7.68		
Independent dummy	9128	0.68	3083	0.71		
Director age	9123	59.5	3082	60.8		
Female dummy	8462	0.09	2840	0.13		
Attendance violation dummy	9128	0.018	3083	0.027		
Number of board seats held	9128	1.49	3083	3.28		
Number of committee memberships (across boards)	9128	1.84	3083	4.43		
<i>N</i>		9128		3083		

**Table 4**

Merger-shocks and firm performance – OLS and reduced-form regressions.

This table presents regression estimates for Eqs. (2) and (3). Columns 1 through 4 present OLS regression results for change in firm performance on change in the total number of board seats held by the firm's directors. Columns 5 through 8 show regression results for change in firm performance on treatment of merger-shocks (Eq. (3)). Firm performance is measured by return on assets and log of Tobin's  $q$ , and multiplied by 100 for percent interpretation. The treatment variable *Treat* is defined by merger-shocks: cases in which directors had a board seat terminated due to a merger between year  $t-1$  and  $t$  (aggregated to firm-years). Control variables include firm size, board size, board independence, leverage, R&D expenses, and stock return volatility. All regressions include dummies for each industry-by-year combination (Fama-French 49 industry classification). Details on variable definition and construction appear in Appendix A. Standard errors clustered by firm appear in parentheses. \*\*\*, \*\*, and \* denote significance at the 1%, 5%, and 10% level.

Dep. var.	OLS				Reduced-form			
	$\Delta$ ROA		$\Delta$ logQ		$\Delta$ ROA		$\Delta$ logQ	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Treat</i>					0.24*** (0.09)	0.26*** (0.09)	1.01*** (0.39)	1.14*** (0.39)
$\Delta$ Number of boards	−0.023 (0.03)	−0.037 (0.03)	0.097 (0.16)	0.027 (0.16)				
$\Delta$ Firm size		−32.3*** (6.45)		−195*** (32.2)		−33.3*** (6.41)		−201*** (32.1)
$\Delta$ Leverage		−12.6*** (0.75)		−42.9*** (3.07)		−12.6*** (0.75)		−42.8*** (3.07)
$\Delta$ R&D		−33.0*** (5.11)		46.1** (18.6)		−32.9*** (5.11)		46.3** (18.6)
$\Delta$ Volatility		−4.85*** (0.48)		−25.8*** (2.04)		−4.85*** (0.48)		−25.8*** (2.04)
$\Delta$ Board size		0.022 (0.03)		−0.48*** (0.15)		0.026 (0.03)		−0.46*** (0.15)
$\Delta$ Board independence		−0.23 (0.48)		2.29 (2.37)		−0.23 (0.48)		2.27 (2.37)
$R^2$	0.17	0.23	0.28	0.31	0.17	0.23	0.28	0.31
Observations	20,718	20,581	20,718	20,581	20,718	20,581	20,718	20,581

to the instrument variable regression results in later sections, from which they differ substantially. In this sample, little evidence relates unconditional variation in number of board seats to the performance measures. The OLS estimates are small and statistically insignificant, perhaps beseeming the mixed finding in the literature. I refer to these regressions as “OLS” to distinguish them from the IV result, although they provide a “within estimator” for this relationship. In these regressions, the change in performance measure is regressed on the change in board seats, controlling for time-invariant characteristics as well as other control variables similar to the IV regressions. However, the source of variation in OLS is not limited to changes imposed by mergers. The OLS estimates use unrestricted variation in board seats due to any exogenous or endogenous reasons. The interpretation of the OLS regression is difficult since the reason for termination (or appointment) of a directorship is unobserved, and prone to many plausible biases. 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 through 4, which correspond to the effect of workload as well as various contaminating effects. The lack of interpretation serves as motivation to the identification strategy that follows.

#### 4.2. Merger-shocks and firm performance

My analysis is based on board-eliminating mergers as a quasi-natural experiment to generate exogenous variation in director appointments. To estimate the effect of a

board seat reduction, I estimate the change in firm performance measures around merger-shocks in a reduced-form approach. A control group for this purpose consists of those firms whose directors did not experience merger-shocks. The specification I regress is:

$$\Delta Performance_{it} = \beta_1 Treat_{it} + B' \Delta Controls_{it} + f_t \times f_{industry} + \varepsilon_{it}, \quad (3)$$

where  $Treat_{it}$  is the number of cases a director on board  $i$  was on a different 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 industry effects ( $f_t$  and  $f_{industry}$ ) and their 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, board independence, financial leverage, R&D expenses, and volatility. Controls appear in differences in the baseline test (but are robust to including some or all controls in levels). To account for possible within-firm serial correlation, standard errors are clustered by firm.

This approach compares a shocked firm in the year after the shock to itself in the year before the shock, and compares that after-minus-before difference to trends that control firms exhibit in similar years. The effect attributed to the board-terminating merger is the conditional average of differences in pre- to post-trends between the two groups.

Columns 5 through 8 of Table 4 present the results. The coefficient of interest is  $\beta_1$  in Eq. (3). Both earnings and market-to-book ratios increase following a merger in which a director loses a seat on an acquired board, relative

**Table 5**

Board appointments held by incumbent CEOs.

This table presents regression results for change in firm performance on merger-shocks to incumbent CEOs (Eq. (4)). The variable  $Treat_{it}^{CEO}$  indicates that the incumbent CEO of company  $i$  was a director at another firm which was acquired between year  $t-1$  and  $t$ .  $Treat_{it}^{CEO}$  equals one for 195 cases of shock and zero for all other firm-years in Compustat-CRSP during the sample period (1997–2013). Firm performance is measured by return on assets and log of Tobin's  $q$  expressed in percent. Control variables include firm size, leverage, R&D expenses, and stock return volatility as well as dummies for each industry-by-year combination. Details on variable definition and construction appear in Appendix A. Standard errors clustered by firm appear in parentheses. \*\*\*, \*\*, and \* denote significance at the 1%, 5%, and 10% level.

Dep. var.	$\Delta ROA$ (1)	$\Delta ROA$ (2)	$\Delta \log Q$ (3)	$\Delta \log Q$ (4)
$Treat_{it}^{CEO}$	1.20* (0.66)	1.36** (0.62)	2.23 (2.03)	2.31 (1.95)
$\Delta$ Firm size		−62.5*** (6.98)		−375*** (31.9)
$\Delta$ Leverage		−11.5*** (0.55)		−21.9*** (1.86)
$\Delta$ R&D		−106*** (2.40)		47.3*** (6.79)
$\Delta$ Volatility		−3.91*** (0.26)		−9.48*** (0.89)
$R^2$	0.07	0.23	0.20	0.21
Observations	71,757	71,757	74,224	74,224

to other control firms operating in the same industry at the same time but whose directors do not experience such a merger event. When performance is measured by return on assets, a merger-shock is associated with an increase of 0.24–0.26% (Columns 5 and 6). When performance is measured by Tobin's  $q$ , a merger-shock is associated with an increase of 1.01% to 1.14% (Columns 7 and 8). For comparison, in Columns 5 and 7 I estimate the baseline regressions in specifications without the firm-level control variables. Including firm-level controls suggests a similar trend of improved performance with marginally larger point estimates.

#### 4.3. Board appointments held by incumbent CEOs

To broaden the central finding on the effect of concurrent board appointments, in this section I ask if appointments to boards of other companies concurrently to a CEO position relate to performance of CEOs. I exploit the fact that some directors who experience a merger which terminates their directorships are CEOs of publicly traded companies. Treating a merger as a possible shock to those CEOs, I apply a reduced-form approach by estimating the following specification:

$$\Delta Performance_{it} = \beta_c Treat_{it}^{CEO} + B_c' \Delta Controls_{it} + f_t \times f_{industry} + v_{it}, \quad (4)$$

where  $Treat_{it}^{CEO}$  indicates that the incumbent CEO of company  $i$  was director on a board acquired in a merger between year  $t-1$  and  $t$ . A detailed description of the exact steps to identify merger-shocks to CEOs is elaborated in Appendix B. Table 5 presents the results. The firms of the 195 shocked CEOs improved in both operating profits (1.2–1.36%) and market-to-book ratios (2.23–2.31%) compared to firms whose CEOs were not shocked. Economically, the effect of a CEO's concurrent board appointment is much

stronger than for directors. Point estimates in Table 5 are well above the magnitudes of the corresponding reduced-form estimates in the director sample. The larger magnitudes are compatible with the fact that CEOs carry a more influential role in a company.

#### 4.4. Board appointments conditional on geographical distances

I exploit variation in geographical distances between company headquarters to test the workload aspect of holding multiple concurrent board seats. Traveling distances are linked to time directors spend because of the travel time incurred and because distant directors are less likely to interact between board meetings (Alam et al., 2014). Obtaining soft information is more difficult for distant directors and they are likely to be more sensitive to excessive external workloads than directors who are nearer to their firms.<sup>14</sup> I use the empirical framework to examine the interaction of concurrent board appointments with geographical distance from the terminated directorship.

The hypothesis is therefore that mergers involving farther locations should provide stronger shocks on average. A merger that terminates a distant board may eliminate a workload duty that is more demanding. By the same token, retained board seats in remote locations require more effort, and thus should benefit more from a marginal unit of extra time.

Since location data of each director's residence are unavailable, I measure distances between locations of firm headquarters. For each merger, I compute the distance between the firm whose board was dismissed (the acquired firm) and the other firm which continues to employ that director (the treated firm). By construction, this distance must be defined for each merger. However, it is only defined for the treated group since it requires a dismissed board, and therefore a standard interaction term is not feasible. In lieu of directors' residence data, I obtain zip codes of firm headquarters and country of origin from Compustat. Foreign firms are automatically classified as “far”.<sup>15</sup> If zip codes of non-foreign firms are missing in Compustat, I obtain them from the firms' filings on EDGAR or from Bloomberg.

Using that distance, I split the sample of treated firms with respect to the median distance. I define the subsample of shocked firms whose distance is greater (less) than the median distance in the sample as “far shocks” (“near shocks”). I then replicate the baseline reduced-form specification replacing the treatment variable ( $Treat$  in Eq. (3)) with treatment by far shocks and treatment by near shocks (denoted by  $Treat_{it}^{far}$  and  $Treat_{it}^{near}$ ). Columns 1 and 2 of Table 6 report the results of this test. A merger-shock that is classified as far is associated with performance gains of

<sup>14</sup> The importance of accessibility to the information that directors need to extract in order to be effective is highlighted in theory (e.g., Adams and Ferreira, 2007; Harris and Raviv, 2008) and empirical work (e.g., Duchin et al., 2010; Armstrong et al., 2014).

<sup>15</sup> See Appendix A for details on construction and calculation of the distance. A firm-year can be shocked by both a far and near shock at the same time, if more than one board member was involved in a merger. I omit these cases from the classification.

**Table 6**

Merger-shocks and firm performance conditional on geographic distance.

This table uses location of companies to study changes in firm performance around mergers that terminate board appointments. The table presents regression results where the treated group is classified according to geographical distances between the firm's headquarters and the headquarters of the firm whose acquisition originated the treatment. The variables  $Treat^{far}$  and  $Treat^{near}$  are constructed similar to  $Treat$  (in Table 4) except restricted to a subsample of merger-shocks classified as "far" or "near", respectively. Each event of merger-shock is classified as "far" if headquarter distance is greater than the median or classified as "near" if less. The variables  $Treat^{farState}$  and  $Treat^{nearState}$  are restricted to a subsample of merger-shocks classified as "far by state" or "near by state". Each merger-shock is classified as "near by state" if the two headquarters are located in the same or in bordering U.S. or Canadian states, or classified as "far by state" if not in the same state nor bordering states. Firm performance is measured by return on assets and log of Tobin's  $q$ , and multiplied by 100 for percent interpretation. Regressions include the control variables in Table 4 as well as dummies for each industry-by-year combination (although not tabulated). Details on variable definition and construction appear in Appendix A. Standard errors clustered by firm appear in parentheses. \*\*\*, \*\*, and \* denote significance at the 1%, 5%, and 10% level.

Dep. var.	Geodesic distance		State distance	
	$\Delta ROA$ (1)	$\Delta \log Q$ (2)	$\Delta ROA$ (3)	$\Delta \log Q$ (4)
$Treat^{near}$	0.21** (0.11)	0.95* (0.48)		
$Treat^{far}$	0.32*** (0.12)	1.39** (0.57)		
$Treat^{nearState}$			0.21* (0.12)	1.05** (0.53)
$Treat^{farState}$			0.30*** (0.11)	1.22** (0.50)
$R^2$	0.23	0.31	0.23	0.31
Observations	20,581	20,581	20,581	20,581

0.32% in ROA and 1.39% in Tobin's  $q$ , while a merger-shock that is classified as near is associated with only of 0.21% in ROA and 0.95% in Tobin's  $q$ . The point estimates for far shocks are noticeably larger, although the differences between the coefficients for far and near shocks are not statistically significant.<sup>16</sup>

Due to the concern that accessibility of company headquarters is not proportional to the geodesic distance in miles, I use an alternative proxy for near and far directorships. The second proxy is based on the classification of companies to U.S. and Canadian states. The advantage of this proxy is the availability of the variable for state of primary location (from Compustat), and the simplified calculation which does not require external modules to compute distances. Although states vary in shape and size, this proxy addresses the argument that state borders add an accessibility burden for reasons that go beyond traveling arrangements. For example, one reason that state borders can burden directors on multiple boards is the need to master regulation differences across states on matters that affect managerial incentives. This consideration is supported by DeLong (2001) who argues that regulation at

the state level influences the market for corporate control which impacts monitoring. Using the state location metric, I define a distance as "near" when the two firms are in the same state or in bordering states (and define as "far" otherwise). Approximately 42% of shocks are far by this rule.

Similar to the geodesic distance, the distance proxied by state defines two subsample of shocked firms according to their location. Columns 3 and 4 of Table 6 present the results of the reduced-form regressions of performance on both the near treatment and the far treatment (denoted by  $Treat^{nearState}$  and  $Treat^{farState}$ ). A merger-shock that is classified as near is associated with performance gains of 1.05% in Tobin's  $q$  and only 0.21% in ROA. These estimates for performance gains around near mergers are smaller in magnitude but not statistically different from far mergers. A merger-shock that is classified as far by the state measure is associated with 1.22% increase in Tobin's  $q$  and an increase of 0.30% in ROA. As with the distance measure in miles, these point estimates suggest a strong effect in response to termination of boards in remote states.

The interaction with geographical distances is important because it provides a more direct test for the workload effect as a mechanism for how board appointments matter to firm performance. This is because distance is naturally linked to the time directors spend, predicting a positive relationship: that directors spend more time when their board is located far. The results in Table 6 are consistent with the view that a merger that eliminates a director's board seat is shocking that director with extra time that can be used to benefit firms that continue to employ the director.

In contrast, geographical distances are not as clearly related to other aspects of merger-induced lost board seats. I consider other possible aspects given the findings of Table 6. One possible aspect is new expertise developed by directors throughout the merger process (perhaps through intense merger negotiations). It is plausible that better expertise predicts performance gains in firms which continue to employ the director. However, it is not clear why this story should predict that the expertise developed from a remote merger is greater than that developed in a near merger. Another possible aspect of a merger-induced loss of a board seat is increased incentive to secure future employment and reputation on remaining boards (perhaps adjusting priors on the credibility of takeover threats). It is plausible that increased incentives predict performance gains in firms which continue to employ the director. However, it is not clear why this story should predict that the incentives will change more when the merger is remote than when the merger is near. Another possible aspect of a merger-induced loss of a board seat is sparser professional networks (perhaps due to cutting ties with the other board members). To be consistent with the findings, this story would first have to explain why narrower networks benefit performance,<sup>17</sup> and then why that effect would be greater when the lost board seat is remote.

Any mechanism (workload, expertise, incentives, networks, or others) that suggests directors respond to a

<sup>16</sup> The statistical power to test the difference between the two coefficients is compromised due to the fact that distance is only defined for treated firms and not defined for untreated. A specification with a standard interaction term is likely to have more statistical power and produce more accurate estimates of that difference, but is not feasible since data on directors' actual location or residence are not available.

<sup>17</sup> For example, Cai and Sevilir (2012) and Andres et al. (2013) study the effect of board connections.

merger in a way that influences their firms implies that boards have causal impact on firm performance. Overall, the workload aspect of reduced board seats offers a simple explanation for performance gains and especially gains when the merger is remote, and this explanation is consistent with the data inspected in this paper.

Moreover, if the effect of merger-shocks on firm performance operates through the channel of workload, then merger-shocks provide an IV for board appointments. Under this assumption, IV estimates of the effect of board appointments on performance are meaningful. A workload explanation is vital for the validity of the IV estimates, since the instrumented variable is the change in number of board seats. The IV estimates for the effect of a board seat may be invalid if alternative mechanisms drive firm performance, because of the possibility that an alternative effect applies without a change in board seats. In contrast, the workload channel motivates the IV setting and validates its exclusion restriction, and this channel is supported by the evidence presented in this section.

#### 4.5. Instrumental variable estimation results

To estimate the effect of a board seat held concurrently by a director, I use an IV approach to instrument for the endogenous variable  $\Delta Boards_{it}$  (the change in the number of board seats) using  $Treat_{it}$  as an instrumental variable. Given the view that the effect of merger-shocks on performance is driven by a workload channel, the IV setting provides estimates for the effect of the workload which a concurrent board seat imposes on a director. For the main IV

regression, I estimate the following first-differenced specification:

$$\Delta Performance_{it} = \gamma_1 \widehat{\Delta Boards}_{it} + \Gamma' \Delta Controls_{it} + f_t \times F_{industry} + \mu_{it}, \quad (5)$$

where  $\widehat{\Delta Boards}_{it}$  is the predicted value from a first-stage regression of changes in number of board seats held by the firm's board members on the instrumental variable  $Treat_{it}$ . Additional control variables are as before, including year and industry effects ( $f_t$  and  $f_{industry}$ ) and their cross products. Standard errors are clustered by firm to account for within-firm serial correlation in  $\mu$ .

The first stage for the IV methodology is a regression of the changes in the total number of board seats held by the firm's board members on the treatment by merger-shocks and the same set of controls:

$$\Delta Boards_{it} = \delta_1 Treat_{it} + \Theta' \Delta Controls_{it} + f_t \times F_{industry} + v_{it}, \quad (6)$$

where the instrumental variable,  $Treat_{it}$ , is the number of cases directors on board  $i$  were on a different board acquired and terminated between year  $t-1$  and year  $t$  (equals zero if no shock).

First-stage regression results are presented in Columns 1 and 2 of Table 7. As expected, the merger-shock predicts changes in board seats held. While Fig. 1 shows that the number of board seats drops by 0.93 on average for shocked directors in the event year, the coefficient on  $Treat$  suggests that the conditional average is a drop of 0.85

**Table 7**

Board appointments and firm performance – IV regression results.

This table presents IV regression results for the relationship between change in firm performance and change in the total number of board seats held by the directors of the firm's board (Eq. (5)). The second-stage regressions (Columns 3–8) are for change in performance on predicted change in the total board seats, fitted from the first-stage regression (Eq. (6)) presented in Columns 1 and 2. For each firm-year, the instrumental variable ( $Treat_{it}$ ) aggregates the cases where directors on board  $i$  had a board seat terminated due to a merger between year  $t-1$  and  $t$ . Firm performance is measured by return on assets (Columns 3 and 4), log of Tobin's  $q$  (Columns 5 and 6), and abnormal stock return (Columns 7 and 8) and multiplied by 100 for percent interpretation. Abnormal return is the excess stock return for the fiscal year given a four-factor model. Control variables include firm size, board size, board independence, leverage, R&D expenses, and stock return volatility. All regressions include dummies for each industry-by-year combination (Fama-French 49 industry classification). Details on variable definition and construction appear in Appendix A. Standard errors clustered by firm appear in parentheses. \*\*\*, \*\*, and \* denote significance at 1%, 5%, and 10% level.

Dep. var.	IV 1st stage		IV 2nd stage					
			$\Delta ROA$		$\Delta \log Q$		Abnormal stock return	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Treat</i>	−0.85*** (0.03)	−0.85*** (0.03)						
$\Delta$ Number of boards			−0.28*** (0.10)	−0.30*** (0.10)	−1.18*** (0.45)	−1.34*** (0.44)	−0.87 (0.89)	−0.70 (0.90)
$\Delta$ Firm size		−2.65* (1.60)		−34.1*** (6.3)		−204*** (31.5)		−124*** (31.5)
$\Delta$ Leverage		−0.09 (0.08)		−12.6*** (0.74)		−43.0*** (3.00)		−39.1*** (4.69)
$\Delta$ R&D		−0.40 (0.33)		−33.0*** (5.01)		45.8** (18.3)		−109*** (30.0)
$\Delta$ Volatility		0.04 (0.05)		−4.84*** (0.47)		−25.7*** (1.99)		15.7*** (4.19)
$\Delta$ Board size		−0.01* (0.01)		0.02 (0.03)		−0.48*** (0.15)		−1.34*** (0.27)
$\Delta$ Board independence		−0.01 (0.08)		−0.23 (0.47)		2.26 (2.32)		2.86 (4.32)
$R^2$	0.18	0.18	0.17	0.23	0.28	0.31	0.17	0.18
Observations	20,718	20,581	20,718	20,581	20,718	20,581	20,694	20,581



board seats. It implies a limited degree (0.15) in which some directors replace the terminated board seat with a new appointment within the year. In Column 2, the first-stage coefficient estimates show how other factors predict the degree of replacement. For example, as their remaining firms increase in size, directors are less likely to replace the terminated board seat within the year. Overall, the first stage finds a strong correlation between merger-shocks and change in total board seats. The instrument is strong and satisfies the relevance criterion. *F*-statistic for the first-stage regression is 2433.6 ( $p < 10^{-4}$ ).

Table 7 shows the IV regression result. The second-stage estimates (Columns 3–8) for the coefficients on the change in number of board seats are negative, suggesting that increased board appointments are associated with lower performance. A reduction of one board seat is associated with an increase of about 0.3% in ROA and an increase of about 1.3% in Tobin's *q*. I use both ROA and Tobin's *q* as the measures of firm performance throughout the study (in which all panel datasets are yearly). The only exception is Columns 7 and 8 of Table 7 where firm performance is proxied by abnormal stock return. Although stock returns are a noisier measure at yearly horizon, I include this specification for completeness. With performance measured by abnormal stock return, the point estimate for the coefficient of change in number of board seats is between  $-0.70\%$  and  $-0.87\%$  (depending on the specification). This coefficient has the expected sign but is not statistically significant.<sup>18</sup> Stock returns are risk adjusted accounting for four factors from Kenneth French's website, although using one market factor or simply raw returns gives qualitatively similar results. These returns are effectively industry adjusted as well since the regressions already include industry dummies. I also estimate the first- and second-stage regressions in specifications without the control variables. As before, omitting the controls from the specifications has a limited impact on coefficient estimates and does not change the interpretation of the findings.

Compared to the reduced-form estimates, the IV estimates yield coefficients larger in absolute value for ROA and Tobin's *q*. Under the identifying assumption, the reason can be attributed to the fact that merger-shocks reduce the number of board seats by a bit less than one seat. The first-stage regression is very strong in this setting, and suggests that merger-shocks reduce approximately 0.85 board seats on average (conditional on covariates and fixed effects). In particular, reduced-form estimates that are close to 0.85 of the effect attributed to one board seat are entirely consistent with the narrative that affected directors receive extra free time: approximately 15% of them quickly spend that time by joining a new board. The flip side of this premise is also entirely consistent: firms quickly recruit about 15% of the directors who received extra time.

According to the view that eliminated board seats free up some time for directors, estimating an effect of board seats adheres to the fact that about 15% of shocks do not reduce net board seats (by the end of the year) while about 85% reduce board seats. The reduced-form estimates are presumably smaller due to this fact (also smaller than the corresponding IV estimates due to it). Alternative views may not share this distinction: they may consider shocks equally important whether board seats are lost or replaced. For example, a direct takeover effect might apply through takeover proceedings or merger negotiations that directors experience. If a strong direct takeover effect drives performance gains (rather than workload), it should apply also through the 15% of shocks that do not reduce the net number of board seats and might yield larger reduced-form estimates. Unlike the reduced-form specification, the IV estimation introduces the number of board seats parameter and uses data on actual board appointments to measure it. Under the identifying assumption, I interpret the IV coefficient as the cost a firm incurs per one external board seat held by a member of its board.

In terms of magnitude, these IV estimates are economically meaningful. A removed board seat is associated with increased ROA (0.28–0.30%) and increased Tobin's *q* (1.18–1.34%) depending on the specification. For the median S&P1500 company, these estimates suggest a removed board seat is worth roughly \$6M in operating earnings or almost \$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 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 et al. (2014), 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 holding multiple board seats may relate to very large trends in ROA and Tobin's *q*. In that sense, my estimates are possibly conservative yet economically meaningful.

Finally, I also use the IV setting in conjunction with geographical distances between company headquarters. Under the premise that distant board seats require more effort, I define treatment by far shocks similar to before. Table 8 presents IV regression results based on variation in board appointments induced by far mergers. These regressions replicate the baseline IV specification (Eq. (5)) using  $Treat_{it}^{far}$  or  $Treat_{it}^{farState}$  in place of the usual

<sup>18</sup> The fact that annual stock returns are a noisier series is consistent with their rare use in studies on board structure and firm performance, especially in comparison to ROA and Tobin's *q*. One exception is Duchin et al. (2010) who use ROA, logQ, and stock returns to measure firm performance. In their sample as well, the stock returns variable is substantially noisier than ROA and logQ.

**Table 8**

IV regression results using geographically distant mergers.

This table shows IV regression results for the relationship between change in firm performance and change in the number of board seats (Eq. (5)), using  $Treat^{far}$  or  $Treat^{farState}$  as instruments for the change in the number of board seats in place of the usual instrument  $Treat$ . The variables  $Treat^{far}$  and  $Treat^{farState}$  are constructed similar to  $Treat$  except restricted to a subsample of merger-shocks classified by distances between the firm's headquarters and the headquarters of the firm whose acquisition originated the treatment. Merger-shocks are "far" if distance is greater than the median, and "far by state" if the two headquarters are not in the same nor bordering states. Firm performance is measured by return on assets and log of Tobin's  $q$ , and multiplied by 100 for percent interpretation. Regressions include the control variables in Table 4 as well as dummies for each industry-by-year combination (although not tabulated). Details on variable definition and construction appear in Appendix A. Standard errors clustered by firm appear in parentheses. \*\*\*, \*\*, and \* denote significance at the 1%, 5%, and 10% level.

Dep. var.	Geodesic distance			State distance		
	IV 1st stage	IV 2nd stage		IV 1st stage	IV 2nd stage	
	(1)	$\Delta ROA$ (2)	$\Delta \log Q$ (3)	(4)	$\Delta ROA$ (5)	$\Delta \log Q$ (6)
$\Delta$ Number of boards		−0.37** (0.15)	−1.62** (0.69)		−0.35*** (0.13)	−1.41** (0.60)
$Treat^{far}$	−0.805*** (0.041)					
$Treat^{farState}$				−0.810*** (0.036)		
$R^2$	0.11	0.23	0.31	0.12	0.23	0.31
Observations	20,581	20,581	20,581	20,581	20,581	20,581

instrument. The IV estimates in Table 8 suggest that reductions in distant board appointments are highly beneficial to the firms employing the directors. Using geodesic distances (Columns 1–3), results suggest that a reduction of board appointment is associated with significantly higher ROA by 0.37% and higher Tobin's  $q$  by 1.62%. Performance gains in ROA and Tobin's  $q$  for each reduced board appointment (0.35% and 1.41%) appear also when distances are defined according to state borders (Columns 4–6). These negative coefficients suggest a large performance gain in response to elimination of boards that are remote. The point estimates using far shocks are larger than the estimates obtained with the baseline IV regressions (Table 7), although the estimates for far boards and for unconditional boards are not statistically different from each other.

#### 4.6. Subsequent committee membership and changes in board inputs

In this section I explore two mechanisms through which directors can affect firm performance: a director's ability to participate in committee assignments and attendance in board meetings. The following tests provide evidence on changes in the behavior of directors that are shocked by mergers. This section traces down changes in behaviors of those directors which is consistent with the finding that their firms perform better, and consistent with the hypothesis that the merger-induced shocks free up some time for directors.

First, I examine a possible channel through which directors can affect firm performance: their ability and willingness to participate in committee assignments. Assignments to board committees has been shown to be positively related with firm performance (Jiraporn et al., 2009). Yet, committee work requires members (and especially chairs) to devote substantial time and effort.

Directors can be more influential as members of committees, since many monitoring-related decisions such as auditing, governance, and executive compen-

sation are made in the forum of board committees (Adams and Ferreira, 2009). To examine the effect of concurrent board appointments on committee participation as member or chair I estimate the following instrumental variables specification:

$$I_{ijt}^{joinCommit} = \lambda_1 \Delta \widehat{Boards}_{ijt} + \Lambda' \Delta Controls_{it} + f_t \times f_{industry} + \epsilon_{ijt}, \quad (7)$$

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. The instrumental variable  $Treat_{ijt}$  captures cases of director  $j$  being on a board that was acquired and terminated between year  $t-1$  and year  $t$ . The dependent variable,  $I_{ijt}^{joinCommit}$ , is equal to one if director  $j$  obtained new membership<sup>19</sup> in board  $i$ 's audit, compensation, governance, or nominating committees.

Table 9 shows results of IV estimation for Eq. (7) and the corresponding probit regression of changes in performance on the merger-shocks. Columns 1 through 3 show increased likelihood of directors joining committees in the directorships they continue to hold following the merger that reduces their external board duties. The positive coefficient in Column 1 and the negative coefficient in Column 3 are consistent with the view that directors use extra time to participate in more committees. A reduction of one board seat elsewhere is associated with approximately 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 17%. In addition, this trend is con-

<sup>19</sup> It is possible that a director would both join committees and leave other committees on the same board. Although a switch requires adjustment, the required time to adjust is not clear, and thus I exclude cases of new memberships if the director leaves more committees than joining.

**Table 9**

Propensity to join board committees, chair board committees, and attend board meetings.

This table documents the relationship between change in board appointments and likelihood of a director's participation in time-intensive board activities. The sample is a panel of directorship-year observations consisting of all the directorships within the firms in the baseline sample (used for Tables 4–7). The dependent variables are an indicator for joining a board committee (Columns 1 and 3), an indicator for becoming chair of a board committee (Columns 4 and 5), and the change in formally reported attendance violations (Column 6 and 7). The committees included are audit, compensation, governance, and nominating committees. The second-stage regressions are for each dependent variable on predicted change in a director's number of board seats, fitted from the first-stage regression (Column 2) of the change in a director's board seats on the instrument  $Treat_{ijt}$  that indicates director  $j$ 's board seats terminated due to mergers between year  $t-1$  and  $t$ , and a full set of controls and fixed effects. In Column 7, estimation is limited to pre-SOX years. Columns 1 and 4 present probit regressions of the dependent variable on the treatment variable. The reported coefficients for the probit regressions are the estimated marginal probabilities. Regressions include the control variables in Table 4 as well as dummies for each industry-by-year combination (although not tabulated). Details on variable definition and construction appear in Appendix A. Standard errors clustered by directorship appear in parentheses. \*\*\*, \*\*, and \* denote significance at the 1%, 5%, and 10% level.

Dep. var.	Joined any committee			Became chair of any committee		$\Delta$ Violation of attendance	
	Probit (1)	IV 1st stage (2)	IV 2nd stage (3)	Probit (4)	IV 2nd stage (5)	IV 2nd stage (6)	1996–2002 only IV 2nd stage (7)
Treat	0.031*** (0.007)	−0.850*** (0.012)		0.022*** (0.005)			
$\Delta$ Boards			−0.041*** (0.010)		−0.030*** (0.008)	0.0016 (0.004)	0.0036 (0.007)
$R^2$	0.039	0.128	0.039	0.014	0.006	0.006	0.005
Observations	147,032	147,232	147,232	145,516	146,166	163,986	58,719

sistent with the increase in committee memberships seen in Fig. 1, starting at event year 1. While Fig. 1 shows the trend in a simple univariate average across the affected directors, the regression confirms the trend in a multivariate analysis with control variables and a control group.

Next, I find a similar trend for the likelihood of directors becoming chairs of board committees. In Columns 4 and 5, Table 9 presents marginal probability estimates from a probit model and IV estimation results for a specification similar to Eq. (7) but where the dependent variable ( $I_{ijt}^{becomeChair}$ ) is a dummy that indicates a new appointment to chair a committee. The results show that after a reduction of a board appointment, that likelihood increases by nearly 3%. This effect is a substantial increase from the average likelihood of about 7%.

Another time-intensive requirement of directors is to attend board meetings. The time aspect of attendance makes it relevant to the investigation of director workload. The underlying assumption is that marginal cost of time is high when facing a heavy total workload. A heavy workload obstructs both the ability of directors to devote time and their willingness to devote it. Full attendance records would be useful to proxy for time devotion and test this view. However, data on attendance in board meetings are generally not available. The available data only indicate a director's violation of attendance defined by attending less than 75% of board and committee meetings in the fiscal year.<sup>20</sup> With this definition, violations are extremely rare, formally occurring in only 1.49% of observations in the directorship sample. Consequently, any statistical test relying on this variable should have very low power.

Despite the shortcomings of the available data, the attendance violation variable is used in several recent

studies. For instance, Li and Srinivasan (2011) consider directors' attendance as a proxy for director diligence and effort and Masulis and Mobbs (2014) consider attendance as a measure of a director's commitment to directorship responsibilities. These studies and others find that the attendance violations of the 75% threshold are associated with various director and firm characteristics.<sup>21</sup>

For its importance concerning directors' time, I explore the attendance violation data despite the caveats. I use the IV setting in the directorship level since attendance violations apply to individual directors. Column 6 of Table 9 reports IV regression estimates for change in attendance violations on instrumented change in board appointments. Here, the dependent variable indicates increased violations (lower attendance),<sup>22</sup> therefore a positive coefficient means busier directors attend less meetings. As expected given the low power, the estimates are not statistically significant. Although the magnitude of the coefficient estimate (0.16%) seems small, it applies with respect to a very rare event; the unconditional likelihood of violations in the full sample is only 1.49%. The positive sign of the coefficient on instrumented change in board seats (Column 6) is consistent with the view that eliminating board seats frees time for directors to devote to boards they remain on. However, it should be interpreted with caution and taken with the caveat that reported violations are rare and the extent to which they indicate low effort is unclear.

<sup>20</sup> I obtain the attendance data from ISS. Its source is proxy statements in which the regulatory requirement of 75% is set. In addition, this variable may be a weak proxy for time directors devote since firms are allowed to count participation by video or phone conference as attendance.

<sup>21</sup> For example, attendance violations are negatively associated with: presence of a founder (Li and Srinivasan, 2011), relative importance of the directorship to the director (Masulis and Mobbs, 2014), female directors (Adams and Ferreira, 2009), and positively associated with the meeting fees (Adams and Ferreira, 2008). Elyasiani and Zhang (2015) find that busy directors are not associated with a greater probability of violations.

<sup>22</sup> In addition to the test's drawbacks, the dummy dependent variable in differences implicitly assigns equal weights to a new violation and ending a violation, by assigning numeric values to each possible change in behavior (+1 or −1), which may not be desired.

Finally, Column 7 presents results for a similar regression in the subperiod ending in 2002. The motivation for limiting the sample to the early years is the fact that violations of directors' attendance are substantially more common before the enactment of the Sarbanes-Oxley Act (SOX). Ferreira (2010) finds that attendance improved dramatically after SOX took effect in 2002, such that extremely few violations of attendance are reported in later years.<sup>23</sup> This trend appears in my sample as well. In an attempt to alleviate the low power, I run this test in pre-SOX years. Point estimate for change in board appointments is 0.36% (Column 7). As in the full sample period, the coefficient is of the right sign (a positive relation means violations increase with board appointments), although not statistically significant.

Overall, the results on participation in board activities corroborate the effect of workload given the view that participation is a mechanism for directors to use their time in a way that can benefit the company. Moreover, the literature that documents a positive relationship between committee membership and firm performance raises the question of why firms do not assign more memberships. My finding that less busy directors are more inclined to accept memberships suggests a possible answer: the reason could be the workload that directors are facing.

## 5. Robustness and competing explanations

This section presents two main tests to check the robustness of the findings and the interpretation of the findings as an effect of workload. 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, because the bid was withdrawn at some stage of the merger process. Although a merger bid took place and was possibly negotiated, the company 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 consider specifications which define a finer control group or treated group: (i) restricted to firms that are ever shocked in the years before the shock; (ii) in a matched sample based on a “nearest neighbor” propensity score matching method; and (iii) excluding mergers-shocks in which target and treated firms are in the same industry.

### 5.1. Direct takeover effect and withdrawn takeover bids: a falsification test

This section addresses the possibility of a direct effect of takeover bids on directors of acquired firms. Such an effect may apply if the takeover proceedings provide the directors of the acquired firm with new incentives or learning opportunities that improve their managerial decision making on other boards. A direct takeover effect is relevant to the study of director workload because it suggests an alternative channel through which merger-shocks may relate

to the operation of other firms still employing the directors. 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 were withdrawn. These include bids which were announced to the markets, prompted acquirer and target responses, were negotiated but ultimately withdrawn.<sup>24</sup>

These bids are interesting since they provide a “falsification test” for the workload interpretation of the baseline findings. If the findings were strongly driven by factors specific to mergers and acquisitions, we might expect a comparable effect for mergers 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 terminate target boards in lieu of the usual merger-shocks. The apparatus is otherwise similar to the baseline regressions of change in performance on (completed) merger-shocks (see Table 4, Columns 5–8). I estimate the relation between merger bids and performance of firms which share directors with acquisition targets. To identify withdrawn mergers, I start with the universe of Thomson One Mergers and Acquisitions database applying the same filters used to identify completed mergers except for requiring the bids have status of “withdrawn” rather than “complete.” These steps identify 102 target firms in the ISS database panel which had a merger bid withdrawn. Similar to the construction of completed mergers-shocks, the target firm is not part of the treated group; only the other remaining board seats are considered shocked. After aggregating to firm level, 322 firm-years are available as a treated group of placebo treatment.

I define the variable  $NonTreat_{it}$  to indicate that one or more directors of firm  $i$  were on the board of a firm receiving a withdrawn bid during the year that ends at time  $t$ . The appropriate setting to test withdrawn bids is a reduced-form<sup>25</sup> specification analogous to Eq. (3). I replicate the baseline test as close as possible using the  $NonTreat_{it}$  variable in place of the usual merger-shock variable  $Treat_{it}$ . The specification is:

$$\Delta Performance_{it} = \beta_0 NonTreat_{it} + B_0' \Delta Controls_{it} + f_t \times f_{industry} + e_{it}. \quad (8)$$

This test provides a falsification test in that it examines directors who are involved in merger bids but whose board appointments are not shocked. For this purpose, the experience (or incentives) gained through proceedings of the withdrawn bid proxies for the experience (or incentives) gained through a completed acquisition. To the extent that the experiences are comparable in both types of events, Treat and NonTreat, this test may capture the direct takeover effect net of the workload effect. To avoid contaminating the test with the effect of completed mergers, I exclude the firms shocked by completed mergers from this regression. This step ensures that placebo-shocks are clean

<sup>23</sup> The important role of regulation in setting the incentives to eliminate violations is discussed in Adams and Ferreira (2012). Consistent with this view, the near elimination of violations since 2003 can be plausibly attributed to the regulation set by SOX.

<sup>24</sup> Seru (2014) and Li (2013) also exploit mergers that ultimately fail or are withdrawn to study the effects of completed mergers.

<sup>25</sup> Since directorships are not terminated, a first stage would fail and the IV setting would not provide a proper falsification test.



**Table 10**

Robustness tests: withdrawn mergers (“placebo” test).

This table presents regression results of a falsification test using withdrawn mergers in place of completed mergers. Change in firm performance is regressed on a placebo-treatment imposed by withdrawn merger bids (Eq. (8)). The variable *NonTreat* is constructed similar to *Treat* (in Table 4) except based on withdrawn merger bids. Firms shocked by completed mergers are excluded from the regressions. Firm performance is measured by return on assets and log of Tobin's *q*, and multiplied by 100 for percent interpretation. Regression includes the control variables in Table 4 as well as dummies for each industry-by-year combination (although not tabulated). Details on variable definition and construction appear in Appendix A. Standard errors clustered by firm appear in parentheses. \*\*\*, \*\*, and \* denote significance at the 1%, 5%, and 10% level.

Dep. var.	$\Delta$ ROA (1)	$\Delta$ logQ (2)
NonTreat	0.01 (0.26)	−0.20 (1.33)
R <sup>2</sup>	0.23	0.32
Observations	18,058	18,058

of capturing a target's ripeness for acquisition and likelihood to be targeted again. It also ensures that the control group for the falsification test is not skewed by the completed merger-shocks which relate to better performance (as seen in previous sections).

If the workload effect—rather than a direct takeover effect—is driving the results, it does not predict finding evidence of a relationship between the “placebo-shocks” and performance of firms that share directors. Table 10 shows exactly that: I do not find evidence of such a relationship in either operating profits (0.01) or market-to-book ratios (−0.20). The estimated relationship between firm performance and placebo-shocks is small. For Tobin's *q*, the point estimates for the coefficients on placebo-shocks are negative, while a direct takeover effect would predict positive coefficients.<sup>26</sup> However, my interpretation of Table 10 is that estimates suggest a weak effect if any rather than a directional trend. Statistically, they are indistinguishable from zero.

It should be noted that this regression is only a falsification exercise. Its interpretation is merely a failure to reject the null hypothesis of the irrelevance of a “direct takeover effect”, rather than statistically proving that placebo-shocks have no effect. The results only speak indirectly in favor of the workload effect by failing to provide evidence for the alternative. Moreover, this attempt to provide evidence for a direct takeover effect is limited by the fact that many fewer withdrawn mergers are available than completed mergers. The weak conditional correlation in Table 10 (between performance gains and placebo-shocks) is only based on the available sample with relatively few placebo-shocks.

To that extent, the alternative channel of direct takeover effects seems less plausible as a dominant driver of the baseline findings. If the placebo-shocks would predict changes in performance of firms who share directors with

the 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 firms—an interesting result for its own sake. However, I find no evidence to support this alternative story.

## 5.2. 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, such as common trends in year and industry. To test the robustness of the results, in this section I consider two 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 observation to an untreated observation that has the closest propensity scores.

Table 11 shows the results of these tests. Columns 1 and 2 present IV estimates for a specification similar to the baseline IV regressions (in Table 7), but with a limited sample as follows. All firms that were not shocked in any year through the sample are not used in this regression. For the firms that are shocked, I also exclude all firm-year observations after the shock (if firms are shocked more than once through the period, I exclude any observation after the first shock). The remaining observations define a control group of firm-years that come before each shock. Change in performance is, on average, higher for shocked firm-years relative to unshocked firm-years of those companies, although statistical power is compromised due to fewer observations. The negative IV point estimates suggest improved performance following reduction of external board seats—relative to other years of the same firm.

Columns 3 and 4 of Table 11 present results for the IV regression in a matched sample. This test essentially compares the treated group to a control group of similar firm-years (matched based on observable characteristics). Comparing the two groups produces similar estimates, suggesting improvement in performance with reduced board appointments relative to control firms with similar observable characteristics. The propensity scores are computed using firm characteristics including all the control variables used in the regressions as well as industry and year effects. The reported IV regression estimates are based on a single closest neighbor matching ( $n = 1$ ) but are robust to using more than one neighbor per treated firm-year.

## 5.3. Industry effects and merger waves

In this section I address industry effects as an identification concern. The concern is that industry effects confound both performance gains of treated firms and mergers of target firms. If so, the regression findings may be merely a consequence of the confounding factor, rather than directors' responses to board appointment changes. Industry considerations would need to involve a time-varying omitted variable that explains high performance specifically at the timing of mergers. This possibility relies on empirical facts that mergers tend to come in waves, and that merger

<sup>26</sup> A small negative effect could potentially be explained by some elevated workload in the weeks leading to the withdrawal of the bid. However, my interpretation is that the coefficients seem close to zero rather than directional.



**Table 11**

Robustness to alternative sets of treated and control firms.

This table presents IV regressions using finer definitions of treated and control firms. Estimates are for second-stage regressions of change in firm performance on predicted change in the total number of board seats held by firm's directors (Eq. (5)) fitted from the corresponding first-stage regression (Eq. (6)). In Columns 1 and 2 the sample is limited to firms that are ever shocked in the panel sample, and limited to the years before the firm is shocked. In Columns 3 and 4 the control group is defined using the "nearest neighbor" propensity score matching method to match each treated observation to a single ( $n=1$ ) control observation with the closest propensity scores. The observable characteristics used for the propensity score are the control variables in Table 4 including industry-by-year dummies. In Columns 5 and 6 the treated group is constructed excluding shocks in which the target firm and the firm that continues to employ the affected director are in the same industry (Fama-French 49 industry classification). Firm performance is measured by return on assets and log of Tobin's  $q$ , multiplied by 100 for percent interpretation. Regression includes the control variables in Table 4 and industry-by-year dummies (although not tabulated). Details on variable definition and construction appear in Appendix A. Standard errors clustered by firm appear in parentheses. \*\*\*, \*\*, and \* denote significance at the 1%, 5%, and 10% level.

Dep. var.	Only firms that are ever shocked IV 2nd stage		Matched sample of nearest neighbor IV 2nd stage		Excluding shocks in the same industry IV 2nd stage	
	$\Delta$ ROA (1)	$\Delta$ logQ (2)	$\Delta$ ROA (3)	$\Delta$ logQ (4)	$\Delta$ ROA (5)	$\Delta$ logQ (6)
$\Delta$ Boards	−0.22 (0.16)	−1.53* (0.80)	−0.30** (0.13)	−1.25** (0.58)	−0.31*** (0.10)	−1.25*** (0.46)
$R^2$	0.30	0.36	0.31	0.38	0.23	0.31
Observations	4150	4150	4848	4857	20,581	20,581

wave periods can be industry specific. While these facts on merger waves are plausible, explaining the findings with industry-specific merger waves requires two additional assumptions: (i) that industry-specific merger waves coincide with periods of high firm performance in the industry, and (ii) that target firms and treated firms are in the same industry.

I address the concern of industry effects in three main ways. First, by controlling for industry-by-year fixed effects in my regressions. The industry-by-year fixed effects absorb periods of flourishing industries thus negating assumption (i). Even if industries flourish during merger wave periods, the regressions suggest that treated firms perform better even beyond their industry in that year. Second, I address industry effects by checking the extent to which assumption (ii) applies. While it is plausible that some directors' prefer a certain industry, directorships in the same industries are problematic due to competition between firms. Competition can create complications that directors would rather avoid, limiting the extent of directorships in the same industry. I check the prevalence of industry overlap in concurrent directorships in the ISS sample and find that the overlap is modest. The frequency of overlapping industries is not very different from the frequency that a random distribution of industries to concurrent directorships would generate. Since the prevalence of overlapping industries is modest, industry effects are not likely to drive the regression results.

Third, I address the concern of industry effects with a robustness test that excludes merger-shocks in the same industry. Specifically, I repeat the regressions in a sample excluding the cases of merger-shock where the target firm and the firm that continues to employ the affected director are in the same industry. Since the prevalence of overlaps is modest, not many cases of merger-shocks need to be discarded and the results change very little. Columns 5 and 6 of Table 11 show estimation results of the baseline IV regressions clean of industry-effect concerns. The results are also robust to excluding merger-shocks in the same one- or two-digit SIC codes. Based on these points, I rule out merger waves as an identification concern and

conclude that industry effects are unlikely driving my findings.

## 6. Conclusion

This paper exploits variation in board appointments generated by mergers in order to examine the direct impact of concurrent board appointments held by a firm's directors. The main finding is that a reduction in board appointments is associated with increased operating profits and higher market-to-book ratios. The baseline finding that performance of one firm responds to a merger of another firm speaks to the question of whether boards matter. Under the assumption that the two firms are only related through their shared director, it follows that this director is channeling the response. The finding that this director can add value is evidence of the relevance of boards. This is the first contribution of the paper.

The next step is to investigate possible underlying mechanisms for the effect of a change in board appointments. Naturally, a central element of a directorship-terminating merger is the potential reduction in shocked directors' workload. Yet, I also consider the extent of evidence for other explanations as to why performance responds to merger-shocks, such as change in network, higher skill or expertise (e.g., through learning), or change in incentives (e.g., through takeover threat). Examining the empirical relevance of these mechanisms, I find that the collective evidence is consistent with the workload effect narrative. I interpret the findings as support that the most likely explanation is reduced 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. Reductions in board seats are especially beneficial when geographically remote. Second, reduction of board seats is followed by increased involvement through board committees, a factor which requires substantial time devotion and may be limited by available time. Third, a falsification test designed to capture potential direct effects of merger bids (net of the effect of workload) does not produce evidence that supports an alternative direct takeover effect. Overall, I find little

evidence for other underlying aspects of reduced appointments, while the workload narrative explains the findings naturally and intuitively.

This paper emphasizes the workload aspect of multiple board appointments, and does not intend to compare that effect to the potential welfare benefits of unrestricted utilization of popular directors on multiple boards. Yet, the literature proposes several benefits of multiple concurrent directorships, such as denser networks, enhanced experience, and repeated use of a director's skills and expertise. I conclude that (i) firm performance is affected by its directors' appointments to other boards, (ii) boards have real impact, and can add value to the firm—not just window dressing, (iii) 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, (iv) director workload is the most probable reason for why changes in board seats affect performance: when directors work less elsewhere they do more to benefit the company, and (v) although the effect of workload is meaningful, it is not irrecoverable: other factors (such as proximity to headquarters) can alleviate director busyness. The latter highlights that this paper does not directly address the debate on restricting concurrent board appointments and does not imply that intervening with firms' hiring decisions is optimal.

## Appendix A. Variable definitions

Variable	Definition	Source
Abnormal stock return	annualized intercept from a four-factor model, calculated as the constant coefficient from a regression of the firm's stock returns (from CRSP, net of RF from French's website) on the variables RMRF, SMB, HML, and MOM downloaded from Kenneth French's website. Calculated with daily returns using each trading day in the fiscal year, and converted to annual terms.	CRSP, Kenneth French's website
Board independence	the fraction of independent directors on the board where independent directors are those classified as independent in ISS' classification (classification = 'I'). The number of independent directors is divided by board size.	ISS
Board size	the number of board members in a firm in a given year.	ISS
Change in number of boards (director level)	change in a director's total number of board seats held in firms in the ISS sample, accounting for possible changes in ISS coverage (defined only in differences). Calculated for any director $j$ as the number of boards in ISS with director $j$ at year $t$ , but without director $j$ at year $t-1$ minus the number of boards in ISS with director $j$ at year $t-1$ , but without director $j$ at year $t$ .	ISS
Change in number of boards (firm level)	change in the total number of seats on any S&P1500 board, held by members of the firm's board. Calculated by aggregating each board member's change in number of boards. (defined only in differences)	ISS
Change in number of committees (director level)	change in the director's total number of committee memberships (as member or chair) across all directorships in the ISS sample. Calculated accounting for possible changes in ISS coverage. (defined only in differences)	ISS
Firm age	the number of years since a firm's first appearance in Compustat.	Compustat
Firm size	book assets (at).	Compustat
Far (near) headquarter distance	equals one if headquarter distance is greater than (less than) the sample median of headquarter distance between a firm and the firm acquired in the merger-shock (defined for treated). Foreign firms automatically classified as far.	Compustat, CivicSpace
Far-by-state distance	equals one if the state of primary location (from Compustat) of the firm and of the firm acquired in the merger-shock, are not the same or adjacent U.S. or Canadian states. (defined for treated)	Compustat
Headquarters distance	distance in miles between (two) firm headquarters. Calculated by converting firm zip codes into latitude-longitude pairs (using "CivicSpace US ZIP codes Database" by Geocoder) and computing distances between latitude-longitude points with Stata GeoDist command.	Compustat, CivicSpace, EDGAR, Bloomberg
Industry	Fama-French classification (of SIC codes from Compustat) to 49 industry groups.	Compustat
Join committee dummy	equals one if the director was appointed to audit, compensation, governance, or nominating committees during that year.	ISS
Leverage	total debt (dlc + dltd) divided by total assets (at).	Compustat
Near-by-state distance	equals one if the state of primary location (from Compustat) of the firm and of the firm acquired in the merger-shock, are the same or adjacent U.S. or Canadian states. (defined for treated)	Compustat
Number of boards (director level)	the total number of seats held by the director on S&P1500 boards. Calculated by counting appearances in the ISS sample, and thus not including positions such as directorships in private companies and smaller public companies outside S&P1500.	ISS
R&D	R&D expenditure (xrd) divided by total assets (at). If xrd is missing, defined as the industry average for that year.	Compustat
Return on assets (ROA)	income before depreciation (oibdp) divided by book assets (at).	Compustat
Tenure of directorship	number of years since first joining the board as director.	ISS
Tobins's $q$	book assets (at) plus the market equity (csho*prcc), minus common equity (ceq), divided by book assets (at).	Compustat
Violation of attendance dummy	equals one if the company reported that the director attended less than 75% of board meetings during the year.	ISS
Volatility	annualized standard deviation of stock returns (from CRSP) calculated with daily returns for each trading day in the fiscal year.	CRSP
Zip code	zip code of the firm's headquarter address (from Compustat). If zip code is missing in Compustat and the firm is not foreign, I obtain the zip code from the firm's Securities and Exchange Commission (SEC) filings on EDGAR or from Bloomberg.	Compustat, EDGAR, Bloomberg

## Appendix B. Detailed construction of merger-shock events

Shocked directors sample: I use the Thomson One Mergers and Acquisitions database to identify the fiscal years in which firms covered by ISS are a target of successful mergers. The standard filters in the literature apply, which exclude repurchases, recapitalizations, exchange offers, privatizations, and uncompleted bids. Additional filters regarding value of transactions are not needed here since any target in ISS is of sufficient value. 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 in ISS. For most targets, the final year in ISS precedes the year of merger completion (in Thomson One) since once a merger is complete the firm is not around for the following ISS issue. Following Jenter and Lewellen (2015), to ensure including cases in which the bid occurred after the end of the firm's last reported fiscal year, I allow a gap of one year between the target's last year in ISS and in Thomson One (and discard mergers if the gap exceeds one year).

These criteria identify 1013 mergers of target firms employing 9128 directors in the last year before the merger, most of whom did not hold additional directorships and thus are not part of the treated sample. These directors held a total of 13,312 board seats (including 9128 target boards). They retained 3623 directorships in the ISS panel, 3083 of which have all the data fields in the pre- and post-year required for first-differenced regressions. The board seats which they retain must persist through the year of the merger to the year after in order to observe differences in firm outcomes (identical data requirements apply to the control group in the sample). I refer to these directorships as “shocked” and construct the variable  $Treat_{ijt}$  to represent these directorship-years. The shock is defined as the first year in which the director is free from a previously held directorship (terminated due to merger). Finally, the shock variable is aggregated to the firm level such that  $Treat_{it}$  represents fiscal years in which one or more directors on the board were shocked.

Shocked CEOs sample: To explore board appointments held concurrently to a CEO position, I identify 195 “shocked” CEOs using the following procedure. I start with all (9128) directors who served on the boards that were terminated when the company was acquired. Absent any file linking between ISS director identifiers and Capital-IQ executive identifiers, I first link the company in which they served as directors to its company identifier in Capital-IQ. I then search for each candidate within the list of individuals associated with the company in the Capital-IQ Professionals' Compensation database, based on their surname and requiring an exact match of spelling. First and middle names do not help matching the datasets since they are often abbreviated or recorded in alternative forms.

A total of 8543 directors satisfy these criteria and are candidates for matching a person-identifier in Capital-IQ. Despite confirming two fields (surname and firm), additional screening is needed since it is common for family members to serve on the same board. Therefore, I apply a third filter using the year of birth variables, available in ISS and in Capital-IQ “Compensation Summary”

database. For each candidate, I check whether the year of birth field is the same in the two databases. Since year of birth in ISS is inferred based on a director's age (an integer), I allow up to one year difference between the two: if the discrepancy exceeds one year I exclude the observation. Only candidates who match all three indicators (year of birth, surname, and firm of directorship) are attributed their Capital-IQ person-identifier.

This procedure yields 3574 professionals who experienced a merger-shock, 313 of which are CEOs of companies in Capital-IQ. Merging with Compustat and CRSP leaves 253 cases, of which 195 are firm-years with complete data for the regressions. I discard Compustat observations with total assets less than \$10 million. Similar to the treatment variable of directors, I define the variable  $Treat_{it}^{CEO}$  as the analogous treatment of CEOs. This variable indicates a firm-year whose CEO lost a directorship to merger. CEO shocks are limited to 1997–2013 since their construction is based on ISS data (earlier or later cases would be missing data entries). However, the shocked firms are generally not covered by ISS, and therefore board and director information is not available for this test. Since the shocked firms may be any firm in the intersection of Compustat and CRSP in the period 1997–2013, the appropriate universe for this test is all Compustat-CRSP firms in the year range.

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