CEO Turnover and Director Reputation[☆]

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Abstract

This paper analyzes the reputational effects of forced CEO turnovers on outside directors. Directors interlocked to a forced CEO turnover experience large and persistent increases in withheld votes at subsequent re-elections relative to non-turnover-interlocked directors. Reputational losses are larger for turnovers with a higher potential for disrupting a firm's management, for directors favorably inclined to the CEO, and for directors with a committee-based responsibility for monitoring the CEO. Our results imply that the average forced CEO turnover signals a governance failure at the board level, and that investors rely on salient actions to update their beliefs about directors' hidden qualities.

Keywords: CEO turnover, Director elections, Director reputation, CEO succession, Shareholder voting

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1. Introduction

Do directors gain or lose reputation from forcing out a CEO? There are two conflicting views on this question, which reflect conflicting views on the corporate governance signal transmitted by a forced CEO turnover. On the one hand, firing a poorly performing CEO may be a sign of effective monitoring by the board and thus indicate a well-functioning corporate governance at the firm. This view represents the predominant position taken in the extant empirical finance literature.[1] Under this view, directors who force out a CEO are expected to gain reputation. On the other hand, the need to fire the CEO may indicate a governance failure (e.g., Jensen, 1993; Marcel et al., 2017). Indeed, a better board may have replaced the CEO before negative performance consequences became observable or by ensuring a less disruptive transition to a new CEO. This view is supported by the case-based study of Mace (1971) as well as contemporaneous anecdotal evidence.[2] This view is also consistent with theoretical work.[3] Under this alternative view, directors involved in a forced CEO turnover are expected to lose reputation. In this paper, we answer the question whether directors gain or lose reputation from forcing out a CEO. This enables us to shed light on the corporate governance signal transmitted by a forced CEO turnover.

A test of the reputational effect of forced CEO turnovers on involved directors is empirically challenging. First, turnover decisions are endogenous and often related to company performance (Fee et al., 2013). Second, widely used measures of director reputation, such as the number of board memberships, are subject to endogenous selection by the directors (Ertimur et al., 2012; Levit & Malenko, 2016). Therefore, a study of the reputational effect of forced CEO turnovers on directors requires a setting that satisfies two conditions. First, the setting must facilitate the isolation of the impact of a forced CEO turnover on director reputation from company-level factors that triggered the turnover decision and may negatively reflect on directors' reputation.

[1] See, for example, Weisbach (1988), Farrell & Whidbee (2000), Huson et al. (2001), Faleye (2007), Guo & Masulis (2015), Kempf et al. (2017), Dasgupta et al. (2018), and Jenter & Lewellen (2021).

[2] A prominent example in our sample is the forced CEO turnover of Ron Johnson at JCPenny, announced on April 8, 2013, which resulted in hefty criticism of the board and calls to resign entirely because of poor handling and timing of the turnover. Reuters reported that "Hours after the switch was announced on Monday, there was at least one call for the entire board to resign, while others suggested shareholders might vote out current directors at the company's next annual meeting. 'It was the wrong thing for the board to do to get rid of Johnson here. With the board firing Johnson now, at this stage in the game, they should tender their own resignation as well,' said Brian McGough, managing director and head of the retail group at research firm Hedgeye Risk Management."

[3] In Dow (2013)'s model, directors choose not to fire a bad CEO because they do not want to reveal that they made a mistake in hiring her in the first place. Aghamolla & Hashimoto (2021) show that while aggressive boards facilitate truthful communication between the CEO and the board, they tend to dismiss talented managers imposing costs on shareholders. The model of Adams & Ferreira (2007) suggests that it may be beneficial for shareholders to elect a board that is friendly towards the CEO and does not monitor her too closely and/or impose a significant threat of replacement. Hence, a forced CEO turnover may signal an aggressive board to shareholders, one that may eventually turn out to be detrimental to shareholder value.

In an ideal experimental setting, forced CEO turnovers would be randomly assigned to some directors but not to others. The change in the reputation measure between directors who force out a CEO and those who do not would then provide a causal estimate of the reputational effect. In this paper, we approximate such a randomized experimental setting by studying outside directors with multiple directorships who force out a CEO at one firm (which we refer to as "turnover firm"). We then compare the change in the reputation measure from before the CEO turnover to after the CEO turnover at other firms on whose board these directors sit (the "interlocked firms") to changes in the reputation measure of directors that are not interlocked to a forced CEO turnover. Our setting's main advantage is that these interlocked firms, and the directors sitting on their boards, are unaffected by characteristics of the turnover firm, including factors that led to the forced CEO turnover. Thus, we can utilize CEO turnovers as exogenous shocks on interlocked directors and use directors not interlocked to a CEO turnover as control directors. This allows us to measure reputational effects net of other influences from the interlocked firm.

Second, a study of the reputational effects of forced CEO turnovers on involved directors requires a measure of reputation that the directors cannot directly influence. Our primary measure for the reputational effect is the percentage of withheld votes in director elections (defined as the sum of votes withheld and votes against, divided by the total number of votes cast). Unlike the number of directorships used in related studies (e.g., Farrell & Whidbee, 2000; Ellis et al., 2021), the percentage of withheld votes is not subject to endogenous selection but is the outcome of shareholders' and not directors' decisions. By using director vote outcomes as a measure of director reputation, we rely on a growing stream of literature that shows that investors actively use withheld votes to evaluate directors' actions and that directors respond to changes in withheld votes (e.g., Ertimur et al., 2012; Brochet & Srinivasan, 2014; Aggarwal et al., 2019; Erel et al., 2021). For instance, Aggarwal et al. (2019) show that, even in uncontested director elections, increased withheld votes lead to higher director turnover, committee demotions, and reduced opportunities in the director labor market. Hence, withheld votes constitute a direct measure of shareholder satisfaction regarding individual directors that is not subject to endogenous selection by the directors.

To implement our identification strategy, we estimate generalized difference-in-differences regressions on a sample of turnover-interlocked director re-elections and a control sample of non-turnover-interlocked director re-elections. Specifically, we regress the change in the share of withheld votes in director re-elections on a treatment dummy indicating whether a director was involved in a forced CEO turnover at another firm after the last election and firm-level and director-level control variables. We first-difference outcome and control variables at the director-firm level. This ensures that time-invariant director characteristics, such as talent, and firm characteristics, such as corporate culture, do not influence our estimates. Moreover, we

include industry-year fixed effects. These fixed effects control for time trends, industry-specific trends, and unobserved time-varying industry shocks in withheld votes, ensuring that we compare changes in vote outcomes between turnover-interlocked and non-turnover-interlocked directors within the same industry and year. In further tests, we augment our baseline regression with either firm or director fixed effects. These fixed effects additionally remove time-invariant firm-specific and director-specific effects from the director re-election outcomes, allowing us to compare turnover-interlocked directors with other non-turnover-interlocked directors at the same firm or with the same director absent a CEO turnover.

Our results show that directors involved in a forced CEO turnover experience a significant increase in withheld votes at their next re-election at interlocked firms compared to directors not interlocked to a forced CEO turnover. In economic terms, the results are sizeable: Turnover-interlocked outside directors experience a 1.20 percentage point increase in withheld votes (a 19.6% increase over the sample mean) at their next board re-election date compared to non-interlocked outside directors in the same industry and year. Director- and company-level control variables and different sets of fixed effects leave this estimate largely unchanged, suggesting that controlling for various time-variant and time-invariant characteristics does not affect our findings. Hence, forced CEO turnovers do not appear to systematically coincide with other changes in director and firm characteristics, providing strong support for the conjecture that forced CEO turnovers represent exogenous shocks on interlocked directors' vote outcomes. Overall, these results imply that involvement in a forced CEO turnover leads to a reputational loss, which challenges the widely accepted view that forcing out a CEO is a sign of well-functioning corporate governance at the board level.

When looking at the cross-sectional variation across different turnover and director types, we find that the adverse reputational effects are confined to CEO departures without a full succession in place, performance-induced forced turnovers, and turnovers that occur during the most productive tenure range of a CEO. Additionally, our results show that the negative reputational effects mainly affect directors favorably inclined to the departing CEO and directors with a committee-based monitoring role towards the CEO. Hence, our cross-sectional results suggest that CEO turnovers with a higher potential to disrupt a firm's management and turnovers in which involved directors failed to perform their duty to monitor and, if necessary, fire CEOs in a timely and value-preserving manner are associated with significant reputational losses. These findings imply that monitoring failures are responsible for the reputational losses suffered by directors involved in forced CEO turnovers. In fact, we are unable to identify turnover or director sub-samples for which we observe reputational gains from an involvement in a forced CEO turnover. In summary, these cross-sectional results lend strong support to our conjecture that CEO turnovers are often the result of a governance failure rather than the realization of a well-functioning governance system.

We also provide evidence on the channels through which directors involved in a forced CEO turnover are penalized. Specifically, we show that the negative vote effect is concentrated in director re-elections in which institutional investors hold above-average ownership stakes in both the turnover and the interlocked firms. Hence, well-informed institutional investors, who monitor both the turnover and interlocked firms, are responsible for penalizing the directors.

We conduct several tests to assess the internal validity of our main result. The identifying assumption central to a causal interpretation of difference-in-differences estimates is that treated and control samples follow parallel trends. We show that there is no significant difference in the change in withheld votes between turnover-interlocked and non-turnover-interlocked directors before the forced CEO turnovers, confirming that the parallel trends assumption holds. We also show that the timing of the changes in withheld votes coincides with that of the turnovers and that there is no subsequent reversion, implying that the reputational loss is indeed driven by the turnover and is persistent.

To ensure that directors are indeed penalized for the act of firing the CEO, we run two placebo tests. First, we replicate our analysis for unforced CEO turnovers. We find that directors interlocked to unforced CEO turnovers experience an economically much smaller increase in withheld votes that disappears altogether in tighter fixed effects specifications, suggesting that involved directors are penalized for the act of firing the CEO but not for a CEO turnover more generally. Second, we analyze whether directors that served under the outgoing CEO, but leave the board of the turnover firm (shortly) before the turnover, experience an increase in withheld votes. We find that only directors directly involved in a forced CEO turnover experience a decline in vote support, while directors leaving the board before a forced CEO turnover do not. Hence, our evidence suggests that directors are not penalized for the factors that lead to the turnover, but for the act of firing the CEO.

A potential concern with the interpretation of our results is an omitted variable bias. Specifically, firms that experience poor stock price performance are more likely to force out their CEO (e.g., Dasgupta et al., 2018; Fee et al., 2018). As poor firm performance can also be linked to bad monitoring (e.g., Klein, 1998; Brick & Chidambaran, 2010), investors might simply penalize directors for poor firm performance, which resulted in the forced CEO turnover, rather than for the CEO turnover. To determine whether forced CEO turnovers or poor firm performance drive the observed reputational loss, we conduct a propensity score matching analysis. In this analysis, we match CEO turnover firms to non-turnover firms with the same propensity to force out a CEO, based on various company performance metrics and other firm, CEO, governance, and industry characteristics. Hence, treatment and control firms only differ in their decision to dismiss the CEO. Our results show that turnover-interlocked directors receive significantly more withheld votes than directors interlocked to matched firms without a forced CEO turnover but

with otherwise similar characteristics. The economic magnitude of this negative vote effect is virtually identical to our baseline estimates. This suggests that the performance of the turnover firms does not constitute an omitted variable that drives our results.

A related concern that may remain is that there is an unknown – and thus unobservable – reason why, in two firms with equally bad performance, one chooses to fire its CEO and the other does not. This unknown reason, in turn, might affect investors' inference about director ability. For instance, if management failure drives the bad performance in one firm, this failure may cause both CEO turnover and a negative updating about director ability. In contrast, if bad performance is simply due to bad luck, this may neither result in a CEO turnover nor convey any signal about directors' ability. To address this concern, we extend the set of covariates by a newspaper-based sentiment index when computing propensity scores. As mistakes of the leadership team can be expected to trigger more negative newspaper coverage than bad luck does, accounting for news sentiment can be expected to result in a matched sample that is more balanced in terms of managers' involvement in negative events that eventually result in negative performance. Our results show that differences in (negative) newspaper sentiment, and thus an omitted variable related to management failures that eventually lead to the turnover decision, are unlikely to drive our results.

An alternative interpretation of our results is director distraction. Forced CEO turnovers may demand significant time and effort from directors involved in the turnover process. Turnover-interlocked directors may thus divert their attention away from the interlocked firms (e.g., Masulis & Zhang, 2019; Stein & Zhao, 2019), resulting in a negative assessment by shareholders. We conduct a number of tests to rule out this alternative explanation. First, we make use of sudden deaths of CEOs. Sudden CEO deaths represent shocks to the time demand of directors similar to forced CEO departures. However, sudden CEO deaths are outside of directors' control and thus not expected to affect the directors' reputation. If distraction drives our results, sudden CEO deaths are expected to trigger an increase in withheld votes at interlocked firms similar to that of forced CEO departures. Consistent with our reputation-based explanation but inconsistent with a distraction-based explanation, we find that sudden CEO deaths do not affect directors' re-election results at interlocked firms. Second, we look at directors who share committee memberships with the turnover-interlocked directors. If CEO turnovers distract directors from performing their duties at the interlocked firms, we would expect this distraction to extend to members of the same committees within these boards and thus committee peers of turnover-interlocked directors to suffer a significant increase in withheld votes as well. We do not find that committee peers of directors interlocked to forced CEO turnovers experience a significant change in withheld votes. In a third test of the distraction hypothesis, we find no evidence of a decline in board meeting attendance rates of directors interlocked to forced CEO turnovers. Hence, the results of these analyses are consistent with our reputation-based

explanation but inconsistent with a distraction-based explanation.

In the final part of the paper, we follow prior research (Farrell & Whidbee, 2000; Ellis et al., 2021) and study the effect of forced CEO turnovers on future directorships. We acknowledge that this analysis may be subject to endogeneity problems. Directors may *choose* to leave some board seats voluntarily, for instance, due to reputational concerns connected to the past performance of the turnover firm, increased busyness as a result of the CEO succession, or disagreements over the turnover (Farrell & Whidbee, 2000; Ertimur et al., 2012; Levit & Malenko, 2016; Fahlenbrach et al., 2017; Masulis & Zhang, 2019; Ellis et al., 2021). Still, if involvement in a forced CEO turnover is associated with a reputational loss for outside directors, turnover-interlocked directors may lose outside directorships, or at least not gain further outside directorships. In addition to changes to the number of directorships held, the board seats that directors later obtain may be at smaller firms. To analyze this empirically, we track our sample directors and their board seats over the years following the CEO turnovers. As expected, results of these analyses are less conclusive than those resulting from the study of withheld votes at interlocked firms. Specifically, we find that outside directors involved in a forced CEO turnover on average do not lose board seats five years after the turnover, with initially lost directorships at the turnover firm offset by newly acquired board seats in the subsequent four years. However, lost board seats of directors previously involved in a forced CEO turnover are replaced by new board seats at smaller firms. These results support our conjecture that directors suffer a reputational loss from involvement in a forced CEO turnover as board seats at smaller companies are known to be associated with smaller compensation packages (Ryan & Wiggins, 2004), less power and prestige (Shivdasani, 1993; Adams & Ferreira, 2008; Masulis & Mobbs, 2014), and reduced networking opportunities (Yermack, 2004; Fich, 2005).[4]

Our paper contributes to three strands of research. First, our paper contributes to the literature on forced CEO turnovers. Many studies resort to using forced CEO turnovers as an outcome variable when analyzing the benefits of good corporate governance (e.g., Weisbach, 1988; Farrell & Whidbee, 2000; Kempf et al., 2017; Dasgupta et al., 2018). This choice is supported by the empirical observation that stock prices typically react positively to forced CEO turnover announcements (e.g., Denis & Denis, 1995; Borokhovich et al., 1996; Huson et al., 2001, 2004). However, short-term event studies around the turnover announcement may not capture shareholders' assessment of the boards' performance in the monitoring and firing of a CEO as CEO turnovers often follow periods of poor company performance. While the market may greet the eventual decision to fire a poorly performing CEO, shareholders' assessment

[4]In further tests, we show that the loss of the directorship at the turnover firm, which may be associated with a decline in influence, network connections, and prestige, does not drive the documented increase in withheld votes. Hence, these results support a reputation-based explanation for the increase in withheld votes.

of the board's willingness and ability to monitor and replace the CEO may depend on how much value was destroyed before the turnover decision was made. We add to this literature by measuring investors' assessment of forced CEO turnovers through directors' vote outcomes at directorships held at other firms. Our setting allows us to separate idiosyncratic turnover firm characteristics as well as specific aspects of these turnovers from shareholders' assessment of forced CEO turnover decisions. Our results show that forced CEO turnovers are associated with a significant reputational loss for involved directors. This finding contradicts the general presumption in the extant empirical finance literature that forced CEO turnovers are a credible signal of the boards' monitoring ability and indicate a well-functioning corporate governance. Instead, our findings suggest that forced CEO turnovers are often perceived as a signal of poor monitoring and, thus, may rather indicate a governance failure at the board level that becomes visible to the public upon the announcement of a forced turnover.

Second, our paper adds to the literature on director incentives and reputation. This literature rests on the assumption that directors are incentivized to signal their quality as monitors of management to shareholders to build a reputation in the market for corporate directors (e.g., Fama, 1980; Fama & Jensen, 1983). Song & Thakor (2006) model how career considerations of directors affect their actions. Similarly, Levit & Malenko (2016) develop a model in which directors' reputational concerns affect both their actions and the structure of corporate boards. A key ingredient in their model is that shareholders struggle to assess *a priori* the intrinsic characteristics of a director. CEO turnovers represent one of the most critical and salient decisions that boards take (e.g., Fama & Jensen, 1983). Therefore, CEO turnovers, and their surroundings, may serve shareholders as *a posterior* ability assessment of directors' quality, as in Levit & Malenko (2016)'s model. Consistent with a well-functioning market for corporate directors that relies on the signaling of ability, our empirical results show that shareholders update their beliefs about directors' hidden qualities once new signals become available.

The existing empirical literature on the consequences of forced CEO turnovers for involved directors is limited. Farrell & Whidbee (2000) find that outside directors have a higher likelihood of leaving the turnover firm following a CEO dismissal but that those directors who remain with the turnover firm have a higher likelihood of gaining additional board seats. Ellis et al. (2021) investigate the learning experience resulting from forced CEO turnovers. They document that turnover-experienced outside directors are more likely to fire CEOs of underperforming firms and hire outside CEOs after a turnover. When analyzing director labor market outcomes following a turnover, they find results that contradict those of Farrell & Whidbee (2000): Directors involved in a CEO dismissal are not rewarded with additional board seats. Instead, the number of other board positions following a forced CEO turnover is reduced. One potential reason for these mixed results is that these studies focus on future directorships as the outcome variable, which is a noisy proxy for director reputation and subject to endogeneity concerns as explained above.

We add to this literature by establishing a link between forced CEO turnovers and shareholder satisfaction as measured through director re-election outcomes at interlocked firms, an arguably cleaner and more granular measure of director reputation. Moreover, our results complement those in Ellis et al. (2021). They find that directors learn from forced CEO turnovers but seem not to be rewarded for this learning experience. We show that forced turnovers are associated with a significant reputation loss as involved directors receive more withheld votes in subsequent re-elections *and* are not rewarded with additional board seats or board seats at larger firms. Taken together, our results suggest that the reputational loss that we document dominates the learning effect found in Ellis et al. (2021).

Finally, our paper relates to the literature studying how board interlocks affect firm policies. Prior research shows that corporate governance practices can propagate through interlocked directorships (Bouwman, 2011). Zhang (2021) shows that a proxy contest experienced by a director at one firm induces policy changes at interlocked firms. This literature also shows that board interlocks may propagate less desirable practices, such as option backdating (Bizjak et al., 2009), earnings management (Chiu et al., 2013), tax avoidance (Brown & Drake, 2014), and risk-taking following bankruptcies (Gopalan et al., 2021). We extend this literature by showing that actions taken by directors at one firm affect shareholders' assessment of these directors at interlocked companies.[5] This is important because it enables us to provide direct evidence on the disciplining effect of the director labor market, as suggested by Fama (1980).

2. Sample and data

2.1. Sample selection

To compile our CEO turnover sample, we start by identifying all CEO departures from S&P 1500 firms between January 2003 and December 2017 in BoardEx. We then conduct extensive news searches in Factiva to identify the exact departure announcement date, the name of the replacement, whether the replacement was announced jointly with the departure, and the respective CEO departure circumstances. We drop departures where the news coverage indicates that a CEO departure results from the firm being acquired, the firm acquiring another company, or the firm selling or spinning off parts of its business because such events often indicate a strategic realignment of the firm. We also drop CEO departments where the news coverage indicates that they result from proxy contests, government interventions, and other types of

[5] In a contemporaneous paper, Johnson et al. (2021) show that directors involved in the adoption of a poison pill experience decreased vote margins and an increase in the probability of losing a board seat at the pill-adopting firm. In some of their tests, they expand their analyses to interlocked firms and show that directors experience a decrease in vote margins across all their directorships.

active monitoring by parties other than the board of directors. Additionally, we remove all CEO departures for which we cannot find sufficient board meeting data in BoardEx and ISS. This leaves us with a sample of 1,773 CEO departures involving 1,739 CEOs at 1,266 turnover firms.

We follow previous literature, in particular, Parrino (1997), Parrino et al. (2003), Peters & Wagner (2014), and Jenter & Kanaan (2015) in classifying CEO turnovers as either "forced" or "unforced". We classify a CEO turnover as forced if newspaper articles indicate that the CEO is fired, is forced out of her position, or departs due to unspecified policy differences. Parrino (1997) argues that CEOs departing below the age of 60 should be treated with special care. He classifies such turnovers as forced if i) the newspaper articles do not indicate that the CEO left due to poor health or acceptance of another position (elsewhere or within the firm) or ii) the articles report that the CEO is retiring but firms do not announce the retirement at least six months before the succession. Our classification procedure follows that of Parrino (1997) but, in an attempt to increase the precision of the forced turnover classification, adds one additional criterion dealing with retained positions of the outgoing CEO: We classify a CEO turnover as unforced if the CEO does not leave the firm within one month after the announced departure date, which includes the termination of a board membership, but does not include a consulting position at the turnover firm. The reason is that, after applying Parrino (1997)'s original algorithm, we discover that CEOs who are forced out of their role remain in an executive or board position at the turnover firm for an average of 1.49 years. Such a long lead-time between the departure announcement and the effective departure date suggests that the departure is not forced but consensual. The relatively short cut-off of one month aims at ensuring that our algorithm captures forced CEO turnovers only, and thus keeps the number of false positives low.[6] This procedure results in 283 (16%) CEO turnovers being classified as forced.

Following Jenter & Lewellen (2021), we also classify the forced turnovers into performance-induced and non-performance-induced turnovers as reputational effects may differ across these two categories.[7] We categorize 238 forced CEO turnovers as performance-induced and 38 forced CEO turnovers as not performance-induced. Seven turnovers cannot be classified as some of the data required for the classification are missing.

Additionally, we categorize the forced CEO turnovers based on the turnover's timing within a CEO's tenure. In a seminal paper, Hambrick & Fukutomi (1991) use a descriptive model to predict an inverted U-shaped relation between CEO tenure and firm value. They argue that in

[6]This additional criterion changes 128 CEO turnovers, classified as "forced" using Parrino (1997)'s algorithm, to "unforced".

[7]Following Jenter & Lewellen (2021), we classify a forced CEO turnover as performance-induced if the implied probability from Jenter & Lewellen (2021)'s two-probit model is above 50%. The results of the two-probit estimation are reported in the Internet Appendix in Table IA.2.

the first years in office CEOs positively contribute to firm performance as they increase their task knowledge by learning about their role within the firm and its environment. In later years, CEOs become more powerful and, as a consequence, more challenging to replace. They also rely on fewer sources to gather information, become less willing or able to learn, and are less engaged, resulting in declining firm performance. Empirical research supports these predictions and shows that the tenure-value relationship peaks after about 13 years (e.g., Brochet et al., 2021). Hence, turnovers taking place between about three and 13 years of tenure may be perceived most negatively by the market. In our sample, 90 forced turnovers occur during the first three years of a CEO's tenure (the "honeymoon period" according to Hambrick & Fukutomi (1991)), 184 between three and 13 years of a CEO's tenure (the "harvest stage"), and 9 after a CEO was at the helm of the company for more than 13 years (the "decline stage").

Next, we identify all outside directors who serve on the boards of the 283 forced CEO turnover firms at the departure announcement date using BoardEx data.[8] This gives us a sample of 2,183 outside directorships of 1,998 directors involved in a CEO turnover. We classify a director as interlocked if she serves as an outside director on the board of another firm besides the turnover firm at the forced CEO turnover announcement date. We identify 957 individual outside directors who hold at least one additional interlocked outside directorship. These directors are interlocked to 265 forced CEO turnovers.

Our main outcome variable is the change in the share of withheld votes in director re-elections. Withheld votes are defined as the sum of votes withheld and votes against, divided by the total number of votes cast (e.g., Aggarwal et al., 2019).[9] We collect data on director re-elections from the ISS Voting Analytics database, which encompass shareholder votes of Russell 3000 firms from January 2003 onward. Our sample ends in December 2017. We only consider regular director elections and exclude, for instance, director elections at special meetings and contested director elections. Regular director elections make up for 96.33% of director elections in the ISS database. We then use fuzzy string matching to match the director names provided in ISS Voting Analytics to BoardEx. This procedure matches 40,443 individual outside directors at 6,640 firms in ISS to BoardEx, resulting in 199,667 director elections. To compute the change in withheld votes from one shareholder meeting to the next, we require at least two consecutive election

[8] We exclude inside (or executive) directors at both the turnover firms and our sample firms for two reasons. First, inside directors are primarily involved in daily business decisions and may thus be punished for the performance that leads to the turnover, while outside directors are responsible and punished for monitoring and, if necessary, firing the CEO (e.g., Fama, 1980; Fama & Jensen, 1983). Second, inside directors' vote results may depend on the perceived performance in their executive roles rather than in their role as corporate directors.

[9] In unreported robustness tests, we alternatively employ the vote margin, defined as the percent of votes for a director minus the percent against, minus the percent abstaining, and find very similar results.

observations per director-firm pair, yielding 140,978 director re-elections.[10] We drop director re-elections for which we cannot retrieve sufficient stock price data from CRSP, accounting data from Compustat, institutional ownership data from Thomson Reuters, and board data from BoardEx and ISS. We also remove re-elections at financial or utility firms (SIC codes 6000-6999 and 4900-4999, respectively) because they operate in regulated industries. Finally, we drop re-elections at the turnover firms and re-elections of the departing CEOs at other firms.

The resulting sample includes 88,406 director re-elections of 18,693 individual outside directors at 3,269 firms. 607 director re-elections concern directors interlocked to 206 forced CEO turnovers, while the remaining 87,799 director re-elections concern non-turnover-interlocked directors.

2.2. Descriptive statistics

Table 1 reports summary statistics on the 206 forced CEO turnovers and 607 interlocked directors at the turnover firms. Panel A reports statistics on the turnovers. In 45% of turnover announcements (or 45% of interlocked directorships), the turnover firms announce the appointment of a full replacement CEO jointly with the departure. In 88% of forced CEO turnovers (or 90% of interlocked directorships), we define the turnover as performance-induced using the classification of Jenter & Lewellen (2021). 32% of departures (or 31% of interlocked directorships) occur during the first three years of a CEO's tenure, 66% (or 69% of interlocked directorships) during years three to 13 of a CEO's tenure, and 2% (or 1% of interlocked directorships) after more than 13 years of a CEO's tenure.

Panel B reports descriptive statistics on the 607 interlocked directors in the turnover firms. About 50% of turnover-interlocked directors in our sample joined the turnover firm when the CEO was already in office. They are often referred to as "co-opted" directors (Coles et al., 2014). Finally, at the departure announcement, 80% of the interlocked directors were members of either the nominating or the compensation committee, making them more responsible for monitoring the CEO (e.g., Chhaochharia & Grinstein, 2009; Guo & Masulis, 2015).

Table 2 reports summary statistics on turnover-interlocked directors and the interlocked firms. This sample constitutes the treatment group in our analysis. Panel A reports characteristics of the interlocked directors. On average, directors are 64 years old, are female in 17% of all cases, and hold 3.1 additional board seats beside the one at the interlocked firm but including the one at the turnover firm. Panel B reports interlocked director characteristics at the interlocked

[10] For roughly one-third of our director-firm pairs, re-elections take place at firms with a staggered board. We keep these observations in our sample but remove director re-elections that are further apart than five years and closer than one quarter from the previous election. Unreported results show that our baseline findings are not sensitive to dropping re-elections of directors at firms with staggered boards.

firms. On average, turnover-interlocked directors receive 7.1% withheld votes. ISS recommends withholding votes for 7.4% of the interlocked directors. Panel C reports summary statistics on interlocked firms. On average, they have 14.7 billion total assets, generate around 14% operating return, and institutional investors own 77% of their shares.

Table 3 replicates Table 2 for the non-turnover-interlocked directors and the firms on whose board they serve. This sample constitutes the control group in our analysis. Overall, turnover-interlocked directors are slightly older and hold more outside board seats than non-turnover-interlocked directors. Interlocked firms also tend to be larger, more profitable, and to have a larger share of institutional ownership than the control firms. Most of the differences across the two samples are statistically significant, as indicated by the asterisks in Table 3. Some of these differences are the consequence of our sample construction. For instance, to obtain treatment status directors need to hold at least two board seats at the CEO departure date, resulting in a significantly higher number of board seats compared to the control directors.[11] The significant difference in the percentage of withheld votes, 7.1% in the treated versus 6.1% in the control sample, provides *prima facie* evidence of a potential negative reputation effect resulting from a board interlock to a forced CEO turnover.

3. The effect of forced CEO turnovers on interlocked directors' vote shares

3.1. Univariate results

We first test for the reputational effect of forced CEO turnovers univariately. To this end, we compare the change in withheld votes in re-elections of directors interlocked to a forced CEO turnover with the change in withheld votes of directors not interlocked to a forced CEO turnover. We do so by regressing the change of withheld votes on a treatment indicator, that is, a dummy variable that equals one if a director forces out a CEO at another firm since the previous election. We estimate this regression repeatedly for re-elections three years before the forced CEO turnover to three years after the forced CEO turnover using up to three leads or lags of the dummy indicating an interlock to a CEO turnover. The coefficient estimates for the treatment dummy obtained in these regressions are displayed in Figure 1. Absent a forced CEO turnover, differences in changes in withheld votes between turnover-interlocked and non-turnover-interlocked directors are small, ranging from -0.60 to +0.28 percentage points, and are statistically insignificant (light grey bars). However, following a forced CEO turnover,

[11] As we discuss in Section 3.2, such a selection bias is unlikely to drive our results because we first-difference at the director-firm level and include the change in the number of outside board seats as a covariate in all our regressions. In Section 4, we also rule out the possibility that selection rather than treatment drives our results by using a propensity score matched control sample and by requiring control directors to have at least two board seats as well.

involved directors experience a significant increase in withheld votes of 1.40 percentage points at interlocked firms relative to non-turnover-interlocked directors (dark grey bar). This increase in withheld votes represents a 22.1% increase over the sample mean. Hence, these univariate results suggest that directors suffer a substantial reputational loss from a forced CEO turnover.

3.2. Multivariate results

Results from the previous section suggest that forced CEO turnovers are associated with negative reputational effects, as proxied by the change in votes withheld. However, these results could be confounded by coinciding changes in a director's characteristics, such as a change in the number of outside board seats, or firm-level changes at the interlocked firms, such as changes in performance. To control for such observable variation in director-level and firm-level attributes, we estimate the following difference-in-differences regression:

$$\Delta_{ij} v_{ijt} = \beta T_{ijt,t-1} + \delta \Delta_{ij} X_{it} + \theta \Delta_{ij} Z_{jt} + \alpha_{st} + \varepsilon_{ijt}, \tag{1}$$

where i, j, s, and t index director, firm, industry, and years, respectively. Δ_{ij} is the first-difference operator between two elections of director i at firm j.[12] v_{ijt} is votes withheld of director i at firm j in year t, in percent. $T_{ijt,t-1}$ is the treatment indicator, that is, a dummy variable equal to one if director i is interlocked to a forced CEO turnover between his election at firm j in year t and his previous election at firm j. X_{it} and Z_{jt} are time-varying director-level and firm-level control variables. α_{st} are interacted industry-year fixed effects. ε_{ijt} is the error term. Standard errors are clustered at the Fama-French 48 industry level.[13]

By first-differencing withheld votes, we remove unobserved heterogeneity at the director-firm-pair level. This specification is similar to a specification in levels with director-firm fixed effects but, unlike a specification in levels, it can accommodate repeated treatments (the possibility that a director is involved in multiple forced CEO turnovers over our sample period). Interacted industry-year fixed effects remove unobserved industry effects, time trends, and industry shocks. These steps ensure that the model captures the structural differences between the treatment and control samples discussed in section 2.2. As time-varying director controls, X_{it}, we include the ISS vote recommendation indicator variable and the number of additional outside board seats. Director characteristics such as age, tenure, and gender are removed due to their time-invariant

[12] For most director-firm pairs, t and $t-1$ refer to the year t and $t-1$, respectively. However, in roughly one-third of our sample, firms do not re-elect all directors each year, but in a staggered manner. Therefore, the time-distance in the first-differencing varies across individual firms and directors based on the specific periodicity of the director re-elections. Hence, for directors of firms with a staggered board, t and $t-1$ refer to the current (t) and the last observable board meeting ($t-1$) for which we observe election outcomes. Unreported results show that our baseline findings are not sensitive to dropping re-elections of directors at firms with staggered boards.

[13] Our results remain similar when we cluster the standard errors at the firm or the director level.

nature in a first-differences setting. The set of time-varying control variables at the firm level, Z_{jt}, is based on prior corporate governance and voting literature (e.g., Cai et al., 2009; Fischer et al., 2009; Matvos & Ostrovsky, 2010; Aggarwal et al., 2019; Johnson et al., 2021), and includes board size, the fraction of outside directors on the board, the fraction of busy outside directors, the institutional ownership, total assets, Tobin's Q, ROA, and past buy-and-hold returns.

The results from the difference-in-differences regression in Equation (1) are reported in Column 1 of Table 4. They show that, following a forced CEO turnover, turnover-interlocked directors on average receive 1.20 percentage points more withheld votes than non-turnover-interlocked directors at firms in the same industry and year. The economic magnitude of this effect is sizeable. The sample mean of non-turnover-interlocked directors is 6.1% (see Table 3). Thus, our estimate implies a 19.6% increase in withheld votes over the sample mean.

Coefficient estimates on the control variables reported in Column 1 are generally consistent with prior research (e.g., Cai et al., 2009). For example, a change of ISS's recommendation from elect to withhold or vote against increases withheld votes by over 18 percentage points. Similarly, directors who gain board memberships subsequently receive more withheld votes. On the other hand, improved firm performance or increases in growth opportunities reduce withheld votes.

In Column 2, we add firm fixed effects to the model. These fixed effects additionally absorb unobserved time-invariant firm-specific heterogeneity. In this specification, we effectively compare turnover-interlocked directors to non-turnover-interlocked directors at the same firm. The results show that turnover-interlocked directors experience a significant increase of 0.88 percentage points withheld votes over non-turnover-interlocked directors at the same firm, or a 14.3% increase over the sample mean. In Column 3, we augment our baseline regression with director fixed effects. These fixed effects remove unobserved time-invariant director characteristics and produce a within-person comparison of the turnover-interlocked directors to themselves, but absent a forced CEO turnover at an interlocked firm. Our results again remain robust in this most conservative specification: Directors with an interlock to a forced CEO turnover face a significant increase in withheld votes of 1.03 percentage points, or a 16.8% increase versus the sample mean, compared to re-elections of the same directors absent a forced CEO turnover.[14]

In summary, these results suggest that directors suffer a reputational loss across all board mandates following a forced CEO turnover. These findings contrast with the presumption that

[14]In Table IA.3 in the Internet Appendix, we report results from replicating our baseline regressions using the change in a dummy variable set equal to one if 15% or more of the votes cast in a director's re-election are against her as dependent variable. Bach & Metzger (2015) argue that this threshold indicates a very unusual level of defiance against directors. Our results show that directors are 3.6% to 4.0% more likely to experience a loss in vote support of at least 15% following a forced CEO turnover at an interlocked firm, a probability that is 44.2% to 49.1% higher than the sample mean.

forced CEO turnovers credibly signal a board's monitoring ability and generally indicate good corporate governance. Moreover, our results survive when we control for a host of observable and unobservable firm and director characteristics, suggesting that forced CEO turnovers do not systematically coincide with other changes in director and firm characteristics. Thus, the findings support our conjecture that forced CEO turnovers represent exogenous shocks on interlocked directors' vote outcomes.

3.3. Cross-sectional results

To corroborate our result that involvement in a forced CEO turnover is associated with a reputational loss for outside directors, we next test whether such a reputational loss is particularly pronounced for turnovers linked to poor (monitoring) performance of the board of directors.

In the first test, we analyze whether the reputational loss for outside directors involved in a forced CEO turnover depends on whether a successor to the dismissed CEO is announced simultaneously with the outgoing CEO's departure. Prior evidence paints a mixed picture regarding the impact of delayed CEO succession on firm performance. Most prior studies point towards a short-term decline in firm performance following interim CEO successions (e.g., Ballinger & Marcel, 2010; Mooney et al., 2017). However, firms might benefit in the long run from taking more time to find a suitable successor, especially when the board does not have a succession plan in place (Rivolta, 2018). Nevertheless, Dalton & Dalton (2007) argue that even after unforeseeable turnovers, the lack of an heir apparent to the outgoing CEO signals the board's unpreparedness and thus conveys a negative signal about the board's quality. Hence, the directors' reputation is expected to suffer more if no full replacement is announced jointly with the CEO's departure, even though such a delay may be beneficial for the turnover firm in the long run. To test whether the reputational loss of outside directors involved in a forced CEO turnover depends on whether a full replacement is announced along with the departure, we replicate Column 1 from Table 4 and split the variable *Forced interlocked (d)* into two variables, one indicating involvement in a forced CEO departure with a full replacement announced simultaneously (*Forced w. full replacement (d)*) and one without such an announcement (*Forced w/o full replacement (d)*).[15] The results are reported in Column 1 of Table 5. We find that outside directors involved in a forced CEO turnover at an interlocked firm with either an interim succession or no succession announced experience a large increase in withheld votes of 1.5 percentage points, or an increase in 24.5 % over the unconditional mean. This coefficient is statistically significant at the 1% level. In contrast, the coefficient for directors involved in a forced turnover

[15]Note that this sample split corresponds to an interaction term between the forced CEO turnover dummy and the dummy whether a full replacement was announced contemporaneously to the turnover, omitting stand-alone turnover characteristics. Stand-alone turnover characteristics are omitted because they are only defined for turnover-interlocked directors but not for non-turnover-interlocked directors.

with a full replacement announced is almost half this size (0.84 percentage points) and not quite significant at conventional levels. These results suggest that delayed successions in forced CEO turnovers are negatively assessed by shareholders compared to other departure announcements and increase the reputational loss of outside directors involved in forced CEO turnovers.

Ertugrul & Krishnan (2011) argue that (forced) CEO turnovers should be separated into performance-induced and non-performance-induced turnovers. They conjecture that non-performance-induced turnovers are indicative of a proactive board that steps in and fires an underperforming CEO before she can cause harm to firm value, while performance-induced forced turnovers indicate a reactive board that only acted once the damage was already done. Hence, we expect the reputational damage to be worse for performance-induced CEO turnovers. To test this conjecture empirically, we follow Jenter & Lewellen (2021) in classifying CEO turnovers into performance-induced and non-performance-induced. In Column 2 of Table 5, we replicate our main specification from Column 1 of Table 4 and split the interlocked forced CEO turnover dummy into two dummy variables indicating performance-induced and non-performance-induced turnovers. For performance-induced turnovers, we find a 1.4 percentage point increase in withheld votes, or an increase of 23.0% versus the unconditional mean. In contrast, the coefficient on the non-performance-induced turnovers is negative and insignificant. These results are consistent with our conjecture that the reputational damages from forced CEO turnovers are confined to reactive dismissals, implying that reactive forced CEO turnovers are perceived as a bad signal for a board's quality by shareholders, while proactive turnovers, i.e., those that likely pre-date poor company performance, are not.

An alternative way to measure the board's reactiveness to an underperforming CEO is to consider the timing of the forced departure within a CEO's tenure. The CEO tenure literature documents an inverted U-shaped relation between CEO tenure and firm value (e.g., Hambrick & Fukutomi, 1991; Henderson et al., 2006; Brochet et al., 2021). During the first few years of a CEO's tenure ("honeymoon stage"), a newly appointed CEO gets to know the company and its board, experiments with different management and organizational styles, and eventually starts to implement changes. During this stage, the board of directors can assess whether the new CEO meets expectations, with a proactive board firing a disappointing CEO to prevent future harm (Ertugrul & Krishnan, 2011). Hence, we do not expect significant reputational damages for directors involved in forced CEO turnovers during this stage. After a successful honeymoon stage, the new CEO's organizational changes start to bear fruit, and the CEO enters the second "season" ("harvest stage"). In the case of a poorly fitting CEO, who was still retained beyond the honeymoon stage, negative performance consequences will become observable. Hence, similar to performance-induced turnovers, forced CEO turnovers in the harvest stage may reflect a reactive board. We thus expect reputational damages for directors involved in forced CEO turnovers that take place during this stage. Finally, after about 13

years of a CEO's tenure, the CEO's positive effects start to be outweighed by the adverse effects of increased power and entrenchment, even if the CEO was an excellent match to the firm initially. During this final stage, sometimes referred to as the "decline stage", the CEOs' performance contribution turns negative. Therefore, we expect no adverse reputational effects of CEO turnovers that take place during the decline stage. In Column 3 of Table 5, we test this hypothesis by splitting the interlocked forced CEO turnover dummy into three dummy variables indicating these three CEO life-cycle stages. We find that the negative reputational effect following an interlocked CEO turnover is confined to turnovers during the harvest stage. For these turnovers, we find a 1.68 percentage point increase in withheld votes or an increase of 27.3% over the unconditional mean. In contrast, we find virtually no reputational effect of forced turnovers during the honeymoon stage or the decline stage. These results suggest that forced CEO turnovers that occur during the most productive period of a CEO's tenure are perceived as a bad signal for the directors' monitoring performance. Such turnovers may reflect an inactive board that took too long to recognize and correct a CEO-firm mismatch.

If the observed increase in withheld votes associated with a forced CEO turnover is the result of the market reassessing involved directors' ability and willingness to monitor senior management, we would expect directors with committee-based responsibility for monitoring the CEO and planning her succession to suffer larger reputational losses. The board committees generally entrusted with these responsibilities are the nominating and compensation committee (e.g., Chhaochharia & Grinstein, 2009; Guo & Masulis, 2015). To test empirically whether outside directors who are members of these committees at the turnover firm suffer larger reputational losses than non-members, we split the forced interlocked turnover variable into two variables based on whether the turnover-interlocked director is a member of the nominating and/or compensation committee of the turnover firm or not. The results are reported in Column 4 of Table 5. We find a statistically significant increase in withheld votes of 1.6 percentage points for members of the nominating and compensation committees, representing an increase of 25.5% over the unconditional mean. In contrast, the coefficient on the dummy indicating no membership in any of these two committees is negative and statistically insignificant at conventional levels. This result is consistent with the conjecture that responsibility for monitoring the CEO is associated with a larger reputational loss following a forced CEO turnover.

Prior research shows that directors appointed after a CEO has resumed office (co-opted directors) are favorably inclined to this CEO and thus provide weaker monitoring (e.g., Coles et al., 2014). Hence, involvement in a forced CEO turnover may reinforce the market's assessment of co-opted directors' limited willingness to act in shareholders' best interests. As a result, we expect the reputational loss of a forced CEO turnover to be larger for co-opted directors than for non-co-opted directors. To test this conjecture empirically, we split the forced interlocked turnover dummy into two dummy variables depending on whether the director was appointed

under the dismissed CEO. The results are reported in Column 5 of Table 5. We find that the reputational loss associated with the involvement in a forced CEO turnover at an interlocked firm is larger for co-opted directors than non-co-opted directors. Both coefficients are positive, but the coefficient for the subset of co-opted directors is about twice the size of the coefficient for non-co-opted directors and only the former is statistically significant. The magnitude of the coefficient for the subset of co-opted directors implies a 1.7 percentage point increase in withheld votes, an increase of 27.2% over the unconditional sample mean. This result is consistent with the expectation that directors who are affiliated with the CEO may suffer a larger reputational loss from a forced CEO turnover than unaffiliated directors. The results in Column 5 of Table 5 show that the negative reputational effect is confined to directors who were appointed after the CEO took office and thus to directors who were not involved in the initial decision to hire the CEO. This suggests that directors are not punished for the hiring decision but for failing to monitor and eventually fire the CEO.[16]

Overall, the results in this section show that the negative reputational effects from forced CEO turnovers on interlocked directors are confined to turnovers with a high potential for disrupting a firm's management and turnovers in which directors have neglected their monitoring duty to shareholders. These results corroborate the findings from the previous sections and show that, contrary to the general presumption in prior empirical finance research, a forced CEO turnover is not a credible signal of a board's monitoring ability and thus does not *per se* indicate good corporate governance. In fact, in none of the turnover or director sub-samples considered in this section do we find reputational gains from an involvement in a forced CEO turnover, but in many we find reputational losses.

3.4. Channels

In this section, we analyze the channel through which directors involved in forced CEO turnovers are penalized. Specifically, we attempt to determine which shareholders tend to vote against directors involved in forced CEO turnovers at other firms. Penalizing directors for actions taken while serving on the board of interlocked firms requires shareholders to i) observe and evaluate directors' actions, ii) observe interlocked directorships, and iii) take action by systematically voting against directors across their board mandates. Institutional investors with significant ownership stakes in both the turnover and interlocked firms are most likely to fulfill these requirements (e.g., He et al., 2019). Institutional investors are considered to be sophisticated (e.g., Gibson et al., 2004) and to impact the governance of their portfolio firms positively,

[16]Note that existing research shows that both initial uncertainty about a CEO's fit to the company at appointment (e.g., Jovanovic, 1979; Allgood & Farrell, 2003; Ali & Zhang, 2015) as well as subsequent shocks to the CEO-firm match quality (e.g., Miller, 1991; Garrett & Pavan, 2012; Eisfeldt & Kuhnen, 2013) make it difficult to assess a CEO's quality even *ex-post*.

independent of whether they follow an active (e.g., Brav et al., 2008) or passive investment approach (e.g., Appel et al., 2016).[17] Hence, we expect that turnover-interlocked directors receive more withheld votes at the interlocked firm if there is significant common ownership by institutional investors in both the interlocked and the turnover firm.

To test this conjecture empirically, we measure institutional investors' common ownership in the turnover and the turnover-interlocked firms using the Thomson Reuters institutional holdings (13F) database. We identify all institutional investors that report holdings in the turnover firms at the last reporting date before the forced CEO turnover announcement and holdings in the interlocked firms at the last reporting date before the re-election meeting date of an interlocked director following the turnover. For each common owner, we define common ownership as the lower value of the fraction of shares outstanding held in the turnover firm and the fraction of shares held in the interlocked firm. We then sum up common ownership across investors at the director re-election level. Finally, we split the dummy variable indicating a turnover-interlock into two dummy variables, one indicating involvement in a forced CEO turnover with common ownership between the turnover and the interlocked firm above its sample mean (median) common ownership and one with common ownership below its sample mean (median).

Results from re-estimating our baseline regression with the split according to mean (median) common ownership are reported in Column 1 (2) of Table 6. We find that outside directors involved in a forced CEO turnover at an interlocked firm with above mean (median) common ownership experience an increase in withheld votes that is larger by 60% (74%) than the increase in withheld votes in response to interlocked forced CEO turnovers with below mean (median) common ownership. These results indicate that the negative reputational effect of being involved in a forced CEO turnover at an interlocked firm mostly stems from institutions with significant joint ownership in both the turnover and the interlocked firms.

These findings support the notion that informed institutional investors engaged in the turnover and the interlocked firms are responsible for penalizing directors for their involvement in the turnover. Moreover, these results suggest that the recent growth in institutional shareholdings and concentration of ownership in the portfolios of a few large asset managers, which triggered a discussion around the resulting incentives to compete (e.g., Azar et al., 2018; Lewellen & Lowry, 2021), might also have a so-far unrevealed positive consequence: Substantial cross-shareholdings

[17] BlackRock, the world's largest institutional investor based on Assets under Management, acted on various concerns by opposing 5,100 director re-elections globally between July, 2019 and June 30, 2020. Notably, BlackRock penalizes directors across all of their board mandates, for instance for holding too many board seats. For more information see BlackRock's 2021 proxy voting guidelines (`https://www.blackrock.com/corporate/literature/fact-sheet/blk-responsible-investment-guidelines-us.pdf`) and their 2021 stewardship expectations guideline (`https://www.blackrock.com/corporate/literature/publication/our-2021-stewardship-expectations.pdf`)

facilitate monitoring of directors across firms, resulting in stronger incentives to perform.

4. Internal validity

4.1. Parallel trends, treatment reversal, pseudo treatments, and selection issues

The identifying assumption central to any difference-in-differences analysis is that treated and control observations share parallel trends before the onset of treatment. Specifically, turnover-interlocked directors (treated directors) and non-turnover-interlocked directors (control directors) need to show insignificant differences in their re-election results prior to the onset of treatment. Figure 1 shows the differences in the change in withheld votes between the turnover-interlocked directors and non-turnover-interlocked directors for a symmetric window covering seven re-elections around the interlocked forced CEO turnover. For the three re-elections before an interlocked forced CEO turnover, there are no significant differences in the changes in withheld votes between turnover-interlocked and non-turnover-interlocked directors. This implies that treated and control directors do not differ in their votes before the onset of the treatment, supporting the notion that the parallel trends assumption holds.

Figure 1 further shows no evidence of a treatment reversal. The differences in the change in withheld votes between turnover-interlocked directors and non-turnover-interlocked directors are economically small and statistically insignificant across all three post-treatment years. To test more formally whether there is evidence of a treatment reversal, we rerun the baseline regression from Column 1 of Table 4 with additional lags for one and two re-election dates, respectively. Columns 1 and 2 in Table 7 report the results. In both regressions, the coefficient on the treatment indicator remains economically large and statistically significant, while the coefficients on both lagged indicator variables are small in magnitude and statistically insignificant. Hence, there is no evidence of a treatment reversal following the CEO turnover, implying that a forced CEO turnover results in a persistent increase in withheld votes of directors involved in a forced CEO turnover at another firm. This finding is consistent with investors updating their beliefs about directors' qualities once new signals become available.

Additionally, we run placebo tests where treatment is set to $t-1$ and $t-2$, respectively. Hence, we look at re-elections that take place before the forced CEO turnover. Columns 3 and 4 of Table 7 present the results of these pseudo treatment regressions. In both regressions, the coefficient on the treatment indicator is small and statistically insignificant, suggesting that the observed increase in withheld votes is due to the forced CEO turnover at the interlocked firm, as opposed to alternative factors, such as deteriorating performance of the turnover firm preceding the CEO turnover.

In an alternative placebo test, we analyze whether directors are punished for firing a CEO, as hypothesized, and not for a CEO turnover more generally. To this end, we rerun our analysis from Table 4 and augment the regression with an indicator variable equal to one if a director is involved in a CEO turnover that we do not classify as forced while serving as an outside director on another firm's board. Results are reported in Columns 5 to 7 of Table 7. Column 5 shows results from estimating our baseline specification that includes interacted year and industry fixed effects. We obtain positive and statistically significant coefficients on both the forced and unforced CEO turnover interlock dummy variables. However, the coefficient on the dummy indicating an interlock to an unforced CEO turnover is about 60% smaller than the coefficient on the dummy indicating an interlock to a forced CEO turnover. Columns 6 and 7 report results from tighter fixed effects specifications that additionally include firm and director fixed effects, respectively. In both columns, the coefficients on the unforced CEO turnover interlock dummy variables turn statistically insignificant and become economically small, while the coefficients on the forced CEO turnover interlock dummy variables remain economically sizeable and statistically significant at the 5% level. Taken together, these results suggest that directors are penalized for an active role in a CEO turnover, i.e., for firing a CEO, and not for a CEO turnover in general.

In another placebo test, we attempt to test whether the involvement in a forced turnover or a failure to monitor the CEO before the turnover drives our results. We define a pseudo treatment dummy variable that is equal to one if a director held a board seat at a turnover firm during the outgoing CEO's tenure but left the board before the forced turnover, and zero otherwise. We identify 131 such directorships in our sample. Column 8 of Table 7 presents results from re-estimating our baseline regression with the additional pseudo treatment dummy (*Left before forced turnover (d)*). The coefficient on the pseudo treatment dummy is negative and statistically insignificant. The treatment dummy, on the other hand, remains statistically significant and economically relevant. These results suggest that the forced turnover itself results in a negative reputation effect, while having served on the board during the tenure of the outgoing CEO does not result in an increase in withheld votes.[18]

Another potential concern with our analysis is a selection bias that results from our identification strategy. Specifically, obtaining treatment status requires interlocked directors to hold at least two outside directorships at the time of the turnover – one at the turnover firm and one at the interlocked firm. In contrast, directors in the control group are not required to hold multiple board seats. As a result, turnover-interlocked directors on average hold 1.4 boards seats more than non-turnover-interlocked directors (see Tables 2 and 3). This selection bias may be prob-

[18] Our results remain similar when we constrain the pseudo treatment dummy variable to those directors who left the turnover firm during the 12 months preceding the forced turnover (53 pseudo-treated directorships).

lematic because directors with board overlaps may fulfill different roles than directors without board overlaps (e.g., Geng et al., 2021). However, such a selection bias is unlikely to drive our results, for two reasons. First, by first-differencing at the director-firm level and including the change in the number of outside board seats in our regression models as a covariate, we control for effects that are directly related to changes in the number of board seats as well as unobservable director characteristics that could be correlated with the number of board seats. Second, our results hold when we add director fixed effects to our regressions, which additionally control for potential selection effects at the director level. Nevertheless, to rule out the possibility that selection rather than treatment drives our results, we exclude all non-turnover-interlocked directors who hold no additional board seats from the control sample and rerun our main analysis from Table 4. The results are reported in Table IA.4 in the Internet Appendix and are virtually identical to the main results in Table 4. Hence, a selection bias resulting from our identification strategy does not drive our results.

In summary, the analyses in this section show that there is no significant difference in the change in withheld votes between turnover-interlocked and non-turnover-interlocked directors before or after the turnover, confirming that the parallel trends assumption holds. The reputational losses are much weaker, or nonexistent, for unforced CEO turnovers, suggesting that the firing of the CEO, and not turnovers more generally, is responsible for the observed reputational loss. We also find no reputational losses for directors leaving the turnover firms before the turnover, suggesting that directors are not penalized for the factors that lead to the turnover, but for the act of firing the CEO. Moreover, the negative reputational effects are persistent, showing no reversal in subsequent years. Finally, we show that our results are unlikely to be driven by a selection bias resulting from the requirement that treated, i.e., interlocked, directors have to hold at least two outside directorships.

4.2. Omitted variables and reverse causality

A potential concern with the interpretation of our results is an omitted variables bias. Specifically, firms that experience poor stock price performance are more likely to force out a CEO (e.g., Dasgupta et al., 2018; Fee et al., 2018). As poor firm performance can also be linked to bad monitoring (e.g., Klein, 1998; Brick & Chidambaran, 2010), investors might simply penalize directors interlocked to poorly performing firms. Under this alternative interpretation, causality would not run from forced CEO turnovers to increases in withheld votes at turnover-interlocked firms. Instead, both CEO turnovers at the turnover firms and increases in withheld votes at the turnover-interlocked firms would be caused by poor firm performance of the turnover firms.

To address concerns of an omitted variables bias, we conduct a propensity score matching analysis, in which we match CEO turnover firms to non-turnover firms with the same propensity

to force out the CEO. We then compare withheld votes of directors interlocked to firms with a forced CEO turnover to withheld votes of directors interlocked to matched firms that do not replace their CEO. By doing so, we obtain balanced treatment and control samples of directors who are interlocked to firms that are similar in terms of the likelihood of firing the CEO but differ only in the effective turnover decision. This setting thus allows us to rule out that factors that lead to the turnover decision drive our finding that an involvement in a forced CEO turnover results in an increase in withheld votes.

We calculate propensity scores using the forced turnover likelihood model of Peters & Wagner (2014). To estimate this model, we construct a turnover-firm panel that comprises each turnover in our sample and, for each turnover, all potential control firms within the S&P 1500. We remove potential control firms that experience a forced CEO turnover within five years before or after the turnover. This ensures that our sample of matched control firms does not include "forced CEO turnover firms in disguise", that is, firms that have recently forced out their CEO or will soon force out the CEO. Additionally, we remove potential control firms for which we find no outside directors with interlocked directorships at other firms or no vote-share data at the interlocked firms. Our final sample for the propensity score matching consists of 183 forced CEO turnovers, 1,822 distinct potential control firms, and 160,311 potential control firm-turnover-pairs. A covariate balance test between the turnover firms and the potential control firms is presented in Panel A of Tables IA.7 in the Internet Appendix. Absent any matching, the two samples differ significantly in terms of most variables.

The results of the propensity score estimations are reported in Table IA.6 in the Internet Appendix.[19] Within each turnover stratum, we identify the control firm with the closest propensity score to the turnover firm.[20] We find nearest neighbors for 181 of the 183 turnovers in our sample. Results of covariate balance tests between the turnover and matched control firms are reported in Panel B of Table IA.7 in the Internet Appendix. The results show that the turnover sample and the matched control sample do not differ significantly in terms of any of the variables used in the analysis. Common support is illustrated in Figure IA.1 in the Internet Appendix. Panel B shows that the density distribution of the propensity scores for turnover and matched control samples align closely across the full propensity score spectrum after matching.

For each firm in our propensity score-matched sample, we identify all outside directors who also

[19] We follow the suggestion of Peters & Wagner (2014) to use a logistic regression. Peters & Wagner (2014) use a linear probability model instead because they use the forced turnover likelihood model as a first stage in a two-stage model and, to ensure consistency with the second stage, resort to a linear probability model in the first stage as well.

[20] We improve match quality by imposing a maximum caliper width, i.e., a maximum allowed distance between the turnover and control firms' propensity score (e.g., Austin, 2011a; Lechner & Strittmatter, 2019). We follow Austin (2011b), who identifies an optimal caliper width of 0.2 times the pooled standard deviation of the logit of propensity scores.

hold outside directorships at other firms in our vote-share sample, following the methodology described in Section 2. We identify 525 turnover-interlocked directorships held by 383 individual directors at 411 turnover-interlocked firms, and 586 matched non-turnover-interlocked directorships held by 418 individual directors at 453 matched non-turnover-interlocked firms.

Using this matched sample, we estimate regressions similar to our baseline regressions in Table 4. However, as this propensity score-matched sample is naturally cross-sectional, we are forced to estimate the regressions in levels and not in changes. Moreover, as the propensity score matching balances the matched sample on the turnover firms only, we include control variables for interlocked directors and the interlocked firms. Table 8, Columns 1 to 3, present the results. Column 1 reports results from a regression without fixed effects. The regression reported in Column 2 additionally includes industry and year fixed effects. In Column 3, we augment the baseline regression with turnover event fixed effects. This most restrictive specification compares withheld votes between turnover-interlocked directors and matched non-turnover-interlocked directors interlocked to firms that have a similar propensity to replace the CEO but do not force her out. Across all three columns, we find that directors interlocked to firms that forced out their CEO receive significantly more withheld votes than directors interlocked to firms with the same propensity of a forced CEO turnover but no actual CEO turnover. The magnitude of the coefficients in Columns 1 and 2 is virtually identical to the magnitude in the baseline regression in Column 1 of Table 4. The coefficient in the most restrictive specification in Column 3 indicates that interlocked directors on average receive 1.01 percentage points more withheld votes than non-turnover-interlocked directors serving on the board of firms with the same propensity to force out a CEO. This constitutes an increase in withheld votes of 16.5% over the sample mean of non-turnover-interlocked directors.

The forced CEO turnover likelihood model of Peters & Wagner (2014) ensures that we obtain balanced treatment and control samples of directors interlocked to firms that are statistically indistinguishable across a wide set of observable firm characteristics, including various metrics of company performance. However, there may still be an unknown reason why, in two firms with equally bad performance, one chooses to fire its CEO and the other does not, and this reason might affect investors' inference about director ability. For example, bad performance in one firm might be due to mistakes of the leadership team, leading to both CEO turnover and negative updating about director ability, while bad performance in the other firm might be due to bad luck, neither leading to a turnover nor conveying any signal about directors' ability. To address this concern, we construct an alternative matched control sample using the CEO turnover likelihood model of Peters & Wagner (2014) augmented with a newspaper-based sentiment index. The rationale behind this test is that mistakes of the leadership team trigger more negative newspaper coverage than "bad luck" does. Matching on news sentiment yields a control sample that is more balanced in terms of managerial involvement in negative events that

lead to negative firm performance, and eventually CEO turnover. We use Ravenpack's aggregate event sentiment (AES) in the time period preceding the turnover announcement as a proxy variable for firms' news sentiment. This is a firm-level news sentiment score that ranges from zero to 100, with values below (above) 50 indicating negative (positive) sentiment. The variable is constructed daily using 91-day rolling windows and takes into account all news sources from the Dow Jones universe (among others, Dow Jones newswires, the Wall Street Journal, Barron's, and MarketWatch). A comparison between turnover and control firms absent any matching shows that turnover firms indeed show a significantly more negative news sentiment before the turnover (see Panel A of Table IA.8), supporting the conjecture that RavenpackâĂŹs newspaper sentiment captures negative news preceding turnover events. Results in Panel B of Table IA.8 show that the propensity score matching results in treatment and control samples that are balanced in terms of all observable characteristics, including news sentiment. Propensity scores also align closely across the full propensity score spectrum after matching (see Figure IA.2). Using this sample based on an augmented matching approach, we reestimate the regressions reported in Columns 1 to 3 of Table 8. Results are reported in Columns 4 to 6 of Table 8 and are virtually identical to those obtained when constructing the control sample with Peters & Wagner (2014)'s original CEO turnover likelihood model. Hence, differences in (negative) newspaper sentiment, and thus an omitted variable related to management failures that lead to a CEO turnover, are unlikely to drive the observed increase in withheld votes.[21]

Another potential concern is reverse causality. Fos et al. (2018) find that the CEO turnover-performance sensitivity increases before director re-elections. Directors under threat of increased withheld votes might feel inclined to fire their CEO to signal a well-functioning corporate governance. Under such a scenario, the increase in withheld votes at interlocked firms might reflect a general increase in withheld votes for the turnover-interlocked directors across all their board seats that started already before the directors decided to force out their CEO. Forced CEO turnovers might then be the result of increases in withheld votes, rather than the other way around. To address this concern and ensure that causality runs from forced CEO turnovers to increases in withheld votes, we again replicate our propensity score matching analysis and extend Peters & Wagner (2014)'s model by a variable that measures the mean change in withheld votes across all director re-elections at the last meeting before the matching date versus the

[21]Note that this test is complemented by one of our pseudo-treatment tests, described in Section 4.1 and reported in Column 8 of Table 7. In this test, both treated and pseudo-treated directors serve on the board of a firm that fires the CEO, but the latter leave (shortly) before the CEO turnover is made public. Hence, both treated and pseudo-treated directors are expected to be characterized by the same unobservable variable that determines why the CEOs are fired. It follows that both should experience similar increases in withheld votes if some unknown reason drives both the decision to fire the CEOs and investors' inference about director ability. However, we find that increases in withheld votes are confined to directors still serving on the board at announcement of the turnover. This suggests that our results are driven by directors experiencing a CEO turnover and not some unknown – and thus unobservable – reason why firms choose to fire their CEOs.

previous meeting. This matched sample is expected to be well-balanced in terms of fading vote support for directors which may eventually pressure them to fire the CEO and thus would address the reverse causality issue raised above. Results in Table IA.9 indicate that turnover and matched control firms differ in terms of most observable characteristics before matching (Panel A), but are statistically indistinguishable after matching (Panel B). Most importantly, the matched sample is well balanced in terms of pressure at the turnover firm that stems from director re-elections before the forced CEO turnover date and is thus expected to capture a potential impact of director re-election outcomes on forced CEO turnovers. Propensity scores align closely across the full propensity score spectrum after matching (see Figure IA.3). Results from reestimating Columns 1 to 3 of Table 8 using the matched sample that is based on this extended propensity score model are reported in Columns 7 to 9 of Table 8. Again, results remain virtually unchanged: Across all three columns, that include different fixed effects, we find that directors interlocked to firms that forced out their CEO receive significantly more withheld votes than directors interlocked to firms with the same propensity of a forced CEO turnover but no actual turnover. Hence, reverse causality is unlikely to drive our results.

In summary, the results in this section support our conjecture that neither poor performance, nor the (unobservable) reason behind the turnover decision, nor negative re-election vote pressure at the turnover firm drive our results. Instead, causality appears to run from forced CEO turnovers to increases in withheld votes at turnover-interlocked firms.[22]

4.3. Reputation loss versus distraction

An alternative interpretation of the increase in withheld votes following a forced CEO turnover is director distraction. A forced CEO turnover may demand significant time and effort from directors involved in the process. Consequently, turnover-interlocked directors likely divert some of their limited attention to the turnover firm, and may be penalized for it by interlocked firms' shareholders at the next re-election. Such an explanation would be in line with Masulis & Zhang (2019) and Stein & Zhao (2019), who associate increased director distraction from various sources, including CEO turnovers at interlocked firms, with reduced monitoring efficiency.

In this section, we conduct three tests of the director distraction hypothesis. The first test explores sudden CEO deaths. Falato et al. (2014) show that committee peers of suddenly deceased directors experience a workload increase that negatively impacts the attention devoted to interlocked firms. Sudden CEO deaths likely require even more attention and time of the board of directors than sudden director deaths. However, sudden CEO deaths are, by definition,

[22]These tests also address the concern that our baseline estimates are biased because of an imbalance in size between treated and control samples. Given that we continue to find results very similar to our baseline regression, it seems unlikely that a sample size imbalance drives the result in our main analysis.

outside of the board's control and thus are not expected to affect the directors' reputation. Hence, if our results are driven by director distraction, we would expect to observe an increase in withheld votes for directors who experience the sudden death of a CEO at another firm on whose board they serve. To test this empirically, we re-estimate the baseline regression in Column 1 of Table 4, augmented with a dummy equal to one if the director is interlocked to another firm that experiences a sudden death of the CEO. To construct this variable, we search for CEO departures caused by sudden deaths as defined by Nguyen & Nielsen (2010). We identify 10 sudden CEO deaths in our sample, resulting in 29 death-interlocked directorships. To augment sample size, in an alternative specification, we extend our measure of sudden CEO deaths to include cases where CEOs take health-related leaves of absence but die subsequently, resulting in four additional CEO deaths and 11 additional death-interlocked directorships.[23] Results are reported in Columns 1 and 2 of Table 9. Consistent with our reputation-based explanation but inconsistent with a distraction-based explanation, the coefficients on the dummy variables indicating an interlocked outside directorship to a firm experiencing the CEO's death are statistically insignificant in both columns.

In a second test of the director distraction hypothesis, we consider directors who share committee memberships with turnover-interlocked directors. The distraction hypothesis postulates that directors involved in a forced CEO turnover deviate their attention away from the interlocked firm towards the turnover firm. Consequently, directors at the interlocked firms that share committees with turnover-interlocked directors will take over a part of the turnover-interlocked director's committee responsibilities. Hence, within turnover-interlocked firms, there are spillover effects in director inattention from the turnover-interlocked directors to their committee peers (Falato et al., 2014). Therefore, if our results are driven by director distraction, committee peers of turnover-interlocked directors would be expected to suffer a significant increase in withheld votes as well. We test this conjecture empirically by re-estimating the baseline regression from Column 1 of Table 4, augmented with a dummy equal to one if a director shares a committee with a turnover-interlocked director. To identify such committee peers, we focus on the four main board committees, i.e., nominating, compensation, corporate governance, and audit committee, since these are typically the most important and labor-intensive committees (Masulis & Zhang, 2019). We identify 1,628 director re-elections of directors who share at least one committee with 600 turnover-interlocked directors.[24] Column 3 of Table 9 presents the results. Consistent with our reputation-based explanation but inconsistent with a distraction-based explanation, the coefficient on the dummy variable indicating a shared committee membership

[23]To ensure that these later deaths still constitute a similar shock to the turnover firm as a sudden death, we restrict the extension to subsequent deaths that happen within 30 days of the sick leave announcement.

[24]We lose seven turnover-interlocked director observations from our sample because we find no data on committees or committee memberships for three observations and no committee peers for four observations.

with a turnover-interlocked director is statistically insignificant.

In a final test of the director distraction hypothesis, we test whether forced CEO turnovers affect board meeting attendance at interlocked firms. Masulis & Zhang (2019) show that directors suffering from external distractions typically attend fewer board meetings and exhibit reduced board commitment, adversely affecting the performance of the firms at which distracted directors hold board seats. If a forced CEO turnover distracts a director in a significant manner, we would expect to observe a decline in board meeting attendance at interlocked firms. To test this conjecture empirically, we re-estimate the baseline regression from Column 1 of Table 4, but with the outcome variable replaced by the change in directors' board meeting attendance at interlocked firms. We obtain board meeting attendance data from ISS. Board meeting attendance is measured by a dummy variable set equal to one if a director attended less than 75% of all board meetings within a fiscal year. Since ISS only provides these data for S&P1500 firms, our sample size is reduced to 51,451 directorships. These directorships are held by 10,382 directors at 1,470 firms. The results are reported in Column 4 of Table 9. The estimated coefficient is close to zero and not statistically significant, suggesting that there is no relationship between interlocked forced CEO turnovers and directors' board meeting attendance. Hence, these results are again inconsistent with director distraction driving our results.

Overall, the results in this section show that exogenous shocks, which are confined to the workload of directors but do not reveal any information on directors' monitoring capabilities, do not result in increases in withheld votes. Together with the finding that forced CEO turnovers do not reduce board meeting attendance at the interlocked firms, this suggest that our results are not caused by director distraction but by shareholders reassessing individual directors' capabilities to monitor and, if needed, fire CEOs.

5. The effect of forced CEO turnovers on directors' board seats

So far, we have shown that involvement in a forced CEO turnover has negative reputational effects on involved directors, as measured by changes in withheld votes at interlocked firms. These reputational effects may extend into the director labor market and affect the career prospects of turnover-interlocked directors. Consistently, Aggarwal et al. (2019) show that directors who suffer an increase in withheld votes in uncontested elections are more likely to undergo committee demotions and sustain reduced labor market opportunities. Similarly, Johnson et al. (2021) find that directors involved in adopting a poison pill suffer reputational damage that impacts their career outlooks. Consequently, we expect that directors lose outside directorships or at least do not gain further outside directorships following a forced CEO turnover.

To analyze this conjecture empirically, we run analyses that are akin to a difference-in-differences

setting. Specifically, we analyze changes in the number of board seats between the forced CEO turnover announcement date and up to five years after the turnover (first difference) and compare these changes between turnover-interlocked directors and non-turnover-interlocked directors (second difference). To construct the sample for these analyses, we use our director re-election sample and retain all outside directors of turnover-interlocked firms at the CEO departure announcement date. Using BoardEx, we collect data on all outside directorships these directors hold at the CEO departure date and all directorships they gain or lose within one and five years after the departure.

We first analyze whether directors involved in a forced CEO turnover have a higher propensity for losing outside directorships compared to other directors of the turnover-interlocked firms. Hence, our first outcome variables are two dummy variables equal to one if a director loses at least one of her directorships within one or five years following the forced CEO turnover, respectively, and zero otherwise. Table 10 reports the results of ordinary least squares (OLS) regressions of these dummy variables on a treatment dummy indicating whether a director is turnover-interlocked and a set of director controls. Columns 1 and 2 consider a one-year horizon for the loss in board seats following the turnover, while Columns 3 and 4 use a five-year horizon. Columns 1 and 3 include turnover event fixed effects, and Columns 2 and 4 additionally include turnover-interlocked firm fixed effects. The coefficients on the dummy variable indicating an interlock to a forced CEO turnover are positive and significant at the 5% level or higher across all four columns, suggesting that involvement in a forced CEO turnover indeed increases the likelihood of losing board seats. The economic magnitude of the estimates ranges from 7.2 percentage points over a one-year horizon (Column 1) to 9.7 percentage points over a five-year horizon (Column 4). The unconditional probability of losing a directorship in our sample is 13.1% and 46.0% over a one-year and five-year period, respectively. Hence, the economic magnitude of the treatment effect is sizeable: An involvement in a forced CEO turnover increases the probability of losing a directorship by 55% versus the sample mean over one year and 21% over five years.

Next, we test whether the loss in directorships following a forced CEO turnover is driven by a loss in the directorship at the turnover firm. To this end, we omit the directorship at the turnover firm when computing our dependent variables and rerun the regressions from Table 10. The results are reported in Panel A of Table 11. We find that the coefficients on the forced interlocked dummy turn economically and statistically insignificant across all four columns. Hence, the overall loss in directorships documented in Table 10 is driven by the loss of the directorship at the turnover firm.

Second, we test whether directors can make up for the lost board seats at the turnover firm by gaining board seats at other firms. To this end, we repeat the analysis from Panel A and

replace the dependent variable with dummy variables that are equal to one if a director gains at least one new directorship within either one or five years following the turnover, respectively, and zero otherwise. The results are reported in Panel B. They show that turnover-interlocked directors are significantly more likely to gain new board seats five years after the forced CEO turnover, but not within one year after the turnover. Five years after the turnover, the likelihood of gaining a new board seat is 8.0 percentage points (Column 3) and 8.7 percentage points (Column 4) higher for turnover-interlocked directors versus non-turnover-interlocked directors, respectively. These coefficient estimates are very similar in magnitude to those for directorship losses reported in Columns 3 and 4 of Table 10, suggesting that the gains in directorships at other firms documented in Panel B of Table 11 substitute for the losses of directorships at the turnover firms. To explicitly test for such substitution effects, we replicate the analysis in Table 10 using dummies that indicate net losses of outside directorships as the dependent variable. The results in Panel C confirm our conjecture: On average, directors involved in a forced CEO turnover have a 7% higher likelihood of losing a directorship in the first year after the turnover (Columns 1 and 2). However, this initial loss, driven by the loss of the directorship at the turnover firm, is offset by a higher likelihood of gaining directorships in subsequent years. As a result, net losses of directorships five years after the turnover do not differ between turnover-interlocked directors and non-turnover-interlocked directors sitting on the boards of the same firms (Columns 3 and 4).

A reputational loss may show not only in the number of outside directorships but also in the quality of these directorships. In general, board seats at larger firms are associated with higher compensation packages (Ryan & Wiggins, 2004), more power and prestige (Shivdasani, 1993; Adams & Ferreira, 2008; Masulis & Mobbs, 2014), and better networking opportunities (Yermack, 2004; Fich, 2005). Hence, we analyze the change in the aggregate total assets represented by all firms at which a director holds an outside board seat in the first year after the turnover or the first five years after the turnover.[25] To account for the skewness in the distribution of this variable, we apply the inverse hyperbolic sine transformation to the total assets change (Burbidge et al., 1988).[26] Results are reported in Panel A of Table 12. They show that turnover-interlocked directors experience a significant decline in aggregate total assets represented by their board seats compared to non-turnover-interlocked directors. However, when we omit the turnover firms from the analysis, as in Panel B, we find no significant difference in the change of aggregate total assets represented by all board seats between turnover-interlocked directors and non-turnover-interlocked directors across both the one- and five-year horizons. Because

[25] In Table IA.10 in the Internet Appendix, we replicate this analysis using the change in market capitalization as dependent variable and find very similar results.

[26] Note that we cannot use the natural logarithm of the change in total assets as these changes can take on negative values.

board seats lost at the turnover firms tend to be replaced by new board seats over the next five years, as shown above, the newly gained board seats have to be at smaller firms, resulting in a decline in total assets represented by the entire portfolio of firms at which turnover-interlocked directors hold board seats.

Next, we test whether the cross-sectional patterns on votes withheld described in Section 3.3 also translate into the labor market and affect directors' losses of directorships. Results from regressions using the loss of board seats as dependent variable are reported in Panel A of Table IA.11 in the Internet Appendix. We find that directors are more likely to lose board seats if there is no full succession in place, if the forced turnovers are performance-induced, and if the turnover occurs during the most productive period in a CEO's tenure. In addition, we find that directors favorably inclined to the departing CEO and directors with a committee-based monitoring role towards the CEO are most likely to lose board seats. These results confirm that forced CEO turnovers in which involved directors failed to perform their duty to monitor and, if necessary, to fire CEOs in a timely and value-preserving manner are not only associated with stronger reputational losses, but also more negative labor market outcomes.[27]

Our finding that turnover-interlocked directors face a significant risk to lose their directorship at the turnover firm, as shown in Tables 10 and 11, may raise the concern that increases in withheld votes documented in Section 3 are driven by shareholders' dissatisfaction with turnover-interlocked directors losing their board seat in the turnover firm. Indeed, the loss of a directorship may be accompanied by a decline in influence, network connections, and prestige. To address this concern, we re-estimate our regressions using the propensity score-matched sample and augment the main specifications in Columns 1 to 3 of Table 8 with a dummy variable set equal to one if a director leaves the turnover/matched control firm before the next re-election at the interlocked firm and an interaction term between this variable and the dummy for an interlock to a forced CEO turnover. If shareholders punish turnover-interlocked directors for losing their board seat at the turnover firm by withholding votes, the aforementioned interaction term will capture such an effect. Results from estimating these regressions are reported in IA.12. They show that neither the interaction term nor the standalone variable indicating the loss of a board seat at the turnover/matched control firm are statistically significant in any of the three specifications. These results suggest that the increase in withheld votes of directors interlocked to a forced CEO turnover is independent of a loss of their board seat at the turnover firm. Hence, these findings again support the reputational effects-based explanation for the decline in vote support after an involvement in a forced CEO turnover.

In summary, the results in this section provide support for our conjecture that involvement in

[27] Consistent with results reported in Panel A of Table 11, we also find no or weakly significant cross-sectional patterns if the turnover firms are excluded from the sample, as reported in Panel B of Table IA.11.

a forced CEO turnover results in a reputational loss for outside directors. Directors who force out a CEO tend to lose board seats, primarily at the turnover firm, and it takes up to five years to make up for the board seat lost. These results are subject to cross-sectional variation similar to that of our main analysis on director vote outcomes. Moreover, the board seats that the directors involved in a forced CEO turnover regain tend to be at smaller firms. While generally consistent with a reputational loss following forced CEO turnovers, these results on director labor market outcomes are less conclusive than the results on votes withheld at interlocked firms presented above. However, it is important to note that future board seats may be subject to endogeneity concerns, in particular, that directors may choose to either terminate some board seats or attempt to solicit new board appointments for reasons unobservable to the researcher (Ertimur et al., 2012; Levit & Malenko, 2016).

6. Conclusion

Monitoring and, if necessary, firing the top management of a corporation is one of the primary tasks of the board of directors (e.g., Fama, 1980; Fama & Jensen, 1983). However, evidence on the consequences of forced CEO turnovers on directors responsible for monitoring the CEO is scarce. Such analysis is empirically challenging as the turnover decision is endogenous and often related to company performance (Fee et al., 2013). Moreover, the outcome variables commonly used to measure reputational effects for turnover-interlocked directors, such as changes in the number of future board memberships, are subject to endogenous selection by the directors (Levit & Malenko, 2016). We overcome these challenges by using changes in the percentage of withheld votes in director re-elections as our primary outcome variable and observing this outcome at interlocked directorships.

Our results establish novel evidence on the reputational effects of forced CEO turnovers on directors involved in the turnover decision. In particular, we find that directors interlocked to a forced CEO turnover experience an economically large and statistically significant increase in withheld votes at subsequent director re-elections, suggesting that directors involved in a forced CEO turnover lose reputation. We then investigate the drivers of this reputational loss and find that the adverse reputational effects are confined to turnovers with a high potential for disrupting a firm's management and turnovers in which directors have neglected their monitoring duty to shareholders. Moreover, we find no reputational gains for turnover-interlocked directors involved in less disruptive turnovers or directors without a committee-based monitoring duty.

The results presented in this paper challenge the widely accepted view that forcing out a CEO is a sign of well-functioning corporate governance at the board level. Our results support an alternative view: Depending on the timing and circumstances of the turnover, forcing out a

CEO can be perceived as a signal of failure in the monitoring, and in particular the firing of the CEO, and thus end up being detrimental to a director's reputation.

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Table 1: Forced CEO turnover and interlocked director characteristics at turnover firms

This table reports descriptive statistics on forced CEO turnover characteristics (Panel A) and interlocked director characteristics at the turnover firms (Panel B). A CEO departure at an S&P1500 firm between 2013 and 2017 is classified as a forced CEO turnover if i) newspaper articles indicate that the CEO is fired, is forced out of her position, or departs due to unspecified policy differences, ii) the CEO does not leave to take over an executive position at another organization, and iii) the CEO leaves within one month after the departure is announced, which includes the termination of a board membership but does not include a consulting position at the turnover firm. Interlocked directors are directors who contemporaneously serve on the board of a firm that announces a forced CEO turnover and on the board of another firm that does not fire the CEO. Interlocked directors appear in our sample if re-election vote data from ISS, director data from BoardEx and ISS, stock price data from CRSP, accounting data from Compustat, and ownership data from Thomson Reuters is available. Definitions and data sources of all variables are provided in Table A.1.

Panel A: Forced turnover characteristics

	Forced turnovers		Interlocked directorships	
	Mean	N	Mean	N
Full replacement announced (d)	0.451	206	0.448	607
Performance-induced (d)	0.881	201	0.902	591
Honeymoon stage (d)	0.320	206	0.308	607
Harvest stage (d)	0.655	206	0.685	607
Decline stage (d)	0.024	206	0.012	607

Panel B: Interlocked director characteristics at turnover firms

	Interlocked directorships	
	Mean	N
Co-opted director (d)	0.504	607
Monitoring committee member (d)	0.796	604

Table 2: Turnover-interlocked director and firm characteristics

This table reports descriptive statistics on turnover-interlocked director characteristics (Panel A), turnover-interlocked director characteristics at the interlocked firms (Panel B), and turnover-interlocked firm characteristics (Panel C). A CEO departure at an S&P1500 firm between 2013 and 2017 is classified as a forced CEO turnover if i) newspaper articles indicate that the CEO is fired, is forced out of her position, or departs due to unspecified policy differences, ii) the CEO does not leave to take over an executive position at another organization, and iii) the CEO leaves within one month after the departure is announced, which includes the termination of a board membership but does not include a consulting position at the turnover firm. Interlocked directors are directors who contemporaneously serve on the board of a firm that announces a forced CEO turnover and on the board of another firm that does not fire the CEO. Interlocked directors appear in our sample if re-election vote data from ISS, director data from BoardEx and ISS, stock price data from CRSP, accounting data from Compustat, and ownership data from Thomson Reuters is available. Definitions and data sources of all variables are provided in Table A.1.

Panel A: Turnover-interlocked director characteristics

	Mean	Median	SD	N
Age (yrs)	63.534	64.739	6.904	607
Female (d)	0.173	0.000	0.379	607
# of other board seats	3.142	3.000	1.918	607

Panel B: Turnover-interlocked director characteristics at interlocked firms

	Mean	Median	SD	N
% votes withheld	7.116	2.687	10.887	607
Tenure (yrs)	8.328	6.995	5.514	607
ISS withhold/against (d)	0.074	0.000	0.262	607

Panel C: Turnover-interlocked firm characteristics

	Mean	Median	SD	N
Total assets (millions)	14,662.478	2,532.490	56,107.593	607
Leverage	0.247	0.217	0.213	604
Tobin's Q	2.028	1.647	1.220	607
ROA	0.138	0.133	0.110	607
BH return (m270,m21)	0.022	-0.007	0.407	607
Board size	9.778	10.000	2.180	607
% outside directors	0.852	0.875	0.076	607
% busy outside directors	0.342	0.333	0.189	607
Institutional ownership (%)	0.773	0.818	0.218	607

Table 3: Non-turnover-interlocked director and firm characteristics

This table reports descriptive statistics on non-turnover-interlocked director characteristics (Panel A), non-turnover-interlocked director characteristics at the non-interlocked firms (Panel B), and non-turnover-interlocked firm characteristics (Panel C). The sample comprises director re-elections between 2003 and 2017 from the ISS Voting Analytics database, augmented with director data from BoardEx and ISS, stock price data from CRSP, accounting data from Compustat, and ownership data from Thomson Reuters. Definitions and data sources of all variables are provided in Table A.1. *, **, and ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively, of tests for differences in means and medians between the turnover-interlocked director sample reported in Table 2 and the non-turnover-interlocked director sample reported in this table.

Panel A: Non-turnover-interlocked director characteristics

	Mean	Median	SD	N
Age (yrs)	63.003	63.819*	8.707	87,786
Female (d)	0.131***	0.000***	0.337	87,799
# of other board seats	1.770***	1.000***	2.093	87,799

Panel B: Non-turnover-interlocked director characteristics at non-interlocked firms

	Mean	Median	SD	N
% votes withheld	6.139**	2.458**	9.623	87,799
Tenure (yrs)	8.691	6.995	6.604	87,799
ISS withhold/against (d)	0.096*	0.000*	0.295	87,799

Panel C: Non-turnover-interlocked firm characteristics

	Mean	Median	SD	N
Total assets (millions)	10,957.859**	1,422.700***	43,764.939	87,799
Leverage	0.240	0.204*	0.236	87,420
Tobin's Q	2.035	1.615	1.316	87,799
ROA	0.103***	0.128***	0.176	87,799
BH return (m270,m21)	0.006	-0.046***	0.718	87,799
Board size	9.223***	9.000***	2.359	87,799
% outside directors	0.836***	0.857***	0.089	87,799
% busy outside directors	0.254***	0.250***	0.193	87,799
Institutional ownership (%)	0.701***	0.794***	0.281	87,799

Table 4: Forced CEO turnovers and withheld votes at interlocked firms

This table reports results from ordinary least squares regressions of the change in withheld votes in director re-elections on a treatment indicator variable set equal to one if a director is involved in a forced CEO turnover at an interlocked firm and control variables. The regression in Column 1 includes industry × year fixed effects. Column 2 additionally includes firm fixed effects. Column 3 replaces firm by director fixed effects. The sample comprises director re-elections between 2003 and 2017 from the ISS Voting Analytics database, augmented with director data from BoardEx and ISS, stock price data from CRSP, accounting data from Compustat, and ownership data from Thomson Reuters. All control variables are in first-differences. Definitions and data sources of all variables are provided in Table A.1. t-statistics are reported in parentheses and are based on standard errors clustered at the Fama-French 48 industry level. *, **, and ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent variable:	% votes withheld		
	(1)	(2)	(3)
Forced interlocked (d)	1.204***	0.878**	1.034**
	(3.071)	(2.553)	(2.407)
ISS withhold/against (d)	18.214***	18.462***	18.644***
	(29.462)	(29.645)	(26.995)
# of other board seats	0.128***	0.133***	0.172***
	(3.474)	(3.100)	(3.866)
Board size	-0.096**	-0.087	-0.103
	(-2.081)	(-1.653)	(-1.632)
% outside directors	2.536**	2.044*	2.009
	(2.427)	(1.840)	(1.607)
% busy outside directors	0.261	0.533	0.193
	(0.614)	(0.987)	(0.354)
Institutional ownership (%)	0.049	-0.009	0.062
	(0.153)	(-0.020)	(0.131)
Total assets, log	0.562***	0.514**	0.434*
	(2.940)	(2.326)	(1.909)
Tobin's Q	-0.194***	-0.140*	-0.151*
	(-2.827)	(-1.976)	(-1.871)
ROA	-1.915**	-2.160**	-1.816*
	(-2.676)	(-2.336)	(-1.981)
BH return (m270,m21)	-0.586***	-0.564***	-0.596***
	(-6.892)	(-6.273)	(-6.711)
Year × Industry FE	Yes	Yes	Yes
Firm FE	No	Yes	No
Director FE	No	No	Yes
Observations	88,406	88,406	88,406
Firms	3,269	3,269	3,269
Directors	18,693	18,693	18,693
Turnover-interlocked directorships	607	607	607
Adjusted R^2	0.333	0.347	0.354

Table 5: Cross-sectional tests: Turnover and director characteristics

This table reports results from ordinary least squares regressions of the change in withheld votes in director re-elections on a treatment indicator variable set equal to one if a director is involved in a forced CEO turnover at an interlocked firm and control variables. In all columns, the treatment indicator is split based on whether a full CEO replacement is announced with the departure or not (Column 1), the turnover is performance-induced or not (Column 2), whether the turnover takes place during the first three years (i.e., Honeymoon stage), between the third and 13th year (i.e., Harvest stage), or after the 13th year of the departing CEO's tenure (i.e., Decline stage; Column 3), the turnover-interlocked director is a member of the nomination or compensation committee of the turnover firm or not (Column 4), and whether the turnover-interlocked director is co-opted, i.e., was appointed to the turnover firm's board during the departing CEO's tenure (Column 5). All regressions include industry × year fixed effects. The sample comprises director re-elections between 2003 and 2017 from the ISS Voting Analytics database, augmented with director data from BoardEx and ISS, stock price data from CRSP, accounting data from Compustat, and ownership data from Thomson Reuters. All control variables are in first-differences. The control variables are the same as in Table 4. t-statistics are reported in parentheses and are based on standard errors clustered at the Fama-French 48 industry level. *, **, and ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent variable:	% votes withheld				
	(1)	(2)	(3)	(4)	(5)
Forced w. full replacement (d)	0.837				
	(1.539)				
Forced w/o full replacement (d)	1.507***				
	(3.172)				
Forced performance-induced (d)		1.415***			
		(3.450)			
Forced non-performance-induced (d)		-0.656			
		(-0.822)			
Forced during Honeymoon stage (d)			0.197		
			(0.314)		
Forced during Harvest stage (d)			1.679***		
			(4.288)		
Forced during Decline stage (d)			-0.121		
			(-0.049)		
Forced/monitoring committee member (d)				1.566***	
				(3.748)	
Forced/not monitoring committee member (d)				-0.246	
				(-0.348)	
Forced/co-opted director (d)					1.670***
					(3.373)
Forced/non-co-opted director (d)					0.720
					(1.566)
Control variables	Yes	Yes	Yes	Yes	Yes
Year × Industry FE	Yes	Yes	Yes	Yes	Yes
Observations	88,406	88,390	88,406	88,403	88,406
Firms	3,269	3,269	3,269	3,269	3,269
Directors	18,693	18,693	18,693	18,693	18,693
Turnover-interlocked directorships	607	591	607	604	607
Adjusted R^2	0.333	0.333	0.334	0.334	0.333

Table 6: Cross-sectional tests: Common ownership in turnover and interlocked firms

This table reports results from ordinary least squares regressions of the change in withheld votes in director re-elections on a treatment indicator variable set equal to one if a director is involved in a forced CEO turnover at an interlocked firm and control variables. In Column 1 (2), the treatment indicator is split into two variables depending on whether common institutional ownership in the turnover and the interlocked firm is above the mean (median) common ownership. All regressions include industry × year fixed effects. The sample comprises director re-elections between 2003 and 2017 from the ISS Voting Analytics database, augmented with director data from BoardEx and ISS, stock price data from CRSP, accounting data from Compustat, and ownership data from Thomson Reuters. All control variables are in first-differences. The control variables are the same as in Table 4. t-statistics are reported in parentheses and are based on standard errors clustered at the Fama-French 48 industry level. *, **, and ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent variable:	% votes withheld	
	(1)	(2)
Forced interlocked ≥ mean common own. (d)	1.442**	
	(2.678)	
Forced interlocked < mean common own. (d)	0.900	
	(1.670)	
Forced interlocked ≥ median common own. (d)		1.513**
		(2.603)
Forced interlocked < median common own. (d)		0.870*
		(1.758)
Control variables	Yes	Yes
Year × Industry FE	Yes	Yes
Observations	88,390	88,390
Firms	3,269	3,269
Directors	18,693	18,693
Turnover-interlocked directorships	591	591
Adjusted R^2	0.334	0.334

Table 7: Treatment reversal and pseudo treatments

This table reports results from ordinary least squares regressions of the change in withheld votes in director re-elections on a treatment indicator variable set equal to one if a director is involved in a CEO turnover at an interlocked firm and control variables. In Columns 1 to 4, treated directors are directors who force out a CEO at an interlocked firm. In Columns 1 and 2, the baseline regression from Column 1 of Table 4 is augmented with one and two lagged treatment indicators, respectively. In Columns 3 and 4, pseudo treatment indicator variables are set equal to one for one and two re-elections prior to the actual forced CEO turnover, respectively. Regressions in Columns 5 to 7 replicate the baseline regressions of Table 4 but use unforced CEO turnovers as pseudo treatment events. In Column 8, we augment the baseline regression from Column 1 of Table 4 with an indicator variable that is equal to one if the director was on the board of the turnover firm during the departing CEO's tenure but left the board before the turnover. The regressions in Columns 1 to 5 and 8 include industry × year fixed effects. Column 6 additionally includes firm fixed effects. Columns 7 replaces firm by director fixed effects. The sample comprises director re-elections between 2003 and 2017 from the ISS Voting Analytics database, augmented with director data from BoardEx and ISS, stock price data from CRSP, accounting data from Compustat, and ownership data from Thomson Reuters. All control variables are in first-differences. The control variables are the same as in Table 4. Definitions and data sources of all variables are provided in Table A.1. t-statistics are reported in parentheses and are based on standard errors clustered at the Fama-French 48 industry level. *, **, and ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent variable:	% votes withheld							
	(1) Baseline plus lag	(2) Baseline plus lags	(3) Pseudo t-1	(4) Pseudo t-2	(5) Unforced turnover	(6) Unforced turnover	(7) Unforced turnover	(8) Left before
Forced interlocked (d)	1.202***	1.279***	0.020	-0.390	1.173***	0.873**	1.038**	1.207***
	(3.052)	(2.863)	(0.054)	(-0.835)	(3.001)	(2.541)	(2.414)	(3.083)
Forced interlocked at $t-1$ (d)	0.097	0.373						
	(0.272)	(1.066)						
Forced interlocked at $t-2$ (d)		-0.325						
		(-0.506)						
Unforced interlocked (d)					0.490***	0.137	0.239	
					(3.848)	(1.150)	(1.304)	
Left before forced turnover (d)								-0.566
								(-0.982)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year × Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	No	No	No	No	Yes	No	No
Director FE	No	No	No	No	No	No	Yes	No
Observations	88,406	64,555	64,450	47,544	88,406	88,406	88,406	88,406
Firms	3,269	2,582	2,577	2,098	3,269	3,269	3,269	3,269
Directors	18,693	13,817	13,788	10,588	18,693	18,693	18,693	18,693
Turnover-interlocked directorships	607	424	420	315	3,325	3,325	3,325	607
Adjusted R^2	0.333	0.360	0.418	0.431	0.334	0.347	0.354	0.333

Table 8: Propensity score matching analysis

This table reports results from ordinary least squares regressions of withheld votes in director re-elections on a treatment indicator set equal to one if a director is involved in a forced CEO turnover at an interlocked firm and control variables. The regressions in Columns 2, 5, and 8 include year and interlocked firm industry fixed effects. The regressions in Columns 3, 6, and 9 include turnover event fixed effects. The treatment sample comprises re-elections of directors involved in a forced CEO turnover at an interlocked firm. The control sample comprises re-elections of directors interlocked to a sample of propensity score matched firms with characteristics similar to the turnover firms. The sample in Columns 1 to 3 is based on propensity scores estimated using the forced CEO turnover likelihood model of Peters & Wagner (2014). The sample in Columns 4 to 6 is based on propensity scores estimated using the forced CEO turnover likelihood model of Peters & Wagner (2014) augmented with aggregate news sentiment prior to the CEO turnover, and the sample in Columns 7 to 9 is based on propensity scores estimated using the forced CEO turnover likelihood model of Peters & Wagner (2014) augmented with the mean change in director re-election votes of all board members prior to the CEO turnover. For details, see Section 4.2. Definitions and data sources of all variables are provided in Table A.1. t-statistics are reported in parentheses and are based on standard errors clustered at the Fama-French 48 industry level (Columns 1, 2, 4, and 5) or at the turnover level (Columns 3 and 6). *, **, and ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent variable:	% votes withheld								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Forced interlocked (d)	1.439**	1.436***	1.010*	1.388**	1.395***	0.846*	1.485**	1.252**	0.971*
	(2.687)	(2.961)	(1.834)	(2.449)	(2.968)	(1.753)	(2.592)	(2.232)	(1.821)
ISS withhold/against (d)	19.504***	19.321***	19.688***	19.074***	19.218***	19.462***	21.802***	22.310***	22.162***
	(10.423)	(10.077)	(10.231)	(9.765)	(9.608)	(11.348)	(11.626)	(11.365)	(14.797)
Female (d)	-0.480	-0.329	-0.379	-0.804	-0.811	-0.077	-1.050	-0.556	-0.586
	(-0.707)	(-0.499)	(-0.701)	(-1.303)	(-1.247)	(-0.115)	(-1.295)	(-0.644)	(-0.985)
Age (yr)	-0.016	-0.037	-0.039	0.026	0.003	0.010	0.045	0.044	0.022
	(-0.354)	(-0.791)	(-1.017)	(0.890)	(0.070)	(0.288)	(1.205)	(1.078)	(0.481)
Tenure (yrs), log	1.507***	1.323***	1.809***	0.894**	0.734*	0.600*	1.215***	1.165***	1.099***
	(4.040)	(3.817)	(5.295)	(2.184)	(1.907)	(1.882)	(2.806)	(2.926)	(2.821)
# of other board seats	-0.034	0.045	0.039	0.251*	0.296**	0.285**	0.265**	0.325***	0.275**
	(-0.217)	(0.364)	(0.220)	(1.957)	(2.205)	(2.204)	(2.442)	(3.057)	(2.091)
Board size	-0.114	-0.043	-0.134	-0.162	-0.055	-0.166	-0.325**	-0.192	-0.300
	(-0.785)	(-0.331)	(-0.832)	(-1.349)	(-0.422)	(-1.335)	(-2.407)	(-1.404)	(-1.602)

Table 8: Propensity score matching analysis (cont.)

Dependent variable:	% votes withheld								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	(1.690)	(-0.093)	(1.227)	(0.944)	(-0.217)	(0.409)	(1.148)	(0.567)	(0.342)
% busy outside dirs	-0.001	0.315	0.663	0.687	1.144	-0.638	0.740	0.923	0.340
	(-0.000)	(0.257)	(0.438)	(0.532)	(0.931)	(-0.473)	(0.479)	(0.635)	(0.221)
Inst. ownership (%)	0.560	0.141	-0.501	-0.631	-0.302	-1.385	0.200	-0.357	-0.533
	(0.486)	(0.129)	(-0.480)	(-0.644)	(-0.373)	(-1.346)	(0.177)	(-0.285)	(-0.497)
Total assets, log	0.140	0.370	0.011	0.436	0.537**	0.375*	0.531*	0.528*	0.411
	(0.551)	(1.347)	(0.043)	(1.666)	(2.169)	(1.795)	(1.758)	(1.868)	(1.516)
Tobin's Q	0.043	0.161	0.053	-0.163	-0.082	-0.249	0.183	0.322*	0.149
	(0.180)	(0.673)	(0.249)	(-0.792)	(-0.343)	(-1.284)	(1.120)	(1.929)	(0.637)
ROA	-2.518	-4.135**	-1.373	-2.246	-2.165	-1.605	-2.533	-3.623*	-1.456
	(-0.963)	(-2.142)	(-0.676)	(-1.599)	(-1.555)	(-0.925)	(-0.954)	(-1.701)	(-0.744)
BH ret (m270,m21)	-1.944***	-1.628***	-2.012***	-1.563***	-1.495***	-1.605***	-3.154***	-3.261***	-3.667***
	(-3.122)	(-2.928)	(-3.198)	(-3.379)	(-3.170)	(-2.881)	(-4.363)	(-3.906)	(-4.579)
Year FE	No	Yes	No	No	Yes	No	No	Yes	No
Industry FE	No	Yes	No	No	Yes	No	No	Yes	No
Turnover FE	No	No	Yes	No	No	Yes	No	No	Yes
Observations	1,111	1,111	1,111	1,071	1,071	1,071	934	934	934
Firms	745	745	745	730	730	730	646	646	646
Directors	762	762	762	720	720	720	619	619	619
Turnover-interl. d'ships	525	525	525	515	515	515	439	439	439
Adjusted R^2	0.284	0.303	0.305	0.268	0.285	0.302	0.351	0.372	0.393

Table 9: Forced CEO turnovers, director distraction, and withheld votes at interlocked firms

This table reports results from ordinary least squares regressions of the change in withheld votes in director re-elections (Columns 1-3) or the change in board meeting attendance (Column 4) on a treatment indicator variable set equal to one if a director is involved in a forced CEO turnover at an interlocked firm and control variables. The regressions in Columns 1-3 augment the baseline regression from Column 1 of Table 4 with different proxy variables for a positive shock to directors' workload at an interlocked firm. In Column 1, the proxy for the workload shock is the unexpected death of the CEO. In Column 2, the sample of unexpected CEO deaths is augmented with CEO sick leaves during which the CEO subsequently dies. In Column 3, the proxy for the workload shock is a joint committee membership with a forced CEO turnover-interlocked director. The regression in Column 4 shows the results of the baseline regression from Column 1 of Table 4, but with the outcome variable replaced by the change in board meeting attendance. Meeting attendance is measured by a dummy variable set equal to one if a director attended less than 75% of all interlocked firm board meetings within a fiscal year. All regressions include industry × year fixed effects. The sample comprises director re-elections between 2003 and 2017 from the ISS Voting Analytics database, augmented with director data from BoardEx and ISS, stock price data from CRSP, accounting data from Compustat, and ownership data from Thomson Reuters. All control variables are in first-differences. The control variables are the same as in Table 4. Definitions and data sources of all variables are provided in Table A.1. t-statistics are reported in parentheses and are based on standard errors clustered at the Fama-French 48 industry level. *, **, and ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent variable:	% votes withheld			Attended < 75% of meetings
	(1)	(2)	(3)	(4)
Forced interlocked (d)	1.211***	1.209***	1.223***	-0.001
	(3.070)	(3.067)	(3.012)	(-0.238)
Death interlocked (d)	-0.586			
	(-1.005)			
Death interlocked (extended) (d)		0.768		
		(0.870)		
Committee-interlocked (d)			0.495	
			(1.495)	
Control variables	Yes	Yes	Yes	Yes
Year × Industry FE	Yes	Yes	Yes	Yes
Observations	88,372	88,363	85,778	51,451
Firms	3,269	3,269	3,256	1,470
Directors	18,692	18,692	18,036	10,382
Turnover-interlocked directorships	605	605	600	434
Death/Committee-interlocked directorships	28	39	1628	-
Adjusted R^2	0.333	0.333	0.338	0.036

Table 10: Forced CEO turnovers and losses of directorships

This table reports results from ordinary least squares regressions of a dummy variable indicating a loss of directorships within t years after the turnover on a treatment dummy variable set equal to one if a director is involved in a forced CEO turnover and control variables. All regressions include turnover event fixed effects. The regressions in Columns 2 and 4 additionally include interlocked firm fixed effects. The treatment sample comprises outside directorships of directors who are interlocked to forced CEO turnovers between 2003 and 2017. The control sample comprises outside directorships of directors who are not interlocked to a forced turnover but share a board seat with a director interlocked to a forced CEO turnover at the turnover announcement. Definitions and data sources of all variables are provided in Table A.1. t-statistics are reported in parentheses and are based on standard errors clustered at the turnover level. *, **, and ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent variable:	Loses directorships within t years (d)			
	(1) t=[0,1]	(2) t=[0,1]	(3) t=[0,5]	(4) t=[0,5]
Forced interlocked (d)	0.072***	0.075***	0.088**	0.097***
	(2.979)	(2.919)	(2.595)	(2.706)
Director age (yrs)	-0.000	0.000	0.009***	0.009***
	(-0.217)	(0.328)	(6.832)	(6.021)
Female (d)	-0.008	-0.018	0.039*	0.027
	(-0.535)	(-1.102)	(1.722)	(1.098)
# of other board seats	0.031***	0.028***	0.046***	0.043***
	(4.760)	(4.052)	(4.391)	(3.703)
Tenure (yrs), log	-0.000	0.000	0.025*	0.045***
	(-0.048)	(0.045)	(1.888)	(2.810)
Turnover FE	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	Yes
Observations	4,013	4,013	3,300	3,300
Firms	465	465	404	404
Directors	2,885	2,885	2,425	2,425
Turnover-interlocked directorships	594	594	489	489
Adjusted R^2	0.061	0.055	0.093	0.092

Table 11: Forced CEO turnovers and gains and losses of directorships

This table reports results from ordinary least squares regressions of a dummy variable indicating a loss (Panel A), a gain (Panel B), or a net loss (Panel C) of directorships within t years after a forced CEO turnover on a treatment dummy variable set equal to one if a director is involved in a forced CEO turnover and control variables. In Panel A, the turnover firms are omitted when constructing the dependent variable. All regressions include turnover event fixed effects. The regressions in Columns 2 and 4 additionally include interlocked firm fixed effects. The treatment sample comprises outside directorships of directors who are interlocked to forced CEO turnovers between 2003 and 2017. The control sample comprises outside directorships of directors who are not interlocked to a forced turnover but share a board seat with a director interlocked to a forced CEO turnover at the turnover announcement. The control variables are the same as in Table 10. Definitions and data sources of all variables are provided in Table A.1. t-statistics are reported in parentheses and are based on standard errors clustered at the turnover level. *, **, and ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A: Director turnover likelihood (w/o turnover firm)

Dependent variable:	Loses directorships within t years (d)			
	(1) t=[0,1]	(2) t=[0,1]	(3) t=[0,5]	(4) t=[0,5]
Forced interlocked (d)	-0.002	-0.000	-0.024	-0.016
	(-0.088)	(-0.018)	(-0.767)	(-0.489)
Director control variables	Yes	Yes	Yes	Yes
Turnover FE	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	Yes
Observations	4,013	4,013	3,300	3,300
Firms	465	465	404	404
Directors	2,885	2,885	2,425	2,425
Turnover-interlocked directorships	594	594	489	489
Adjusted R^2	0.054	0.046	0.086	0.084

Table 11: Forced CEO turnovers and gains and losses of directorships (cont.)

Panel B: The likelihood of new directorships

Dependent variable:	Gains directorships within t years (d)			
	(1) t=[0,1]	(2) t=[0,1]	(3) t=[0,5]	(4) t=[0,5]
Forced interlocked (d)	0.017	0.018	0.080***	0.087***
	(0.793)	(0.777)	(2.929)	(2.942)
Director control variables	Yes	Yes	Yes	Yes
Turnover FE	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	Yes
Observations	4,013	4,013	3,300	3,300
Firms	465	465	404	404
Directors	2,885	2,885	2,425	2,425
Turnover-interlocked directorships	594	594	489	489
Adjusted R^2	0.021	0.017	0.089	0.087

Panel C: The likelihood of a net loss of board seats

Dependent variable:	Net loss of directorships within t years (d)			
	(1) t=[0,1]	(2) t=[0,1]	(3) t=[0,5]	(4) t=[0,5]
Forced interlocked (d)	0.067***	0.068***	0.031	0.033
	(3.038)	(2.870)	(1.098)	(1.103)
Control variables	Yes	Yes	Yes	Yes
Turnover FE	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	Yes
Observations	4,013	4,013	3,300	3,300
Firms	465	465	404	404
Directors	2,885	2,885	2,425	2,425
Turnover-interlocked directorships	594	594	489	489
Adjusted R^2	0.046	0.042	0.083	0.092

Table 12: Forced CEO turnovers and the cumulative asset size of all board seats

This table reports results from ordinary least squares regressions of the change in the cumulative assets of all firms, where a director holds board seats t years after a forced CEO turnover, on a treatment dummy variable set equal to one if a director is involved in the forced CEO turnover and control variables. In Panel B, the turnover firms are omitted when constructing the dependent variable. The dependent variable is transformed using the inverse hyperbolic sine transformation. All regressions include turnover event fixed effects. The regressions in Columns 2 and 4 additionally include interlocked firm fixed effects. The treatment sample comprises outside directorships of directors who are interlocked to forced CEO turnovers between 2003 and 2017. The control sample comprises outside directorships of directors who are not interlocked to a forced turnover but share a board seat with a director interlocked to a forced CEO turnover at the turnover announcement. The control variables are the same as in Table 10. Definitions and data sources of all variables are provided in Table A.1. t-statistics are reported in parentheses and are based on standard errors clustered at the turnover level. *, **, and ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A: The change in the sum of total assets of all board seats

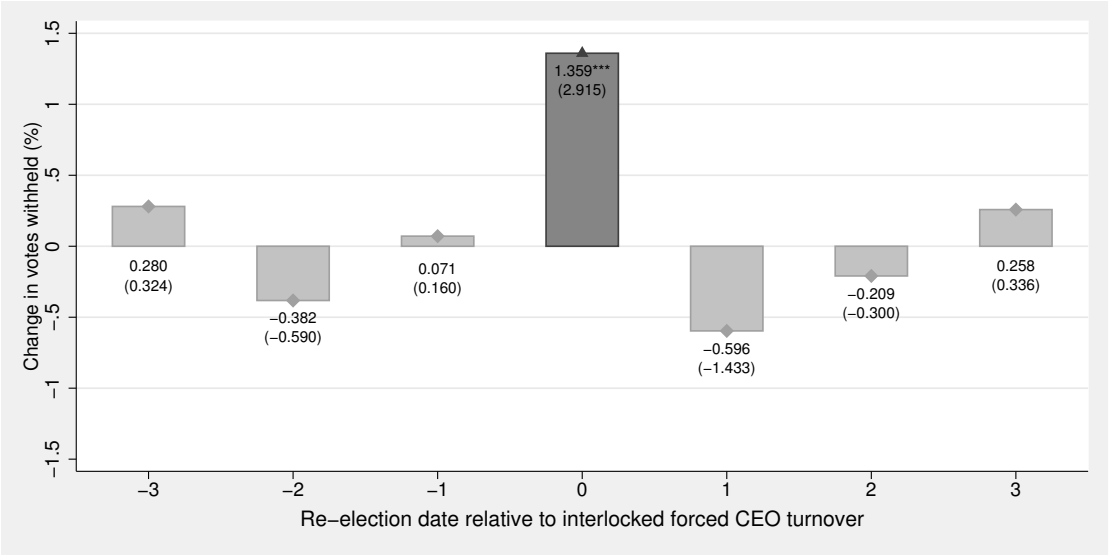
Dependent variable:	Difference in the sum of total assets over all directorships			
	(1)	(2)	(3)	(4)
	$t_1 - t_0$	$t_1 - t_0$	$t_5 - t_0$	$t_5 - t_0$
Forced interlocked (d)	-1.065***	-1.100***	-1.827***	-1.783***
	(-3.632)	(-3.503)	(-3.631)	(-3.313)
Director control variables	Yes	Yes	Yes	Yes
Turnover FE	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	Yes
Observations	4,013	4,013	3,300	3,300
Firms	465	465	404	404
Directors	2,885	2,885	2,425	2,425
Turnover-interlocked directorships	594	594	489	489
Adjusted R^2	0.025	0.027	0.106	0.108

Table 12: Forced CEO turnovers and the cumulative asset size of all board seats (cont.)

Panel B: The change in the sum of total assets of all board seats (w/o turnover firm)

Dependent variable:	Difference in the sum of total assets over all directorships			
	(1) $t_1 - t_0$	(2) $t_1 - t_0$	(3) $t_5 - t_0$	(4) $t_5 - t_0$
Forced interlocked (d)	-0.055	-0.067	0.623	0.660
	(-0.217)	(-0.249)	(1.275)	(1.267)
Director control variables	Yes	Yes	Yes	Yes
Turnover FE	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	Yes
Observations	4,013	4,013	3,300	3,300
Firms	465	465	404	404
Directors	2,885	2,885	2,425	2,425
Turnover-interlocked directorships	594	594	489	489
Adjusted R^2	0.015	0.015	0.097	0.099

Figure 1: Forced CEO turnovers and withheld votes at interlocked firms
This figure plots coefficient estimates obtained from an ordinary least squares regression of the change in withheld votes in director re-elections on a treatment indicator set equal to one if a director is involved in a forced CEO turnover at an interlocked firm. Each bar represents the coefficient estimate from a separate regression using up to three leads or up to three lags of the turnover-interlock dummy to capture re-elections three years before the forced CEO turnover to three years after the forced CEO turnover. The sample comprises director re-elections between 2003 and 2017 from the ISS Voting Analytics database, augmented with director data from BoardEx and ISS, stock price data from CRSP, accounting data from Compustat, and ownership data from Thomson Reuters. t-statistics are reported in parentheses and are based on standard errors clustered at the Fama-French 48 industry level. *, **, and ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively. Definitions and data sources of all variables are provided in Table A.1.



Appendix

Table A.1: Variable definitions

This table reports variable definitions of all variables used in the paper as well as their data sources. Database mnemonics are provided in italic capitals (if available).

Panel A: Director characteristics

Variable	Definition	Source
Forced interlocked (d)	Dummy equal to one if a director is involved in a forced CEO turnover at an interlocked firm, zero otherwise	Hand-collected
Unforced interlocked (d)	Dummy equal to one if a director is involved in a CEO turnover not classified as forced at an interlocked firm, zero otherwise	Hand-collected
Death interlocked (d)	Dummy equal to one if a director is involved in a CEO turnover at an interlocked firm that is caused by the sudden death of the CEO, zero otherwise	Hand-collected
Death interlocked (extended) (d)	Dummy equal to one if a director is involved in a CEO turnover at an interlocked firm that is caused by the sudden death of the CEO, zero otherwise. We also consider cases where CEOs take health-related leaves of absence but die subsequently	Hand-collected
Left before forced turnover (d)	Dummy equal to one if a director was on the board of the turnover firm during the departing CEO's tenure but left the turnover firm's board before the turnover, zero otherwise	Hand-collected
Age (yrs)	Age of the director	BoardEx/ISS
Female (d)	Dummy equal to one if the director is female, zero otherwise	BoardEx/ISS
Tenure (yrs)	Time in years the director has spent on a firm's board	BoardEx/ISS
# of other board seats	Number of other outside board seats held by the director	BoardEx
ISS withhold/against (d)	Dummy equal to one if ISS recommended shareholders to withhold their votes or vote against the re-election of the director, zero otherwise	ISS
% votes withheld	Fraction of votes withheld and against the re-election of the director; $(votedagainst + votedabstain)/(votedfor + votedagainst + votedabstain)$	ISS
Co-opted director (d)	Dummy equal to one if the interlocked director became director after the departing CEO was appointed, zero otherwise	BoardEx/ISS
Monitoring committee (d)	Dummy equal to one if the interlocked director is member of the nomination or compensation committee, zero otherwise	BoardEx/ISS
Committee-interlocked (d)	Dummy equal to one if a director is a member of the same main committee as a forced turnover interlocked director, zero otherwise. Main committees are the nominating, compensation, corporate governance, and audit committee	BoardEx/ISS

Table A.1: Variable definitions (cont.)

Panel B: Firm characteristics

Variable	Definition	Source
Total assets	Total assets in million USD; AT	Compustat
Leverage	Financial leverage; $(DLC + DLTT)/AT$	Compustat
Tobin's Q	Tobin's Q (market value of assets to book value); $(AT + CSHO * PRCC\_F - CEQ - TXDB)/AT$. Missing values in $TXDB$ have been set to 0. Winsorized at the 1st and 99th percentiles	Compustat
ROA	Return on assets; $coalesce(OIBDP, SALE - XOPR, REVT - XOPR)/((AT + lag(AT))/2)$. Winsorized at the 1st and 99th percentiles	Compustat
BH return (m270,m21)	Buy-and-hold-return from $t-270$ to $t-21$ with $t=0$ being the director re-election date; adjusted for equally weighted market return. Winsorized at the 1st and 99th percentiles	CRSP
Board size	Number of directors on the board	Boardex
% outside directors	The percentage of outside directors in the board, as a fraction of board size	BoardEx
% busy outside directors	The percentage of outside directors with more than 2 board memberships, as a fraction of number of outside directors	BoardEx
Institutional ownership (%)	The percentage of shares owned by 13F institutions	Thomson Reuters

Panel C: Forced turnover characteristics

Variable	Definition	Source
Full replacement announced (d)	Dummy equal to one if the company announced a full replacement CEO at the departure announcement, zero otherwise	Hand-collected
Performance-induced (d)	Dummy equal to one if the forced CEO turnover was performance-induced as estimated using the two-probit model by Jenter & Lewellen (2021), zero otherwise	See Table IA.1
Honeymoon stage (d)	Dummy equal to one if the outgoing CEO had a tenure of less than three years, zero otherwise	BoardEx/ISS
Harvest stage (d)	Dummy equal to one if the outgoing CEO had a tenure of between three and 13 years, zero otherwise	BoardEx/ISS
Decline stage (d)	Dummy equal to one if the outgoing CEO had a tenure of more than 13 years, zero otherwise	BoardEx/ISS

Panel D: Board turnover variables

Variable	Definition	Source
Loses directorships within t years (d)	Dummy equal to one if the director loses any of her outside directorships within t years following the interlocked forced CEO turnover, zero otherwise	BoardEx
Gains directorships within t years (d)	Dummy equal to one if the director obtains a new outside directorships within t years following the interlocked forced CEO turnover, zero otherwise	BoardEx
Net loss of directorships within t years (d)	Dummy equal to one if the director loses more outside directorships than she gains new ones within t years following the interlocked forced CEO turnover, zero otherwise	BoardEx
Sum of total assets over all directorships	Sum of total assets in million USD over all directorships a director holds; AT	BoardEx, Compustat

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Table IA.1: Variable definitions of variables used in the performance-induced turnover model

This table reports variable definitions of all variables used in the two-probit performance-induced turnover model borrowed from (Jenter & Lewellen, 2021). Database mnemonics are provided in italic capitals (if available).

Variable	Definition	Source
Turnover (d)	Dummy equal to one if a firm experiences a CEO turnover in a given year, zero otherwise	BoardEx
Scaled return	Average one year industry-adjusted monthly stock returns, scaled by the standard deviation of returns over the past 48 months. $t = 0$ is defined as the fiscal year for non-turnover years, and the 12 months until the turnover announcement date for turnover years	CRSP, Ken French's Data Library
Age (yrs)	Age of the CEO	BoardEx
Age 61-63 (d)	Dummy equal to one if the age of the CEO is between 61 and 63 years, zero otherwise	BoardEx
Age 64-66 (d)	Dummy equal to one if the age of the CEO is between 64 and 66 years, zero otherwise	BoardEx
Age $>$ 66 (d)	Dummy equal to one if the age of the CEO is above 66 years	BoardEx
Tenure (yrs)	Time in years the CEO spent in office, zero otherwise	BoardEx
Dividend payer (d)	Dummy equal to one if the company paid a non-zero dividend, zero otherwise; $DVPSX\_F > 0$	Compustat
Total assets	Total assets in million USD; AT	Compustat

Table IA.2: Determinants of performance-induced CEO turnovers

This table reports results from probit models of a dummy equal to one if a firm experiences a CEO turnover in a given year on firm performance measures and controls (Jenter & Lewellen, 2021). The sample consists of all firm-years of S&P1500 firms for which data to construct the full set of explanatory variables is available. Definitions and data sources of all variables are provided in Table IA.1 in the Appendix. z-statistics are reported in parentheses. *, **, and ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent variable:	Turnover (d)	
	(1) Performance-induced	(2) Other
Scaled return t=0	-0.028***	
	(-7.837)	
Scaled return t=-1	-0.021***	
	(-5.902)	
Scaled return t=-2	-0.006*	
	(-1.888)	
Age (yrs)	0.036***	0.025***
	(17.881)	(8.556)
Age 61-63 (d)		0.218***
		(4.910)
Age 64-66 (d)		0.449***
		(8.280)
Age > 66 (d)		0.148**
		(2.468)
Tenure (yrs)	-0.009***	-0.008***
	(-4.562)	(-4.248)
Dividend payer (d)	-0.086***	-0.082***
	(-3.012)	(-2.891)
Total assets, log	-0.014*	-0.013*
	(-1.786)	(-1.649)
Observations	20,471	20,471
Firms	2,523	2,523
Pseudo R^2	0.036	0.034

Table IA.3: Robustness test: Exploring unusually high losses in vote support

This table reports results from ordinary least squares regressions of the change in withheld votes in director re-elections on a treatment indicator variable set equal to one if a director is involved in a forced CEO turnover at an interlocked firm and control variables. The table reports results from tests that replicate the baseline estimates in Table 4, but uses as dependent variable the change in a dummy variable set equal to one if 15% or more of the votes cast in a director's re-election are against her (Bach & Metzger, 2015). The regression in Column 1 includes industry × year fixed effects. Column 2 additionally includes firm fixed effects. Column 3 replaces firm by director fixed effects. The sample comprises director re-elections between 2003 and 2017 from the ISS Voting Analytics database, augmented with director data from BoardEx and ISS, stock price data from CRSP, accounting data from Compustat, and ownership data from Thomson Reuters. All control variables are in first-differences. The control variables are the same as in Table 4. t-statistics are reported in parentheses and are based on standard errors clustered at the Fama-French 48 industry level. *, **, and ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent variable:	≥15% votes withheld (d)		
	(1)	(2)	(3)
Forced interlocked (d)	0.038***	0.036***	0.040***
	(3.012)	(2.913)	(2.705)
Control variables	Yes	Yes	Yes
Year × Industry FE	Yes	Yes	Yes
Firm FE	No	Yes	No
Director FE	No	No	Yes
Observations	88,406	88,406	88,406
Firms	3,269	3,269	3,269
Directors	18,693	18,693	18,693
Turnover-interlocked directorships	607	607	607
Adjusted R^2	0.148	0.159	0.170

Table IA.4: Robustness test: Excluding directors without multiple board seats

This table reports results from ordinary least squares regressions of the change in withheld votes in director re-elections on a treatment indicator variable set equal to one if a director is involved in a forced CEO turnover at an interlocked firm and control variables. The table reports results from tests that replicate the baseline estimates in Table 4, but omit control directors who do not hold multiple board seats. The regression in Column 1 includes industry × year fixed effects. Column 2 additionally includes firm fixed effects. Column 3 replaces firm by director fixed effects. The sample comprises director re-elections between 2003 and 2017 from the ISS Voting Analytics database, augmented with director data from BoardEx and ISS, stock price data from CRSP, accounting data from Compustat, and ownership data from Thomson Reuters. All control variables are in first-differences. The control variables are the same as in Table 4. Definitions and data sources of all variables are provided in Table A.1. t-statistics are reported in parentheses and are based on standard errors clustered at the Fama-French 48 industry level. *, **, and ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent variable:	% votes withheld		
	(1)	(2)	(3)
Forced interlocked (d)	1.116***	0.842**	0.878**
	(2.843)	(2.378)	(2.044)
Control variables	Yes	Yes	Yes
Year × Industry FE	Yes	Yes	Yes
Firm FE	No	Yes	No
Director FE	No	No	Yes
Observations	62,226	62,226	62,226
Firms	3,136	3,136	3,136
Directors	12,705	12,705	12,705
Turnover-interlocked directorships	607	607	607
Adjusted R^2	0.331	0.345	0.352

Table IA.5: Variable definitions for variables used in propensity score matching
This table reports variable definitions of all variables used in the propensity score estimation model of Peters & Wagner (2014). Database mnemonics are provided in italic capitals (if available).

Variable	Definition	Source
Forced turnover (d)	Dummy equal to one if a firm experiences a forced CEO turnover in a given year, zero otherwise	Hand-collected
Industry volatility in t-1	Industry volatility in $t-1$ years with $t=0$ being the CEO turnover date, calculated over 10 years, using monthly returns	Ken French's Data Library
Total assets	Total assets in million USD; AT	Compustat
Tobin's Q	Tobin's Q (market value of assets to book value); $(AT + CSHO * PRCC\_F - CEQ - TXDB)/AT$. Missing values in $TXDB$ have been set to 0. Winsorized at the 1st and 99th percentiles	Compustat
Idiosyncratic return (m270,m21)	Buy-and-hold-return from $t-270$ to $t-21$ with $t=0$ being the CEO turnover date; adjusted for equally weighted industry return. Winsorized at the 1st and 99th percentiles	CRSP, Ken French's Data Library
Market-adj. industry return (m270,m21)	Buy-and-hold-return from $t-270$ to $t-21$ with $t=0$ being the CEO turnover date; adjusted for equally weighted market return. Winsorized at the 1st and 99th percentiles	CRSP, Ken French's Data Library
Industry-adj. volatility in t-1	Volatility in $t-1$ years with $t=0$ being the CEO turnover date, calculated over 48 months, adjusted for equally weighted industry returns, using monthly returns	CRSP, Ken French's Data Library
Equity-based pay >0 (d)	Dummy equal to one if the CEO received a positive amount of equity-based pay, zero otherwise	Execucomp
Dollar incentives	Pay-performance-sensitivity, calculated based on the SAS code from Daniel et al. (2020). Winsorized at the 1st and 99th percentiles	Execucomp
CEO age ≥ 60 (d)	Dummy equal to one if the CEO is older than 59, zero otherwise	BoardEx
CEO tenure	Time in years the CEO spent in office	BoardEx/ Execucomp
CEO is outsider (d)	Dummy equal to one if the CEO joined the company less than one year prior to his appointment as CEO, zero otherwise	BoardEx/ Execucomp
CEO is chairman (d)	Dummy equal to one if the CEO seves as chairman of the board, zero otherwise	BoardEx
Board size	Number of directors on the board	BoardEx
% independent directors >50 (d)	Dummy equal to one if a majority of directors is independent, zero otherwise	BoardEx
GIM index (mod.)	Modified G-Index based on Peters & Wagner (2014)	ISS
Mean change in withheld votes (%)	Mean change in withheld votes of all board member re-election at the most recent annual meeting	ISS
Aggregate Event Sentiment (m21,m111)	Aggregate news sentiment on a company, measured over a rolling 91-day window from $t-111$ to $t-21$ with $t=0$ being the CEO turnover date. It takes into account all news sources from the Dow Jones universe	Ravenpack

Table IA.6: Propensity score estimation

This table reports the results of logistic regressions of a forced CEO turnover dummy on a set of predictors. The regressions in Columns 1 and 2 are based on Model 5 of Peters & Wagner (2014)'s first-stage regression. Columns 3 and 4 extend the Peters & Wagner (2014) model by aggregate news sentiment prior to the turnover date, and Columns 5 and 6 extend the Peters & Wagner (2014) model by the mean change in withheld votes at director re-elections of all board members prior to the turnover date. Columns 1, 3 and 5 report log-odds. Columns 2, 4 and 6 report odd ratios. Turnover firms are S&P 1500 firms that force out a CEO. Control firms are S&P 1500 firms that do not force out a CEO within five years before or after the turnover date and for which we can identify outside directors with interlocked directorships and re-election vote data at the interlocked firms. Definitions and data sources of all variables are provided in Table IA.5 in the Appendix. z-statistics are reported in parentheses. *, **, and ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent variable:	Forced turnover (d)					
	(1) Log-odds	(2) Odd ratios	(3) Log-odds	(4) Odd ratios	(5) Log-odds	(6) Odd ratios
Industry volatility in t-1	2.304	10.012	3.098	22.152	0.736	2.087
	(0.663)	(0.663)	(0.881)	(0.881)	(0.184)	(0.184)
Total assets, log	0.074	1.076	0.053	1.054	0.065	1.068
	(0.998)	(0.998)	(0.709)	(0.709)	(0.867)	(0.867)
Tobin's Q	-0.106	0.900	-0.091	0.913	-0.111	0.895
	(-1.029)	(-1.029)	(-0.888)	(-0.888)	(-0.973)	(-0.973)
Idiosyncratic return (m270,m21)	-3.004***	0.050***	-2.877***	0.056***	-3.116***	0.044***
	(-9.196)	(-9.196)	(-8.535)	(-8.535)	(-8.686)	(-8.686)
Market-adj. industry return (m270,m21)	-1.717***	0.180***	-1.538***	0.215***	-1.872***	0.154***
	(-4.023)	(-4.023)	(-3.511)	(-3.511)	(-3.749)	(-3.749)
Industry-adj. volatility in t-1	1.265	3.544	1.200	3.319	2.635	13.945
	(0.741)	(0.741)	(0.693)	(0.693)	(1.383)	(1.383)
Equity-based pay > 0 (d)	0.111	1.118	0.094	1.098	-0.137	0.872
	(0.397)	(0.397)	(0.333)	(0.333)	(-0.463)	(-0.463)
Dollar incentives, log	-0.084	0.919	-0.077	0.926	-0.099	0.906
	(-1.237)	(-1.237)	(-1.128)	(-1.128)	(-1.321)	(-1.321)

Table IA.6: Propensity score estimation (cont.)

Dependent variable:	Forced turnover (d)					
	(1) Log-odds	(2) Odd ratios	(3) Log-odds	(4) Odd ratios	(5) Log-odds	(6) Odd ratios
CEO age $\geq$ 60 (d)	-0.974***	0.378***	-0.961***	0.383***	-0.882***	0.414***
	(-3.571)	(-3.571)	(-3.521)	(-3.521)	(-3.022)	(-3.022)
CEO tenure	-0.056***	0.945***	-0.058***	0.943***	-0.067***	0.935***
	(-3.193)	(-3.193)	(-3.267)	(-3.267)	(-3.434)	(-3.434)
CEO is outsider (d)	0.689***	1.992***	0.707***	2.028***	0.761***	2.140***
	(4.325)	(4.325)	(4.423)	(4.423)	(4.223)	(4.223)
CEO is chairman (d)	-0.705***	0.494***	-0.687***	0.503***	-0.741***	0.476***
	(-3.382)	(-3.382)	(-3.260)	(-3.260)	(-3.085)	(-3.085)
Board size, log	-1.117**	0.327**	-1.003**	0.367**	-1.328**	0.265**
	(-2.450)	(-2.450)	(-2.197)	(-2.197)	(-2.520)	(-2.520)
% independent directors > 50 (d)	-0.325	0.723	-0.355	0.701	-0.169	0.844
	(-1.032)	(-1.032)	(-1.129)	(-1.129)	(-0.412)	(-0.412)
GIM index (mod.)	-0.026	0.974	-0.021	0.979	-0.022	0.978
	(-0.546)	(-0.546)	(-0.435)	(-0.435)	(-0.385)	(-0.385)
Aggregate Event Sentiment (m21,m111)			-0.008**	0.992**		
			(-2.270)	(-2.270)		
Mean change in withheld votes (%)					0.022**	1.022**
					(2.361)	(2.361)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	160,494	160,494	157,896	157,896	115,909	115,909
Firms	1,888	1,888	1,868	1,868	1,710	1,710
Pseudo R^2	0.098	0.098	0.100	0.100	0.119	0.119

Table IA.7: Covariate balancing tests with Peters & Wagner (2014)'s forced CEO turnover likelihood model

This table reports differences in firm characteristics between firms that force out a CEO and firms that do not force out a CEO. The table reports means and the number of observations as well as results from tests for differences in means between the two samples. Turnover firms are S&P 1500 firms that force out a CEO. Control firms are S&P 1500 firms that do not force out a CEO within five years before or after the turnover date and for which we can identify outside directors with interlocked directorships and re-election vote data at the interlocked firms. Panel A reports results of balancing tests for the full sample. Panel B reports results of balancing tests after matching control firms using the forced CEO turnover likelihood model of Peters & Wagner (2014). Definitions and data sources of all variables are provided in Table IA.5 in the Appendix. *, **, and ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A: Pre-matching

	Turnover firms		Control firms		Difference	
	N	Mean	N	Mean	Diff.	t-value
Industry volatility in t-1	183	0.08	160,311	0.07	0.00***	2.80
Total assets, log	183	7.89	160,311	8.32	-0.43***	-3.46
Tobin's Q	183	1.64	160,311	1.82	-0.18**	-2.43
Idiosyncratic return (m270,m21)	183	-0.28	160,311	-0.04	-0.24***	-10.69
Market-adj. industry return (m270,m21)	183	0.02	160,311	0.02	-0.00	-0.34
Industry-adj. volatility in t-1	183	0.06	160,311	0.04	0.01***	4.00
Equity-based pay $>$ 0 (d)	183	0.91	160,311	0.91	-0.00	-0.22
Dollar incentives, log	183	4.75	160,311	5.55	-0.80***	-7.51
CEO age $\geq$ 60 (d)	183	0.09	160,311	0.30	-0.21***	-6.13
CEO tenure	183	4.88	160,311	7.60	-2.72***	-5.64
CEO is outsider (d)	183	0.44	160,311	0.29	0.15***	4.47
CEO is chairman (d)	183	0.27	160,311	0.55	-0.28***	-7.49
Board size, log	183	2.18	160,311	2.26	-0.08***	-4.38
% independent directors $>$ 50 (d)	183	0.93	160,311	0.96	-0.02	-1.38
GIM index (mod.)	183	5.30	160,311	5.50	-0.21	-1.59

Table IA.7: Covariate balancing tests with Peters & Wagner (2014)'s forced CEO turnover likelihood model (cont.)

Panel B: Post-matching

	Turnover firms		Control firms		Difference	
	N	Mean	N	Mean	Diff.	t-value
Industry volatility in t-1	181	0.08	181	0.08	0.00	0.10
Total assets, log	181	7.91	181	8.04	-0.13	-0.73
Tobin's Q	181	1.65	181	1.67	-0.03	-0.30
Idiosyncratic return (m270,m21)	181	-0.28	181	-0.27	-0.01	-0.30
Market-adj. industry return (m270,m21)	181	0.02	181	0.01	0.01	0.59
Industry-adj. volatility in t-1	181	0.06	181	0.05	0.00	0.95
Equity-based pay $>$ 0 (d)	181	0.91	181	0.90	0.01	0.18
Dollar incentives, log	181	4.78	181	4.76	0.03	0.19
CEO age $\geq$ 60 (d)	181	0.09	181	0.08	0.02	0.56
CEO tenure	181	4.89	181	4.79	0.10	0.23
CEO is outsider (d)	181	0.43	181	0.40	0.03	0.53
CEO is chairman (d)	181	0.28	181	0.25	0.02	0.48
Board size, log	181	2.19	181	2.20	-0.01	-0.43
% independent directors $>$ 50 (d)	181	0.93	181	0.96	-0.02	-0.92
GIM index (mod.)	181	5.29	181	5.04	0.25	1.34

Table IA.8: Covariate balancing tests with Peters & Wagner (2014)'s forced CEO turnover likelihood model extended by aggregate news sentiment

This table reports differences in firm characteristics between firms that force out a CEO and firms that do not force out a CEO. The table reports means and the number of observations as well as results from tests for differences in means between the two samples. Turnover firms are S&P 1500 firms that force out a CEO. Control firms are S&P 1500 firms that do not force out a CEO within five years before or after the turnover date and for which we can identify outside directors with interlocked directorships and re-election vote data at the interlocked firms. Panel A reports results of balancing tests for the full sample. Panel B reports results of balancing tests after matching control firms using the forced CEO turnover likelihood model of Peters & Wagner (2014), augmented with the aggregate company news sentiment prior to the turnover date. Definitions and data sources of all variables are provided in Table IA.5 in the Appendix. *, **, and ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A: Pre-matching

	Turnover firms		Control firms		Difference	
	N	Mean	N	Mean	Diff.	t-value
Industry volatility in t-1	181	0.08	157,715	0.07	0.00***	2.97
Total assets, log	181	7.86	157,715	8.33	-0.47***	-3.73
Tobin's Q	181	1.64	157,715	1.82	-0.18**	-2.36
Idiosyncratic return (m270,m21)	181	-0.28	157,715	-0.03	-0.24***	-10.65
Market-adj. industry return (m270,m21)	181	0.02	157,715	0.02	-0.00	-0.37
Industry-adj. volatility in t-1	181	0.06	157,715	0.04	0.01***	4.03
Equity-based pay $>$ 0 (d)	181	0.91	157,715	0.91	-0.01	-0.29
Dollar incentives, log	181	4.74	157,715	5.55	-0.81***	-7.58
CEO age $\geq$ 60 (d)	181	0.09	157,715	0.30	-0.21***	-6.06
CEO tenure	181	4.89	157,715	7.61	-2.72***	-5.61
CEO is outsider (d)	181	0.44	157,715	0.29	0.16***	4.62
CEO is chairman (d)	181	0.27	157,715	0.55	-0.28***	-7.46
Board size, log	181	2.18	157,715	2.26	-0.08***	-4.36
% independent directors $>$ 50 (d)	181	0.93	157,715	0.96	-0.02	-1.50
GIM index (mod.)	181	5.32	157,715	5.51	-0.19	-1.48
Aggregate Event Sentiment (m21,m111)	181	62.35	157,715	70.13	-7.78***	-5.22

Table IA.8: Covariate balancing tests with Peters & Wagner (2014)'s forced CEO turnover likelihood model extended by aggregate news sentiment (cont.)

Panel B: Post-matching

	Turnover firms		Control firms		Difference	
	N	Mean	N	Mean	Diff.	t-value
Industry volatility in t-1	178	0.08	178	0.08	-0.00	-0.06
Total assets, log	178	7.89	178	7.64	0.26	1.35
Tobin's Q	178	1.65	178	1.65	0.01	0.09
Idiosyncratic return (m270,m21)	178	-0.27	178	-0.25	-0.02	-0.51
Market-adj. industry return (m270,m21)	178	0.02	178	0.00	0.01	0.75
Industry-adj. volatility in t-1	178	0.06	178	0.06	-0.00	-0.29
Equity-based pay > 0 (d)	178	0.90	178	0.85	0.05	1.46
Dollar incentives, log	178	4.77	178	4.75	0.03	0.17
CEO age ≥ 60 (d)	178	0.10	178	0.06	0.03	1.18
CEO tenure	178	4.91	178	5.33	-0.43	-0.95
CEO is outsider (d)	178	0.43	178	0.46	-0.03	-0.53
CEO is chairman (d)	178	0.28	178	0.29	-0.01	-0.24
Board size, log	178	2.19	178	2.16	0.03	1.11
% independent directors > 50 (d)	178	0.94	178	0.93	0.01	0.22
GIM index (mod.)	178	5.33	178	5.50	-0.17	-0.94
Aggregate Event Sentiment (m21,m111)	178	62.83	178	64.37	-1.54	-0.65

Table IA.9: Covariate balancing tests with Peters & Wagner (2014)'s forced CEO turnover likelihood model extended by aggregate board vote

This table reports differences in firm characteristics between firms that force out a CEO and firms that do not force out a CEO. The table reports means and the number of observations as well as results from tests for differences in means between the two samples. Turnover firms are S&P 1500 firms that force out a CEO. Control firms are S&P 1500 firms that do not force out a CEO within five years before or after the turnover date and for which we can identify outside directors with interlocked directorships and re-election vote data at the interlocked firms. Panel A reports results of balancing tests for the full sample. Panel B reports results of balancing tests after matching control firms using the forced CEO turnover likelihood model of Peters & Wagner (2014), augmented with the mean change in withheld votes at director re-elections of all board members prior to the turnover date. Definitions and data sources of all variables are provided in Table IA.5 in the Appendix. *, **, and ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A: Pre-matching

	Turnover firms		Control firms		Difference	
	N	Mean	N	Mean	Diff.	t-value
Industry volatility in t-1	149	0.07	115,760	0.07	0.00**	2.32
Total assets, log	149	7.90	115,760	8.42	-0.52***	-3.71
Tobin's Q	149	1.63	115,760	1.82	-0.19**	-2.29
Idiosyncratic return (m270,m21)	149	-0.27	115,760	-0.02	-0.25***	-10.14
Market-adj. industry return (m270,m21)	149	0.01	115,760	0.02	-0.01	-0.73
Industry-adj. volatility in t-1	149	0.06	115,760	0.04	0.02***	5.02
Equity-based pay > 0 (d)	149	0.89	115,760	0.92	-0.03	-1.43
Dollar incentives, log	149	4.69	115,760	5.53	-0.84***	-7.23
CEO age ≥ 60 (d)	149	0.10	115,760	0.31	-0.21***	-5.54
CEO tenure	149	4.92	115,760	7.74	-2.82***	-5.33
CEO is outsider (d)	149	0.46	115,760	0.28	0.18***	4.88
CEO is chairman (d)	149	0.25	115,760	0.53	-0.28***	-6.90
Board size, log	149	2.17	115,760	2.26	-0.09***	-4.87
% independent directors > 50 (d)	149	0.95	115,760	0.97	-0.01	-0.84
GIM index (mod.)	149	5.33	115,760	5.59	-0.26*	-1.85
Mean change in withheld votes (%)	149	2.86	115,760	0.84	2.03***	3.14

Table IA.9: Covariate balancing tests with Peters & Wagner (2014)'s forced CEO turnover likelihood model extended by aggregate board vote (cont.)

Panel B: Post-matching

	Turnover firms		Control firms		Difference	
	N	Mean	N	Mean	Diff.	t-value
Industry volatility in t-1	147	0.07	147	0.08	-0.00	-0.92
Total assets, log	147	7.93	147	7.69	0.24	1.25
Tobin's Q	147	1.63	147	1.69	-0.06	-0.59
Idiosyncratic return (m270,m21)	147	-0.27	147	-0.26	-0.01	-0.23
Market-adj. industry return (m270,m21)	147	0.01	147	0.02	-0.00	-0.15
Industry-adj. volatility in t-1	147	0.06	147	0.06	-0.00	-0.07
Equity-based pay > 0 (d)	147	0.89	147	0.90	-0.01	-0.38
Dollar incentives, log	147	4.71	147	4.74	-0.02	-0.16
CEO age ≥ 60 (d)	147	0.10	147	0.08	0.02	0.60
CEO tenure	147	4.90	147	4.63	0.27	0.64
CEO is outsider (d)	147	0.45	147	0.46	-0.01	-0.12
CEO is chairman (d)	147	0.25	147	0.26	-0.01	-0.13
Board size, log	147	2.18	147	2.16	0.02	0.70
% independent directors > 50 (d)	147	0.95	147	0.97	-0.02	-0.92
GIM index (mod.)	147	5.32	147	5.45	-0.13	-0.61
Mean change in withheld votes (%)	147	2.71	147	2.47	0.24	0.26

Table IA.10: Forced CEO turnovers and the cumulative market capitalization of all board seats

This table reports results from ordinary least squares regressions of the change in the in the cumulative market capitalization of all firms, where a director holds board seats t years after a forced CEO turnover, on a treatment dummy variable set equal to one if a director is involved in the forced CEO turnover and control variables. In Panel B, the turnover firms are omitted when constructing the dependent variable. The dependent variable is transformed using the inverse hyperbolic sine transformation. All regressions include turnover event fixed effects. The regressions in Columns 2 and 4 additionally include interlocked firm fixed effects. The treatment sample comprises outside directorships of directors who are interlocked to forced CEO turnovers between 2003 and 2017. The control sample comprises outside directorships of directors who are not interlocked to a forced turnover but share a board seat with a director interlocked to a forced CEO turnover at the turnover announcement. The control variables are the same as in Table 10. t-statistics are reported in parentheses and are based on standard errors clustered at the turnover level. *, **, and ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A: The change in the sum of market capitalization of all board seats

Dependent variable:	Difference in the sum of market cap. over all directorships			
	(1) $t_1 - t_0$	(2) $t_1 - t_0$	(3) $t_5 - t_0$	(4) $t_5 - t_0$
Forced interlocked (d)	-1.163***	-1.186***	-1.808***	-1.817***
	(-4.210)	(-4.044)	(-4.214)	(-4.029)
Director control variables	Yes	Yes	Yes	Yes
Turnover FE	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	Yes
Observations	4,013	4,013	3,300	3,300
Firms	465	465	404	404
Directors	2,885	2,885	2,425	2,425
Turnover-interlocked directorships	594	594	489	489
Adjusted R^2	0.032	0.036	0.127	0.131

Panel B: The change in the sum of market capitalization of all board seats (w/o turnover firm)

Dependent variable:	Difference in the sum of market cap. over all directorships			
	(1) $t_1 - t_0$	(2) $t_1 - t_0$	(3) $t_5 - t_0$	(4) $t_5 - t_0$
Forced interlocked (d)	-0.169	-0.167	0.472	0.474
	(-0.712)	(-0.675)	(1.037)	(0.985)
Director control variables	Yes	Yes	Yes	Yes
Turnover FE	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	Yes
Observations	4,013	4,013	3,300	3,300
Firms	465	465	404	404
Directors	2,885	2,885	2,425	2,425
Turnover-interlocked directorships	594	594	489	489
Adjusted R^2	0.018	0.020	0.113	0.116

Table IA.11: Forced CEO turnovers and losses of directorships: Cross-sectional tests

This table reports results from ordinary least squares regressions of a dummy variable indicating a loss of directorships within 5 years after the turnover on a treatment dummy variable set equal to one if a director is involved in a forced CEO turnover and control variables. In Panel B, the turnover firms are omitted when constructing the dependent variable. All regressions include turnover event and interlocked firm fixed effects. The treatment sample comprises outside directorships of directors who are interlocked to forced CEO turnovers between 2003 and 2017. The control sample comprises outside directorships of directors who are not interlocked to a forced turnover but share a board seat with a director interlocked to a forced CEO turnover at the turnover announcement. t-statistics are reported in parentheses and are based on standard errors clustered at the turnover level. *, **, and ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A: Including turnover firm

Dependent variable:	Loses directorships within t years (d)				
	(1) t=[0,5]	(2) t=[0,5]	(3) t=[0,5]	(4) t=[0,5]	(5) t=[0,5]
Forced w. full replacement (d)	0.066				
	(1.377)				
Forced w/o full replacement (d)	0.122***				
	(2.694)				
Forced performance-induced (d)		0.113***			
		(3.024)			
Forced non-performance-induced (d)		0.001			
		(0.014)			
Forced during Honeymoon stage (d)			0.063		
			(1.148)		
Forced during Harvest stage (d)			0.114***		
			(2.746)		
Forced during Decline stage (d)			-0.173		
			(-0.554)		
Forced/committee member (d)				0.093**	
				(2.435)	
Forced/not committee member (d)				0.100	
				(1.475)	
Forced/co-opted director (d)					0.100**
					(2.113)
Forced/non-co-opted director (d)					0.093**
					(2.043)
Director control variables	Yes	Yes	Yes	Yes	Yes
Turnover FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Observations	3,300	3,191	3,300	3,292	3,300
Firms	404	391	404	402	404
Directors	2,425	2,345	2,425	2,418	2,425
Turnover-interlocked directorships	489	473	489	488	489
Adjusted R^2	0.092	0.091	0.092	0.091	0.091

Table IA.11: Forced CEO turnovers and losses of directorships: Cross-sectional tests (cont.)

Panel B: Excluding turnover firm

Dependent variable:	Loses directorships within t years (d)				
	(1) t=[0,5]	(2) t=[0,5]	(3) t=[0,5]	(4) t=[0,5]	(5) t=[0,5]
Forced w. full replacement (d)	-0.043				
	(-0.919)				
Forced w/o full replacement (d)	0.007				
	(0.166)				
Forced performance-induced (d)		-0.012			
		(-0.337)			
Forced non-performance-induced (d)		-0.020			
		(-0.219)			
Forced during Honeymoon stage (d)			-0.006		
			(-0.112)		
Forced during Harvest stage (d)			-0.015		
			(-0.398)		
Forced during Decline stage (d)			-0.380*		
			(-1.738)		
Forced/committee member (d)				-0.018	
				(-0.477)	
Forced/not committee member (d)				-0.012	
				(-0.193)	
Forced/co-opted director (d)					-0.029
					(-0.608)
Forced/non-co-opted director (d)					-0.002
					(-0.057)
Director control variables	Yes	Yes	Yes	Yes	Yes
Turnover FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Observations	3,300	3,191	3,300	3,292	3,300
Firms	404	391	404	402	404
Directors	2,425	2,345	2,425	2,418	2,425
Turnover-interlocked directorships	489	473	489	488	489
Adjusted R^2	0.084	0.082	0.084	0.083	0.084

Table IA.12: Propensity score matching analysis: Left before re-election

This table reports results from ordinary least squares regressions of withheld votes in director re-elections on an indicator set equal to one if a director is involved in a forced CEO turnover at an interlocked firm, an indicator variable set equal to one if a director leaves the turnover/matched control firm before the next re-election at the interlocked firm, an interaction term between these two variables, and control variables. The regression in Column 2 includes year and interlocked firm industry fixed effects. The regression in Column 3 includes turnover event fixed effects. The treatment sample comprises re-elections of directors involved in a forced CEO turnover at an interlocked firm. The control sample comprises re-elections of directors interlocked to a sample of propensity score matched firms with characteristics similar to the turnover firms. The sample is based on propensity scores estimated using the forced CEO turnover likelihood model of Peters & Wagner (2014). For details, see Section 4.2. t-statistics are reported in parentheses and are based on standard errors clustered at the Fama-French 48 industry level. *, **, and ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent variable:	% votes withheld		
	(1)	(2)	(3)
Forced interlocked (d)	1.454**	1.522***	1.053*
	(2.586)	(2.822)	(1.793)
Loses board seat (d)	-0.874	-0.398	-0.667
	(-0.882)	(-0.293)	(-0.581)
Forced × Loses board seat (d)	0.212	-0.701	-0.251
	(0.158)	(-0.441)	(-0.157)
ISS withhold/against (d)	19.501***	19.324***	19.695***
	(10.438)	(10.095)	(10.239)
Female (d)	-0.507	-0.358	-0.402
	(-0.760)	(-0.556)	(-0.739)
Age (yr)	-0.016	-0.036	-0.037
	(-0.330)	(-0.752)	(-0.987)
Tenure (yrs), log	1.491***	1.311***	1.788***
	(3.962)	(3.690)	(5.260)
# of other board seats	-0.048	0.030	0.024
	(-0.294)	(0.236)	(0.132)

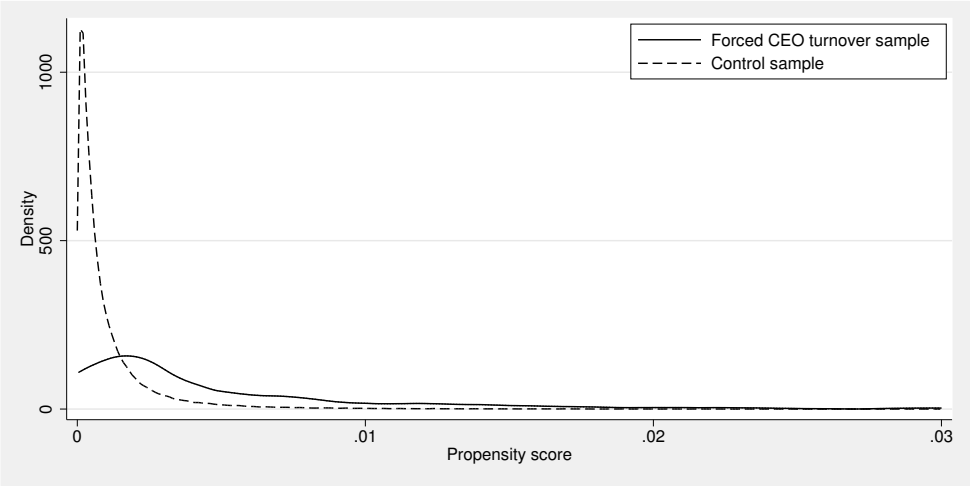
Table IA.12: Propensity score matching analysis: Left before re-election (cont.)

Dependent variable:	% votes withheld		
	(1)	(2)	(3)
Board size	-0.106	-0.032	-0.122
	(-0.716)	(-0.243)	(-0.763)
% outside directors	6.869*	-0.299	4.528
	(1.695)	(-0.066)	(1.231)
% busy outside directors	0.059	0.355	0.692
	(0.045)	(0.286)	(0.453)
Institutional ownership (%)	0.569	0.131	-0.507
	(0.491)	(0.116)	(-0.483)
Total assets (millions), log	0.130	0.363	0.005
	(0.515)	(1.321)	(0.019)
Tobin's Q	0.044	0.155	0.052
	(0.190)	(0.650)	(0.244)
ROA	-2.458	-4.078**	-1.309
	(-0.936)	(-2.093)	(-0.651)
BH return (m270,m21)	-1.964***	-1.643***	-2.042***
	(-3.142)	(-2.963)	(-3.244)
Year FE	No	Yes	No
Industry FE	No	Yes	No
Turnover FE	No	No	Yes
Observations	1,111	1,111	1,111
Firms	745	745	745
Directors	762	762	762
Turnover-interlocked directorships	525	525	525
Adjusted R^2	0.283	0.302	0.304

Figure IA.1: Common support with Peters & Wagner (2014)'s forced CEO turnover likelihood model

These figures plot the density distribution of the propensity scores to force out a CEO for the turnover firm sample and the matched control sample. Panel A plots the density distribution pre-matching. Control observations with a higher propensity than the maximum propensity of the turnover sample are dropped from the figure to increase legibility. Panel B plots the density distribution after applying a one-to-one nearest-neighbor matching algorithm with a maximum caliper width of 0.2 times the pooled standard deviation of the logarithm of the propensity scores. The sample in Panel A consists of 183 turnover firm observations and 160,311 potential control firm observations. The sample in Panel B consists of 181 turnover firm observations and 181 matched control firm observations. Turnover firms are S&P 1500 firms that force out a CEO. Control firms are S&P 1500 firms that do not force out a CEO within five years before or after the turnover date and for which we can identify outside directors with interlocked directorships and re-election vote data at the interlocked firms. The propensity scores are estimated using the forced CEO turnover likelihood model of Peters & Wagner (2014).

Panel A: Propensity score distribution, pre-matching



Panel B: Propensity score distribution, post-matching

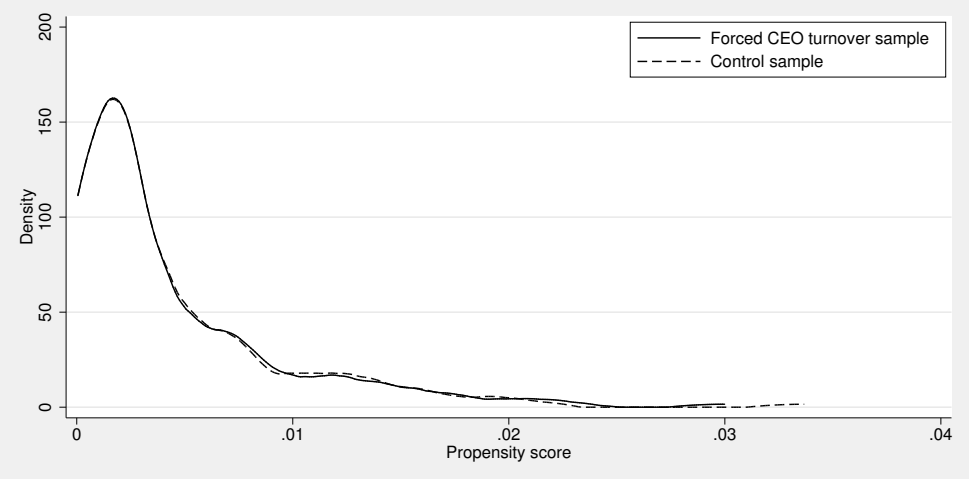
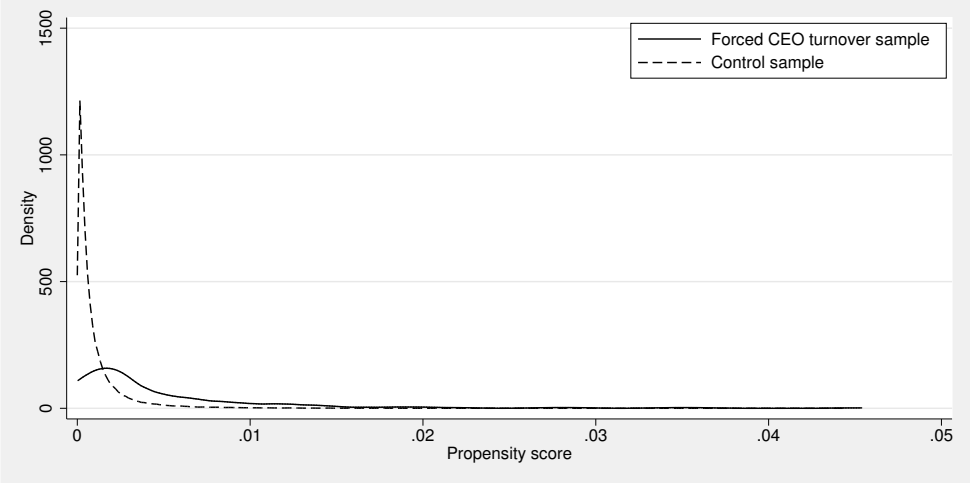


Figure IA.2: Common support with Peters & Wagner (2014)'s forced CEO turnover likelihood model extended by aggregate news sentiment

These figures plot the density distribution of the propensity scores to force out a CEO for the turnover firm sample and the matched control sample. Panel A plots the density distribution pre-matching. Control observations with a higher propensity than the maximum propensity of the turnover sample are dropped from the figure to increase legibility. Panel B plots the density distribution after applying a one-to-one nearest-neighbor matching algorithm with a maximum caliper width of 0.2 times the pooled standard deviation of the logarithm of the propensity scores. The sample in Panel A consists of 183 turnover firm observations and 160,311 potential control firm observations. The sample in Panel B consists of 181 turnover firm observations and 181 matched control firm observations. Turnover firms are S&P 1500 firms that force out a CEO. Control firms are S&P 1500 firms that do not force out a CEO within five years before or after the turnover date and for which we can identify outside directors with interlocked directorships and re-election vote data at the interlocked firms. The propensity scores are estimated using the forced CEO turnover likelihood model of Peters & Wagner (2014), augmented with the aggregate company news sentiment prior to the turnover date.

Panel A: Propensity score distribution, pre-matching



Panel B: Propensity score distribution, post-matching

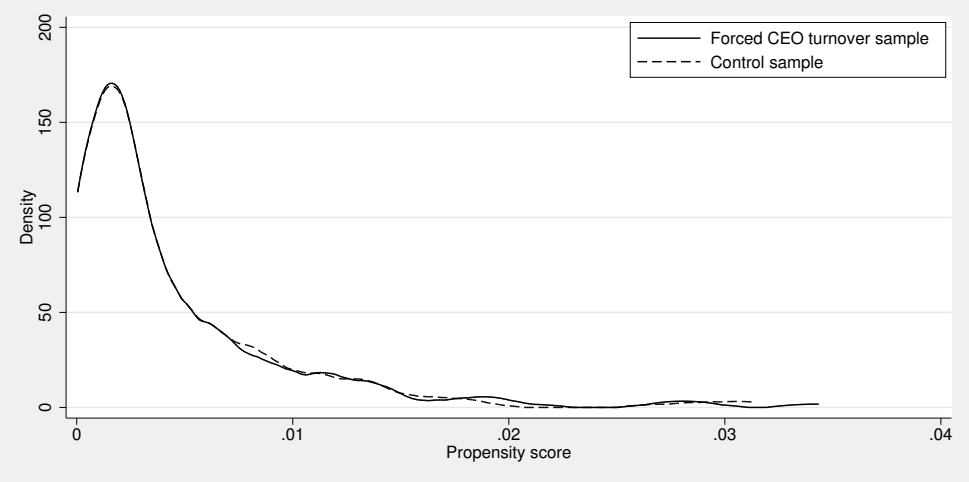
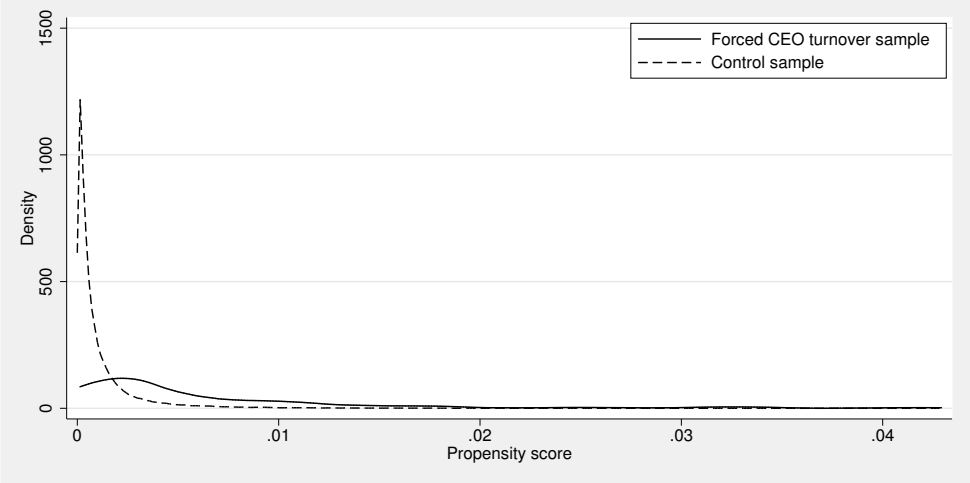


Figure IA.3: Common support with Peters & Wagner (2014)'s forced CEO turnover likelihood model extended by aggregate board vote

These figures plot the density distribution of the propensity scores to force out a CEO for the turnover firm sample and the matched control sample. Panel A plots the density distribution pre-matching. Control observations with a higher propensity than the maximum propensity of the turnover sample are dropped from the figure to increase legibility. Panel B plots the density distribution after applying a one-to-one nearest-neighbor matching algorithm with a maximum caliper width of 0.2 times the pooled standard deviation of the logarithm of the propensity scores. The sample in Panel A consists of 183 turnover firm observations and 160,311 potential control firm observations. The sample in Panel B consists of 181 turnover firm observations and 181 matched control firm observations. Turnover firms are S&P 1500 firms that force out a CEO. Control firms are S&P 1500 firms that do not force out a CEO within five years before or after the turnover date and for which we can identify outside directors with interlocked directorships and re-election vote data at the interlocked firms. The propensity scores are estimated using the forced CEO turnover likelihood model of Peters & Wagner (2014), augmented with the mean change in withheld votes at director re-elections of all board members prior to the matching date.

Panel A: Propensity score distribution, pre-matching



Panel B: Propensity score distribution, post-matching