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ABSTRACT

This paper analyzes the reputational effects of forced CEO turnovers on outside directors. We find that 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. Directors are not penalized for an involvement in a turnover per se but for forced CEO turnovers that are related to governance failures by the board. Our results challenge the widespread view that forcing out a CEO can generally be understood as a sign of a well-functioning corporate governance.

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1. Introduction

Monitoring and, if necessary, firing a CEO is one of the most important decisions made by corporate boards (e.g., Fama, 1980; Fama and Jensen, 1983; Jenter and Kanaan, 2015; Jenter and Lewellen, 2021). However, evidence on the reputational consequences of forced CEO turnovers on involved directors is scarce. Moreover, the existing literature has conflicting views on the direction of such reputational consequences, which reflect conflicting views on the corporate governance signal transmitted by forced CEO turnovers. 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. 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) and theoretical work.² Under this alternative view, directors involved in a forced turnover are expected to lose reputation. In this paper, we set out to empirically investigate whether directors gain or lose reputation from forcing out a CEO. Unambiguous evidence on the direction of reputational effects derived from such an analysis allows us to paint a clearer picture of the corporate governance signal transmitted by a forced CEO turnover.

A test of the reputational effect of forced CEO turnovers on involved directors is subject to two major empirical challenges. First, turnover decisions are endogenous and often related to company performance (Fee et al., 2013). Second, widely used measures of director reputation, such as gains and losses of board memberships, are subject to endogenous selection by directors (Levit and Malenko, 2016). To address the first major endogeneity concern, we study directors with multiple directorships who force out a CEO at one firm (which we refer to as “turnover firm”) and observe director-level reputation measures at the other firms on whose boards these directors sit (“interlocked firms”). These interlocked firms, and the directors sitting on their boards, are largely unaffected by the characteristics of the turnover firm, including factors that led to the forced turnover. Hence, this setting allows us to isolate the impact of a forced CEO turnover on director reputation from company-level factors at the turnover firm. To address the second major endogeneity concern, we employ the change in the percentage of withheld votes (defined as the sum of votes withheld and votes against, divided by the total number of votes cast) in director re-elections as our primary measure of director reputation. Voting results reflect shareholders’ and not directors’ decisions. Hence, withheld votes constitute a direct measure of shareholder satisfaction regarding individual directors that is not subject to endogenous selection by directors. 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 and Srinivasan, 2014; Aggarwal et al., 2019; Liu et al., 2020; Erel et al., 2021).³

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 between two consecutive director elections on a treatment dummy variable indicating whether a director was involved in a forced CEO turnover at another firm between the two election dates as well as firm-level and director-level control variables. We first-difference outcome and control variables at the director-firm level. This approach ensures that time-invariant director characteristics, such as talent, and firm characteristics, such as corporate culture, do not influence our estimates.⁴ 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 forced CEO turnover.

Our results show that directors involved in a forced CEO turnover experience a significant increase in withheld votes at their subsequent re-election at interlocked firms compared to directors not interlocked to a forced turnover in the same industry and year. Director- and company-level control variables and different sets of fixed effects leave our results largely unchanged, suggesting that controlling for various time-variant and time-invariant characteristics does not affect our findings. Hence, forced turnovers do not appear to systematically coincide with other changes in director and firm characteristics at turnover-interlocked firms, providing support for the conjecture that forced turnovers represent exogenous shocks on interlocked directors’ vote outcomes. Overall, our finding of an increase in withheld votes associated with involvement in a forced CEO turnover is consistent with directors suffering a reputational loss, which challenges the predominant view that forcing out a CEO is a sign of well-functioning corporate governance at the board level.

In economic terms, we find that turnover-interlocked outside directors experience a 1.20 percentage point increase in withheld votes. While this increase may appear modest, it is important to note that vote support for directors is generally high, with the average of withheld votes in our sample amounting to 6.1%. Hence, an increase of 1.20 percentage points represents a sizeable increase of 19.6% over the sample mean. Moreover, recent research shows that modest increases in withheld votes often result in negative consequences for directors (e.g., Aggarwal et al., 2019). To put the economic magnitude of the documented effect into perspective, we conduct two additional analyses. First, we compare the increase in withheld votes resulting from a forced CEO turnover at an interlocked firm to the increase in withheld votes resulting from other corporate events at an interlocked firm. We

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

² 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 and Hashimoto (2021) show that while aggressive boards facilitate truthful communication between the CEO and the board, they tend to dismiss talented managers, resulting in costs to shareholders. The model of Adams and Ferreira (2007) suggests that it may be beneficial for shareholders to elect a board that is friendly toward the CEO and does not monitor her too closely and/or impose a significant threat of replacement. Hence, a forced turnover may signal an aggressive board to shareholders, one that may eventually turn out to be detrimental to shareholder value.

³ For instance, Aggarwal et al. (2019) show that an increase in withheld votes in uncontested director elections leads to higher director turnover, committee demotions, and reduced opportunities in the director labor market.

⁴ As we discuss below, our setting is akin to a staggered difference-in-differences setting in levels that includes two-way fixed effects (subject and time) but can easily accommodate repeated treatments (e.g., Heider and Ljungqvist, 2015). As we show below, our setting is unlikely to suffer from the “bad comparison” problem recently discussed in the literature (e.g., Callaway and Sant’Anna, 2021; Sun and Abraham, 2021; Baker et al., 2022).

consider four alternative events that have been shown to hurt director reputation, including restatements (Srinivasan, 2005), class action lawsuits (Fich and Shivdasani, 2007), poison pill adoptions (Johnson et al., 2023), and bankruptcy filings (Gow et al., 2018). The increase in withheld votes associated with firing a CEO is larger than the increase in withheld votes resulting from the other reputational events and is only exceeded by a bankruptcy filing, arguably the most detrimental event for corporate shareholders. Still, the increase in withheld votes following a forced CEO turnover amounts to approximately half the increase resulting from a bankruptcy. Thus, these results further support our assertion that the documented increase in withheld votes following forced CEO turnovers is sizable. Second, we test whether forced turnovers have the potential to result in unusually high levels of withheld votes at interlocked firms, defined as at least 15% of votes cast (Bach and Metzger, 2017). We find that the likelihood of experiencing very high levels of withheld votes at an interlocked firm increases significantly following a forced CEO turnover, again confirming that such turnovers have a sizeable impact on interlocked directors' withheld votes.

We also test whether reputational losses are confined to certain types of turnovers or depend on directors' involvement in the turnover. Results from these tests show that directors are not penalized for all turnovers, but for forced CEO turnovers that appear to be related to governance failures by the directors. Specifically, directors are penalized for reactive forced turnovers, firings during the most productive period of a CEO's tenure, poor monitoring of the CEO, and the lack of a successor to the outgoing CEO. In contrast, directors are not penalized for unforced turnovers or for hiring the wrong CEO in the first place. Importantly, we uncover no sub-sample for which we find significant reputational gains from an involvement in a forced CEO turnover. Taken together, these results suggest that a forced CEO turnover is not a credible signal of a board's monitoring ability. In contrast, they suggest that firing the CEO is often indicative of governance failure at the board level.

We also investigate which shareholders penalize directors for involvement in forced CEO turnovers. Systematically penalizing directors for their actions across all board mandates requires shareholders to observe and evaluate directors' actions *and* to observe interlocked directorships. We hypothesize that institutional investors with significant ownership stakes in both the turnover and interlocked firms are most likely to fulfill these requirements. Our results confirm this conjecture: We find the negative vote effect to be concentrated in director re-elections in which institutional investors hold above-average ownership stakes in both the turnover and the interlocked firms.

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 forced turnovers, supporting the notion 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 reversal, suggesting that the reputational loss is indeed driven by the turnover and is persistent. In a placebo test, we show that directors joining the board of the turnover firm after a forced departure but before their next re-election at the interlocked firm do not experience an increase in withheld votes. This finding again supports our previous results suggesting that directors lose reputation for an involvement in a forced turnover, not for sitting on the board of a firm that recently fired the CEO.

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 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), investors might simply penalize directors for the poor firm performance, which resulted in the

forced CEO turnover, rather than for the turnover. To distinguish between these two possible interpretations, we conduct a propensity score matching analysis. In this analysis, we match firms with a forced CEO turnover to firms without a forced turnover but the same propensity to force out a CEO. We estimate the propensity of a forced CEO turnover using the model of Peters and Wagner (2014) that includes various company performance metrics and other firm, CEO, governance, and industry characteristics. Our results show that turnover-interlocked directors receive significantly more withheld votes than directors interlocked to matched firms without a forced turnover but otherwise similar characteristics. The economic magnitude of this negative vote effect is virtually identical to our baseline estimates, suggesting that the performance of the turnover firms does not constitute an omitted variable that drives our results.

A related concern is that there is an unknown – and thus omitted – reason why, in two firms with equally bad performance, one fires the CEO and the other does not. This unknown reason, in turn, might affect investors' inference about director ability. For instance, if a management failure drives bad performance, this failure may cause both a forced 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 forced CEO turnover nor convey any signal about director ability. To address this concern, we extend the set of covariates in our propensity score estimation by a newspaper-based sentiment index. As mistakes of the leadership team can be expected to trigger more negative newspaper coverage than bad luck, accounting for news sentiment can be expected to produce a sample that is balanced in terms of management's involvement in the negative events that eventually lead to bad performance. Our results show that differences in newspaper sentiment, and thus a potential omitted variable related to management failures prior to the forced CEO turnover, 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. Turnover-interlocked directors may thus divert their attention away from the interlocked firms (e.g., Masulis and Zhang, 2019; Stein and Zhao, 2019), resulting in a negative assessment by shareholders. We conduct two 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 represent CEO departures that are outside of directors' control and thus not expected to affect director 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. Our results show that sudden CEO deaths do not affect directors' re-election results at interlocked firms. Second, we find no evidence of a decline in board meeting attendance of directors interlocked to a forced CEO turnover. Hence, we find no evidence supporting a distraction-based explanation.

In the final part of the paper, we follow prior research (Farrell and 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 and Whidbee, 2000; Ertimur et al., 2012; Levit and Malenko, 2016; Fahlenbrach et al., 2017; Masulis and Zhang, 2019; Ellis et al., 2021). To analyze director labor market outcomes empirically, we track our sample of turnover-interlocked directors and their board seats over the years following the forced CEO turnovers and compare them to other directors sitting on the board of the same interlocked firms. At the extensive margin, we find that directors involved in a forced CEO turnover have a higher propensity to leave the director labor market. At the intensive margin, we document that

directors lose board seats five years after the turnover, with initially lost directorships, primarily at the turnover firm, being offset by newly acquired board seats in the subsequent four years. However, such newly acquired board seats are at smaller firms. Overall, these results suggest that reputational losses from involvement in a forced CEO turnover adversely affect director labor market opportunities.⁵

Our paper contributes to three strands of research. First, our paper adds to the literature on forced CEO turnovers. Many studies resort to forced CEO turnovers as an outcome variable when analyzing the benefits of good corporate governance (e.g., Weisbach, 1988; Farrell and 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 and 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 such turnovers often follow periods of poor company performance. While shareholders may greet the eventual decision to fire a poorly performing CEO, their assessment 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 turnover decisions. Our results show that forced CEO turnovers are associated with significant reputational losses for involved directors. This finding contradicts the general presumption in the extant empirical finance literature that forced CEO turnovers are a credible signal of boards' monitoring ability and indicate well-functioning corporate governance. Instead, our findings suggest that forced CEO turnovers are often perceived as a signal of poor monitoring by the board and, thus, may indicate a governance failure that becomes visible upon the announcement of a forced turnover.

The existing empirical literature on the consequences of forced CEO turnovers for involved directors is very limited and produces ambiguous findings. Farrell and Whidbee (2000) find that outside directors have a higher likelihood of leaving the turnover firm following a CEO dismissal but directors remaining with the turnover firm have a higher likelihood of gaining additional board seats. In contrast, Ellis et al. (2021) investigate directors' learning from forced turnovers and find that directors involved in forced CEO turnovers, while showing some learning, do not gain other board positions following CEO dismissals. One potential reason for these ambiguous results is that both studies use future directorships as an outcome variable, which are a noisy proxy for director reputation and subject to endogeneity concerns, as explained above. We add to this sparse literature on the consequences for directors from firing a CEO by establishing an unambiguous negative link between forced turnovers and shareholder satisfaction as measured through director re-election outcomes at interlocked firms, an arguably cleaner and more granular measure of director reputation. Our results also suggest that the reputational losses that we document dominate the learning benefits found in Ellis et al. (2021).

Second, we contribute to the literature on director voting. This literature uses vote outcomes as a measure of shareholder satisfaction and director reputation (e.g., Ertimur et al., 2012; Brochet and Srinivasan, 2014; Liu et al., 2020; Erel et al., 2021). Results of recent studies,

such as (Aggarwal et al., 2019), suggest that even modest increases in withheld votes lead to higher director turnover, committee demotions, and reduced opportunities in the labor market. We contribute to this literature by establishing that forced CEO turnovers significantly affect vote outcomes. In fact, we show that forced turnovers have the second largest effect on vote outcomes among a set of corporate events that have been shown to hurt director reputation and are only exceeded by bankruptcy filings, arguably the most detrimental event for corporate shareholders.

Finally, our paper relates to the literature on board interlocks. Prior research shows that such board interlocks can propagate firm policies (Davis, 1991; Bizjak et al., 2009; Stuart and Yim, 2010; Bouwman, 2011; Chiu et al., 2013; Brown and Drake, 2014; Gopalan et al., 2021; Zhang, 2021; Foroughi et al., 2022). We extend this literature by showing that actions taken by directors at one firm affect shareholders' assessment of these directors at interlocked firms.⁶ Showing that directors are held accountable for actions taken at other firms is important because it enables us to provide direct evidence on the disciplining effect of the director labor market, as suggested by Fama (1980) and Fama and Jensen (1983).

2. Sample and data

2.1. Sample selection

To compile our sample of CEO turnovers, we first identify 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 determine the exact departure announcement date, the name of the replacement, whether the replacement was announced jointly with the departure, and the circumstances of the departure. We drop departures that result 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 departures that result from proxy contests, government interventions, and other types of active monitoring by parties other than the board of directors. Additionally, we remove departures for which we cannot find sufficient board meeting data in BoardEx and ISS. This leaves us with a sample of 1773 CEO departures involving 1739 CEOs at 1266 turnover firms.

We follow previous literature in classifying CEO turnovers as either "forced" or "unforced" (e.g., Parrino, 1997; Parrino et al., 2003; Peters and Wagner, 2014; Jenter and Kanaan, 2015). We classify a 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) articles do not indicate that the CEO left due to poor health or acceptance of another position (elsewhere or within the firm) or (ii) 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 at the turnover firm: We classify a 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. The reason is that, after applying (Parrino,

⁵ 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.

⁶ In a contemporaneous paper, Johnson et al. (2023) show that directors involved in the adoption of a poison pill experience a decrease in 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.

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 suggests that the departure is not forced but consensual. The relatively short cut-off of one month ensures that our algorithm captures forced CEO turnovers only, and thus keeps the number of false positives low.⁷ This results in 283 (16%) turnovers being classified as forced.⁸

Next, we identify all outside directors who serve on the boards of the 283 turnover firms at the forced CEO departure announcement date and their interlocked directorships using BoardEx.⁹ We classify a director as turnover-interlocked if she serves as an outside director on the board of another firm besides the turnover firm at the turnover announcement date.

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).¹⁰ We collect data on director re-elections from ISS Voting Analytics, which encompasses shareholder votes of Russell 3000 firms from January 2003 onward. Our sample ends in December 2017. We only consider regular director elections, which make up for 96.33% of director elections in ISS, and exclude, for instance, elections at special meetings and contested elections. Computing the change in withheld votes from one shareholder meeting to the next requires two consecutive elections per director-firm pair.¹¹

We match directors in ISS Voting Analytics and BoardEx. This merged dataset allows us to classify director re-elections as either treated or control observations. Specifically, the treatment group comprises re-elections of directors who were involved in a forced CEO turnover at another firm between this re-election date and the previous election date, while the control group comprises re-elections of directors who were not involved in a forced CEO turnover at another firm between this re-election date and the previous election date. 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 and utility firms (SIC codes 6000–6999 and 4900–4999, respectively). Finally, we drop the first re-elections of all directors at the turnover firms following the forced CEO turnovers and the first re-elections of the departing CEOs at other firms following the turnovers.^{12,13}

⁷ This additional criterion changes 128 CEO turnovers, classified as “forced” using Parrino (1997)’s algorithm, to “unforced”.

⁸ Figure IA.1 in the Internet Appendix displays the distribution of turnovers as well as forced turnovers over time. The number of (forced) turnovers is fairly evenly distributed across sample years. Hence, our results are unlikely to be driven by market-wide spikes in forced CEO turnovers.

⁹ 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 and 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.

¹⁰ 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.

¹¹ About one-fifth of re-elections in our sample 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 or closer than one quarter from the previous election. In Section 4.1, we show that our baseline findings are not sensitive to dropping re-elections of directors at firms with staggered boards. We also show that the reputational effect is independent of the time between the turnover announcement and the re-election date.

Table 1

Forced CEO turnover and interlocked director characteristics at turnover firms.

Panel A: Forced turnover characteristics				
	Forced CEO turnovers		Interlocked directorships	
	Mean	N	Mean	N
Successor announced (d)	0.451	206	0.448	607
Performance-induced (d)	0.876	201	0.895	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

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 2003 and 2017 is classified as a forced 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 are available. Definitions and data sources of all variables are provided in Table A.1.

After applying these filters, the final sample includes 88,406 director re-elections of 18,693 individual outside directors at 3,269 firms. The treatment sample includes 607 director re-elections that concern directors interlocked to 206 forced CEO turnovers. The remaining 87,799 director re-elections concern non-turnover-interlocked directors and thus are eligible for the control sample.

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 forced turnovers. In 45% of turnover announcements (or 45% of interlocked directorships), firms announce the appointment of a full replacement to the outgoing CEO jointly with the departure. In 88% of turnovers (or 90% of interlocked directorships), we define the turnover as performance-induced using the model of Jenter and Lewellen (2021).¹⁴ Following Hambrick and Fukutomi (1991) and Brochet et al. (2021), we categorize forced CEO turnovers based

¹² Table IA.1 in the Internet Appendix illustrates the reduction in CEO turnovers and turnover-interlocked directorships for each of the seven filters applied to the initial sample of 283 forced CEO turnovers and 1,787 turnover-interlocked directorships, resulting in the final sample of 206 forced CEO turnovers and 607 turnover-interlocked directorships.

¹³ There are only 238 (0.27%) director re-elections of previously fired CEOs in the control sample used in our main analysis. To address concerns that including previously fired CEOs in the control group affects our results, we drop these re-elections from the control sample and replicate our main analysis. Results are virtually identical (as shown in Table IA.2 in the Internet Appendix).

¹⁴ We classify a forced CEO turnover as performance-induced if the implied probability from Jenter and Lewellen (2021)’s two-probit model is above 50%. The results of the two-probit estimation are reported in Table IA.4 in the Internet Appendix. The percentage of performance-induced forced CEO turnovers is consistent with Jenter and Lewellen (2021), who find 82% of forced CEO turnovers to be performance-induced (see their Table 5, Panel A).

Table 2

Turnover-interlocked director and firm characteristics.

<i>Panel A: Turnover-interlocked director characteristics</i>				
	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
<i>Panel B: Turnover-interlocked director characteristics at interlocked firms</i>				
	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
<i>Panel C: Turnover-interlocked firm characteristics</i>				
	Mean	Median	SD	N
Total assets (millions)	14,662.478	2,532.490	56,107.593	607
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

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 2003 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 are available. Definitions and data sources of all variables are provided in Table A.1.

on the timing of the turnover within a CEO's tenure. Around 32% of departures (or 31% of interlocked directorships) occur during the first three years of a CEO's tenure (the "honeymoon period"), 66% (or 69% of interlocked directorships) during years three to 13 of tenure (the "harvest stage"), and 2% (or 1% of interlocked directorships) after more than 13 years of tenure (the "decline stage").

Panel B reports descriptive statistics on the 607 interlocked directors at the turnover firms as of the date of the CEO departure announcement. About 50% of turnover-interlocked directors are "co-opted", i.e., were appointed under the departing CEO (Coles et al., 2014). Almost 80% are members of either the nominating or the compensation committee, making them more responsible for monitoring CEOs (e.g., Chhaochharia and Grinstein, 2009; Guo and Masulis, 2015).

Table 2 reports summary statistics on turnover-interlocked directors and interlocked firms as of the date of the first re-election following the forced CEO turnover. This sample constitutes the treatment group in our analysis. Panel A reports summary statistics on the 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 besides the one at the interlocked firm but including the one at the turnover firm. Panel B reports interlocked director characteristics at the interlocked 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% return on assets, 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. While many differences across treatment

Table 3

Non-turnover-interlocked director and firm characteristics.

<i>Panel A: Non-turnover-interlocked director characteristics</i>				
	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
<i>Panel B: Non-turnover-interlocked director characteristics at non-interlocked firms</i>				
	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
<i>Panel C: Non-turnover-interlocked firm characteristics</i>				
	Mean	Median	SD	N
Total assets (millions)	10,957.859**	1,422.700***	43,764.939	87,799
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

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 ISS Voting Analytics, 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.

and control samples are statistically significant, as indicated by the asterisks in Table 3, these differences in levels are unlikely to drive our results for at least two reasons. First, we estimate our regressions in first-differences at the director-firm level and saturate our models with different fixed effects (see Section 3.2). Second, we use propensity score-matched control samples to address concerns that our results are driven by selection rather than treatment. In these matched samples, all differences in director- and firm-level control variables between treatment and control samples are statistically insignificant. Still, our results remain qualitatively unchanged (see Section 4.2).¹⁵

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

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 consecutive 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 in withheld votes on a treatment indicator, that is, a dummy variable equal to one if a director was involved in a forced CEO turnover at another firm between the date of this election and the date of the previous election,

¹⁵ Note: To obtain treatment status, directors need to hold at least two board seats at the CEO's departure, resulting in a significantly higher number of board seats compared to control directors. This additional condition for the treatment sample may cause some of the documented differences across the treatment and control samples as shown in Table 3. In Section 4.1, we conduct an additional robustness test, in which we require control directors to have at least two board seats as well. Again, results remain virtually unchanged.

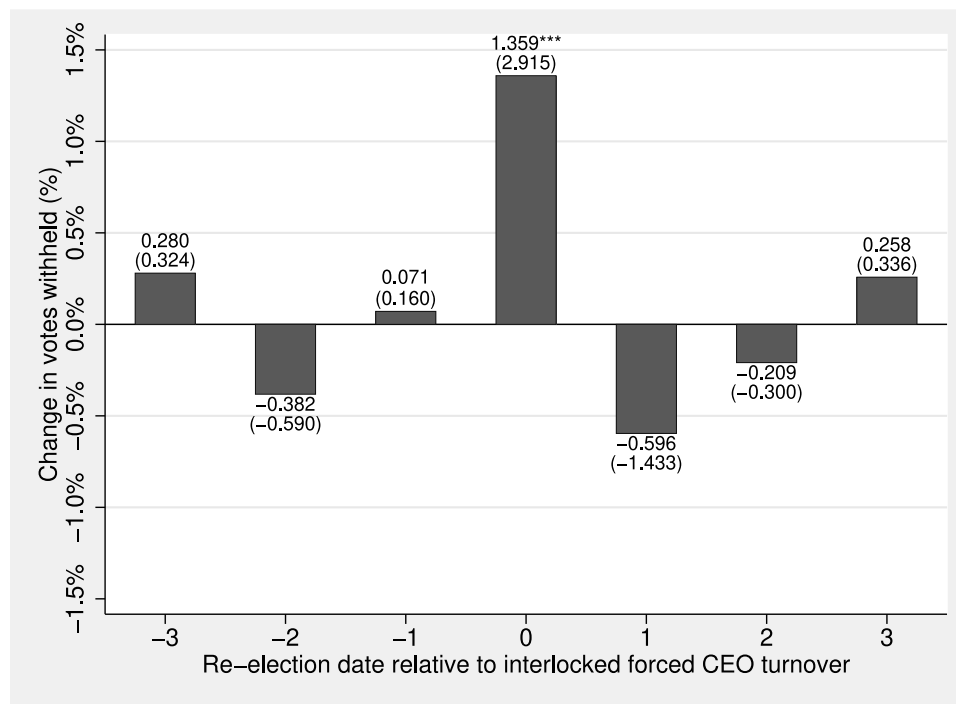


Fig. 1. Forced CEO turnovers and withheld votes at interlocked firms. This figure plots coefficient estimates obtained from an ordinary least squares regression of the change in withheld votes in director re-elections on a dummy variable set equal to one if a director was involved in a forced CEO turnover at another firm between the date of this re-election and the date of the previous election, and zero otherwise. Each bar represents the coefficient estimate obtained from a separate regression that uses leads or lags of up to three elections relative to the first re-election after the forced CEO turnover. The sample comprises director re-elections between 2003 and 2017 from ISS Voting Analytics, 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.

and zero otherwise. We estimate this regression repeatedly for three re-elections before to three re-elections after the first re-election that follows the forced CEO turnover using a treatment dummy variable indicating a turnover-interlock with a lead or lag of up to three re-elections. The coefficient estimates for the treatment dummy variable obtained in these regressions are displayed in Fig. 1. Absent a forced 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. However, following a forced CEO turnover, turnover-interlocked directors experience a significant increase in withheld votes of 1.36 percentage points relative to non-turnover-interlocked directors. 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.¹⁶

3.2. Multivariate results

The results in the previous section suggest that forced CEO turnovers are associated with increases in withheld votes at interlocked firms. However, these results could be confounded by coinciding changes in director characteristics, such as changes in the number of outside board seats, changes in interlocked firm characteristics, such as changes in

performance, or industry shocks that may affect the value of certain director characteristics. To control for such confounding factors, we estimate the following difference-in-differences regression:

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

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 . v_{ijt} is withheld votes 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 was involved in a forced CEO turnover at another firm between the date of her re-election at firm j in year t and the date of her previous election, $t - 1$, at firm j , and zero otherwise.¹⁷ 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.¹⁸

By estimating the regressions in first-differences, we remove unobserved heterogeneity at the director-firm level. Such a specification is similar to a regression in levels with director-firm fixed effects but, unlike a specification in levels, it can easily accommodate repeated

¹⁶ Figure IA.2 in the Internet Appendix displays changes in withheld votes around forced CEO turnovers for turnover-interlocked and non-turnover-interlocked directors separately. The figure shows that the increase in withheld votes of turnover-interlocked versus non-turnover-interlocked directors around the turnover is driven by an increase in withheld votes of turnover-interlocked directors and not a reduction in withheld votes of non-turnover-interlocked directors.

¹⁷ For most director re-elections, t and $t - 1$ refer to the year t and $t - 1$, respectively. However, in one-fifth of our sample, firms do not re-elect all directors each year but in a staggered manner. For staggered re-elections, t refers to the election in year t , while $t - 1$ refers to the previous shareholder meeting for which we observe election outcomes. In Section 4.1, we show that our baseline findings are not sensitive to dropping re-elections of directors at firms with staggered boards. We also show that the reputational effect is independent of the time between the turnover announcement and the re-election date.

¹⁸ Our results remain similar when we cluster the standard errors at the firm or the director level.

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

Dependent variable:	% votes withheld		
	(1)	(2)	(3)
Forced CEO turnover (d)	1.204*** (3.071)	0.878** (2.553)	1.034** (2.407)
ISS withhold/against (d)	18.214*** (29.462)	18.462*** (29.645)	18.644*** (26.995)
# of other board seats	0.128*** (3.474)	0.133*** (3.100)	0.172*** (3.866)
Total assets, log	0.562*** (2.940)	0.514** (2.326)	0.434* (1.909)
Tobin's Q	-0.194*** (-2.827)	-0.140* (-1.976)	-0.151* (-1.871)
ROA	-1.915** (-2.676)	-2.160** (-2.336)	-1.816* (-1.981)
BH return (m270,m21)	-0.586*** (-6.892)	-0.564*** (-6.273)	-0.596*** (-6.711)
Board size	-0.096** (-2.081)	-0.087 (-1.653)	-0.103 (-1.632)
% outside directors	2.536** (2.427)	2.044* (1.840)	2.009 (1.607)
% busy outside directors	0.261 (0.614)	0.533 (0.987)	0.193 (0.354)
Institutional ownership (%)	0.049 (0.153)	-0.009 (-0.020)	0.062 (0.131)
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

This table reports results from ordinary least squares regressions of the change in withheld votes in director re-elections on a dummy variable set equal to one if a director was involved in a forced CEO turnover at another firm between the date of this re-election and the date of the previous election, and zero otherwise, as well as control variables and fixed effects. All regressions include interacted industry-year fixed effects. The regression reported in Column 2 additionally includes firm fixed effects. The regression reported in Column 3 replaces firm fixed effects with director fixed effects. The sample comprises director re-elections between 2003 and 2017 from ISS Voting Analytics, augmented with director data from BoardEx and ISS, stock price data from CRSP, accounting data from Compustat, and ownership data from Thomson Reuters. All 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.

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 nature in a first-differences setting. The set of time-varying control variables at the firm level, Z_{it} , is based on prior corporate governance and voting literature (e.g., Cai et al., 2009; Fischer et al., 2009; Matvos and Ostrovsky, 2010; Aggarwal et al., 2019; Johnson et al., 2023), and includes total assets, Tobin's Q, ROA, past buy-and-hold returns, board size, the fraction of outside directors on the board, the fraction of busy outside directors, and institutional ownership.

Results from estimating the difference-in-differences regression in Eq. (1) are reported in Column 1 of Table 4. Following a forced CEO turnover, turnover-interlocked directors receive 1.20 percentage points more withheld votes than non-turnover-interlocked directors at

firms in the same industry and year. In Column 2, we add firm fixed effects to the model. These fixed effects additionally absorb unobserved time-invariant firm-specific heterogeneity. This specification effectively compares turnover-interlocked directors to non-turnover-interlocked directors at the same firm. The results show that turnover-interlocked directors experience a significant increase in withheld votes of 0.88 percentage points over non-turnover-interlocked directors at the same firm. 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 absent an involvement in a forced CEO turnover. Our results again remain robust in this most conservative specification: Directors interlocked to a forced turnover face a significant increase in withheld votes of 1.03 percentage points compared to the re-elections of the same directors absent a forced turnover. Hence, these findings are consistent with the notion that directors suffer a reputational loss across all board mandates following a forced CEO turnover. Coefficient estimates on the control variables 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 significantly.¹⁹ 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.²⁰

To gauge the economic magnitude of the documented vote effects, we first compare the observed increases in withheld votes to the sample mean. Directors generally receive high vote support, with a mean of only 6.1% of votes withheld (see Table 3). Hence, increases in withheld votes of between 0.88 and 1.20 percentage points, as reported in Table 4, represent a sizeable increase of between 14.3% and 19.6% over the mean. Indeed, recent research shows that even modest increases in withheld votes often have negative consequences for directors (e.g., Aggarwal et al., 2019). Second, we compare changes in withheld votes following forced CEO turnovers to changes in withheld votes following other corporate incidents that have been shown to adversely affect director reputation, including restatements (Srinivasan, 2005), class action lawsuits (Fich and Shivdasani, 2007), poison pill adoptions (Johnson et al., 2023), and bankruptcy filings (Gow et al.,

¹⁹ We acknowledge that ISS' vote recommendations at turnover-interlocked firms may be affected by the forced CEO turnovers. Hence, the ISS withhold/against control variable may represent a "proxy control variable" (Angrist and Pischke, 2009), a covariate whose omission can result in an omitted variables bias that outweighs the bias resulting from the inclusion of a bad control variable. To gauge the sensitivity of our results to the inclusion of the ISS withhold/against control variable, we re-estimate our main analysis reported in Table 4 without this control variable. Results are reported in Table IA.5 in the Internet Appendix and are very similar to those reported in Table 4, with coefficients of interest becoming somewhat larger and statistically more significant. Hence, our conclusions are not affected by the inclusion of the ISS withhold/against control variable.

²⁰ Recent work in econometrics (e.g., Callaway and Sant'Anna, 2021; Sun and Abraham, 2021) and finance (e.g., Baker et al., 2022) argues that standard DiD regressions with staggered treatment timing may not provide valid estimates of a causal effect even under random assignment of treatment. While this literature has yet to converge on how to correct for such bias, it agrees that the bias tends to be smaller the larger the share of never-treated units. In our case, the share of never-treated directors among all sample directors is 97.6%, suggesting that any bias, if present, is likely to be small. Still, to mitigate concerns that such a bias affects our estimates, we rerun our baseline regression from Table 4, first by completely excluding treated directors from the control group and, second, by excluding treated directors from the control group after the onset of treatment. Results are reported in Panels A and B of Table IA.6 in the Internet Appendix, respectively. Results are similar to those in our baseline regression, suggesting that such a bias is likely to be small and thus unlikely to affect our results.

Table 5
Cross-sectional tests: Turnover types.

Dependent variable:	% votes withheld					
	(1)	(2)	(3)	(4)	(5)	(6)
Forced CEO turnover (d)	1.173*** (3.001)	0.873** (2.541)	1.038** (2.414)			
Unforced CEO turnover (d)	0.490*** (3.848)	0.137 (1.150)	0.239 (1.304)			
Forced CEO turnover, performance-induced (d)				1.392*** (3.249)	1.044*** (2.730)	1.263** (2.691)
Forced CEO turnover, non-performance-induced (d)				−0.312 (−0.461)	−0.491 (−0.621)	−0.685 (−0.600)
Unforced CEO turnover, performance-induced (d)				0.596* (1.985)	0.212 (0.617)	0.225 (0.562)
Unforced CEO turnover, non-performance-induced (d)				0.375*** (2.943)	0.036 (0.296)	0.117 (0.704)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Year × Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	No	Yes	No
Director FE	No	No	Yes	No	No	Yes
Observations	88,406	88,406	88,406	88,322	88,322	88,322
Firms	3,269	3,269	3,269	3,268	3,268	3,268
Directors	18,693	18,693	18,693	18,691	18,691	18,691
Interlocked directorships	3,868	3,868	3,868	3,784	3,784	3,784
Adjusted R^2	0.334	0.347	0.354	0.334	0.347	0.354

This table reports results from ordinary least squares regressions of the change in withheld votes in director re-elections on a dummy variable set equal to one if a director was involved in a certain type of CEO turnover at another firm between the date of this re-election and the date of the previous election, and zero otherwise, as well as control variables and fixed effects. Regressions reported in Columns 1 to 3 include a dummy variable set equal to one if a director was involved in a forced turnover, zero otherwise, and a dummy variable set equal to one if a director was involved in an unforced turnover, and zero otherwise. Regressions reported in Columns 4 to 6 include four dummy variables indicating whether a director was involved in a performance-induced forced turnover, a non-performance-induced forced turnover, a performance-induced unforced turnover, and a non-performance-induced unforced turnover, and zero otherwise, respectively. The sample comprises director re-elections between 2003 and 2017 from ISS Voting Analytics, augmented with director data from BoardEx and ISS, stock price data from CRSP, accounting data from Compustat, and ownership data from Thomson Reuters. All variables are in first-differences. 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.

2018). To this end, we replicate our baseline regression from Column 1 of Table 4 and replace the interlocked turnover dummy variable with alternative treatment indicators that are equal to one for directors interlocked to a firm that restates earnings, a firm that becomes subject to a securities litigation, a firm that adopts a poison pill, or a firm that files for bankruptcy, and zero otherwise. Results are reported in Table IA.7 in the Internet Appendix. We find that the increase in withheld votes resulting from a forced CEO turnover (1.2 percentage points) is approximately 2.5 times larger than the vote effect of restating earnings (0.5 percentage points), 1.5 times larger than the vote effect of securities litigation or a poison pill adoption (both about 0.9 percentage points), and half the vote effect of a bankruptcy filing (2.4 percentage points). Hence, the increase in withheld votes associated with firing a CEO is larger than the increase in withheld votes resulting from other reputational events and is only exceeded by the increase in withheld votes resulting from a bankruptcy filing, arguably the most detrimental event for corporate shareholders. Finally, we investigate whether forced CEO turnovers have the potential to lead to unusually high levels of withheld votes at interlocked firms, defined as at least 15% of votes cast (Bach and Metzger, 2017). The results in Table IA.8 in the Internet Appendix show that directors are between 3.6% and 4.0% more likely to experience such high levels of withheld votes following a forced turnover at an interlocked firm, which is 44.2% to 49.1% higher than the sample mean. Thus, forced CEO turnovers often result in unusually high levels of withheld votes at interlocked firms.

In summary, these results suggest that directors suffer an economically large and statistically significant reputational loss across all board mandates following a forced CEO turnover. These findings contrast with the presumption that 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. Are directors penalized for governance failures?

In this section, we test whether reputational losses are confined to certain types of CEO turnovers or whether they depend on directors' involvement in the turnover. Such cross-sectional tests allow us to shed light on the driving forces behind the documented reputational loss. While replacing a CEO is not expected to generally reflect negatively on involved directors, the need to fire the CEO may indicate a governance failure (e.g., Jensen, 1993; Marcel et al., 2017). For example, we would expect directors to be held accountable for CEO turnovers that are reactive and happen only after significant performance declines or poorly prepared CEO turnovers that have disruptive effects on corporate leadership. More generally, we would expect that directors suffer reputational losses for a failure to monitor the outgoing CEO and take timely and appropriate action to replace a poorly performing CEO. Directors may also suffer reputational losses for hiring the wrong CEO in the first place.

In our first test, we analyze whether directors are punished for firing a CEO and not for a CEO turnover more generally. While directors take an active role in forced CEO turnovers, which have a high potential for disruption, unforced CEO turnovers are often the result of a CEO retiring or taking over responsibilities on the board of directors. Unforced turnovers are thus expected to be smoother and less disruptive and consequently to be associated with smaller reputational losses than forced turnovers. To compare re-election outcomes of directors interlocked to forced and unforced turnovers, we augment the baseline regressions from Table 4 with a dummy variable set equal to one if a director was involved in a CEO turnover that we do not classify as forced at another firm between the date of this re-election and the date of the previous election, and zero otherwise. Column 1 of Table 5

Table 6

Cross-sectional tests: Turnover and director characteristics.

Dependent variable:	% votes withheld				
	(1)	(2)	(3)	(4)	(5)
Forced CEO turnover, honeymoon stage (d)	0.197 (0.314)				
Forced CEO turnover, harvest stage (d)	1.679*** (4.288)				
Forced CEO turnover, decline stage (d)	−0.121 (−0.049)				
Forced CEO turnover (d)		1.204*** (3.068)			
Hired CEO and left before forced turnover (d)		−0.002 (−0.003)			
Forced CEO turnover, co-opted director (d)			1.670*** (3.373)		
Forced CEO turnover, non-co-opted dir. (d)			0.720 (1.566)		
Forced CEO turnover, monitoring comm. (d)				1.566*** (3.748)	
Forced CEO turnover, non-mon. comm. (d)				−0.246 (−0.348)	
Forced CEO turnover, successor announced (d)					0.837 (1.539)
Forced CEO turnover, successor not ann. (d)					1.507*** (3.172)
Control variables	Yes	Yes	Yes	Yes	Yes
Year × Industry FE	Yes	Yes	Yes	Yes	Yes
Observations	88,406	88,406	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	607	607	604	607
Adjusted R^2	0.334	0.333	0.333	0.334	0.333

This table reports results from ordinary least squares regressions of the change in withheld votes in director re-elections on a dummy variable set equal to one if a director was involved in a forced CEO turnover at another firm between the date of this re-election and the date of the previous election, and zero otherwise, as well as control variables and interacted industry-year fixed effects. In the regression reported in Column 1, the dummy variable is split based on whether the turnover takes place during the first three years (Honeymoon stage), between the third and 13th year (Harvest stage), or after the 13th year of the departing CEO's tenure (Decline stage). The regression reported in Column 2 includes an indicator variable set equal to one if a director was involved in a forced turnover at another firm and an indicator variable set equal to one if a director was involved in hiring the fired CEO but left the interlocked firm's board. In the regression reported in Column 3, the dummy variable is split based on whether or not the turnover-interlocked director was appointed to the turnover firm's board during the departing CEO's tenure (co-opted). In the regression reported in Column 4, the dummy variable is split based on whether or not a full CEO replacement is announced with the departure. In the regression reported in Column 5, the dummy variable is split based on whether or not the turnover-interlocked director was a member of the nominating or compensation committee of the turnover firm. The sample comprises director re-elections between 2003 and 2017 from ISS Voting Analytics, augmented with director data from BoardEx and ISS, stock price data from CRSP, accounting data from Compustat, and ownership data from Thomson Reuters. All variables are in first-differences. 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.

reports results from regressions with interacted year and industry fixed effects. While we find positive and statistically significant increases in withheld votes for both forced and unforced turnovers, the vote effect for unforced turnovers is about 60% smaller than the effect for forced turnovers. Columns 2 and 3 report results from regressions that additionally include firm and director fixed effects, respectively. The coefficients on the unforced turnover dummy variable turn statistically insignificant and become economically smaller, while the coefficients on the forced turnover dummy variable remain statistically significant and economically sizeable. These results suggest that directors are penalized for forced turnovers, in which they took an active role and which have a higher potential to be disruptive, and less so for unforced turnovers.

Second, we test whether reputational losses are larger for reactive turnovers than for timely, proactive turnovers. Ertugrul and Krishnan (2011) conjecture that non-performance-induced turnovers are indicative of a proactive board that fires an underperforming CEO before she can cause harm to firm value, while performance-induced forced turnovers are indicative of a reactive board that only reacts to poor performance. Hence, we expect the largest reputational losses to result from forced turnovers that are performance-induced. Following Ertugrul and Krishnan (2011), we use performance-induced

turnovers, classified using the procedure proposed by Jenter and Lewellen (2021), as a proxy for late, reactive turnovers as they follow significant performance declines. We then replicate our baseline regressions with four dummy variables indicating interlocks to forced and unforced turnovers that are either performance-induced or non-performance-induced, and zero otherwise. In the specification with interacted year and industry fixed effects, reported in Column 4 of Table 5, we find positive and significant increases in withheld votes associated with performance-induced forced turnovers and insignificant reductions in withheld votes for non-performance-induced forced turnovers. Unforced turnovers are associated with significant increases in withheld votes that are economically much smaller than those of performance-induced forced turnovers. When adding firm or director fixed effects, as in Columns 5 and 6, we continue to find statistically significant and economically sizeable increases in withheld votes for performance-induced forced turnovers, while the effects of the three other turnover types are statistically insignificant. Hence, reputational losses from CEO turnovers are confined to performance-induced forced turnovers, which are indicative of reactive boards that act too late when damage is already done.

An alternative way to measure the board's reactivity 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 and Fukutomi, 1991; Henderson et al., 2006; Brochet et al., 2021). Brochet et al. (2021) show that in a large sample of public US companies, firm value peaks at about 13 years of CEO tenure on average. During the first three years of tenure, the “honeymoon stage”, a newly appointed CEO gets to know the company and starts implementing changes. The board can assess whether the new CEO meets expectations, with a proactive board firing a disappointing CEO to prevent future harm (Ertugrul and Krishnan, 2011). Hence, we do not expect significant reputational damages for directors involved in forced turnovers during this stage. After a successful honeymoon stage, the CEO’s organizational changes begin to bear fruit, and the CEO enters the “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, forced turnovers in the harvest stage may reflect a reactive board, and we expect reputational damages for involved directors. Finally, after about 13 years of tenure, the positive effects start to be outweighed by the adverse effects of a deteriorating CEO-firm match and increased power and entrenchment, even if the CEO was an excellent match to the firm at appointment and remained so during the harvest stage. During this “decline stage”, the CEOs’ performance contribution turns negative. Turnovers during this stage are on average value increasing (e.g., Brochet et al., 2021), while not necessarily indicative of a reactive board, so we expect no adverse reputational effects for involved directors. We test this hypothesis by splitting the forced turnover dummy variable into three dummy variables indicating forced turnovers during these CEO life-cycle stages.²¹ Results are reported in Column 1 of Table 6. We find large and significant increases in withheld votes for forced turnovers during the harvest stage and insignificant changes in withheld votes, that are close to zero, for turnovers during the honeymoon and the decline stages. These results suggest that reputational losses are confined to forced turnovers during the most productive period of a CEO’s tenure, that is turnovers that likely reflect a reactive board that took too long to recognize and correct a CEO-firm mismatch.

We also test whether directors are not only penalized for firing the CEO but also for hiring the wrong CEO in the first place (e.g., Laux, 2010). Specifically, firing a CEO may reveal new information on the CEO’s quality, the CEO-company match, and ultimately the CEO’s value contribution to the company.²² However, shocks to the quality of a CEO-firm match may result in a CEO firing, even though this CEO may have been an excellent match at the time of appointment (e.g., Eisfeldt and Kuhnen, 2013; Brochet et al., 2021). Thus, a CEO eventually fired for poor performance may still have been a reasonable choice at the time of appointment, and her firing may thus not reflect negatively on directors involved in the CEO’s appointment. To separate reputational effects from involvement in the CEO’s firing from potential reputational effects from involvement in the CEO’s hiring, we augment our baseline specification with a dummy variable equal to one if a director held a board seat at a turnover firm at the time of the fired CEO’s appointment but left the board before the turnover, and zero otherwise. Results are reported in Column 2 of Table 6. While the coefficient on the

forced turnover-interlock dummy variable remains economically and statistically significant, the coefficient on the dummy variable capturing involvement in the CEO’s hiring is economically small and statistically insignificant. This result suggests that directors are penalized for their involvement in the CEO’s firing but not for their involvement in the hiring of the later fired CEO.

Next, we test whether directors are penalized for neglecting their monitoring duties toward the CEO. As a first proxy for directors’ monitoring intensity and quality, we use the co-option measure of Coles et al. (2014). They show that directors appointed after a CEO has resumed office (co-opted directors) are favorably inclined to this CEO and thus provide weaker monitoring. Hence, involvement in a forced turnover may reinforce shareholders’ assessment of co-opted directors’ limited willingness to act in shareholders’ best interests.²³ We would thus expect that the reputational loss of a forced CEO turnover is larger for 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. Results are reported in Column 3 of Table 6. Both coefficient estimates are positive, but the estimate for co-opted directors is about twice the size of the estimate for non-co-opted directors and only the former is statistically significant. This result confirms our conjecture that directors are penalized for a failure to monitor the CEO properly. Our second proxy variable for directors’ ability and willingness to monitor senior management is based on board committee membership. The board committees generally entrusted with monitoring the CEO are the nominating and compensation committees (e.g., Chhaochharia and Grinstein, 2009; Guo and Masulis, 2015). Hence, we rerun our baseline regression with two dummy variables, one is equal to one for turnover-involved directors who are members of either the nominating or the compensation committee at the turnover firm, and zero otherwise, and the other variable is equal to one for turnover-involved directors who are not members of these two committees, and zero otherwise. Results are reported in Column 4 of Table 6. We find large positive and statistically significant increases in withheld votes for members of the nominating and compensation committees and small and statistically insignificant reductions for all other directors. This result suggests that directors suffer reputational losses from poor monitoring of the CEO.

Finally, we directly test whether reputational losses are larger for directors involved in poorly prepared forced CEO turnovers. Dalton and Dalton (2007) and Cvijanović et al. (2023) argue that the lack of an heir apparent to the outgoing CEO signals the board’s unpreparedness and results in more disruptive turnovers. Hence, director reputation is expected to suffer more if no full replacement is announced jointly with the CEO’s departure. To test this hypothesis, we replicate our baseline regression by replacing the forced CEO turnover interlock dummy variable with two dummy variables, one indicating involvement in a forced CEO departure with a full replacement announced simultaneously and one without such an announcement. Results are reported in Column 5 of Table 6. We find that directors involved in a forced CEO turnover with either an interim succession or no succession announced experience an increase in withheld votes that is twice the size of the increase in withheld votes of directors involved in a forced turnover with a full replacement announced. These results suggest that shareholders penalize directors for poorly prepared, disruptive CEO turnovers.

²¹ Note that this sample split corresponds to interaction terms between the forced CEO turnover dummy variable and the CEO life-cycle stage dummies, 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.

²² Note that previous research shows that both initial uncertainty about a CEO’s fit to the company at appointment (e.g., Jovanovic, 1979; Allgood and Farrell, 2003; Ali and Zhang, 2015) as well as subsequent shocks to the CEO-firm match quality (e.g., Miller, 1991; Garrett and Pavan, 2012; Eisfeldt and Kuhnen, 2013) make it difficult to assess a CEO’s quality at the time of appointment even *ex-post*.

²³ Note that co-opted directors were hired after the CEO took office, while non-co-opted directors were involved in the CEO’s hiring. However, the previous test, reported in Column 2 of Table 6, shows that directors are not penalized for hiring the fired CEOs, suggesting that a test based on the co-option measure only captures shareholders’ reaction to weak monitoring and not to a potential involvement in the CEO’s hiring. Moreover, any reputational loss from hiring would generate the opposite effect, so the documented penalty for weak monitoring of co-opted directors would be a conservative estimate.

The results in this section show that directors are not penalized for all CEO turnovers but for forced turnovers that are related to governance failures. Specifically, directors are penalized for reactive forced turnovers, firings during the most productive period of a CEO's tenure, poor monitoring of the CEO, and the lack of a successor to the fired CEO. In contrast, directors are not penalized for hiring the wrong CEO in the first place. Importantly, our analysis uncovers no sub-sample for which we find significant reputational gains from involvement in a turnover. Taken together, these results contradict the general presumption in prior empirical finance research that a forced CEO turnover is a credible signal of a board's monitoring ability. In contrast, they suggest that firing the CEO is indicative of governance failure at the board level.

3.4. Which shareholders vote against turnover-involved directors?

In this section, we investigate which shareholders vote against directors involved in forced CEO turnovers. Systematically penalizing directors across all board mandates requires shareholders to (i) observe and evaluate directors' actions, (ii) observe interlocked directorships, and (iii) systematically vote against directors across all firms where they hold board seats. 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; Liu et al., 2020). Institutional investors are considered to be sophisticated (e.g., Gibson et al., 2004) and to impact the governance of their portfolio firms positively, independent of whether they follow an active (e.g., Brav et al., 2008) or passive investment approach (e.g., Appel et al., 2016).²⁴ Hence, we expect that turnover-interlocked directors receive more withheld votes 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 turnover and the turnover-interlocked firms using Thomson Reuters' institutional holdings (13F) database. We identify all institutional investors that report holdings in the turnover firms at the reporting date before the turnover announcements and holdings in the interlocked firms at the reporting date before the re-election of turnover-interlocked directors. Common ownership is the lower value of the fraction of shares outstanding held in the turnover firm and the fraction of shares outstanding 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 the sample mean (median) and one with common ownership below the sample mean (median).

Results from re-estimating our baseline regression with the treatment dummy split according to mean (median) common ownership are reported in Column 1 (2) of Table 7. We find that outside directors involved in a forced CEO turnover with above mean (median) common ownership experience an increase in withheld votes at an interlocked firm that is 60% (74%) larger than the increase in withheld votes at an interlocked firm in response to turnovers with below mean (median) common ownership. These results indicate that informed institutional investors engaged in both the turnover and the interlocked firms are responsible for penalizing directors for their involvement in the turnover.²⁵

²⁴ 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.

²⁵ These results suggest that the recent growth in institutional shareholdings and concentration of ownership in the portfolios of a few large asset managers,

Table 7

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

Dependent variable:	% votes withheld	
	(1)	(2)
Forced CEO turnover \geq mean common ownership (d)	1.442** (2.678)	
Forced CEO turnover < mean common ownership (d)	0.900 (1.670)	
Forced CEO turnover \geq median common ownership (d)		1.513** (2.603)
Forced CEO turnover < median common ownership (d)		0.870* (1.758)
Control variables	Yes	Yes
Year \times 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

This table reports results from ordinary least squares regressions of the change in withheld votes in director re-elections on a dummy variable set equal to one if a director was involved in a forced CEO turnover at another firm between the date of this re-election and the date of the previous election, and zero otherwise, as well as control variables and interacted industry-year fixed effects. In the regression reported in Column 1 (2), the dummy variable is split into two variables depending on whether or not common institutional ownership in the turnover and the interlocked firm is above the mean (median) common ownership. The sample comprises director re-elections between 2003 and 2017 from ISS Voting Analytics, augmented with director data from BoardEx and ISS, stock price data from CRSP, accounting data from Compustat, and ownership data from Thomson Reuters. All variables are in first-differences. 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.

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 and non-turnover-interlocked directors need to show insignificant differences in their re-election results prior to forced CEO turnovers. Fig. 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 forced CEO turnovers. In the three re-elections prior to forced CEO turnovers, there are no significant differences in the changes in withheld votes between turnover-interlocked and non-turnover-interlocked directors, supporting the notion that the parallel trends assumption holds.

Fig. 1 further shows no evidence of a treatment reversal. Across all three post-turnover re-elections, differences in the change in withheld votes between turnover-interlocked directors and non-turnover-interlocked directors are economically small and statistically insignificant. To test for treatment reversal more formally, we rerun the baseline regression from Column 1 of Table 4 with additional lags for one and two re-elections, respectively. Results are reported in Columns 1 and 2 of Table 8. The coefficient on the treatment indicator remains economically large and statistically significant in both regressions,

which triggered a discussion around the resulting incentives for firms to compete (e.g., Azar et al., 2018; Lewellen and Lowry, 2021), might also have a so-far unrevealed positive consequence: Substantial cross-shareholdings facilitate monitoring of directors across firms, resulting in stronger incentives to perform.

Table 8
Treatment reversal and pseudo treatments.

Dependent variable:	% votes withheld				
	Baseline plus lag (1)	Baseline plus lags (2)	Pseudo $t-1$ (3)	Pseudo $t-2$ (4)	Joined after (5)
Forced CEO turnover (d)	1.202*** (3.052)	1.279*** (2.863)	0.020 (0.054)	-0.390 (-0.835)	1.202*** (3.074)
Forced CEO turnover (d) $t-1$	0.097 (0.272)	0.373 (1.066)			
Forced CEO turnover (d) $t-2$		-0.325 (-0.506)			
Joined after forced CEO turnover (d)					0.629 (0.583)
Control variables	Yes	Yes	Yes	Yes	Yes
Year \times Industry FE	Yes	Yes	Yes	Yes	Yes
Observations	88,406	64,555	64,450	47,544	88,406
Firms	3,269	2,582	2,577	2,098	3,269
Directors	18,693	13,817	13,788	10,588	18,693
Turnover-interlocked directorships	607	424	420	315	607
Adjusted R^2	0.333	0.360	0.418	0.431	0.333

This table reports results from ordinary least squares regressions of the change in withheld votes in director re-elections on a dummy variable set equal to one if a director was involved in a forced CEO turnover at another firm between the date of this re-election and the date of the previous election, and zero otherwise, as well as control variables and interacted industry-year fixed effects. In the regressions reported in Columns 1 and 2, the baseline regression reported in Column 1 of Table 4 is augmented with lags of the dummy variable indicating a forced turnover at another firm. In the regressions reported in Columns 3 and 4, pseudo-treatment dummy variables are set equal to one for one and two re-elections prior to the actual forced turnover, and zero otherwise, respectively. In the regression reported in Column 5, the baseline regression reported in Column 1 of Table 4 is augmented with an indicator variable equal to one if the director joined the board after the departure of the CEO but before the date of her re-election. The sample comprises director re-elections between 2003 and 2017 from ISS Voting Analytics, augmented with director data from BoardEx and ISS, stock price data from CRSP, accounting data from Compustat, and ownership data from Thomson Reuters. All variables are in first-differences. 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.

while the coefficients on the lagged treatment indicators are small in magnitude and statistically insignificant. Hence, there is no evidence of a treatment reversal, implying that directors involved in a forced CEO turnover experience a persistent increase in withheld votes at interlocked firms.

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 turnovers. Columns 3 and 4 present the results. The coefficient on the pseudo-treatment indicator is small and statistically insignificant in both regressions, suggesting that increases in withheld votes are due to forced turnovers at interlocked firms, as opposed to alternative factors, such as deteriorating performance of the turnover firms preceding the turnovers.

In a related placebo test, we analyze re-elections of directors appointed to the board of the turnover firms after the forced CEO turnovers. If directors are penalized for involvement in a forced CEO turnover, directors appointed shortly after the turnover should, in contrast to directors on the board at the time of the turnover, not be held accountable for the turnover and thus not face an increase in withheld votes at their next re-election. To test this conjecture, we augment the baseline specification from Column 1 of Table 4 with an indicator variable equal to one if a director joined the board of the turnover firm after the departure of the CEO but before the date of her next re-election at the interlocked firm. The results in Column 5 of Table 8 show that the coefficient on the placebo dummy variable is statistically insignificant while the coefficient on the treatment dummy variable remains virtually unchanged compared to the baseline regression. These results again suggest that it is an involvement in a forced CEO turnover that results in a negative reputational effect.

A 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 board seats more than non-turnover-interlocked directors (see Tables 2 and 3). This selection bias may be problematic because directors with board overlaps may fulfill different roles than directors without board overlaps (e.g., Geng et al., 2023). 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 board seats as a covariate in our regressions, we control for effects that are directly related to changes in the number of board seats as well as unobservable director characteristics correlated with the change in 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 address the concern that selection rather than treatment drives our results, we rerun our main analysis from Table 4 on a sample that excludes all non-turnover-interlocked directors who hold no additional board seats from the control sample. The results are reported in Table IA.9 in the Internet Appendix and are virtually identical to the main results. Hence, a selection bias resulting from our identification strategy does not drive our results.

Finally, we analyze whether reputational effects are muted if interlocked directors are re-elected in a staggered manner. In staggered boards, the time gap between a CEO's departure and the next director re-election can be as long as five years. Such a long time lag may result in a muted reputational effect if shareholders tend to forget about directors' actions in the distant past. In Column 1 of Table IA.10 in the Internet Appendix, we report results obtained when we replicate the baseline regression from Column 1 of Table 4 but exclude firms with staggered boards. The estimate of the treatment effect is very similar to the one obtained in our baseline regression. In Column 2, we split the treatment indicator into two dummy variables depending on whether the time between the date of the turnover announcement and the date of the next re-election is below or above one year, and zero otherwise, with time gaps above one year generally picking up staggered director re-elections. The coefficients on both variables are similar in magnitude and remain statistically significant. In Column 3 (4), we split the treatment indicator into two dummy variables depending on whether

the time between the date of the turnover announcement and the date of the next re-election is below or above 0.75 (1.25) years. The results are very similar to those in Column 2. Taken together, these results suggest that variation in the time between elections of directors caused by staggered elections does not bias our results.

In summary, this section shows that there is no significant difference in the change in withheld votes between turnover-interlocked and non-turnover-interlocked directors before forced turnovers, providing empirical evidence in support of the parallel trends assumption. Moreover, the negative reputational effects are persistent, showing no reversal in subsequent years.

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 and Chidambaram, 2010), investors might simply penalize directors interlocked to poorly performing firms. Under this alternative interpretation, causality would not run from forced turnovers to increases in withheld votes of turnover-interlocked directors. Instead, both forced CEO turnovers at the turnover firms and increases in withheld votes of the turnover-interlocked directors 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 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 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 comprising directors interlocked to firms that are similar in terms of the likelihood of firing the CEO and differ only in the effective turnover decision. This setting thus allows us to address the concern that factors that lead to turnover also drive increases in withheld votes of turnover-interlocked directors.

We calculate propensity scores using the forced turnover likelihood model of Peters and Wagner (2014). To estimate this model, we construct a turnover-firm panel that comprises each forced 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 filter ensures that the matched control firms do not include firms that have recently forced out or will soon force out their 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 turnovers, 1,822 distinct potential control firms, and 160,312 potential control firm-turnover pairs. A covariate balancing test between the turnover firms and the potential control firms is presented in Panel A of Table IA.13 in the Internet Appendix. Absent any matching, the two samples differ significantly in most variables.

Results from estimating propensity scores are reported in Table IA.12 in the Internet Appendix.²⁶ Within each turnover stratum, we identify the control firm with the closest propensity score to the turnover firm.²⁷ We find nearest neighbors for 181 of the 183 forced

turnovers in our sample. The results of covariate balancing tests between the turnover and matched control firms are reported in Panel B of Table IA.13 in the Internet Appendix. The turnover sample and the matched control sample do not differ significantly in terms of any of the variables. Common support is illustrated in Figure IA.3 in the Internet Appendix. Panel B shows that the density distributions of the propensity scores for the turnover and matched control samples align closely across the full spectrum of propensity scores after matching.

For each firm in our propensity score-matched sample, we identify all outside directors who also hold outside directorships at other firms in our vote-share sample, following the methodology described in Section 2. We identify 526 turnover-interlocked directorships held by 383 individual directors at 411 firms, and 586 matched non-turnover-interlocked directorships held by 418 individual directors at 453 matched firms.

Using this matched sample, we estimate regressions similar to our baseline regressions in Table 4. Because this propensity score-matched sample is naturally cross-sectional, we estimate the regressions in levels and not in changes. Hence, we add time-invariant director-level control variables to these specifications. Table 9, 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 regression from Column 1 with turnover event fixed effects. This most restrictive specification compares withheld votes between turnover-interlocked directors and matched non-turnover-interlocked directors within each matched pair separately. 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 turnover but no actual 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 1.03 percentage points more withheld votes (16.8% over the unconditional sample mean) than non-turnover-interlocked directors serving on the board of firms with the same propensity to force out a CEO. These results suggest that the performance of the turnover firm is unlikely to constitute an omitted variable.²⁸

The forced CEO turnover likelihood model of Peters and 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 fires the 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 a forced turnover and negative updating about director ability. In contrast, bad performance in the other firm might be due to bad luck, neither leading to a forced turnover nor conveying any signal about director ability. To address this concern, we construct an alternative

²⁶ We follow the suggestion of Peters and Wagner (2014) to use a logistic regression. Peters and 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.

²⁷ 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 and 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.

²⁸ A concern with the use of the Peters and Wagner (2014) model for estimating propensity scores is that it relies on performance measures computed over one year, which may be too short to capture the entire effect of performance on forced turnovers. Hence, in a robustness test reported in Table IA.14 in the Internet Appendix, we replicate the analysis reported in Table 9 using performance measures computed over three years preceding the turnover when estimating propensity scores. The results remain very similar.

Table 9
Propensity score matching analysis.

Dependent variable:	% votes withheld								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Forced CEO turnover (d)	1.437** (2.679)	1.433*** (2.953)	1.026* (1.855)	1.380** (2.436)	1.387*** (2.956)	0.856* (1.772)	1.480** (2.593)	1.243** (2.225)	0.972* (1.809)
ISS withhold/against (d)	19.520*** (10.431)	19.334*** (10.076)	19.707*** (10.252)	19.092*** (9.768)	19.234*** (9.620)	19.472*** (11.363)	21.822*** (11.650)	22.316*** (11.383)	22.199*** (14.852)
Female (d)	-0.456 (-0.678)	-0.301 (-0.463)	-0.384 (-0.714)	-0.783 (-1.267)	-0.785 (-1.216)	-0.075 (-0.112)	-1.036 (-1.289)	-0.541 (-0.633)	-0.594 (-1.005)
# of other board seats	-0.030 (-0.190)	0.049 (0.393)	0.042 (0.235)	0.255* (1.995)	0.301** (2.241)	0.287** (2.226)	0.270** (2.494)	0.329*** (3.102)	0.281** (2.127)
Age (yrs), log	-0.642 (-0.235)	-1.852 (-0.684)	-1.983 (-0.884)	1.958 (1.101)	0.578 (0.246)	1.104 (0.530)	3.016 (1.409)	2.939 (1.275)	1.636 (0.614)
Tenure (yrs), log	1.490*** (4.056)	1.301*** (3.820)	1.786*** (5.244)	0.882** (2.171)	0.716* (1.874)	0.570* (1.791)	1.214*** (2.830)	1.157*** (2.922)	1.082*** (2.804)
Total assets, log	0.140 (0.552)	0.372 (1.348)	0.020 (0.080)	0.434 (1.655)	0.535** (2.159)	0.382* (1.828)	0.530* (1.749)	0.527* (1.856)	0.426 (1.561)
Tobin's Q	0.045 (0.189)	0.160 (0.670)	0.058 (0.269)	-0.162 (-0.789)	-0.082 (-0.345)	-0.242 (-1.243)	0.186 (1.138)	0.321* (1.935)	0.158 (0.678)
ROA	-2.474 (-0.954)	-4.112** (-2.143)	-1.339 (-0.659)	-2.196 (-1.568)	-2.112 (-1.525)	-1.535 (-0.884)	-2.470 (-0.938)	-3.574* (-1.690)	-1.303 (-0.665)
BH return (m270,m21)	-1.938*** (-3.144)	-1.613*** (-2.952)	-2.011*** (-3.201)	-1.557*** (-3.399)	-1.486*** (-3.173)	-1.596*** (-2.863)	-3.147*** (-4.358)	-3.244*** (-3.896)	-3.649*** (-4.556)
Board size	-0.115 (-0.789)	-0.046 (-0.353)	-0.136 (-0.844)	-0.162 (-1.350)	-0.057 (-0.432)	-0.167 (-1.342)	-0.324** (-2.399)	-0.192 (-1.399)	-0.304 (-1.622)
% outside directors	6.880* (1.700)	-0.293 (-0.065)	4.610 (1.254)	3.232 (0.956)	-0.517 (-0.180)	1.450 (0.446)	7.033 (1.159)	3.466 (0.578)	1.719 (0.381)
% busy outside dirs	-0.027 (-0.021)	0.312 (0.252)	0.609 (0.403)	0.663 (0.518)	1.127 (0.929)	-0.657 (-0.490)	0.688 (0.452)	0.918 (0.630)	0.213 (0.138)
Inst. ownership (%)	0.560 (0.486)	0.111 (0.101)	-0.497 (-0.478)	-0.637 (-0.653)	-0.312 (-0.382)	-1.400 (-1.364)	0.190 (0.168)	-0.400 (-0.320)	-0.563 (-0.530)
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	1,112	1,112	1,112	1,072	1,072	1,072	935	935	935
Firms	745	745	745	730	730	730	646	646	646
Directors	762	762	762	720	720	720	619	619	619
Turnover-interl. d'ships	526	526	526	516	516	516	440	440	440
Adjusted R ²	0.284	0.303	0.305	0.268	0.285	0.303	0.351	0.372	0.394

This table reports results from ordinary least squares regressions of withheld votes in director re-elections on a dummy variable set equal to one if a director was involved in a forced CEO turnover at another firm between the date of this re-election and the date of the previous election, and zero otherwise, as well as control variables. The regressions reported in Columns 2, 5, and 8 include year and interlocked firm industry fixed effects. The regressions reported 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 another 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 and Wagner \(2014\)](#). The sample in Columns 4 to 6 is based on propensity scores estimated using the forced CEO turnover likelihood model of [Peters and Wagner \(2014\)](#) augmented with aggregate news sentiment prior to the CEO departure date, and the sample in Columns 7 to 9 is based on propensity scores estimated using the forced CEO turnover likelihood model of [Peters and 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, 5, 7, and 8) or at the turnover level (Columns 3, 6, and 9). *, **, and ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively.

matched control sample of firms using the CEO turnover likelihood model of [Peters and Wagner \(2014\)](#) augmented with a newspaper-based sentiment index. The rationale behind adding newspaper sentiment is that mistakes of the leadership team can be expected to trigger more negative newspaper coverage than bad luck. Matching on news sentiment thus yields a control sample that is 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 period preceding the turnover announcement as a proxy for firms' news sentiment.²⁹ A comparison

²⁹ Ravenpack's AES 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 a 91-day rolling window, and considers all news sources from the Dow Jones universe (among others, Dow Jones Newswires, the Wall Street Journal, Barron's, and MarketWatch).

between turnover and potential control firms absent matching shows that turnover firms show a significantly more negative news sentiment before the turnover (see Panel A of Table IA.15 in the Internet Appendix), supporting the conjecture that Ravenpack's AES captures negative news preceding turnover events. The results in Panel B of Table IA.15 in the Internet Appendix 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 spectrum of propensity scores after matching (see Figure IA.4 in the Internet Appendix). Using the sample based on an augmented matching approach, we re-estimate the regressions reported in Columns 1 to 3 of Table 9. The results are reported in Columns 4 to 6 and are virtually identical to those obtained when constructing the control sample with [Peters and 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 forced CEO turnover, are unlikely to drive the observed increase in withheld votes.

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 before the directors decided to force out their CEO. Forced turnovers might then be the result of increases in withheld votes rather than the other way around. To address this concern and to ensure that causality runs from forced CEO turnovers to increases in withheld votes, we again replicate our propensity score matching analysis and extend (Peters and 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 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 should address the reverse causality issue. The results in Table IA.16 in the Internet Appendix indicate that turnover and 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 turnover. Propensity scores align closely across the full spectrum of propensity scores after matching (see Figure IA.5 in the Internet Appendix). The results from re-estimating Columns 1 to 3 of Table 9 using the matched sample that is based on this extended propensity score model are reported in Columns 7 to 9 of Table 9. Across all three columns, which 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 and similar pre-turnover election outcomes. 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.³⁰

4.3. Reputation versus distraction

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

We conduct two 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

Table 10

Forced CEO turnovers and director distraction at interlocked firms.

Dependent variable:	% votes withheld		Attends < 75% of meetings (d)
	(1)	(2)	
Forced CEO turnover (d)	1.211*** (3.070)	1.209*** (3.067)	−0.001 (−0.238)
Sudden CEO death (d)	−0.586 (−1.005)		
Sudden CEO death (extended) (d)		0.768 (0.870)	
Control variables	Yes	Yes	Yes
Year × Industry FE	Yes	Yes	Yes
Observations	88,372	88,363	51,451
Firms	3,269	3,269	1,470
Directors	18,692	18,692	10,382
Turnover-interlocked directorships	605	605	434
Death-interlocked directorships	28	39	—
Adjusted R ²	0.333	0.333	0.036

This table reports results from ordinary least squares regressions of the change in withheld votes in director re-elections (Columns 1 and 2) or the change in board meeting attendance (Column 3) on a dummy variable set equal to one if a director was involved in a forced CEO turnover at another firm between the date of this re-election and the date of the previous election, and zero otherwise, as well as control variables and interacted industry-year fixed effects. The regression reported in Column 1 augments the baseline regression reported in Column 1 of Table 4 with a dummy variable set equal to one if a director was involved in a sudden CEO death at another firm, and zero otherwise. In the regression reported in Column 2, the sample of unexpected CEO deaths is augmented with CEO sick leaves during which the CEO subsequently dies. The regression in Column 3 reports results of the baseline regression reported in Column 1 of Table 4 with the outcome variable replaced by the change in board meeting attendance. Meeting attendance is a dummy variable set equal to one if a director attends less than 75% of all board meetings within a fiscal year at the interlocked firm, and zero otherwise. The sample comprises director re-elections between 2003 and 2017 from ISS Voting Analytics, augmented with director data from BoardEx and ISS, stock price data from CRSP, accounting data from Compustat, and ownership data from Thomson Reuters. All variables are in first-differences. 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.

from the board of directors than sudden director deaths do. However, sudden CEO deaths are, by definition, outside of the board's control and thus are not expected to affect director reputation. Hence, if our results are driven by director distraction, we would expect to observe an increase in withheld votes for directors serving on the board of another firm whose CEO suddenly passes away. To test this empirically, we re-estimate the baseline regression in Column 1 of Table 4, augmented with a dummy variable equal to one if the director is interlocked to another firm that experiences a sudden death of the CEO, and zero otherwise. To construct this variable, we search for CEO departures caused by sudden deaths as defined by Nguyen and Nielsen (2010). To increase 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 and die subsequently.³¹ Results are reported in Columns 1 and 2 of Table 10. 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 test whether forced CEO turnovers affect board meeting attendance at interlocked firms. Masulis and Zhang (2019) show that directors suffering

³⁰ These tests using matched samples also address the concern that our baseline estimates are biased because of a size imbalance between treated and control samples. Given that we continue to find results that are very similar to our baseline regression, it seems unlikely that a sample size imbalance drives the result in our main analysis.

³¹ To ensure that these later deaths constitute a shock similar to sudden deaths, we restrict the extension to deaths that occur within 30 days of the sick leave announcement.

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 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, we re-estimate the baseline regression from Column 1 of Table 4 with the change in directors' board meeting attendance at interlocked firms as an outcome variable. We obtain board meeting attendance data from ISS, measured using a dummy variable set equal to one if a director attends less than 75% of all board meetings within a fiscal year, and zero otherwise. Since ISS only provides these data for S&P1500 firms, our sample size is reduced to 51,451 directorships. The results are reported in Column 3 of Table 10. The estimated coefficient is close to zero and statistically insignificant, suggesting that there is no relationship between forced CEO turnovers and directors' board meeting attendance at interlocked firms. 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, these results suggest that our results are not caused by director distraction but by shareholders reassessing individual directors' capabilities to monitor and, if necessary, fire CEOs.

5. The effect of forced CEO turnovers on directors' labor market opportunities

So far, we have shown that forced CEO turnovers negatively affect director reputation, as measured by changes in withheld votes at interlocked firms. These reputational effects may extend into the director labor market and affect career prospects. Consistently, Aggarwal et al. (2019) show that directors who suffer an increase in withheld votes in uncontested elections are more likely to sustain reduced labor market opportunities. Similarly, Johnson et al. (2023) find that directors involved in the adoption of a poison pill suffer reputational damage that impacts their career outlooks. Consequently, we expect that directors' labor market opportunities are reduced following a forced CEO turnover.

To test this conjecture empirically, we conduct analyses akin to a difference-in-differences setting. Specifically, we analyze changes in directors' labor market opportunities 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 this analysis, 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. In these analyses, we include turnover event fixed effects, controlling for all turnover-specific effects, as well as firm fixed effects, absorbing time-invariant characteristics of the interlocked firms.

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 turnover, respectively, and zero otherwise. The positive and significant coefficients reported in Columns 1 and 2 of Table 11 suggest that involvement in a forced turnover increases the likelihood of losing board seats. The obtained estimates indicate a 7.6 percentage point increase in the probability of losing a board

seat over a one-year horizon and a 9.7 percentage point increase over a five-year horizon, respectively. The unconditional probabilities of losing a directorship in our sample are 13.1% and 46.0% over one and five years, respectively. Hence, the economic magnitude is sizeable: An involvement in a forced CEO turnover increases the probability of losing a directorship by 58% 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 Columns 1 and 2. Results reported in Columns 3 and 4 show that the coefficients on the forced interlocked dummy variable turn economically and statistically insignificant in both columns. Hence, the overall loss in directorships is driven by a loss of directorships at the turnover firm.^{32,33}

We also 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 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 Columns 5 and 6. They show that turnover-interlocked directors are significantly more likely to gain new board seats five years after the forced turnover, but not within one year after the turnover. Five years after the turnover, the likelihood of gaining a new board seat is 8.7 percentage points higher for turnover-interlocked directors versus non-turnover-interlocked directors. This coefficient estimate is very similar in magnitude to that for directorship losses reported in Column 2, suggesting that the gains in directorships at other firms substitute for the losses of directorships at the turnover firms. To explicitly test for such substitution effects, we again replicate the analysis and use a dummy variable that indicates a net loss of outside directorships as the dependent variable. The results reported in Columns 7 and 8 confirm our conjecture: On average, directors involved in a forced turnover have a 7% higher likelihood of losing a directorship in the first year after the turnover. 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 forced

³² We also test whether the cross-sectional patterns on withheld votes described in Section 3.3 translate into the labor market and affect directors' losses of directorships. Results from cross-sectional regressions using the loss of board seats as a dependent variable reported Table IA.17 in the Internet Appendix are in line with those in Section 3.3.

³³ Our finding that turnover-interlocked directors face a significant risk of losing their directorship at the turnover firm 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, which may be accompanied by a decline in influence, network connections, and prestige. We conduct two tests to address this concern. First, we drop turnover-interlocked directors who leave the board of the turnover firm between the forced CEO turnover and the subsequent re-election from the sample and replicate our main analysis in Table 4 using this reduced sample. Results are reported in Table IA.18 in the Internet Appendix and are almost identical to those reported in Table 4. Second, we re-estimate our regressions from Columns 1 to 3 of Table 9 with a dummy variable set equal to one if a director leaves the turnover/matched control firm before the subsequent re-election at the interlocked firm, and zero otherwise, and an interaction term between this variable and the dummy variable for an interlock to a forced turnover. Results reported in Table IA.19 in the Internet Appendix show that neither the interaction term nor the standalone variable indicating the loss of a board seat at the turnover/matched control firm is statistically significant. Results from both these tests suggest that the increase in withheld votes of turnover-interlocked directors is independent of a loss of their board seat at the turnover firm.

Table 11

Forced CEO turnovers and gains and losses of directorships.

Dependent variable:	Loss of directorship (d)				Gain of directorship (d)		Net loss of directorship (d)	
	w/ turnover firm		w/o turnover firm					
	t = [0,1]	t = [0,5]	t = [0,1]	t = [0,5]	t = [0,1]	t = [0,5]	t = [0,1]	t = [0,5]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Forced CEO turnover (d)	0.076*** (2.925)	0.097*** (2.711)	−0.000 (−0.009)	−0.016 (−0.478)	0.018 (0.776)	0.087*** (2.925)	0.068*** (2.875)	0.033 (1.106)
# of other board seats	0.028*** (4.052)	0.043*** (3.696)	0.029*** (4.127)	0.045*** (3.769)	0.016*** (3.302)	0.032*** (5.059)	0.021*** (3.958)	0.017*** (3.099)
Female (d)	−0.018 (−1.147)	0.024 (0.950)	−0.014 (−0.903)	0.019 (0.806)	0.001 (0.048)	0.018 (0.654)	−0.011 (−0.758)	0.035* (1.725)
Age (yrs), log	0.006 (0.107)	0.487*** (5.484)	−0.001 (−0.022)	0.478*** (5.343)	−0.220*** (−3.774)	−0.673*** (−8.410)	0.056 (1.001)	0.750*** (9.984)
Tenure (yrs), log	0.001 (0.134)	0.049*** (3.057)	0.001 (0.128)	0.052*** (3.322)	−0.016 (−1.541)	−0.067*** (−4.699)	0.000 (0.012)	0.074*** (5.709)
Turnover FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,013	3,300	4,013	3,300	4,013	3,300	4,013	3,300
Firms	465	404	465	404	465	404	465	404
Directors	2,885	2,425	2,885	2,425	2,885	2,425	2,885	2,425
Turnover-interlocked directorships	594	489	594	489	594	489	594	489
Adjusted R ²	0.055	0.089	0.046	0.081	0.017	0.084	0.042	0.086

This table reports results from ordinary least squares regressions of variables capturing gains and losses of directorships within t years after the turnover on a dummy variable set equal to one if a director was involved in a forced CEO turnover between the date of this re-election and the date of the previous election, and zero otherwise, as well as director control variables, turnover event fixed effects, and interlocked firm fixed effects. The dependent variable is a dummy variable set equal to one in case of a loss of directorships (Columns 1–4), a gain in directorships (Columns 5–6), or a net loss in directorships (Columns 7–8), and zero otherwise. In the regressions reported in Columns 3 and 4, turnover firms are omitted when constructing the dependent variable. The treatment sample comprises outside directorships of directors interlocked to forced CEO turnovers between 2003 and 2017. The control sample comprises outside directorships of directors 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.

Table 12

Forced CEO turnovers, changes in directorship quality, and exits from director labor market.

Dependent variable	Difference in the sum of total assets over all directorships		Exit from labor market (d)	
	$t_1 - t_0$	$t_5 - t_0$	$t = 1$	$t = 5$
	(1)	(2)	(3)	(4)
Forced CEO turnover (d)	−1.100*** (−3.504)	−1.785*** (−3.298)	0.121*** (4.525)	0.092*** (4.773)
Director control variables	Yes	Yes	Yes	Yes
Turnover FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Observations	4,013	3,300	4,019	3,305
Firms	465	404	465	404
Directors	2,885	2,425	2,887	2,426
Turnover-interlocked directorships	594	489	594	489
Adjusted R ²	0.026	0.102	0.115	0.057

This table reports results from ordinary least squares regressions of changes in total assets of all directorships (Columns 1 and 2) or a dummy variable indicating whether a director holds zero board seats within t years after the turnover (Columns 3 and 4) on a dummy variable set equal to one if a director was involved in a forced CEO turnover between the date of this re-election and the date of the previous election, and zero otherwise, as well as director control variables, turnover event fixed effects, and interlocked firm fixed effects. The treatment sample comprises outside directorships of directors interlocked to forced CEO turnovers between 2003 and 2017. The control sample comprises outside directorships of directors not interlocked to a forced turnover but share a board seat with a director interlocked to a forced CEO turnover at the turnover announcement. Director control variables are the same as in Table 11. 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.

CEO turnover do not differ between turnover-interlocked directors and non-turnover-interlocked directors sitting on the boards of the same firms.

A reputational loss may show not only in the number but also in the quality of the outside directorships. Board seats at larger firms are associated with higher compensation (Ryan and Wiggins, 2004), more power and prestige (Shivdasani, 1993; Adams and Ferreira, 2008; Masulis and 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 outside board seats in the first year after the forced CEO turnover

or five years after the turnover.³⁴ To account for the skewness in the distribution of these variables, we apply the inverse hyperbolic sine transformation (Burbidge et al., 1988).³⁵ Results are reported in Columns 1 and 2 of Table 12 and show that turnover-interlocked

³⁴ In Table IA.20 in the Internet Appendix, we replicate this analysis using the change in market capitalization as the dependent variable and find very similar results.

³⁵ Note that we cannot use the natural logarithm of the change in total assets as these changes can take on negative values.

Table A.1

Variable definitions.

Panel A: Director characteristics		
Variable	Definition	Source
% votes withheld	Fraction of votes withheld and against the re-election of the director; (<i>votedagainst</i> + <i>votedabstain</i>)/(<i>votedfor</i> + <i>votedagainst</i> + <i>votedabstain</i>)	ISS
≥15% votes withheld (d)	Dummy variable equal to one if 15% or more of the votes cast in a director's re-election are against her and were not against her at the previous election, and zero otherwise	ISS
Attends < 75% of meetings (d)	Dummy variable equal to one if the director attends less than 75% of all board meetings in a fiscal year, zero otherwise	ISS
ISS withhold/against (d)	Dummy variable equal to one if ISS recommends shareholders to withhold their votes or vote against the re-election of the director, zero otherwise	ISS
# of other board seats	Number of other outside board seats held by the director	BoardEx
Female (d)	Dummy variable equal to one if the director is female, zero otherwise	BoardEx/ISS
Age (yrs)	Age of the director	BoardEx/ISS
Tenure (yrs)	Time in years since the director joined the board	BoardEx/ISS
Monitoring committee (d)	Dummy variable equal to one if the director is a member of the nominating or compensation committee of the turnover firm at the turnover announcement date, zero otherwise	BoardEx/ISS
Co-opted director (d)	Dummy variable equal to one if the director became director of the turnover firm after the departing CEO was appointed, zero otherwise	BoardEx/ISS
≥ (<) <i>t</i> yrs until meeting date (d)	Dummy variable equal to one if the time between turnover announcement date and the next re-election date is larger or equal to (below) <i>t</i> years, zero otherwise	ISS
Leaves board (d)	Dummy variable equal to one if the director leaves the board of the turnover firm until the next re-election, zero otherwise	ISS
Panel B: Firm characteristics		
Variable	Definition	Source
Total assets	Total assets in million USD; <i>AT</i>	Compustat
Tobin's Q	Tobin's Q (market value of assets to book value); $(AT + CSHO * PRCC_F - CEQ - TXDB)/AT$. Missing values in <i>TXDB</i> 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 <i>t</i> – 270 to <i>t</i> – 21 with <i>t</i> = 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, as a fraction of board size	BoardEx
% busy outside directors	The percentage of outside directors with more than two board memberships, as a fraction of the number of outside directors	BoardEx
Institutional ownership (%)	The percentage of shares owned by 13F institutions, as a fraction of shares outstanding	Thomson Reuters
≥ (<) median (mean) common ownership (d)	Dummy variable equal to one if the common ownership between the interlocked firm and the turnover firm is at least equal to (below) the median (mean), zero otherwise	Thomson Reuters
Panel C: Events at interlocked firms		
Variable	Definition	Source
Forced CEO turnover (d)	Dummy variable equal to one if a director was involved in a forced CEO turnover at an interlocked firm, zero otherwise. A CEO departure is classified as forced 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.	Hand-collected (Factiva/BoardEx)
Unforced CEO turnover (d)	Dummy variable equal to one if a director was involved in a CEO turnover not classified as forced at an interlocked firm, zero otherwise	Hand-collected (Factiva/BoardEx)
Hired CEO and left before forced CEO turnover (d)	Dummy variable equal to one if the director hired the CEO who gets fired but left the turnover firm before the firing, zero otherwise	Hand-collected (Factiva/BoardEx)
Joined after forced CEO turnover (d)	Dummy variable equal to one if a director joined the board of the turnover firm after the departure of the CEO and before the date of her re-election at the interlocked firm, zero otherwise	Hand-collected (Factiva/BoardEx)
Sudden CEO death (d)	Dummy variable equal to one if a director was involved in a CEO turnover at an interlocked firm caused by the sudden death of the CEO, zero otherwise	Hand-collected (Factiva/BoardEx)
Sudden CEO death (extended) (d)	Dummy variable equal to one if a director was involved in a CEO turnover at an interlocked firm caused by the CEO's sudden death, zero otherwise. We also consider cases where CEOs take health-related leaves of absence and die within 30 days	Hand-collected (Factiva/BoardEx)
Restatement (d)	Dummy variable equal to one if a director was involved in a restatement at an interlocked firm, zero otherwise	Audit Analytics
Class action lawsuit (d)	Dummy variable equal to one if a director was involved in a class action lawsuit at an interlocked firm, zero otherwise	Securities Class Action Clearinghouse
Poison pill (d)	Dummy variable equal to one if a director was involved in the adoption of a poison pill at an interlocked firm, zero otherwise	Shark-Repellent
Bankruptcy (d)	Dummy variable equal to one if a director was involved in a bankruptcy filing at an interlocked firm, zero otherwise	Florida-UCLA-LoPucki

(continued on next page)

directors experience a significant decline in aggregate total assets represented by their board seats compared to non-turnover-interlocked directors. Hence, board seats lost, in particular at the turnover firm, tend to be replaced by board seats 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.

An even more drastic labor market consequence than losses of directorships or declines in the quality of directorships is a complete

Table A.1 (continued).

Panel D: CEO turnover characteristics		
Variable	Definition	Source
Successor announced (d)	Dummy variable equal to one if the company announces a full replacement with the departure of the outgoing CEO, zero otherwise	Hand-collected (Factiva/Boardex)
Performance-induced (d)	Dummy variable equal to one if the CEO turnover is performance-induced as estimated using the two-probit model by Jenter and Lewellen (2021), zero otherwise	See Table IA.3
Honeymoon stage (d)	Dummy variable equal to one if the outgoing CEO has a tenure of less than three years, zero otherwise	BoardEx/ISS
Harvest stage (d)	Dummy variable equal to one if the outgoing CEO has a tenure of between three and 13 years, zero otherwise	BoardEx/ISS
Decline stage (d)	Dummy variable equal to one if the outgoing CEO has a tenure of more than 13 years, zero otherwise	BoardEx/ISS
Panel E: Director labor market outcomes		
Variable	Definition	Source
Loss of directorship (d)	Dummy variable equal to one if the director loses any of her outside directorships within t years following the forced CEO turnover at an interlocked firm, zero otherwise	BoardEx
Gain of directorship (d)	Dummy variable equal to one if the director obtains a new outside directorship within t years following the forced CEO turnover at an interlocked firm, zero otherwise	BoardEx
Net loss of directorship (d)	Dummy variable equal to one if the director loses more outside directorships than she gains within t years following the forced CEO turnover at an interlocked firm, zero otherwise	BoardEx
Sum of total assets (market cap.) over all directorships	Sum of total assets (market capitalization) in million USD over all directorships a director holds; <i>AT</i>	BoardEx, Compustat
Exit from labor market (d)	Dummy variable equal to one if the director is alive and has no board memberships t years following the forced CEO turnover at an interlocked firm, zero otherwise	BoardEx

This table reports variable definitions. Database mnemonics are provided in italics (if available).

exit from the labor market. In our final set of tests, we thus study whether directors involved in a forced turnover have a higher propensity to leave the director labor market post-turnover. To this end, we construct a dummy variable equal to one if the total number of board seats held declines to zero and the director is still alive, and zero otherwise. Results in Columns 3 and 4 show that the probability of exiting the director labor market increases significantly following a forced turnover. Specifically, directors interlocked to a forced turnover are 12.1 (9.2) percentage points more likely to exit the director labor market one (five) year(s) after a forced turnover relative to directors on the same board not interlocked to a turnover. These findings provide additional evidence that involvement in a forced CEO turnover has adverse effects on director labor market opportunities.

In summary, results in this section provide some evidence consistent with directors' labor market opportunities being reduced following a forced CEO turnover and thus support our main finding of a reputational loss resulting from involvement in a forced turnover. However, it is important to note that all outcome variables used here may be subject to endogeneity concerns, in particular, that directors may choose to either terminate some board seats or attempt to solicit new ones for reasons unobservable to the researcher (Ertimur et al., 2012; Levit and Malenko, 2016).

6. Conclusion

Monitoring and, if necessary, firing the CEO of a corporation is one of the primary tasks of the board of directors (e.g., Fama, 1980; Fama and Jensen, 1983; Jenter and Kanaan, 2015; Jenter and Lewellen, 2021). Hence, directors' involvement in forced CEO turnovers can be expected to be assessed by the market and to affect directors' reputation. However, research studying the consequences of forced CEO turnovers on involved directors 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 directors, such as gains and losses in the number of board memberships, are subject to endogenous selection by directors (Levit and 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.

This paper establishes novel evidence on the reputational effects of forced CEO turnovers on directors involved in the turnover decision. Directors interlocked to a forced turnover experience an economically large and statistically significant increase in withheld votes at subsequent director re-elections, suggesting that directors involved in a forced turnover lose reputation. Additional tests show that directors are not penalized for involvement in a CEO turnover *per se* but for forced CEO turnovers that are related to governance failures by the board.

The results presented in this paper challenge the predominant 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 be detrimental to a director's reputation.

CRedit authorship contribution statement

Felix von Meyerinck: Writing – review & editing, Writing – original draft, Methodology, Formal analysis, Conceptualization. **Jonas Romer:** Writing – review & editing, Writing – original draft, Methodology, Data curation, Formal analysis, Conceptualization. **Markus Schmid:** Writing – review & editing, Writing – original draft, Supervision, Methodology, Conceptualization.

Declaration of competing interest

The corresponding author serves as a member of the advisory board of Inrate AG, Switzerland, a proxy advisory firm and ESG rating agency. He has no other conflicts of interest to disclose.

The first and second author declares that they have no conflicts of interest to disclose.

Appendix A

See Table A.1.

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