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Directors' career concerns: Evidence from proxy contests and board interlocks★

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Abstract

This paper studies the disciplinary spillover effects of proxy contests on companies that share directors with target firms, that is, interlocked firms. In difference-in-differences tests, I find that interlocked firms reduce excess cash holdings, increase shareholder payouts, cut CEO compensation, and engage in less earnings management in the year after proxy contests. The effects are more pronounced when both the interlocked and target firms have a unitary board and when the interlocking director is up for election, is younger, or has shorter tenure. Overall, the evidence highlights the importance of directors' career concerns in policy spillovers across firms with board interlocks.

JEL classification: G30; G34; G35

Keywords: Proxy contests; Board interlocks; Career concerns; Corporate governance

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1. Introduction

Proxy contests often focus on directorial positions, where activist shareholders nominate an alternative slate of directors in an attempt to replace incumbent board members. Given shareholders' limited ability to vote out directors in uncontested elections (e.g., Cai, Garner, and Walkling, 2009), proxy contests remain a powerful mechanism for director removal. In recent years, activists have become increasingly successful in accessing the US boardroom. According to FactSet, activists obtained board seats in 73% of proxy contests in 2014.[1]

At the firm level, prior research shows that proxy contests create shareholder value for target firms (e.g., Dodd and Warner, 1983; Mulherin and Poulsen, 1998; Fos, 2017). At the director level, however, proxy contests can impose significant career costs on individual directors. Existing evidence suggests that, following proxy contests, directors suffer losses of board seats not only at target firms but also at nontarget firms (Fos and Tsoutsoura, 2014). Despite the adverse effects of proxy contests on directors' careers, little is known about whether or how directors respond to proxy contests. Do directors change their behavior to mitigate potential career consequences? Do directors initiate policy changes at other firms where they also hold board seats, that is, interlocked firms? These remain open and important questions in the literature.

In this paper, I examine the impact of proxy contests on directors' behavior by analyzing how a proxy contest at one firm affects corporate policies at interlocked firms. On the one hand, proxy contests can threaten directors' board seats at interlocked firms. Thus, directors sitting on multiple boards could be motivated to act preemptively and strengthen corporate governance at interlocked firms to minimize the possibility of losing other board seats. I label this hypothesis the "career concerns hypothesis." On the other hand, proxy contests can distract directors and thus weaken corporate governance at interlocked firms. In particular,

[1] See https://www.wsj.com/articles/activists-are-on-a-roll-with-more-to-come-1420150089.

engaging with activists at target firms can distract directors and reduce the time and resources they allocate to interlocked firms. Prior studies show that distracted directors are detrimental to board monitoring (e.g., Fich and Shivdasani, 2006; Falato, Kadyrzhanova, and Lel, 2014). I label this hypothesis the "director distraction hypothesis."

To test these hypotheses, I analyze firm policy changes after a company with whom the firm shares at least one board member is targeted by an activist in a proxy contest, using a sample of US firms in the Institutional Shareholder Services database from 1998 to 2014. The analysis is based on a difference-in-differences design that matches each interlocked firm to another company with the closest market capitalization in the same industry. I find that, in the year after proxy contests, interlocked firms reduce excess cash holdings, increase shareholder payouts, cut CEO compensation, and engage in less earnings management. The evidence is consistent with improved corporate governance at interlocked firms.

Next, I investigate variation in the policy changes across interlocked firms. This analysis yields a number of interesting results. First, I find greater reductions in CEO compensation and earnings management when the interlocking director sits on the compensation and audit committees, respectively. These results are consistent with the view that interlocking directors initiate the policy changes. Second, I find that the policy changes are more prominent when both the interlocked and target firms have a unitary board and when the interlocking director is up for election at both firms. These results indicate that directors have stronger motivation to act preemptively when they face higher risk of removal. Third, using age and tenure as proxies for directors' implicit career incentives, I find that younger and shorter-tenured interlocking directors, who are earlier in their careers and are thus more concerned about the market's perception of their ability, induce greater policy changes. These heterogeneous effects collectively support the career concerns hypothesis.

I conduct several tests to assess the internal validity of these main results. To begin, I

examine the validity of the parallel trends assumption and show that interlocked firms and matched control firms display no differential time trends before proxy contests or around pseudo proxy contest years. I also examine the possibility that the main findings are confounded by endogenous firm-director matching based on unobservable characteristics. To address this concern, I conduct two falsification tests: I examine companies that have board interlocks with target firms only before (but not during) proxy contests and companies that have board interlocks with industry peers of target firms. To the extent that the main findings are driven by unobserved firm heterogeneity underlying board interlocks with target firms, these two groups of companies may also exhibit policy changes. However, I find that neither group exhibits policy reactions. The main results are also robust to controlling for the predicted probability of a proxy contest, mitigating omitted variables bias concerns, and to excluding interlocked firms with newly recruited interlocking directors, mitigating reverse causality concerns that interlocked firms intentionally appoint directors with proxy contest experience to implement desired changes.

The main results are unchanged after including various firm and board characteristics as well as year, industry-year, and firm fixed effects in the regressions. Moreover, the main results are robust to excluding interlocked firms in the same industry or state as target firms, controlling for the frequency of proxy contests in the firm's industry or state, controlling for the total number of the firm's board interlocks with other companies, focusing on outside interlocking directors, and using an alternative sample based on propensity score matching.

I then investigate whether the policy changes affect interlocking directors' careers in the three years after proxy contests. Consistent with the career concerns hypothesis, I find that directors are less likely to lose board seats at interlocked firms that make greater overall policy changes. I also find evidence that the policy changes lower the likelihood of a proxy contest at interlocked firms.

Finally, I examine the implications of the policy changes for shareholder value. I find that interlocked firms experience positive cumulative abnormal returns in the three-day window around announcements of proxy contests, suggesting that shareholders anticipate value-enhancing policy changes. Consistent with this view, I find that interlocked firms increase operational profitability after proxy contests, while their long-term investment policies are unaffected. Results of supplementary analyses indicate that the policy changes do not persist in the long run.

This paper provides novel evidence on the role of career concerns in aligning directors' incentives with those of shareholders. In his seminal work, Fama (1980) argues that the implicit incentives induced by agents' career concerns in the labor market can resolve moral hazard problems. Although directors rarely lose board seats in uncontested elections, proxy contests represent a unique situation in which directors' career concerns are substantially intensified. This paper contributes to the literature by exploring whether or how directors change their behavior following proxy contests. The results are consistent with directors facilitating policy changes that shareholders desire when facing increased career concerns.

This paper is related to the long-standing debate on the efficacy of shareholders' ability to remove directors. Insulation from the threat of removal may allow directors to deviate from maximizing shareholder value (Manne, 1965). Bebchuk (2005) advocates giving shareholders more power to replace directors. Fos, Li, and Tsoutsoura (2018) find that directors provide more effective monitoring when they are closer to elections. A strand of literature shows that staggered boards are associated with poor quality of governance and low firm valuation due to director entrenchment (e.g., Bebchuk and Cohen, 2005; Faleye, 2007; Cohen and Wang, 2013). However, there is controversy over the value implications of staggered boards (e.g., Amihud and Stoyanov, 2017; Cohen and Wang, 2017; Catan and Klausner, 2017). Some studies find that staggered boards enhance firm value by enabling managers to focus on

long-term investment and develop stakeholder relationships (e.g., Johnson, Karpoff, and Yi, 2015; Cremers, Litov, and Sepe, 2017). The results in this paper shed new light on the debate and show that the risk of removal by shareholders motivates directors to improve governance.

This paper also contributes to the ongoing debate on the influence of shareholder activism. Whereas Cremers et al. (2016) show that activists seek short-term gains at the expense of long-term value creation, Brav et al. (2008) and Bebchuk, Brav, and Jiang (2015) find contrary results. Some studies find that shareholder activism has positive long-term effects on firm productivity (Brav, Jiang, and Kim, 2015) and innovation efficiency (Brav et al., 2018). This paper offers new insight by showing that, after board members experience shareholder activism at other firms, companies increase payouts but do not reduce long-term investment. The findings in this paper are inconsistent with shareholder-driven short-termism.

Furthermore, this paper adds to a burgeoning literature that establishes that the impact of shareholder activism can reach beyond target firms. Some recent studies find that shareholder activism has adverse effects on industry peers of target firms (Aslan and Kumar, 2016) and peer firms' bondholders (Feng, Xu, and Zhu, 2018). Another strand of literature finds that shareholder activism causes policy changes at industry peers of target firms (e.g., Bourveau and Schoenfeld, 2017; Gantchev, Gredil, and Jotikasthira, 2019). While existing studies focus on industry spillovers, this paper examines a completely different network through which the disciplinary effects of shareholder activism can spill over to nontarget firms—the board interlocking network.

Last, this paper expands the literature on board interlocks between firms. Prior research shows that governance practices can propagate through common board members (Bouwman, 2011). The board interlocking network is also found to promote the propagation of other corporate practices, such as option backdating (Bizjak, Lemmon, and Whitby, 2009), earnings management (Chiu, Teoh, and Tian, 2012), and tax avoidance (Brown and Drake,

2013). This paper adds to the literature by showing that a proxy contest at one firm induces policy changes at interlocked firms. In addition, this paper complements previous work that finds a positive association between boardroom connections and firm performance (e.g., Larcker, So, and Wang, 2013; Field, Lowry, and Mkrtchyan, 2013). The results in this paper illustrate a specific channel through which board interlocks could create shareholder value.

The rest of the paper is organized as follows. Section 2 describes the data and sample. Section 3 examines interlocked firms' policy changes following proxy contests. Section 4 investigates the effects of policy changes on interlocking directors' careers. Section 5 presents an event study of proxy contest announcements. Section 6 concludes.

2. Data and sample

To construct the sample, I begin with the universe of firms in the Institutional Shareholder Services (ISS, formerly RiskMetrics) database between 1998 and 2014. From the ISS database, I collect information on individual directors of Standard and Poor's (S&P) 1500 firms, including directors' independence status, age, tenure, shareholdings, and board committee memberships. The sample period begins in 1998 because some director information is largely missing prior to that year. I obtain financial data from the Compustat database, stock return data from the Center for Research in Security Prices (CRSP) database, and CEO compensation data from the ExecuComp database.

Information on proxy contests for board seats is retrieved from the Securities Data Company (SDC) Platinum. From the Proxy Fights database in the SDC Platinum's Corporate Governance module, I obtain a list of US firms targeted in proxy contests announced between January 1, 1999 and December 31, 2013.[2] Of these proxy contests, 28% were won or partially won by activists, 17% were won by management, 28% were settled, 13% were

[2] Because the ISS sample is from 1998 to 2014 and the main analysis focuses on a three-year period from one year before to one year after a proxy contest, I define the proxy contest sample from 1999 to 2013.

withdrawn, and 14% were unresolved or pending.[3] After merging the ISS data set with the proxy contest sample, I identify companies that share directors with target firms, that is, interlocked firms. Fig. 1 displays the number of target firms and the number of interlocked firms by year. The pattern suggests that proxy contests became more frequent over the sample period. In a given year, 1.9% of firms were targeted in a proxy contest.

I use a difference-in-differences design to study the effects of proxy contests on interlocked firms. Treatment firms are defined as companies that share directors with target firms in the year of proxy contests. In the initial sample, treatment firms tend to be larger than the rest of firms. The average market capitalization of treatment firms is double that of other firms. To control for heterogeneity related to firm size and industry, I construct a matched sample. Each treatment firm is matched to a control firm in the same Fama-French 48 industry that has the closest market capitalization in the year before proxy contests and does not share directors with target firms.[4] The matched sample contains treatment and control firms for three years: the year before, year of, and year after a proxy contest. By comparing policy changes between treatment and control firms, the difference-in-differences design helps isolate the causal effects of board interlocks with target firms.

Table 1, Panel A presents summary statistics for the matched sample. I exclude dual-class firms from the analysis because these firms are rarely targeted in proxy contests.[5] Observations with missing firm or board data are also excluded. After imposing the data requirements, I obtain a matched sample of 5,922 firm-years. On average, the firm's market capitalization is $4.7 billion and the board comprises ten members. All continuous variables are winsorized at the 1st and 99th percentiles to reduce the influence of outliers. Detailed

[3] The distribution of proxy contest outcomes is comparable to that reported in Fos (2017).

[4] In a robustness test, I use propensity score matching to construct an alternative sample and show that the results are robust to a more comprehensive matching algorithm. See Section 3.5 for detailed discussion.

[5] Only 3% of proxy contests in the sample involve a dual-class target firm. Activists also have a lower likelihood of winning proxy contests when target firms have a dual-class structure.

definitions of all variables are provided in the Appendix.

Before conducting difference-in-differences tests, I assess the covariate balance between treatment and control firms. Table 1, Panel B reports the mean values of firm and board characteristics in the year before proxy contests for treatment and control firms. Firm characteristics include market capitalization, market-to-book ratio, stock return, and return volatility. Board characteristics include board size, board independence, CEO duality, director ownership, director busyness, and director co-option. In addition, I compare the predicted probability of a proxy contest between treatment and control firms, as a higher threat of activism might induce policy changes. I estimate the probability of a proxy contest using a pooled probit model, in which explanatory variables include all aforementioned firm and board characteristics as well as year and industry indicators.[6] The results suggest that treatment and control firms are reasonably similar ex ante. The pre-contest differences in means are indistinguishable from zero, except for the slightly higher presence of independent and busy directors on the boards of treatment firms. To control for the remaining heterogeneity, I include all firm and board characteristics in the regressions.

3. The effects of proxy contests on interlocked firms

I begin with the main analysis of four corporate policies: cash holdings, shareholder payouts, executive compensation, and financial reporting.

3.1. Main results

Using the matched sample of treatment and control firms over the three-year period around proxy contests, I estimate the following model:

[6] The results are similar using an alternative specification that includes market capitalization, market-to-book ratio, stock return, institutional ownership, trading illiquidity, year, and industry indicators. The selection of these explanatory variables is based on findings in prior studies about the characteristics of activism target firms (e.g., Brav et al., 2008; Fos, 2017).

$$y_{i,t} = \alpha + \beta_1 Treatment_{i,t} \times Post_{i,t} + \beta_2 Treatment_{i,t} + \beta_3 Post_{i,t} + \varepsilon_{i,t}, \quad (1)$$

where y is one of the corporate policies, *Treatment* is an indicator equal to one for treatment firms and zero otherwise, and *Post* is an indicator equal to one for the year after a proxy contest and zero otherwise. The coefficient on the interaction term *Treatment*×*Post*, β_1, captures the policy changes of treatment firms relative to those of control firms in the year after proxy contests.

Table 2 presents the regression results. Each panel corresponds to a different outcome variable. In Column 1, the specification is the same as the baseline model in Eq. (1). In Column 2, I augment the baseline model with various firm and board characteristics measured in the previous year. Year fixed effects are also included to control for market-wide time trends.[7] In Column 3, I include industry-year fixed effects to control for industry-specific time trends. Industry is defined by the Fama-French 48 industry classification. In Column 4, I include the lagged dependent variable. In Column 5, I add firm fixed effects to control for time-invariant firm heterogeneity. Standard errors are adjusted for heteroskedasticity and two-way clustering by both firm and year (Petersen, 2009).[8]

The first corporate policy is cash holdings. In Panel A of Table 2, the dependent variable *Excess cash* is defined as residuals from the cash holdings model developed by Dittmar and Mahrt-Smith (2007), in which explanatory variables include assets, working capital, cash flow, industry standard deviation of cash flow, market-to-book ratio, research and development intensity, and year indicators. These covariates proxy for "legitimate" reasons of why firms hold cash. In other words, *Excess cash* is the difference between actual cash holdings and the predicted normal level of cash holdings. Excess cash reserves are held above those needed for operations and investments and are therefore at managers' discretion. Details

[7] Year fixed effects and the indicator *Post* can be simultaneously included in the same specification because treatment firms are treated at different points in time.

[8] Table A.1 in the Online Appendix shows that the results are robust to adjusting standard errors for clustering at only the firm level.

on the estimation of *Excess cash* are provided in the Appendix.

The results in Table 2, Panel A suggest that interlocked firms reduce excess cash holdings in the year following proxy contests. Across all specifications, the coefficient on the interaction term *Treatment*×*Post* is negative and statistically significant. The estimates in Column 5 imply that treatment firms decrease the level of *Excess cash* by 6.6% relative to the sample mean, consistent with greater restraint on managers from reserving excessive cash.

The second corporate policy is shareholder payouts. In Panel B of Table 2, the dependent variable *Shareholder payout* is defined as the total amount of dividends and share repurchases divided by the lagged market value of equity. Dividends are defined as cash dividends declared on common stock. Repurchases are defined as the purchase of common and preferred stock minus any reduction in the value of preferred stocks outstanding.

The results in Table 2, Panel B suggest that interlocked firms distribute more cash to shareholders after proxy contests. The coefficient on the interaction term *Treatment*×*Post* is positive and statistically significant across all specifications. The estimates indicate that treatment firms increase shareholder payouts by 0.4–0.7 percentage points, representing an increase of 9%–16% relative to the sample mean. The economic magnitude is large, in light of the finding in Brav et al. (2008) that companies targeted by hedge fund activists increase payouts by 0.3–0.5 percentage points. Table A.2, Panel A in the Online Appendix shows that treatment firms increase both dividends (about 0.2 percentage points) and repurchases (about 0.4 percentage points).

The third corporate policy is executive compensation. In Panel C of Table 2, the dependent variable *Total compensation* is defined as the natural logarithm of CEO annual total compensation. Across all specifications, the coefficient on the interaction term *Treatment*×*Post* is negative and statistically significant. The estimates in Column 5 show that treatment firms reduce CEO compensation by 5.1%, which translates into a reduction of

$0.39 million in annual compensation for an average CEO in the sample. The effect is economically meaningful.

Table A.2, Panel B in the Online Appendix reports results on the structure of CEO compensation. I find that treatment firms reduce cash compensation (salary and bonus) and equity compensation (stocks and option grants) by 7.5% and 4.3%, respectively. I also find that treatment firms increase the proportion of stocks and option grants in CEO compensation. The results suggest that, after proxy contests, interlocked firms cut executive compensation but increase the sensitivity of executive compensation to shareholder wealth.

The fourth corporate policy is financial reporting. I use *Discretionary accruals*, the absolute value of performance-matched discretionary accruals, as the primary measure of earnings management. To construct this measure, I use the modified Jones (1991) model developed by Dechow, Sloan, and Sweeney (1995), in which the dependent variable is total accruals and independent variables include the inverse of assets, the change in sales net of the change in accounts receivables, and property, plant, and equipment. The modified Jones model is estimated for each industry-year group based on the Fama-French 48 industry classification. Following Kothari, Leone, and Wasley (2005), I define performance-matched discretionary accruals as the difference in residuals between a given firm and another firm with the closest return on assets in the same industry-year. Details on the estimation of *Discretionary accruals* are provided in the Appendix.

Table 2, Panel D reports results from regressions of *Discretionary accruals*. I find that interlocked firms engage in less earnings management after proxy contests. Across all specifications, the coefficient on the interaction term *Treatment*×*Post* is negative and statistically significant. The estimates in Column 5 indicate that treatment firms lower the level of *Discretionary accruals* by 23.7% relative to the sample mean, consistent with improved quality of financial disclosure.

For robustness, I consider two alternative measures of earnings management. The first is the absolute value of discretionary accruals estimated from the modified Jones model without performance matching. The second is an indicator equal to one if the firm's reported earnings per share meets or beats the most recent analyst consensus forecast by up to one cent and zero otherwise (e.g., Graham, Harvey, and Rajgopal, 2005; Bhojraj et al., 2009; Fang, Huang, and Karpoff, 2016).[9] Table A.2, Panel C in the Online Appendix shows that inferences are unchanged using the alternative measures of earnings management.

Overall, the evidence in Table 2 suggests improved corporate governance at interlocked firms, consistent with the career concerns hypothesis.[10]

3.2. Interlocking directors' board committee memberships

Next, I examine whether interlocking directors initiate the policy changes by exploiting variation in their board committee memberships. Table 3 reports these results.

First, I consider the compensation committee. Because compensation committee members are in charge of setting executive compensation, they receive lower votes in director elections and lose more board seats for poor monitoring of executive compensation (Cai, Garner, and Walkling, 2009; Ertimur, Ferri, and Maber, 2012). I expect a larger reduction in CEO compensation if the interlocking director sits on the compensation committee. To test this prediction, I separate treatment firms into two groups and reestimate the regression of *Total compensation*. In Column 1 of Table 3, the variable *Treatment_committee member* indicates treatment firms for which the interlocking director is a compensation committee member at both the interlocked and target firms, and the variable *Treatment_not committee member* indicates the rest of treatment firms. The regression includes the two separate indicators and their interaction terms with the indicator *Post*. I find that the coefficient on

[9] I collect information on analyst forecasts from the Institutional Brokers' Estimate System (I/B/E/S) database.

[10] Alternatively, directors may entrench themselves by taking defensive measures such as classifying the board. See Boyson and Pichler (2018) for discussion of firms' hostile resistance to activism.

Treatment_committee member×*Post* is negative and significant at the 1% level, whereas the coefficient on *Treatment_not committee member*×*Post* is negative but insignificant at conventional levels.

I obtain similar results on individual components of CEO compensation. Table A.2, Panel B in the Online Appendix shows that reductions in both cash and equity compensation are highly statistically significant when the interlocking director is a compensation committee member and insignificant otherwise. The increase in the proportion of stocks and option grants in CEO compensation is also larger when the interlocking director serves on the compensation committee.

Second, I consider the audit committee. Because audit committee members oversee financial reporting, they are most likely to suffer career consequences of financial reporting failures (Srinivasan, 2005; Brochet and Srinivasan, 2014). I expect a greater reduction in earnings management if the interlocking director sits on the audit committee. To test this conjecture, I separate treatment firms into two groups and reestimate the regression of *Discretionary accruals*. In Column 2 of Table 3, the variable *Treatment_committee member* indicates treatment firms for which the interlocking director is an audit committee member at both the interlocked and target firms, and the variable *Treatment_not committee member* indicates the rest of treatment firms. I find that the coefficient on *Treatment_committee member*×*Post* is negative and significant at the 1% level, whereas the coefficient on *Treatment_not committee member*×*Post* is negative but insignificant at conventional levels.

Overall, the results in Table 3 suggest that the decreases in CEO compensation and earnings management are concentrated in companies with the interlocking director sitting on the compensation and audit committees, respectively. These heterogeneous effects are consistent with the policy reactions being triggered by interlocking directors.

3.3. Interlocking directors' career concerns

To further investigate the career concerns hypothesis, I exploit variation in the strength of interlocking directors' career concerns. First, I examine board structure and director election cycles. Second, I examine directors' personal characteristics, namely, age and tenure. Table 4 reports these results.

A staggered board consists of multiple classes of directors, with only one class (usually one-third of the board) up for election each year. As a result, the staggered board structure gives directors more protection from being voted out than the unitary board structure. If interlocking directors sit on staggered boards, they may have weaker incentives to promote policy changes. To test this prediction, I separate treatment firms into two groups based on board structure. In Panel A of Table 4, the variable *Treatment_unitary board* indicates treatment firms for which both the interlocked and target firms have a unitary board, and the variable *Treatment_staggered board* indicates the rest of treatment firms. The regressions include the two separate indicators and their interactions with the indicator *Post*. The dependent variables are *Excess cash* in Column 1, *Shareholder payout* in Column 2, *Total compensation* in Column 3, and *Discretionary accruals* in Column 4. I reestimate the regressions and find clear differences between the two groups. The coefficient on *Treatment_unitary board*×*Post* is statistically significant in all regressions, while the coefficient on *Treatment_staggered board*×*Post* is smaller in magnitude and insignificant at conventional levels. The results suggest that interlocked firms exhibit stronger policy reactions when boards have a unitary structure.

Next, I examine individual directors' election cycles. Because the staggered board structure allows only a fraction of directors to be elected at each annual shareholder meeting, it generates heterogeneous director exposure to elections. Fos and Tsoutsoura (2014) show that, following proxy contests, nominated directors experience more severe losses of outside

directorships than non-nominated directors.[11] If interlocking directors face an election and thus higher risk of removal, they may have stronger motivation to initiate policy changes. To test this conjecture, I identify interlocking directors who are nominated for election and partition treatment firms into two groups accordingly. In Panel B of Table 4, the variable *Treatment_up for election* indicates treatment firms for which the interlocking director is up for election at both the interlocked and target firms, and the variable *Treatment_not up for election* indicates the rest of treatment firms. The results suggest that treatment firms exhibit significant policy changes only when the interlocking director is up for election. To the extent that the election cycles are predetermined, this test further helps cleanly identify the effects of directors' career concerns.

In addition, I consider variation in directors' age and tenure. Holmström (1982) argues that agents tend to work harder early in their careers when the market is still assessing their ability. Gibbons and Murphy (1992) show that younger managers receive lower explicit incentives from compensation contracts because they have higher implicit incentives from career concerns. Chevalier and Ellison (1999), Hong, Kubik, and Solomon (2000), and Yermack (2004) find that the sensitivity of job termination to performance is higher for younger mutual fund managers, younger security analysts, and directors with shorter tenure, respectively. Following these prior studies, I use age and tenure as proxies for directors' career incentives. I expect that younger and shorter-tenured directors have stronger career incentives and therefore induce greater policy changes.

In Panel C of Table 4, treatment firms are separated into two groups based on the interlocking director's age at the time of proxy contests. The variable *Treatment_young* indicates treatment firms for which the interlocking director's age is below the median (63 years), and the variable *Treatment_old* indicates the rest of treatment firms. The results

[11] Fos and Tsoutsoura (2014) explore two potential mechanisms for this incremental effect—nominated directors receive more media coverage and face the possibility of being voted out by shareholders.

suggest that only treatment firms with younger interlocking directors make significant changes in *Shareholder payout*, *Total compensation*, and *Discretionary accruals*. The change in *Excess cash* is also larger and more statistically significant at treatment firms with younger interlocking directors.

An alternative is to examine directors' tenure. Directors with shorter tenure could be more sensitive to employment risk compared to those with longer tenure. In Panel D of Table 4, I separate treatment firms into two groups based on the interlocking director's tenure at the time of proxy contests. The variable *Treatment_short tenure* indicates treatment firms for which the interlocking director's tenure at the interlocked firm is below the median (seven years), and the variable *Treatment_long tenure* indicates the rest of treatment firms. The results suggest that, although the two groups exhibit similar changes in *Excess cash*, only treatment firms with shorter-tenured interlocking directors are associated with significant changes in *Shareholder payout*, *Total compensation*, and *Discretionary accruals*.

Overall, the evidence in Table 4 supports the career concerns hypothesis. The policy changes are concentrated in companies with interlocking directors who face higher risk of removal and have stronger career incentives. It is difficult to reconcile these heterogeneous effects with alternative explanations. In the next section, I conduct several analyses to evaluate the validity of the empirical design.

3.4. Validity of empirical design

The key identifying assumption in a difference-in-differences design is that treatment and control firms should display common time trends in the absence of the treatment, that is, the parallel trends assumption. This assumption is more likely to hold if treatment assignment is not correlated with omitted variables that simultaneously determine the outcomes of interest. The results in Table 2 are reassuring. With the incremental inclusion of various controls and

fixed effects from Columns 1 to 5, the adjusted *R*-squared increases significantly, but the treatment effect estimate remains quite stable. Insofar as the included controls correlate with relevant omitted variables, such a pattern alleviates concerns that the main results are driven by correlated omitted variables (Oster, 2019). Nevertheless, in this section, I provide further tests of the validity of the empirical design, examining the parallel trends assumption as well as the possibility of endogeneity.

3.4.1. Parallel trends assumption

If the parallel trends assumption holds, treatment and control firms should not display differential time trends before proxy contests. Panel A of Table 5 reports the mean values of differences in policy changes between treatment and control firms in the three years preceding proxy contests. I find that the pre-contest differences in policy changes between the two groups are indistinguishable from zero. There is no evidence of preexisting divergences of corporate policies.

Table 5, Panel B reports results from regressions estimated over the [−1, +1] period around pseudo proxy contest years, which are arbitrarily set as three years before or after actual proxy contest years. The variable *Pseudo post* indicates the year following a pseudo proxy contest year. I find that the coefficient on the interaction term *Treatment*×*Pseudo post* is insignificant in all regressions, suggesting that treatment and control firms exhibit similar policy changes around pseudo proxy contest years. The findings in Panels A and B of Table 5 support the validity of the parallel trends assumption and the credibility of the difference-in-differences design.

3.4.2. Endogeneity

Board composition is endogenously determined (Hermalin and Weisbach, 1998, 2003). To further establish causality, I perform two falsification tests. First, I investigate companies

that have board interlocks with target firms only before (but not during) proxy contests. Second, I investigate companies that have board interlocks with industry peers of target firms. If the main findings are driven by endogenous firm-director matching, these two groups may also exhibit policy changes.

In Panel C of Table 5, the variable *Pseudo treatment* indicates companies that share directors with target firms in the year before but not in the year of proxy contests. I construct a matched sample for *Pseudo treatment* firms as in the main analysis and reestimate the regressions. I find that the coefficient on the interaction term *Pseudo treatment*×*Post* is insignificant in all regressions. There is no evidence of policy changes at *Pseudo treatment* firms around proxy contests.

In Panel D of Table 5, the variable *Target peer interlock* indicates companies that share directors with industry peers of target firms. Industry peers are defined as firms in the same Fama-French 48 industry that have the closest market capitalization in the year before proxy contests.[12] Using a matched sample for *Target peer interlock* firms, I repeat the analysis and find that the coefficient on the interaction term *Target peer interlock*×*Post* is insignificant. The results suggest that *Target peer interlock* firms do not exhibit policy changes around proxy contests.

I also examine specific alternative channels that could confound the main findings. For example, the policy changes at interlocked firms might be explained by a higher threat of shareholder activism.[13] In Panel E of Table 5, I explicitly control for the threat of activism, measured by the predicted probability of a proxy contest. As defined in Section 2, the probability of a proxy contest is estimated using a pooled probit model, in which explanatory variables include all firm and board characteristics as well as year and industry indicators. I

[12] I obtain similar results when I define industry peers as firms in the same Fama-French 48 industry that belong to the same *Market capitalization* and *Market-to-book* quintile as target firms, where quintile ranks are determined within each industry in the year prior to proxy contests.

[13] Fos (2017) shows that corporate policies are affected by the ex ante likelihood of a proxy contest.

find that the results are robust to the inclusion of this control variable.

Another potential endogeneity problem is that interlocked firms may appoint directors with proxy contest experience to implement the policy changes, that is, reverse causality. This explanation, however, is highly unlikely, because interlocking directors by definition experience proxy contests after joining the boards of interlocked firms.[14] Panel F of Table 5 shows that the results are virtually unchanged when I exclude treatment firms that recruit interlocking directors one year before proxy contests. Overall, the results in Panels C–F of Table 5 mitigate concerns about endogenous firm-director matching based on correlated omitted variables and reverse causality.

3.5. Robustness tests

To further evaluate the robustness of the main findings, I conduct a battery of tests and report these results in Table A.3 in the Online Appendix.

First, I consider the possibility that the policy changes are driven by industry spillover effects of proxy contests. Gantchev, Gredil, and Jotikasthira (2019) show that shareholder activism induces industry peers of target firms to undertake policy changes. However, I find that fewer than 10% of treatment firms are in the same industry as target firms. Panels A and B of Table A.3 show that the results are robust to excluding treatment firms in the same Fama-French 48 industry as target firms as well as controlling for the total number of proxy contests in the firm's industry.

Second, I consider the possibility that the policy changes are driven by local spillover effects of proxy contests. If companies that share board members tend to be geographically proximate, the policy changes may reflect treatment firms' responses to local proxy contests. However, I find that 74% of treatment firms are headquartered in a different state from target

[14] The median tenure of interlocking directors is seven years at the time of proxy contests.

firms.[15] Panels C and D of Table A.3 show that the results are robust to excluding treatment firms located in the same state as target firms as well as controlling for the total number of proxy contests in the firm's state.

Third, I consider the degree of board connectedness. Compared to control firms, treatment firms may share directors with more companies in general. Panel E of Table A.3 shows that the results remain unchanged after controlling for the total number of the firm's board interlocks with other companies.

Fourth, I focus on outside interlocking directors. Because outside directors are primarily responsible for the board's monitoring function, I expect stronger policy reactions at treatment firms with outside interlocking directors. Consistent with this prediction, Panel F of Table A.3 shows that treatment firms make significant policy changes only when the interlocking director is an outside director at both the interlocked and target firms.

Fifth, I employ the approach of propensity score matching to construct an alternative sample. Specifically, I estimate a probit model in which the dependent variable is an indicator for treatment firms and independent variables include all firm and board characteristics. In the year before proxy contests, each treatment firm is matched to a control firm with the closest propensity score in the same Fama-French 48 industry. Panel G of Table A.3 reports that the results are similar using this alternative matched sample.

3.6. Supplementary analyses

In this section, I provide supplementary analyses of interlocked firms' operational performance, long-term investment, and persistence of policy changes.

Prior research shows that, following poor firm performance, directors receive lower votes in elections (e.g., Cai, Garner, and Walkling, 2009) and are more likely to lose board seats (e.g., Yermack, 2004; Iliev et al., 2015). I expect that interlocking directors have incentives to

[15] I obtain historical information on the location of firm headquarters from the SEC Analytics Suite database.

improve operational profitability of interlocked firms. Columns 1 and 2 of Panel A in Table 6 report results from regressions of two operational performance measures. The first is *Return on assets*, defined as operating income before depreciation divided by lagged total assets. The second is *Return on net operating assets*, defined as operating income before depreciation divided by lagged net operating assets. The two measures are adjusted for the Fama-French 48 industry median.[16] In both columns, the coefficient on the interaction term *Treatment*×*Post* is positive and statistically significant, suggesting that interlocked firms also achieve improvement in operational performance.

In Columns 3 and 4 of Panel A in Table 6, I examine whether interlocked firms' long-term investment policies are affected. Policy changes such as payout increases may reduce resources available for long-term investment. Cremers et al. (2016) find decreases in long-term investment when companies respond to activists' demands. Therefore, I test whether interlocked firms sacrifice investment in long-term growth to please shareholders with larger payouts. The dependent variable in Column 3 is *Capital expenditures*, defined as capital expenditures divided by lagged total assets.[17] The dependent variable in Column 4 is *R&D expense*, defined as the research and development expense divided by lagged total assets. I find that the coefficient on the interaction term *Treatment*×*Post* is insignificant in both regressions. These results facilitate the interpretation that interlocked firms finance the increased payouts with internal funds instead of real investment cuts. The evidence is consistent with the notion that interlocked firms disgorge excess cash to shareholders and reduce agency costs of free cash flow (Jensen, 1986).

In Panel B of Table 6, I analyze the persistence of interlocked firms' policy reactions. I reestimate the regressions over an extended period from three years before to three years after a proxy contest. To track the effect over time, I construct three variables *Post +1*, *Post +2*,

[16] The results are similar without the industry adjustment.

[17] The results are robust to using *Net capital expenditures* as the dependent variable, defined as capital expenditures minus the sale of property divided by lagged total assets.

and *Post +3*, which indicate the first, second, and third years after a proxy contest, respectively. The three separate indicators and their interactions with the indicator *Treatment* are included in the regressions. The results suggest that interlocked firms exhibit significant policy changes in the year immediately after proxy contests, but the policy changes do not appear to persist in following years.

4. The effects of policy changes on interlocking directors' careers

In this section, I investigate whether the policy changes affect interlocking directors' careers after proxy contests. The career concerns hypothesis predicts that the policy changes should alleviate the adverse career consequences of proxy contests for directors. To test this prediction, I examine whether interlocking directors are more likely to retain board seats if interlocked firms exhibit greater policy changes.

I use a variable *Improvement index* to summarize changes in different policies. To construct this variable, I first compute the firm's change in each policy. For *Shareholder payout*, the change is measured as the level of increase. For *Excess cash*, *Total compensation*, and *Discretionary accruals*, the change is measured as the level of decrease. I then sort interlocked firms into quintiles by the change in each policy. The variable *Improvement index* is defined as the firm's average quintile rank of all policy changes, ranging from zero to four with a mean value of two.

Table 7 reports results on interlocking directors' career consequences. The sample includes interlocked firms over the [−3, +3] period, where year 0 is the proxy contest year.[18] In Panel A of Table 7, I estimate a linear probability model, in which the dependent variable is an indicator equal to one if the interlocking director remains on the board of the interlocked firm and zero otherwise and independent variables include *Post*, *Improvement index*, the

[18] Because directors on staggered boards are appointed for three-year terms, a three-year period following proxy contests ensures that each director faces at least one reappointment decision.

interaction *Post×Improvement index* as well as firm, board, and interlocking director characteristics. To control for unobserved heterogeneity underlying director turnovers, I add year, industry-year, proxy contest event, and firm fixed effects in different specifications. The coefficient on *Post×Improvement index* captures the effect of policy changes on the likelihood of directors retaining board seats at interlocked firms after proxy contests.

Across all specifications, the coefficient on *Post* is negative and significant at the 1% level, consistent with the finding in Fos and Tsoutsoura (2014) that directors lose outside directorships following proxy contests. The estimates indicate that 16.9%–22.7% of interlocking directors are removed from the boards of interlocked firms that make the lowest level of policy changes (i.e., *Improvement index* = 0). More important, the coefficient on *Post×Improvement index* is positive and statistically significant, corroborating the prediction that policy changes mitigate the adverse career consequences of proxy contests for directors. At interlocked firms that make the highest level of policy changes (i.e., *Improvement index* = 4), only 4.1%–11.9% of interlocking directors lose board seats.[19] The estimates in Column 4 imply that an increase in overall policy changes from the lowest to the highest level leads to a 65.2% decrease in the probability of director removal. The effect is economically significant.

In Panel B of Table 7, I examine the likelihood of proxy contests at interlocked firms. The regression specifications are the same as those in Panel A of Table 7, except that the dependent variable is an indicator equal to one if the interlocked firm is targeted in a proxy contest and zero otherwise. I find that the coefficient on *Post×Improvement index* is negative and statistically significant across all specifications. The estimates suggest that the probability of a proxy contest at interlocked firms decreases with the level of policy changes. If interlocking directors bring about greater policy changes, they are less likely to be challenged by activist shareholders again in the future.

[19] At the highest level of policy changes, the probability of director removal is significant at the 5% level in Columns 1 and 2 and insignificant at conventional levels in Columns 3 and 4.

Overall, Table 7 presents evidence that the policy changes protect interlocking directors' board seats and prevent proxy contests at interlocked firms. These findings in turn reinforce the proposition that interlocking directors' career concerns are the motivating force for the policy changes.

5. Stock price reaction to proxy contest announcements

In the final analysis, I attempt to shed light on the value implications of the policy changes. Table 8 reports cumulative abnormal returns (CARs) of both target and interlocked firms around announcements of proxy contests. I consider a three-day window [−1, +1], where day 0 is the proxy contest announcement date. To estimate abnormal returns, I use both the market model and the four-factor model (the Fama-French three factors plus the momentum factor). I search the Factiva database in the three-day window and exclude observations with confounding events (e.g., earnings announcements) from the analysis.

Table 8, Panel A shows that stock prices of target firms react positively to announcements of proxy contests. The mean (median) CAR of target firms is 4.453% (2.853%) using the market model and 4.357% (2.717%) using the four-factor model, all of which are significant at the 1% level. Consistent with previous studies, the results indicate that proxy contests create shareholder value for target firms.[20]

I also observe a positive stock price reaction of interlocked firms to announcements of proxy contests. Table 8, Panel B shows that the mean (median) CAR of interlocked firms is 0.508% (0.371%) using the market model and 0.444% (0.321%) using the four-factor model, all of which are significant at the 1% level. These results are consistent with the view that shareholders anticipate value-enhancing policy changes at interlocked firms.

[20] Dodd and Warner (1983) report an average CAR of 1.2% over the [−1, 0] window for 87 proxy contests in 1962–1978. DeAngelo and DeAngelo (1989) report an average CAR of 4.85% over the [−1, 0] window for 60 proxy contests in 1978–1985. Fos (2017) reports an average CAR of 6.5% over a two-month window for 1,061 proxy contests in 1994–2012.

6. Conclusion

This paper investigates whether the disciplinary effects of proxy contests spill over to companies that share board members with target firms. I find that interlocked firms reduce excess cash holdings, increase shareholder payouts, cut CEO compensation, and engage in less earnings management in the year after proxy contests. The effects are more pronounced when both the interlocked and target firms have a unitary board and when the interlocking director is up for election, is younger, or has shorter tenure. Furthermore, I find that the policy changes protect interlocking directors' board seats and prevent shareholder activism at interlocked firms. Finally, I find that stock prices of interlocked firms react positively to the announcements of proxy contests. These findings collectively suggest that the risk of removal creates shareholder value by strengthening directors' career concerns. The evidence in this paper has implications for both investors and regulators and informs public debate about corporate governance reforms aimed at empowering shareholders to replace directors (e.g., proxy access).

Appendix

In this appendix, I define the variables used in the analyses.

1. Dependent variables

Excess cash is the residuals (including firm fixed effects) from the cash holdings model:

$$Ln\left(\frac{Cash_{i,t}}{NA_{i,t}}\right) = \beta_0 + \beta_1 Ln\left(NA_{i,t}\right) + \beta_2 \frac{FCF_{i,t}}{NA_{i,t}} + \beta_3 \frac{NWC_{i,t}}{NA_{i,t}} + \beta_4 IndustrySigma_{i,t}$$
$$+\beta_5 \frac{MV_{i,t}}{NA_{i,t}} + \beta_6 \frac{RD_{i,t}}{NA_{i,t}} + Year\ \ dummies + Firm\ \ fixed\ \ effects + \mu_{i,t},$$

where *Cash* is cash and short-term investments, *NA* is net assets (total assets minus cash), *FCF* is free cash flow (operating income before depreciation minus interest expense minus income taxes), *NWC* is net working capital (current assets minus current liabilities minus cash), *IndustrySigma* is the industry average of the standard deviation of $\frac{FCF}{NA}$ over the prior ten years, *MV* is the market value of assets, and *RD* is the research and development expense.

Shareholder payout is the total amount of dividends and repurchases divided by the lagged market value of equity. Dividends are defined as cash dividends declared on common stock. Repurchases are defined as the purchase of common and preferred stock minus any reduction in the value of preferred stocks outstanding. Repurchases are set to zero if the amount is smaller than 1% of the lagged market value of equity.

Total compensation is the natural logarithm of CEO annual total compensation.

Discretionary accruals is the absolute value of the difference in discretionary accruals between a given firm and another firm with the closest return on assets (net income divided by total assets) in the same industry-year. Discretionary accruals are residuals from the modified Jones model estimated for each industry-year group:

$$\frac{TA_{i,t}}{Asset_{i,t-1}} = \beta_0 + \beta_1 \frac{1}{Asset_{i,t-1}} + \beta_2 \frac{\Delta Sale_{i,t} - \Delta AR_{i,t}}{Asset_{i,t-1}} + \beta_3 \frac{PPE_{i,t}}{Asset_{i,t-1}} + \mu_{i,t},$$

where *TA* is total accruals (the change in current assets minus the change in cash and short-term investments minus the change in current liabilities plus the change in debt in current liabilities minus depreciation and amortization), *Asset* is total assets, Δ*Sale* is the

change in sales, ΔAR is the change in accounts receivables, and *PPE* is net property, plant, and equipment.

Return on assets is operating income before depreciation divided by lagged total assets.

Return on net operating assets is operating income before depreciation divided by lagged net operating assets, defined as operating assets (total assets minus cash and short-term investments) net of operating liabilities (total assets minus debt in current liabilities minus long-term debt minus minority interest minus preferred stock minus common equity).

Capital expenditures is capital expenditures divided by lagged total assets.

R&D expense is the research and development expense divided by lagged total assets. *R&D expense* is set to zero if the research and development expense is missing.

2. Key independent variables

Treatment is an indicator equal to one for firms that share directors with proxy contest target firms and zero otherwise.

Post is an indicator equal to one for the year after a proxy contest and zero otherwise.

Improvement index is the firm's average quintile rank of all policy changes.

3. Control variables: firm characteristics

Market capitalization is the natural logarithm of the market value of equity.

Market-to-book is the ratio of the market value of total assets to the book value of total assets.

Stock return is the stock return over the year.

Return volatility is the standard deviation of monthly stock returns over the last five years.

4. Control variables: board characteristics

Board size is the total number of directors on the board.

Board independence is the percentage of independent directors on the board.

CEO duality is an indicator equal to one if the CEO is also the chairman of the board and zero otherwise.

Director ownership is the total percentage of shares owned by all directors.

Director busyness is the percentage of independent directors holding at least three board seats.

Director co-option is the percentage of directors appointed after the CEO assumed office.

5. Control variables: interlocking director characteristics

Independent is an indicator equal to one for independent directors and zero otherwise.

Age is the age of the director.

Tenure is the number of years the director has served on the board.

Busy is an indicator equal to one if the director holds at least three board seats and zero otherwise.

Co-opted is an indicator equal to one if the director was appointed after the CEO assumed office and zero otherwise.

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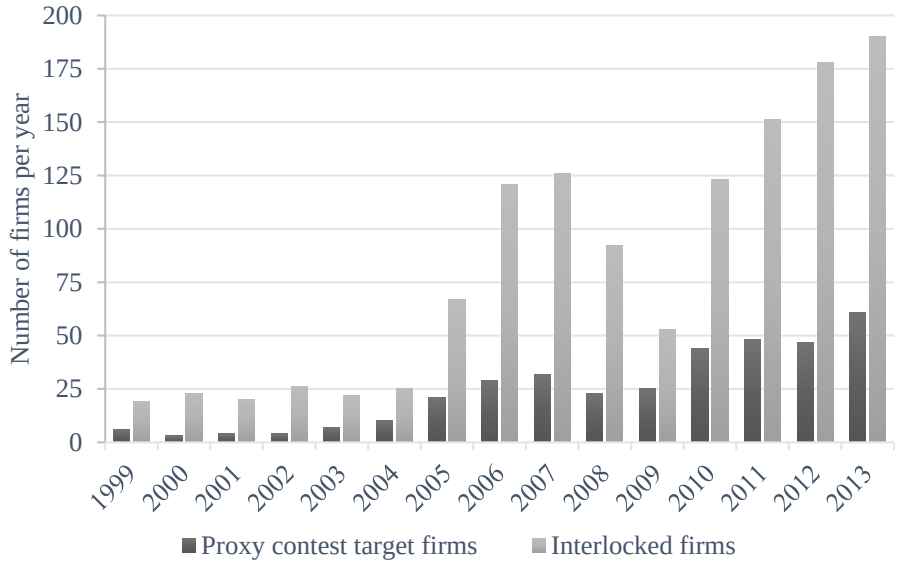

Fig. 1. The distribution of target and interlocked firms by year. This figure plots the number of proxy contest target firms and the number of interlocked firms over the 1999–2013 period. The sample is based on S&P 1500 companies. The list of target firms is obtained from the Proxy Fights database in the SDC Platinum. Interlocked firms are companies that share directors with target firms in the year of proxy contests. Director information is obtained from the ISS database.

Table 1

Summary statistics and covariate balance.

Panel A reports summary statistics for the matched sample of treatment and control firms over the three-year period [−1, +1] around proxy contests. Treatment firms are companies that share directors with proxy contest target firms. Control firms are companies that have the closest market capitalization in the same Fama-French 48 industry as treatment firms. The sample is based on S&P 1500 firms from 1998 to 2014. Panel B reports the mean values of firm and board characteristics in the year before proxy contests. The *p*-values for testing the statistical significance of the differences between treatment and control firms are reported (*t*-test). Superscripts ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. Detailed variable definitions are provided in the Appendix.

Panel A: Summary statistics for the matched sample

	Observations	Mean	Median	Standard deviation
Dependent variables				
Excess cash	4,884	0.259	0.241	0.198
Shareholder payout	5,910	0.043	0.034	0.043
Total compensation	5,556	8.595	8.686	0.888
Discretionary accruals	5,052	0.076	0.046	0.106
Control variables				
Market capitalization	5,922	8.460	8.414	1.517
Market-to-book	5,922	1.823	1.493	1.023
Stock return	5,922	0.160	0.137	0.361
Return volatility	5,922	0.105	0.096	0.046
Board size	5,922	9.986	10.000	2.327
Board independence	5,922	0.783	0.800	0.120
CEO duality	5,922	0.559	1.000	0.497
Director ownership	5,922	0.051	0.017	0.094
Director busyness	5,922	0.305	0.286	0.207
Director co-option	5,922	0.350	0.300	0.291

Panel B: Covariate balance in the pre-contest year

	Treatment firms (1)	Control firms (2)	Difference (1)–(2)	*p*-value
Market capitalization	8.312	8.319	−0.007	(0.936)
Market-to-book	1.809	1.855	−0.046	(0.304)
Stock return	0.186	0.194	−0.008	(0.605)
Return volatility	0.106	0.105	0.001	(0.373)
Board size	9.928	9.770	0.158	(0.245)
Board independence	0.785	0.768	0.017*	(0.092)
CEO duality	0.541	0.534	0.007	(0.782)
Director ownership	0.048	0.057	−0.009	(0.233)
Director busyness	0.324	0.291	0.033**	(0.046)
Director co-option	0.343	0.357	−0.014	(0.355)
Predicted contest probability	0.022	0.021	0.001	(0.287)

Table 2

The effects of proxy contests on interlocked firms.

This table reports results from regressions estimated using the matched sample of treatment and control firms over the three-year period [−1, +1] around proxy contests. The sample is based on S&P 1500 firms from 1998 to 2014. The dependent variables are *Excess cash* in Panel A, *Shareholder payout* in Panel B, *Total compensation* in Panel C, and *Discretionary accruals* in Panel D. The indicator *Treatment* equals one for firms that share directors with proxy contest target firms and zero otherwise. The indicator *Post* equals one for the year after a proxy contest and zero otherwise. In parentheses are standard errors adjusted for heteroskedasticity and clustering by both firm and year. Superscripts ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Excess cash					
	(1)	(2)	(3)	(4)	(5)
Treatment×Post	−0.019**	−0.017**	−0.012**	−0.012**	−0.017***
	(0.008)	(0.007)	(0.005)	(0.006)	(0.006)
Treatment	0.018	0.013	0.012	0.003	0.009
	(0.011)	(0.008)	(0.007)	(0.006)	(0.008)
Post	0.005	0.014	0.013*	0.004	0.006
	(0.008)	(0.008)	(0.007)	(0.005)	(0.004)
Market capitalization		0.101***	0.102***	0.018***	0.024**
		(0.004)	(0.005)	(0.003)	(0.009)
Market-to-book		−0.002	−0.005	0.003	−0.004
		(0.007)	(0.007)	(0.002)	(0.009)
Stock return		−0.055***	−0.044***	−0.015**	0.001
		(0.011)	(0.012)	(0.006)	(0.006)
Return volatility		1.010***	1.110***	0.171***	−0.013
		(0.150)	(0.144)	(0.049)	(0.180)
Board size		−0.005**	−0.002	0.001	0.001
		(0.002)	(0.002)	(0.001)	(0.001)
Board independence		0.024	0.018	−0.018**	0.022
		(0.033)	(0.030)	(0.008)	(0.024)
CEO duality		−0.022**	−0.010	−0.001	0.008
		(0.010)	(0.009)	(0.003)	(0.007)
Director ownership		−0.077*	−0.036	−0.013	−0.010
		(0.041)	(0.039)	(0.011)	(0.046)
Director busyness		−0.015	−0.020	−0.002	0.001
		(0.016)	(0.015)	(0.007)	(0.012)
Director co-option		0.035*	0.018	0.010	−0.003
		(0.017)	(0.015)	(0.007)	(0.015)
Lag excess cash				0.822***	
				(0.023)	
Year fixed effects	No	Yes	No	Yes	Yes
Industry-year fixed effects	No	No	Yes	No	No
Firm fixed effects	No	No	No	No	Yes
Adjusted *R*-squared	0.006	0.459	0.513	0.834	0.855
Observations	4,884	4,884	4,884	4,884	4,884

Panel B: Shareholder payout					
	(1)	(2)	(3)	(4)	(5)
Treatment×Post	0.005**	0.005**	0.007**	0.004*	0.006**
	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)
Treatment	0.003	0.001	−0.001	−0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
Post	0.003	−0.003	−0.005	−0.002	−0.004
	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)
Market capitalization		0.004***	0.004***	0.003***	0.015***
		(0.001)	(0.001)	(0.001)	(0.003)
Market-to-book		−0.001	−0.002	−0.001*	−0.007***
		(0.001)	(0.001)	(0.001)	(0.002)
Stock return		−0.004	−0.004	0.003	−0.002
		(0.003)	(0.003)	(0.002)	(0.002)
Return volatility		−0.160***	−0.181***	−0.083***	−0.169***
		(0.024)	(0.026)	(0.023)	(0.035)
Board size		0.001	0.001	−0.001	−0.001
		(0.001)	(0.001)	(0.001)	(0.001)
Board independence		0.010	0.012*	0.003	−0.006
		(0.006)	(0.006)	(0.004)	(0.010)
CEO duality		−0.001	0.002	−0.001	−0.001
		(0.002)	(0.002)	(0.001)	(0.002)
Director ownership		0.011	0.001	0.001	−0.001
		(0.010)	(0.010)	(0.007)	(0.027)
Director busyness		0.006	0.006	0.005*	−0.005
		(0.004)	(0.003)	(0.003)	(0.005)
Director co-option		−0.001	−0.004	0.001	−0.002
		(0.003)	(0.003)	(0.002)	(0.003)
Lag shareholder payout				0.420***	
				(0.051)	
Year fixed effects	No	Yes	No	Yes	Yes
Industry-year fixed effects	No	No	Yes	No	No
Firm fixed effects	No	No	No	No	Yes
Adjusted *R*-squared	0.012	0.132	0.212	0.276	0.418
Observations	5,910	5,910	5,910	5,910	5,910

Panel C: Total compensation					
	(1)	(2)	(3)	(4)	(5)
Treatment×Post	−0.067**	−0.066**	−0.084**	−0.047*	−0.051**
	(0.029)	(0.027)	(0.032)	(0.026)	(0.019)
Treatment	0.113**	0.023	0.027	0.012	−0.017
	(0.047)	(0.036)	(0.034)	(0.028)	(0.030)
Post	0.047	0.059	0.071	0.039	0.033
	(0.046)	(0.036)	(0.044)	(0.033)	(0.028)
Market capitalization		0.400***	0.406***	0.182***	0.141***
		(0.022)	(0.022)	(0.019)	(0.038)
Market-to-book		0.088**	0.103**	0.049**	0.022
		(0.037)	(0.043)	(0.021)	(0.023)
Stock return		0.097*	0.095*	0.059*	0.100***
		(0.051)	(0.046)	(0.033)	(0.033)
Return volatility		1.712***	1.453***	0.931***	−0.748
		(0.391)	(0.484)	(0.214)	(0.618)
Board size		−0.001	0.014	−0.001	−0.002
		(0.008)	(0.009)	(0.004)	(0.009)
Board independence		0.486***	0.461***	0.160**	−0.006
		(0.133)	(0.127)	(0.072)	(0.129)
CEO duality		0.089**	0.106**	0.027	0.015
		(0.036)	(0.036)	(0.020)	(0.037)
Director ownership		−0.192	−0.364**	−0.132	−0.064
		(0.176)	(0.163)	(0.110)	(0.193)
Director busyness		0.318***	0.190**	0.121**	0.018
		(0.082)	(0.069)	(0.054)	(0.057)
Director co-option		−0.013	−0.053	−0.022	0.062
		(0.047)	(0.046)	(0.030)	(0.069)
Lag total compensation				0.542***	
				(0.031)	
Year fixed effects	No	Yes	No	Yes	Yes
Industry-year fixed effects	No	No	Yes	No	No
Firm fixed effects	No	No	No	No	Yes
Adjusted *R*-squared	0.019	0.518	0.557	0.673	0.753
Observations	5,556	5,556	5,556	5,556	5,556

Panel D: Discretionary accruals					
	(1)	(2)	(3)	(4)	(5)
Treatment×Post	−0.015**	−0.017**	−0.017**	−0.018**	−0.018**
	(0.007)	(0.008)	(0.007)	(0.007)	(0.007)
Treatment	0.003	0.005	0.007*	0.006	0.003
	(0.005)	(0.003)	(0.004)	(0.004)	(0.004)
Post	0.013*	0.014*	0.016*	0.015*	0.015*
	(0.007)	(0.007)	(0.008)	(0.008)	(0.008)
Market capitalization		−0.001	−0.003	−0.001	−0.013*
		(0.002)	(0.002)	(0.002)	(0.007)
Market-to-book		0.009***	0.007***	0.009***	0.005
		(0.003)	(0.002)	(0.002)	(0.006)
Stock return		0.013**	0.012*	0.014**	0.022**
		(0.006)	(0.006)	(0.006)	(0.009)
Return volatility		0.262***	0.167**	0.242***	0.134**
		(0.056)	(0.069)	(0.058)	(0.060)
Board size		−0.001	0.001	−0.001	−0.001
		(0.001)	(0.001)	(0.001)	(0.001)
Board independence		−0.009	−0.023	−0.008	−0.013
		(0.017)	(0.018)	(0.016)	(0.023)
CEO duality		−0.003	−0.001	−0.003	−0.002
		(0.005)	(0.004)	(0.005)	(0.005)
Director ownership		0.002	−0.011	−0.002	−0.046
		(0.015)	(0.018)	(0.015)	(0.035)
Director busyness		0.012	0.005	0.011	0.014
		(0.008)	(0.009)	(0.008)	(0.013)
Director co-option		0.010	0.010	0.009	0.020*
		(0.007)	(0.006)	(0.007)	(0.011)
Lag discretionary accruals				0.063***	
				(0.015)	
Year fixed effects	No	Yes	No	Yes	Yes
Industry-year fixed effects	No	No	Yes	No	No
Firm fixed effects	No	No	No	No	Yes
Adjusted *R*-squared	0.005	0.035	0.084	0.044	0.153
Observations	5,052	5,052	5,052	5,052	5,052

Table 3

Interlocking directors' board committee memberships.

This table reports analyses of variation in interlocking directors' board committee memberships. The regressions are estimated using the matched sample of treatment and control firms over the three-year period [−1, +1] around proxy contests. The sample is based on S&P 1500 firms from 1998 to 2014. The dependent variables are *Total compensation* in Column 1 and *Discretionary accruals* in Column 2. *Treatment_committee member* indicates treatment firms for which the interlocking director sits on the board committee at both the interlocked and target firms, and *Treatment_not committee member* indicates the rest of treatment firms. The board committee refers to the compensation committee in Column 1 and the audit committee in Column 2. In parentheses are standard errors adjusted for heteroskedasticity and clustering by both firm and year. Superscripts ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	(1) *Total compensation*	(2) *Discretionary accruals*
Committee =	Compensation committee	Audit committee
Treatment_committee member×Post	−0.113***	−0.027***
	(0.032)	(0.008)
Treatment_not committee member×Post	−0.006	−0.008
	(0.041)	(0.006)
Treatment_committee member	0.033	0.001
	(0.046)	(0.008)
Treatment_not committee member	−0.046	−0.001
	(0.029)	(0.004)
Post	0.033	0.015*
	(0.026)	(0.008)
Market capitalization	0.141***	−0.014*
	(0.038)	(0.007)
Market-to-book	0.022	0.006
	(0.023)	(0.006)
Stock return	0.101***	0.022**
	(0.033)	(0.009)
Return volatility	−0.732	0.133**
	(0.622)	(0.057)
Board size	−0.001	−0.001
	(0.009)	(0.001)
Board independence	−0.001	−0.013
	(0.130)	(0.023)
CEO duality	0.017	−0.002
	(0.036)	(0.005)
Director ownership	−0.061	−0.046
	(0.192)	(0.036)
Director busyness	0.013	0.015
	(0.055)	(0.013)
Director co-option	0.066	0.019*
	(0.069)	(0.011)
Year fixed effects	Yes	Yes
Firm fixed effects	Yes	Yes
Adjusted *R*-squared	0.754	0.154
Observations	5,556	5,052

Table 4

Interlocking directors' career concerns.

This table reports analyses of variation in the strength of interlocking directors' career concerns. The regressions are estimated using the matched sample of treatment and control firms over the three-year period [−1, +1] around proxy contests. The sample is based on S&P 1500 firms from 1998 to 2014. The dependent variables are *Excess cash* in Column 1, *Shareholder payout* in Column 2, *Total compensation* in Column 3, and *Discretionary accruals* in Column 4. In Panel A, *Treatment_unitary board* indicates treatment firms for which both the interlocked and target firms have a unitary board, and *Treatment_staggered board* indicates the rest of treatment firms. In Panel B, *Treatment_up for election* indicates treatment firms for which the interlocking director is up for election at both the interlocked and target firms, and *Treatment_not up for election* indicates the rest of treatment firms. In Panel C, *Treatment_young* indicates treatment firms for which the interlocking director's age is below the median, and *Treatment_old* indicates the rest of treatment firms. In Panel D, *Treatment_short tenure* indicates treatment firms for which the interlocking director's tenure is below the median, and *Treatment_long tenure* indicates the rest of treatment firms. In parentheses are standard errors adjusted for heteroskedasticity and clustering by both firm and year. Superscripts ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Board structure of the interlocked and target firms

	(1) *Excess cash*	(2) *Shareholder payout*	(3) *Total compensation*	(4) *Discretionary accruals*
Treatment_unitary board×Post	−0.020***	0.007**	−0.101***	−0.024***
	(0.006)	(0.003)	(0.027)	(0.007)
Treatment_staggered board×Post	−0.009	0.005	−0.011	−0.011
	(0.006)	(0.003)	(0.026)	(0.009)
Treatment_unitary board	0.011	0.002	−0.010	0.005
	(0.007)	(0.002)	(0.032)	(0.008)
Treatment_staggered board	0.012	0.001	−0.025	0.001
	(0.008)	(0.002)	(0.034)	(0.006)
Post	0.009*	−0.002	0.040	0.012*
	(0.005)	(0.002)	(0.029)	(0.006)
Market capitalization	0.020**	0.013***	0.129***	−0.015*
	(0.009)	(0.003)	(0.041)	(0.007)
Market-to-book	0.001	−0.005**	0.015	0.007
	(0.009)	(0.002)	(0.022)	(0.006)
Stock return	−0.001	−0.002	0.089**	0.025**
	(0.007)	(0.002)	(0.034)	(0.011)
Return volatility	−0.071	−0.184***	−0.545	0.126
	(0.184)	(0.039)	(0.615)	(0.083)
Board size	−0.001	−0.001	−0.001	0.001
	(0.001)	(0.001)	(0.010)	(0.001)
Board independence	0.027	−0.008	0.025	−0.001
	(0.025)	(0.011)	(0.130)	(0.027)
CEO duality	0.009	−0.001	0.031	−0.001
	(0.007)	(0.002)	(0.038)	(0.005)
Director ownership	0.002	−0.001	−0.088	−0.037
	(0.034)	(0.030)	(0.191)	(0.037)
Director busyness	−0.002	−0.003	0.034	0.009
	(0.013)	(0.005)	(0.059)	(0.014)
Director co-option	−0.003	−0.001	0.031	0.017
	(0.016)	(0.003)	(0.071)	(0.015)
Year fixed effects	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Adjusted *R*-squared	0.860	0.434	0.765	0.158
Observations	4,884	5,910	5,556	5,052

Panel B: Director election cycles at the interlocked and target firms				
	(1) *Excess cash*	(2) *Shareholder payout*	(3) *Total compensation*	(4) *Discretionary accruals*
Treatment_up for election×Post	−0.021***	0.008**	−0.075***	−0.023***
	(0.005)	(0.003)	(0.024)	(0.007)
Treatment_not up for election×Post	−0.005	0.005	−0.014	−0.008
	(0.008)	(0.004)	(0.026)	(0.013)
Treatment_up for election	0.008	0.001	−0.009	0.003
	(0.006)	(0.002)	(0.043)	(0.005)
Treatment_not up for election	0.009	0.002	−0.018	0.003
	(0.010)	(0.004)	(0.037)	(0.012)
Post	0.008	−0.001	0.040	0.013*
	(0.005)	(0.002)	(0.026)	(0.007)
Market capitalization	0.021**	0.013***	0.127***	−0.015*
	(0.009)	(0.003)	(0.042)	(0.008)
Market-to-book	0.001	−0.005**	0.016	0.007
	(0.009)	(0.002)	(0.022)	(0.006)
Stock return	−0.001	−0.001	0.090**	0.025**
	(0.007)	(0.002)	(0.035)	(0.011)
Return volatility	−0.064	−0.186***	−0.537	0.130
	(0.184)	(0.039)	(0.615)	(0.083)
Board size	−0.001	−0.001	−0.001	0.001
	(0.002)	(0.001)	(0.010)	(0.001)
Board independence	0.027	−0.008	0.020	−0.001
	(0.026)	(0.011)	(0.132)	(0.028)
CEO duality	0.009	−0.001	0.032	−0.001
	(0.007)	(0.002)	(0.038)	(0.005)
Director ownership	0.002	−0.001	−0.103	−0.037
	(0.034)	(0.030)	(0.194)	(0.037)
Director busyness	−0.001	−0.003	0.029	0.009
	(0.013)	(0.005)	(0.060)	(0.014)
Director co-option	−0.003	−0.001	0.031	0.016
	(0.016)	(0.003)	(0.074)	(0.015)
Year fixed effects	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Adjusted *R*-squared	0.866	0.436	0.761	0.177
Observations	4,884	5,910	5,556	5,052

Panel C: Age of the interlocking director				
	(1) *Excess cash*	(2) *Shareholder payout*	(3) *Total compensation*	(4) *Discretionary accruals*
Treatment_young×Post	−0.019***	0.008**	−0.081**	−0.023**
	(0.005)	(0.003)	(0.031)	(0.009)
Treatment_old×Post	−0.014*	0.003	−0.013	−0.010
	(0.007)	(0.003)	(0.028)	(0.009)
Treatment_young	0.010*	−0.002	−0.015	0.004
	(0.005)	(0.003)	(0.043)	(0.005)
Treatment_old	0.008	0.003	−0.035	−0.001
	(0.005)	(0.003)	(0.034)	(0.005)
Post	0.006	−0.003	0.030	0.012*
	(0.004)	(0.003)	(0.024)	(0.007)
Market capitalization	0.024**	0.015***	0.145***	−0.014**
	(0.009)	(0.003)	(0.038)	(0.007)
Market-to-book	−0.004	−0.007***	0.022	0.006
	(0.009)	(0.002)	(0.022)	(0.006)
Stock return	0.001	−0.002	0.096***	0.022**
	(0.006)	(0.002)	(0.032)	(0.009)
Return volatility	−0.013	−0.170***	−0.698	0.143**
	(0.182)	(0.034)	(0.603)	(0.057)
Board size	0.001	−0.001	−0.001	0.001
	(0.001)	(0.001)	(0.009)	(0.001)
Board independence	0.021	−0.006	0.004	−0.014
	(0.024)	(0.010)	(0.127)	(0.023)
CEO duality	0.008	−0.001	0.016	−0.002
	(0.007)	(0.002)	(0.037)	(0.004)
Director ownership	−0.009	−0.002	−0.061	−0.051
	(0.047)	(0.026)	(0.191)	(0.036)
Director busyness	0.001	−0.005	0.009	0.014
	(0.012)	(0.005)	(0.053)	(0.013)
Director co-option	−0.004	−0.002	0.062	0.020*
	(0.015)	(0.003)	(0.068)	(0.011)
Year fixed effects	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Adjusted *R*-squared	0.869	0.420	0.808	0.158
Observations	4,884	5,910	5,556	5,052

Panel D: Tenure of the interlocking director				
	(1) *Excess cash*	(2) *Shareholder payout*	(3) *Total compensation*	(4) *Discretionary accruals*
Treatment_short tenure×Post	−0.019**	0.008**	−0.089***	−0.021**
	(0.007)	(0.003)	(0.026)	(0.008)
Treatment_long tenure×Post	−0.016**	0.002	−0.007	−0.011
	(0.006)	(0.003)	(0.035)	(0.009)
Treatment_short tenure	0.003	−0.003	−0.025	0.001
	(0.006)	(0.004)	(0.029)	(0.006)
Treatment_long tenure	0.010*	0.003	0.038	0.004
	(0.005)	(0.003)	(0.031)	(0.005)
Post	0.006	−0.003	0.039	0.012*
	(0.004)	(0.003)	(0.024)	(0.007)
Market capitalization	0.024**	0.015***	0.144***	−0.014*
	(0.009)	(0.003)	(0.037)	(0.007)
Market-to-book	−0.004	−0.007***	0.021	0.006
	(0.009)	(0.002)	(0.023)	(0.006)
Stock return	0.001	−0.002	0.097***	0.022**
	(0.006)	(0.002)	(0.032)	(0.009)
Return volatility	−0.011	−0.168***	−0.739	0.142**
	(0.182)	(0.034)	(0.605)	(0.057)
Board size	0.001	−0.001	−0.001	−0.001
	(0.001)	(0.001)	(0.009)	(0.001)
Board independence	0.022	−0.006	−0.001	−0.015
	(0.024)	(0.010)	(0.127)	(0.024)
CEO duality	0.008	−0.001	0.016	−0.002
	(0.007)	(0.002)	(0.036)	(0.004)
Director ownership	−0.009	−0.001	−0.064	−0.050
	(0.046)	(0.026)	(0.194)	(0.036)
Director busyness	0.001	−0.005	0.016	0.014
	(0.012)	(0.005)	(0.053)	(0.013)
Director co-option	−0.003	−0.002	0.062	0.020*
	(0.015)	(0.003)	(0.069)	(0.011)
Year fixed effects	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Adjusted *R*-squared	0.856	0.450	0.797	0.202
Observations	4,884	5,910	5,556	5,052

Table 5

Validity of empirical design.

This table reports analyses of the validity of the empirical design. The sample is based on S&P 1500 firms from 1998 to 2014. Panel A reports the mean values of differences in policy changes between treatment and control firms in the three years preceding proxy contests. Year 0 is the proxy contest year. The *p*-values for testing the statistical significance of the differences are reported in parentheses (*t*-test). In Panel B, the regressions are estimated using the matched sample of treatment and control firms around pseudo proxy contest years, which are set as three years before or after actual proxy contest years. *Pseudo post* indicates the year after a pseudo proxy contest year. In Panel C, the regressions are estimated using a matched sample for *Pseudo treatment* firms, which share directors with target firms in the year before but not in the year of proxy contests. In Panel D, the regressions are estimated using a matched sample for *Target peer interlock* firms, which share directors with industry peers of target firms. Industry peers are firms in the same Fama-French 48 industry that have the closest market capitalization in the year before proxy contests. In Panels E and F, the regressions are estimated using the matched sample of treatment and control firms. In Panel E, *Predicted contest probability* is the probability of being targeted in a proxy contest. In Panel F, treatment firms that appoint interlocking directors one year before proxy contests are excluded. The regressions in Panels B–F include all firm and board characteristics as well as year and firm fixed effects. The coefficients on the control variables are omitted for brevity. In parentheses are standard errors adjusted for heteroskedasticity and clustering by both firm and year. Superscripts ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Pre-contest differences in policy changes between treatment and control firms

	(1) *Excess cash*	(2) *Shareholder payout*	(3) *Total compensation*	(4) *Discretionary accruals*
From year −1 to year 0	−0.003	0.001	0.027	0.001
p-value	(0.454)	(0.900)	(0.291)	(0.920)
From year −2 to year −1	−0.005	0.003	−0.021	−0.005
p-value	(0.273)	(0.245)	(0.428)	(0.438)
From year −3 to year −2	−0.002	−0.001	0.010	−0.006
p-value	(0.713)	(0.517)	(0.730)	(0.313)

Panel B: Pseudo proxy contest years

	(1) *Excess cash*	(2) *Shareholder payout*	(3) *Total compensation*	(4) *Discretionary accruals*
	Pseudo proxy contest year = three years before the actual year			
Treatment×*Pseudo post*	−0.001	0.002	0.034	0.010
	(0.005)	(0.002)	(0.022)	(0.007)
Treatment	0.007	−0.001	−0.002	0.001
	(0.005)	(0.002)	(0.029)	(0.009)
Pseudo post	−0.004	−0.002	0.011	−0.008
	(0.004)	(0.002)	(0.027)	(0.005)
	Pseudo proxy contest year = three years after the actual year			
Treatment×*Pseudo post*	0.008	0.001	0.023	−0.001
	(0.005)	(0.002)	(0.057)	(0.007)
Treatment	0.002	−0.003	0.002	0.004
	(0.006)	(0.002)	(0.029)	(0.007)
Pseudo post	−0.006	0.001	0.041	0.002
	(0.004)	(0.002)	(0.037)	(0.006)

Panel C: Board interlocks with target firms only before (but not during) proxy contests

	(1) *Excess cash*	(2) *Shareholder payout*	(3) *Total compensation*	(4) *Discretionary accruals*
Pseudo treatment×Post	−0.001	0.007	0.068	−0.006
	(0.012)	(0.007)	(0.069)	(0.016)
Pseudo treatment	−0.026	0.008	0.043	0.016
	(0.015)	(0.011)	(0.114)	(0.015)
Post	0.002	−0.001	0.078	0.014
	(0.007)	(0.008)	(0.094)	(0.008)

Panel D: Board interlocks with industry peers of target firms

	(1) *Excess cash*	(2) *Shareholder payout*	(3) *Total compensation*	(4) *Discretionary accruals*
Target peer interlock×Post	0.002	0.001	−0.002	0.009
	(0.006)	(0.002)	(0.026)	(0.007)
Target peer interlock	0.005	−0.002	0.032	0.006
	(0.004)	(0.002)	(0.024)	(0.006)
Post	−0.004	−0.001	0.006	−0.007*
	(0.006)	(0.002)	(0.024)	(0.004)

Panel E: Control for the predicted probability of a proxy contest

	(1) *Excess cash*	(2) *Shareholder payout*	(3) *Total compensation*	(4) *Discretionary accruals*
Treatment×Post	−0.017***	0.006**	−0.052**	−0.018**
	(0.006)	(0.003)	(0.023)	(0.008)
Treatment	0.010	0.001	−0.019	0.003
	(0.007)	(0.003)	(0.031)	(0.004)
Post	0.007	−0.004	0.032	0.014*
	(0.004)	(0.003)	(0.023)	(0.007)
Predicted contest probability	−0.117	0.009	0.621	0.064
	(0.150)	(0.065)	(0.948)	(0.238)

Panel F: Exclude treatment firms with newly recruited interlocking directors

	(1) *Excess cash*	(2) *Shareholder payout*	(3) *Total compensation*	(4) *Discretionary accruals*
Treatment×Post	−0.018***	0.005**	−0.048**	−0.018**
	(0.006)	(0.002)	(0.022)	(0.007)
Treatment	0.009	0.001	−0.020	0.002
	(0.007)	(0.002)	(0.033)	(0.004)
Post	0.006	−0.002	0.034	0.015*
	(0.005)	(0.002)	(0.027)	(0.008)

Table 6

Supplementary analyses.

This table reports supplementary analyses. The sample is based on S&P 1500 firms from 1998 to 2014. In Panel A, the regressions are estimated using the matched sample of treatment and control firms over the three-year period [−1, +1] around proxy contests. The dependent variables are *Return on assets* in Column 1, *Return on net operating assets* in Column 2, *Capital expenditures* in Column 3, and *R&D expense* in Column 4. In Panel B, the regressions are estimated using the matched sample of treatment and control firms over the [−3, +3] period around proxy contests. The dependent variables are *Excess cash* in Column 1, *Shareholder payout* in Column 2, *Total compensation* in Column 3, and *Discretionary accruals* in Column 4. The variables *Post +1*, *Post +2*, and *Post +3* indicate the first, second, and third years after a proxy contest, respectively. In parentheses are standard errors adjusted for heteroskedasticity and clustering by both firm and year. Superscripts ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Other corporate outcomes

	(1) *Return on assets*	(2) *Return on net operating assets*	(3) *Capital expenditures*	(4) *R&D expense*
Treatment×Post	0.006**	0.035**	0.001	0.001
	(0.003)	(0.016)	(0.001)	(0.001)
Treatment	−0.001	0.001	−0.001	−0.001
	(0.003)	(0.017)	(0.001)	(0.001)
Post	−0.001	0.006	−0.001	−0.001
	(0.003)	(0.011)	(0.001)	(0.001)
Market capitalization	−0.004	−0.009	0.004**	−0.007***
	(0.003)	(0.035)	(0.002)	(0.002)
Market-to-book	0.035***	0.139***	0.008***	0.011***
	(0.004)	(0.036)	(0.002)	(0.002)
Stock return	0.006*	−0.023	−0.001	−0.002**
	(0.003)	(0.023)	(0.002)	(0.001)
Return volatility	−0.018	0.600	0.012	−0.009
	(0.061)	(0.400)	(0.022)	(0.019)
Board size	−0.001	−0.013	−0.001	−0.001
	(0.001)	(0.008)	(0.001)	(0.001)
Board independence	0.014	−0.060	0.016*	0.001
	(0.012)	(0.089)	(0.009)	(0.004)
CEO duality	−0.003	0.005	−0.001	−0.001
	(0.003)	(0.026)	(0.001)	(0.001)
Director ownership	−0.012	−0.013	−0.029**	0.012
	(0.023)	(0.091)	(0.012)	(0.008)
Director busyness	−0.004	0.087	−0.004	0.001
	(0.008)	(0.060)	(0.003)	(0.003)
Director co-option	−0.005	−0.031	0.002	0.001
	(0.005)	(0.020)	(0.003)	(0.001)
Year fixed effects	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Adjusted *R*-squared	0.760	0.491	0.823	0.907
Observations	5,778	5,778	5,862	5,922

Panel B: An extended period				
	(1) *Excess cash*	(2) *Shareholder payout*	(3) *Total compensation*	(4) *Discretionary accruals*
Treatment×Post +1	−0.017***	0.006***	−0.054**	−0.018*
	(0.006)	(0.002)	(0.024)	(0.009)
Treatment×Post +2	−0.003	0.002	−0.059	0.006
	(0.005)	(0.002)	(0.036)	(0.010)
Treatment×Post +3	0.002	−0.001	−0.035	0.001
	(0.009)	(0.002)	(0.036)	(0.007)
Treatment	0.011	0.001	−0.023	0.001
	(0.007)	(0.002)	(0.034)	(0.007)
Post +1	0.009	−0.002*	0.029	0.012
	(0.005)	(0.001)	(0.028)	(0.009)
Post +2	0.002	−0.001	0.027	−0.008
	(0.006)	(0.002)	(0.031)	(0.007)
Post +3	−0.006	−0.001	0.070*	0.002
	(0.008)	(0.002)	(0.034)	(0.005)
Market capitalization	0.031***	0.012***	0.153***	−0.009*
	(0.008)	(0.002)	(0.027)	(0.005)
Market-to-book	−0.011	−0.005***	0.027	0.004
	(0.007)	(0.001)	(0.017)	(0.003)
Stock return	0.003	−0.002	0.115***	0.015***
	(0.005)	(0.002)	(0.029)	(0.005)
Return volatility	0.102	−0.141***	−0.356	0.013
	(0.122)	(0.032)	(0.485)	(0.042)
Board size	0.001	−0.001*	−0.001	−0.002*
	(0.001)	(0.001)	(0.006)	(0.001)
Board independence	0.016	0.010	0.079	−0.012
	(0.021)	(0.006)	(0.098)	(0.014)
CEO duality	0.006	−0.001	0.025	−0.002
	(0.005)	(0.001)	(0.025)	(0.004)
Director ownership	−0.049	0.007	−0.276*	−0.036
	(0.031)	(0.017)	(0.135)	(0.025)
Director busyness	−0.004	−0.001	0.071	0.001
	(0.010)	(0.003)	(0.054)	(0.010)
Director co-option	−0.001	−0.002	0.056	0.007
	(0.010)	(0.002)	(0.051)	(0.007)
Year fixed effects	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Adjusted *R*-squared	0.833	0.376	0.796	0.130
Observations	7,588	8,778	8,638	7,854

Table 7

The effects of policy changes on interlocking directors' careers.

This table reports analyses of the career consequences for interlocking directors. Linear probability models are estimated for interlocked firms over the [−3, +3] period around proxy contests. The sample is based on S&P 1500 firms from 1998 to 2014. In Panel A, the dependent variable is an indicator equal to one if the interlocking director remains on the board of the interlocked firm and zero otherwise. In Panel B, the dependent variable is an indicator equal to one if the interlocked firm is targeted in a proxy contest and zero otherwise. *Improvement index* is the firm's average quintile rank of all policy changes. *Post* is an indicator equal to one for the three years after a proxy contest and zero otherwise. In parentheses are standard errors adjusted for heteroskedasticity and clustering by both firm and year. Superscripts ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Probability of the interlocking director remaining on the board of the interlocked firm				
	(1)	(2)	(3)	(4)
Post×Improvement index	0.027**	0.025**	0.032**	0.030**
	(0.011)	(0.010)	(0.012)	(0.014)
Post	−0.227***	−0.209***	−0.169***	−0.184***
	(0.030)	(0.032)	(0.028)	(0.033)
Improvement index	0.001	0.001	0.002	0.002
	(0.003)	(0.004)	(0.003)	(0.004)
Market capitalization	0.005	0.006	0.007	0.041**
	(0.007)	(0.006)	(0.006)	(0.017)
Market-to-book	0.001	0.003	0.010*	0.013
	(0.005)	(0.006)	(0.006)	(0.015)
Stock return	0.015**	0.010	0.012	0.024
	(0.007)	(0.008)	(0.011)	(0.017)
Return volatility	−0.236	−0.410**	−0.195	−0.230
	(0.153)	(0.172)	(0.184)	(0.441)
Board size	−0.005*	−0.005*	−0.004	−0.001
	(0.002)	(0.003)	(0.003)	(0.004)
Board independence	−0.110*	−0.149**	−0.068	0.083
	(0.055)	(0.054)	(0.054)	(0.080)
CEO duality	0.008	0.003	0.001	0.002
	(0.009)	(0.012)	(0.009)	(0.012)
Director ownership	0.035	0.062	0.140**	0.117
	(0.060)	(0.077)	(0.062)	(0.102)
Director busyness	0.099**	0.105**	0.064*	0.122***
	(0.038)	(0.042)	(0.035)	(0.037)
Director co-option	0.084***	0.089***	0.079***	0.054**
	(0.024)	(0.028)	(0.024)	(0.024)
Independent	0.057***	0.048**	0.052**	0.077**
	(0.019)	(0.018)	(0.020)	(0.029)
Age	−0.008**	−0.008**	−0.003*	−0.008***
	(0.003)	(0.003)	(0.001)	(0.003)
Tenure	−0.002	−0.002	−0.002*	−0.001
	(0.002)	(0.002)	(0.001)	(0.002)
Busy	0.010	0.010	−0.024	−0.026
	(0.022)	(0.023)	(0.015)	(0.026)
Co-opted	−0.032**	−0.030**	−0.038*	−0.057**
	(0.014)	(0.014)	(0.019)	(0.026)
Year fixed effects	Yes	No	Yes	Yes
Industry-year fixed effects	No	Yes	No	No
Proxy contest event fixed effects	No	No	Yes	No
Firm fixed effects	No	No	No	Yes
Adjusted *R*-squared	0.153	0.233	0.257	0.420
Observations	5,383	5,383	5,383	5,383

Panel B: Probability of a proxy contest at the interlocked firm				
	(1)	(2)	(3)	(4)
Post×Improvement index	−0.008**	−0.007**	−0.008**	−0.006**
	(0.003)	(0.003)	(0.004)	(0.003)
Post	0.022**	0.022**	0.023**	0.019***
	(0.009)	(0.008)	(0.009)	(0.007)
Improvement index	−0.002	−0.004	−0.003	−0.003
	(0.003)	(0.003)	(0.004)	(0.004)
Market capitalization	−0.004*	−0.007**	−0.006*	−0.005
	(0.002)	(0.003)	(0.003)	(0.005)
Market-to-book	−0.004**	−0.011***	−0.007*	−0.014**
	(0.002)	(0.003)	(0.003)	(0.005)
Stock return	−0.021***	−0.018***	−0.014*	−0.026***
	(0.006)	(0.006)	(0.008)	(0.007)
Return volatility	0.137**	0.226***	0.153**	0.106
	(0.052)	(0.060)	(0.063)	(0.064)
Board size	−0.001	−0.001	−0.002	−0.003
	(0.001)	(0.001)	(0.001)	(0.003)
Board independence	−0.014	−0.018	−0.038	−0.038
	(0.015)	(0.017)	(0.027)	(0.030)
CEO duality	−0.003	−0.003	−0.001	0.004
	(0.007)	(0.008)	(0.009)	(0.010)
Director ownership	0.022	0.007	0.008	−0.097
	(0.024)	(0.025)	(0.034)	(0.071)
Director busyness	−0.002	−0.006	−0.001	−0.015
	(0.012)	(0.013)	(0.013)	(0.017)
Director co-option	−0.008	−0.008	−0.012	−0.011
	(0.008)	(0.008)	(0.009)	(0.010)
Independent	0.005	0.006	0.001	0.011
	(0.008)	(0.008)	(0.009)	(0.011)
Age	−0.001	−0.001	−0.001	−0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Tenure	0.001	0.001	0.001	−0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Busy	−0.001	−0.001	−0.002	−0.001
	(0.001)	(0.002)	(0.003)	(0.003)
Co-opted	0.005	0.006	0.005	0.002
	(0.007)	(0.007)	(0.010)	(0.020)
Year fixed effects	Yes	No	Yes	Yes
Industry-year fixed effects	No	Yes	No	No
Proxy contest event fixed effects	No	No	Yes	No
Firm fixed effects	No	No	No	Yes
Adjusted *R*-squared	0.023	0.096	0.116	0.189
Observations	5,383	5,383	5,383	5,383

Table 8

Stock price reaction to proxy contest announcements.

This table reports cumulative abnormal returns (CARs) over the three-day window [−1, +1], where day 0 is the proxy contest announcement date. Panel A reports CARs of target firms, and Panel B reports CARs of interlocked firms. The sample is based on S&P 1500 firms from 1998 to 2014. Abnormal returns are computed using both the market model and the four-factor model (the Fama-French three factors plus the momentum factor). The model parameters are estimated over the [−150, −50] period before announcement dates. Observations with confounding events in the three-day window are eliminated from the analysis. The *p*-values for testing the statistical significance of mean CARs (standardized cross-sectional test) and median CARs (signed-rank test) are reported in parentheses. Superscripts ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A: CARs of target firms

	Market model	Four-factor model
Mean	4.453%***	4.357%***
p-value	(0.001)	(0.001)
Median	2.853%***	2.717%***
p-value	(0.001)	(0.001)
Observations	335	335

Panel B: CARs of interlocked firms

	Market model	Four-factor model
Mean	0.508%***	0.444%***
p-value	(0.001)	(0.001)
Median	0.371%***	0.321%***
p-value	(0.001)	(0.001)
Observations	1,060	1,060