

The Effect of Directors on Investor Relations: Evidence from Proxy Fight Experiences

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The Effect of Directors on Investor Relations: Evidence from Proxy Fight Experiences

ABSTRACT: This study explores whether directors affect firms' investor relations (IR) activities. To isolate the effect of directors, we examine whether a firm adjusts its IR activities when one of its directors experiences a proxy fight at another company. Using a difference-in-differences design, we find that a director's recent proxy fight experience increases the firm's IR activity. The effect is greater when the director experienced a proxy fight that was ultimately lost to activists. The increase in IR activity is also stronger for firms with greater ownership by long-term institutional investors, fewer anti-takeover provisions, or lower pre-existing levels of shareholder engagement. Furthermore, this rise in IR activity is associated with a reduced likelihood of proxy fights at the firm, suggesting that these additional IR efforts are aimed at strengthening shareholder relationships. Overall, our results highlight the influence of directors in shaping firms' IR strategies.

Keywords: investor relations; investor outreach; IR; directors; experiences; shareholder engagement; proxy fights; board interlocks; staggered difference-in-differences

I. Introduction

The IR function of a firm manages communication between firm insiders and current or prospective shareholders.¹ While corporate directors are typically not visible in public IR activities, they are likely to shape a firm's IR strategy. Bound by their fiduciary duty to maximize shareholder value, directors have incentives to ensure an effective IR program, given the significant implications of IR for firm value (Kirk and Vincent 2014, Karolyi et al. 2020). In addition, as representatives of shareholders, directors could derive value from a robust IR function that facilitates receiving shareholder feedback on both the firm's performance and their own (Brown et al. 2019, IR Impact 2024). Consistent with this perspective, practitioners and regulators have recently urged directors to pay more attention to firm-shareholder communications (SEC 2013, Chudoba and Dennig 2018, Kingsdale Advisors 2019). Despite these arguments for and the growing recognition of directors' prominence in IR among practitioners and regulators, empirical evidence on whether directors have any effect on a firm's IR activity is scant in the literature. We attempt to address this void.

Isolating directors' effects on firm actions is challenging since director- and board-level characteristics are likely correlated with firm attributes that also affect those actions. To identify the effect of directors on IR, we study whether directors' recent external experiences lead to changes in a firm's IR activity. Prior research shows that individual directors often influence firm policies through their experiences gained from serving on other companies' boards (e.g., Gopalan et al. 2021, Ellis et al. 2021). Building on this insight, we examine whether a *focal firm* adjusts its IR activity when one of its directors experiences a proxy fight at another company ("target firm").²

¹ We use IR, investor relations, IR engagement, shareholder engagement, and investor outreach, interchangeably.

² We refer to the company facing the proxy fight as the "target firm" and the firm sharing a director with the target as the "focal firm" or "treatment firm."

Exploiting proxy fight experiences provides a compelling research setting to isolate directors' influence, because proxy fights result from shareholders' disapproval about the target board and potentially create strong incentives for shared directors to take preemptive actions at the focal firm.

There are two reasons why directors experiencing proxy fights at other companies would influence focal firms to *increase* IR activity. First, because proxy fights are among the most severe events driven by shareholder dissatisfaction, personally experiencing one is likely to heighten directors' awareness of the value of IR for building investor confidence in the firm (Chapman et al. 2022). Such first-hand exposure to proxy fights may also equip directors with relevant insights into effective shareholder engagement (e.g., Ellis et al. 2021, Gopalan et al. 2021), making them well positioned to advise the focal firm on strengthening shareholder relationships. Second, prior research finds that proxy fights negatively affect incumbent directors' careers, not only at the target firm but also at other firms where they serve on the board (Fos and Tsoutsoura 2014). To the extent that directors are motivated by career and reputational concerns, those experiencing a proxy fight may have incentives to push for increased IR activity at the focal firm as a means of protecting their positions and reputations.

On the other hand, directors may have only limited influence on the level of a firm's IR activity. While corporate directors are increasingly recognizing the importance of the IR function (e.g., PwC 2022), in practice, the execution of firms' IR activities remains primarily under the purview of executives and IR officers (IROs). Because changes to IR strategy require buy-in from both executives and IROs, it is unclear whether directors, even if motivated, can meaningfully influence a firm's IR activity. Management and IROs may be especially hesitant to act on directors' inputs, given that IR activities demand specialized expertise in finance, law, and communication (Chapman et al. 2019), which directors may lack.

To study our question, we utilize a staggered difference-in-differences (DID) design over a seven-year window [-3, +3] centered around proxy fights at target firms. We define treatment firms (“focal firms”) as those that share a director (“interlocking director”) with a target firm during the year of the proxy fight. Each treatment firm is matched with a control firm from the same industry with the closest market capitalization (Zhang 2021). Although proxy fights are publicly observable, only the treatment firms have directors who experience the event firsthand. In contrast, control firms lack such exposure through their directors. Therefore, any observed differential change in treatment firms’ IR activities can be attributed to directors’ experience.³

To measure a firm’s IR activity, we focus on events that facilitate private meetings between firms and investors, as face-to-face interactions are effective in strengthening shareholder relationships and are therefore most relevant to our setting (Solomon and Soltes 2015, Roghanizad and Bohns 2017). Prior studies identify several venues for such meetings (hereafter, IR events), including investor conferences, analyst/investor days, non-deal roadshows, etc. (e.g., Bushee et al. 2011, Green et al. 2014, Kirk and Markov 2016, Bradley et al. 2021). We obtain data on all publicly observable instances of these events from Bloomberg. Since our data is recorded at the firm-day level, we measure IR activity by counting the number of days a firm attends IR events within a year (see Section 3.1 for details). We then merge this data with our matched sample of treatment and control firms around 1,155 proxy fights obtained from SDC Platinum, resulting in a final sample of 9,113 firm-years across 1,113 firms from 2004 to 2020.

We find that, following proxy fights, focal firms spend 0.73 more days attending IR events per year (i.e., about 15% of the sample median) compared to control firms, consistent with our

³ The treatment effect is also distinct from the industry spillovers documented in prior work, whereby activism affects non-target firms through industry-wide threat (Bourveau and Schoenfeld 2017). This is because we require treatment and control firms to be in the same industry, whereas neither of them is required to be in the target firm’s industry.

hypothesis that a director's proxy fight experience affects the firm's IR activity. To assess the validity of our DID design, we test for pre-trends and find no significant differences in IR activity between treatment and control firms prior to the proxy fight. These results support the credibility of our design. Additionally, we observe that the increase in IR activity is concentrated in the years immediately following the proxy fight, consistent with the impact of individual experience declining over time.

To support that the DID effects originate from director experience, we examine whether the effects are stronger when directors are involved in costly proxy fights with negative outcomes. Prior research highlights the importance of conditioning on the severity of experiences, showing that individuals respond differently depending on whether the experience results in costly and adverse consequences (Gao et al. 2020, Bernile et al. 2017).⁴ Accordingly, we expect the increase in IR activity to be greater when directors experience losing proxy fights to activists (Gow et al. 2024). Our evidence is consistent with this prediction.

In the second set of cross-sectional analysis, we examine whether the DID effects are stronger when directors perceive greater benefits from or urgency to avoiding activism by promoting IR activities at focal firms. Specifically, we exploit the following variations across focal firms: (i) ownership by long-term institutional investors, who can serve as allies of the firm in the event of activist pressure (Ernst & Young 2015); and (ii) the strength of anti-takeover defenses or the level of pre-existing shareholder engagement, which act as mechanisms to combat potential activism (Karpoff et al. 2022, Chapman et al. 2022). We predict and find that the increase in IR activity is greater among firms with a long-term focused shareholder base, suggesting that directors

⁴ Negative outcomes heighten individuals' awareness of the risks and costs associated with an event, while positive outcomes boost their confidence and ability to handle similar situations. For example, Bernile et al. (2017) find that CEOs become more risk-averse after disasters with severe consequences but more risk-seeking after those without.

are more likely to push for IR activity when relationship-building with investors is effective in mitigating a potential activism threat. We also find a stronger main effect for firms with weaker anti-takeover defenses or lower baseline levels of shareholder engagement, consistent with the idea that directors see a heightened urgency for the firm to engage with shareholders when the firm is more vulnerable to activism events.

Our findings suggest a director-driven effect on IR, but one alternative explanation is that unobserved similarities between target and focal firms (i.e., factors that led to the board interlock between the two) drive the results. To address this concern, we conduct several analyses. First, we use *placebo*-treatment firms that shared directors with target firms during the periods immediately before, but not during the year of the proxy fight (Gopalan et al. 2021). These firms lack directors with proxy fight experience but are likely to possess characteristics that recently led to board interlocks with target firms. Repeating our main analysis using these firms yields no significant DID effects.

Second, we exploit the staggered board structure at *focal firms*, where directors are divided into separate classes, with only one class up for reelection each year. We use this intertemporal variation at *focal firms* during the post-treatment period to examine whether their IR activity is higher in years when interlocking directors face reelection, relative to years they do not. The rationale is that directors should have greater incentives to promote IR activity during their own election years to secure their board seats. We find evidence consistent with this expectation. This result is hard to reconcile with the alternative explanation, as the within-firm variation in election cycles is likely exogenous to factors underlying the formation of board interlocks between focal and target firms (Fos and Tsoutsoura 2014).

Third, we apply the impact threshold of confounding variable procedure (Frank 2000) to evaluate the sensitivity of our results to omitted variables. An omitted variable would need to be at least 34% more influential than firm size, the most impactful control, to overturn our results. These results combined limit the scope for alternative interpretations: any unobserved confounders would need to (i) emerge in the treatment year but not just before, (ii) correlate with the timing of interlocking directors' reelection at focal firms, and (iii) have a stronger effect than firm size.

We interpret our main findings as evidence that directors experiencing proxy fights, motivated by an incentive to prevent similar activism shocks, influence focal firms to increase IR activity. However, activism prevention may not be the sole purpose of a firm's IR efforts. Prior literature highlights the informational role of IR, showing that IR events can convey valuable information to the market (Kirk and Markov 2016, Bradley et al. 2021). Given the possible multiple objectives of IR activity, we aim to support our inference by examining whether the observed director-driven increase in IR activity is more closely associated with (i) activism prevention, rather than (ii) information delivery. We first examine the activism prevention purpose, which aligns more directly with our interpretation. We find that the increase in IR activity predicts a lower likelihood of future proxy fights at the firm, the most aggressive form of shareholder activism. This result supports our inference that the main effects reflect IR activity geared toward building a shareholder base supportive of the firm over the long term (Chapman et al. 2022).

Next, we also explore the informational purpose by examining whether the short-horizon market reaction to IR events attended by focal firms increases following directors' proxy fight experience. We find no significant changes in the average event informativeness, inconsistent with the notion that the increased IR activity is primarily intended to deliver new information to investors. Taken together, these additional results reinforce our interpretation that directors who

experience proxy fights at other companies push focal firms to strengthen the relationship-building aspect of IR as a proactive defense against potential activism.

Our contribution is threefold. First, we contribute to the IR literature by identifying directors as a key determinant of a firm's IR function. While research shows associations between IR and firm characteristics such as institutional ownership and profitability (e.g., Bushee and Miller 2012, Kirk and Vincent 2014), the effect of directors on IR remains unknown. This omission is notable given that both regulators and practitioners emphasize the growing importance of directors in investor engagement (SEC 2013, PwC 2022). By leveraging variation in directors' recent (externally acquired) experiences, we isolate the effect of directors and provide the first evidence of their influence on firms' IR function. In addition, we present new evidence on firms' *preemptive* changes in IR activity aimed at building shareholder relationships and lowering activism risk. This finding highlights the corporate governance value of IR activity, an aspect that has received less attention in the IR literature (Chapman et al. 2022).

Second, our study is related to the literature that examines the effects of top executives' experiences on corporate outcomes. While much prior research focuses on early-life experiences (e.g., Bertrand and Schoar 2003, Malmendier and Nagel 2011, Malmendier et al. 2011, Schoar and Zuo 2017), less is known about the impact of more recent professional experiences (Dittmar and Duchin 2016, Kubick and Li 2023). Our results highlight the importance of directors' recent proxy fight experiences acquired during their board service in altering firms' IR strategies.

Lastly, our results contribute to enhancing our understanding of how directors respond to career shocks from shareholder activism. Because proxy fights significantly damage directors' reputations and careers (Fos and Tsoutsoura 2014), experiencing these events likely triggers changes in their decisions, especially regarding fostering shareholder relationships. Yet evidence

on this topic is scarce. One exception is Zhang (2021), which finds that directors strengthen governance practices to mitigate career costs. We complement this work by showing that they also promote IR activity, a potentially faster and more cost-effective way to reduce the risk of future activism.

II. Background

2.1 Investor Relations

Most studies in IR literature focus on the informational benefits of a firm's IR program in capital markets. For example, Bushee and Miller (2012), Kirk and Vincent (2014), and Karolyi et al. (2020) find that initiating an IR program is associated with increased institutional ownership, analyst following, liquidity, and firm valuations. Similarly, Chapman et al. (2019) and Kim et al. (2021) show that IR programs reduce firms' uncertainty in capital markets. Expanding beyond this informational perspective, Chapman et al. (2022) highlights another important function of IR: cultivating a supportive shareholder base through ongoing communication. They find that IR programs are associated with greater investor confidence in management and the board, as well as a reduced likelihood of shareholder activism. Although these two functions of IR—providing information and building relationships—are not mutually exclusive, our study focuses primarily on the latter, i.e., fostering long-term shareholder trust in the firm through effective communication.

The importance of firm-shareholder communication has also gained increasing recognition among regulators and practitioners in the corporate governance domain. For example, former SEC Chair Mary Jo White emphasizes that effective governance goes beyond formal structures and procedures; it also requires proactive outreach and clear, honest communication about how and why decisions are made. Because corporate directors represent shareholders and play a central role in governance, she specifically noted that “the board of directors is—or ought to be—a central player in shareholder engagement.” (SEC 2013). Echoing this view, numerous practitioner articles

suggest that directors can be particularly valuable for firms' IR in addressing shareholder concerns about governance matters such as executive compensation and leadership succession (e.g., Chudoba and Dennig 2018, Kingsdale Advisors 2019). Although directors are well positioned to contribute to such engagement, the IR function has traditionally fallen under the purview of executives and IROs rather than directors. This may reflect the fact that, unlike directors' governance and reporting responsibilities, which are clearly defined in corporate charters, their influence on IR strategy is neither mandated nor explicit. As a result, the well-documented influence of directors on governance policies cannot be directly extrapolated to their potential effect on investor relations.

2.2 The Effect of Directors

Earlier research on directors' influence on firm policies focuses largely on board-level characteristics such as board size, staggered reelection structures, or the proportion of independent directors. More recently, a growing body of work emphasizes the impact of individual directors' professional experiences on corporate outcomes. This emerging literature finds that directors' prior experiences at other companies can shape a focal firm's policies, such as CEO turnover and compensation, shareholder payouts, risk-taking, and accounting conservatism (Ellis et al. 2021, Gopalan et al. 2021, Zhang 2021, Kubick and Li 2023).

We build on this line of research to design our analysis, as focusing on individual directors' external experiences offers a relatively cleaner identification strategy compared to relying on board-level characteristics or firm-level events at the focal firm. In particular, we study directors' proxy fight experience at other companies because such events arise from shareholder dissatisfaction about the board's performance and are shown to impose substantial consequences on the directors involved (Fos and Tsoutsoura 2014, Zhang 2021). Because proxy fight experiences

are likely to alter directors' career incentives and future decision-making regarding shareholder engagement, this setting provides a powerful laboratory to isolate the effects of directors on firms' IR activities.

III. Data, Design, and Sample

3.1 Data and Measure of IR Activity

To measure firms' IR activity, we focus on their participation in IR events that facilitate private meetings with investors, as these interactions are known to be effective in preventing shareholder activism and are therefore most relevant to our proxy fight setting (Chapman et al. 2022). Private meetings serve this purpose in several ways. First, they enable firms to better understand investor demands and concerns through two-way conversations. Surveys indicate that firms prefer private, direct communication for investor engagement because it helps them better identify and address points of disagreement (BlackRock and Ceres 2015, First Sentier MUFG 2023). Consistent with this view, Chapman et al. (2022) suggests that two-way dialogue with investors creates less fertile ground for activists to gain influence over the larger shareholder base. Second, extensive research across sociology, psychology, and business emphasizes face-to-face interactions as a key mechanism for fostering rapport and relationships among participants (e.g., Goffman 1967, Forbes 2009, Roghanizad and Bohns 2017, Holmes 2018, Hadley et al. 2022). In the firm-investor context, private meetings are primary venues where firms initiate and strengthen relationships with influential shareholders (Bank of New York Mellon Corporation 2010).

Prior research identifies various types of IR events in which such private meetings can take place, including investor conferences organized by brokerages or industry associations, analyst/investor days (AI days), and non-deal roadshows (NDR) (e.g., Bushee et al. 2011, Green et al. 2014, Kirk and Markov 2016, Cheng et al. 2016, Bradley et al., 2021). We obtain data for firm participation in these IR events from Bloomberg Events, which track all publicly available

records of firm events (e.g., Green et al. 2014, Bradley et al. 2021). This dataset includes information on the event name, event type, date, hosting organization, and participating firms. While we caution that our data are unable to capture private meetings that lack any public documentation (e.g., ad hoc small-group meetings), Solomon and Soltes (2015), using proprietary data of *all* private meetings conducted by a NYSE-listed firm, shows that 85% of such meetings occur at investor conferences or roadshows, which are included in our dataset. This finding provides confidence that our data provides a reasonably comprehensive coverage of firm-investor interactions.⁵

Our IR event data is compiled at the firm-day level. For example, if a firm attends a two-day conference with multiple private meetings, it is recorded as two firm-day observations. Appendix C presents the breakdown of the different types of IR events at the firm-day level, matched to our final sample. Conference attendance accounts for over 91.5% of firm-days, followed by 6.6% for NDRs and 1.9% for AI days. The distribution closely mirrors that of the full Bloomberg data before matching, suggesting that our sample firms are comparable to typical Bloomberg-covered firms in terms of the types of events they attend.⁶

To comprehensively capture a firm's private meetings with investors, we construct our measure of IR activity, *IEvents*, by counting the total number of days a firm attends the aforementioned events within a year. While this measure approximates the quantity of time a firm dedicates to participating in all IR events, we acknowledge potential heterogeneity in the format and effectiveness of investor engagement across different event types. For example, conferences

⁵ Obtaining a full list of private meetings across a broad set of firms is inherently difficult, as firms themselves are not required to keep a record of these meetings (Solomon and Soltes 2015). Despite this limitation, we are not aware of any systematic bias in data coverage that would undermine our inferences. It seems reasonable to assume that firms with higher participation in publicly observable IR Events also conduct more overall IR activity, including private meetings that are not publicly observable.

⁶ Despite differences in datasets and sample periods, the IR event frequencies in our sample are broadly consistent with prior studies, including Green et al. (2014) for conferences and Kirk and Markov (2016) for AI days.

may allow firms to interact with a larger number of investors, offering greater breadth of engagement. NDRs typically involve smaller audiences, which may offer greater opportunities for investors to raise private concerns and for firms to receive individualized feedback (Bradley et al. 2021). AI days are more informal and often feature more senior executives, potentially enabling more in-depth communication (Kirk and Markov 2016). Given the difficulty in quantifying the relative effectiveness of different event types in building investor relationships (e.g., the number of meetings held at each event is unknown),⁷ we weight each firm-day equally across event types when constructing the *IREvents* measure for simplicity. To ensure this empirical choice does not compromise the validity of our inference, we repeat the main analysis using an alternative *IREvents* measure based solely on investor conferences, the predominant event type in our sample. This approach, which avoids issues related to combining or equal-weighting different events, yields similar results (untabulated).⁸

3.2 Research Design and Sample

To isolate the effect of directors on a focal firm's IR activity, we exploit individual directors' recent proxy fight experiences at other companies. This research design requires data on proxy fight target firms and the directors who serve on both the target and focal firms.

We begin by identifying U.S. target firms of proxy fights using data from SDC Platinum. For each target firm, we obtain the list of directors serving on its board during the year of the proxy fight from ISS Director data. We then identify focal firms, those that share a director with a target

⁷ Solomon and Soltes (2015, p.337) show that the number of private meetings per event is relatively similar across conferences, roadshows, and in-house events at corporate headquarters.

⁸ The DID estimate using only conference IR events is 0.624 ($t = 2.82$). Alternatively, when *IREvents* is based solely on non-conference IR events (e.g., AI days or NDRs), the DID effect remains positive but is not statistically significant (0.098, $t = 1.12$). This is likely due to (i) limited statistical power (only 8% of total firm-days correspond to attending non-conference IR events) and (ii) higher barriers to adding these IR events (e.g., higher costs and greater need for advance planning). Nonetheless, the consistent direction of the estimates offers some reassurance that the main effects are not exclusive to any specific type of IR events and that firms appear to manage their participation across different IR event types in a consistent manner.

firm in the proxy fight year (“treatment year”) and define them as treatment firms. We match each treatment firm ($Treat = 1$) to a control firm ($Treat = 0$) in the same Fama-French 48 industry that does *not* share directors with any target firm and has the closest market capitalization as of the year before the treatment year (Zhang 2021). Since shareholder activism tends to occur in industry waves (e.g., Bourveau and Scheonfeld 2017), it is important to match treatment and control firms within the same industry to ensure they perceive a similar activism threat. This matching approach helps attribute the observed effects at focal firms to directors’ personal experience of proxy fights.⁹ We define pre-period ($Post = 0$) and post-period ($Post = 1$) as $[-3, -1]$ and $[0, +3]$, respectively, where year 0 is the year of the proxy fight.¹⁰ We use a seven-year window (“test window”) to ensure that directors of focal firms are up for reelection at least once during the post-period.¹¹

We obtain financial and stock data from Compustat and CRSP. We exclude firms in the financial services and utilities industries and firms with dual-class ownership structures. We use stock tickers to merge the IR events with the rest of the dataset. Our final matched sample consists of 9,113 firm-year observations, corresponding to 1,155 unique proxy fights and 1,113 unique firms, spanning the period from 2004 to 2020.¹² See Appendix B for sample construction.

3.3 Sample Descriptive Statistics

Panel A of Table 1 reports summary statistics for the matched sample. Appendix A provides variable definitions. We winsorize all continuous variables at the 1 and 99 percent levels.

⁹ If the increase in IR activity at treatment firms is merely due to these firms’ observing proxy fights at target firms, we would find no DID effects, as control firms can observe the events through non-director public channels.

¹⁰ To ensure that treatment and control firm-year observations are distinct, we require that control firms do not share directors with any target firm facing proxy fights during the $[-6, +6]$ window. We also require that neither treatment nor control firms are targeted by any proxy fight in the pre-period.

¹¹ If multiple treatments occur for a given focal firm within any consecutive seven-year period (i.e., the seven-year test window of a treatment overlaps with that of another treatment), we keep both treatments but define the $Post$ indicator for those overlapping years based on the earlier treatment. Our main results are robust to excluding the second treatment entirely from the sample. See Section 4.5 for a detailed discussion.

¹² The sample begins in 2004 due to limited IR event coverage in Bloomberg prior to that year.

On average, firms in our sample spend 6.05 days per year participating in IR events (*IREvents*), including 5.34 days for investor conferences and 0.70 days for other types of investor interaction events such as AI days and NDRs (unpublished). Our sample firms have a median market capitalization of approximately \$2.9 billion and a median board size of nine members. The median firm has a market-to-book ratio of 1.7 and is followed by about 11 analysts. Summary statistics for firm and board characteristics are comparable to those reported in prior research (Zhang 2021).

Panel B of Table 1 presents the means and variances of key regression variables and reports the differences between treatment and control groups. The difference in the number of observations between the two groups is primarily due to the use of controls with replacement and missing data.¹³ While treatment and control firms are generally comparable, we observe statistically significant differences in several characteristics, resulting in an unbalanced sample.¹⁴ To control for any confounding effects from these observed differences, we implement entropy-balancing to achieve covariate balance on both the first and second moments between treatment and control firms (Hainmueller 2012). Panel C of Table 1 presents the entropy-balanced sample, showing that all covariate differences become indistinguishable from zero. We use this balanced sample in all our OLS regression analyses. In Panel D of Table 1, consistent with prior research, we find a positive correlation between IR activities and firm characteristics such as firm size, growth opportunities, and analyst coverage (e.g., Bushee and Miller 2011, Kirk and Vincent 2014). We also observe that firms with a more active IR function tend to exhibit lower stock return volatility (Chapman et al. 2019).

¹³ Given a seven-year test window, some firm-years are missing due to incomplete data for the main variables in Panel A of Table 1. Missing observations are more prevalent among control firm-years than treatment firm-years.

¹⁴ Because control firms are matched to treatment firms as of the year before the treatment event, firm characteristics between the two groups are allowed to differ over the seven-year test window.

IV. Empirical Results

4.1 The Effect of Corporate Directors on a Firm's IR Activity

To test the effects of directors' proxy fight experience on firms' IR activity, we estimate the following staggered DID specification:

$$IREvents_{it} = \beta_0 + \beta_1 Treat_{it} \times Post_{it} + \Gamma Ctrls + FixedEffects + \varepsilon_{it} \quad (1)$$

where i and t indicate a firm and a year, respectively. $IREvents$ is the number of days a firm participates in IR events during year t . $Treat$ equals one if the firm shares at least one director with a target firm in the proxy fight year, and zero otherwise. $Post$ equals one for the year of or after the proxy fight, and zero otherwise. The coefficient of interest, β_1 , captures the differential change in IR activity at treatment firms relative to control firms after the proxy fight at the target firm.

In a staggered DID estimation, some firms may serve as both treatment and control at different times and treatment effects may vary across groups or over time. This combination of treatment timing and treatment effect heterogeneity can introduce bias in the DID estimates. To address any potential bias, we follow recent studies by estimating β_1 within clean 2×2 cohorts (e.g., Goodman-Bacon 2021; Baker et al. 2022). Specifically, we define $Cohort$ based on the year of a proxy fight and include $Cohort \times Firm$ and $Cohort \times Year$ fixed effects in model (1).¹⁵ These fixed effects also control for unobserved time-invariant firm characteristics and time trends in IR activity. $Ctrls$ includes firm and board characteristics (Chapman et al. 2019, 2022, Zhang 2021). Standard errors are clustered at the firm level.

Table 2 presents the results from estimating model (1). In columns (1) and (2), we perform OLS regressions using the matched sample without and with control variables, respectively. In

¹⁵ All the treatment and control firm-year observations that correspond to proxy fights occurring in any given calendar year will all belong to the same $Cohort$. The standalone variables, $Treat$ and $Post$, are therefore subsumed by the $Cohort \times Firm$ and $Cohort \times Year$ fixed effects, respectively.

both columns, we find that β_1 is positive and significant (e.g., 0.758, $t=3.05$ in column (1)). The magnitude of the effect indicates that treatment firms, following their directors' proxy fight experience, spend 0.758 more days per year attending investor interaction events (about 15% of the sample median value of *IREvents*), relative to the control firms. The magnitude of the effect after including control variables (see column (2)) is almost identical to that in column (1). Finally, we also estimate model (1) using Poisson regressions because the dependent variable is a count variable. We report the results in columns (3) and (4). We find that β_1 is comparable in statistical and economic significance to those in columns (1) and (2). Treatment firms show a 12% ($= e^{0.114} - 1$) greater increase in their time commitment to attending IR events following their directors' proxy fight experience, compared to control firms. Overall, these results suggest that directors who experience proxy fights at other companies subsequently prompt focal firms to increase IR activity.

4.2 Parallel Trends Assumption

A critical identifying assumption of any DID analysis is the absence of differential pre-trends between treatment and control firms. To assess the validity of the parallel trends assumption, we examine the dynamic treatment effects on *IREvents*. Specifically, we modify model (1) by replacing *Post* with six year-specific indicator variables (i.e., *Year-2*, *Year-1*, *Year0*, *Year+1*, *Year+2*, and *Year+3*) to estimate yearly DID effects over the test window. We use *Year-3* as the benchmark period, instead of the average of *Years-3* to *Year-1* as in model (1).

Table 3 and Figure 1 report the results of this analysis. Columns (1) and (2) of Table 3 present the results without and with control variables, respectively. In both columns, the coefficient estimates on *Treat* \times *Year-2* and *Treat* \times *Year-1* are statistically insignificant. This mitigates concerns regarding pre-existing differences in IR trends between treatment and control firms, supporting the validity of our DID design.

For the post-period, we find that all yearly DID coefficients are statistically significant. For example, the coefficient on $Treat \times Year0$ ($Treat \times Year+I$) indicates that treatment firms' increase in IR event participation in $Year0$ ($Year+I$), relative to $Year-3$, exceeds that of control firms by 1.410 days (1.235 days) (column (2)). Given that the magnitude of DID effects tapers over time, we check the robustness of yearly estimates using alternative benchmark years (e.g., $Year-1$ or $Year-2$ instead of $Year-3$). In untabulated tests, we find that the DID effects in $Year0$ and $Year+I$ remain robust across benchmark choices, whereas the effects in $Year+2$ and $Year+3$ lose statistical significance at conventional levels. Taken together, these findings suggest that the effect of directors' proxy fight experience on IR activity is strongest in the treatment year and the following year, and then appears to diminish thereafter.¹⁶ This pattern is consistent with the career concerns of interlocking directors, who likely have stronger incentives to promote IR activity at the focal firm *before* their first reelection at the firm than after.¹⁷ It also aligns with prior evidence that the impact of individual experience tends to decline with time (Watson 1930, Erev and Roth 1998, Dittmar and Duchin 2015, Kubick and Li 2023).

4.3 Heterogeneous Effects Tests

4.3.1 Costs of Directors' Proxy Fight Experiences

We conduct cross-sectional analyses to support our inference that the main effects are driven by directors' experiences. Prior research highlights a nonlinear relation between the personal experiences and their impact, showing that experiences can differentially shape how

¹⁶ To assess if the main effects significantly decline in the post-period, we compare the estimates for post-years to those in $Year0$ (the treatment year). The differences are marginally significant in $Year+2$ and $Year+3$ (-0.609, $t=-1.65$ and -0.606, $t=-1.49$) but insignificant in $Year+1$ (-0.175, $t=0.58$), suggesting that the DID effects of directors' experiences on IR activity remain similar in magnitude for at least two years following the treatment.

¹⁷ Directors likely have the strongest incentives to advocate for increased IR activity before their first reelection at the focal firm following a proxy fight experience. After this initial reelection, they either secure their position through reappointment or exit from the firm. Thus, the proportion of career-concerned interlocking directors in treatment firms decreases over time, naturally leading to a pronounced effect in the early post-period that gradually declines thereafter.

individuals later evaluate the risks associated with similar events conditional on the costs or severity of the past experiences. For example, Gao, Liu, and Shi (2020) documents a negative (positive) association between households' experiences of nonfatal (fatal) earthquakes and their subsequent life insurance purchases. Similarly, Bernile et al. (2017) and Gopalan et al. (2021) find that CEOs who have encountered events such as natural disasters and bankruptcies engage in *greater* risk-taking behaviors when those experiences were relatively costless (e.g., disasters with limited economic damage or shorter, less-costly bankruptcies). Applying this insight to our setting, we predict that the increase in IR activity at focal firms will be stronger when interlocking directors are exposed to more costly proxy fights, where the importance of robust IR becomes evident.

To capture the costs associated with proxy fight experiences, we use two proxies. First, we use an indicator for whether the target firm loses the proxy fight. When a target firm loses, the firm typically makes significant concessions to dissident shareholders, such as executive/ director turnover and forced changes in firm policies (Gow et al. 2024). These negative outcomes should heighten directors' awareness of the costs of proxy fights and the importance of IR as a preventive strategy. Second, we assess whether the interlocking director is prescheduled for reelection at the *target firm* during the proxy fight. Fos and Tsoutsoura (2014) finds that, in a staggered board where only a subset of directors face reelection each year, those up for reelection during a proxy fight experience greater career consequences than those who are not.¹⁸ Accordingly, an interlocking director scheduled for reelection at the target firm during a proxy fight is likely to perceive the experience as more costly and thus be more motivated to promote IR activity at focal firm than an interlocking director who does not face reelection during a proxy fight. To test heterogeneous DID

¹⁸ Directors who are up for reelection during proxy fights face a greater risk of losing their board seats not only at the target firm but also at other non-target firms, compared to directors who are not up for reelection during proxy fights.

effects based on these two proxies, we alter model (1) by decomposing the treatment group into two subgroups as follows:

$$IREvents_{it} = \beta_0 + \beta_1 TreatHigh_{it} \times Post_{it} + \beta_2 TreatLow_{it} \times Post_{it} + \Gamma Ctrls + \\ FixedEffects + \varepsilon_{it}. \quad (2)$$

TreatHigh (*TreatLow*) is an indicator variable that equals one (zero) if: (i) the target firm loses the proxy fight to dissidents, or (ii) the interlocking director is predetermined to be up for reelection at the target firm in the year of the proxy fight, and zero (one) otherwise.^{19, 20} The coefficient β_1 (β_2) represents a DID estimate derived from comparing *TreatHigh* (*TreatLow*) and control groups, and the difference between β_1 and β_2 captures the varying magnitudes of the main DID effects across these partitions. We expect the difference to be positive (i.e., $\beta_1 - \beta_2 > 0$), indicating a stronger DID effect for the *TreatHigh* subsample compared to the *TreatLow* subsample.

We present the results in Table 4. In column (1), we find that the difference between β_1 and β_2 is positive and significant (0.781, $t = 2.10$), consistent with the idea that the costliness of directors' experiences moderates the main effects. Specifically, the DID effects for treatment firms with a director who experiences a lost proxy fight (*TreatHigh*=1) is, on average, 0.78 days greater than the DID effects for treatment firms with a director who experiences a proxy fight that was won (*TreatLow*=1). The lack of significant DID effects among the *TreatLow* subsample suggests that directors may not perceive an increase in IR activity to be warranted or valuable when their

¹⁹ For ease of interpretation, we use the label *TreatHigh* (*TreatLow*) to indicate subsamples where treatment effects are expected to be stronger (weaker) across all heterogeneity tests. Both *TreatHigh* and *TreatLow* are set as zero to denote control firms.

²⁰ Given that the partitioning variables are based on *target* firm characteristics, it is unlikely that the effects of the control variables—all derived from *focal firm* characteristics—on the focal firm's IR activity would systematically differ across the state of the partitioning variables. As a robustness check (untabulated), we modify model (2) to include interaction terms between *TreatHigh* and all control variables as well as interaction terms between *TreatLow* and all control variables. The results remain quantitatively and qualitatively similar.

proxy fight experience is less costly. This result aligns with the prior evidence that experiencing a crisis without adverse outcomes can lower an individual's estimated costs of similar future events.

In column (2), we examine heterogenous DID effects based on interlocking directors' reelection status at target firms during the proxy fight. We find modest evidence that the DID effects are stronger when the interlocking director is up for reelection at the target firm during the proxy fight. Both β_1 and β_2 are positive and statistically significant, with β_1 more than twice the magnitude of β_2 . However, the difference between the two is not significant at conventional levels (0.981, $t = 1.54$). One interpretation is that this proxy—unlike the more clear-cut distinction between winning or losing a proxy fight—may lack the precision needed to pinpoint the threshold at which the nonlinear effects of experiences emerge (e.g., Bernile et al. 2017), because interlocking directors not up for reelection may still be negatively affected by the proxy fight. Thus, we caution readers against drawing strong conclusions from this result.

4.3.2 Benefits of Increasing IR Activity at Focal Firms

At the core of directors' incentives to influence a focal firm's IR activity following a proxy fight experience is their desire to avoid encountering another activism-related crisis. To support our inference that the main effects are driven by these incentives, we exploit two focal firm conditions under which directors are likely to have stronger motivation to promote IR activity: (i) when the focal firm's shareholder base is more likely to allow for effective IR activity in deterring activism, and (ii) when the focal firm is more exposed or vulnerable to potential activist intervention.

To capture the first condition, we utilize variation in the proportion of long-term institutional ownership across focal firms. Prior research and practitioner reports suggest that long-term institutional investors play a pivotal role during activist campaigns and that IR efforts are

more effective when directed toward them, as firms cannot count on transient investors who may not hold shares when their support is most needed (Kingsdale Advisors 2019, Chapman et al. 2022). For example, a 2015 Ernst and Young survey notes that “companies are taking proactive measures to prepare for potential activist investor campaigns, including engaging *long-term* institutional investors.”²¹ Because experiencing proxy fights highlights the importance of cultivating shareholder allies, we expect interlocking directors to be more likely to push for increased IR activity when a higher proportion of the focal firm’s shares are held by long-term institutional investors.

To measure the second condition (i.e., a focal firm’s vulnerability to shareholder activism), we use two proxies: (i) pre-existing levels of IR activity, and (ii) anti-takeover provisions. Chapman et al. (2022) suggest that robust IR engagement makes it harder for activists to rally shareholder support for their proposed changes, thereby deterring both initiation and escalation of activist campaigns. Similarly, anti-takeover provisions act as barriers that limit activists’ ability to gain control of the board and management, offering directors a degree of insulation from activist pressure. Therefore, if the main effects are due to director incentives to avoid another proxy fight, we expect them to be particularly motivated to promote IR at focal firms that lack such protective mechanisms.

To test this prediction, we estimate model (2), which partitions treatment firms into *TreatHigh* and *TreatLow* groups. To capture variation in long-term shareholder composition, *TreatHigh* (*TreatLow*) equals one (zero) if the focal firm is *above* the sample median on any of the following measures, and zero (one) otherwise: (i) the average holding period of institutional investors, (ii) the ratio of institutional holdings of at least five years relative to total institutional

²¹ <https://corpgov.law.harvard.edu/2015/04/10/shareholder-activism-an-engagement-opportunity/>

holdings, or (iii) the ratio of dedicated and quasi-indexing institutional ownership to total institutional holdings (Bushee 1998). Similarly, to assess variation in pre-existing protective mechanism against activism, *TreatHigh* (*TreatLow*) is defined as one (zero) if the focal firm is *below* the sample median on any of the following measures, and zero (one) otherwise: (i) the frequency of *IREvents* participation during the pre-period or (ii) the level of anti-takeover provisions (*Eindex*) in the year preceding the treatment year.²² We measure the latter using Bebchuk et al. (2009)'s E-index, which sums the presence of the following features: a staggered board, limits to shareholder bylaws amendments, poison pills, golden parachutes, and supermajority requirements for mergers and charter amendments. The index ranges from zero to six, with higher values indicating stronger takeover defenses. We expect to observe a greater DID effects for the *TreatHigh* group than for the *TreatLow* group (i.e., $\beta_1 - \beta_2 > 0$).

We present the results in Table 5. Panel A shows positive and significant differences in the DID effects between treatment firms with a shareholder base characterized by more long-term institutional investors and focal firms without (e.g., $\beta_1 - \beta_2 = 1.019$, $t = 3.48$ in column (1)). In terms of economic magnitude, the increase in IR activity following directors' proxy fight experiences is 2.3-4.6 times larger for the *TreatHigh* group, relative to the *TreatLow* group (i.e., 1.298/0.279, 1.157/0.415, and 1.087/0.469 in columns (1), (2) and (3), respectively). These results suggest that interlocking directors are more likely to advocate for increasing IR activity at the focal firm when the firm has an investor base conducive to long-term relationship-building and thereby mitigating activism risk.

²² We use the median values to convert continuous partitioning variables into binary indicators. This method simplifies exposition of the results and ensures consistency across tables. Given the lack of evidence on the specific form in which the underlying constructs (i.e., the effectiveness of IR activity and the firm's vulnerability to activism) vary as a function of these partitioning variables, we use the median values to create two subsamples of roughly equal size. Appendix D presents the correlation matrix of partitioning variables used in Tables 4 and 5, showing that the cross-sectional partitions applied in different tables do not capture the same underlying constructs.

In column (1) of Panel B in Table 5, we find a positive and significant heterogeneity in the main effects ($\beta_1 - \beta_2 = 0.734$, $t = 2.27$), suggesting that directors' proxy fight experiences lead to a greater increase IR activity at focal firms that have not already established strong shareholder engagement practices. Similarly, column (2) shows that the DID effects are larger and statistically stronger ($\beta_1 - \beta_2 = 0.603$, $t = 2.16$) for focal firms with fewer anti-takeover provisions, compared to focal firms with more provisions. These results suggest that directors may feel a heightened urgency to strengthen shareholder relations when the firm and the board are more exposed to shareholder activism. Overall, the results in Table 5 suggest that our main effect is stronger when directors perceive greater net benefits from IR activity as a means of avoiding future activism.²³

4.4. Alternative Explanations: Selection into Board Interlocks

We utilize board interlocks to identify the effect of directors' external experiences on a focal firm's policies. A common concern in studies based on board networks is that interlocks are not randomly formed, raising the possibility of selection bias. In our setting, any time-invariant confounding factors are controlled for by *Cohort* \times *Firm* fixed effects. However, if some unobserved time-varying firm characteristic led both target and focal firms to appoint the same director, these shared traits could still possibly make focal firms more susceptible to activism than control firms in later years, which in turn could explain the observed greater increase in IR activity at focal firms. To address this concern, we conduct several additional analyses.

4.4.1. Placebo Analyses

²³ While we do not make formal predictions for the coefficients on *TreatLow* \times *Post* (β_2), the positive yet insignificant estimates of β_2 in Table 5 are not inconsistent with our story. Because IR is a firm-level policy primarily carried out by executives and IROs, directors may only be able to increase IR activity when there is a clear and positive net benefit for the firm as a whole. If the marginal value of additional IR efforts in mitigating activism plateaus beyond a certain threshold, firms that have already reached that point through preexisting IR practices or antitakeover measures are less likely to allocate further corporate resources to IR. For reference, we also show the time trends of the differences in the DID coefficient estimates (i.e., $\beta_1 - \beta_2$) reported in Tables 4 and 5 in the Internet Appendix, although the assumptions for triple-differences are not formally agreed upon in the literature (Olden and Moen 2022).

To assess the extent to which confounding factors related to the formation of board interlocks might influence our main results, we conduct placebo analyses using placebo-treatment firms. Specifically, we identify firms that shared directors with target firms in the period immediately preceding the proxy fights but not during the proxy fight year. These placebo-treatment firms do not have interlocking directors with proxy fight experience but still likely possess firm characteristics that led to the board interlocks with target firms in the recent past. If our findings are primarily driven by selection bias, we would expect to find significant DID estimates when comparing these placebo-treatment firms to control firms.

We follow the same procedure as in the main analysis to match placebo-treatment firms to control firms and re-estimate model (1) using this matched sample. In Panel A (Panel B) of Table 6, we present the results for placebo-treatment firms that shared directors with target firms one year (two years) prior to, but not during, the year of the proxy fight. We identify 141 and 245 unique placebo-treatment firms in Panel A and Panel B, respectively. In both panels, we find no significant difference in IR activity at placebo-treatment firms after the proxy fight relative to control firms, with most DID estimates being negative and insignificant. Although we acknowledge that severing a board interlock may also be non-random and that these analyses may have lower statistical power due to the smaller samples compared to Table 2, the results in Table 6 are inconsistent with our findings being driven by firm selection into board interlocks.

4.4.2 Interlocking Director's Reelection Year at the *Focal Firm*

In a staggered board, directors stand for reelection in predetermined cycles rather than annually, creating within-firm variation in the director's reelection timing that is plausibly exogenous to factors that drive board interlocks among firms. To further address the selection concern, we exploit within-firm intertemporal variation in an interlocking director's incentives to

promote IR activity at the *focal firm*—i.e., the variation between the director’s reelection and non-reelection years at the focal firm during the post-period.

Fos and Tsoutsoura (2014) suggest that directors who experience proxy fights at one firm face an increased risk of losing board seats at other non-target firms where they also serve. As a result, career-concerned interlocking directors are more likely to prioritize maintaining strong shareholder relations at focal firms during their own election years, compared to non-election years when their positions are not immediately at risk. If our main findings stem from directors’ proxy fight experiences, we should observe the following pattern: within the four years after the proxy fight, IR activity at focal firms is higher in years when the interlocking director is up for reelection at focal firms than in years when they are not. We test this hypothesis using the following model:

$$IREvents_{it} = \beta_0 + \beta_1 UpForElection_{it} + \Gamma Ctrs + FixedEffects + \varepsilon_{it} \quad (3)$$

UpForElection is an indicator variable that is set to one in years when interlocking directors are scheduled for reelection at focal firms and zero in years when they are not up for reelection at these firms. *Ctrs* refers to the same control variables as those in model (1). We restrict the sample to treatment firms in the post-period (i.e., subsequent to proxy fights) in estimating model (3). We include both firm and year fixed effects in our estimations. β_1 is the coefficient of interest and we predict it to be positive and significant.

Table 7 reports the estimation results. Columns (1) and (2) are based on all focal firms and focal firms with a staggered board structure, respectively. We find that the coefficients of interest are positive and significant in both the columns, and the magnitude is about 20% greater when focusing only on firms with a staggered board structure ($\beta_1 = 0.309$, $t = 1.68$ in column (1) and $\beta_1 = 0.377$, $t = 2.09$ in column (2)). The economic magnitude suggests that focal firms spend approximately 0.3 more days attending IR events in years when interlocking directors are up for

reelection, compared to years when they are not. These results support our inference that the increased IR activity at focal firms are due to interlocking directors' proxy fight experience.

4.4.3 Impact Threshold of Confounding Variable (ITCV)

We assess the robustness of our main results to potential correlated omitted variables using the ITCV procedure (e.g., Larcker and Rusticus 2010, Frank 2000). This method calculates the minimum effect size an unobservable omitted variable would need, relative to control variables, to invalidate the main result. We apply the ITCV procedure on the estimation results of model (1). The untabulated analysis ranks the influence of each control variable on the DID estimates, showing that firm size and ROA are the most and second-most impactful controls, respectively. We find that an omitted variable would need to have an effect at least 34% greater than that of firm size and 293% greater than that of ROA to overturn our inference drawn from the DID estimates.²⁴

Taken together, the results in Section 4.4 suggest that for selection effects to fully explain our main findings, the unobserved confounding factors would need to satisfy three stringent conditions: (i) they must arise during the treatment year and not in the years prior, (ii) they must be correlated with the timing of interlocking directors' reelection years at focal firms, and (iii) they must exert a larger impact than firm size or ROA on IR activity. Such a confounding factor appears to be unlikely, lending support to our inference that the effects on IR activity are driven by directors.

4.5 Robustness Tests: Alternative Methods for Constructing Treatment and Control Samples

Given that we use a seven-year window surrounding a director's proxy fight experience, it is possible that the test window of one treatment overlaps with that of another treatment. In our analyses, we keep both treatments and, for those overlapping years, we code the *Post* dummy

²⁴ In untabulated analysis, we further test whether common ownership between the target and focal firms could explain our main results. Specifically, we re-estimate model (1) after including an additional interaction term between *Post* and an indicator variable for the presence of blockholders holding stakes in both the target and focal firms. The coefficient of interest, β_1 , remains robust to the addition of this control.

according to the earlier treatment, assuming lasting impacts of the treatment. In this section, we check the robustness of our results using an alternative approach that retains only the first treatment in cases of overlaps. We re-estimate model (1) using this approach and present the OLS regression results in columns (1) and (2) of Table 8. In both columns, the statistical and economic significances of the DID estimates are comparable to those in columns (1) and (2) of Table 2, and our inference remains unchanged.

Baker et al. (2022) discusses the estimation issues with a staggered DID design. In particular, using already-treated units as controls for later-treated units can bias staggered DID estimates. This issue occurs in only 5.1% (=469/9,113) of the cases in our sample, suggesting that the potential bias from the “bad comparison” problem is trivial. Our main specification also mitigates this concern by including *Cohort*×*Firm* and *Cohort*×*Year* fixed effects. Furthermore, we re-estimate model (1) after eliminating all cases where a firm that was treated earlier serves as a matched control for a firm treated later in the sample. The results, reported in columns (3) and (4) of Table 8, are similar to those in Table 2. Overall, our inferences are robust to alternative, more conservative sample construction procedures designed to minimize potential bias.

V. Additional Analyses: Purpose of Increased IR Activity at Focal Firms

The existing IR literature suggests two main purposes of a firm’s IR activity: (i) fostering shareholder relationships to deter activism (Chapman et al. 2022) and (ii) addressing investors’ informational needs by conveying firm-specific information and reducing market uncertainty (e.g., Kirk and Markov 2016, Bradley et al. 2021). Thus far, we interpret our findings as evidence that directors, motivated by a desire to prevent future activism, prompt firms to increase IR activity. Our empirical measure *IEvents*, which is intended to capture face-to-face interactions, also reflects this emphasis on the relationship-building role of IR activity rather than its informational

function. To support our interpretation and measurement, we conduct additional analyses to examine which of these two purposes more closely ties to the observed increase in firms' IR activity.

5.1 Activism Prevention

Chapman et al. (2022) finds that an IR program helps maintain shareholder satisfaction with both the firm and the board, thereby reducing the risk of shareholder activism, particularly aggressive activism such as proxy fights. Building on this insight, we examine whether the observed increase in focal firms' participation in IR events following a director's proxy fight experience at another companies decreases the likelihood of future proxy fights at focal firms over the same period. To test this hypothesis, we estimate the following model for focal firms:

$$\Delta\text{Proxy Fights}_j = \beta_0 + \beta_1\Delta\text{IREvents}_j + \Gamma\Delta\text{Ctrls} + \text{FixedEffects} + \varepsilon_j \quad (4)$$

where j indicates a treatment event. $\Delta\text{Proxy Fights}$ and $\Delta\text{IREvents}$ refer to the change in the average incidence of proxy fights and IR activities at focal firms from the pre- to post-periods, respectively. Similarly, we transform all control variables included in model (1) to change variables. We also include controls for industry and year fixed effects.²⁵ Standard errors are clustered at either the industry or year level. If the increase in IR activity at focal firms is mainly driven by the motivation to build a more supportive shareholder base and deters potential activism, we expect a negative association between $\Delta\text{IREvents}$ and $\Delta\text{Proxy Fights}$ (i.e., $\beta_1 < 0$).

Table 9 reports the results from estimating model (4). Columns (1) and (2) show a negative and statistically significant coefficient on $\Delta\text{IREvents}$ (β_1), suggesting that the increased IR activity at focal firms is associated with a reduced likelihood of these firms experiencing proxy fights. Given that the base rate of proxy fights among focal firms during our sample period is about 1%,

²⁵ In model (4), the unit of analysis is a focal firm (i.e., one observation per firm), precluding the inclusion of firm fixed effects.

the coefficient estimate indicates that one additional day of IR event participation per year in the post-period (relative to the pre-period) corresponds to a 10% ($-0.001/0.01 = -0.1$) reduction in the probability of a proxy fight. The results likely underestimate the full benefits of the increased IR activity at focal firms, because the benefits of shareholder engagement and relationship-building tend to accrue over the long term, potentially extending beyond the duration of our test window, and can manifest in various forms (for example, only a subset of activist campaigns escalate to proxy fights). In addition, these results suggest that the director-driven increase in IR activity benefits the firms as a whole, rather than serving only the interests of individual directors, consistent with the valuable advisory role of corporate directors (e.g., Adams 2009, Adams and Ferreira 2007, Dass et al. 2014).

5.2. Information Delivery

Prior studies find significant short-term market reactions around investor interaction events attended by firms (e.g., investor conferences, AI days), indicating that firms may use these events as a channel to convey or clarify information to investors (e.g., Kirk and Markov 2016, Bradley et al. 2021). To explore whether the director-driven increase in IR activity reflects this informational motive, we examine whether the average informativeness of IR events attended by focal firms increases after directors' proxy fight experiences. Specifically, we compute the absolute three-day cumulative abnormal stock return (*AbsCAR*) surrounding each IR event used in our *IEvents* measure. This analysis is based on 9,407 firm-day observations of IR events in the pre-period and 19,714 observations in the post-period. We then compute the average values of *AbsCAR* for the firm-day observations of focal firms in pre- and post-periods separately. If the increased IR activity at focal firms is primarily driven by incentives to meet investors' informational needs, we would expect a significant rise in the average *AbsCAR*.

Table 10 reports that the average *AbsCAR* of IR events attended by focal firms is 2.00% in the pre-period and 2.02% in the post-period, with the difference being neither economically nor statistically significant. While the null results are not conclusive evidence, they are inconsistent with the interpretation that focal firms' increased participation in IR events following directors' proxy fight experiences is primarily aimed at information delivery. Taken together, the results in Section 5 align well with our interpretation of the main findings, as directors with such experiences are likely to guide firms to focus their IR efforts primarily on building better shareholder relationships to prevent future activism, rather than on addressing short-term informational needs of investors.

VI. Conclusion

This paper studies the influence of corporate directors on firms' IR activity. Specifically, we investigate whether directors' recent experiences of proxy fights at other companies affect the level of IR activity at the focal firms. Using a DID approach, we find that focal firms increase their IR activity following their directors' exposure to proxy fights at other companies, suggesting that director experiences shape a firm's IR function. The effect is stronger when directors experience a costly proxy fight; for example, when the target firm loses to activists. The effect is also more pronounced when directors have stronger incentives or face greater urgency to promote IR at focal firms in order to avoid activism; for example, when the focal firm has a higher proportion of long-term institutional investors, lower pre-existing IR engagement, or weaker anti-takeover provisions. Robustness checks, including analyses based on predetermined director reelection years, placebo-treatment firms, and the ITCV procedure, collectively suggest that our main results are not driven by omitted factors potentially driving the formation of board interlocks between focal and target firms. Additional analyses show that these director-driven increased IR activities are associated

with a reduced likelihood of future proxy fights at focal firms. Overall, our study provides the first systematic empirical evidence on the impact of corporate directors on firms' IR function.

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Appendix A. Variable Descriptions

Variable	Definition
IREvents	The number days that the firm participates in IR events during the year; IR events include broker-hosted/industry investor conferences, AI days, NDRs, analyst marketing meals, etc. [Bloomberg]
Treat	An indicator that equals one if the firm shares directors with the target firm of the proxy fight during the proxy fight year, and zero otherwise [ISS Directors, SDC Platinum]
Post	An indicator that equals one for the year of or years after the proxy fight, and zero otherwise [ISS Directors, SDC Platinum]
Marketcap	The natural logarithm of the market value of equity [Compustat]
Mtb	The ratio of the market value of total assets to the book value of total assets [Compustat]
StockRet	The stock return over the year [CRSP]
RetVol	The standard deviation of monthly stock returns over the previous five years [CRSP]
InstOwn	The total percentage of shares owned by institutional investors [Thomson/Refinitiv Institutional Holdings (13F)]
AnaCov	The number of analysts following the firm [I/B/E/S]
R&D	The ratio of R&D expense to total assets (set to zero if missing) [Compustat]
Roa	Net income minus special items divided by total assets [Compustat]
Lev	The sum of long-term debt and debt in current liabilities divided by total assets [Compustat]
BoardSize	The total number of directors on the board [ISS Directors]
BoardIndep	The percentage of independent directors on the board [ISS Directors]
CEOduality	An indicator equal to one if the CEO is also the chairman of the board and zero otherwise [ISS Directors]
DirectorOwn	The total percentage of shares owned by all directors [ISS Directors]
DirectorBusyness	The percentage of directors holding at least three board seats in public companies [ISS Directors]
DirectorCooption	The percentage of directors appointed after the CEO assumed office [ISS Directors]
Proxy Fight	An indicator equal to one if a firm experiences a proxy fight in the year, and zero otherwise [FactSet Corporate Governance]
AbsCAR	The absolute market-adjusted cumulative stock return during the window [-1, +1] around the IR event date (in percentage) [CRSP]

Appendix B. Sample Construction

This table details the construction of the sample and outlines the composition of the subsamples used in each table.

	N	Used in
Firm-years covered by Compustat, ISS Directors, CRSP, IBES, and Thomson Reuters 13F (Primary sample used in empirical analysis)	9,113	Table 1; Table 2; Table 3; Columns (1)-(2) of Table 5, Panel A; Table 5, Panel B
Primary sample less: unresolved or pending proxy fights	8,104	Columns (1) of Table 4
Primary sample less: treatment firms interlocked to target firms with non-staggered boards	5,770	Column (2) of Table 4
Primary sample less: observations without Bushee's Institutional Investor Classification data	8,299	Column (3) of Table 5, Panel A
Treatment firm-years in the post period	3,082	Column (1) of Table 7
Treatment firm-years with staggered boards in the post period	1,086	Column (2) of Table 7
Treatment events less: observations with a proxy fight (from FactSet) in the pre-period	1,144	Table 9
IR events that the treatment firms attend over the test window	29,121	Table 10
Alternative definitions of the treatment group	6,701 or 8,642	Table 8
Placebo treatment samples	1,826 or 3,301	Table 6

Appendix C. Distribution of Investor Events by Type

This table is based on 55,580 firm-day level investor interaction events from the Bloomberg Event dataset, matched to our final sample firm-years. The table presents the distribution by the type of events.

Classification	Frequency	%	Note
Conferences	50,879	0.915	This classification includes investor conferences organized by sell-side brokerage firms or industry organizations
NDRs	3,642	0.066	This classification includes non-deal roadshows and other sell-side analyst organized meals
Analyst/Investor days	1,059	0.019	This classification includes investor days, analyst days, capital market days, site visits, field trip, and other tours

Appendix D. Correlation Matrix for Partitioning Variables

This table presents the correlation coefficients for partitioning variables used in Tables 4 and 5. Values in bold indicate statistical significance at 5% or better.

	1	2	3	4	5	6
1. Proxy fight outcome						
2. Up-for-relection at target firm	-0.027					
3. Average holding period of institutional investors	0.045	-0.084				
4. Proportion of long-term institutional investors	0.018	-0.047	0.743			
5. Proportion of dedicated investors & quasi-indexers	0.045	-0.039	0.260	0.255		
6. Existing frequency of IR events	0.014	-0.139	0.337	0.323	0.205	
7. Anti-takeover measures (Eindex) at focal firms	0.025	-0.012	-0.037	-0.042	0.003	-0.103

Figure 1. Dynamic Effects of Directors' Proxy Fight Experience on Investor Outreach

This figure plots the coefficient estimates from the dynamic DID regression reported in column 2 of Table 3. The horizontal axis represents the time in years relative to a treatment event. Each point corresponds to a coefficient estimate. The vertical bars represent the confidence intervals at the 95% level.

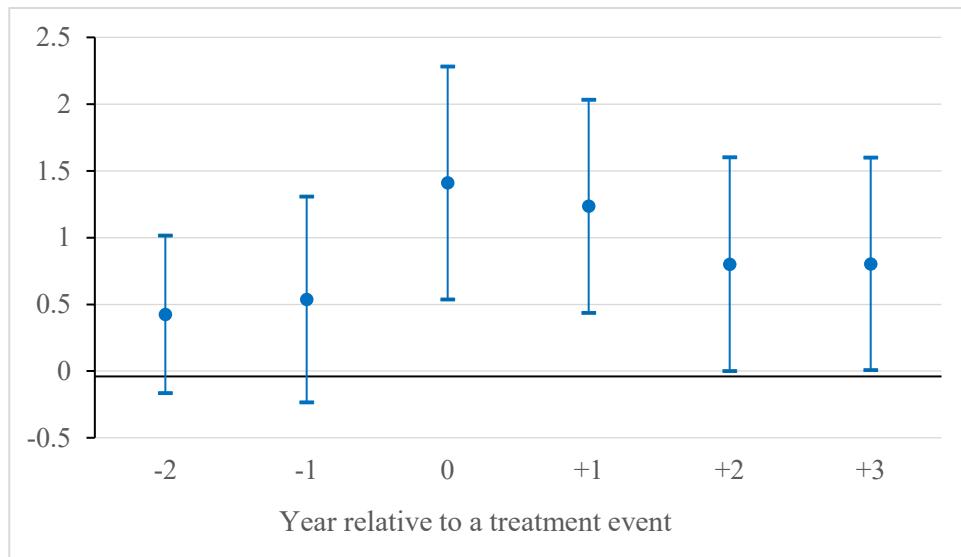


Table 1. Summary Statistics

Panels A to C report descriptive statistics for key variables. The sample is based on S&P 1500 firms and spans 17 years from 2004 to 2020. Panel D presents the correlation coefficients for key variables. Values in bold indicate statistical significance at 5% or better. Appendix A provides a description of variables. All continuous variables are winsorized at the 1 percent and 99 percent levels.

Panel A. Full sample						
Variable	N	Mean	Std Dev	Q1	Median	Q3
IREvents	9,113	6.054	6.150	1.000	5.000	9.000
Marketcap	9,113	8.141	1.521	7.043	7.964	9.137
Mtb	9,113	2.091	1.180	1.325	1.734	2.455
StockRet	9,113	0.144	0.380	-0.086	0.115	0.328
RetVol	9,113	0.105	0.044	0.074	0.096	0.127
InstOwn	9,113	0.821	0.210	0.740	0.857	0.947
AnaCov	9,113	12.310	7.765	6.000	11.000	17.000
R&D	9,113	0.028	0.045	0.000	0.004	0.039
Roa	9,113	0.080	0.073	0.043	0.074	0.116
Lev	9,113	0.228	0.178	0.080	0.218	0.338
BoardSize	9,113	9.183	2.054	8.000	9.000	11.000
BoardIndep	9,113	0.760	0.180	0.714	0.800	0.875
CEOduality	9,113	0.460	0.501	0.000	0.000	1.000
DirectorOwn	9,113	0.060	0.104	0.008	0.021	0.055
DirectorBusyness	9,113	0.239	0.180	0.111	0.222	0.364
DirectorCooption	9,113	0.490	0.318	0.222	0.455	0.750

Panel B. Treatment and Control groups (Unbalanced)								
Variable	Treatment (1)			Control (2)			Difference (1) – (2)	
	N	Mean	StdDev	N	Mean	StdDev	Mean	P-value
IREvents	5,140	6.811	6.635	3,973	5.074	5.301	1.737***	(0.000)
Marketcap	5,140	8.537	1.589	3,973	7.629	1.254	0.908***	(0.000)
Mtb	5,140	2.065	1.140	3,973	2.125	1.230	-0.060**	(0.016)
StockRet	5,140	0.145	0.363	3,973	0.143	0.402	0.002	(0.807)
RetVol	5,140	0.100	0.042	3,973	0.112	0.044	-0.013***	(0.000)
InstOwn	5,140	0.813	0.211	3,973	0.833	0.208	-0.020***	(0.000)
AnaCov	5,140	13.750	7.987	3,973	10.440	7.045	3.302***	(0.000)
R&D	5,140	0.027	0.045	3,973	0.029	0.046	-0.002**	(0.011)
Roa	5,140	0.080	0.071	3,973	0.080	0.076	0.000	(0.945)
Lev	5,140	0.245	0.173	3,973	0.207	0.183	0.038***	(0.000)
BoardSize	5,140	9.729	1.989	3,973	8.476	1.917	1.253***	(0.000)
BoardIndep	5,140	0.785	0.176	3,973	0.729	0.180	0.056***	(0.000)
CEOduality	5,140	0.471	0.500	3,973	0.445	0.501	0.027**	(0.012)
DirectorOwn	5,140	0.045	0.092	3,973	0.079	0.115	-0.034***	(0.000)
DirectorBusyness	5,140	0.298	0.173	3,973	0.163	0.160	0.134***	(0.000)
DirectorCooption	5,140	0.473	0.307	3,973	0.512	0.331	-0.039***	(0.000)

Panel C. Treatment and Control groups (Entropy balanced)

Variable	Treatment (1)			Control (2)			Difference (1) – (2)	
	N	Mean	StdDev	N	Mean	StdDev	Mean	P-value
IREvents	5,140	6.811	6.635	3,973	6.101	6.202	0.711***	(0.000)
Marketcap	5,140	8.537	1.589	3,973	8.566	1.597	-0.029	(0.382)
Mtb	5,140	2.065	1.140	3,973	2.071	1.143	-0.006	(0.806)
StockRet	5,140	0.145	0.363	3,973	0.141	0.362	0.004	(0.606)
RetVol	5,140	0.100	0.042	3,973	0.099	0.042	0.001	(0.492)
InstOwn	5,140	0.813	0.211	3,973	0.812	0.212	0.001	(0.779)
AnaCov	5,140	13.750	7.987	3,973	13.890	8.016	-0.143	(0.394)
R&D	5,140	0.027	0.045	3,973	0.027	0.044	0.000	(0.876)
Roa	5,140	0.080	0.071	3,973	0.080	0.071	-0.000	(0.879)
Lev	5,140	0.245	0.173	3,973	0.247	0.175	-0.003	(0.442)
BoardSize	5,140	9.729	1.989	3,973	9.760	1.991	-0.030	(0.468)
BoardIndep	5,140	0.785	0.176	3,973	0.779	0.189	0.006	(0.113)
CEOduality	5,140	0.471	0.500	3,973	0.464	0.500	0.007	(0.516)
DirectorOwn	5,140	0.045	0.092	3,973	0.045	0.092	0.000	(0.929)
DirectorBusyness	5,140	0.298	0.173	3,973	0.300	0.174	-0.002	(0.582)
DirectorCooption	5,140	0.473	0.307	3,973	0.472	0.306	0.001	(0.839)

Variables	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	
1.IREvents																
2.Marketcap		0.140														
3.Mtb		0.123	-0.025													
4.StockRet		0.040	0.003	0.260												
5.RetVol		-0.105	-0.143	-0.134	0.077											
6.InstOwn		0.005	-0.047	-0.012	0.005	0.005										
7.AnаЖov		0.374	0.211	0.167	-0.023	-0.182	-0.051									
8.R&D		0.236	-0.027	0.273	-0.001	0.169	0.008	0.040								
9.Roa		0.007	0.001	0.529	0.159	-0.238	-0.009	0.131	0.01							
10.Lev		0.056	0.106	-0.145	-0.074	-0.122	-0.052	0.070	-0.211	-0.194						
11.BoardSize		0.159	0.303	-0.098	-0.023	-0.313	-0.172	0.298	-0.162	0.015	0.214					
12.BoardIndep		0.064	0.155	-0.069	-0.006	-0.065	0.162	0.041	0.005	-0.014	0.009	0.112				
13.CEOduality		0.017	0.026	-0.018	0.01	-0.037	0.019	0.069	-0.068	0.021	-0.004	0.067	0.190			
14.DirectorOwn		-0.128	-0.161	0.020	0.015	0.116	-0.246	-0.139	-0.048	-0.012	-0.024	-0.078	-0.203	-0.015		
15.DirectorBusyness		0.156	0.369	-0.052	-0.032	-0.151	-0.033	0.231	0.039	-0.021	0.139	0.293	0.205	0.071	-0.205	
16.DirectorCooption		0.024	-0.061	0.049	0.031	0.083	-0.011	-0.030	0.057	0.001	-0.017	-0.127	0.015	0.282	0.073	-0.062

Table 2. The Effect of Directors on Firms' Investor Outreach

This table reports the results from estimating model (1) using the matched sample of treatment and control firms over the seven-year period surrounding proxy fights. The sample is based on S&P 1500 firms from 2004 to 2020. All variables are described in Appendix A. The *t*-statistics are presented in parentheses below the coefficients. Standard errors are clustered by firm. Two-tailed *p*-values are indicated: *** *p* < 0.01, ** *p* < 0.05, * *p* < 0.10.

Dependent variable =	OLS		Poisson	
	(1) IREvents	(2) IREvents	(3) IREvents	(4) IREvents
Treat × Post	0.758*** (3.05)	0.733*** (3.06)	0.115*** (3.53)	0.114*** (3.51)
Marketcap		1.043*** (5.15)		0.155*** (5.28)
Mtb		0.094 (0.64)		0.016 (0.92)
StockRet		0.139 (1.03)		0.021 (1.02)
RetVol		1.335 (0.43)		0.182 (0.36)
InstOwn		0.028 (0.06)		0.039 (0.67)
AnaCov		0.077*** (2.94)		0.006* (1.79)
R&D		1.029 (0.15)		0.844 (1.42)
Roa		-3.135** (-2.14)		-0.193 (-1.43)
Lev		1.461* (1.77)		0.113 (0.97)
BoardSize		0.062 (1.00)		0.011 (1.35)
BoardIndep		0.021 (0.03)		-0.038 (-0.48)
CEOduality		0.224 (1.07)		0.034 (1.33)
DirectorOwn		3.417** (2.02)		0.215 (1.00)
DirectorBusyness		-0.691 (-1.23)		-0.057 (-0.73)
DirectorCooption		-0.349 (-1.08)		-0.073* (-1.87)
N	9,113	9,113	9,113	9,113
Adjusted R ²	0.749	0.754		
Pseudo R ²			0.526	0.529
Entropy balanced	Yes	Yes	No	No
Cohort-Firm FE	Yes	Yes	Yes	Yes
Cohort-Year FE	Yes	Yes	Yes	Yes

Table 3. Dynamic Analysis of the Effect of Directors on Firms' Investor Outreach

This table reports the estimation results from a modified model (1) in which we replace *Post* with indicator variables *Year-2*, *Year-1*, *Year0*, *Year+1*, *Year+2*, and *Year+3*. All variables are described in Appendix A. The *t*-statistics are presented in parentheses below the coefficients. Standard errors are clustered by firm. Two-tailed *p*-values are indicated: *** *p* < 0.01, ** *p* < 0.05, * *p* < 0.10.

Dependent variable =	(1) IREvents	(2) IREvents
Treat × Year-2	0.450 (1.49)	0.426 (1.42)
Treat × Year-1	0.568 (1.43)	0.537 (1.37)
Treat × Year0	1.417*** (3.11)	1.410*** (3.17)
Treat × Year+1	1.287*** (3.06)	1.235*** (3.04)
Treat × Year+2	0.861** (2.02)	0.801** (1.96)
Treat × Year+3	0.890** (2.12)	0.804** (1.98)
N	9,113	9,113
Adjusted R ²	0.750	0.755
Controls	No	Yes
Cohort-Firm FE	Yes	Yes
Cohort-Year FE	Yes	Yes
Entropy balanced	Yes	Yes

Table 4. Heterogeneous Effects: Costs of Interlocking Directors' Proxy Fight Experiences

This table reports the results from estimating model (2) using the matched sample of treatment and control firms over the seven-year period surrounding proxy fights. In column (1), the number of observations reduces to 8,104 after excluding unresolved and pending proxy fights. In column (2), the number of observations reduces to 5,770, as the sample includes only treatment firms that are interlocked to target firms with a staggered board. The sample is based on S&P 1500 firms from 2004 to 2020. All variables are described in Appendix A. The *t*-statistics are presented in parentheses below the coefficients. Standard errors are clustered by firm. Two-tailed *p*-values are indicated: *** *p* < 0.01, ** *p* < 0.05, * *p* < 0.10.

Proxy =	Proxy fight outcomes	Up for election at <i>target</i> firm
TreatHigh	Dissident Victory or Settled	Up for election during proxy fights
TreatLow	Management Victory or Withdrawn	Not up for election during proxy fights
Dependent variable =	(1) IREvents	(2) IREvents
TreatHigh × Post	1.025*** (3.73)	1.728*** (2.98)
TreatLow × Post	0.244 (0.68)	0.748* (1.94)
N	8,104	5,770
Adjusted R ²	0.762	0.753
Controls	Yes	Yes
Cohort-Firm FE	Yes	Yes
Cohort-Year FE	Yes	Yes
Entropy balanced	Yes	Yes
Differences in interaction terms:		
TreatHigh × Post –		0.981
TreatLow × Post	0.781** (2.10)	(1.54)

Table 5. Heterogeneous Effects: Benefits of Increasing IR at Focal Firms

This table reports the results from estimating model (2) using the matched sample of treatment and control firms over the seven-year period surrounding proxy fights. Panel A reports cross-sectional analyses based on the investment horizon of institutional investors. In column (3), the number of observations reduces to 8,299 due to data availability for Bushee's classification of institutional investors (Bushee 1998). Panel B reports cross-sectional analyses based on the pre-existing frequency of IR events and anti-takeover provisions at the focal firm. The sample is based on S&P 1500 firms from 2004 to 2020. All variables are described in Appendix A. The *t*-statistics are presented in parentheses below the coefficients. Standard errors are clustered by firm. Two-tailed *p*-values are indicated: *** *p* < 0.01, ** *p* < 0.05, * *p* < 0.10.

Panel A: The proportion of long-term institutional investors			
Proxy =	Average holding period	Proportion of long-term investors (>= 5 years)	Proportion of dedicated investors & quasi-indexers
TreatHigh = 1	Above p50	Above p50	Above p50
TreatLow = 1	Below p50	Below p50	Below p50
Dependent variable =	(1) IREvents	(2) IREvents	(3) IREvents
TreatHigh × Post	1.298*** (4.35)	1.157*** (3.79)	1.087*** (3.70)
TreatLow × Post	0.279 (1.06)	0.415 (1.59)	0.469 (1.61)
N	9,113	9,113	8,299
Adjusted R ²	0.755	0.755	0.768
Controls	Yes	Yes	Yes
Cohort-Firm FE	Yes	Yes	Yes
Cohort-Year FE	Yes	Yes	Yes
Entropy balanced	Yes	Yes	Yes
Differences in interaction terms:			
TreatHigh × Post -			
TreatLow × Post	1.019*** (3.48)	0.742** (2.48)	0.619* (1.96)

Panel B: Vulnerability to potential shareholder activism		
Proxy =	Pre-existing frequency of IR events	Anti-takeover measures (Eindex)
TreatHigh	Below p50	Below Median
TreatLow	Above p50	Above Median
	(1) IREvents	(2) IREvents
Dependent variable =		
TreatHigh × Post	1.025*** (3.73)	0.977*** (3.57)
TreatLow × Post	0.244 (0.68)	0.374 (1.34)
N	9,113	9,113
Adjusted R ²	0.755	0.755
Controls	Yes	Yes
Cohort-Firm FE	Yes	Yes
Cohort-Year FE	Yes	Yes
Entropy balanced	Yes	Yes
Differences in interaction terms:		
TreatHigh × Post –		0.603**
TreatLow × Post	0.734** (2.27)	(2.16)

Table 6. Ruling Out Alternative Explanations: Placebo Analyses

This table reports the results from estimating model (1) using placebo-treatment samples. The sample is based on S&P 1500 firms from 2004 to 2020. All variables are described in Appendix A. The *t*-statistics are presented in parentheses below the coefficients. Two-tailed *p*-values are indicated: *** *p* < 0.01, ** *p* < 0.05, * *p* < 0.10.

Panel A. Placebo treatment: Firms interlocked with target firms <i>one year prior to, but not during, the year of the proxy fight</i>				
Dependent variable =	OLS		Poisson	
	(1) IREvents	(2) IREvents	(3) IREvents	(4) IREvents
Treat × Post	0.101 (0.14)	-0.091 (-0.14)	-0.077 (-1.07)	-0.090 (-1.25)
N	1,826	1,826	1,826	1,826
Adjusted R ²	0.726	0.735		
Pseudo R ²			0.495	0.502
Entropy balanced	Yes	Yes	No	No
Cohort-Firm FE	Yes	Yes	Yes	Yes
Cohort-Year FE	Yes	Yes	Yes	Yes
Panel B. Placebo treatment: Firms interlocked with target firms <i>two years prior to, but not during, the year of the proxy fight</i>				
Dependent variable =	OLS		Poisson	
	(1) IREvents	(2) IREvents	(3) IREvents	(4) IREvents
Treat × Post	-0.411 (-1.16)	-0.340 (-1.00)	-0.058 (-1.18)	-0.042 (-0.84)
N	3,301	3,301	3,301	3,301
Adjusted R ²	0.773	0.781		
Pseudo R ²			0.508	0.513
Entropy balanced	Yes	Yes	No	No
Cohort-Firm FE	Yes	Yes	Yes	Yes
Cohort-Year FE	Yes	Yes	Yes	Yes

Table 7. Ruling Out Alternative Explanations: Predetermined Director Election Years

This table reports the results from estimating model (3) using treatment firms in the post-period following proxy fights. Up-for-election at *focal firms* is an indicator equal to one in years when interlocking directors are scheduled for reelection at focal firms and zero otherwise. The sample is based on S&P 1500 firms from 2004 to 2020. The *t*-statistics are presented in parentheses below the coefficients. Standard errors are clustered by firm. Two-tailed *p*-values are indicated: *** *p* < 0.01, ** *p* < 0.05, * *p* < 0.10.

	All treatment firms	Treatment firms with staggered boards
	(1) IREvents	(2) IREvents
Dependent variable =		
Up-for-election at <i>focal firms</i>	0.309* (1.68)	0.377** (2.09)
Marketcap	1.124*** (3.12)	0.008 (0.01)
Mtb	-0.057 (-0.24)	0.042 (0.08)
StockRet	0.060 (0.25)	-0.020 (-0.06)
RetVol	-5.674 (-0.90)	6.564 (0.58)
InstOwn	-0.366 (-0.60)	0.488 (0.37)
AnaCov	0.039 (0.97)	0.007 (0.10)
R&D	8.227 (0.65)	3.560 (0.23)
Roa	1.020 (0.49)	3.317 (1.36)
Lev	1.139 (0.85)	0.051 (0.03)
BoardSize	0.116 (1.30)	0.029 (0.19)
BoardIndep	1.123 (0.93)	-0.269 (-0.20)
CEOduality	0.085 (0.25)	0.219 (0.48)
DirectorOwn	-2.098 (-1.08)	-3.261 (-0.85)
DirectorBusyness	-0.543 (-0.55)	2.710* (1.85)
DirectorCooption	0.138 (0.27)	-2.090** (-2.44)
N	3,082	1,086
Adjusted R ²	0.705	0.664
Year fixed effects	Yes	Yes
Firm fixed effects	Yes	Yes

Table 8. Robustness Tests: Alternative Construction of Treatment and Control Groups

This table reports the results from estimating model (1) using alternative methods of constructing the treatment and control groups. In columns (1) and (2), we include only the first treatment in cases of overlapping treatment windows. In columns (3) and (4), we eliminate all cases where a firm that was treated earlier serves as a matched control firm for another firm treated later in the sample. The sample is based on S&P 1500 firms from 2004 to 2020. All variables are described in Appendix A. The *t*-statistics are presented in parentheses below the coefficients. Two-tailed *p*-values are indicated: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Dependent variable =	Include first treatment event only		Exclude already-treated firms from control	
	(1) IREvents	(2) IREvents	(3) IREvents	(4) IREvents
Treat \times Post	0.849*** (3.40)	0.831*** (3.37)	0.806*** (2.97)	0.811*** (3.10)
Marketcap		0.639*** (2.93)		1.029*** (4.88)
Mtb		0.222 (1.40)		0.107 (0.68)
StockRet		-0.122 (-0.71)		0.136 (0.97)
RetVol		-0.881 (-0.24)		0.884 (0.27)
InstOwn		-0.210 (-0.52)		-0.055 (-0.12)
AnaCov		0.082*** (2.86)		0.077*** (2.86)
R&D		1.141 (0.16)		1.505 (0.22)
Roa		-2.042 (-1.33)		-3.281** (-2.20)
Lev		1.296 (1.49)		1.493* (1.72)
BoardSize		0.009 (0.13)		0.044 (0.69)
BoardIndep		0.501 (0.66)		-0.192 (-0.31)
CEOduality		0.136 (0.83)		0.311 (1.46)
DirectorOwn		3.261** (2.03)		3.683** (2.01)
DirectorBusyness		-0.071 (-0.12)		-0.392 (-0.67)
DirectorCooption		-0.446 (-1.36)		-0.390 (-1.13)
N	6,701	6,701	8,642	8,642
Adjusted R ²	0.724	0.727	0.750	0.755
Controls	Yes	Yes	Yes	Yes
Cohort-Firm FE	Yes	Yes	Yes	Yes
Cohort-Year FE	Yes	Yes	Yes	Yes
Entropy balanced	Yes	Yes	Yes	Yes

Table 9. Purpose of Increased IR Activities: Likelihood of Proxy Fights at Focal Firms

This table reports the results from estimating model (4) using treatment events from the main sample. Δ Proxy Fights is the change in the average incidence of proxy fights at focal firms from the pre- to post-periods. Δ IREvents and all control variables are similarly constructed as change variables. The sample is based on S&P 1500 firms from 2004 to 2020. All variables are described in Appendix A. The *t*-statistics are presented in parentheses below the coefficients. Standard errors are clustered as indicated. Two-tailed *p*-values are indicated: *** *p* < 0.01, ** *p* < 0.05, * *p* < 0.10.

	(1)	(2)
Dependent variable =	Δ Proxy Fights	
Δ IREvents	-0.001** (-2.27)	-0.001*** (-3.22)
Δ Marketcap	-0.007 (-1.54)	-0.007 (-1.55)
Δ Mtb	-0.006 (-1.47)	-0.006* (-2.05)
Δ StockRet	-0.000 (-0.04)	-0.000 (-0.05)
Δ RetVol	-0.021 (-0.29)	-0.021 (-0.26)
Δ InstOwn	0.033** (2.10)	0.033** (2.61)
Δ AnaCov	-0.000 (-0.04)	-0.000 (-0.04)
Δ R&D	0.219 (1.14)	0.219 (1.26)
Δ Roa	-0.035 (-0.64)	-0.035 (-1.00)
Δ Lev	-0.019 (-1.15)	-0.019 (-0.75)
Δ BoardSize	0.005** (2.23)	0.005*** (3.05)
Δ BoardIndep	-0.012 (-0.67)	-0.012 (-0.68)
Δ CEOduality	0.003 (0.68)	0.003 (0.43)
Δ DirectorOwn	0.134** (2.34)	0.134** (2.74)
Δ DirectorBusyness	-0.019 (-0.87)	-0.019 (-1.00)
Δ DirectorCooption	-0.005 (-0.69)	-0.005 (-0.61)
N	1,144	1,144
Adjusted R ²	0.057	0.057
Year fixed effects	Yes	Yes
Industry fixed effects	Yes	Yes
Clustering	Industry	Year

Table 10. Purpose of Increased IR Activities: Informativeness of IR Events by Focal Firms
 This table reports the average absolute three-day cumulative stock returns ($[-1, +1]$) around investor interaction events attended by focal firms ($AbsCAR$) during the pre-period and post-period (in percentage). The sample is based on S&P 1500 firms from 2004 to 2020. The t -statistics are presented in parentheses below the coefficients. Two-tailed p -values are indicated: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

	Average $AbsCAR$	N
Pre	2.000*** (87.74)	9,407
Post	2.026*** (124.27)	19,714
Differences	0.026 (0.92)	