Firm Disclosure Response to the Threat of Takeover: Testing the Corporate Control Contest Hypothesis

Shuping Chen*
Professor
University of Texas at Austin
shuping.chen@mccombs.utexas.edu

Kristen Valentine
PhD Student
University of Texas at Austin
kristen.valentine@phd.mccombs.utexas.edu

Bin Miao
Assistant Professor
Hong Kong Polytech University
bin.miao@polyu.edu.hk

ABSTRACT

We test the corporate control contest hypothesis which predicts that managers use voluntary disclosure to reduce the likelihood of job loss when faced with control threats. We exploit the exogenous control shock experienced by firms upon the hostile takeover announcements of rival firms. This identification is based on existing research findings that a merger announcement in the industry increases the likelihood of peer firms becoming targets themselves and that corporate control contests increase the probability of management turnover. We employ a difference-in-difference design to test peer disclosure reaction relative to control firms. We find that, after the control shock, peer managers resort to more transparent disclosure as evidenced by managerial conference call language that is easier to understand and more transparent. Peer managers also provide more management guidance, more bad news guidance, and issue more 8-K filings after the shock. Our study contributes to the voluntary disclosure literature by providing much needed evidence supporting the corporate control contest hypothesis.

Preliminary. Please do not quote or circulate. Comments welcome.

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1. Introduction

Corporate control contests have long been hypothesized as a motivating factor behind managers' voluntary disclosure decisions (Healy and Palepu 2001; Graham, Harvey, and Rajgopal 2005). Specifically, the corporate control contest hypothesis postulates that managers employ voluntary disclosure to mitigate the possibility of a job loss when faced with control threats. Despite both the intuitive appeal of this hypothesis and a voluminous voluntary disclosure literature, there has been surprisingly very little empirical research devoted to this. Healy and Palepu (2001) lament: "There has been relatively little research on voluntary disclosure accompanying hostile takeovers or for target firms engaged in proxy contests." This paucity of research persists decades after Healy and Palepu (2001) first identify this issue. For example, Beyer, Cohen, Lys, and Walther (2010) acknowledge that "our understanding of how management's career concerns affect their disclosure strategies is still limited." One notable exception is Brennan (1999), who, using U.K. data, documents that targets of contested takeover bids are more likely to make profit forecasts than targets of friendly bids.

We fill the void in this literature and offer a direct test of the corporate control contest hypothesis by studying the disclosure reaction of peer firms to the hostile takeover announcements of a rival firm in the same industry.[1] Examining peer firms, who experience an exogenous shock in corporate control but are not in negotiation with acquirers, mitigates the endogeneity concern inherent with employing takeover targets to test this hypothesis, because other factors, such as targets' desire to negotiate a better takeover premium, could be driving the disclosure change. Our identification of this

[1] We use "rival firms" or "target firms" to designate hostile takeover targets and "peer firms" to reference peers in the same industry as hostile takeover targets.

control shock experienced by peer firms builds upon prior research findings that 1) a takeover announcement in the industry significantly increases the probability of peer firms themselves becoming takeover targets (Song and Walking 2000), and that 2) corporate control contests increase target management turnover (Martin and McConnell 1991; Krug and Shill 2008).[2]

We predict that peers of hostile takeover targets have incentives to improve transparency via voluntary disclosure so as to mitigate the possibility of becoming takeover targets themselves, which in turn mitigates the possibility of job loss. Hostile takeover targets are likely empire builders with over-investment and free cash flow problems (e.g., Martin and McConnell 1991). Recent accounting research suggests that transparent disclosure can discipline managers and mitigate empire building (Hope and Thomas 2008; Leuz and Wysocki 2016; Bens and Monahan 2004; Goodman, Neamtiu, Shroff, and White 2014). More transparent voluntary disclosure allows peer firm managers to expediently signal that they will reduce empire building behavior, making their firm a less desirable future takeover target.[3] Our prediction is in line with the recent finding in Servaes and Tamayo (2014) that peer firms of hostile takeover targets resort to operational changes such as reducing capital spending and increasing leverage to reduce agency costs and diminish control threat.

To examine whether managers provide more transparent voluntary disclosure as a reaction to control threats, we employ a difference-in-difference research design and compare the voluntary disclosure behavior of "shocked" firms to control firms that do not experience such a shock. Our

[2] Another potential setting to test the corporate control contest hypothesis is the passage of anti-takeover state laws in the 1980s and 1990s – these laws effectively insulate managers from hostile takeover attempts (Bertrand and Mullainathan 2003). As a result managers should have fewer concerns for job security. We note that the implication of *increased* job security for managerial disclosure behavior is ambiguous *ex ante*. The "quiet life" view would indicate that increased job security leads managers to be less transparent, resulting in fewer management forecasts. However, it is well documented that managers are reluctant to cut earnings guidance they are already providing, as they face other costs if they lax in their reporting and disclosure (etc. Chen, Matsumoto, Rajgopal 2011). Consistent with this, Armstrong, Balakrishnan, and Cohen (2012) find that managerial career concerns have no effect on firms' financial statement informativeness after the passage of these laws. We also note that these law passages predate the availability of the data we employ to perform our tests.

[3] This argument requires that an increase in voluntary disclosure is viewed as a credible commitment to continued transparency, which is reasonable given that empirically voluntary disclosure choices are persistent.

treatment sample consists of industry peers of target firms. We further restrict these peer firms to have three-day announcement cumulative abnormal returns (CARs) to be greater than one percent. This latter restriction allows us to better identify firms that experience the biggest control shock, and is consistent with Song and Walking's (2000) finding that peer announcement period abnormal returns are systematically related to variables associated with the probability of acquisition. We generate our control sample by randomly selecting 100 firms for each takeover event from the non-peer and non-target pool. Thus, our control firms consist of firms that are least likely to experience a control shock and are representative of the population of firms through random selection.

We capture managers' voluntary disclosure behavior, specifically the transparency of voluntary disclosure, using two approaches. Our first approach exploits the linguistic features of managers' conference call language that proxy for transparency. We use two measures: the "readability" of managers' conference call language, and the obfuscation component of managers' conference call language following the procedure advanced in Bushee, Gow, and Taylor (2016). Our second approach captures the quantity of voluntary disclosure, proxied by the frequencies of 8-Ks and management forecasts. Evidence of a lower Fog index, lower obfuscation in conference call language, and a greater number of 8-Ks and management forecasts is consistent with the prediction arising from the corporate control contest hypothesis: managers respond to control shock with increased transparency in voluntary disclosure.

We identify 117 hostile takeover events from 1997-2014. Our sample stops in 2014 because we need two years of post-event data for our analyses. We match each hostile takeover target firm each year with industry peers using the *text-based network industry classifications* (TNIC3) advanced in Hoberg and Phillips (2010; 2016), and retain the peer firms with three-day announcement CARs greater than one percent as our treatment sample. For each target-year we randomly select one hundred firms from the

pool of firms that are neither targets nor peers of targets as our control firms. Our difference-in-difference analyses reveal that, *ceteris paribus*, managers of treatment firms use language that is easier to comprehend and with less obfuscation after the shock. Treatment firms also provide a greater number of 8-Ks and management forecasts relative to control firms post shock. These results are consistent with the prediction of the corporate control contest hypothesis.

We further supplement the above analyses using an alternative treatment sample and an alternative control sample. Our alternative treatment sample relies on the notion that more entrenched managers face lower career concerns even in the face of control threat. We use a parsimonious measure to capture managerial entrenchment – CEO duality, where CEOs are also chairmen of the board of directors. Our alternative treatment sample is TNIC3 peer firms where CEOs are not chairmen of the board, and we find a similar increase in disclosure transparency for all disclosure proxies using this sample. Furthermore, we show that in the subsample where CEOs are also chairmen, there is no significant change in management's conference call language attributes or the frequency of 8-K issuance post shock. Secondly, we rerun our main analysis using an alternative control sample, namely firms in the same two-digit SIC code as peer firms, following Beatty, Liao, and Yu (2013). This alternative control sample might be more similar to our treatment firms in underlying firm fundamentals than a random sample of firms, but may still be subject to some control threat, though the control threat is not as intense as that for treatment firms. Thus, while neither control group is perfect, consistent results across both sets of control firms bolsters our interpretation. The results using this alternative control sample are also robust.

Our study contributes to the voluntary disclosure literature by offering much-needed empirical evidence on a long-standing hypothesis of voluntary disclosure – the corporate control contest hypothesis. To our knowledge, Brennan (1999) is the only paper that provides some evidence in the

corporate control contest setting. We differ from Brennan (1999) along several dimensions. First, we use peer firms of targets to identify an exogenous control shock while Brennan (1999) restricts her analysis to takeover targets without a control sample. Our research design mitigates endogeneity concern inherent with relying on the disclosure behavior of takeover targets, as targets' behavior might be driven by factors other than managers' career concerns, such as the incentive to negotiate better terms with the acquirers. Brennan's (1999) finding that target managers in contested bids provide more good news forecasts, and subsequently experience higher takeover bids, is consistent with this alternative explanation. Second, while Brennan (1999) examines only one dimension of voluntary disclosure – management earnings forecasts, our research examines a wider spectrum of disclosure choices managers can make, from how they conduct conference calls to the quantity of 8-Ks and management forecasts. Lastly, while the key takeaway from Brennan (1999) is that hostile takeover targets are more likely to make profit forecasts than friendly takeover targets, we find that managers of our treatment firms make more bad news management forecasts than control firms not subject to the control shock. To the extent bad news forecasts are more credible and hence more transparent, our results are more consistent with the corporate control contest hypothesis.

We also contribute to a growing literature documenting a "peer effect" in firms' behavior – managers make economic and accounting decisions not in isolation of, but in light of, their rival firms' economics and accounting practices. The vast majority of this research focuses on firms' operational response to their peers' operational decisions and economic shocks (Leary and Roberts 2014; Kaustia and Rantala 2015; Servaes and Tamayo 2016), and relatively fewer papers examine firms' accounting and disclosure responses to rival firms' economic or accounting shocks (Durnev and Mangen 2009; Bourveau and Schoenfeld 2016). Though we primarily employ the peer firm setting to identify an

exogenous shock to the risk of takeover, our results add to this second stream of literature by offering new evidence on peer firms' accounting response to the attempted hostile takeover of rival firms.

The rest of the paper is organized as follows. Section 2 offers a literature review and presents our prediction. Section 3 discusses sample and research design, while Section 4 presents the empirical results. Section 5 concludes.

2. Literature Review and Hypotheses Development

The corporate control contest hypothesis is first formally articulated in Healy and Palepu (2001). This hypothesis is motivated by early evidence in finance that boards of directors and investors hold managers accountable for current stock performance and can fire managers based on poor performance (Warner et al. 1988; Weisbach 1988). This, coupled with the evidence that poor stock performance is associated with hostile takeovers (Morck, Shleifer, and Vishny 1990), leads Healy and Palepu (2001) to thus state: "Voluntary disclosure theory hypothesizes that, given the risk of job loss accompanying poor stock and earnings performance, managers use corporate disclosures to reduce the likelihood of undervaluation and to explain away poor performance." They further argue that firms will offer "more expansive disclosure" to mitigate a control threat.

Reasoning along this line, the prospect of a job loss can motivate managers to change their disclosure behavior, whether the threat of a job loss comes from dismissal by the Board of Directors or from the threat of a takeover. The finance literature has long established that takeovers are associated with high turnover for target managers. For example, Martin and McConnell (1991) document a target manager turnover rate of over 60% in the two years following the completion of a takeover. Based on an analysis of the career tracks of more than 23,000 executives involved in 5,000 target companies spanning 17 years, Krug and Shill (2008) confirm the high target executive turnover rate persists

throughout the 1990s and 2000s and is much higher than the turnover rate among non-merged firms. Thus, it is plausible that managers can perceive potential takeovers to pose a significant threat to their job security.

While the corporate control contest hypothesis, as originally stated by Healy and Palepu (2001), does not offer predictions on managers' specific disclosure choices, we believe in our setting peer managers' overarching disclosure objective is to reduce market's perception of over-investment by providing more transparent disclosure. This prediction is based upon the following three strands of related research findings: first, hostile takeover targets are likely "empire builders" with over-investment and free cash flow problems (e.g, Jensen 1986,1993; Martin and McConnell 1991). Second, peer firms of acquisition targets earn abnormal returns upon takeover announcements because of the increased probability that they will become targets themselves (Song and Walking 2000). This, coupled with the finding that the stock price reaction upon announcement of a rival's takeover is more positive and larger for peer firms with higher capital spending and higher free cash flows (Servaes and Tamayo 2014), suggests that the market perceives peer firms with greater free cash flow problems to be more attractive future takeover targets. Third, accounting research has demonstrated that transparent disclosure can serve as a mechanism to monitor managers and constrain managers' empire-building behavior. For example, Hope and Thomas (2008) document that a decrease in transparent disclosure leads multi-national firms to sub-optimally expand foreign sales that produce lower profit margins and lower firm value. Bens and Monahan (2004) show that analyst ratings of voluntary disclosure are positively associated with the portion of firm value attributable to diversification and interpret this as evidence that voluntary disclosure helps to discipline management's investment decisions. Goodman et al. (2014) find that management forecast accuracy is positively associated with efficient investments in capital

expenditures, where investment efficiency is measured using deviations from a model of expected investment. This line of reasoning leads to our formal prediction, stated in alternative form, as follows:

P1: Firms experiencing an exogenous control shock provide more transparent voluntary disclosure than firms not experiencing a control shock.

Healy and Palepu (2001) point out that the corporate control contest hypothesis has one potential drawback – the single period nature of the prediction. They state: "One limitation is that this analysis does not take account of multi-period considerations. For example, if managers expect that a commitment to provide extensive disclosure today could be used to hold them more accountable for any subsequent poor performance, managers of firms subject to corporate control actions many not wish to expand disclosure in a period of poor performance." Thus, the above prediction may not hold if managers' concerns about future accountability for expanded disclosure outweigh their job security concerns.

We note, however, that our prediction is in line with recent research findings that peer firms reassess their own probability of becoming the next takeover targets and take actions to mitigate such a possibility. Song and Walking (2000) state: "(T)he appearance of an unexpected acquisition attempt within an industry generates shock waves that cause firm-specific reassessment of the probability of an acquisition attempt for rivals" (P.144). Second, Servaes and Tamayo (2014) document that peer firms of hostile takeover targets respond by cutting capital spending, free cash flows, and cash holdings, and increasing their leverage and payouts to shareholders. In addition, Healy and Palepu's (2001) conjecture relates more to firms with poor performance than peer firms of takeover targets, who experience an exogenous control shock but may not be in distress. Thus, the original Healy and Palepu (2001) concern about *ex post* settling up with expansive disclosure is likely diminished in our setting.

As we discuss in the introduction, research on the corporate control contest hypothesis is sorely lacking. An early paper by Brennen (1999) contrasts 224 contested bids (which include both hostile bids

and competing bids) with 477 friendly takeovers for companies listed in London Stock Exchange from 1988 to 1992. She finds that targets of contested bids offer more profit forecasts than targets of friendly bids, and that forecasts in hostile bids are associated with an increase in offer price but not associated with whether the bid was eventually successful. Taken together, her findings are more consistent with hostile takeover targets using good news disclosure to obtain higher premiums rather than to fend off takeover attempt. The peer firm setting we examine mitigates the endogeneity concern in Brennen's setting: while peer firms face an increased probability of becoming a hostile takeover target themselves and an increased probability of managerial turnover, there is no negotiation between peer firms and acquirer firms. As such, the disclosure behavior of peer firms can be more cleanly attributed to peer managers' incentives to mitigate control threat.

3. Sample, Research Design, and Measurement of Variables

We start by identifying hostile takeover targets from the SDC database from 1997 to 2014. Our sample stops in 2014 because we need two years of data after 2014 to perform our empirical analyses. We are able to identify a total of 117 hostile takeover announcements over our sampling years.[4] We tabulate the frequencies of hostile takeover announcements by year in column (1) of Table I.

Our research design calls for the construction of two samples – a treatment sample consisting of peer firms of hostile takeover targets, and a control sample of firms that are not themselves takeover targets nor peer firms of takeover targets. We match each hostile takeover target firm each year with industry peers using the *text-based network industry classifications* (TNIC3) advanced by Hoberg and

[4] The frequency of our hostile takeover announcements is higher each year than that reported in Servaes and Tamayo (2014) for the years where our samples overlap. This is because Servaes and Tamayo (2014) impose multiple data screens and drop some deals from their sample (see Page 383). For example, they drop bids for financial firms because of the difficulty involved in measuring investments in the financial sector. We do not impose such data restrictions because our research question is different.

Phillips (2016). In contrast to traditional static industry classifications such as SIC and NAIC where firms are grouped together using fixed product market definitions and industry membership is constrained to be transitive, TNIC treats industries as time-varying intransitive networks and allows each firm to have its own time-varying set of rivals. Following Hoberg and Phillips (2010; 2016), we classify firms in the same TNIC3 code of target firms as peer firms. We further restrict our treatment sample to be peer firms with three-day announcement CAR greater than one percent. This allows us to better identify a treatment sample that experiences the biggest control shock, as Song and Walking (2001) find that announcement abnormal returns are positively associated with variables capturing the probability of acquisition. Column (2) of Table I tabulates the total number of unique treatment firms for each year, and column (3) reports the average number of treatment firms matched for each target each year.

Our ideal control sample should consist of firms not subject to the control shock but are similar in economics to the treatment firms. Thus, we start with firms that are not in the same TNIC3 code and are not themselves acquisition targets. Since firms that are competitors are more likely to be similar in their underlying economics than firms that are not competitors, removing the TNIC3 firms and target firms from the control sample pool poses a challenge in identifying firms with similar economics. We thus resort to randomly selecting one hundred firms from the control sample pool for each takeover event. This comparison group is least likely to be subject to a takeover threat, but may be more dissimilar to peer firms in terms of firm fundamentals. As such we also offer triangulating evidence by using firms that are in the same two-digit SIC code but not TNIC3 or three-digit SIC code as peer firms and are not themselves takeover targets as an alternative control sample (reported in Panel B of Table IV later). This approach follows the approach employed in Beatty, Liao and Yu (2013). This alternative control sample is not subject to the same intensity of takeover pressure and we assume they are more similar to treatment firms in economics than a random sampling from the population. While neither

control group is perfect, consistent results across both sets of control firms bolsters our interpretation. Columns (4) and (5) of Table I tabulate the number of unique control firms per year and the number of unique control firms per event, which is one hundred by definition in our main control sample.

We employ a difference-in-difference (hereafter DID) design to test our prediction. Specifically, we estimate the following regression:

$$Disclosure\ Proxies_{i,t} = \beta_0 + \beta_1 Treat_{i,t} + \beta_2 Post_{i,t} + \beta_3 (Treat_{i,t} * Post_{i,t}) + Controls_{i,t} + e_{i,t}$$

Where *Disclosure Proxies* are different measures intended to capture transparent voluntary disclosure (detailed below), *Treat* is an indicator variable defined as one for treatment firms and zero for control firms, and *Post* is an indicator variable coded as one for up to two years following a hostile takeover announcement and coded as zero for up to two years preceding the announcement. Our emphasis is on the coefficient β_3 for the interaction variable ($Treat_{i,t} * Post_{i,t}$). A significantly positive (negative) coefficient would indicate greater increase (decrease) in certain voluntary disclosure features for peer firms relative to control firms after a target firm's hostile takeover announcement.

We employ two sets of proxies to capture transparent voluntary disclosure. Our first set of proxies is based on the textual attributes of the language managers employ in quarterly earnings conference calls. We assume that managerial language that is easier to comprehend and is low in obfuscation is more transparent. Thus, our first proxy is the Gunning (1952) Fog index of managerial language over both the presentation section and the Q&A section of earnings calls. We obtain conference call transcripts from the StreetEvent database. Following Bushee, Gow, and Taylor (2016), we construct the Fog index using the *Lingua:: EN: Sentence* Perl routine that identifies sentence breaks. Lower values of the Fog index correspond to language that is easier to understand. We separately consider the Fog index for 1) the presentation section of the conference call (FOG_PR), 2)

management's responses in the Q&A portion of the call (FOG_QA), and 3) the average Fog index for both portions of the call (FOG).

Our second proxy based on textual attributes captures the extent of obfuscation embedded in managers' conference call language following the approach advanced in Bushee et al. (2016). Specifically, Bushee et al. (2016) separate managers' linguistic complexity (hereafter LC, measured using the Fog index) into an information component and an obfuscation component. They estimate these two latent constructs of linguistic complexity by using analysts' LC during conference calls' Q&A as a measure of the information component, and run the following model using quarterly earnings conference call data:

$$LC\_Manager = b_0 + b_1\, LC\_Analyst + \Sigma\, b_k\, Business\ Complexity\ Variables + \eta$$

Where *LC_Manager* is managers' linguistic complexity measured as the Fog Index over managers' remarks over both the presentation and Q&A session, and *LC_Analyst* is analysts' linguistic complexity measured as Fog Index over analysts' remarks during the Q&A session. The fitted values from the above regression are measures of the information component and the residual values from the above regression stand for the obfuscation component. Following the Bushee et al. (2016) procedure, we first estimate the above regression over all earnings conference call transcripts from 2001-2015, and obtain the obfuscation score as residuals for our sample firms. We separately consider the Obfuscation score for 1) the presentation section of the conference call (OBFU_PR), 2) management's remarks in the Q&A portion of the call (OBFU_QA), and 3) the average Obfuscation score for both portions of the call (OBFU). A lower obfuscation score indicates higher transparency. Detailed definition of the Business Complexity Variables included in the above regression are presented in Appendix A.

Our second set of disclosure proxies rely on the quantity of voluntary disclosure. These proxies include: the number of management forecasts (MF) – following Guay, Samuels, and Taylor (2017) we

include all types of forecasts and not just earnings forecasts, the number of bad news management forecasts (BNMF), the number of good news management forecasts (GNMF), and the number of 8-Ks issued (N8K). Higher frequencies of the above proxies are generally accepted as measures of more transparent disclosure (e.g., Kasznik 1999; Chen, Chen, Cheng 2008; Boone and White 2015).

We follow Guay et al. (2017) and include the following control variables that are intended to capture business complexity in our difference-in-difference regression: SIZE, ROA, LOSS, MTB, LEV, SPI, RET, RET_STD, NAF. Detailed definitions of all these variables are presented in Appendix A. We also include year and quarter fixed effects in our regressions. All standard errors are clustered by firm.

4. Empirical Results

4.1 Descriptive statistics

We present the pre- and post-shock descriptive statistics on our voluntary disclosure proxies in Table II. Panel A presents the results on the textual proxies measured using quarterly intervals (four quarters before and after), and Panels B and C present the results on the quantity proxies using both annual (two years before and after) and quarterly intervals (four quarters before and after).

At a univariate level, the average Fog index (FOG) and obfuscation component (OBFU) both decrease for the treatment firms after the control shock, though the decreases are not statistically significant. For the control sample, both textual measures show an increasing trend though again the increases are not statistically significant. When broken out into the presentation and Q&A sections of the conference calls, the FOG and OBFU measures only have statistically significant differences for the FOG_PR score in the control group. The magnitude of our FOG index measures are comparable to those reported in prior literature (Bushee et al. 2016; Allee and DeAngelis 2014). Taken literally, the Fog index is intended to capture the hypothetical years of formal education needed to comprehend the

materials. For example, a Fog index of 16 indicates college education while a Fog index of 12 indicates formal education level of a high school senior. It is not surprising that the Fog index of the presentation section is higher than that for the Q&A section, as the presentation section is usually carefully scripted beforehand.

For the disclosure quantity measures, following Servaes and Tamayo (2014) we employ annual measurement intervals – two years before and two years after (Panel B). If there is any change in the number of management forecasts and 8-K filings, these changes will take a longer time to manifest themselves than changes to the language used in quarterly conference calls. For comparison purposes with Panel A, we also present the results based on four-quarters before and after the shock in Panel C.

Panel B shows that for the treatment firms both quantity measures increase post shock: management forecasts increase from 3.9 in the pre-period to 6.4 in the post period, whereas the number of 8-Ks issued increases from 6.1 in the pre-period to 8.8 in the post period. Both increases are statistically significant. For the control firms, the quantities also increase, however the increases are not as large as those for control firms. Panel C shows a similar pattern in the increases of total number of management forecasts and bad news forecasts for both the treatment and control groups, though the number of good news forecasts decrease for both groups using the short time horizon and the number of 8-Ks issued exhibit no difference before and after. This later insignificant result in 8-Ks is not surprising as any change in 8-K issuance will more likely exhibit in longer measurement intervals.

4.2 Difference-in-difference regression results

We present our DID regression results in Tables III and IV.

Table III tabulates the results on the textual disclosure attributes. We control for firm characteristics likely associated the underlying business complexity in our regressions. Before the

control shock, treatment firms on average exhibit higher levels of complex and obfuscatory tone in management's conference call language, as shown by the positive coefficient on *TREAT* in Table III. The coefficient on *POST* is not significant, thus there is no evidence of change in textual attributes for the control firms around the hostile takeover announcement. In the POST period treatment firms experience greater decreases in both the average Fog index (FOG) and obfuscation score (OBFU) in the POST period relative to the control firms. This suggests that treatment firm managers use language that is easier to understand and less infused with obfuscation in earnings conference calls after the control shock, consistent with our empirical prediction derived from the corporate control contest hypothesis. This same pattern of results persists when we further break out the Fog index and obfuscation score into their component parts from the presentation and Q&A section respectively.

Table IV presents the results using the disclosure quantity measures. Panel A of Table IV presents the results on annual intervals (two years before and after) while Panel B presents the results using quarterly intervals (four quarters before and after).

Panel A shows that treatment firms in the pre-period issue fewer bad news management forecasts but more 8-Ks than control firms while the total number of management forecasts and good news forecasts are not different between the two groups. In the POST period, treatment firms issue a greater number of total management forecasts and bad news forecasts than control firms, though the frequency of good news forecasts are not different between the two groups. To the extent bad news forecasts are more transparent (Chen et al. 2008), this set of result is consistent with our prediction derived from the corporate control contest hypothesis. Column (4) of Panel A further shows that treatment firms issue a greater number of 8-Ks than control firms in the POST period.

Table IV Panel B runs the same specification as in Table IV Panel A, with the observations measured at quarterly intervals to be comparable to the analyses on textual attributes presented in Table III. The results are similar to those in Panel B: the coefficients on *TREAT x POST* are significantly positive for the total number of management forecasts, bad news forecasts, and the number of 8-Ks issued. Taken together, the results based on the frequency of voluntary disclosure measures are consistent with the results based on the textual attributes of management conference call language: managers of treatment firms appear to resort to more transparent disclosure after the control shock relative to managers of control firms.

4.3 Robustness tests

Our main tests presented in Tables III and IV employ a sample of treatment firms that are in the same industry as targets of hostile takeovers and that have a three-day CAR around the takeover announcement greater than 1%. This latter requirement is designed to identify firms with the greatest risk of a change in control. In our first robustness test, we employ an alternative way to identify treatment managers at greatest risk of turnover in the event of a change in control – managers who are less entrenched. Thus, our alternative treatment sample consists of firms in the same industry as a takeover target and whose CEO is not the chairman of the board. CEOs who are also chairmen of the board are more entrenched and are subject to lower likelihood of turnover.

We present our results using this alternative proxy for management turnover risk in Panel A of Table V, and for parsimony we tabulate only the coefficients on the interaction variable *Treat * Post*. The results are qualitatively similar to those obtained in Tables III and IV: management conference call language is more transparent (lower Fog and lower obfuscation score) and treatment firms also issue a greater number of management forecasts, bad news forecasts, and 8-Ks post shock. In contrast, in the

same panel we also present the results using the subsample where CEOs are also chairmen of the board as the "treated" sample. The results on the textual attributes of management conference call language and the number of 8-Ks issued are not significant, whereas this subsample of firms also issues more management forecasts and bad news forecasts. Overall, these results show that managers with greater career concerns become more transparent in their disclosure, consistent with the corporate control hypothesis.

As a second robustness test, we return to our main treatment sample of firms that are in the same industry as hostile takeover targets and announcement CAR >1%, and instead consider an alternative control sample. Our main control sample includes a random sample of 100 firms. In our robustness test, we alternatively consider control firms that are in the same two-digit SIC code as treatment firms, but are not in the same three-digit SIC or TNIC3 industry. This alternative control sample has the advantage of being more similar to our treatment firms in terms of fundamentals than a random sampling of firms, but potentially understates any disclosure changes in response to a control threat as this set of control firms is arguably subject to a control shock risk, albeit attenuated. The results, presented in Panel B of Table V, are consistent with those in our primary results in Tables III and IV. Specifically, treatment managers use less complex and obfuscatory language in their conference calls after a rival's hostile takeover announcement than control firms, as measured by the average Fog index and obfuscation score. Additionally, treatment firms issue more forecasts, specifically bad news forecasts, and have more 8-Ks than control firms after the control shock. These results show that our main findings are robust to an alternative control group.

5. Conclusion

We test the prediction of the corporate control contests hypothesis, namely managers experiencing control shock resort to more transparent voluntary disclosure to mitigate the possibility of job loss as a result of the change of control. Despite the intuitive appeal of this hypothesis and a long-standing voluntary disclosure literature, research evidence on this hypothesis is scant.

We identify an exogenous shock to corporate control as the shock experienced by industry peers of firms that are hostile takeover targets, and perform a difference-in-difference analysis comparing these peer firms (i.e., treatment firms) to control firms not experiencing the control shock. Such an identification strategy is supported by research findings that the possibility of takeover increases for peer firms of hostile takeover targets (Song and Walking 2000) and that corporate control contests increase target management turnover (Martin and McConnell 1991; Krug and Shill 2008).

We capture voluntary disclosure transparency using both the textual attributes of management language in earnings conference calls and the quantity of voluntary disclosure. We deem conference call language that is less complex and easier to understand (i.e., lower Fog index) and with less obfuscation element (measured following the approach advanced in Bushee et al. 2016) as consistent with more transparent disclosure. Following prior literature, we also employ the number of management forecasts and 8-Ks issued as proxies for more transparent disclosure (Kasznik 1999; Chen et al. 2008; Boone and White 2015).

Our difference-in-difference analyses reveal that relative to control firms, treatment firms exhibit lower Fog index in their conference call language and use less obfuscatory language in conference calls after the announcement of a rival's hostile takeover. Treatment firms also issue more management forecasts, especially bad news forecasts, and release more 8-Ks after the control shock. These results are robust to different ways of forming treatment and control samples. Our evidence is consistent with the

interpretation that managers use transparent disclosure to signal a commitment to curb inefficient investment, which in turn makes the firm a less attractive takeover target and thereby reduces management exposure to turnover risk.

Our study contributes to the voluntary disclosure literature by providing much needed evidence in support of the corporate control contest hypothesis. We also add to the growing "peer effects" literature by offering evidence of peer firms' disclosure response to the economic shocks experienced by rival firms in the same industry.

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Table I: Distribution of Treatment and Control Samples by Year

This table presents descriptive statistics on sample composition and the voluntary disclosure variables. Panel A tabulates sample composition by year. The treatment sample consists of firms in the same TNIC3 as target firms and with three-day CAR greater than 1%. The control sample consists of 100 firms per target randomly selected from the pool of firms that are neither treatment firms nor target firms.

Year	# of hostile takeover announcement	# of Unique Treatment Firms	Average # of Treatment Firms per Event	# of Unique Control Firms	Average # of Controls Firms per Event
	(1)	(2)	(3)	(4)	(5)
1997	17	646	43	1530	100
1998	13	598	54	1201	100
1999	18	222	14	1618	100
2000	9	485	60	859	100
2001	4	337	84	391	100
2002	6	214	36	573	100
2003	10	385	44	926	100
2004	4	223	56	389	100
2005	4	135	34	389	100
2006	5	70	14	484	100
2007	2	57	29	200	100
2008	6	283	47	575	100
2009	3	323	144	295	100
2010	3	21	7	297	100
2011	9	312	35	835	100
2012	2	61	31	196	100
2014	2	295	148	197	100

Table II: Descriptive Statistics

This table presents descriptive statistics the voluntary disclosure variables. Panel A presents the mean values of conference call Fog index and obfuscation score for the presentation section of the call (FOG_PR, OBFU_PR), the Q&A portion (FOG_QA, OBFU_QA) and the average of both the presentation and Q&A section (FOG, OBFU), respectively, and test of difference in means (Diff) using quarterly measurement intervals (four quarters Pre and Post, respectively). Panel B presents the mean values of the number of management forecasts (NMF), bad news management forecasts (BNMF), good news management forecasts (GNMF), 8-Ks (N8K) and test of difference in means (Diff) using annual measurement intervals (two years Pre and two years Post). Panel C presents the mean values of the # management forecasts (NMF), bad news management forecasts (BNMF), good news management forecasts (GNMF), 8-Ks (N8K) and test of difference in means (Diff) using quarterly measurement intervals (four quarters Pre and Post, respectively). ***, ***, * indicates statistical significance at 1%, 5%, and 10%, respectively.

Panel A: Changes in Fog Index (FOG) and the Obfuscation Score (OBFU), quarterly intervals (q-4, q+4)

	Treatment			**Control**		
	Pre	Post	Diff	Pre	Post	Diff
FOG	14.308	14.290	-0.019	13.718	13.745	0.027
FOG_PR	16.205	16.227	0.022	15.522	15.582	0.059*
FOG_QA	12.412	12.353	-0.059	11.913	11.908	-0.005
OBFU	0.252	0.231	-0.021	-0.143	-0.122	0.020
OBFU_PR	0.283	0.298	0.015	-0.207	-0.159	0.048
OBFU_QA	0.221	0.164	-0.057	-0.078	-0.085	-0.007

Panel B: Changes in Disclosure Quantity – NMF, BNMF, GNMF and N8K, annual intervals (t-2, t+2)

	Treatment			**Control**		
	Pre	Post	Diff	Pre	Post	Diff
NMF	3.967	6.429	2.462***	3.130	4.255	1.125***
BNMF	0.846	1.400	0.554***	1.013	1.393	0.390***
GNMF	0.707	0.926	0.219***	0.742	0.906	0.164***
N8K	6.100	8.771	2.671***	4.250	6.028	1.778***

Panel C: Changes in Disclosure Quantity – NMF, BNMF, GNMF and N8K, quarterly intervals (q-4, q+4)

	Treatment			**Control**		
	Pre	Post	Diff	Pre	Post	Diff
NMF	0.301	0.348	0.047***	0.252	0.272	0.020***
BNMF	0.352	0.389	0.037***	0.350	0.370	0.020**
GNMF	0.443	0.412	-0.031*	0.357	0.316	-0.041***
N8K	1.811	1.877	0.066	1.158	1.188	0.030

Table III: Difference-in-Difference Regression on Textual Attributes of Managerial Conference Call Language

This table presents the difference-in-difference regression results with the conference call Fog index and obfuscation score for the presentation section of the call (FOG_PR, OBFU_PR), the Q&A portion (FOG_QA, OBFU_QA) and the average of both the presentation and Q&A section (FOG, OBFU), respectively, as dependent variables, measured using four quarters pre and post. TREAT is an indicator variable coded as one for the treatment sample and zero for the control sample. POST is an indicator variable coded as one for the post-shock period and zero for the pre-shock period. Detailed definitions of all variables are provided in Appendix A. Firm-clustered t-statistics are reported in parentheses. ***, ***, * indicates statistical significance at 1%, 5%, and 10%, respectively (two-tailed). Regression model:

$$Disclosure\ Proxies_{i,t} = \beta_0 + \beta_1 Treat_{i,t} + \beta_2 Post_{i,t} + \beta_3 (Treat_{i,t} * Post_{i,t}) + Controls_{i,t} + e_{i,t}$$

Quarterly Intervals (q-4, q+4)

	FOG (1)	OBFU (2)	FOG_PR (3)	OBFU_PR (4)	FOG_QA (5)	OBFU_QA (6)
Intercept	767.878***	-26.015*	894.751***	-25.148	641.004***	-26.882
	(48.67)	(-1.74)	(46.59)	(-1.31)	(34.28)	(-1.59)
TREAT	20.304***	17.168***	21.285***	19.928***	19.323***	14.407***
	(5.29)	(4.69)	(4.77)	(4.48)	(3.88)	(3.15)
POST	-1.202	-1.572	-1.071	-1.314	-1.332	-1.83
	(-0.66)	(-0.94)	(-0.58)	(-0.72)	(-0.49)	(-0.76)
TREAT x POST	-7.143**	-5.614**	-5.550**	-5.000*	-8.735**	-6.228
	(-2.46)	(-2.09)	(-2.00)	(-1.82)	(-2.01)	(-1.58)
SIZE	1.525	1.933	-6.435***	2.499	9.485***	1.368
	(1.22)	(1.54)	(-4.24)	(1.62)	(6.11)	(0.92)
ROA	-36.163	-34.968	-31.926	-32.124	-40.4	-37.813
	(-1.51)	(-1.42)	(-1.11)	(-1.09)	(-1.26)	(-1.23)
LOSS	22.983***	16.92***	19.755***	16.198***	26.211***	17.641***
	(5.6)	(4.25)	(3.9)	(3.2)	(5.24)	(3.79)
MTB	4.659***	1.884	4.77***	3.025*	4.549***	0.744
	(3.5)	(1.41)	(2.71)	(1.7)	(3.04)	(0.52)
LEV	15.817	8.612	35.12***	11.977	-3.486	5.246
	(1.59)	(0.91)	(2.73)	(0.93)	(-0.31)	(0.51)
SPI	147.564	122.278	46.97	32.585	248.158*	211.971*
	(1.47)	(1.23)	(0.36)	(0.25)	(1.9)	(1.65)
RET	-4.545***	-3.289**	-0.29	-0.251	-8.799***	-6.327***
	(-2.62)	(-1.98)	(-0.15)	(-0.13)	(-3.59)	(-2.79)
RET_STD	61.516***	29.546	37.277	24.943	85.755***	34.149*
	(3.17)	(1.51)	(1.33)	(0.84)	(3.71)	(1.78)
INST	7.814	7.574	-3.451	-2.705	19.078*	17.852*
	(0.87)	(0.88)	(-0.32)	(-0.25)	(1.74)	(1.79)
NAF	1.5	2.247	0.861	1.006	2.139	3.487
	(0.67)	(1.1)	(0.36)	(0.42)	(0.69)	(1.29)
Year and Quarter Fixed Effect	Included	Included	Included	Included	Included	Included
N	16,653	16,653	16,653	16,653	16,653	16,653
Adj-Rsq	0.045	0.030	0.073	0.039	0.030	0.013

Table IV: Difference-in-Difference Regression on the Number of Management Guidance and 8-K Filings

This table presents the difference-in-difference regression results with the number of management forecasts (NMF), bad news management forecasts (BNMF), good news management forecasts (GNMF), 8-Ks (N8K) as dependent variables, measured using two different measurement intervals – annual intervals of two years before and after (Panel A) and quarterly intervals of four quarters (Panel B). TREAT is an indicator variable coded as one for the treatment sample and zero for the control sample. POST is an indicator variable coded as one for the post-shock period and zero for the pre-shock period. Detailed definitions of all variables are provided in Appendix A. Firm-clustered t-statistics are reported in parentheses. ***, ***, * indicates statistical significance at 1%, 5%, and 10%, respectively (two-tailed). Regression model:

$$Disclosure\ Proxies_{i,t} = \beta_0 + \beta_1 Treat_{i,t} + \beta_2 Post_{i,t} + \beta_3 (Treat_t * Post_i) + Controls_{i,t} + e_{i,t}$$

Panel A Annual Intervals (t-2, t +2)

	MF (1)	NBMF (2)	NGMF (3)	N8K (4)
Intercept	168.697**	134.127***	50.457**	780.751***
	(2.45)	(4.45)	(1.99)	(11.98)
TREAT	-25.757	-13.438**	-0.212	101.807***
	(-1.27)	(-2.31)	(-0.04)	(8.08)
POST	10.674	-6.065	-11.99**	-2.41
	(0.84)	(-1.03)	(-2.06)	(-0.36)
TREAT x POST	76.84***	18.083**	1.01	48.126***
	(4.13)	(2.32)	(0.14)	(5.00)
SIZE	-41.012***	-7.365***	0.815	21.411***
	(-6.04)	(-2.83)	(0.35)	(3.00)
ROA	202.1***	24.1	23.049	-298.054***
	(4.2)	(1.16)	(1.41)	(-5.53)
LOSS	-156.471***	-15.88**	-23.873***	58.935***
	(-7.69)	(-2.31)	(-3.64)	(3.74)
MTB	8.698	-0.399	-1.773	-13.201***
	(1.36)	(-0.22)	(-0.92)	(-3.03)
LEV	52.682	-29.356*	5.385	323.027***
	(1.03)	(-1.93)	(0.39)	(9.38)
SPI	-899.319***	-43.396	-45.325	141.527
	(-6.52)	(-0.75)	(-1.07)	(0.94)
RET	15.589**	-0.545	0.987	16***
	(2.49)	(-0.38)	(0.39)	(3.47)
RET_STD	-56.15	-28.753	28.243	76.899
	(-0.95)	(-1.15)	(1.02)	(1.37)
INST	497.819***	60.735***	51.024***	182.435***
	(11.07)	(4.96)	(4.5)	(5.41)
NAF	327.184***	24.599***	2.612	66.987***
	(28.42)	(5.24)	(0.56)	(4.61)
Year Fixed Effect	Included	Included	Included	Included
N	19,077	6,002	6,002	36,727
Adj-Rsq	0.2846	0.1031	0.06667	0.4486

Table IV: Difference-in-Difference Regression on the Number of Management Guidance and 8-K Filings (continued)

Panel B Quarterly Intervals (q-4, q+4)

	MF (1)	NBMF (2)	NGMF (3)	N8K (4)
Intercept	-1.139	52.941***	6.026	181.591***
	(-0.29)	(8.72)	(1.13)	(8.25)
TREAT	-1.296	-1.327	1.435	30.95***
	(-1.37)	(-0.92)	(0.84)	(10.43)
POST	-0.799**	0.593	-1.181	0.152
	(-2.01)	(0.63)	(-1.32)	(0.11)
TREAT x POST	1.795***	2.882*	-0.215	5.333**
	(2.74)	(1.74)	(-0.14)	(2.53)
SIZE	4.163***	-1.802***	1.42***	10.765***
	(10.03)	(-4.50)	(3.18)	(7.71)
ROA	0.152	4.55	31.674***	0.253
	(1.55)	(0.70)	(2.72)	(0.55)
LOSS	-6.844***	8.433***	-11.49***	26.627***
	(-8.43)	(5.58)	(-7.25)	(10.77)
MTB	-0.225	0.051	-0.074	-0.095
	(-0.74)	(0.10)	(-0.12)	(-0.10)
LEV	-2.556	-7.161**	9.328**	91.37***
	(-1.10)	(-2.39)	(2.51)	(13.43)
SPI	-159.63***	-9.332	-110.018***	-257.849***
	(-9.80)	(-0.26)	(-2.80)	(-4.30)
RET	-2.252***	-5.293***	3.267***	0.225
	(-6.70)	(-7.21)	(4.30)	(0.20)
RET_STD	19.683***	6.787	18.311***	61.627***
	(6.78)	(0.90)	(2.59)	(5.27)
INST	50.016***	15.347***	7.799**	40.681***
	(19.85)	(5.18)	(2.56)	(5.47)
NAF	11.226***	5.879***	-0.375	9.12***
	(19.74)	(6.34)	(-0.41)	(3.52)
Year and Quarter Fixed Effect	Included	Included	Included	Included
N	120,416	21,108	21,108	120,808
Adj-Rsq	0.211	0.035	0.320	0.328

Table V: Difference-in-Difference Regressions - Alternative Treatment Sample and Control Sample

This table reports the DID coefficients on *Treat * Post* and firm-clustered t-statistics (in parentheses) in the following model using alternative treatment and control samples. Panel A employs two treatment samples – the TNIC3 peers where CEO is not Chairman of the Board, and the TNIC3 peers with CEO also serving as Chairman of the Board. We measure textual attributes using quarterly intervals (q-4, q+4) and disclosure quantity using annual intervals (t-2, t+2). The control sample for Panel A are 50 firms per takeover event randomly selected from the pool of firms that are neither treatment firms nor target firms. The treatment sample in Panel B employs is TNIC3 peers with 3-day CAR around the rival's announcement date >1%. The control sample for Panel B is firms in the same two-digit SIC code as treatment firms that are neither TNIC3 peers nor target firms.

$$Disclosure\ Proxies_{i,t} = \beta_0 + \beta_1 Treat_{i,t} + \beta_2 Post_{i,t} + \beta_3 (Treat_{i,t} * Post_{i,t}) + Controls_{i,t} + e_{i,t}$$

Panel A: Alternative Treatment Sample using CEO Duality as a proxy of entrenchment

	Conference call textual attributes		Quantity of management forecasts and 8-Ks		
Treatment Sample	FOG (q-4, q+4)	OBFU (q-4, q+4)	NMF (t-2, t+2)	N8K (t-2, t+2)	NBMF (t-2, t+2)
CEO ≠ Chairman					
Treat x Post	-8.493*	-7.220*	208.030***	60.311***	16.873
(t-statistics)	(-1.69)	(-1.67)	(6.01)	(3.29)	(1.56)
Control variables & YR/QTR FE	Included	Included	Included	Included	Included
N	8,618	8,618	10,793	20,055	3,774
Adj. R2	0.069	0.032	0.304	0.560	0.104
CEO = Chairman					
Treat x Post	-1.283	-5.343	194.268***	47.283***	11.623
(t-statistics)	(-0.25)	(-1.12)	(5.82)	(3.00)	(1.07)
Control variables and YR/QTR FE	Included	Included	Included	Included	Included
N	8,367	8,367	10,836	22,215	4,193
Adj. R2	0.067	0.036	0.310	0.438	0.108

Panel B: Alternative Control Sample using firms in the same two-digit SIC codes of treatment firms as controls

	Conference call textual attributes		Quantity of management forecasts and 8-Ks		
	FOG (q-4, q+4)	OBFU (q-4, q+4)	NMF (t-2, t+2)	N8K (t-2, t+2)	NBMF (t-2, t+2)
Treat x Post	-9.045***	-6.723**	48.174***	52.926***	14.249**
(t-statistics)	(-3.06)	(-2.40)	(2.78)	(5.85)	(2.03)
Control variables & YR/QTR FE	Included	Included	Included	Included	Included
N	21,119	21,119	20,302	40,363	6,513
Adj. R2	0.813	0.046	0.308	0.449	0.089

Appendix A Definition of Variables

Disclosure Proxies

MF	Number of management guidance issued within year t
BNMF	Number of bad news management earnings guidance issued within year t. Bad news guidance is defined as guidance EPS is below the prevailing analyst consensus forecast at the time of guidance issuance.
GNMF	Number of good news management earnings guidance issued within year t. Good news guidance is defined as guidance EPS is above the prevailing analyst consensus forecast at the time of guidance issuance.
N8K	Number of 8-K filings made with SEC within year t.
FOG	The average Fog index for managements' language in both the presentation and Q&A sections of the conference call.
OBFU	The average obfuscation as measured in Bushee et al. (2016) for managements' language in both the presentation and Q&A sections of the conference call.
FOG_PR	The Fog index for managements' language in the presentation section of the conference call.
OBFU_PR	Obfuscation as measured in Bushee et al. (2016) for managements' language in the presentation section of the conference call.
FOG_QA	The Fog index for managements' language in the Q&A section of the conference call.
OBFU_QA	Obfuscation as measured in Bushee et al. (2016) for managements' language in the Q&A section of the conference call.

Control Variables for Difference-in-Difference Regression

PEER	Dummy variable that equals to 1 if the firm is defined as a peer firm to the hostile takeover target, and 0 otherwise.
POST	Dummy variable that equals to 1 if the year is in year t+1 and t+2, where year t if the year of hostile takeover announcement.
SIZE	Natural logarithmic of market value of equity (PRCC_F*CSHO) at the end of year t
ROA	Income before extraordinary items (IB) divided by total assets (AT), adjusted by industry-year medians.
LOSS	Dummy variable that equals to 1 if net income is negative.

MTB	Market value of equity (PRCC_F*CSHO) plus book value of total liabilities (LT), divided by book value of total assets (AT).
LEV	Long term debt (DLTT) plus short term debt (DD1), divided by total assets (AT).
SPI	Special items (SPI) divided by total assets (AT).
RET	Buy-and-hold stock return over year t.
RET_STD	Standard deviation of monthly returns over year t.
NAF	Number of analysts issuing earnings forecast at the end of year t.

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Size	Natural log of market value of equity at the beginning of the quarter.
Leverage	Long term debt plus short term debt, scaled by total assets at the beginning of the quarter.
BM	Book value of equity scaled by market value of equity at the beginning of the quarter.
Returns	Buy-and-hold return over the quarter, in percent.
Acquisitions	Total acquisitions during the quarter, scaled by total assets at the beginning of the quarter.
CapIntensity	Net plant, property, and equipment scaled, scaled by total assets at the beginning of the quarter.
Capex	Amount of capital expenditures scaled by total assets at the beginning of the quarter.
R&D	Ratio of research and development expense to sales
Financing	Amount raised from stock and debt issuances during the quarter scaled by total assets at the beginning of the quarter.
σCFO	Standard deviation of cash flows from operations scaled by total assets at over the prior five years.
Goodwill	Indicator variable for whether the firm had a goodwill impairment charge that quarter.
Restructuring	Indicator variable for whether the firm had a restructuring charge that quarter.