Watchdog or Mouthpiece?

The Role of Financial News Media in Corporate Communication

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Abstract

We investigate how financial news media coverage causally affects managers' manipulation decisions in corporate disclosures. While prior research shows that media coverage decreases manipulation incentives for "hard manipulation", such as accounting fraud ("watchdog role"), the role of the media for manipulating qualitative information, "soft manipulation", is ex-ante unclear; theory even predicts that news media may uncritically disseminate corporate information ("mouthpiece role"), thereby increasing managers' incentives for soft manipulation. Using a stacked difference-in-differences design based on restructuring events at the Wall Street Journal, we provide first empirical evidence that news media coverage reduces managers' incentives for soft manipulation. The effect is driven by articles with more editorial content and more pronounced for firms with less monitoring through other corporate governance mechanisms.

Keywords: financial media, managerial manipulation, corporate disclosure, earnings conference calls

JEL Classification: D82, G14, G34, M41

1. Introduction

In corporate disclosures, managers have incentives to manipulate negative pieces of their private information to avoid negative financial consequences for their firms or themselves, e.g. to prevent their firm's stock price from falling (Goldman et al., 2022). Prior empirical research shows that the news media act as an effective monitor of adverse manager behavior ("watchdog") regarding "hard manipulation", i.e., the manipulation of verifiable "hard" information, such as accounting fraud (Miller, 2006; Dyck et al., 2010), and other forms of corporate misconduct which carry severe legal consequences if uncovered (Dyck et al., 2008; Dai et al., 2015; Heese et al., 2022). That is, intensive firm-specific media coverage reduces managers' incentives for hard manipulation and corporate misconduct, as these acts are uncovered with a higher probability.

Alternatively, managers can manipulate the qualitative content of their disclosures which is harder to verify ("soft manipulation"), for instance by obfuscating their communication. The news media's role in mediating soft manipulation is ambiguous for two reasons. First, from the perspective of the manager and the firm, uncovering soft manipulation by the news media carries a much lower expectation of negative legal consequences than those associated with hard manipulation such as accounting fraud. Second, while no empirical studies address this issue, recent theoretical work by Goldman et al. (2022), hereafter *GMS*, suggests that concerning soft manipulation the news media's role may be reversed. Their model predicts that rather than acting as a watchdog, news media may inadvertently serve as a mere disseminator of corporate communication ("mouthpiece"). In these circumstances, an increase in media coverage reinforces managers' incentives to manipulate information, as it raises the likelihood that the manipulated information will reach market participants, boosting the firm's stock price. Consequently, an exogenous increase in firm-specific media coverage should lead to an *increase* in soft manipulation.

Ex ante, it is thus unclear whether the news media act as a watchdog or mouthpiece in mediating managers' incentives to use soft manipulation in their disclosures. Our objective is, thus, to empirically test the news media's role in mediating managers' use of soft manipulation. Specifically, we provide first causal evidence that a plausibly exogenous increase in media coverage *decreases* managers' incentives to verbally obfuscate information in earnings conference calls. Our finding that the news media acts as a watchdog for soft manipulation in corporate disclosures rejects the theoretically derived prediction of *GMS* that the news media acts as a company mouthpiece; in contrast, our results align with related empirical literature studying hard manipulation. We find that the aggregate watchdog effect for soft manipulation is driven by news articles that contain a higher proportion of editorial content: While we observe significantly less manager obfuscation following news articles with high editorial content (*watchdog-like articles*), we observe no significant effect following news articles with low editorial content which merely repeat the information provided in the firm's initial earnings announcement (*mouthpiece-like articles*). This result suggests that the watchdog role of the news media is limited to instances where managers expect a high probability of their soft manipulation being uncovered by the news media. Moreover, we show that the watchdog role of the media is most important for firms with otherwise low monitoring intensity through other corporate governance mechanisms, e.g., financial analysts. Managers of firms that are already subject to high levels of monitoring through analyst coverage do not adjust their obfuscation in response to additional media coverage. Therefore, financial news media coverage serves as a substitute for other forms of monitoring.

To measure the causal effect that news coverage has on soft manipulation, we leverage the process of US firms announcing their quarterly earnings: At the end of each quarter, firm managers observe their firm's recent performance and then report on it via a regularly scheduled public press release that the firm publishes on its website and is also required to report to the SEC in form 8-K (*we therefore collect firms' quarterly earnings announcement press releases*). Financial journalists then have the opportunity to write articles about the firm's earnings announcement. This type of day-to-day reporting is conducted by financial newspapers such as the Wall Street Journal (*we therefore collect earnings-related articles from the Wall Street Journal [Online]*). As timely information about firms' fundamentals is essential for investors, financial journalists who want to satisfy this demand report very quickly after the earnings announcement is published—usually within a few hours. After the earnings announcement, firms hold an earnings conference call with analysts and investors, explaining the results of the quarter (*we therefore collect transcripts of quarterly earnings conference calls*). At that point, the firm managers were able to observe the existence and content of the news articles covering the initial earnings announcement. Therefore, they may adjust their use of soft manipulation in relation to a) the existence of an article being written about the earnings announcement and b) the content of the article. We summarize the timing of the steps in the earnings announcement process in Table 1.

[Table 1 – about here]

The setting of firms' quarterly earnings announcements is ideally suited for investigating the causal effect that news coverage has on soft manipulation. Our empirical setting allows us to construct a measure of soft manipulation related to the linguistic complexity of managers' speech in the earnings conference call. Specifically, we use the decomposition approach proposed by Bushee et al. (2018) to identify the portion of managers' linguistic complexity that is related to a latent information component and a latent obfuscation component, respectively. Our empirical estimate for the obfuscation component then represents the linguistic complexity "intended to reduce the informativeness of the disclosure" (Bushee et al., 2018). Moreover, our empirical setting closely resembles the *GMS* model of media coverage of corporate announcements. Therefore, our setting offers a direct empirical test of the mouthpiece prediction by *GMS*, namely that an exogenous increase in media coverage leads to an increase in soft manipulation.

One link in the chain of events that we cannot observe is whether managers are aware of the presence or absence as well as the contents of the Wall Street Journal (WSJ) news articles. However, given the prominence of the WSJ, it is highly likely that managers take notice whether and how their firm is covered between the earnings announcement and the conference call—even on relatively short notice. In fact, managers typically receive daily compilations of media coverage about their firm through professional press clipping services (e.g., LexisNexis). The plausibility of this assumption is further strengthened by the anecdotal evidence we provide in Internet Appendix Part A, Table A.1 where we demonstrate multiple examples of managers referring in the conference call to WSJ coverage from the same day.

In line with our overall empirical design, we construct a comprehensive sample of firms most likely to receive media attention, i.e., S&P 500 firms, for which we merge earnings announcement press releases, the associated WSJ news articles, and the associated earnings conference calls. Following Guest (2021), we use three restructuring events at the WSJ allowing us to exploit plausibly exogenous variation in earnings-related firm-specific news coverage. The restructuring events at the WSJ in January 2007 (January 2008 & Summer 2013) were editorial decisions which made earnings-related articles a lower (higher) priority leading to fewer (more) articles. To identify the average treatment effect that a plausibly exogenous change in news coverage has on manager obfuscation (ATT), we refrain from using the two-way fixed effects model for estimation. This is because using the two-way fixed effects model in a setting with staggered treatment adoption implicitly assumes homogenous treatment effects across both firms and time (Baker et al., 2022). However, treatment effects are unlikely to be homogenous in our case, e.g. because firms may differ in their sensitivity to reputational concerns, leading to varying responses in obfuscation when exposed to media coverage. To address this issue, we estimate a stacked difference-in-difference (DID) model (e.g., Duchin et al., 2025; Dyer et al., 2024; Jeffers, 2024). To identify the appropriate ATT, we use the stacked difference-in-differences estimator recently proposed by Wing et al. (2024), which eliminates bias induced from different implicit weights to treatment and control trends in the basic stacked DID regression model. This approach ensures an unbiased estimate of the ATT, even in the presence of heterogeneous treatment effects. We argue that our implementation of the stacked DID model conforms with the identifying assumptions of difference-in-differences, most notably the parallel trends assumption, and we provide a detailed discussion of this in Section 2.4. Moreover, we conduct standard DID plausibility checks in Section 4.2 which corroborate our arguments.

Our work contributes to multiple strands of literature. First, we add to the research on the role of the press in shaping corporate behavior. As of yet, there exist no empirical studies investigating the news media's role in mediating soft manipulation. However, theoretical work by *GMS* suggests that in the case of soft manipulation, the news media may reinforce firms' incentives to engage in manipulative practices. For hard manipulation and, more generally, corporate misconduct, a substantial body of literature finds that the press serves as a watchdog.

For instance, Miller (2006) shows that the press played a role in the early identification of 29% of accounting malfeasance cases later sanctioned by the SEC. Similarly, Heese et al. (2022) provide evidence that local firms experience a 1.1% increase in violations and a 15.2% rise in penalties following the closure of a local newspaper, indicating that such closures weaken corporate misconduct monitoring. These findings align with several other empirical studies examining the media's role in addressing severe corporate misconduct (Dyck et al., 2010; Dai et al., 2015; Kölbel et al., 2017; An et al., 2020). Our study contributes to this debate by providing empirical evidence that the news media acts as a watchdog for soft manipulation, challenging the theory-based predictions of *GMS*.

Second, we contribute to the literature on firms' impression management. Ahern (2014) shows that firms have incentives to manage media coverage in order to influence their stock prices during important corporate events, consistent with the idea that impression management is an important tool for influencing firms' market performance. Similarly, Flugum (2023) demonstrates that firms short of earnings expectations are more likely to cite stakeholder-focused objectives in their public communication in an effort to spin the narrative towards more favorable outcomes. Moreover, firms use prosocial claims to dilute negative media attention (McDonnel and King 2013). We contribute to this literature by demonstrating that the firm managers' tendency to conduct impression management is heavily influenced by coverage in financial news media.

Finally, we contribute to recent literature investigating manager disclosure behavior in earnings conference calls. Prior research has documented various factors influencing managerial disclosure behavior. Bochkay et al. (2019) find that CEOs' forward-looking disclosures become less frequent and relatively less optimistic as their tenure progresses. Mishra (2024) shows that CEOs who receive option-based compensation are more likely to emphasize political risks in their disclosures in line with the argument that risk-talking substitutes actual risk-taking. Barth et al. (2022) identify 1,364 trigrams that signal nonanswers by managers in earnings calls, which are associated with significantly lower abnormal returns following the call. Similarly, Hollander et al. (2010) provide evidence that managers strategically withhold information from investors by avoiding certain questions. Bushee et al. (2018) show that the complexity of managerial speech in earnings calls has both an information and an obfuscation component. Guest and Yan (2025) show that market participants, especially analysts, do not immediately incorporate the negative performance implications of manager soft manipulation in their firm assessments, therefore not assuming the role of a watchdog for soft manipulation in firm communication. We contribute to this literature by highlighting that manager soft manipulation in earnings conference calls is indeed shaped by prior press coverage of the firm, where the press acts as a watchdog.

2. Data and Empirical Strategy

2.1. Data Sources and Sample Selection

We collect all firms that were included in the S&P 500 at any point in our data collection timeframe from 2000 and 2022 from LSEG Workspace. We focus on S&P 500 firms, because they make up approximately 80% of the U.S. stock market capitalization and receive the most media attention and therefore readers' attention. We match these firms with newspaper articles and metadata from the Factiva database using ISINs, Exchange+Ticker combinations, CUSIPs and company names. We manage to identify 908 of 1031 initial S&P 500 firms in the Factiva database. For these firms, we then collect and combine further datasets from multiple sources.

First, we collect firms' quarterly earnings announcement press releases. Firms are required to report these press releases to the SEC as Exhibit 99 via form 8-K. Consequently, we scrape the SEC website for all 8-K filings of our sample firms. Next, we clean this data to extract only the press releases related to the firms' quarterly earnings announcements in line with the procedure in Li (2008), Miller (2010), Dyer et al. (2017) and Guest (2021). We describe the cleaning steps in part B of our Internet Appendix.

Second, we collect transcripts of the quarterly earnings conference calls for our sample firms from LSEG Workspace including call date and participant list. We conduct multiple data cleaning steps to ensure high data quality. First, we split each call into its key components: firm presentation, and Q&A Session, where we segment the Q&A session further into analyst/investors questions and managers' (mostly CEO and CFO) answers. Calls that cannot be properly segmented (e.g., because one section is missing) are removed from the dataset.

Second, we delete all HTML text and tags in the transcripts. We were able to obtain valid earnings calls from 899 firms of our matched sample of 908 S&P 500 firms. We only keep observations where the earnings call was held after the filing of the SEC 8-K earnings announcement press release, but up to a maximum of 48 hours, to ensure that only relevant earnings calls are matched to the announcements. In the rare case when multiple earnings announcement press releases were matched to a single earnings call, we keep only the press release which is closest in time to the given earnings call.

Third, from the database Factiva, we collect firm-specific articles in the print and online version from the Wall Street Journal. We focus on the WSJ because it is the world's leading business newspaper, making it particularly well-suited for measuring managers' reactions to media coverage. We restrict our sample to articles that focus only on a single company to make sure that any content and the existence of the article itself is only related to the focal firm, and not to competitors. In line with the proposed timeline of our empirical design, we only keep a firm-specific earnings article if its publication datetime (which is collected by Factiva) was in between the earnings announcement and earnings call.[1] In the case that there is more than one firm-specific article between announcement and call, we keep the article with the highest cosine similarity, in accordance with Guest (2021). We manually checked for a random subsample of articles that these are indeed the actual articles reporting about the corresponding announcement. We remove any articles with fewer than 50 words, which are not proper earnings articles but rather short news snippets. We remove articles that belong to recurring non-earnings sections (i.e., Stocks to Watch, Overheard, What to Watch, In Brief), which are unlikely to contain substantive earnings discussion.

Finally, we obtain quarterly firm-specific fundamental data and analyst data from LSEG Workspace, as well as daily stock market data from LSEG Datastream, to use it as controls in

[1] Note that for our main analyses, we do not pre-filter articles using the Factiva subject code identifier (c151, i.e. earnings-related articles), as it is somewhat inconsistent and fails to flag numerous articles related to earnings announcements. Instead, we match only those firm-specific articles published in a relative short time frame between earnings announcement and call to ensure that our sample comprises all earnings-related articles. However, our results remain qualitatively unchanged when using the smaller subset of earnings-related articles categorized as such by Factiva.

our subsequent analyses. We explain our choice of control variables as well as their definitions in detail in Section 2.2.

We remove observations with missing control variables or outcome variables. After merging the different data sources, we end up with 5,154 firm-quarter observations around the WSJ restructuring events, taking place in 2007Q1, 2008Q1, and 2013Q4 respectively. We thus require sample firms to have at least one observation in both the respective pre- and post-treatment periods which leaves us with 4,987 observations. Further, as described in more detail in Section 3.4, observations are only eligible for the control sample if their media coverage status matches the treatment observations in the pre-event period. Deleting all observations that are not eligible as treatment or control observations results in a final sample of 2,675 observations that are used for our empirical analysis. This sample size is very much in line with the study of Guest (2021), considering that we have introduced an additional data source (transcripts of earnings conference calls).

2.2. Measurement of Soft Manipulation: Manager Obfuscation

We construct a measure of manager soft manipulation, i.e., the qualitative content of manager disclosure which is harder to verify compared to forms of hard manipulation, e.g., accounting fraud. *GMS* suggest this can be measured through the linguistic complexity of the disclosure. We exploit our setting of firms' quarterly earnings announcements by applying the decomposition approach proposed by Bushee et al. (2018). Thus, we further decompose the complexity in manager speech during earnings conference calls into its two latent components: an information component, i.e. linguistic complexity attributable to informative technical disclosure, and an obfuscation component, i.e. linguistic complexity "intended to reduce the informativeness of the disclosure". Bushee et al. (2018) validate their approach by showing that the information component is negatively associated with information asymmetry, while the obfuscation component is positively associated with information asymmetry. Many recent studies use this decomposition method to measure manager obfuscation, e.g., Mekhaimer et al. (2024), Bushee and Huang (2024), Chourou et al. (2024).

We therefore adopt Bushee et al. (2018)'s regression decomposition approach to extract the obfuscation component from the complexity of managerial speech in the presentation portion of earnings conference calls. We regress the manager's language complexity in the presentation portion of the call on the analysts' language complexity in the Q&A section of the call and other variables that control for other sources of business complexity. The rationale is that we want to isolate the portion of linguistic complexity that represents managers' appropriate response to business complexity (in the absence of obfuscation) in the predicted value of manager's language complexity. Thereby, obfuscation incentives are isolated in the residuals of this regression. This is achieved by exploiting the variation in linguistic complexity of an outside party that is unlikely to have obfuscation incentives, i.e., the analysts, as a benchmark which is unique to the earnings call setting.

We construct the measure of manager obfuscation based on the manager's linguistic complexity in the presentation of the call similar to Chourou et al. (2024). In our case, the presentation section arguably best captures managers' reactions to reporting by the news media. This approach is consistent with the anecdotal evidence in Internet Appendix A, Table A.1 where all examples of managers mentioning press coverage in the Wall Street Journal originate from the presentation section. Additionally, manager complexity in the presentation section most directly reflects a reaction to the existence and contents of a media news article, before the manager interacts with further outside participants regarding the quarterly earnings, since analyst Q&A are done after the presentation section in the earnings call. In contrast, the complexity of managerial answers during the Q&A portion is likely influenced by analyst behavior to a large extent (e.g., managers may obfuscate more when responding to negatively toned questions), making it a noisier indicator of how managers react to news media. Also, managers use much more complex language in the presentation part of the call than in the response part of the call (see Bushee et al., 2018). To construct our measure of soft manipulation, we estimate the following regression:

$$FogManager_{i,t} = \alpha + \beta FogAnalyst_{i,t} + \boldsymbol{\theta}'\mathbf{Controls_{i,t}} + \epsilon_{i,t} \tag{1}$$

where $FogManager_{i,t}$ is the Fog Index of the manager's presentation in the conference call of firm i in year-quarter t. The Fog Index is an established proxy for assessing the linguistic complexity of financial documents (e.g. Li, 2008; Lo et al., 2017). The Fog Index attempts to measure the years of formal education a person requires to understand a given text on the first reading (Gunning, 1952). It is calculated as a linear combination of the average number of words per sentence and the fraction of complex words (fraction of words with three or more syllables). $FogAnalyst_{i,t}$ measures the linguistic complexity of the analysts' language in the Q&A section of the call and controls for the benchmark level of complexity that one would expect in the absence of obfuscation (Bushee et al., 2018).

Regressing the complexity of manager's speech on the complexity of analysts' speech (as well as control variables) then gives the information component (i.e., the part of the manager's speech complexity that is used to convey information) as the fitted value $Fog\widehat{Manag}er_{i,t}$, and the obfuscation component as the residuals $\widehat{\epsilon_{i,t}}$. The control variables represent other sources of business complexity beyond the complexity conveyed in $FogAnalyst_{i,t}$ and assist in separating the part of the variation in $FogManager_{i,t}$ that is necessary to communicate information versus obfuscation. Control variables include firm size via the total assets of the firm (tot_assets), the income before discontinued operations and extraordinary items ($income\_before$), the number of sell-side analysts covering the firm in a given year-quarter ($n\_analyst$), the book-to-market ratio (bm_ratio), the percentage of debt to total assets of the firm ($leverage$), an indicator whether the firm fell short on their actual earnings compared to the mean analyst forecast in the given quarter ($fell\_short$), the one year stock return (RI_1_year), the rolling standard deviation of daily returns over the prior 260 trading days ($P\_std\_260\_days$), and the earnings per share (eps_actual). We report the results of this regression in the Internet Appendix Table A.2.[2] Similar to Bushee et al. (2018), we find that $FogAnalyst_{i,t}$ is an important determinant of $FogManager_{i,t}$ with a t-statistic of over 45.

2.3. Summary Statistics

In Table 2 we present summary statistics of our dependent variable as well as all explanatory variables. The highest Fog Index observed in the presentation portion of the call is 23.32. The mean Fog Index in the presentation is 13.94, which is much lower than the typical range of 18–

[2] Note that we estimate the obfuscation measure based on the full sample of earnings calls, prior to further sample selection for our DID design. However, our results remain identical if we estimate the obfuscation measure based on our final sample only.

21 observed for financial documents (e.g., Li, 2008; Loughran and McDonald, 2014; Lo et al., 2017; Dyer et al., 2017) likely due to spoken language being naturally less complex than business documents. However, the mean Fog Index is in line with Bushee et al. (2018) who also analyze transcripts of earnings conference calls. For our measure of Obfuscation, we observe a standard deviation of 1.37. To enhance the interpretability of our coefficients, we standardize our measure of Obfuscation to a standard deviation of one.

[Table 2 – about here]

2.4. Identification Strategy

To measure the causal impact of news coverage on managers' obfuscation in corporate communication and specifically earnings calls, we follow Guest (2021) and use three restructuring events at the WSJ, allowing us to exploit plausibly exogenous variation in earnings-related firm-specific news coverage. The restructuring event in January 2007 made earnings-related articles a lower priority leading to fewer articles. On the contrary, the restructuring events in January 2008 and Summer 2013 made earnings-related articles a higher priority leading to more articles. In Figure 1, we plot overall earnings-related coverage in the WSJ around these three events.[3] In particular, the events are the following: (I) January 2007: Gordon Crovitz, then WSJ publisher, and then-managing editor Paul Steiger implement a paper redesign, Journal 3.0, that shifts the focus of reporting from breaking news articles to articles reporting exclusive "scoops" (Crovitz 2007). As earnings announcements are breaking news from the WSJ's perspective, this event decreased the number of firms covered in earnings-related articles each quarter. (II) January 2008: Robert Thomson takes over as WSJ publisher and reverts much of the Journal 3.0 design, therefore also restoring the proportion of articles covering breaking news events. This led to an increase in firms covered in earnings-related

[3] Note that Figure 1 is based on the universe of all earnings-related articles in the WSJ. For our empirical identification, we then match those articles that were published right in between an earnings announcement and an earnings call.

articles. (III) Summer 2013: Gerard Baker, managing editor of the WSJ, announces the integration of the newsrooms of the DJ Newswire and the WSJ. The goal of this integration is to accelerate the output of "unrivaled news coverage". New reporters and editors are hired and sometimes positions are consolidated, whereby the redundant WSJ reporter keeps his/her position, and the redundant DJ Newswire reporter is laid off. This led to an increase in firms covered in earnings-related articles.

[Figure 1 – about here]

We follow Guest (2021) in how we transform these restructuring events, which at first have no firm-level variation, into a design that can be used in a difference-in-difference analysis. We assign those firms to the treatment group for which the coverage has changed into the expected direction around the respective WSJ restructuring. That is, treatment firms are firms that did receive coverage two quarters before the first event and then did not receive coverage two quarters after the event; similarly, firms that did not receive coverage two quarters before the second and third event and then did receive coverage two quarters after the event. To account for the fact that treated firms are either affected by a coverage-decreasing event (first event) or a coverage-increasing event (second and third events), we follow Gad et al. (2024) and set the treatment indicator to -1 for firms that were affected by a coverage-decreasing event (i.e., event 1), and to +1 for firms that were affected by a coverage-increasing event (i.e., events 2 and 3). This effectively reverses the sign of the ATT for the coverage-decreasing event 1. This allows us to estimate an aggregate average treatment effect on the treated over all three events in the same stacked regression specification. Note that our estimated stacked DID regression model accounts for heterogeneous treatment effects. Thus, even if the treatment effects of decreasing coverage are different from the treatment effects of increasing coverage, our estimation of the aggregate ATT will remain unbiased. The cutoffs for the pre- and post-periods are set at 2007Q1, 2008Q1, and 2013Q4 for the three events, respectively. Control firms are those firms that had the same coverage-status before the event as the treatment firms (i.e., coverage before the first event; no coverage before the second and third event), but for which coverage has not changed because of the restructuring. To understand what a typical treated firm in our sample looks like, note that these are neither the largest firms in the S&P 500, which are always covered, nor only the very small firms in the S&P 500 which never receive any coverage. Instead, these are middle-to-large-sized firms in the S&P 500 for which media coverage depends, e.g., on the capacity of the WSJ and may fluctuate from quarter to quarter. Representative examples of treated firms in our sample are Altria (99th by current market cap), U.S. Bancorp (144th), Apollo Global Management (153rd), Schlumberger (160th) or CSX Corporation (163rd).

While we follow the general empirical idea of Guest (2021), we strongly deviate from his implementation of the DID model. This is because Guest (2021)'s design implicitly sets very restrictive assumptions that we argue are unlikely to hold. First, while describing his DID model that explains financial market outcomes, they note that they do not include main effects of his treatment indicator $Treat_{i,q}$ because it is "perfectly colinear with the fixed effects". While this would be true for a standard two-way fixed effect model where the treatment status is non time-varying, it is incorrect for the design of Guest (2021) because a firm's treatment status varies across events (see also the subscript (i, q) of the variable indicating group membership), meaning that a firm can appear as treatment and control firm in the data during different quarters. Therefore, the model of Guest (2021) is missing the treatment status as important control (see Deshpande and Li (2019) for the correct specification in such a design). This essentially leads to a problem of omitted-variable bias. Specifically, if there is a base effect of $Treat_{i,q}$ on the dependent variable, i.e., if we were to add $Treat_{i,q}$ to the regression model its true coefficient would be $\neq 0$, then omitting the base effect generally biases the estimates of all other coefficients. One very restrictive way to ensure that the true base effect of $Treat_{i,q}$ on the dependent variables is $= 0$ is to assume that treatment assignment is done randomly, and thus $Treat_{i,q}$ is independent of the outcome variable by construction. We argue that random treatment assignment is unlikely to be the case for Guest (2021) as well as for us, as variables that influence the level of the outcome (market outcomes or Obfuscation respectively) are likely to also influence the treatment decision, i.e. the (exogenous) journalist decision to start/stop covering the firm. This is because Guest (2021) and we assume that after the WSJ restructuring events, journalists do not randomly start/stop covering firms but rather have an inherent implicit newsworthiness ranking of firms which is dependent on various observable and unobservable factors. Phrased differently, while it is plausible to assume that the WSJ events exogenously lead to fewer/additional firms to be covered, it is not plausible to assume that the firms chosen are chosen randomly, independent of variables correlated with the outcome variable. To sum up, omitting the base effect of $Treat_{i,q}$ requires, for instance, the assumption of random treatment assignment, which is much stronger than the canonical parallel trend assumption, where treatment assignment must only be mean-independent of variables that affect the trend in the outcome (including unobserved variables in the error $\epsilon_{i,q}$) rather than the level of the outcome (see Roth et al., 2023).

Second, since publication of Guest (2021), econometricians have found more general flaws in using the two-way fixed effect model for estimating the average treatment effect on the treated (ATT) when there is staggered treatment adoption due to "forbidden comparisons" where early-treated units are used as control group for later-treated units. Then, in the presence of heterogeneous treatment effects either across units or across time, the two-way fixed effects model may not yield a sensible estimand for the ATT (for details we refer to e.g. Baker et al., 2022 and Roth et al., 2023). We do not see any theoretical justification to safely assume homogenous treatment effects. Instead, treatment effects may be heterogeneous due to both firm- and time-specific factors. Firms differ in their sensitivity to public scrutiny and reputational concerns, leading to varying responses in obfuscation when exposed to media coverage. Firm visibility and prior exposure to the press may also shape the marginal impact of treatment. Over time, changes in the media environment, investor attention, or broader market conditions can further influence the strength of the treatment effect.

To circumvent both problems, we deviate from the regression specifications of Guest (2021) and estimate a stacked difference-in-difference model (e.g., Duchin et al., 2025; Dyer et al., 2024; Jeffers, 2024). The idea of a stacked difference-in-difference model is to create event-specific 2×2 datasets that are "clean", i.e. include only not-yet treated or never treated units as control group but exclude those units that have already been treated at an earlier event. These datasets are then stacked together, and a TWFE DID regression is estimated using unit-event and time × event fixed effects.[4] The stacked DID regression then estimates the ATT from each clean 2×2 dataset and finally applies variance-weighting to combine the treatment effects across cohorts efficiently (Baker et al., 2022). This efficient variance-weighting is determined by the number of treated units and the variance of treatment within each stacked event. While the basic stacked DID regression only involves clean controls, Wing et al. (2024) show that it still does not identify the ATT of interest because it applies different implicit weights to treatment and control trends across different events. However, this bias can be calculated and thus eliminated by using corrective sample weights, as shown by Wing et al. (2024).

Therefore, we create a clean dataset for each of the three WSJ restructuring events and then stack these events together into a single dataset. As control group for a given event, we use all not-yet treated units (i.e., never-treated firms as well as firms treated at a later event). We estimate the following DID regression using weighted least squares with the sample weights of Wing et al. (2024):

$$Obfuscation_{i,e,t} = \beta(Treat_{i,e} \times Post_t) + \boldsymbol{\theta}'\mathbf{Controls}_{\mathbf{i,e,t}} + \alpha_{i,e} + \gamma_t + \varepsilon_{i,e,t} \quad (2)$$

where $Obfuscation_{i,e,t}$ is our measure of soft manipulation for firm i during restructuring event e in year-quarter t. The $\alpha_{i,e}$ are firm × event fixed effects which subsume the main effect of $Treat_{i,e}$. The γ_t are year-quarter fixed effects.[5] As explained above, the treatment status $Treat_{i,e}$ is defined as -1 for firms that were affected by the (coverage-reducing) first event, and $+1$ for firms that were affected by the (coverage-increasing) second or third event, and 0 for control firms. The indicator $Post_t$ equals 1 in the two quarters following each restructuring event and 0 else. Then, the coefficient on the $Treat_{i,e} \times Post_t$ interaction gives our average treatment effect on the treated (ATT) of interest which yields the causal effect of an exogenous

[4] Note that in our case, as our events are non-overlapping in time, time × event fixed effects are equivalent to time fixed effects.

[5] Note that in our case, the inclusion of $year\text{-}quarter$ fixed effects is identical to the inclusion of $event \times year\text{-}quarter$ fixed effects. This is because our events are non-overlapping in time, meaning that a given year-quarter cannot appear in more than one event. Thus, $Post_t$ is perfectly colinear with the time trend with just the inclusion of $year\text{-}quarter$ fixed effects.

increase in news coverage on Obfuscation. In accordance with Wing (2024), we cluster standard errors at the group level on which treatment is independently assigned, i.e. the firm level, but not the firm × event level to allow for firm-level standard error dependence across events.

We argue that our stacked DID model conforms with the identifying assumptions of difference-in-differences, most notably the assumption of parallel trends. For this we must assume that the WSJ decision to cover a given firm (our treatment) must be mean-independent of variables that affect the trend in manager obfuscation (our outcome). As the concept of "manager obfuscation" revolves around the idea of managers possessing negative private information which they want to avoid revealing, we have a strong case that WSJ coverage decisions are independent of these factors which are unobservable for the journalist and that (at least partly) drive the trend in manager obfuscation. Of course, since treatment assignment (WSJ coverage decisions) takes place before the obfuscation in the earnings call, journalists can also not condition their coverage decisions on noticing these latent factors through manager obfuscation. For parallel trends to be violated, there must thus exist some further factor, that is 1) observable for the journalist thereby potentially affecting coverage decisions, 2) actually does affect the journalist's coverage decision (i.e., affects the perceived newsworthiness of the firm from the journalist's perspective), and 3) also affects the trend (not just the level) in manager obfuscation between pre- and post-treatment periods. Our restriction of the sample period to the WSJ restructuring events further alleviates the concern of a violation of parallel trends, because it induces plausibly exogenous variation in WSJ coverage decisions. That is, the *changes* in coverage decisions in the timeframe around the WSJ restructuring events are less likely to correlate with *changes* in any factor observable to the journalist that influences how newsworthy the journalist deems the firm (see conditions 1 and 2 above). Instead, many of the changes in coverage around the WSJ restructuring events may be driven by none of these observable factors, or not even by any factor at all except for the restructuring event itself. To phrase it differently, as Guest (2021) argues, if journalists have an implicit (or explicit) newsworthiness ranking of firms that is stable from pre- to post-treatment period, then the parallel trend assumption holds. In conversations with journalists, Guest (2021) was able to confirm this notion of implicit (and sometimes explicit) newsworthiness rankings of firms by journalists, lending credibility to this assumption. This notion of a stable implicit newsworthiness ranking is further strengthened by a well-documented finding across several disciplines that firms are more likely to be covered if they have been covered in the past due to familiarity of the audience with the firm as well as lower reporting costs for the journalist (Graf-Vlachy et al., 2020). Still, we conduct empirical tests that strengthen the plausibility of the parallel trends assumption. First, we operationalize the concept of an implicit newsworthiness ranking and find that these rankings are indeed persistent over time (see Internet Appendix C). Second, we find no significant violation of parallel pre-trends between control and treatment groups (see Section 4.2). Third, we find no significant treatment effect in pseudo-event tests (see Section 4.2).

3. Results

3.1. Manager Obfuscation after an Exogenous Change in News Coverage

In this section we analyze whether the news media acts as a watchdog or mouthpiece for soft manipulation, i.e., whether an exogenous increase in media coverage leads to a decrease or an increase in manager obfuscation in the earnings call. We report our estimation results of regression equation (2) in Table 3:

[Table 3 – about here]

We find that there is a significant negative relationship between media coverage and managers' obfuscation during earnings calls. Specifically, an exogenous increase in media coverage leads to a decrease in obfuscation by 0.11 standard deviations. This is in line with the watchdog hypothesis and preceding empirical studies which have investigated the effect of media coverage on hard manipulation, e.g., accounting fraud. Our study is the first to document that media also acts as a watchdog soft manipulation, specifically, obfuscation in corporate communication.

3.2. Treatment Effect Heterogeneity: Editorial Content of the Article

In this section, building upon our main result that the press acts as a watchdog for soft manipulation, we test for effect heterogeneity in the cross-section. Specifically, we examine whether the media's aggregate watchdog role depends on the extent of editorial content within the news article covering the earnings announcement. This is reasonable to assume, as if the manager observes an article with a high degree of editorial content (i.e., a watchdog-like article) about their firm, they will fear that their manipulation attempts will be uncovered in the future by the investigative research efforts of the journalist. On the contrary, if the manager observes an article that merely repeats statements issued by the manager itself (i.e., a mouthpiece-like article), they will not feel pressured.

To test the watchdog role for different degrees of editorial content contained in an article, we utilize the part of the earnings announcement process we have not used until now: the initial earnings announcement press release. In particular, we want to measure the manager's reaction to two distinct types of articles: 1) watchdog-like articles, where the journalist adds a high degree of editorial content to the information provided in the earnings announcement and 2) mouthpiece-like articles, which merely repeat the information provided in the firm's initial earnings announcement. We proceed by computing the similarity between the firm's earnings announcement press release and the news article covering it to construct a measure for the degree of editorial content contained in an article. We use the cosine similarity, an often utilized measure to compute the similarity between sparse vectors of words, to measure the degree of similarity between the press release and the news article ranging from 0 (highly dissimilar) to 1 (highly similar). First, we remove stop words from both the earnings announcement and the article text and apply stemming. We then regress the cosine similarity on the log length of the article and the log length of the press release and use the residuals of this regression to avoid that variation in cosine similarity between two document-pairs is driven by document length (see the proof provided in Brown and Tucker, 2011). We then multiply the residuals by minus one so that a higher value corresponds to higher dissimilarity, as this is ultimately what we are interested in, and denote this variable as $EditorialContent$. We then perform a median split on $EditorialContent$ so that one group resembles mouthpiece-like articles (low $EditorialContent$) and one group the watchdog-like articles (high $EditorialContent$).[6]

There is one caveat in including $EditorialContent$ into the regression models: We can only compute the cosine similarity between WSJ article and press release for those observations that have received coverage. As most of our sample firms did not receive WSJ coverage, we only have 197 out of the 2,627 observations for which we can compute $EditorialContent$. Therefore, this restricts us in how we can tackle this question from an empirical standpoint. Consequently, to provide robust results while coping with this limitation, we provide two different model specifications.

First, we conduct a standard sample split where we use all observations for which we can compute $EditorialContent$ and then split the sample based on the top and bottom half of $EditorialContent$. We then estimate model (2) for both subsamples. While this is the most common approach to combining a DID analysis with a sample split (e.g., Cookson, 2018; Bruhn and Love, 2014), it effectively reduces our number of observations to only around 150 observations per sample, which may hinder the detection of effects due to limited statistical power. Therefore, we conduct a second sample split in which we utilize all control observations, greatly increasing the sample size of our statistical tests. Specifically, we take the treated observations from the bottom and the top half based on the median split on $EditorialContent$, and add them to the sample of all control observations, respectively. We then estimate model (2) for both of the samples. To be in line with our arguments, we expect to find a decrease in obfuscation for articles with high $EditorialContent$ (i.e., watchdog-like articles), while we do not expect to find a manager reaction to articles that just repeat firm communication (i.e., mouthpiece-like articles). Results of our two analyses are presented in Table 4.

[Table 4 about here]

[6] We find unchanged results when using a tercile split of $EditorialContent$, similar to our analyses in Chapter 3.3.

Our results are consistent across both specifications. We find that for an exogenous increase in media coverage there is a statistically significant decrease in obfuscation for the subsample of articles with high $EditorialContent$ (watchdog-like articles) while we find no significant manager reaction for the subsample of articles with low $EditorialContent$ (mouthpiece-like articles). Specifically, based on the specification (2) which uses all control observations, for the subsample of watchdog-like articles, an exogenous increase in media coverage leads to a 0.52 standard deviation decrease in obfuscation. This effect is substantially larger than the one reported in our baseline analysis in Section 3.1, suggesting that the aggregate ATT is diluted by mouthpiece-like articles.[7] Overall, this finding is intuitive as the manager will only fear that their manipulation attempts are uncovered, and therefore the incentives for manipulation are reduced, if the journalist goes beyond repeating the narratives provided by the firm itself.

3.3. Treatment Effect Heterogeneity: Relationship with Alternative Monitoring Mechanisms

In this section, we conduct a second test for treatment effect heterogeneity where we dive into the cross-section of firms and investigate whether the watchdog role of the media is equally important to all firms. A-priori it is reasonable to assume that this is not the case. Instead, financial news media may act either as 1) a substitute for or 2) a complement to other corporate governance mechanisms. 1) According to the substitution hypothesis, firms that are already in the spotlight and receive extensive monitoring through other mechanisms may behave as desired and refrain from soft manipulation even in the absence of media coverage. In contrast, for firms that typically operate under the radar and face limited monitoring, a sudden increase in media attention can significantly influence managerial misbehavior. 2) If financial media acts as a complement to other monitoring mechanisms, a sudden spike in media attention will have the strongest impact on firms already under intense monitoring through other governance channels. This may be e.g. the case for analyst monitoring, where the media may disseminate critical analyst opinions about a firm to the wider market. To analyze how obfuscation changes

[7] This is even more pronounced based on specification (1), where an exogenous increase in media coverage leads to a 1.57 standard deviation decrease in obfuscation. The smaller statistical significance for specification (1) compared to (2) may be driven by smaller sample size.

in response to an exogenous increase in media coverage when there are low/high levels of monitoring through other corporate governance mechanisms, we use financial analysts which have been shown to be one of the most important mechanisms to identify corporate fraud (Dyck et al. 2010). We then tackle this question by estimating models for two different subsamples: one comprising firms that receive low levels of monitoring and the other consisting of firms with high levels of monitoring by financial analysts.

To measure the monitoring intensity of financial analysts, a simple approach would be to count the firm-specific analyst following, i.e., the number of analysts that provide earnings predictions for a firm in a given quarter. However, a split based on *raw* analyst following is not feasible for two reasons. First, some firms, e.g., very large ones, tend to attract more analysts, which is not necessarily associated with more intensive monitoring because they are often more complex or operate in more opaque environments. Conversely, smaller firms may have simpler and more transparent business models, requiring fewer analysts for a similar level of monitoring. Thus, a high number of analyst following does not automatically translate into more intense or meaningful monitoring but may just be driven by business complexity. Second, as analyst following and media coverage are generally highly correlated, a sample split based on raw analyst following would leave the low analyst following sample with near-to-no treated observations and there would be not enough power to detect any possible effect. To address both issues, we follow the seminal work of Yu (2008), and instead use the abnormal analyst following of a firm. Specifically, we calculate abnormal analyst following by regressing firm log analyst following in a given year-quarter on the same set of control variables as in our main regressions to control for business complexity. We then use the residuals of this regression as abnormal analyst following. This measure (i) controls for size- and complexity-related effects in raw analyst following and (ii) ensures sufficient variation of the treatment indicator within both subsamples.

However, to ensure that our results are not driven by the specific computation of our measure for abnormal analyst monitoring, we construct two alternative measures of analyst monitoring based on the previous earnings call (t-1) held by each firm. First, we compute the number of questions posed by the analysts in the Q&A section. Second, we compute the number of words spoken in the Q&A section of the call. Both measures are proxies for the due diligence and monitoring efforts by financial analysts (Chen et al., 2018; Jung et al., 2018). We lag both variables one quarter so that for each firm-quarter observation we measure the amount of monitoring in the preceding earnings call.

For all three measures of analyst monitoring, we split our sample into two subsamples based on low (bottom tercile) and high (top tercile) analyst monitoring.[8] We then estimate model (2) for both of the subsamples. We present results of these analyses in Table 5.

[Table 5 – about here]

In line with the substitution hypothesis, we find that for firms that have a high level of monitoring, an exogenous increase in media coverage does not influence the managers' tendency to obfuscate, because those firms are very much in the spotlight anyway and receive large amounts of monitoring through financial analysts. Contrary, for the sample of firms who receive low amounts of monitoring through financial analysts, we find that an exogenous increase in media coverage leads to a statistically significant decrease in obfuscation. This finding is robust across all different measures of analyst monitoring. In particular, we find that an exogenous increase in media news coverage for the subsample of low abnormal analyst following (number of questions by analysts) [total words in the Q&A part] leads to a 0.32 (0.24) [0.20] standard deviation decrease in obfuscation, which is all substantially larger than the ATT reported in our baseline analysis in Section 3.1 for the pooled sample. Our findings are in line with the argument that financial news media acts as a substitute for other monitoring mechanisms because an exogenous increase in media coverage is most important to those firms with only little attention from financial analysts.

[8] Results are qualitatively unchanged when using other splits (e.g., quartile or quintile).

4. Robustness Checks

4.1. Time Span Between Earnings Announcement Press Release and Call

One potential concern about our empirical design is that sometimes the time between the earnings announcement and the earnings call is small, which makes it less likely that the manager will be able to read an article and think about how to respond to it in a limited time frame. In our main analyses in Chapter 3, we did not impose any minimum time requirement between the announcement and call, because it is unclear where the cutoff for a sensible time requirement lies. However, as a robustness check we analyze how our results change when we consider observations with different time spans between earnings announcement and call. In line with our story and proposed timeline (see Table 1), we expect that our main results in Section 3.1 are primarily driven by observations where there exists a minimum time span between announcement and call. On the contrary, it would be puzzling to find significant effects when the time span between announcement and call is very small while finding no significant relationship for those calls where the time span is rather large. Note that this does not interfere with our previous analyses regarding $EditorialContent$, as much of the article is already prepared in advance of the earnings announcement (Guest 2021).

To test this, we take the extreme observations in the bottom quintile of time span (which is roughly half an hour, 33 minutes to be precise) between announcement and call and re-estimate model (2) for this subset and the remaining sample.[9] Results are presented in Table 6.

[Table 6 – about here]

In line with our arguments above, we find that for those observations where there is only a small time span between earnings announcement and call, an exogenous increase in media coverage does not lead to a significant decrease in obfuscation, perhaps because the manager did not have enough time to notice the presence of an article and to adopt their behavior based

[9] Note that this does not influence the share of treated observations in both subsamples as the WSJ prepares those articles largely before the earnings announcement.

on the article. Instead, our main results in Section 3.1 are driven by observations where there is indeed enough time for the manager to read the article and think about how they will explicitly or implicitly address the article in the following earnings call. Note that this is not only evident from the non-existing statistical significance in the sample with low time difference, which could also be driven by sample size and a lack of power, but also from the lower effect size in the reduced sample (20% reduction in size).

4.2. DID Plausibility Checks

Next, we conduct some tests to verify our model's identifying assumptions of parallel trends and to enhance causal validity. First, we change our baseline model from equation (2) to a dynamic DID specification which allows treatment and control group to differ between each period. By testing for systematic differences between treatment and control group across the pre-event periods, we can gauge the validity of the parallel trends assumption. Specifically, we estimate the following adjusted specification of model (2):

$$Obfuscation_{i,e,t} = \sum_{j \neq -2} \beta_j Treat_{i,e} \cdot 1(t = j) + \boldsymbol{\theta}'\mathbf{Controls_{i,e,t}} + \alpha_{i,e} + \gamma_t + \varepsilon_{i,e,t} \tag{3}$$

where the treatment indicator is interacted with a dummy specifying relative time to treatment. In line with Dyer et al. (2024), we set the reference period to the earliest available quarter before treatment, which is in our case two quarters prior to the event.[10] Results are presented in Figure 2.

[Figure 2 – about here]

We find that there is no significant trend before the restructuring events at the WSJ, but we find a significant drop in Obfuscation after an exogenous increase in coverage, which especially manifests in the second quarter after the event. This is in line with the argument in

[10] Intuitively, this makes more sense than setting the reference period to one quarter before treatment because treatment status is assigned based on the second quarter before treatment. However, as we only observe two pre-periods anyway, there is also no pre-trend when we set the reference period to one quarter before treatment.

Guest (2021) that coverage changes induced by the restructuring events is not always immediate.

Next, we test our setting using pseudo restructuring events, in which the treatment is artificially assigned six months earlier. The results of the pseudo-event tests are provided in Internet Appendix Part A, Table A.3. We do not find a significant coefficient on the interaction of interest, namely $Treat \times Post$, based on pseudo restructuring events two quarters prior to the actual restructuring events at the WSJ. The pseudo-event tests confirm the validity of our identification strategy as we do not find a significant difference between treated and control units when there was in fact no treatment (i.e., no exogenous change in coverage).

5. Conclusion

We examine how press coverage in financial news media influences the degree to which managers attempt to manipulate via corporate communication. Ex-ante, the direction of this effect is ambiguous: Related empirical research suggests that for hard forms of manipulation, such as accounting fraud, the media acts as a watchdog, with increased coverage reducing manipulation. While there exists no empirical research examining the role of the media for soft forms of manipulation—for example, obfuscation in corporate communication—recent theoretical work by *GMS* argues in this case the media's role may be reversed: The media could act as a mouthpiece and spread the manipulated signals of the firm which increases the incentives for the firm to manipulate if media coverage about the firm increases.

To measure manipulation in corporate communication and thereby capture soft manipulation, we isolate the portion of complexity in managerial speech during an earnings conference call that cannot be attributed to information-driven complexity following Bushee et al. (2018). To establish a causal link between media coverage and obfuscation, we employ a difference-in-differences design based on restructuring events at the WSJ. Using a stacked difference-in-differences estimation in combination with the corrective weights of Wing et al. (2024), we mitigate the recently discussed bias inherent to staggered DID designs estimated via two-way fixed effects.

We find that an exogenous increase in media coverage leads to a significant decrease in obfuscation indicating that news media take on a watchdog role even for soft manipulation. This is in line with related literature on more severe forms of manipulation such as accounting fraud but contradicts recent theoretical work by *GMS*.

Furthermore, we conduct detailed analyses regarding effect heterogeneity. First, we show that the watchdog role of the media is centered around articles with a high degree of editorial content. This is in line with the argument that the probability of manipulation being uncovered––and therefore making manipulation less incentivized—drastically increases when the journalist produces articles with greater editorial depth (*watchdog-like articles*). For articles that just repeat the earnings announcements (*mouthpiece-like articles*) of the firm, we find no impact on the degree of manipulation by managers. Second, diving into the cross-section of firms, we find that the watchdog role of the media is most important for firms that are less monitored through other corporate governance mechanisms, as proxied by financial analysts which was shown to be the most important governance mechanism besides the press. Firms that already receive high amounts of monitoring through other channels do not react to sudden media attention. This finding is robust across different measures of analyst monitoring.

Our findings have important implications for financial journalism and firm behavior. Our study indicates that financial news media serve as crucial monitors not only for severe forms of manipulation, such as accounting fraud, but also in promoting more transparent corporate communication, strengthening the role of the media as a corporate governance mechanism. Moreover, our results emphasize the importance of high-quality journalism, as managers only adjust their behavior when faced with in-depth reporting, while low-effort articles fail to exert any meaningful pressure. Finally, the media's role is especially important for firms that do not receive much monitoring through other corporate governance channels.

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Table 1: Timing of the Steps in the Earnings Announcement Process

This table presents the timing of the steps in the earnings announcement process. We have data on steps 1, 2, and 4. We are unable to observe step 3. We provide anecdotal evidence on step 3 in Internet Appendix Part A, Table A.1.

	Step	More Detail	Timing
1	Firm publishes quarterly earnings announcement press release (Form 8-K, Exhibit 99)	The quarterly earnings announcements are regularly published as a press release on the firm's website and reported to the SEC in the Form 8-K, Exhibit 99.	t
2	WSJ [Online] decides whether to write a newspaper article about the earnings announcement	The WSJ [Online] may or may not cover the firm's earnings announcement depending on perceived newsworthiness and potentially other factors.	$\mathrm{t} + \mathrm{x}$ hours
3	*Manager observes the article's presence or absence and its content*	*Management sees whether an article was published and, if so, whether it contains substantial amounts of editorial content beyond the information in the firm's earnings announcement (high diff) or not (low diff).*	$\mathrm{t} + \mathrm{x} + \mathrm{y}$ hours
4	Earnings conference call; manager decides on possible manipulation in preparation	During the call, management may choose to manipulate qualitative information, e.g. by obfuscating their communication. The manipulation decision may be dependent on the manager's observation of media coverage.	$\mathrm{t} + \mathrm{x} + \mathrm{y} + \mathrm{z}$ hours (where $\mathrm{x} + \mathrm{y} + \mathrm{z} \leq 48$ hours)

Table 2: Summary Statistics

This table presents summary statistics for all variables used in the regression to estimate our measure of obfuscation, as well as those used in our main analysis from Section 3. The variables tot_assets and $income\_before$ are measured in billions of dollars. Summary statistics are based on our final sample comprised of 2,675 firm-quarter observations.

	Mean	**SD**	**Min**	**Q25**	**Median**	**Q75**	**Max**
Obfuscation	0.08	1.37	-6.40	-0.78	0.11	0.95	9.39
Fog_Presentation	13.94	1.40	7.73	13.08	13.95	14.83	23.32
Fog_Participant	9.02	1.17	5.12	8.19	8.91	9.76	16.89
tot_assets	32.90	101.77	0.08	3.56	8.73	23.89	2,020.97
income_before	0.29	0.80	-2.41	0.04	0.11	0.27	11.66
n_analyst	15.25	7.44	1.00	10.00	15.00	20.00	45.00
bm_ratio (%)	44.34	35.78	0.32	23.52	35.11	56.83	609.18
Leverage (%)	26.20	16.59	0.00	13.12	24.50	36.61	102.35
fell_short	0.28	0.45	0.00	0.00	0.00	1.00	1.00
RI_1_year	0.21	0.35	-0.84	0.02	0.19	0.36	3.34
P_std_260_days	0.02	0.01	0.01	0.01	0.02	0.02	0.07

Table 3: Manager Obfuscation after an Exogenous Change in News Coverage

This table presents our estimation of the average treatment effect on the treated (ATT) based on the stacked DID regression model from equation (2). Obfuscation is estimated as the residual from a regression of the Fog Index of manager speech in the presentation portion of the earnings conference call on the complexity of the analyst speech in the same call and controls. Treatment firms are firms for which the WSJ coverage changed in the expected direction after the given event, i.e. a decrease in coverage for event 1 and an increase in coverage for events 2 and 3. Following Gad et al. (2024), we set the treatment indicator to -1 for firms that were affected by the coverage-decreasing event 1, and to +1 for firms that were affected by the coverage-increasing events 2 and 3. Control firms are those firms that had the same coverage-status before the event as the treatment firms (i.e., coverage before the first event; no coverage before the second and third events), but for which coverage did not change because of the restructuring event. Control variables are defined in Section 2.2. t-statistics are in parentheses and estimated using standard errors clustered on the firm-level. * denotes statistical significance at the 10% level.

Dependent Variable	Obfuscation
$Treat \times Post$	-0.11* (-1.91)
Controls	Yes
Firm × event FE	Yes
Year-quarter FE	Yes
Observations	2,498
Adjusted R^2	0.76

Table 4: Impact of Editorial Content in News Coverage on Manager Obfuscation

This table presents our estimation of the average treatment effect on the treated (ATT) based on the stacked DID regression model from equation (2), where we perform median splits by the variable $EditorialContent$. $EditorialContent$ is the cosine difference between the firm's earnings announcement and the associated article covering that announcement in the WSJ, as defined in Section 3.2. In column (1), we perform a median split of $EditorialContent$, after removing all observations for which the variable could not be computed, i.e. observations without a WSJ news article. In column (2), we take the treated observations from the bottom and the top half of the distribution of $EditorialContent$, and add them to the sample of all control observations, respectively. Obfuscation is estimated as the residual from a regression of the Fog Index of manager speech in the presentation portion of the earnings conference call on the complexity of the analyst speech in the same call and controls. Treatment firms are firms for which the WSJ coverage changed in the expected direction after the given event, i.e. a decrease in coverage for event 1 and an increase in coverage for events 2 and 3. Following Gad et al. (2024), we set the treatment indicator to -1 for firms that were affected by the coverage-decreasing event 1, and to +1 for firms that were affected by the coverage-increasing events 2 and 3. Control firms are those firms that had the same coverage-status before the event as the treatment firms (i.e., coverage before the first event; no coverage before the second and third events), but for which coverage did not change because of the restructuring event. Control variables are defined in Section 2.2. t-statistics are in parentheses and estimated using standard errors clustered on the firm-level. * and ** denote statistical significance at the 10% level and 5% level, respectively.

Dependent Variable	Obfuscation			
	(1)		(2)	
	Low EditorialContent	*High EditorialContent*	*Low EditorialContent*	*High EditorialContent*
$Treat \times Post$	-0.15 (-0.47)	-1.57* (-1.86)	0.07 (0.46)	-0.52** (-2.19)
Controls	Yes	Yes	Yes	Yes
Firm × event FE	Yes	Yes	Yes	Yes
Year-quarter FE	Yes	Yes	Yes	Yes
Observations	150	147	2,113	2,115
Adjusted R^2	0.75	0.80	0.76	0.77

Table 5: Impact of News Coverage on Manager Obfuscation in the Presence of Alternative Monitoring Mechanisms

This table presents our estimation of the average treatment effect on the treated (ATT) based on the stacked DID regression model from equation (2), where we perform tercile splits, forming subsamples one with low (bottom tercile) and one with high (top tercile) analyst monitoring. The tercile splits are done by the variables $Abnormal\ Analyst\ Following$, $Number\ of\ Questions$, and $Total\ Words$, as defined in Section 3.3. In specification (1) we measure analyst monitoring by the number of abnormal analyst following of a firm, in specification (2) by the number of questions posed by analysts in the previous earnings call, and in specification (3) by the number of total words spoken in the Q&A part of the previous earnings call. Obfuscation is estimated as the residual from a regression of the Fog Index of manager speech in the presentation portion of the earnings conference call on the complexity of the analyst speech in the same call and controls. Treatment firms are firms for which the WSJ coverage changed in the expected direction after the given event, i.e. a decrease in coverage for event 1 and an increase in coverage for events 2 and 3. Following Gad et al. (2024), we set the treatment indicator to -1 for firms that were affected by the coverage-decreasing event 1, and to +1 for firms that were affected by the coverage-increasing events 2 and 3. Control firms are those firms that had the same coverage-status before the event as the treatment firms (i.e., coverage before the first event; no coverage before the second and third events), but for which coverage did not change because of the restructuring event. Control variables are defined in Section 2.2. t-statistics are in parentheses and estimated using standard errors clustered on the firm-level. *, ** and *** denote statistical significance at the 10%, 5% and 1% level, respectively.

Dependent Variable	Obfuscation					
	(1)		(2)		(3)	
	Low Abnormal Analyst Following	*High Abnormal Analyst Following*	*Low Number of Questions*	*High Number of Questions*	*Low Total Words*	*High Total Words*
$Treat \times Post$	-0.32*** (-2.81)	-0.04 (-0.38)	-0.24** (-2.33)	-0.08 (-0.56)	-0.20* (-1.84)	-0.14 (-1.38)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm × event FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	825	849	852	879	826	849
Adjusted R^2	0.79	0.73	0.79	0.75	0.80	0.75

Table 6: Media Coverage and Obfuscation with Minimal Time Span Between Announcement and Call

This table presents our estimation of the average treatment effect on the treated (ATT) based on the stacked DID regression model from equation (2), where we split the sample by the time span between earnings announcement press release and earnings call. (1) includes only the observations with the bottom quintile of time span and (2) includes the remainder of the sample. Obfuscation is estimated as the residual from a regression of the Fog Index of manager speech in the presentation portion of the earnings conference call on the complexity of the analyst speech in the same call and controls. Treatment firms are firms for which the WSJ coverage changed in the expected direction after the given event, i.e. a decrease in coverage for event 1 and an increase in coverage for events 2 and 3. Following Gad et al. (2024), we set the treatment indicator to -1 for firms that were affected by the coverage-decreasing event 1, and to +1 for firms that were affected by the coverage-increasing events 2 and 3. Control firms are those firms that had the same coverage-status before the event as the treatment firms (i.e., coverage before the first event; no coverage before the second and third events), but for which coverage did not change because of the restructuring event. Control variables are defined in Section 2.2. t-statistics are in parentheses and estimated using standard errors clustered on the firm-level. ** denotes statistical significance at the 5% level.

Dependent Variable	*Obfuscation*	
	(1)	(2)
	Bottom Quintile Time Span	*Remaining Sample*
$Treat \times Post$	-0.12	-0.15**
	(-0.68)	(-2.08)
Controls	Yes	Yes
Firm × event FE	Yes	Yes
Year-quarter FE	Yes	Yes
Observations	500	1.998
Adjusted R^2	0.76	0.77

Figure 1: Coverage Around WSJ Restructuring Events

This figure shows the number of all earnings-related articles in the WSJ per quarter around the three restructuring events. For our empirical identification, we then match those articles that were published right in between an earnings announcement and an earnings call.

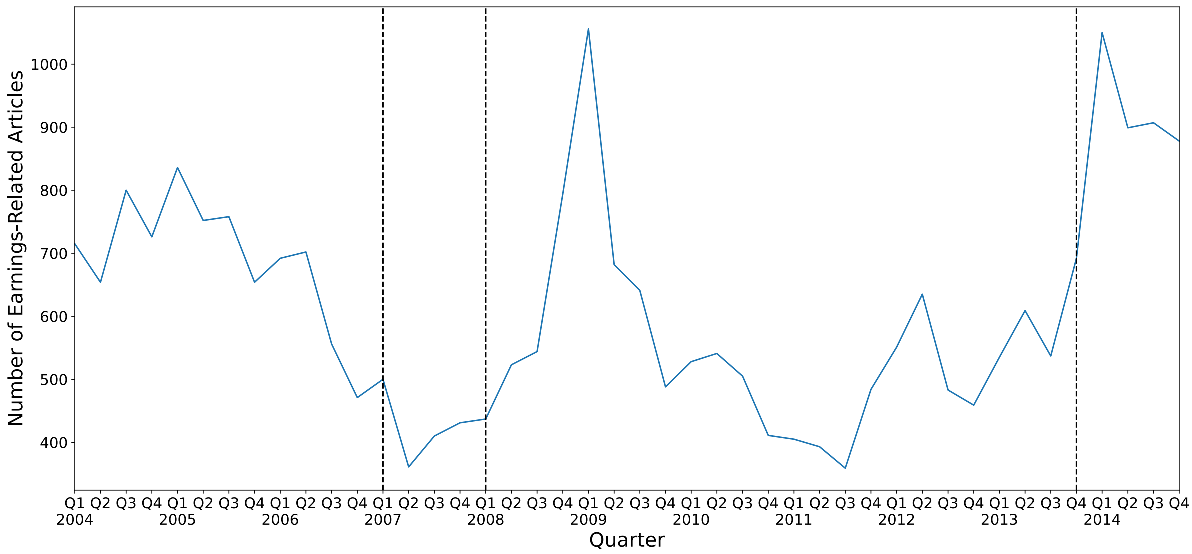
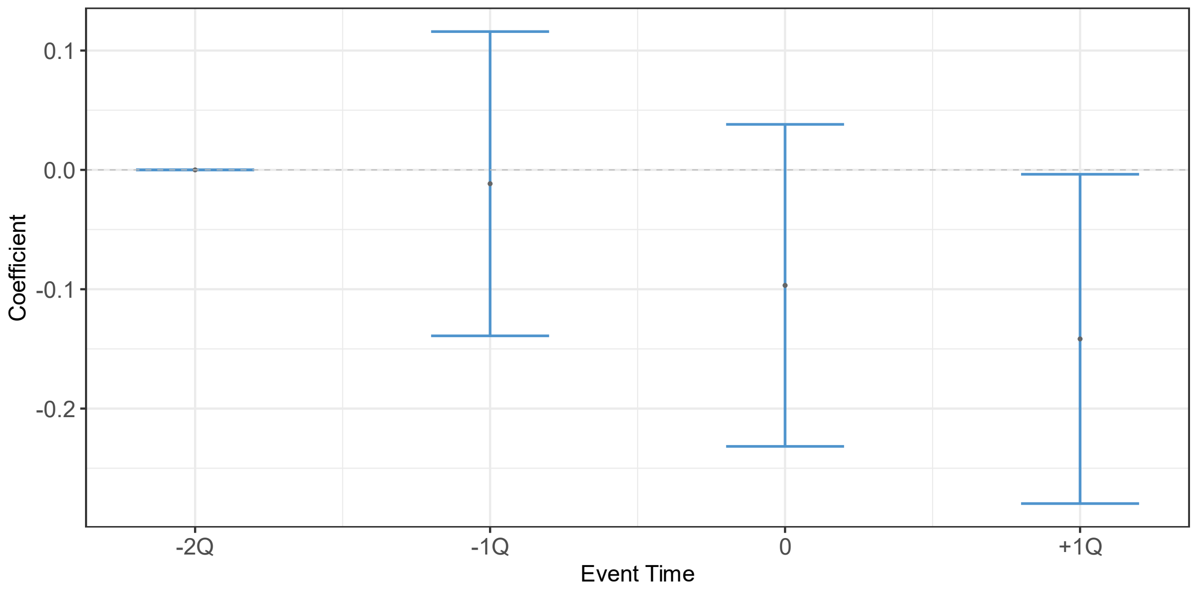


Figure 2: Dynamic Difference-in-Differences Specification

This figure presents our main results based on a dynamic specification of the DID model as in equation (3). Confidence intervals (90%) are based on standard errors clustered at the firm-level.



Internet Appendix to "Watchdog or Mouthpiece? The Role of Financial News Media in Corporate Communication"

Internet Appendix A: Tables

Table A.1: Anecdotal Evidence on Managers' Reactions to WSJ Mentions in Earnings Calls

This table provides anecdotal evidence of how managers react and sometimes even directly respond to mentions of their firm in Wall Street Journal coverage. All excerpts are from the presentation portion of the earnings conference call.

Call	Excerpt
Activision Blizzard Q1 2018	"Before we start the call, we want to share with you an extraordinary error that was made by The Wall Street Journal earlier today, which caused trading volatility in our stock. The Wall Street Journal made a mistake and prematurely released a completely inaccurate headline reporting our Q1 revenues as \$1.7 billion instead of our actual Q1 2018 GAAP revenues of a record \$1.97 billion. Not only did they report inaccurately, they did so in violation of our written embargo agreement. They have since issued an apology."
Kansas City Southern Q2 2017	"If you haven't already seen it, there's a great opinion piece in today's Wall Street Journal, with which we agree wholeheartedly. I'll come back to that in my concluding comments."
ProLogis Q1 2008	"Like many of you, I read the Wall Street Journal and Financial Times online in the morning; and frankly, the headlines are pretty frightening. But then I talk to our people in Asia and I feel a bit better. Then I talk to our team in Europe and I feel even better. Then I talk to our North American team, and I hear that things are still relatively stable. So by mid-morning, I feel pretty good about the business. Then I get up the next morning, read the papers, and the cycle starts all over again."
Tribune Q4 2006	"Also, you may have seen the article in the Wall Street Journal this morning, our competitive position has been enhanced versus NewsCorps, my network TV affiliates, the former UPN affiliates."
Chubb Q3 2002	"I'd like to clarify a statement attributed to Chubb in today's Wall Street Journal, regarding risks across different parts of our businesses. The Journal article might be read as overstating our current capability."
Comcast Q1 2002	"I would refer everybody to the front page Wall Street Journal article this morning that just highlights how QVC continues to attract new -- new buyers, and also people who want to get their products onto QVC."

Table A.2: Decomposition of Managers' Linguistic Complexity into Information and Obfuscation Components

This table shows OLS estimates from regressing $FogManager$ on $FogAnalyst$ and other control variables potentially related to business complexity. Unit of observation is firm-quarters. t-statistics are given in parentheses. *** and ** denote statistical significance at the 1% and 5% level, respectively. Residuals of this regression constitute our measure for manager obfuscation.

Variable	Fog Manager
Fog Analyst	0.254*** (45.71)
tot_assets	0.000 (0.51)
income_before	-0.062*** (-9.56)
n_analyst	-0.005*** (-5.08)
bm_ratio	0.001*** (8.14)
leverage	0.003*** (6.18)
fell_short	0.040** (2.45)
RI_1_year	0.004 (0.25)
P_std_260_days	9.364*** (14.81)
eps_actual	0.030*** (9.35)
Observations	42,635
Adjusted R^2	0.06

Table A.3: Placebo Test

This table presents results for the placebo test of our main analysis. We shift the WSJ restructuring events to two quarters prior to the actual events. Variable definitions remain the same. t-statistics are in parentheses and estimated by using standard errors clustered on the firm-level.

Variable	Obfuscation
$Treat \times Post$	0.08 (0.82)
Controls	Yes
Firm × event FE	Yes
Year-Quarter FE	Yes
Observations	2.443
Adjusted R^2	0.70

Internet Appendix B: Cleaning Earnings Announcement Press Releases

We clean the earnings announcement press releases in line with the procedure in Li (2008), Miller (2010), Dyer et al. (2017) and Guest (2021):

- To ensure we focus only on earnings-related filings, we filter on those filings classified under Item 2.02 or Item 12, which denote results of operations and financial conditions.
- We use regular expressions to only keep the Exhibit 99 part of the filing. If a firm files multiple Exhibit 99s with one 8-K, we only keep the first Exhibit 99 as this is always the earnings press release.
- We keep only those Exhibit 99s that indeed include an earnings-related press release by identifying whether one of the terms “press release”, “news release”, “media release”, “immediate release”, or some contact information is present in the Exhibit, which would be the case for press releases.
- Then, we further clean the press releases before comparing them to news articles. Specifically, we remove all HTML text and remaining tags (e.g., <TEXT>, <DOCUMENT>). We remove all tables unless they contain more than 75% alphabetic characters because table tags are sometimes used to format text. We delete lines with fewer than 20 characters or 15 alphanumeric characters to remove section headings and lines of just numbers.

Internet Appendix C: Operationalization of an Implicit Firm Newsworthiness Ranking by the WSJ

Our parallel trends assumption holds if there exists an implicit firm newsworthiness ranking by the WSJ which remains stable from pre-period to post-period for each WSJ restructuring event. We operationalize this concept of an implicit firm newsworthiness ranking for empirical testing. Subsequently, we show that the newsworthiness ranking is indeed very persistent over time.

Our empirical setting generally prohibits us from computing a meaningful newsworthiness ranking explicitly regarding earnings-related articles, as we only identify the one article directly related to the firm's earnings announcement. Instead, we use all articles (including non-earnings-related articles) published in the WSJ and WSJO filtered for our S&P 500 sample firms. The underlying assumption is that coverage decisions for earnings-related news articles versus non-earnings-related news articles follow a similar latent concept of an implicit newsworthiness ranking (but not necessarily the same ranking). The specific factors influencing the coverage decisions of earnings-related articles versus non-earnings-related articles may of course differ; it must only hold that the concept is the same within the WSJ.

For each year-quarter, we thus compute the total number of articles that mention a given firm. We then form a newsworthiness ranking of all firms ranked by total articles that mention the firm in the given year-quarter. For each pair of adjacent year-quarters, we then compute Spearman's rank correlation coefficient for the pairs of newsworthiness rankings. Spearman's rank coefficient can generally range from -1 to +1, where +1 represents a perfectly similar rank order, and -1 represents a perfectly opposed rank order. We find that the Spearman correlation is consistently high. In our sample timeframe around the WSJ restructuring events, we observe an average Spearman correlation of 0.73. If we consider all quarters observable to us from 2000Q1 until 2023Q3, we observe a similar average Spearman correlation of 0.73. We conclude that while the newsworthiness ranking is not perfectly stable as would be indicated by a value of 1, it still is highly stable in general and in our sample period. This supports the notion that a stable implicit newsworthiness ranking exists, which strengthens our parallel trends assumption.