ANALYST COVERAGE AND MANAGERS' DISCLOSURE OF FORWARD-LOOKING INFORMATION

by

JAMES WARREN

(Under the Direction of Stephen Baginski)

ABSTRACT

Prior research documents that managers respond to an exogenous decrease in analyst coverage by increasing the quantity of earnings forecasts, presumably to fill the information void left by a reduction in coverage. I extend this research by considering management forecast quality and an alternative form of guidance, managers' forward-looking textual disclosures. First, although forecast quantity increases after loss of coverage and liquidity partially improves, I find forecast quality decreases (i.e., larger signed and unsigned errors) and the decrease in quality attenuates the improvement in liquidity. These results suggest analysts not only play an informational role, but also a monitoring role with respect to managers' forward-looking disclosures. These findings are more pronounced when other monitors are less present and when managers have insider selling incentives to engage in this disclosure behavior. Second, with respect to textual disclosures, managers' quantity and tone of forward-looking statements increase following loss of coverage. Although these results do not vary with the presence of other monitors, the increase in positive tone is concentrated in managers who engage in insider selling. Overall, my study provides a more nuanced view of analysts' role in influencing managers' forward-looking disclosures.

INDEX WORDS: Analyst coverage, Management forecasts, Forward-looking disclosure

ANALYST COVERAGE AND MANAGERS' DISCLOSURE OF FORWARD-LOOKING INFORMATION

by

JAMES WARREN

Bachelor of Science, Texas A&M University, 2014

Master of Finance, Texas A&M University, 2014

A Dissertation Submitted to the Graduate Faculty of The University of Georgia in Partial Fulfillment of the Requirements for the Degree

DOCTOR OF PHILOSOPHY

ATHENS, GEORGIA

2021

ANALYST COVERAGE AND MANAGERS' DISCLOSURE OF FORWARD-LOOKING INFORMATION

by

JAMES WARREN

Major Professor: Stephen Baginski

Committee: John Campbell
Frank Heflin
Santhosh Ramalingegowda

Electronic Version Approved:

Ron Walcott
Dean of the Graduate School
The University of Georgia
May 2021

TABLE OF CONTENTS

Page

LIST OF TABLES

Page

LIST OF FIGURES

Page

CHAPTER 1

INTRODUCTION

A long-debated question in the disclosure literature is: What influence do professional analysts have on managers' disclosure quality? Some studies contend that analysts serve to monitor managers, suggesting disclosure quality will deteriorate if analyst coverage decreases because managers feel less pressure to "tell the truth." However, others argue that analysts serve an information intermediary role, suggesting managers will respond to losing analyst coverage by increasing the quality of their firm's disclosures to substitute for the lost information source. Managers make many disclosure decisions that may be influenced by analysts' monitoring and information intermediary roles, including disclosures about both actual earnings and forward-looking information. In this paper, I focus on how professional analysts influence disclosure of *forward-looking information*.

Extant literature on the relation between analyst following and forward-looking disclosure primarily focuses on quantitative management forecasts. The evidence is mixed, with the endogeneity of analyst following and voluntary disclosure complicating the investigation. Importantly, both Anantharaman and Zhang (2011) and Balakrishnan et al. (2014) help resolve the endogeneity issue. They exploit brokerage house mergers and closures to identify plausibly exogenous reductions in analyst coverage, and provide convincing evidence that managers respond to a reduction in coverage by increasing the *quantity* of management earnings forecasts. Further, Balakrishnan et al. (2014) show that these guiding firms are able to partially reverse the negative liquidity shock that occurs from losing coverage. This evidence is consistent with analysts playing

an information intermediary role, as managers seemingly respond to losing coverage by "filling the informational gap" with their own forecasts. At first glance, this evidence appears inconsistent with analysts playing a monitoring role with respect to forward-looking information because managers use guidance to successfully recover their firm's liquidity. However, the increase in liquidity is partial, suggesting that, after losing analyst coverage, managers might issue forecasts that are of lower quality than the forecasts they issued before losing coverage (possibly for opportunistic reasons), which would be evidence consistent with analysts' monitoring role.

An assumption underlying much disclosure theory is that disclosure must be truthful (e.g., Jorgensen and Kirschenheiter 2012; Dye and Sridhar 1995; Jung and Kwon 1988). Under this purview, the act of disclosure (or propensity to disclose more frequently) equates to higher quality disclosure. However, it is possible disclosure quantity and quality move in opposite directions. For example, Hribar and Yang (2016) provide empirical evidence that overconfident CEOs are more likely to forecast, but their forecasts are lower quality (i.e., overly optimistic). In addition, Billings, Cedergren, and Dube (2016) find that subsequent to litigation, managers reduce overall forecasting activity, but forecasts that are provided are higher quality (i.e., more timely bad news, less timely good news). Further, the idea that disclosure quantity and quality can have separate effects on liquidity is not new. Easley and O'hara (2004) develop a model that explains the effects of information on firms' cost of capital. They show theoretically that higher disclosure quantity, and separately, higher disclosure quality decrease the cost of capital (and thus increase liquidity). My goal is to examine directly whether a loss of analyst coverage leads to a decrease in disclosure quality, and if that tempers the beneficial liquidity effects of increases in disclosure quantity.

I provide evidence on this issue in two phases. In the first phase, I focus on management earnings forecasts. I replicate prior work on the effects of analyst coverage loss on the *quantity* of

earnings forecasts and then extend the literature by examining whether a reduction in coverage influences the *quality* of earnings forecasts. In the second phase, I extend the literature to qualitative forward-looking information by examining whether loss of coverage affects the quantity and quality of managers' *textual disclosures*. Recent evidence suggests an important role of textual forward-looking information in security pricing (e.g., Bozanic et al. 2018) and only sampling forecasts has been long recognized as a limitation (e.g., Skinner 1994). Although the purpose of both numeric forecasts and forward-looking textual disclosures is to influence the market's perceptions of the future, it is unclear whether language, which is difficult to verify ex post (i.e., "cheap talk"), is monitored by analysts to the same extent as numeric forecasts.

In both phases of my study, I employ a staggered difference-in-differences design in which I measure the change in disclosure following an exogenous decrease in analyst following, relative to a matched sample of firms that experience no such shock. I follow prior research and use brokerage house mergers and closures to identify plausibly exogenous reductions in analyst coverage. I begin by replicating both Anantharaman and Zhang (2011) and Balakrishnan et al.'s (2014) finding that forecast frequency increases following the exogenous loss of coverage. In the first phase dealing with management forecasts, I examine how two dimensions of forecast quality change following this event: (1) unsigned forecast error (i.e., forecast accuracy) and (2) signed forecast error (i.e., forecast bias). I find management forecast accuracy decreases following an exogenous loss of coverage. Second, I examine if the decrease in forecast accuracy is driven by managers becoming either consistently more optimistically or pessimistically biased. I find that forecasts are more optimistically biased after a reduction in analyst coverage. In untabulated analysis, I find these results are concentrated in firms with below median pre-shock analyst coverage, consistent with losing an analyst being more impactful to a firm with already thin

coverage. Overall, this evidence suggests that although managers increase the quantity of earnings forecasts following a reduction in analyst coverage, they decrease the quality of their forecasts.

If this behavior is intentional, I expect the results to vary with (1) monitoring from other sources and (2) managers' incentives. With respect to monitoring, I expect these results to be stronger when other monitors cannot substitute for the lost monitoring from the departing analyst. Accordingly, I partition my sample on two other monitoring mechanisms: (1) monitoring institutions (e.g., Ayers, Ramalingegowda, and Yeung 2011) and (2) abnormal audit fees, because auditors encourage accurate and forthcoming managerial forecasts (e.g., Ball, Jayaraman, and Shivakumar 2012). I find forecast accuracy (bias) decreases (increases) more for firms with less monitoring from these two sources. With respect to incentives, prior research suggests managers use disclosure to increase insider trading gains (e.g., Cheng, Luo, and Yue 2013; Roh and Zarowin 2019). Accordingly, I partition my sample on whether the manager engages in opportunistic insider selling. I find some evidence to suggest the decrease in forecast quality is concentrated in firms whose managers engage in opportunistic insider selling.

To gauge the consequences of lower quality forecasts, I replicate the Balakrishnan et al. (2014) finding that the negative liquidity shock from losing an analyst is partially reversed for firms that respond by increasing the provision of guidance. I then provide evidence that the extent to which managers successfully recover their firm's liquidity is attenuated by decreases in forecast quality. This evidence is consistent with disclosure theory suggesting disclosure quantity and disclosure quality should influence liquidity (Easley and O'hara 2004).

In the study's second phase, I turn my attention to textual disclosure. I follow Bozanic et al. (2018) and measure the (1) quantity of earnings-related forward-looking statements and (2) tone of these statements, both measured from earnings announcement press releases. I use earnings

announcements because in recent years, almost 90% of quantitative forecasts are bundled with the earnings announcement, making earnings announcements a primary venue for managers to disclose forward-looking information (e.g., Rogers and Van Buskirk 2013). Further, using earnings announcements allows me to expand my sample for the textual disclosure tests beyond firms that provide quantitative earnings guidance. Consistent with prior research and my replication that management forecast frequency increases following a reduction in analyst coverage, I find the quantity of earnings-related forward-looking statements also increases. Consistent with my finding that earnings forecasts become more optimistic, I document that the net positivity of forward-looking statements increases following a decrease in analyst following. Although statistically significant, these results are economically modest. Further, cross-sectional tests on the presence of other monitors reveal few differences in results across partitions. However, with respect to insider selling, I find the increase in positive tone of forward-looking statements is concentrated in firms whose managers engage in opportunistic insider selling. Finally, I find no evidence that liquidity is affected by the change in quantity or tone of the textual disclosures. Taking the evidence on textual disclosure as a whole, there is only economically modest support for the informational or monitoring role of analysts with respect to qualitative forward-looking disclosure. The contrast of strong evidence of analysts' monitoring role with respect to management forecasts with the economically modest evidence with respect to textual disclosures is consistent with the ex post verifiability of the former and the cheap talk nature of the latter.

Finally, I perform three sets of additional analysis. First, prior studies use this same setting to identify decreases in analyst coverage. Accordingly, I examine the implication of my results for papers most relevant to my study that use this same setting. Second, I examine alternative, *unintentional* reasons why management forecast quality could decrease after losing coverage.

Although an archival study cannot definitely ascribe manager intentionality, I provide circumstantial evidence that managers are likely responding with intentionally low-quality disclosure. Third, I examine two other attributes of management forecasts: precision and news. I find no change in precision following loss of analyst coverage, and some evidence that managers increase the frequency of good news earnings forecasts but not bad news earnings forecasts.

My study makes several contributions. First, I contribute to the management forecast literature. Prior research examines whether analysts affect the *quantity* of management forecasts but does not consider analysts' influence on forecast *quality*. I identify a setting in which forecast quantity and quality move in divergent directions. By doing so, I provide a more nuanced view of how analysts influence disclosures and highlight the importance of examining multiple properties of guidance. Second, I contribute to the broader literature on forward-looking disclosures. I document that managers increase the frequency and tone of forward-looking statements, but these results are economically modest and hardly differ based on other monitor presence. As such, I highlight differences between hard and soft forward-looking information, and how various capital markets participants influence these disclosures. Third, I contribute to the analyst literature. Although many studies address the effect of analyst following on disclosure quality in some form, the evidence is mixed. Some studies document a positive association between voluntary disclosure quality and analyst following, while others find a negative association or even predict and argue for no association. The mixed evidence is likely due to endogeneity issues. However, I employ a plausibly exogenous shock to analyst following and take a step in the direction of causal inference with respect to how analysts influence managers' forward-looking disclosures.

CHAPTER 2

BACKGROUND AND HYPOTHESIS DEVELOPMENT

2.1 Background on Management Guidance

Prior research identifies many factors that motivate managers to issue earnings guidance. For example, management guidance can reduce stock price volatility (Billings et al. 2015), correct market expectations (Ajinkya and Gift 1984), and signal managers' control over the firm (e.g., Trueman 1986). Further, theoretical work suggests guidance can reduce information asymmetry (Verrecchia 2001), increase liquidity (Diamond and Verrecchia 1991), and lower the cost of capital (Leuz and Verrecchia 2000; Easley and O'hara 2004). Empirical work is mostly consistent with these findings. For example, Coller and Yohn (1997) document that management forecasts can reduce information asymmetry, while papers such as Botosan (1997) and Baginski and Hinson (2016) suggest voluntary disclosure can reduce firms' cost of capital.

An assumption underlying much disclosure theory is that disclosure is truthful (e.g., Jorgensen and Kirschenheiter 2012; Dye and Sridhar 1995; Jung and Kwon 1988). Under this purview, the act of disclosure (or more frequent disclosure) equates to higher quality disclosure. However, if the assumption of truthful disclosure is relaxed, the link between disclosure frequency and disclosure quality becomes ambiguous. Notably, extant research suggests managers face incentives to not "tell the truth" with their guidance. In other words, managers have reasons be opportunistic rather than forthcoming with their earnings guidance.

Although managers face incentives not to be forthcoming with their guidance, they are constrained for a few reasons. First, external parties such as institutions monitor managers (e.g.,

Ayers et al. 2011). For example, firms with greater institutional ownership issue more accurate forecasts (Ajinkya et al. 2005). Second, auditors monitor managers. Ball et al. (2012) show that management forecasts are more accurate in the presence of higher excess audit fees. Finally, financial analysts may serve a monitoring role. However, existing evidence regarding financial analysts' influence on managers' earnings forecast quality is mixed.

2.2 Background on Professional Analysts

Two primary roles of financial analysts are advanced in the literature. First, some argue analysts monitor managerial decisions. Specifically, analysts have training, resources, and knowledge of the firms they cover, all of which help them monitor. Jensen and Meckling (1976) advance this view by stating, "One of the groups who seem to play a large role in these [monitoring] activities is composed of security analysts employed by institutional investors, brokers, and investment advisory services." Under this purview, managers are constrained to make decisions that maximize firm value in the presence of more analysts. Extant empirical work suggests analysts do in fact play a monitoring role with respect to mandatory reporting and real decisions. Specifically, analyst coverage reduces accruals earnings management (Yu 2008; Irani and Oesch 2013; Chen et al. 2015) and opportunistic non-GAAP reporting discretion (Christensen et al. 2020). Further, analysts reduce CEOs' likelihood of making a value-destroying acquisition (Chen et al. 2015). In the context of my study, if analysts' primary role is to monitor managers, this will lead to *lower quality* forward-looking disclosure when the firm loses analyst coverage.[1]

[1] The fact that analysts play an effective monitoring role with respect to mandatory reporting and real decisions does not imply they also effectively monitor managers' forward-looking disclosures. Forward-looking information such as management guidance is not verifiable at the time of issuance. As such, when subsequent earnings are released and the manager's forecast is revealed as inaccurate, the manager may defend the inaccuracy by stating the uncertainty inherent in forecasting and that they made their best effort to provide an accurate forecast using the information they had at the time. Further, it is more difficult to identify intentional inaccuracy when uncertainty is higher (Rogers and Stocken 2005).

A second role of analysts is to serve as information intermediaries. Analysts undoubtedly provide information about the firm to the capital markets, and this information has been shown to reduce information asymmetry (e.g., Amiram et al. 2016), cost of capital (Easley and O'hara 2004) and illiquidity (Kelly and Ljungqvist 2012; Derrien and Kecskés 2013). In the context of my study, if analysts' dominant role is to provide information about the firm, managers should respond to losing analyst coverage by increasing (or at least hold constant) the quality of their forward-looking disclosures to substitute for the lost source of information from the departing analyst.

It is unclear from the literature whether analysts' monitoring or information role dominates with respect to forward-looking disclosures. For example, Lang and Lundholm (1996) document a positive association between disclosure quality and analyst following. Further, Ke et al. (2018) report a positive association between analyst following and management forecast accuracy. In contrast, both Yang (2012) and Baginski et al. (2019) document a negative association. Finally, other papers predict and argue for no association (Feng et al. 2009; Ittner and Michels 2017).

This mixed evidence is likely due to a correlated omitted variable problem. First, analysts might tend to cover firms with fewer agency problems, which is likely the same firms willing to disclose more forward-looking information (Chen et al. 2015). Second, investor demand for information could influence analyst coverage (e.g., Frankel et al. 2006), but managers could respond to information demand by providing more forward-looking disclosure (e.g., Healy and Palepu 2001). These correlated omitted variable issues make it difficult to estimate the effect of analyst coverage on disclosure quality. Accordingly, Chen et al. (2015) note "There is a striking paucity of papers that have explicitly tested for the governance role of analysts…likely driven by potential endogeneity concerns." To address this issue, recent papers use brokerage house closures and mergers to identify exogenous reductions in analyst following. Specifically, Kelly and

Ljungqvist (2012) show that in the late 1990's, the economics of equity research began to change, leading to the closure or mergers of many brokerage houses. Because of these closures/mergers, some firms lost analyst following. Importantly, Kelly and Ljungqvist (2012) show that these reductions in analyst coverage are unrelated to firm fundamentals and thus plausibly exogenous.[2]

2.3 Hypothesis Development

Most relevant to my study is Balakrishnan et al. (2014) who exploit brokerage house mergers and closures as a source of exogenous variation in analyst coverage. Consistent with Kelly and Ljungqvist (2012), Balakrishnan et al. (2014) document that firms losing analyst coverage experience a negative shock to liquidity. Importantly, they also provide convincing evidence that managers respond by increasing the frequency of earnings guidance, and this increased forecast frequency partially reverses the negative shock to liquidity. Overall, this paper, along with Anantharaman and Zhang (2011), suggests managers increase the quantity of their guidance following a reduction in analyst coverage. This evidence is consistent with analysts playing an information intermediary role, as managers seemingly respond to losing coverage by "filling the information gap" with their own forecasts. However, we have less evidence on how analysts influence the *quality* of guidance, and thus whether analysts play a monitoring role with respect to forward-looking disclosures. In addition, no study of which I am aware examines if analysts influence qualitative, textual disclosures of managers' forward-looking information.

The fact that liquidity decreases following loss of coverage suggests analysts play an information role (Kelly and Ljungqvist 2012). Further, at first glance, the evidence in Balakrishnan

[2] In the case of brokerage house closures, the firm loses coverage if an analyst employed at the closed brokerage house was covering the firm prior to the closure. In the case of brokerage mergers, the firm loses analyst coverage if an analyst at both of the brokerage houses covered the firm, because the newly merged company typically discontinues coverage of one of the analysts due to redundancy. I provide more details in Section 3.

et al. (2014) is inconsistent with analysts playing a *monitoring role* with respect to managers' forward-looking disclosures. Specifically, because Balakrishnan et al. show that liquidity recovers for firms that increase forecast frequency following a drop in analyst coverage, one might assume these forecasts are high quality. However, the effect of analysts' *monitoring role* in this setting is still unclear. Specifically, forecast *quality* could have decreased, but only partially offsetting the increase in liquidity from additional forecasting behavior, such that the overall effect of additional forecasting is to increase liquidity. What one can infer from Balakrishnan et al. is that the effect on liquidity of disclosure *quantity* is on average greater than any effect of disclosure *quality*. Notably, it is also possible that investors do not immediately recognize a decrease in disclosure quality. Specifically, manager forecasts are made for *future earnings*, making it difficult for investors to immediately assess the accuracy or bias of a forecast.

On one hand, finding an increase in management forecast quality following a loss of coverage suggests analysts serve an information role. In other words, this would be consistent with managers increasing the quality of their guidance to fill an information gap left by a loss of coverage (e.g., Kelly and Ljungqvist 2012; Derrien and Kecskés 2013; Balakrishnan et al. 2014). On the other hand, finding a decrease in forecast quality following a loss of coverage would be consistent with analysts monitoring managers' voluntary disclosures. Notably, analysts participate in and ask questions to managers during conferences calls, especially about forward-looking information (e.g., Chapman and Green 2018). Further, analysts typically provide their own earnings guidance, and management guidance that deviates from that of analysts is likely to raise questions from investors. Thus, when analyst coverage declines, managers may feel less monitoring from analysts to explain and "tell the truth" with their guidance.

Overall, prior work provides credible arguments for analysts' role being one of *information providers* or one of *monitors* with respect to management forecast quality. It is ambiguous ex ante which effect will dominate in this setting. As such, it is unclear whether management forecast quality will increase, decrease, or stay the same following a decrease in analyst following.

Management must choose a level of forecast quantity and quality to maximize forecast benefits in the presence of a reduction in analyst following. To replace the loss of analyst-provided information (the informational role of analysts), managers must increase forecast quantity and at possibly maintain a level of forecast quality equal to that of the lost analyst. Thus, I expect to find increased forecast quantity (as others have found). If analysts also serve as monitors, a reduction in analyst following enhances the opportunity to manage forecast quality, and the many non-informational-based incentives identified in the literature suggest the possibility that managers will reduce quality rather than at least maintain it. Accordingly, I state my hypothesis in the null form:

> **H1:** Management forecast quality does not change following an exogenous decrease in analyst following.

In addition to quantitative management forecast quality, I also examine textual, qualitative managerial disclosures. On one hand, analysts rely heavily on managerial disclosures, consume firms' earnings announcements, and participate in conference calls. Thus, it is possible that analyst following significantly influences managers' textual forward-looking disclosures. On the other hand, while analysts might monitor quantitative forecasts, it is unclear whether and to what extent they monitor textual disclosures. Therefore, it is possible that textual "cheap talk" exists regardless of analyst presence (possibly because analysts do not monitor textual disclosure), and that analyst

departure has little effect on managers' textual disclosure of forward-looking information. Accordingly, I also state this hypothesis in the null:

> **H2:** Textual, qualitative disclosures do not change following an exogenous decrease in analyst following.

CHAPTER 3

SAMPLE AND RESEARCH DESIGN

3.1 Sample Selection and Descriptive Statistics

3.1.1 Data Sources

Following prior research, I use quarterly management earnings forecasts from the I/B/E/S Guidance file (formerly CIG). Financial data come from Compustat, and stock return data is from CRSP. Institutional ownership data comes from Thomson Reuters Institutional (13f) database, and data on institutional ownership type comes from Brian Bushee's website. Audit fees data is from Audit Analytics and analyst coverage data is from I/B/E/S. Finally, I scrape earnings announcements from the SEC's EDGAR database for textual analysis.

3.1.2 Timeline of Events and Variable Measurement

Figure 1 illustrates the timeline of events. Quarter 0 is the quarter in which the treated firm loses analyst coverage due to a brokerage house closure or merger. Because I examine changes in guidance quality, I require all treatment and control firms provide at least one earnings forecast during both the four quarters before and after the event quarter. [3, 4] However, because most firms

[3] Sampling to require a level of pre-event forecasting behavior follows prior research. Specifically, Anantharaman and Zhang (2011) require treatment firms provide at least one earnings forecast in the pre- and post-four quarter period around the analyst coverage shock (as I do), arguing this choice increases sample homogeneity. Further, Balakrishnan et al. (2014) require treatment firms to have a "history of guidance" and show that firms very infrequently become guiders following the loss of coverage. Practically, I must require each firm issue at least one pre- and post-shock forecast to conduct my tests. Specifically, setting missing forecast accuracy or bias equal to zero would be inappropriate, as that implies perfect accuracy rather than a non-forecast.

[4] If brokerage house mergers and closures disproportionally occur at certain times during the year, this could affect the relative time period of my pre- and post-shock measurement windows. I do a few things to address this concern. First, I note that brokerage closures and mergers in my sample appear to occur evenly throughout the fiscal year. Specifically, 23%, 29%, 22%, and 26% of my sample of closures and mergers occur in fiscal quarter 1, 2, 3, and 4, respectively. Second, I use quarterly management forecasts rather than annual forecasts. The use of shorter horizon quarterly forecasts helps to homogenize the forecast horizon for treatment and control firms in the pre- and post-window.

do not provide a forecast in all eight quarters, I take the four-quarter average of pre- and post-treatment event values and collapse the sample such that there is one "Pre" and one "Post" period. For example, when *Post* = 0 (*Post* = 1), *Accuracy* is measured as the average accuracy of all forecasts issued by the firm in quarters -4 to -1 (+1 to +4). This choice follows prior research that characterizes guidance quality over a period of time (e.g., Goodman et al. 2013).

3.1.3 Treated and Control Firms

To identify treatment firms, I follow prior research and employ brokerage house mergers and closures as a source of plausibly exogenous variation in analyst coverage. I use the list of 43 brokerage house closures and mergers from Kelly and Ljungqvist (2012) spanning from 2000 to 2008, and supplement this data with an additional 20 hand-collected closure/merger events from Chen, Chiu, and Shevlin (2018) spanning from 2008 to 2012.

Table 1 outlines my sample section procedure. In line with prior research (e.g., Derrien and Kecskés 2013; Christensen et al. 2020), I use the I/B/E/S Broker Translation file to link brokerage house names with I/B/E/S codes.[5] I identify 6,350 unique firm-quarters with a decrease in analyst coverage due to a brokerage merger or closure. I identify brokerage closures versus mergers using different methodologies, both of which follow prior research. For brokerage closures, I require the analyst at the brokerage house to make at least one earnings per share forecast for the firm in the four quarters prior to the closure. Importantly, I use the I/B/E/S stop file to ensure the broker did not terminate coverage prior to the brokerage closure date. Excluding these "endogenous" coverage drops is important to identify and isolate plausibly exogenous drops in coverage. This

[5] As of October 18, 2018, Thomson-Reuters re-assigned a significant portion of the broker and analyst identifiers in I/B/E/S. See Thompson-Reuters "Product Change Notification" documentation on WRDS. Further, an email from WRDS on September 6, 2019 states that individual broker and analyst identifiers have been and will continue to be subject to reshuffle without warning. For these reasons, I use I/B/E/S data files downloaded and saved from WRDS prior to October 18, 2018 and confirm these files were not subject to the data changes described above.

step removes 864 firm-quarters. For brokerage mergers, I require the firm to be covered by both the acquiring and target brokerage in the four quarters prior to the merger date. I also require that only the acquiring or target broker's analyst continue covering the firm after the merger, and thus analyst coverage decreases because of the merger. This ensures the loss of analyst coverage is likely due to redundancy at the merged firm, and thus plausibly exogenous.

Next, I filter out 1,715 "serially shocked" firm-quarters. These firm-quarters are affected by another brokerage merger or closure in the prior four quarters. To ensure the pre-shock window does not contain another brokerage closure or merger, I exclude these serially shocked firm-quarters. Finally, my treatment sample is reduced to 2,678 after I require firms to issue at least one forecast in the pre- and post-shock period, leaving me with 1,093 treatment firm-quarters.

My pool of potential control firms consists of firms that both (1) experience no analyst coverage shock in the current or prior four quarters and (2) are consistent guiders (i.e., issued at least one forecast in the pre- and post-treatment period). Following Balakrishnan et al. (2014), I propensity score match (without replacement) control firms using log market value of equity, the number of analysts covering the firm, and return volatility, all measured in the quarter before the treated firm's analyst coverage loss event. Importantly, because I examine changes in signed and unsigned forecast error, I also match on pre-treatment values of management forecast accuracy. Of the candidate treatment firm-quarters, I can successfully match all but 112 within a caliper of 0.005. After dropping 80 firm-quarters without required control variables, 901 treatment firm-quarters remain, consisting of 606 unique firms.

Table 2 reports descriptive statistics for treatment and control samples. Tests of difference in means and medians suggest few differences remain after the matching procedures. Importantly, no differences remain between the two groups for my dependent variables. Further, few differences

remain for control variables. Two exceptions are *Afollow*, which exhibits a marginal difference in mean, (but not median) and *Volatility*, which has a statistical difference in mean and median. Notably, the differences for both variables are arguably economically insignificant. However, to ensure robust inferences, I include these variables as controls in my regression analyses (Shipman, Swanquist, and Whited 2017).

3.2 Research Design

I estimate the following difference-in-differences model:

$$\begin{aligned} \textit{Disclosure Variable}_{i,t} = {} & \alpha_0 + \boldsymbol{\alpha_1 Treat*Post_{i,t}} + \alpha_2 Treat_{i,t} + \alpha_3 Post_{i,t} + \alpha_4 Controls_{i,t} + \\ & \textit{Firm fixed effects} + \textit{Year-quarter fixed effects} + e_{i,t} \end{aligned} \quad (1)$$

Where *Disclosure Variable* is equal to *N_forecasts*, *Accuracy*, *or Bias.*[6] *Treat* is an indicator equal to one for firms experiencing a plausibly exogenous reduction in analyst coverage, and equal to zero for control firms. *Post* is an indicator variable equal to one for the four-quarter post-treatment period, and zero for the four-quarter pre-treatment period. My term of interest is *Treat*Post*, which measures the difference in the change in *Disclosure Variable* for treatment firms relative to control firms after the brokerage house closure or merger event.

Recall that because most firms do not provide a forecast for all eight quarters, I take the four-quarter average of pre- and post-treatment event values and then collapse the sample such that there is one "Pre" and one "Post" period. Specifically, per the Table 2 descriptive statistics, there are 901 treatment firms and 901 matched control firms. The sample contains pre- and post-observations for each group, which are the average values over the four quarters before and after

[6] I define all variables in Appendix A.

the brokerage house closure or merger event, respectively. Thus, my main regression analyses contain 3,604 (i.e., 901*4) observations.

I employ three dependent variables as *Disclosure Variable* in equation one. The first variable is the number of quarterly forecasts issued by the firm (*N_forecasts*). For the pre (post) four-quarter event period, this variable is the count of all quarterly forecasts issued by the firm. The second dependent variable is management forecast accuracy (*Accuracy*), defined as the unsigned difference between the forecast and the actual reported earnings for the period forecasted (scaled by price) and multiplied by negative one, such that larger values equate to more accurate forecasts.[7] To avoid small scalar problems, I drop firms with stock price less than one dollar. The third and final dependent variable is signed management forecast error (*Bias*). Unlike *Accuracy*, *Bias* is a signed measure, which sheds light on whether management forecasts are systematically above or below the subsequently reported actual value of earnings. I calculate *Bias* as difference between the forecast and the actual earnings for the period forecasted (scaled by price and multiplied by 100). Note that *Bias* is calculated such that higher values equate to more optimistically biased forecasts (i.e., the forecast was higher than subsequently reported earnings).

Controls is a vector of firm-level characteristics shown by prior research to influence management forecast behavior. Although I employ propensity score matching to identify appropriate matched control firms, propensity score matching is not a panacea. Thus, it is possible that any documented change in forecasting behavior following a reduction in coverage is driven by firm-level characteristics, which would merit the inclusion of firm-level control variables. However, on the other hand, one could argue that a difference-in-differences design should not

[7] If management issues a range forecast, I use the upper bound of the range (Ciconte et al. 2014). However, my results are qualitatively similar if I instead use the midpoint.

require controls, and that inappropriate controls can distort or mask the true effect of the treatment (Whited et al. 2021). Accordingly, I present three specifications of each test: (1) no control variables, (2) "Basic" controls, and (3) "Additional" controls.

"Basic" controls are those common to the disclosure literature: The natural logarithm of the firm's market value of equity (*Size*), the market-to-book ratio (*MB*), and analyst following (*Afollow*). "Additional" controls include other variables shown to be associated with voluntary disclosure behavior: Institutional ownership (*InstOwn*), litigation risk (*LitRisk*) as measured by Kim and Skinner (2012), an indicator for whether the firm issued equity during the period (*EquityIssue*), stock return volatility during the quarter (*Volatility*), and the horizon of the management forecast (*Horizon*). For all regressions, I include firm fixed effects to control for firm-specific time-invariant correlated omitted variables and year-fiscal quarter fixed effects to account for any time trends that may influence forecasting behavior.[8] Finally, I cluster standard errors by firm and year-fiscal quarter, and winsorize all continuous variables at the 1st and 99th percentiles.

[8] Because each firm-event quarter in the sample has a single "pre" and "post" period, the fiscal year-quarter fixed effect is the year-quarter of the event quarter for both the pre and post period. Otherwise, these time fixed effects would be perfectly collinear with the *Post* indicator variable. Results are insensitive to the exclusion or inclusion of year-quarter fixed effects.

CHAPTER 4

RESULTS – MANAGEMENT FORECASTS

4.1 Main Results

I begin my analysis by replicating prior research documenting managers increase the issuance of quarterly earnings forecasts following a reduction in analyst coverage. Figure 2 provides a graphical illustration. Consistent with prior research, Figure 2 suggests managers who lose analyst coverage respond by increasing the frequency of earnings guidance. Importantly, this figure also indicates parallel trends of the treatment and control samples in the pre-event period.

Table 3 presents a difference-in-differences regression for management forecast frequency following the plausibly exogenous shock. I present results using an OLS model in which the dependent variable is the natural logarithm of one plus the number of earnings forecasts issued (1+*N_Forecasts*). However, because *N_forecasts* is a count variable by nature, I also present results using a Poisson model. As discussed, all regression results employ three groups of control variables: (1) no controls, (2) "Basic" controls, and (3) "Additional" controls.

The results in Table 3 replicate both Anantharaman and Zhang (2011) and Balakrishnan et al.'s (2014) finding in my sample that managers increase the quantity of guidance following a reduction in analyst coverage. Specifically, the coefficient on *Treat*Post* is positive and significant in all six columns. Notably, the coefficient magnitude is stable across specifications, suggesting it is unlikely an omitted variable plays a significant role (Oster 2019). In addition, the coefficient on

Treat exhibits little statistical significance, suggesting a lack of pre-treatment difference between the treatment and control sample.

Next, I turn to my primary analyses in which I explore two measures of forecast quality: (1) unsigned forecast error (*Accuracy*) and (2) signed forecast error (*Bias*). Figures 3A and 3B present graphical trends for management forecast accuracy and bias, respectively. Figure 3A suggests forecast accuracy decreases following a reduction in coverage. Figure 3B sheds light on the directional change in this effect by documenting that forecasts become more optimistically biased following the treatment event. Importantly, both graphs provide visual evidence of similar trends for the treatment and control groups prior to the brokerage house merger/closure event.

Table 4 presents regression results of Figures 3A and 3B, respectively. Beginning with forecast accuracy in columns 1 through 3, the coefficient on *Treat*Post* is negative and significant in all three columns. This suggests management forecast accuracy decreases following a reduction in analyst following. Notably, the coefficient magnitude is similar regardless of what control variables are included in the model. Economically, the result suggests forecast accuracy decreases by approximately 20% of the mean value of *Accuracy* for treated firms following decline in analyst following. Additionally, the coefficient on *Treat* is insignificant, further supporting the evidence in both Table 2 descriptive statistics and the visual inspection of Figure 3A that suggests there is no pre-treatment difference in forecast accuracy between the treatment and control sample. Next, to shed light on directional changes in forecast error, I examine signed forecast error (i.e., *Bias*). Table 4, columns 4 through 6 present results using *Bias* as the dependent variable. Consistent with Figure 3B, the coefficient on *Treat*Post* is positive and significant. This suggests management

forecast optimistic bias increases following a reduction in analyst coverage.[9] Overall, the evidence in Tables 3 and 4 suggest that although managers increase the *frequency* of forecasts following a reduction in coverage, these forecasts are of lower *quality* (i.e., decreased accuracy and increased optimistic bias).[10] This finding of decreased management forecast *quality* after analyst departure is consistent with analysts playing a *monitoring role* with respect to voluntary disclosure quality.[11]

As an immediate sensitivity check, I examine whether these results vary based on pre-shock analyst coverage. The intuition is that losing an analyst should be more meaningful to a firm with already thin coverage, while firms with high coverage may not respond to losing one analyst if coverage remains high. In untabulated analysis, I confirm my results for Accuracy and Bias are concentrated in firms with below median pre-shock analyst coverage. To provide further evidence that managers are responding to a lack of monitoring, I next examine variation in forecast quality conditional on the presence of other monitoring mechanisms.

4.2 Cross-sectional Tests

4.2.1 Cross-sectional Tests on Other Monitoring Mechanisms

If managers' guidance becomes less accurate and more biased following a reduction in analyst following because they perceive their disclosures are less scrutinized by analysts, I expect

[9] Drawing reliable inferences using a difference-in-differences design requires satisfaction of the parallel trends assumption. An inspection of Figure 3 suggests the parallel trends assumption is satisfied. However, I also perform statistical tests following Chen et al. (2018). Specifically, I perform a placebo test in which I move the brokerage house merger or closure dates either back four quarters or forward four quarters. I then re-perform the tests in Table 4. If my documented results are attributable to plausibly exogenous decreases in analyst coverage, I should find no significant results for the difference-in-differences estimator using these placebo dates. In untabulated analysis, I confirm that the coefficient on *Treat*Post* is statistically insignificant in all specifications.

[10] If for some reason treated firms' performance is systematically better or worse than control firms in the post period (and managers do not anticipate this), this could drive my change in bias and accuracy findings. However, Kelly and Ljungqvist (2012) demonstrate that these brokerage house closures and mergers are unrelated to firm fundamentals, and thus plausibly exogenous. Nonetheless, in untabulated analysis, I use return on equity as an alternative dependent variable and confirm that the coefficient on *Treat*Post* is insignificant, suggesting treated and control firms' performance in the post-event period is similar.

[11] These results are not limited to a small number of firms. I find that 37% (54%) [49%] of firms increase guidance frequency (decrease accuracy) [increase bias] after losing coverage.

these results to vary with the presence of other monitors. Accordingly, I conduct two cross-sectional tests. First, I test whether these results vary with the level of dedicated institutional ownership, because research suggests dedicated institutional owners monitor managers' disclosures. I expect the forecast accuracy and bias results to be concentrated in firms with less dedicated institutional ownership. Second, I examine whether the results vary with the level of excess audit fees, because research shows that firms with greater abnormal audit fees issue higher quality forecasts. As such, I expect my results to be stronger for firms with lower excess audit fees.

Table 5, Panel A presents results for cross-sectional tests based on dedicated institutional ownership. *Low IO* (*High IO*) is an indicator variable equal to one (zero) if the firm has below (above) median dedicated institutional ownership, measured in the quarter prior to the loss of coverage. For brevity, I only present results using the "Basic" controls specification and suppress results of control variables, although results are insensitive to controls used. When using *Accuracy* (*Bias*) as the dependent variable, the coefficient on *Treat*Post* is negative (positive) and significant only in the *Low IO* group. A test of difference confirms the coefficient on *Treat*Post* is statistically different in the *Low IO* group for *Accuracy*, although not for *Bias*. This suggests managers who experience a reduction in analyst coverage only respond by decreasing forecast quality when they face less scrutiny from institutional investors. Next, Panel B presents results for changes in *Accuracy* and *Bias* conditional on the firm's audit fees. *Low Fees* (*High Fees*) is an indicator variable equal to one (zero) if the firm has below (above) median excess audit fees, measured in the quarter prior to coverage loss. Similar to Panel A, the decrease (increase) in *Accuracy* (*Bias*) is concentrated in firms with below median excess audit fees. A test of difference confirms the coefficient on *Treat*Post* is statistically different in the *Low Fees* group for both *Accuracy* and *Bias*. Overall, the results in Table 5 support the idea that analysts monitor

management forward-looking disclosure quality. After a reduction in coverage, managers decrease the quality of their guidance more so when there are fewer opportunities for other monitoring mechanisms to substitute for analyst monitoring.

4.2.2 Cross-sectional Tests on Managerial Incentives

Prior research suggests managers sometimes use disclosure opportunistically to profit via insider trading gains (e.g., Cheng et al. 2013). Accordingly, it is possible managers take advantage of decreased monitoring from analysts by using low-quality forecasts to increase profits from insider trading. Specifically, issuing an optimistically biased (and thus less accurate) forecast could increase the firm's stock price just prior to an insider sale, thereby increasing selling profits. To test this conjecture, I partition my sample on whether the manager engages in opportunistic insider selling, as measured by Cohen, Malloy and Pomorski (2012). Table 6 reports the results. *Opp Trader* (*Not Opp Trader*) is an indicator variable equal to one (zero) if, following the loss of coverage, the manager engaged (did not engage) in opportunistic insider trading within 30 days following a management forecast. The decrease (increase) in forecast A*ccuracy* (*Bias*) is only present among firms whose managers engage in opportunistic insider selling (i.e., columns 1a and 2a). However, I note that a test of differences is not significant at conventional levels. Nonetheless, Table 6 provides some evidence that managers respond to loss of analyst coverage by decreasing forecast quality when their incentives to do so are stronger.

4.3 Liquidity Effects Conditional on Forecast Quality

Before turning to an examination of forward-looking textual disclosures, I reconcile the liquidity effects documented in prior literature with the decreased forecast quality effects documented here. As discussed, both (1) higher *quantity* and separately, (2) higher *quality*

disclosure could increase liquidity. Empirically, although liquidity falls for firms experiencing a decrease in coverage (Kelly and Ljungqvist 2012), it partially recovers for firms that respond with increased forecasting (Balakrishnan et al. 2014). However, this does not preclude the possibility of a simultaneous decrease in forecast *quality*. To provide evidence on this conjecture, I examine changes in liquidity following reduction in coverage, *conditional on changes in forecast quality.* If forecast quality decreases following loss of coverage, I expect the liquidity reversal effect of increased forecasting frequency to be weaker for firms that issue lower-quality guidance.

To perform this test, I retain my sample of treated firms (i.e., forecasting firms that experience a decrease in coverage), but replace my control firms with those that do *not* issue guidance, but *also experience an exogenous decrease in coverage*. As such, all firms in this sample experience a decrease in analyst coverage, but only my treatment sample (i.e, "*Guider*" firms) respond by issuing earnings guidance. By using this "control" sample of shocked firms, I can more directly assess differences in liquidity for guiding versus non-guiding firms.[12]

Table 7 presents results. The dependent variable is the natural log of one plus the Amihud illiquidity measure (*AIM*) from Amihud (2002). In columns 1 through 3, I replicate the results of prior work. Specifically, the coefficient on *Post* is positive and significant, suggesting firms experience an increase in illiquidity following a decrease in analyst coverage. However, the coefficient on *Guide*Post is* negative and significant. This result also replicates prior research suggesting guiding firms partially reverse the liquidity shock by issuing additional guidance.

[12] Similar to my primary analysis, I propensity score match firms (without replacement) on log market value of equity, the number of analysts covering the firm, and return volatility, all measured in the quarter before the analyst coverage loss event. Because control firms in this test do *not* provide earnings forecasts, I do not match on pre-disclosure behavior. I am able to match 878 treated firms with a control firm using a caliper of 0.005, compared to 901 successful matches in my primary analysis.

In columns 4 through 6, I add a proxy for firms that respond to loss of coverage by issuing lower *quality* forecasts. Specifically, *LowQuality* is an indicator variable equal to one if the *Guider* firm's forecast accuracy decreased by more than the *Guider* sample median following the coverage shock, and zero otherwise.[13] The coefficient on *Guider*Post*LowQuality* is positive and significant. This suggests illiquidity recovers less so for firms that respond with *low quality* voluntary disclosure. It is also noteworthy that the absolute value of the coefficient on *Guider*Post* (-0.038), is greater than the coefficient on *Guider*Post*LowQuality* (0.019). Consistent with Balakrishnan et al. (2014), this result suggests the effect on liquidity of a forecast *quantity* increase is on average greater than the effect of a forecast *quality* decrease.

Overall, the results of Table 7 suggest the following: (1) Illiquidity increases for firms that experience a decrease in analyst coverage (Kelly and Ljungqvist 2012), but (2) partially recovers for firms that provide forecasts following the shock (Balakrishnan et al. 2014), and (3) the recovery in liquidity for guiding firms is lower for firms that respond with *lower quality* guidance.

[13] I define *LowQuality* using forecast accuracy, rather than forecast bias, because forecast accuracy is an unsigned measure, making it easier to define changes in quality. Although I find that forecasts become more optimistically biased, on average, if there are some managers that become more *pessimistically* biased, using forecast bias to define *LowQuality* would have less power. Nonetheless, the pattern of results is similar if I use forecast bias to define *LowQuality*, although statistically weaker.

CHAPTER 5

RESULTS – FORWARD-LOOKING QUALITATIVE DISCLOSURES

5.1 Main Results

My results thus far suggest managers respond to a loss of coverage by issuing less accurate and more optimistically biased quantitative earnings forecasts. Although it is possible managers also change qualitative aspects of disclosures, it is unclear ex ante whether analysts monitor textual forward-looking information to the same extent as numeric forecasts. Accordingly, in this section, I examine the extent to which forward-looking qualitative disclosures change following a reduction in coverage. Examining forward-looking qualitative disclosures is important to provide a holistic view of how analysts influence managers' voluntary disclosures. For example, Bozanic et al. (2018) caution researchers against using standard point and range earnings forecasts as summary measures of voluntary disclosure because doing so may be inadequate. As such, I follow Bozanic et al. (2018) and measure the change in (1) quantity of earnings-related forward-looking statements and (2) tone of these statements, both measured from earnings press releases.

Measuring forward-looking statements from earnings announcements affords a few benefits. First, because essentially all firms issue quarterly earnings press releases, I am not constrained to limit my sample to firms that provide an earnings forecast in the pre- and post-event period as I am in my earnings forecast analysis. Thus, for forward-looking statements, I can make inferences about more than just firms that provide earnings guidance. Second, most forecasts are "bundled" with the earnings announcement, making earnings announcements a primary venue for

managers to disclose forward-looking information (e.g., Rogers and Van Buskirk 2013). Third, as noted by Bozanic et al. (2018), firms plan earnings announcements regardless of economic events, so a change in forward-looking information is likely driven by managerial discretion.

I use a series of Python scripts to scrape the SEC's EDGAR database and download all firms' Exhibit 99.1 filings (earnings announcements). Because earnings announcements are not systematically available in EDGAR until 2004, my sample begins in 2005 (because I require four quarters prior to the brokerage closure/merger) and continues through 2013 (one year after the latest shock date of 2012). Recall that my two dependent variables of interest are (1) the quantity of earnings-related forward-looking statements and (2) the tone of these statements. To identify earnings-related forward-looking statements, I use the dictionaries of earnings-related and forward-looking words provided by Bozanic et al (2018). I then calculate my first dependent variable, *PctFwd*, as the number of sentences containing earnings-related forward-looking statement(s) scaled by the total number of sentences in the earnings announcement. Next, to calculate the net tone of these statements, I use the Loughran and Mcdonald (2011) financial sentiment dictionary. I define *FwdTone* as the number of positive forward-looking sentences minus negative forward-looking sentences, scaled by total forward-looking sentences.

Table 8 columns 1 through 3 present results for *PctFwd.* The coefficient on *Treat*Post* is positive and significant in all three columns. This suggests that, similar to quantitative earnings forecasts, managers increase the use of qualitative earnings-related forward-looking statements following a reduction in analyst following. However, this result is economically modest. Specifically, *PctFwd* increases by approximately 2% of its mean for treated firms following a decline in analyst following. Next, columns 4 through 6 present results for *FwdTone*. Again, the coefficient on *Treat*Post* is positive and significant, indicating that managers increase the positive

tone of forward-looking statements following a loss of coverage. This is consistent with my primary analysis, which shows earnings forecast optimism increases following loss of coverage.

Overall, earnings-related forward-looking statements exhibit a similar pattern to that of quantitative management forecasts. First, similar to earnings forecasts, managers increase the *frequency* of earnings-related forward-looking statements. Second, similar to earnings forecast optimism, managers increase the positivity of forward-looking statements. However, the economically modest textual analysis results suggest that analysts likely do not monitor managers' textual disclosures to the same extent as quantitative forecasts.

5.2 Cross-sectional Tests

5.2.1 Cross-sectional Tests on Other Monitoring Mechanisms

Because analysts appear not to monitor textual disclosures to the same extent as quantitative forecasts, it is possible other monitors have little influence on forward-looking textual disclosures. For example, if auditors are concerned with hard numbers but do not monitor managers' "cheap talk", it is unlikely audit quality will matter in the cross-section. Further, while dedicated institutions might pay attention to textual disclosures, they may also be more likely to ignore textual fluff. To shed light on potential differences in other monitors' influence on textual disclosures, I perform the same two cross-sectional tests for my textual analysis variables (i.e., cross-sectional tests on dedicated institutional ownership and abnormal audit fees).

Table 9, Panel A presents results for dedicated institutional ownership. For both *PctFwd* and *FwdTone*, the coefficient on *Treat*Post* is positive and significant only in the *Low IO* group. However, tests of differences in *Treat*Post* are not significant at conventional levels. Although directionally consistent with my results using *Accuracy* and *Bias*, Table 9, Panel A provides only

weak evidence that managers increase the amount of forward-looking statements more when they face less scrutiny from institutional investors. Next, Panel B presents results for changes in *PctFwd* and *FwdTone,* conditional on the firm's excess audit fees. For both dependent variables, the change is economically and statistically indistinguishable across partitions. This result is consistent with auditors not playing a significant role in monitoring cheap talk disclosures.

Overall, the results in Tables 9 support the idea that while institutions and auditors serve to substitute for monitoring by analysts with respect to quantitative management forecasts, neither entity serves a large role in monitoring managers' forward-looking textual disclosures. Overall, these results highlight the contrast between various monitors' role in scrutinizing quantitative versus qualitative disclosures.

5.2.2 Cross-sectional Tests on Managerial Incentives

Next, for *PctFwd* and *FwdTone*, I perform cross-sectional tests on managers who engage in opportunistic insider selling. Perhaps unsurprisingly, I find no difference between the frequency of forward-looking earnings-related statements based on opportunistic insider selling (i.e., Table 10, column 1a vs. 1b). However, I find results using *FwdTone* are concentrated in managers who engage in opportunistic insider selling (i.e., results concentrated in column 2a and not 2b). Intuitively, this result suggests managers appear to increase the positivity of their forward-looking language to increase profitability of insider sales after losing monitoring from analysts.

5.3 Liquidity Effects Conditional on Forward-Looking Statement Frequency and Positivity

In my final test, I examine whether, similar to quantitative earnings forecasts, forward-looking textual disclosures have liquidity effects following loss of coverage. On one hand, it is possible forward-looking statements have a similar effect to earnings forecasts (i.e., higher forecast

frequency recovers liquidity, but lower quality forecasts attenuate this effect). On the other hand, given the lower economic magnitudes of the forward-looking statement results, it is possible changes in forward-looking statements do not meaningfully influence liquidity.

Similar to my test of liquidity using forecasting variables in Table 7, I retain my sample of treated firms (i.e., firms that experience a decrease in coverage), but replace my control firms with those that also experience an exogenous decrease in coverage (matching on the same dimensions as in my main tests). I then define indicator variables for changes in forward-looking earnings-related statement quantity and quality: (1) *TextGuider* is equal to one for firms whose increase in forward-looking statements following loss of coverage is above the sample median, and zero otherwise, and (2) *LowQualityText* equals one for firms whose increase in tone of forward-looking statements after loss of coverage is above the sample median, and zero otherwise.

Table 11 presents the results. The dependent variable is the natural log of one plus the Amihud's illiquidity measure (*AIM*). Columns 1 through 3 provide results only conditional on forward-looking statement quantity. I replicate the results of prior work by showing that, following the loss of coverage, liquidity decreases (i.e., the coefficient on *Post* is positive and significant). However, the coefficient on *TextGuider*Post* is insignificant. Further, in Columns 4 through 6 where I include a measure of low-quality forward-looking statements, the coefficient on both *TextGuider*Post* and *TextGuider*Post*LowQualityText* are insignificant. Overall, the results of Table 11 provide no evidence that changes in forward-looking earnings-related statements influence liquidity following loss of analyst coverage.

CHAPTER 6

ADDITIONAL ANALYSIS AND ROBUSTNESS

6.1 The Effect of Earnings Management

Prior research using this setting finds an increase in accruals earnings management after losing coverage (e.g., Irani and Oesch 2013; Chen et al. 2015). Because earnings management can influence perceived forecast quality (i.e., accuracy and bias), it is important to understand its potential effect in this setting. First, prior research suggests firms that manage earnings are reluctant to forecast (e.g., Dye 1988; Schipper 1989; Jo and Kim 2007). Because my sample is consistent guiders, this suggests that, on average, these firms are not heavily managing earnings.[14] Second, earnings management activity should bias against my results. I document a *decrease* in forecast accuracy, and it is unclear why managers would use earnings management to manage to a *less* accurate forecast. Third, my inferences are similar if I include a control for earnings management (using absolute discretionary accruals, as in Irani et al. 2013). Fourth, if earnings management drives my results, I would expect my results to be concentrated in "high earnings management" firms. Thus, I perform a cross-sectional test partitioning on absolute discretionary accruals, which suggests no differences between *Treat*Post* across partitions for forecast accuracy or bias. Fifth, I replace *earnings* forecasts with *sales* forecasts, because prior work suggests revenues are less vulnerable to manipulation than earnings (Ertimur et al. 2003; Koo and Lee

[14] Consistent with this statement, the mean discretionary accruals in my sample is only 0.026 compared to 0.036 for the Compustat universe for the same years.

2018). My accuracy and bias inferences are similar using management *sales* forecasts. Overall, the results of these analyses suggest earnings management is unlikely influencing my inferences.

6.2 Examining Unintentional Reasons for Decrease in Forecast Quality

It is possible managers *unintentionally* reduce forecast quality after losing coverage. Although an archival study cannot definitely ascribe managers' intent, I discuss two alternative explanations for my results and provide circumstantial evidence to suggest my results are unlikely driven by managers' unintentional actions.

First, an alternative explanation for my management forecast results is that managers feel compelled to increase forecast frequency to replace the lost information from analysts, and that these "forced" forecasts are naturally lower quality. However, there are at least three reasons this explanation is unlikely. First, my management forecast results vary predictably based on monitoring from other parties. If managers simply felt compelled to provide more forecasts after losing coverage, I would not expect cross-sectional variation based on other monitoring mechanisms. Second, although a "forced forecasting" explanation may explain decreased accuracy, it is less likely to explain the directional forecast bias I document. Finally, managers tend to withhold bad news, on average (e.g., Kothari et al. 2009; Baginski et al. 2018). If managers feel compelled to disclose previously withheld news, it is reasonable that it would be bad news, on average. However, I document an increase in optimistically biased and good news forecasts.

A second alternative explanation is that managers issue less accurate forecasts after losing coverage because managers lose information from analysts with which to develop their forecasts. Although a commonly held view is that most of the information analysts produce actually comes from managers (e.g., Chen et al. 2007), Brown et al. (2018) suggest that up to 55% of companies

rely on analysts for information about industry and macroeconomic trends. To test this alternative interpretation, I use Hutton et al. (2012) to identify when analysts have an information advantage over managers - when the firm's earnings move in concert with the macro economy. If this alternative explanation holds, I expect my management forecast accuracy results to be concentrated in firms who lose an analyst when the analyst has the information advantage. However, in untabulated analysis I find no difference in the decrease in forecast accuracy for firms with above versus below median analyst information advantage.

6.3 Other Management Forecast Attributes

It is also possible managers respond to loss of coverage by changing other characteristics of their guidance. Accordingly, I explore two other forecasts characteristics: (1) forecast precision and (2) forecast news. Because I follow Ciconte et al. (2014) and use the upperbound of forecasted earnings to measure accuracy and bias, it is possible I do not capture changes in precision. If managers enjoy less monitoring from analysts, they may issue more or less precise forecasts for opportunistic reasons (e.g., Cheng et al. 2013). However, forecast precision is relatively stable in the post-Regulation Fair Disclosure period. In untabulated analysis, I find no evidence of a change in forecast precision following a reduction in coverage. Second, I explore forecast news (i.e., the management forecast relative to the outstanding analyst consensus). Recall that I document an increase in forecast *optimism* (i.e., the management forecast is greater than the subsequently reported actual earnings). As such, perhaps unsurprisingly, I find some evidence that managers increase the frequency of good news earnings forecasts but not bad news earnings forecasts.

CHAPTER 7

CONCLUSION

Prior research documents that managers respond to an exogenous decrease in analyst coverage by increasing the *quantity* of earnings forecasts, presumably to fill the information void left by a reduction in coverage. I extend this research by considering management forecast *quality* and an alternative form of guidance, managers' forward-looking *textual disclosures*. First, although forecast *quantity increases* after loss of coverage and liquidity partially improves, I find forecast *quality decreases* (i.e., larger signed and unsigned errors) and the decrease in quality attenuates the improvement in liquidity. These results suggest analysts not only play an informational role, but also a monitoring role with respect to managers' forward-looking disclosures. These findings are more pronounced when other monitors are less present and when managers have insider selling incentives to engage in this disclosure behavior. Second, with respect to *textual disclosures*, managers' quantity and tone of forward-looking statements increase following loss of coverage. Although these results do not vary with the presence of other monitors, the increase in positive tone is concentrated in managers who engage in insider selling. Overall, my study provides a more nuanced view of analysts' role in influencing managers' forward-looking disclosures.

REFERENCES

Ajinkya, Bipin B., and Michael J. Gift. 1984. "Corporate Managers' Earnings Forecasts and Symmetrical Adjustments of Market Expectations." *Journal of Accounting Research* 22 (2): 425–44.

Ajinkya, Bipin, Sanjeev Bhojraj, and Partha Sengupta. 2005. "The Association between Outside Directors, Institutional Investors and the Properties of Management Earnings Forecasts." *CFA Digest* 35 (4): 15–17.

Amihud, Yakov. 2002. "Illiquidity and Stock Returns: Cross-Section and Time-Series Effects." *Journal of Financial Markets* 5 (1): 31–56.

Amiram, Dan, Edward Owens, and Oded Rozenbaum. 2016. "Do Information Releases Increase or Decrease Information Asymmetry? New Evidence from Analyst Forecast Announcements." *Journal of Accounting & Economics* 62 (1): 121–38.

Anantharaman, Divya, and Yuan Zhang. 2011. "Cover Me: Managers' Responses to Changes in Analyst Coverage in the Post-Regulation FD Period." *The Accounting Review* 86 (6): 1851–85.

Ayers, Benjamin C., Santhosh Ramalingegowda, and P. Eric Yeung. 2011. "Hometown Advantage: The Effects of Monitoring Institution Location on Financial Reporting Discretion." *Journal of Accounting & Economics* 52 (1): 41–61.

Baginski, Stephen P., John L. Campbell, James Moon, and James Warren. 2019. "Towards a Risk-Neutral Disclosure Policy: The Role of Compensation in Management Forecast Quality." SSRN Scholarly Paper ID 3277365.

Baginski, Stephen P., and Lisa A. Hinson. 2016. "Cost of Capital Free-Riders." *The Accounting Review* 91 (5): 1291–1313.

Balakrishnan, Karthik, Mary Brooke Billings, Bryan Kelly, and Alexander Ljungqvist. 2014. "Shaping Liquidity: On the Causal Effects of Voluntary Disclosure." *Journal of Finance* 69 (5): 2237–78.

Ball, Ray, Sudarshan Jayaraman, and Lakshmanan Shivakumar. 2012. "Audited Financial Reporting and Voluntary Disclosure as Complements: A Test of the Confirmation Hypothesis." *Journal of Accounting & Economics* 53 (1/2): 136–66.

Billings, Mary Brooke, Matthew C. Cedergren, and Svenja Dube. 2016. "Do Managers Respond to Litigation with Silence?" SSRN Scholarly Paper ID 2723716.

Billings, Mary Brooke, Robert Jennings, and Baruch Lev. 2015. "On Guidance and Volatility." *Journal of Accounting & Economics* 60 (2/3): 161–80.

Botosan, Christine A. 1997. "Disclosure Level and the Cost of Equity Capital." *The Accounting Review* 72 (3): 323.

Bozanic, Zahn, Darren T. Roulstone, and Andrew Van Buskirk. 2018. "Management Earnings Forecasts and Other Forward-Looking Statements." *Journal of Accounting & Economics* 65 (1): 1–20.

Brown, Lawrence D., Andrew C. Call, Michael B. Clement, and Nathan Y. Sharp. 2018. "Managing the Narrative: Investor Relations Officers and Corporate Disclosure." *Journal of Accounting and Economics*, January.

Chapman, Kimball, and Jeremiah R. Green. 2018. "Analysts' Influence on Managers' Guidance." *The Accounting Review* 93 (1): 45–69.

Chen, Qi, Itay Goldstein, and Wei Jiang. 2007. "Price Informativeness and Investment Sensitivity to Stock Price." *Review of Financial Studies.*

Chen, Novia X., Peng-Chia Chiu, and Terry Shevlin. 2018. "Do Analysts Matter for Corporate Tax Planning? Evidence from a Natural Experiment." *Contemporary Accounting Research* 35 (2): 794–829.

Chen, Tao, Jarrad Harford, and Chen Lin. 2015. "Do Analysts Matter for Governance? Evidence from Natural Experiments." *Journal of Financial Economics* 115 (2): 383–410.

Cheng, Qiang, Ting Luo, and Heng Yue. 2013. "Managerial Incentives and Management Forecast Precision." *The Accounting Review* 88 (5): 1575–1602.

Christensen, Theodore E., Enrique Gomez, Matthew Ma, and Jing Pan. 2020. "Analysts' Role in Shaping Non-GAAP Reporting: Evidence from a Natural Experiment." *Review of Accounting Studies*, Forthcoming.

Ciconte, William, Marcus Kirk, and Jennifer Tucker. 2014. "Does the Midpoint of Range Earnings Forecasts Represent Managers' Expectations?" *Review of Accounting Studies* 19 (2): 628–60.

Cohen, Lauren, Christopher Malloy, and Lukasz Pomorski. 2012. "Decoding Inside Information." The Journal of Finance 67 (3): 1009–43.

Coller, Maribeth, and Teri Lombardi Yohn. 1997. "Management Forecasts and Information Asymmetry: An Examination of Bid-Ask Spreads." *Journal of Accounting Research* 35 (2): 181–91.

Derrien, François, and Ambrus Kecskés. 2013. "The Real Effects of Financial Shocks: Evidence from Exogenous Changes in Analyst Coverage." *The Journal of Finance* 68 (4): 1407–40.

Diamond, Douglas W., and Robert E. Verrecchia. 1991. “Disclosure, Liquidity, and the Cost of Capital.” *Journal of Finance* 46 (4): 1325–59.

Dye, Ronald A. 1988. “Earnings management in an overlapping generations model.” *Journal of Accounting Research*.” 26, 195-235.

Dye, Ronald A., and Sri S. Sridhar. 1995. “Industry-Wide Disclosure Dynamics.” *Journal of Accounting Research* 33 (1): 157–74.

Easley, David, and Maureen O’hara. 2004. “Information and the Cost of Capital.” *Journal of Finance* 59 (4): 1553–83.

Ellul, Andrew, and Marios Panayides. 2018. “Do Financial Analysts Restrain Insiders’ Informational Advantage?” *Journal of Financial and Quantitative Analysis* 53 (1): 203–41.

Ertimur, Y., J. Livnat, and M. Martikainen. 2003. “Differential Market Reactions to Revenue and Expense Surprises.” *Review of Accounting Studies* 8 (2–3): 185–211.

Feng, Mei, Chan Li, and Sarah McVay. 2009. “Internal Control and Management Guidance.” *Journal of Accounting & Economics* 48 (2/3): 190–209.

Frankel, Richard, S.p. Kothari, and Joseph Weber. 2006. “Determinants of the Informativeness of Analyst Research.” *Journal of Accounting & Economics* 41 (1/2): 29–54.

Goodman, Theodore H., Monica Neamtiu, Nemit Shroff, and Hal D. White. 2013. “Management Forecast Quality and Capital Investment Decisions.” *The Accounting Review* 89 (1): 331–65.

Healy, Paul M., and Krishna G. Palepu. 2001. “Information Asymmetry, Corporate Disclosure, and the Capital Markets: A Review of the Empirical Disclosure Literature.” *Journal of Accounting & Economics* 31 (1–3): 405–40.

Hribar, Paul, and Holly Yang. 2016. “CEO Overconfidence and Management Forecasting.” *Contemporary Accounting Research* 33 (1): 204–27.

Hutton, Amy P., Lian Fen Lee, and Susan Z. Shu. 2012. “Do Managers Always Know Better? The Relative Accuracy of Management and Analyst Forecasts.” *Journal of Accounting Research* 50 (5): 1217–44.

Irani, Rustom M., and David Oesch. 2013. “Monitoring and Corporate Disclosure: Evidence from a Natural Experiment.” *Journal of Financial Economics* 109 (2): 398–418.

Ittner, Christopher D., and Jeremy Michels. 2017. “Risk-Based Forecasting and Planning and Management Earnings Forecasts.” *Review of Accounting Studies* 22 (3): 1005–47.

Jensen, Michael C., and William H. Meckling. 1976. “Theory of the Firm: Managerial Behavior, Agency Costs and Ownership Structure.” *Journal of Financial Economics* 3 (4): 305–60.

Jo, Hoje, and Yongtae Kim. 2007. “Disclosure frequency and earnings management.” *Journal of Financial Economics*. 84, 561-590.

Jorgensen, Bjorn N., and Michael T. Kirschenheiter. 2012. “Interactive Discretionary Disclosures.” *Contemporary Accounting Research* 29 (2): 382–97.

Jung, Woon-Oh, and Young K. Kwon. 1988. “Disclosure When the Market Is Unsure of Information Endowment of Managers.” *Journal of Accounting Research* 26 (1): 146–53.

Ke, Ruihao, Meng Li, Zhejia Ling, and Yuan Zhang. 2018. “Social Connections Within Executive Teams and Management Forecasts.” *Management Science*, March.

Kelly, Bryan, and Alexander Ljungqvist. 2012. “Testing Asymmetric-Information Asset Pricing Models.” *Review of Financial Studies* 25 (5): 1366–1413.

Kim, I., and D. Skinner. 2012. Measuring securities litigation risk. *Journal of Accounting & Economics* 53 (1/2): 290–310.

Koo, David S., and Dongyoung Lee. 2018. “Influential Chief Marketing Officers and Management Revenue Forecasts.” *The Accounting Review* 93 (4): 253–81.

Kothari, S. P., Peter D. Wysocki, and Susan Shu. 2009. “Do Managers Withhold Bad News?” Journal of Accounting Research 47 (1): 241–76.

Lang, Mark, and Russell Lundholm. 1993. “Cross-Sectional Determinants of Analyst Ratings of Corporate Disclosures.” *Journal of Accounting Research* 31 (2): 246–71.

———. 1996. “The Relation Between Security Returns, Firm Earnings, and Industry Earnings.” *Contemporary Accounting Research* 13 (2): 607–29.

Leuz, Christian, and Robert E. Verrecchia. 2000. “The Economic Consequences of Increased Disclosure.” *Journal of Accounting Research* 38 (3): 91–124.

Loughran, Tim, and Bill Mcdonald. 2011. “When Is a Liability Not a Liability? Textual Analysis, Dictionaries, and 10-Ks.” *Journal of Finance* 66 (1): 35–65.

Oster, Emily. 2019. “Unobservable Selection and Coefficient Stability: Theory and Evidence.” *Journal of Business & Economic Statistics* 37 (2): 187–204.

Rogers, Jonathan L., and Phillip C. Stocken. 2005. “Credibility of Management Forecasts.” *The Accounting Review* 80 (4): 1233–60.

Rogers, Jonathan L., and Andrew Van Buskirk. 2013. “Bundled Forecasts in Empirical Accounting Research.” *Journal of Accounting & Economics* 55 (1): 43–65.

Roh, Yongoh, and Paul Zarowin. 2019. “Disclosure of Financial Statement Line Items and Insider Trading Around Earnings Announcements.” SSRN Scholarly Paper ID 3365212.

Schipper, Katherine. 1989. “Commentary on earnings management.” *Accounting Horizons*. 3, 91-102.

Skinner, Douglas J. 1994. “Why Firms Voluntarily Disclose Bad News.” *Journal of Accounting Research* 32 (1): 38–60.

———. 1997. “Earnings Disclosures and Stockholder Lawsuits.” *Journal of Accounting & Economics* 23 (3): 249–82.

Shipman, Jonathan E., Quinn T. Swanquist, and Robert L. Whited. 2017. “Propensity Score Matching in Accounting Research.” *The Accounting Review* 92 (1): 213–44.

Trueman, Brett. 1986. “Why Do Managers Voluntarily Release Earnings Forecasts?” *Journal of Accounting & Economics* 8 (1): 53–71.

Verrecchia, Robert E. 2001. “Essays on Disclosure.” *Journal of Accounting & Economics* 32 (1–3): 97–180.

Whited, Robert, Quinn Swanquist, James Moon and Jonathan Shipman. 2021. Out of Control: The (Over)use of Controls in Accounting Research.

Yang, Holly I. 2012. “Capital Market Consequences of Managers’ Voluntary Disclosure Styles.” *Journal of Accounting & Economics* 53 (1/2): 167–84.

Yu, Fang (Frank). 2008. “Analyst Coverage and Earnings Management.” *Journal of Financial Economics* 88 (2): 245–71.

APPENDIX A

VARIABLE DEFINITIONS

Dependent Variables:	
N_forecasts	The number of management earnings forecasts issued by the firm.
Accuracy	The absolute difference between the management earnings forecast and actual earnings, multiplied by -100, scaled by beginning-of-the-period price.
Bias	The signed difference between the management earnings forecast and actual earnings, multiplied by -100, scaled by beginning-of-the-period price.
FwdTone	Net positive tone of forward-looking sentences in the firm's current earnings announcement, defined as positive forward-looking sentences minus negative forward-looking sentences, scaled by total forward looking sentences.
PctFwd	Following Bozanic, Roulstone, and Van Buskirk (2017): The number of sentences in the firm's earnings announcement that contain a forward-looking and earnings-related term, scaled by the total number of sentences.
AIM	Amihud Illiquidity, as measured in Amihud (2002).

Other Variables:	
Afollow	The number of analysts following the firm at the beginning of the period.
Equity Issue	An indicator variable equal to one if the firm was a net equity issuer during the period, and zero otherwise.
Horizon	The number of days between the forecast date and the end of the fiscal period, scaled by 365.
InstOwn	The percent of shares held by institutions, measured as the average institutional ownership during the year in which the management forecast was released.
LitRisk	Litigation risk, as measured in Kim and Skinner (2012).
MB	Market-to-book ratio at beginning-of-period.
Size	The natural logarithm of market value at beginning-of-the period.
Volatility	Stock return volatility during the quarter.

FIGURE 1

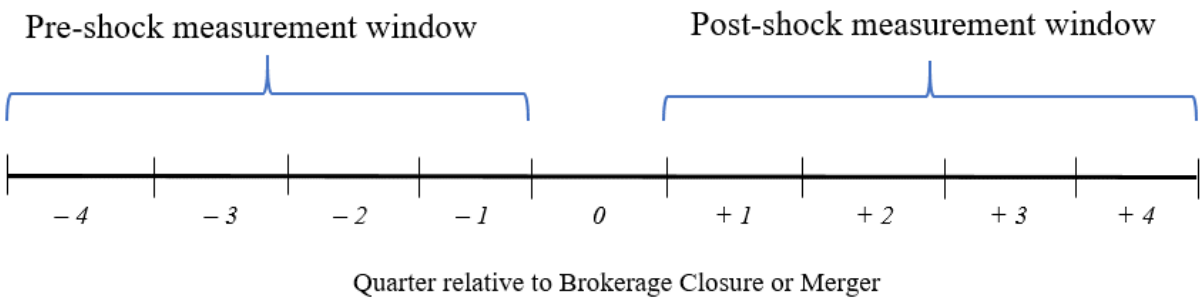

Pre-shock measurement window
Post-shock measurement window
– 4
– 3
– 2
– 1
0
+ 1
+ 2
+ 3
+ 4
Quarter relative to Brokerage Closure or Merger

FIGURE 2

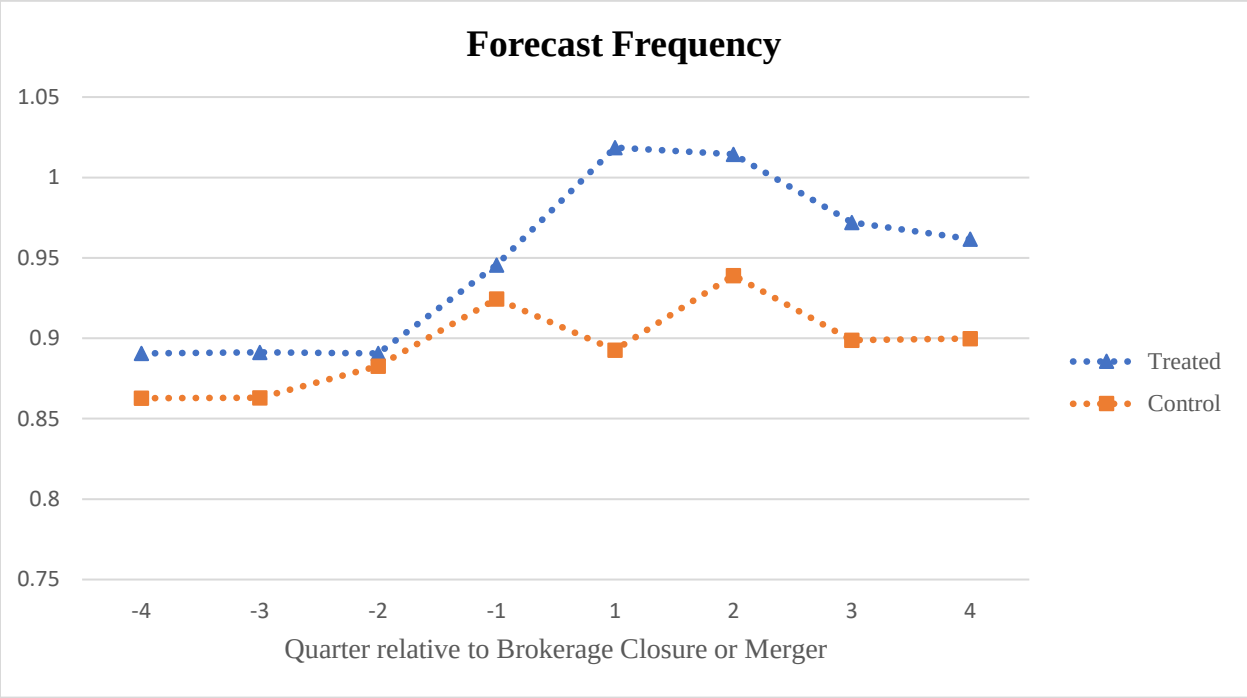
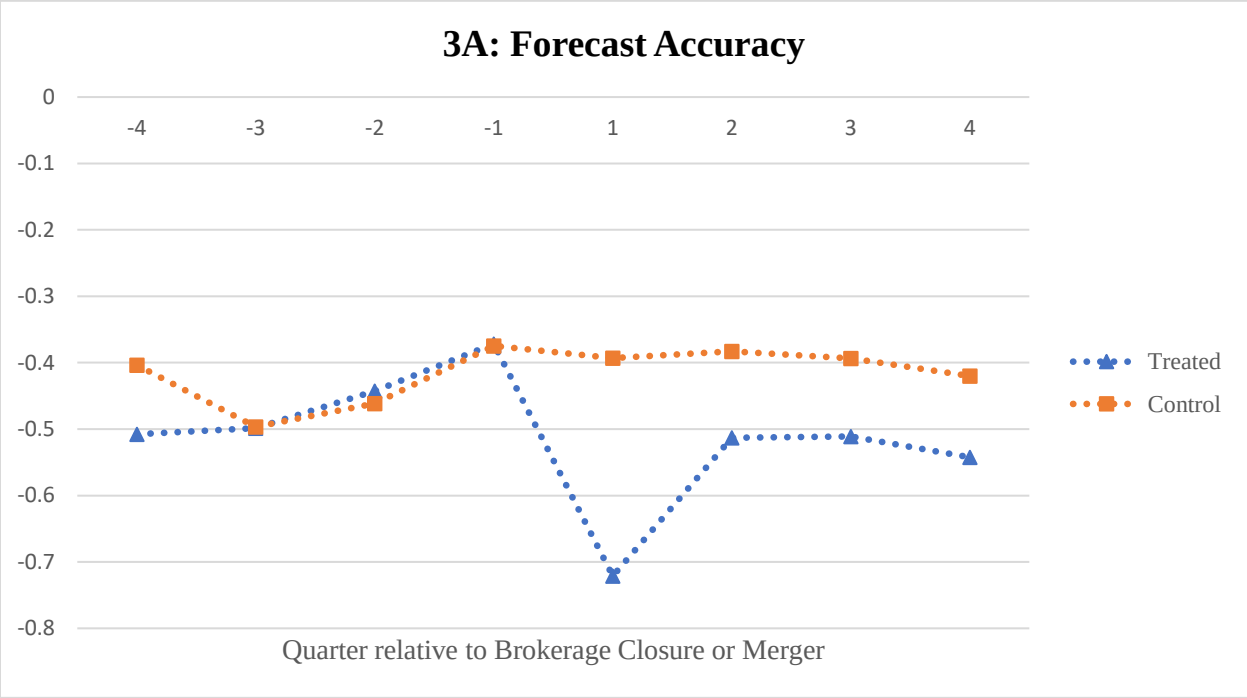

Forecast Frequency
1.05
1
0.95
0.9
0.85
0.8
0.75
-4
-3
-2
-1
1
2
3
4
Quarter relative to Brokerage Closure or Merger
Treated
Control

FIGURE 3


3A: Forecast Accuracy
0
-0.1
-0.2
-0.3
-0.4
-0.5
-0.6
-0.7
-0.8
-4
-3
-2
-1
1
2
3
4
Quarter relative to Brokerage Closure or Merger
Treated
Control

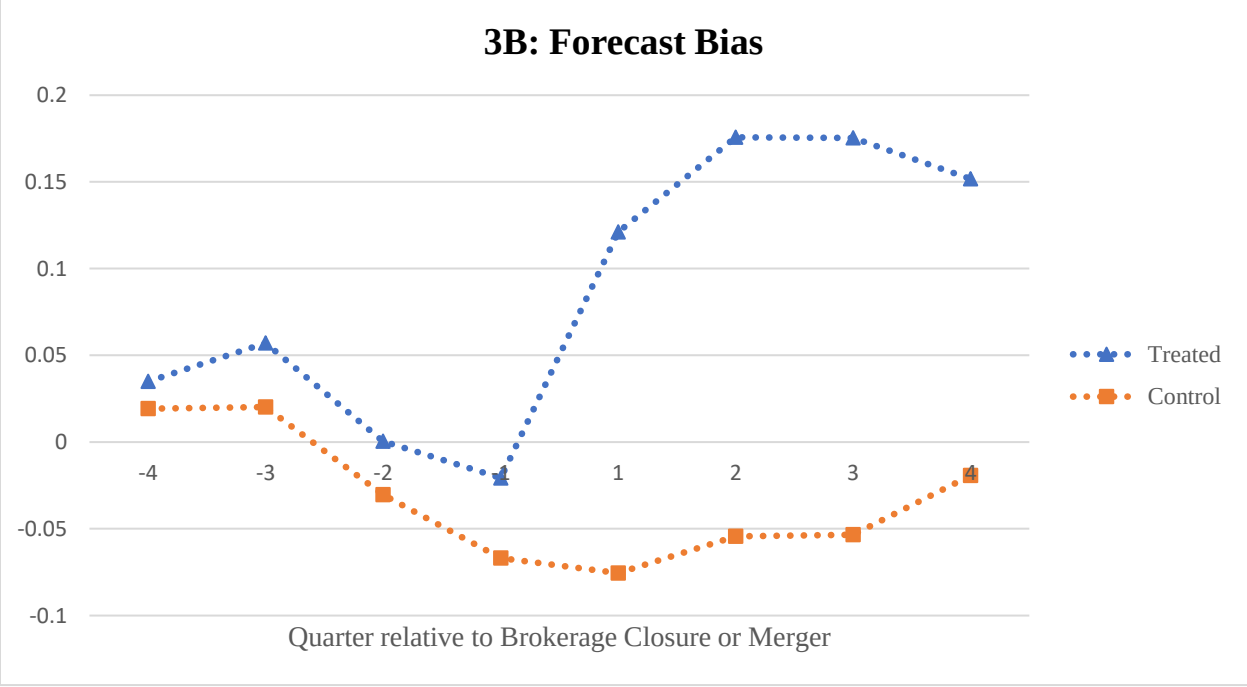

3B: Forecast Bias
0.2
0.15
0.1
0.05
0
-0.05
-0.1
-4
-3
-2
-1
1
2
3
4
Quarter relative to Brokerage Closure or Merger
Treated
Control

TABLE 1

Sample Selection

Firm-quarters from Kelly and Ljungqvist (2012) coverage terminations		4,518
Additional firm-quarters from Chen et al. (2017) coverage terminations		1,832
Total before applying filters		6,350
Less firm-quarters removed due to:		
Endogenous shocks	(864)	
Serially shocked firms	(1,715)	
Non-guiding firms	(2,678)	
No valid match within caliper of 0.005	(112)	
Eligible firm-quarters		1,093
Missing required controls	(80)	
Final Treatment Sample:		
Firm treatment quarters		901
Unique firms		606

Table 1 presents the sample selection procedure for main analyses. Kelly and Ljungqvist (2012) coverage terminations consistent of 43 brokerage house closures or mergers from 2000 to 2008. Chen et al. (2017) coverage terminations consist of an additional 20 brokerage house closures and mergers from 2008 to 2012. Endogenous shocks are those for which coverage was terminated prior to the brokerage merger or closure event date. Serially shocked observations are firm-quarters that also experienced another brokerage merger or closure event in the prior four quarters. Non-guiding firms are those that do not provide at least one earnings forecast in both the four quarters prior to and subsequent to the event quarter.

TABLE 2

Descriptive Statistics

	Treated Firms						Control Firms					Test of diff. (p-values)	
	N	**Mean**	**Lower Quartile**	**Median**	**Upper Quartile**	**Std Dev**	**Mean**	**Lower Quartile**	**Median**	**Upper Quartile**	**Std Dev**	**Mean**	**Med**
Dependent Variables:													
N_forecasts	901	3.72	2.00	4.00	5.00	2.57	3.63	2.00	4.00	5.00	2.55	0.11	0.26
Accuracy	901	-0.50	-0.28	-0.11	-0.04	1.46	-0.46	-0.29	-0.11	-0.04	1.38	0.26	0.87
Bias	901	0.02	-0.07	-0.01	0.08	0.93	0.06	-0.07	0.00	0.10	0.87	0.29	0.17
FwdTone	1314	-0.08	-0.12	-0.07	-0.04	0.05	-0.08	-0.12	-0.08	-0.05	0.05	0.79	0.75
PctFwd	1314	0.06	0.03	0.05	0.08	0.04	0.06	0.03	0.05	0.08	0.04	0.66	0.43
AIM	878	0.083	0.002	0.007	0.033	0.325	0.090	0.002	0.008	0.042	0.332	0.65	0.16
Other Variables:													
Afollow	901	5.57	2.00	3.00	8.00	6.08	5.04	1.00	3.00	7.00	5.66	0.07*	0.13
EquityIssue	901	0.40	0.00	0.00	1.00	0.49	0.42	0.00	0.00	1.00	0.49	0.36	0.36
Horizon	901	0.17	0.14	0.17	0.19	0.09	0.17	0.14	0.17	0.19	0.09	0.59	0.29
InstOwn	901	0.69	0.59	0.78	0.90	0.29	0.69	0.60	0.76	0.87	0.28	0.78	0.11
Litrisk	901	0.08	0.04	0.06	0.09	0.06	0.07	0.03	0.05	0.09	0.06	0.09	0.02
MB	901	2.19	1.32	1.82	2.54	1.35	2.20	1.30	1.80	2.60	1.35	0.89	0.91
Size	901	7.84	6.68	7.75	8.90	1.58	7.93	6.77	7.92	9.06	1.61	0.25	0.21
Volatility	901	0.03	0.02	0.02	0.03	0.01	0.03	0.02	0.02	0.03	0.01	0.00**	0.00***

Table 2 presents descriptive statistics for variables used in the main analyses. Descriptive statistics are calculated as the average pre-period values (i.e., the average of the four quarters prior to the analyst brokerage house merger or closure quarter). However, *N_forecasts* is the sum of all management earnings forecasts issued in the four quarters prior to the brokerage house closure/merger. For tests of differences, *** (**, *) denotes significance at the p<0.01 (p<0.05, p<0.10) level. All variables are defined in Appendix A.

TABLE 3

Management Forecast Frequency Following a Reduction in Analyst Coverage

	DV = log(1+N_forecasts)			*DV = N_forecasts*		
	[1]	[2]	[3]	[4]	[5]	[6]
Treat*Post	**0.046****	**0.046****	**0.048****	**0.043***	**0.043***	**0.046****
	(0.019)	**(0.018)**	**(0.016)**	**(0.060)**	**(0.059)**	**(0.036)**
Treat	-0.003	-0.007	-0.011	-0.051*	-0.049*	-0.056**
	(0.885)	(0.746)	(0.565)	(0.057)	(0.063)	(0.025)
Post	0.068**	0.068**	0.060**	0.037*	0.037*	0.030
	(0.013)	(0.013)	(0.022)	(0.067)	(0.070)	(0.105)
Size		0.000	0.000		0.000	-0.000
		(0.870)	(0.656)		(0.536)	(0.920)
Afollow		0.005**	0.005**		0.002	0.002
		(0.045)	(0.047)		(0.396)	(0.335)
MB		0.008**	0.005		0.009*	0.007
		(0.044)	(0.121)		(0.092)	(0.159)
InstOwn			0.008			-0.082
			(0.917)			(0.454)
Litrisk			-0.292			0.006
			(0.158)			(0.987)
EquityIssue			-0.007			-0.017
			(0.719)			(0.481)
Volatility			-2.519***			0.431
			(0.005)			(0.767)
Horizon			1.794***			2.414***
			(0.000)			(0.000)
Observations	3,604	3,604	3,604	3,604	3,604	3,604
Cluster	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr
Fixed Effects	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr
Model	OLS	OLS	OLS	Poisson	Poisson	Poisson

Table 3 presents coefficients (p-values) for the difference in treatment and control firms' management forecast frequency around brokerage house closures/mergers. *Treat* equals one for firms who experience loss of analyst coverage due to a brokerage house closure/merger, and zero for matched control firms. In columns 1 through 3, an OLS model is used, and the dependent variable is the natural logarithm of one plus the number of forecasts issued. In columns 4 through 6, a Poisson model is used, and the dependent variable is the number of forecasts issued. *** (**, *) denotes two-tailed significance at the p<0.01 (p<0.05, p<0.10) level. All variables are defined in Appendix A.

TABLE 4

Management Forecast Accuracy and Bias Following a Reduction in Analyst Coverage

	DV = Accuracy			*DV = Bias*		
	[1]	[2]	[3]	[4]	[5]	[6]
Treat*Post	**-0.089****	**-0.088****	**-0.090****	**0.105****	**0.105****	**0.107*****
	(0.030)	**(0.031)**	**(0.023)**	**(0.012)**	**(0.012)**	**(0.008)**
Treat	0.042	0.048	0.063	0.003	-0.018	-0.023
	(0.341)	(0.260)	(0.136)	(0.945)	(0.652)	(0.584)
Post	-0.014	-0.014	-0.006	-0.050	-0.049	-0.057
	(0.699)	(0.684)	(0.862)	(0.217)	(0.218)	(0.157)
Size		0.000*	0.000		0.000	0.000
		(0.096)	(0.206)		(0.516)	(0.231)
Afollow		0.001	0.001		0.013*	0.013*
		(0.768)	(0.876)		(0.058)	(0.089)
MB		0.015**	0.018***		-0.029***	-0.032***
		(0.016)	(0.002)		(0.006)	(0.001)
InstOwn			0.793***			-0.217
			(0.007)			(0.567)
Litrisk			1.621			-0.559
			(0.213)			(0.702)
EquityIssue			-0.009			0.043
			(0.855)			(0.374)
Volatility			-10.743*			6.843
			(0.058)			(0.174)
Horizon			-2.053***			2.020***
			(0.000)			(0.000)
Observations	3,604	3,604	3,604	3,604	3,604	3,604
Cluster	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr
Fixed Effects	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr
Adjusted R^2	0.752	0.752	0.760	0.442	0.446	0.456

Table 4 presents coefficients (p-values) for the difference in treatment and control firms' management forecast accuracy and bias around brokerage house closures/mergers. Columns 1-3 (4-6) present results where the dependent variable is *Accuracy* (*Bias*). Treat equals one for firms who experience loss of analyst coverage due to a brokerage house closure/merger, and zero for matched control firms. *** (**, *) denotes two-tailed significance at the p<0.01 (p<0.05, p<0.10) level. All variables are defined in Appendix A.

TABLE 5

Management Forecast Accuracy and Bias Following a Reduction in Analyst Coverage: Cross-sectional tests on Other Monitoring Mechanisms

Panel A: *Cross-section on Dedicated Institutional Ownership*

	DV = Accuracy		*DV = Bias*	
	[1a]	[1b]	[2a]	[2b]
	Low IO	*High IO*	*Low IO*	*High IO*
Treat*Post	**-0.159****	**-0.008**	**0.151*****	**0.055**
	(0.030)	**(0.839)**	**(0.007)**	**(0.228)**
Treat	0.230	0.018	0.066	-0.034
	(0.147)	(0.598)	(0.605)	(0.317)
Post	-0.001	-0.039	-0.045	-0.042
	(0.984)	(0.239)	(0.397)	(0.400)
Test of difference (Low IO vs High IO):	-0.151**		0.096	
Treat*Post	(0.048)		(0.135)	
Observations	1,798	1,806	1,798	1,806
Cluster	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr
Fixed Effects	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr
Adjusted R^2	0.736	0.767	0.374	0.539
Controls	Basic	Basic	Basic	Basic

Panel B: *Cross-section on Audit Fees*

	DV = Accuracy		*DV = Bias*	
	[1a]	[1b]	[2a]	[2b]
	Low Fees	*High Fees*	*Low Fees*	*High Fees*
Treat*Post	**-0.141****	**-0.054**	**0.195*****	**0.039**
	(0.020)	**(0.471)**	**(0.002)**	**(0.567)**
Treat	0.091	0.058	-0.081*	0.025
	(0.111)	(0.298)	(0.079)	(0.770)
Post	-0.023	-0.005	-0.073	-0.028
	(0.521)	(0.926)	(0.137)	(0.630)
Test of difference (Low Fees vs High Fees):	-0.087*		0.156*	
Treat*Post	(0.089)		(0.067)	
Observations	1,670	1,934	1,670	1,934
Cluster	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr
Fixed Effects	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr
Adjusted R^2	0.800	0.717	0.558	0.345
Controls	Basic	Basic	Basic	Basic

Table 5 presents coefficients (p-values) for tests of management forecast accuracy and bias based on cross-sectional tests of other monitoring mechanisms. **Panel A** presents results based on dedicated institutional ownership. A firm is considered to have Low IO (High IO) if it has (above) median dedicated institutional ownership, measured in the quarter prior to the brokerage house merger/closure. **Panel B** presents results based on excess audit fees. A firm is considered to have Low Fees (High Fees) if it has below (above) median excess audit fees, measured in the quarter prior to the brokerage house merger/closure. In all panels, columns 1a and 1b (2a and 2b) present results where the dependent variable is *Accuracy* (*Bias*). For brevity, all panels present results using "basic controls" (i.e., controls from columns 2 and 5 in Table 3: *Size*, *AFollow*, and *MB*) *** (**, *) denotes two-tailed significance at the p<0.01 (p<0.05, p<0.10) level. However, one-tailed significance is used for bolded coefficients in columns 1a and 2a. All variables are defined in Appendix A.

TABLE 6

Management Forecast Accuracy and Bias Following a Reduction in Analyst Coverage: Cross-sectional test on Managerial Incentives

	DV = Accuracy		*DV = Bias*	
	[1a]	[1b]	[2a]	[2b]
	Opp Trader	*Not Opp Trader*	*Opp Trader*	*Not Opp Trader*
Treat*Post	**-0.149****	**-0.034**	**0.156****	**0.061**
	(0.021)	**(0.717)**	**(0.026)**	**(0.363)**
Treat	0.143**	-0.301*	-0.183**	-0.002
	(0.024)	(0.072)	(0.013)	(0.964)
Post	0.037	0.073	-0.058	-0.034
	(0.514)	(0.388)	(0.386)	(0.440)
Test of difference (Opp Trader vs. Not):	-0.115		0.095	
Treat*Post	(0.155)		(0.198)	
Observations	1,848	1,756	1,848	1,756
Cluster	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr
Fixed Effects	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr
Adjusted R^2	0.748	0.767	0.347	0.513
Controls	Basic	Basic	Basic	Basic

Table 6 presents coefficients (p-values) for tests of management forecast accuracy and bias based on a cross-sectional test on managerial incentives. This table presents results based on whether the manager engages in opportunistic insider sales, as defined by Cohen, Malloy, and Pomorski (2012). A manager is considered an opportunistic trader (i.e., Columns 1a and 2a *Opp Trader*) if the manager engaged in an opportunistic insider sale during the year, and otherwise considered not to be an opportunistic trader (i.e., Columns 1b and 2b "*Not Opp Trader"*). Columns 1a and 1b (2a and 2b) present results where the dependent variable is *Accuracy* (*Bias*). For brevity, all panels present results using "basic controls" (i.e., controls from columns 2 and 5 in Table 3: *Size*, *AFollow*, and *MB*) *** (**, *) denotes two-tailed significance at the p<0.01 (p<0.05, p<0.10) level. However, one-tailed significance is used for bolded coefficients in columns 1a and 2a. All variables are defined in Appendix A.

TABLE 7

Illiquidity Following a Reduction in Analyst Coverage

	DV = Log(1+AIM)					
	[1]	[2]	[3]	[4]	[5]	[6]
Guider	0.008	0.009	0.007	0.008	0.009	0.008
	(0.197)	(0.155)	(0.230)	(0.182)	(0.138)	(0.209)
Post	0.050***	0.050***	0.050***	0.050***	0.050***	0.050***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Guider*Post	**-0.028*** **	**-0.028*** **	**-0.028*** **	**-0.038*** **	**-0.038*** **	**-0.038*** **
	(0.007)	**(0.007)**	**(0.007)**	**(0.001)**	**(0.001)**	**(0.001)**
Guider*Post*LowQuality				**0.019****	**0.019****	**0.020****
				(0.026)	**(0.026)**	**(0.026)**
Observations	3,512	3,512	3,512	3,512	3,512	3,512
Cluster	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr
Fixed Effects	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr
Controls	None	Basic	Additional	None	Basic	Additional
Adjusted R^2	0.647	0.647	0.646	0.646	0.647	0.647

Table 7 presents coefficients (p-values) for the difference in guiding and non-guiding firms' illiquidity response to a decrease in analyst following via brokerage house closures/mergers. The dependent variable is the natural logarithm of one plus Amihud's Illiquidity Measure (*AIM*). *Guider* equals one for firms that provide at least one forecast in both the four quarters before and after the brokerage closure/merger, and zero otherwise. *LowQuality* equals one for *Guider* firms whose decrease in management forecast accuracy following the brokerage closure is below the sample median. Note that for this table, all firms in the sample (Guiders and non-Guiders) experience a decrease in analyst following due to a brokerage house merger/closure. *** (**, *) denotes two-tailed significance at the p<0.01 (p<0.05, p<0.10) level. All variables are defined in Appendix A.

TABLE 8

Provision and Tone of Earnings-Related Forward-Looking Statements Following a Reduction in Analyst Coverage

	DV = PctFwd			*DV = FwdTone*		
	[1]	[2]	[3]	[4]	[5]	[6]
Treat*Post	**0.001***	**0.001***	**0.001***	**0.011****	**0.011****	**0.011****
	(0.063)	**(0.063)**	**(0.062)**	**(0.012)**	**(0.013)**	**(0.013)**
Treat	-0.000	-0.001	-0.001	-0.002	-0.002	-0.001
	(0.811)	(0.370)	(0.283)	(0.809)	(0.784)	(0.821)
Post	0.000	0.000	0.000	0.000	0.001	0.001
	(0.861)	(0.859)	(0.858)	(0.922)	(0.918)	(0.918)
Size		-0.000*	-0.000**		0.000***	0.000***
		(0.080)	(0.013)		(0.006)	(0.006)
Afollow		0.001***	0.000**		-0.000	-0.000
		(0.001)	(0.020)		(0.756)	(0.738)
MB		-0.001***	-0.001***		-0.001	-0.001
		(0.000)	(0.000)		(0.125)	(0.110)
InstOwn			0.011**			0.003
			(0.017)			(0.951)
Litrisk			0.043**			-0.055
			(0.013)			(0.649)
EquityIssue			-0.003			0.003
			(0.143)			(0.717)
Volatility			-0.030			0.294
			(0.621)			(0.572)
Observations	5,256	5,256	5,256	5,256	5,256	5,256
Cluster	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr
Fixed Effects	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr
Adjusted R^2	0.186	0.196	0.202	0.586	0.587	0.587

Table 8 presents coefficients (p-values) for the difference in treatment and control firms' forward-looking statements in earnings announcements around brokerage house closures/mergers. Columns 1-3 (4-6) present results where the dependent variable is *PctFwd* (*FwdTone*). *Treat* equals one for firms who experience loss of analyst coverage due to a brokerage house closure/merger, and zero for matched control firms. *** (**, *) denotes two-tailed significance at the p<0.01 (p<0.05, p<0.10) level. All variables are defined in Appendix A.

TABLE 9

Provision and Tone of Earnings-Related Forward-Looking Statements Following a Reduction in Analyst Coverage: Cross-sectional tests on other Monitoring Mechanisms

Panel A: *Cross-section on Dedicated Institutional Ownership*

	DV = PctFwd		*DV = FwdTone*	
	[1a]	[1b]	[2a]	[2b]
	Low IO	*High IO*	*Low IO*	*High IO*
Treat*Post	**0.002****	**0.000**	**0.012****	**0.009**
	(0.028)	**(0.826)**	**(0.028)**	**-0.185**
Treat	-0.003	0.001	-0.009	0.000
	(0.191)	(0.397)	(0.324)	(0.997)
Post	-0.000	0.001	0.002	-0.000
	(0.568)	(0.405)	(0.769)	(0.948)
Test of difference (Low IO vs High IO):	0.002		0.003	
Treat*Post	(0.161)		(0.481)	
Observations	2,596	2,660	2,596	2,660
Cluster	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr
Fixed Effects	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr
Adjusted R^2	0.669	0.673	0.591	0.62
Controls	Basic	Basic	Basic	Basic

Panel B: *Cross-section on Audit Fees*

	DV = PctFwd		*DV = FwdTone*	
	[2a]	[2b]	[2a]	[2b]
	Low Fees	*High Fees*	*Low Fees*	*High Fees*
Treat*Post	**0.001**	**0.001**	**0.008**	**0.013***
	(0.164)	**(0.345)**	**(0.126)**	**-0.057**
Treat	-0.003	-0.004**	0.003	-0.007
	(0.141)	(0.034)	(0.707)	(0.509)
Post	0.000	0.000	0.004	-0.003
	(0.933)	(0.693)	(0.532)	(0.693)
Test of difference (Low Fees vs High Fees):	0.000		-0.005	
Treat*Post	(0.936)		(0.722)	
Observations	2,614	2,642	2,614	2,642
Cluster	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr
Fixed Effects	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr
Adjusted R^2	0.669	0.666	0.639	0.566
Controls	Basic	Basic	Basic	Basic

Table 9 presents coefficients (p-values) for tests of provision of and net positivity of earnings-related forward-looking statements based on cross-sectional tests on other monitoring mechanisms. **Panel A** presents results based on dedicated institutional ownership. A firm is considered to have Low IO (High IO) if it has (above) median dedicated institutional ownership, measured in the quarter prior to the brokerage house merger/closure. **Panel B** presents results based on excess audit fees. A firm is considered to have Low Fees (High Fees) if it has below (above) median excess audit fees, measured in the quarter prior to the brokerage house merger/closure. In all panels, columns 1a and 1b (2a and 2b) present results where the dependent variable is *PctFwd* (*FwdTone*). For brevity, all panels present results using "basic controls" (i.e., controls from columns 2 and 5 in Table 3: *Size*, *AFollow*, and *MB*) *** (**, *) denotes two-tailed significance at the p<0.01 (p<0.05, p<0.10) level. However, one-tailed significance is used for bolded coefficients in columns 1a and 2a. All variables are defined in Appendix A.

TABLE 10

Provision and Tone of Earnings-Related Forward-Looking Statements Following a Reduction in Analyst Coverage: Cross-sectional test on Managerial Incentives

	DV = PctFwd		*DV = FwdTone*	
	[1a]	[1b]	[2a]	[2b]
	Opp Trader	*Not Opp Trader*	*Opp Trader*	*Not Opp Trader*
Treat*Post	**0.001**	**0.001**	**0.024****	**-0.002**
	(0.161)	**(0.164)**	**(0.014)**	**(0.865)**
Treat	-0.004*	-0.002	0.004	-0.004
	(0.086)	(0.221)	(0.785)	(0.761)
Post	-0.000	0.001	-0.005	0.006
	(0.678)	(0.428)	(0.616)	(0.266)
Test of difference (Opp Trader vs. Not):	0.000		0.026*	
Treat*Post	(0.891)		(0.089)	
Observations	2,554	2,702	2,554	2,702
Cluster	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr
Fixed Effects	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr
Adjusted R^2	0.659	0.684	0.598	0.599
Controls	Basic	Basic	Basic	Basic

Table 10 presents coefficients (p-values) for tests of management forecast accuracy and bias based on a cross-sectional test on managerial incentives. This table presents results based on whether the manager engages in opportunistic insider sales, as defined by Cohen, Malloy, and Pomorski (2012). A manager is considered an opportunistic trader (i.e., Columns 1a and 2a *Opp Trader*) if the manager engaged in an opportunistic insider sale during the year, and otherwise considered not to be an opportunistic trader (i.e., Columns 1b and 2b "*Not Opp Trader"*). Columns 1a and 1b (2a and 2b) present results where the dependent variable is *PctFwd* (*FwdTone*). For brevity, all panels present results using "basic controls" (i.e., controls from columns 2 and 5 in Table 3: *Size*, *AFollow*, and *MB*) *** (**, *) denotes two-tailed significance at the p<0.01 (p<0.05, p<0.10) level. However, one-tailed significance is used for bolded coefficients in columns 1a and 2a. All variables are defined in Appendix A.

TABLE 11

Illiquidity Following a Reduction in Analyst Coverage, Conditional on Forward-Looking Statement Quantity and Quality

	DV = Log(1+AIM)					
	[1]	[2]	[3]	[4]	[5]	[6]
Guider	-0.004	0.001	-0.006	-0.013	-0.013	-0.010
	(0.741)	(0.943)	(0.727)	(0.210)	(0.210)	(0.357)
Post	0.036**	0.037**	0.031**	0.036	0.036	0.028*
	(0.043)	(0.041)	(0.026)	(0.137)	(0.137)	(0.092)
TextGuider*Post	**-0.007**	**-0.007**	**-0.009**	**-0.001**	**0.000**	**0.006**
	(0.272)	**(0.172)**	**(0.265)**	**(0.961)**	**(0.997)**	**(0.839)**
TextGuider*Post*LowQualityText				**-0.004**	**-0.005**	**-0.013**
				(0.897)	**(0.851)**	**(0.662)**
Observations	5,188	5,188	5,188	5,188	5,188	5,188
Cluster	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr
Fixed Effects	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr	firm & yr-qtr
Controls	None	Basic	Additional	None	Basic	Additional
Adjusted R^2	0.116	0.182	0.289	0.816	0.816	0.825

Table 11 presents coefficients (p-values) for the difference in the illiquidity response to a decrease in analyst following via brokerage house closures/mergers based on the change in amount and tone of forward-looking statements in earnings announcement press releases. The dependent variable is the natural logarithm of one plus Amihud's Illiquidity Measure (*AIM*). *TextGuider* is equal to one for firms whose increase in forward-looking earnings-related statements following the brokerage closure is above the sample median, and zero otherwise. *LowQualityText* equals one for firms whose increase in tone of forward-looking earnings-related statements following the brokerage closure is above the sample median, and zero otherwise. Note that for this table, all firms in the sample experience a decrease in analyst following due to a brokerage house merger/closure. *** (**, *) denotes two-tailed significance at the p<0.01 (p<0.05, p<0.10) level. All variables are defined in Appendix A.