Internet Appendix:

The Information Role of the Media in Earnings News

January 2021

Abstract

This Internet Appendix provides supplementary results for the paper “The Information Role of the Media in Earnings News”, including additional evidence on (1) industry and pseudo-firm fixed effects, (2) pre-treatment parallel trends, (3) whether results arise around pseudo-events, (4) the potential for media coverage to lead to investor overreaction and subsequent reversal, (5) nonlinearities in the earnings response coefficient, (6) the decomposition of trading volume into retail and institutional components, and (7) whether increases and decreases in coverage have asymmetric impacts on market reactions.

Keywords: Business journalism, media, price discovery, volume, returns, earnings

JEL Codes: M40, M41, G10, G11, G12, G14, G20
IA1. Industry and Pseudo-Firm Fixed Effects

As discussed in the main paper, firm fixed effects are not feasible in the ERC regressions of equation (1) and Table 4 because interactions between each firm fixed effect and Surprise would be necessary to provide a within firm analysis, drastically reducing degrees of freedom. However, firm fixed effects would help control for fixed differences across sample firms. Thus, to address this concern using a close alternative, I follow Gipper et al. (2020) and confirm that my estimates of equation (1) are robust to including fixed effects (and their interactions with Surprise) for the 12 Fama-French industries or 100 “pseudo-firms” that group firms with similar Market Cap and Book-To-Market. The Fama-French 12 industry definitions are available on Kenneth French’s data website. To create pseudo-firms I (independently) sort firms into size and book-to-market deciles and assign firms to the resulting 100 (10x10) size/book-to-market combinations.

The magnitude and statistical significance of the Surprise and Surprise $\times$ Treat $\times$ TreatYr coefficients in Panels A and B of IA Table 1 are almost identical to those in Table 4. This evidence suggests that my results are unlikely to be explained by time-invariant industry or firm characteristics.

IA2. Pre-Treatment Parallel Trends

The identifying parallel-trends assumption of the models tested in Section 4 is that the dependent variables would have varied similarly across time in the treatment and control groups in a counterfactual world without WSJ restructuring events. Although I cannot observe what the trends in the dependent variables would have been without the WSJ experiments, in this section, I test for parallel trends across groups in the pre-treatment periods. Finding pre-treatment parallel trends gives some comfort that there would also have been parallel trends in the post-period absent treatment.

To test for a trend, I regress each of my three dependent variables on a variable, Trend, that increases from one in the first quarter of the year to four in the last quarter of the year,
and the interaction of Trend with an indicator for treatment firms, Treat. I estimate this regression for each pre-treatment year: 2006, 2007, and the fourth quarter of 2012 through the third quarter of 2013. The main effect of Trend is subsumed by year-quarter fixed effects. In Panel A (Panels B and C) the coefficient on Surprise × Trend × Treat (Trend × Treat) reflects the difference in trends between the treatment and control groups. In Panels A-C of IA Table 2, none of these coefficients are significantly different from zero, suggesting the treatment and control groups have similar pre-treatment trends.

IA3. Pseudo-Events

Another way to investigate the parallel trends assumption is to assign treatment around a pseudo-event during which there was no treatment. Finding the supposed treatment effect around a pseudo-event during the pre-treatment period would suggest treatment and control firms were already trending apart before the events. Finding no treatment effect instead would suggest that treatment and control firms were similar in the pre-treatment period and the treatment effect arose at the time of treatment, which are the WSJ restructuring events in this case.

Since the pre-treatment period in my setting is one year, I split this period in half and assign treatment at pseudo-events six months earlier than the respective WSJ restructuring. Because the changes in coverage that accompanied the restructurings actually happened later, I eliminate more observations relating to changes that went in the opposite direction predicted by the actual restructuring than in the main sample (901 vs. 238). As a result, the samples in IA Table 3 are smaller than the respective samples in Tables 4 and 7. The sample differences do not invalidate this pseudo-event test since any non-parallel trend would have to coincide with the direction of treatment assignment in the main tests. Also, because I use events that both increased and decreased coverage, an omitted variable driving a non-parallel trend would need to switch direction depending on the event. But this does not appear to be the case. Consistent with there being parallel trends in the pre-period and the treatment effect arising later, specifically at the WSJ restructurings, the coefficients of interest in IA...
Table 3 are economically small and none are significantly different from zero.

IA4. Overreaction

Throughout the paper, I assume higher earnings response coefficients, intraperiod timeliness, and abnormal trading volume indicate a more efficient market response to earnings. However, it is well known that there is little, if any, mispricing of large firms’ earnings (Foster, Olsen, and Shevlin, 1984; Bernard and Thomas, 1989). Hence, it is possible that my results reflect investor overreaction induced by WSJ earnings articles. In this section, I test this alternative explanation by considering whether WSJ articles cause post-earnings-announcement reversal, the opposite of post-earnings-announcement drift, which would indicate earnings-announcement overreaction.

In IA Table 4, I present a test that considers the relationship between abnormal returns and earnings surprise in the quarter following the earnings announcement. Specifically, I repeat the tests in Table 4 using abnormal returns during the 75 trading days following the earnings announcement, instead of abnormal announcement returns, as the dependent variable. I include the subsequent earnings announcement because prior research consistently finds that a significant proportion of earnings mispricing is corrected around future earnings announcements. Consistent with prior research, I do not find post-earnings-announcement drift or reversal in my sample of S&P 500 firms. More importantly, the coefficients on Surprise $\times$ Treat $\times$ TreatYr are statistically and economically insignificant, suggesting WSJ articles do not induce overreaction. Instead, my results are consistent with WSJ articles helping investors fully impound earnings news into price a few hours or days earlier than they would otherwise.

IA5. Nonlinearity

Prior researchers have long found that losses are less persistent than profits (Hayn, 1995) and that extreme earnings surprises are less persistent than small surprises (Freeman and Tse, 1992). Failing to model these nonlinearities likely at least partially explains the low
ERC in my main tests and may bias my estimates to the extent that treatment and control firms differ in their propensity to report losses or extreme surprises. At the very least, my result may not generalize to all S&P 500 firms and instead apply to only firms with small profits. To address these possibilities, in this section I add controls for extreme earnings surprise and losses to the main ERC model.

Specifically, following Gipper, Leuz, and Maffett (2020), I define Nonlinear as the product of Surprise and the absolute value of Surprise, the equivalent of including a cubic term. Also, Loss is an indicator set to one if earnings per share is negative. Because Bad News, or negative earnings surprise, is highly correlated with Nonlinear and Loss, I exclude it from the set of control variables in all but the last tests in this section.

Consistent with prior research, Panel A of IA Table 5 shows that the coefficient Nonlinear is negative and marginally significant. Also, including Nonlinear slightly increases the coefficient on Surprise. Importantly, the coefficient on \( \text{Surprise} \times \text{Treat} \times \text{TreatYr} \) is still significant in all specifications. Finally, the negative coefficient on the interaction between the treatment indicator and Nonlinear indicates that the treatment effect is significantly stronger for small surprises than extreme surprises. This may be because earnings surprises are relatively small in the rich information environment of the relatively stable firms in my sample.

Similarly, Panel B of IA Table 5 shows that my results hold when I include indicators for negative earnings (Loss) and negative earnings surprise (Bad News). I also find that the treatment effect does not differ between profit and loss (or good and bad news) firms. Again, this could be due to the relatively low information content of earnings announcements in the S&P 500. Overall, these results corroborate the main findings and indicate that, while somewhat helpful, modeling nonlinearities in the ERC is not crucial to making correct inferences in my setting.
IA6. Retail vs. Institutional Trading

On the one hand, a preponderance of intuition and evidence suggests institutional investors are more sophisticated than retail investors, on average. So editorial content in earnings-related press coverage may help retail investors more, while institutional investors may rely primarily on their own analyses or those of potentially more sophisticated intermediaries, such as sell-side analysts.

On the other hand, active mutual funds do not even outperform the market before fees on average (see Fama and French, 2010), suggesting many institutional investors could use help with their investment theses. Perhaps fund managers and buy-side analysts, whose portfolios typically span dozens of industries and firms, are spread more thin than journalists who often cover only a handful of firms each. Or perhaps the prudent-person rule makes fund managers appreciate media coverage they can use as evidence that prudent market participants support their investment thesis. So the media’s earnings editorial content could also help institutional investors. In this section, I address this question empirically by separately considering retail and institutional earnings-announcement trading volume.

Boehmer, Jones, Zhang, and Zhang (2020) develop a methodology that determines whether trades recorded in the Trade and Quote (TAQ) database are initiated by a retail or institutional investor. Most retail investors’ trades are not executed on a registered exchange, instead they are filled from their broker’s own inventory or sent to a wholesaler. The counterparty who internalizes or wholesales the retail order typically pays for this order flow by agreeing to buy (sell) for a fraction of a cent (e.g., 0.01 cents, 0.1 cent, and 0.2 cents) more (less) than the National Best Bid (Offer). However, institutional orders are almost always executed on exchanges or dark pools that are prohibited from having subpenny limit prices by Regulation NMS, resulting in execution prices in round or half pennies.

For each firm-announcement in my study, I first define Abnormal Retail Volume (Abnormal Inst. Volume) as the difference between average daily share turnover initiated by retail (institutional) investors during the earnings announcement and during the 60 prior trading
days. I also consider the ratio of retail investor trading volume to total trading volume during the earnings announcement window, $\% \text{Retail Volume}$. IA Table 6 reports analyses based on this decomposition. Panel A reports distributional statistics for the retail and institutional volume dependent variables. On average, retail trading volume increases by about 4 bps during earnings announcements, and institutional trading volume increases by about 69 bps. As one would hope, the two components sum to roughly the total abnormal trading volume shown in Table 2. Retail investors initiate approximately 4.2% of the average total shares traded during earnings announcements.

Panel B shows difference-in-differences estimates from regressions that use the measures of retail and institutional volume as dependent variables. Perhaps surprisingly, I find evidence consistent with the findings in Panel B of Table 7 for both types of investor; that is, WSJ articles increase both retail and institutional trading volume. Specifically, the coefficients on $\text{Treat} \times \text{TreatYr}$ are positive and statistically and economically significant in both the abnormal retail and institutional volume regressions. The coefficient on $\text{Treat} \times \text{TreatYr}$ in the $\% \text{Retail Volume}$ regression leads to a similar inference, that WSJ articles do not make retail investors trade significantly more or less relative to institutional investors during earnings announcements. This evidence nicely complements the study’s main findings, highlighting journalists’ broad readership and resulting potential to inform a diverse set of market participants.

**IA7. Increase vs. Decrease in Coverage**

This section examines the potential for increases and decreases in media coverage to asymmetrically influence investors’ responses to earnings announcements. For example, one potential role of media coverage discussed in the main paper is to make investors more aware of value-relevant information provided by firms and other intermediaries. Thus, an increase in coverage could make investors aware of many more information sources, leading to increases in the earnings response. However, a decrease in coverage would not necessarily make investors forget or neglect the information sources to which prior media coverage called
attention, possibly resulting in no change in the earnings response.

To test this hypothesis, I separately analyze the coverage decreasing and increasing events and contrast the findings with the main tests in the paper that pool coverage decreasing and increasing events. Specifically, in IA Table 7 I repeat the fully specified regression in Table 4 and the “Baseline” regressions in Table 7 after separating the sample into the quarters surrounding the coverage decreasing (i.e., first) event and the coverage increasing (i.e., second and third) events. Panel A considers coverage decreases and shows that for all three dependent variables the coefficients of interest have the same sign as in the main results, but they are not statistically significant. In contrast, Panel B considers coverage increases and shows that for all three dependent variables the coefficients of interest are positive and statistically significant, as in the main results.

This evidence is consistent with the story outlined above, i.e., that coverage increases have a greater impact on earnings responses than coverage decreases. However, note that the coefficient in the abnormal volume regression of Panel A is economically large (0.074) even though it is not statistically significant. This suggests the possibility that the differences documented in IA Table 7 are due to insufficient statistical power in the Panel A test instead of actual asymmetric economic impacts.
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IA Table 1. Industry and Pseudo-Firm Fixed Effects

This table shows difference-in-difference estimates from the earnings response regressions in Table 4 after including fixed effects for the 12 Fama-French industries (Panel A) or 100 “pseudo-firms” that group firms with similar Market Cap and Book-To-Market (Panel B). Some regressions also include the interactions of these fixed effects with Surprise. The Fama-French 12 industry definitions are available on Kenneth French’s data website. To create pseudo-firms I (independently) sort firms into size and book-to-market deciles and assign firms to the resulting 100 (10x10) size/book-to-market combinations. Other variables are defined in Table 2. The sample consists of 5,347 S&P 500 earnings announcements within a year of three WSJ restructuring events. I standardize continuous variables to have unit variance to facilitate the interpretation of coefficients. For brevity, I do not report coefficients for the control variables. t-statistics are based on standard errors that are clustered by firm. All regressions include year-quarter fixed effects, and some regressions include the interaction of these effects with Surprise. *** *, and * indicate significance at the 1, 5, and 10 percent two-tailed level, respectively.

Panel A: Fama-French 12 Industries

| Variable | Coef. | t-stat. | Coef. | t-stat. | Coef. | t-stat. | Coef. | t-stat. |
|----------|-------|---------|-------|---------|-------|---------|-------|---------|
| Surprise | 0.178*** | 12.95 | 0.147*** | 7.34 | 0.065 | 0.25 | 0.125 | 0.47 |
| Surprise × Treat × TreatYr | 0.058** | 2.23 | 0.048* | 1.86 | 0.065 | 0.66 | 0.042* | 0.67 |
| Surprise × Treat | -0.015 | -0.59 | 0.003 | 0.10 | 0.001 | 0.06 |
| Surprise × TreatYr | 0.039* | 1.92 | 0.042* | 1.66 | 0.042* | 1.67 |
| Treat | 0.029 | 1.07 | 0.032 | 1.17 | 0.031 | 1.15 |
| Treat × TreatYr | -0.030 | -1.16 | -0.030 | -1.47 | -0.040 | -1.49 |
| Controls? | No | No | Yes | Yes |
| Surprise × Controls? | No | No | Yes | Yes |
| Yr.-Qtr. FEs? | Yes | Yes | Yes | Yes |
| Surprise × Yr.-Qtr. FEs? | No | No | No | Yes |
| Industry FEs? | Yes | Yes | Yes | Yes |
| Surprise × Industry FEs? | No | No | No | Yes |
| R² | 0.0346 | 0.0373 | 0.0713 | 0.0768 |
| Observations | 5,347 | 5,347 | 5,347 | 5,347 |

Panel B: Pseudo-Firm 100 Portfolios

| Variable | Coef. | t-stat. | Coef. | t-stat. | Coef. | t-stat. | Coef. | t-stat. |
|----------|-------|---------|-------|---------|-------|---------|-------|---------|
| Surprise | 0.183*** | 13.48 | 0.152*** | 7.68 | 0.094 | 0.34 | 0.042 | 0.15 |
| Surprise × Treat × TreatYr | 0.057** | 2.17 | 0.047* | 1.79 | 0.050* | 1.91 |
| Surprise × Treat | -0.016 | -0.66 | 0.001 | 0.03 | 0.005 | 0.20 |
| Surprise × TreatYr | 0.041** | 2.02 | 0.039 | 1.57 | 0.037 | 1.49 |
| Treat | 0.029 | 1.05 | 0.030 | 1.11 | 0.029 | 1.03 |
| Treat × TreatYr | -0.035 | -1.34 | -0.032 | -1.19 | -0.031 | -1.13 |
| Controls? | No | No | Yes | Yes |
| Surprise × Controls? | No | No | Yes | Yes |
| Yr.-Qtr. FEs? | Yes | Yes | Yes | Yes |
| Surprise × Yr.-Qtr. FEs? | No | No | No | Yes |
| Pseudo-Firm FEs? | Yes | Yes | Yes | Yes |
| Surprise × Pseudo-Firm FEs? | No | No | No | Yes |
| R² | 0.0541 | 0.0658 | 0.0832 | 0.0972 |
| Observations | 5,347 | 5,347 | 5,347 | 5,347 |
IA Table 2. Pre-Treatment Parallel Trends

This table shows estimates from regressions that test for parallel trends in the dependent variables in the pre-treatment periods. In Panel A (Panel B) [Panel C], I estimate a regression for each pre-treatment year (2006, 2007, and 2012q4 through 2013q3) of earnings response coefficient (intraperiod timeliness) [abnormal volume] on the interaction between a trend variable that increases by one each quarter (Trend) and an indicator for treatment firms (Treat). In Panel A, the main effect of Trend is subsumed by year-quarter fixed effects. In Panel B and C, the main effect of Trend is subsumed by year-quarter fixed effects, and the main effect of Treat is subsumed by firm fixed effects. Variables are defined in Table 2. t-statistics are based on standard errors that are clustered by firm. The Panel A regressions include year-quarter fixed effects and the interaction of these effects with Surprise. The Panel B and C regressions include firm and year-quarter fixed effects. ***, **, and * indicate significance at the 1, 5, and 10 percent two-tailed level, respectively.

### Panel A: Earnings Response Coefficients

| Variable          | 2006       | 2007       | 2013       |
|-------------------|------------|------------|------------|
|                   | Coef.      | t-stat.    | Coef.      | t-stat.    | Coef.      | t-stat.    |
| Surprise          | 0.198***   | 3.31       | 0.046      | 0.70       | 0.154***   | 3.56       |
| Surprise × Trend × Treat | -0.119     | -1.12      | -0.010     | -0.12      | -0.028     | -0.44      |
| Trend × Treat     | -0.001     | -0.01      | 0.125      | 0.92       | 0.053      | 0.78       |
| Treat             | -0.014     | -0.22      | -0.093     | -1.13      | 0.005      | 0.11       |
| Yr.-Qtr. FEs?     | Yes        | Yes        | Yes        |
| Surprise × Yr.-Qtr. FEs? | Yes        | Yes        | Yes        |
| R²                | 0.0642     | 0.0567     | 0.0315     |
| Observations      | 594        | 937        | 1,341      |

### Panel B: Intraperiod Timeliness

| Variable          | 2006       | 2007       | 2013       |
|-------------------|------------|------------|------------|
|                   | Coef.      | t-stat.    | Coef.      | t-stat.    | Coef.      | t-stat.    |
| Trend × Treat     | -0.049     | -0.43      | 0.009      | 0.09       | -0.004     | -0.05      |
| Firm & Yr.-Qtr. FEs? | Yes        | Yes        | Yes        |
| R²                | 0.0213     | 0.0170     | 0.0050     |
| Observations      | 445        | 696        | 968        |

### Panel C: Abnormal Volume

| Variable          | 2006       | 2007       | 2013       |
|-------------------|------------|------------|------------|
|                   | Coef.      | t-stat.    | Coef.      | t-stat.    | Coef.      | t-stat.    |
| Trend × Treat     | -0.045     | -0.44      | -0.011     | -0.13      | 0.045      | 0.65       |
| Firm & Yr.-Qtr. FEs? | Yes        | Yes        | Yes        |
| R²                | 0.0415     | 0.0524     | 0.0787     |
| Observations      | 594        | 937        | 1,341      |
IA Table 3. Pseudo-Events

This table shows difference-in-difference estimates from the regressions in Table 4 and the “Baseline” regressions in Table 7 after assigning treatment at pseudo-events defined six months earlier than the actual WSJ restructurings. Specifically, the pseudo-(actual)-events take place at the beginning of 2006q3 (2007q1), 2007q3 (2008q1), and 2013q2 (2013q4). The resulting samples are smaller than those in Tables 4 and 7 because fewer observations meet the criteria, described in Section 3.3 of the main paper, for inclusion in either the treatment or control group. Variables are defined in Table 2. I standardize continuous variables to have unit variance to facilitate the interpretation of coefficients. For brevity, I do not report coefficients for the control variables. t-statistics are based on standard errors that are clustered by firm. All the Panel A regressions include year-quarter fixed effects, and some Panel A regressions include the interaction of these effects with Surprise. The Panel B and C regressions include firm and year-quarter fixed effects. ***, **, and * indicate significance at the 1, 5, and 10 percent two-tailed level, respectively.

Panel A: Earnings Response Coefficients

| Variable               | Abnormal Returns         | Coef. | t-stat. | Coef. | t-stat. | Coef. | t-stat. |
|------------------------|--------------------------|-------|---------|-------|---------|-------|---------|
| Surprise               |                          | 0.142*** | 6.26    | 0.127 | 0.48    | 0.303 | 0.88    |
| Surprise × Treat × TreatYr |                      | -0.002  | -0.04   | 0.006 | 0.23    | -0.008 | -0.19   |
| Surprise × Treat        |                          | -0.001  | -0.03   | 0.015 | 0.53    | -0.002 | -0.05   |
| Surprise × TreatYr      |                          | 0.026   | 1.10    | 0.033*| 1.72    | 0.009  | 0.29    |
| Treat × TreatYr         |                          | -0.005  | -0.11   | 0.018 | 0.80    | 0.006  | 0.13    |
| Treat                   |                          | -0.003  | -0.05   | -0.026| -0.93   | -0.012 | -0.23   |

| Controls?              | Yes                     |       |         | Yes    |       |       |         |
| Surprise × Controls?   | No                      |       |         | Yes    |       |       |         |
| Yr.-Qtr. FEs?         | Yes                     |       |         | Yes    |       |       |         |
| Surprise × Yr.-Qtr. FEs? | No                   |       |         | Yes    |       |       |         |
| R²                     |                         | 0.0294 |        | 0.0782 |        | 0.0826 |       |
| Observations           |                         | 5,079  |        | 5,079  |        | 5,079  |       |

Panel B: Intraperiod Timeliness

| Variable               | Intraperiod Timeliness | Coef. | t-stat. | Coef. | t-stat. |
|------------------------|------------------------|-------|---------|-------|---------|
| Treat × TreatYr        |                        | 0.054 | 1.42    | 0.046 | 1.22    |

| Controls?              | Yes                     |       |         |       |         |
| Firm & Yr.-Qtr. FEs?   | Yes                     |       |         | Yes    |         |
| R²                     |                         | 0.0125 |        | 0.0200 |        |
| Observations           |                         | 3,817  |        | 3,817  |        |

Panel C: Abnormal Volume

| Variable               | Abnormal Volume         | Coef. | t-stat. | Coef. | t-stat. |
|------------------------|-------------------------|-------|---------|-------|---------|
| Treat × TreatYr        |                         | 0.023 | 0.62    | 0.011 | 0.31    |

| Controls?              | Yes                     |       |         |       |         |
| Firm & Yr.-Qtr. FEs?   | Yes                     |       |         | Yes    |         |
| R²                     |                         | 0.0522 |        | 0.0749 |        |
| Observations           |                         | 5,079  |        | 5,079  |        |

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IA Table 4. Overreaction

This table shows difference-in-differences estimates from post-earnings-announcement returns regressions. I regress abnormal returns during the 75 trading days after the earnings announcement (Abnormal Returns (+2,+75)) on earnings surprise, the treatment indicator, and control variables. Variables are defined in Table 2. The sample consists of 5,347 S&P 500 earnings announcements within a year of three WSJ restructuring events. I standardize continuous variables to have unit variance to facilitate the interpretation of coefficients. For brevity, I do not report coefficients for the control variables. t-statistics are based on standard errors that are clustered by firm. All regressions include year-quarter fixed effects, and the second regression includes the interaction of these effects with Surprise. ***, **, and * indicate significance at the 1, 5, and 10 percent two-tailed level, respectively.

| Variable | Coef. | t-stat. | Coef. | t-stat. |
|----------|-------|---------|-------|---------|
| Surprise | 0.004 | 0.15    | 0.095 | 0.31    |
| Surprise × Treat × TreatYr | -0.003 | -0.10 | -0.004 | -0.12 |
| Surprise × Treat | -0.038 | -1.17 | -0.014 | -0.45 |
| Surprise × TreatYr | 0.020 | 0.86 | 0.022 | 0.75 |
| Treat × TreatYr | -0.039 | -1.20 | -0.034 | -1.15 |
| Treat | 0.055 | 1.15 | 0.043 | 0.91 |
| Controls? | No | Yes |
| Surprise × Controls? | No | Yes |
| Yr.-Qtr. FEs? | Yes | Yes |
| Surprise × Yr.-Qtr. FEs? | No | Yes |
| R² | 0.0184 | 0.1046 |
| Observations | 5,347 | 5,347 |
IA Table 5. Nonlinearities in Earnings Response

This table shows difference-in-differences estimates from earnings response (ERC) regressions that account for nonlinearities in the ERC. I regress abnormal returns on earnings surprise, the treatment indicator, interactions between these variables and controls for nonlinearity, and other control variables. Nonlinear is the product of Surprise and the absolute value of Surprise, the equivalent of including a cubic term. Loss is an indicator set to one if earnings per share is negative. Other than in the last two regressions of Panel B, Bad News is excluded from the set of control variables because it is highly correlated with Nonlinear and Loss. Other variables are defined in Table 2. The sample consists of 5,347 S&P 500 earnings announcements within a year of three WSJ restructuring events. I standardize continuous variables to have unit variance to facilitate the interpretation of coefficients. For brevity, I only report coefficients for the control variables of interest (i.e., Nonlinear, Loss, and Bad News). t-statistics are based on standard errors that are clustered by firm. All regressions include year-quarter fixed effects, and some regressions include the interaction of these effects with Surprise. ***, **, and * indicate significance at the 1, 5, and 10 percent two-tailed level, respectively.

### Panel A: Control for Nonlinearity

| Variable | Coef. | t-stat. | Coef. | t-stat. | Coef. | t-stat. | Coef. | t-stat. |
|----------|-------|---------|-------|---------|-------|---------|-------|---------|
| Surprise | 0.122*** | 7.07 | 0.124*** | 7.31 | 0.043 | 0.16 | 0.034 | 0.13 |
| Surprise × Treat × TreatYr | 0.047* | 1.82 | 0.218** | 2.41 |
| Nonlinear: Surprise × | | | | |
| Surprise | -0.004 | -1.43 | -0.004* | -1.68 | -0.004 | -1.61 |
| Treat × TreatYr × Nonlinear | -0.175** | -2.04 |
| Treat × TreatYr | 0.010 | 0.44 | 0.009 | 0.38 |
| Surprise × Treat × Nonlinear | -0.004 | -0.20 |
| Surprise × Treat | 0.003 | 0.13 | 0.004 | 0.16 |
| Surprise × TreatYr × Nonlinear | 0.021 | 1.37 |
| Surprise × TreatYr | 0.048* | 1.93 | 0.058** | 2.31 |
| Treat × Nonlinear | 0.001 | 0.05 |
| Treat | 0.035** | 2.24 | 0.033** | 2.12 |

| Controls? | Yes | Yes | Yes | Yes |
| Surprise × Controls? | No | No | Yes | Yes |
| Yr.-Qtr. FEs? | Yes | Yes | Yes | Yes |
| Surprise × Yr.-Qtr. FEs? | No | No | Yes | Yes |
| R² | 0.0679 | 0.0683 | 0.0725 | 0.0735 |
| Observations | 5,347 | 5,347 | 5,347 | 5,347 |
### Panel B: Control and Interaction for Loss Firms

**Dependent Variable = Abnormal Returns**

| Variable                        | Coef. | t-stat. | Coef. | t-stat. | Coef. | t-stat. | Coef. | t-stat. |
|---------------------------------|-------|---------|-------|---------|-------|---------|-------|---------|
| **Surprise**                    | 0.061 | 0.23    | 0.050 | 0.19    | 0.073 | 0.27    | 0.050 | 0.19    |
| **Loss**                        | -0.074| -1.39   | -0.072| -1.34   | -0.071| -1.33   | -0.070| -1.31   |
| **Surprise × Loss**             | -0.064| -1.55   | -0.063| -1.52   | -0.060| -1.42   | -0.068| -1.64   |
| **Surprise × Treat × TreatYr**  | 0.072***| 2.62 | 0.241***| 2.65 | 0.072***| 2.62 | 0.230**| 2.52 |
| $^* \times \text{Loss}$         | -0.010| -0.10   | -0.009| -0.10   | -0.010| -0.11   | -0.010| -0.10   |
| **Treat × TreatYr × Loss**      | 0.072 | 0.90    | 0.074 | 0.94    | 0.071 | 0.88    | 0.073 | 0.92    |
| **Treat × TreatYr**             | 0.001 | 0.05    | -0.002| -0.07   | 0.001 | 0.05    | -0.002| -0.09   |
| **Surprise × Treat × Loss**     | 0.136***| 2.85 | 0.142***| 2.79 | 0.136***| 2.86 | 0.147***| 2.89 |
| **Surprise × Treat**            | -0.024| -0.85   | -0.022| -0.78   | -0.024| -0.85   | -0.022| -0.77   |
| **Surprise × TreatYr × Loss**   | 0.043 | 0.94    | 0.056 | 1.21    | 0.043 | 0.95    | 0.062 | 1.33    |
| **Surprise × TreatYr**          | 0.044* | 1.67   | 0.051*| 1.92    | 0.040 | 1.48    | 0.046*| 1.69    |
| **Treat × Loss**                | 0.002 | 0.15    | 0.002 | 0.12    | 0.002 | 0.14    | 0.002 | 0.13    |
| **Treat**                       | 0.036**| 2.27   | 0.033**| 2.14 | 0.035**| 2.27 | 0.035**| 2.21 |
| **Nonlinear**                   | -0.004*| -1.69  | -0.004*| -1.66 | -0.004*| -1.69 | -0.004*| -1.65 |
| **Treat × TreatYr × Nonlinear** | -0.174**| -2.04 | -0.162*| -1.90 |
| **Surprise × Treat × Nonlinear**| 0.007 | 0.37    | 0.012 | 0.57    |
| **Surprise × TreatYr × Nonlinear** | 0.021 | 1.27    | 0.027 | 1.47    |
| **Treat × Nonlinear**           | 0.019 | 1.47    | 0.020 | 1.52    |
| **Bad News**                    | -0.058 | -1.63 | -0.045***| -1.24 |
| **Surprise × Bad News**         | -0.005 | -0.20 | 0.022 | 0.74    |

Other Controls? | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
Surprise × Other Controls? | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
Yr.-Qtr. FEs? | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
Surprise × Yr.-Qtr. FEs? | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
R^2 | 0.0735 | 0.0746 | 0.0740 | 0.0750 |
Observations | 5,347 | 5,347 | 5,347 | 5,347 |
IA Table 6. Retail vs. Institutional Volume

This table reports analyses based on a decomposition of trading volume into its retail and institutional components. Specifically, I use the procedure developed by ? to determine whether each trade recorded in the Trade and Quote (TAQ) database was initiated by a retail investor or institutional investor. These analyses omit the first two quarters of 2006 because the procedure identifies retail trades beginning May 15, 2006, resulting in a subsample of 5,132 of the 5,347 observations in the main sample. Panel A reports distributional statistics for the retail and institutional volume dependent variables. Panel B shows difference-in-differences estimates from retail and institutional volume regressions. Abnormal Retail Volume (Abnormal Inst. Volume) is the difference between average daily share turnover initiated by retail (institutional) investors during the earnings announcement, $t-1$ to $t+1$, and during the 60 prior trading days, multiplied by 100. % Retail Volume is the ratio of retail investor trading volume to total trading volume during the earnings announcement window, multiplied by 100. Other variables are defined in Table 2. The regressions use standardized continuous variables that have unit variance to facilitate the interpretation of coefficients. For brevity, I do not report coefficients for the control variables. t-statistics are based on standard errors that are clustered by firm. The regressions include firm and year-quarter fixed effects. ***, **, and * indicate significance at the 1, 5, and 10 percent two-tailed level, respectively.

Panel A: Descriptive Statistics

| Variable                  | Mean  | Std. Dev. | 25th  | Median | 75th  |
|---------------------------|-------|-----------|-------|--------|-------|
| Abnormal Retail Volume    | 0.036 | 0.079     | 0.002 | 0.014  | 0.039 |
| Abnormal Inst. Volume     | 0.691 | 0.986     | 0.155 | 0.418  | 0.892 |
| % Retail Volume           | 4.214 | 2.169     | 2.633 | 3.791  | 5.374 |

Panel B: Volume Regressions

| Variable | Coef. | t-stat. | Coef. | t-stat. | Coef. | t-stat. |
|----------|-------|---------|-------|---------|-------|---------|
| $Treat \times TreatYr$ | 0.064** | 2.40 | 0.062** | 2.32 | 0.032 | 1.11 |

Controls? | Yes | Yes | Yes
Firm & Yr.-Qtr. FE? | Yes | Yes | Yes
$R^2$ | 0.0655 | 0.1042 | 0.2878
Observations | 5,132 | 5,132 | 5,132
IA Table 7. Increase vs. Decrease in Coverage

This table shows difference-in-difference estimates from the fully specified regression in Table 4 and the “Baseline” regressions in Table 7 after separating the sample into the quarters surrounding the coverage decreasing (i.e., first) event and the coverage increasing (i.e., second and third) events. Panel A considers coverage decreases and Panel B considers coverage increases. Variables are defined in Table 2. I standardize continuous variables to have unit variance to facilitate the interpretation of coefficients. For brevity, I do not report coefficients for the control variables. t-statistics are based on standard errors that are clustered by firm. The Abnormal Returns regressions include year-quarter fixed effects and the interaction of these effects with Surprise. The Intraperiod Timeliness and Abnormal Volume regressions include firm and year-quarter fixed effects. ***, **, and * indicate significance at the 1, 5, and 10 percent two-tailed level, respectively.

### Panel A: Coverage Decrease (Event 1)

| Sample = | Event 1 |
|----------|---------|
| Dependent Variable = | Abnormal Returns | Intraperiod Timeliness | Abnormal Volume |
| Variable | Coef. | t-stat. | Coef. | t-stat. | Coef. | t-stat. |
| Surprise | 0.857 | 1.58 | | | | |
| Surprise × Treat × TreatYr | 0.014 | 0.15 | | | | |
| Treat × TreatYr | -0.032 | -0.51 | 0.029 | 0.42 | 0.074 | 1.10 |
| Surprise × Treat | -0.061 | -0.67 | | | | |
| Surprise × TreatYr | 0.027 | 0.50 | | | | |
| Treat | -0.005 | -0.16 | | | | |
| Controls? | Yes | Yes | Yes |
| Surprise × Controls? | Yes | No | No |
| Yr.-Qtr. FEs? | Yes | Yes | Yes |
| Surprise × Yr.-Qtr. FEs? | Yes | No | No |
| Firm FEs? | No | Yes | Yes |
| R² | 0.1594 | 0.0117 | 0.0328 |
| Observations | 872 | 619 | 872 |

### Panel B: Coverage Increase (Events 2 and 3)

| Sample = | Events 2 and 3 |
|----------|----------------|
| Dependent Variable = | Abnormal Returns | Intraperiod Timeliness | Abnormal Volume |
| Variable | Coef. | t-stat. | Coef. | t-stat. | Coef. | t-stat. |
| Surprise | -0.238 | -0.79 | | | | |
| Surprise × Treat × TreatYr | 0.057* | 1.77 | | | | |
| Treat × TreatYr | 0.057* | 1.82 | 0.096*** | 3.51 | 0.076*** | 2.95 |
| Surprise × Treat | -0.001 | -0.04 | | | | |
| Surprise × TreatYr | 0.001 | 0.05 | | | | |
| Treat | -0.060* | -1.65 | | | | |
| Controls? | Yes | Yes | Yes |
| Surprise × Controls? | Yes | No | No |
| Yr.-Qtr. FEs? | Yes | Yes | Yes |
| Surprise × Yr.-Qtr. FEs? | Yes | No | No |
| Firm FEs? | No | Yes | Yes |
| R² | 0.0773 | 0.0084 | 0.0774 |
| Observations | 4,445 | 3,349 | 4,445 |