# Do Journalists Help Investors Analyze Firms' Earnings News?

by

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## Abstract

I examine whether the market's reaction to firms' earnings news varies with analysis (or editorial content) produced by financial journalists. A series of natural experiments at *The Wall Street Journal* (WSJ) suggests that WSJ articles increase trading volume and improve price discovery at S&P 500 earnings announcements. The effects are stronger when an article contains more original analysis and less content reproduced from the firm's press release. This evidence refines inferences from prior studies that find media dissemination, but not analysis, makes the market's earnings response more efficient. Instead, my paper suggests media analysis also enhances investors' trading decisions by improving their understanding of earnings news, albeit for a limited set of large firms. In other words, journalists' analysis efforts provide value to readers, which helps explain the continued production of costly earnings-related analysis amid increasing pressure from low-cost information sources.

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# 1. Introduction

A central question about the media's role in capital markets is whether media coverage impacts market reactions to firms' disclosures, such as earnings releases [see Section 5 of 54]. While several studies find journalists *disseminate* firms' earnings releases to a wider audience, prior research has not found journalists' *analyses* help investors understand the implications of firms' earnings news. In this context, "analysis" refers to editorial content describing new facts, trends, and ideas or synthesizing pre-existing information. The lack of evidence supporting an analysis role is puzzling because the media expends substantial resources analyzing, not merely disseminating, earnings.

In contrast to prior studies, I show that news articles with analysis enhance investors' reactions to earnings news. My findings hinge on several key methodological innovations, which I introduce briefly here and describe in further detail below. First, I focus on the S&P 500, highly visible firms whose news attracts many readers and is widely disseminated. Second, I study *Wall Street Journal* (WSJ) earnings articles, which feature more analysis than most other earnings-related media.[1] Third, I exploit a series of restructuring events at the WSJ that produced plausibly exogenous variation in earnings coverage. Finally, I measure the extent to which each article contains original analysis instead of content reproduced from the firm's press release.

Media analysis likely facilitates readers' trading decisions through several channels, including the following: (1) helping investors interpret opaque, technical earnings releases; (2)

[1]Throughout the paper, I use the terms "journalist" and "reporter" interchangeably to refer to the author(s) of a WSJ article and the editor(s) who review and revise the article before publication.

synthesizing information from a variety of sources, including other intermediaries; (3) benchmarking firms' results to broader industry- or economy-wide outcomes; and (4) emphasizing issues that the firm and analysts lack incentives to clearly disclose. For example, Appendix A contains excerpts from a WSJ article covering General Dynamics' January 22, 2014, earnings release. This article highlights several potential implications of the firm's earnings release by contrasting perceived good news (share buyback) and bad news (lower profit and sales forecasts) in the same sentence, comparing the timing of the firm's and competitors' buyback programs, and discussing the impact of government spending cuts on the industry in general and the firm in particular.

However, the media's editorial content may have no effect on investors' ability to understand earnings, perhaps if journalists lack sufficient training in finance and accounting needed to provide new insights about technical regulatory disclosures. Or high-frequency and other sophisticated traders may impound earnings news into price before journalists can digest the news and publish their articles. Finally, media analysis could even impede market reactions by, for example, making investors fixate on trivial or sensational ideas and ignore value-relevant information [62]. If any of these is true, then journalists' earnings articles might simply entertain investors (e.g., day traders) but not improve, or even impact, their trading. This entertainment role would explain the media's expenditures on in-depth earnings coverage and the lack of empirical support for the analysis role. My study does not rule out the entertainment role, but suggests earnings articles also play an analysis role.

[12] and [24] find support for the media dissemination, but not analysis, hypothesis at earnings announcements. My study contradicts their analysis findings but does not directly reexamine the dissemination hypothesis. The following two factors reconcile my results with theirs. First, the benefits to the media of creating information increase in firm size and visibility, which reflect the number of stakeholders (i.e., potential readers). However, the prior papers include small and medium-sized firms to maximize variation in dissemination. As a result, their analyses are better suited to test dissemination, which applies to thousands of smaller firms, than analysis, which likely applies only to the largest few hundred firms.

Instead, my empirics consider only the S&P 500 firms, which are among the largest and most visible in the economy.

Second, prior studies count "full" news articles, which include editorial content produced by a human author, as a proxy for earnings analysis. However, many of these articles do little more than supplement a few basic facts from the earnings release with analyst forecasts and recent stock returns. For example, [12] and [24] feature many articles from Dow Jones (DJ) Newswires, an outlet specializing in immediate but short summaries. My tests instead focus on WSJ articles, which are longer and emphasize analysis more than most other full earnings articles.[2]

Empirically identifying the effects of media coverage on investor decisions is challenging because the coverage is endogenous to the firm and news being covered (or not covered). For example, it is unclear whether investors react more when the media covers an earnings release because of the coverage or whether they would react more anyway, perhaps because the media covers firms with extreme news [e.g., 1]. I attempt to mitigate this concern by exploiting three distinct restructuring events at the WSJ that produced plausibly exogenous variation in earnings-related media coverage.

First, in 2007 the WSJ responded to technological advances allowing market participants to monitor breaking news online by decreasing (increasing) the proportion of breaking (exclusive) news articles.[3] Second, in 2008 Rupert Murdoch purchased the WSJ and instructed editors to return the amount of breaking news coverage to previous levels, arguing the WSJ was still in the business of breaking news. Third, in 2013 the newsrooms of the WSJ and DJ Newswires merged to reduce redundancy, which increased the number of reporters contribut-

[2]DJ Newswires articles are published within minutes of a press release, while WSJ articles often have a deadline of an hour or more (Source: [26] and discussions with a WSJ reporter). Also, [24] find WSJ earnings articles contain more than twice as many words as full earnings articles (which include both DJ Newswires and WSJ), on average (i.e., 530 vs. 248).

[3]Breaking news refers to currently occurring or developing events, including but not limited to announcements of earnings, mergers and acquisitions, and new products. Exclusive or "scoop" news reveals unexpected, surprising, or secret information. Examples of scoops include uncovering accounting fraud, bribery, or price-fixing. Note that exclusive news articles fill primarily the analysis (or creation) role, as defined in this paper. However, breaking news articles can fill the dissemination role, the analysis role, or both.

ing to the WSJ. Section 3.1 describes these natural experiments in further detail. Because earnings releases are breaking news from the WSJ's perspective, all three restructuring events changed the number of earnings releases covered each quarter. Figure 1 shows the number of S&P 500 earnings reports covered by the WSJ during each quarter of 2004-2015. This number changed discretely in the expected direction at each of the events, which are denoted by vertical lines and numbered in the figure.

Since these coverage changes coincide with restructuring, they are more likely to have been determined exogenously by changes in WSJ leadership's strategy. That is, many of the changes would likely not have happened without restructuring, which is exogenous to firm-specific earnings responses. For this reason, I exclude changes during non-restructuring periods that were more likely to be determined endogenously by firm- or announcement-specific factors. This design provides some assurance that my estimates represent the causal impact of WSJ articles on market outcomes. In addition, using events that both increased and decreased coverage raises the bar for alternative explanations. To explain my results, an omitted variable would need to be correlated with the dependent variables, treatment firms, and events; and the correlation would need to switch direction depending on the event.

Using a difference-in-differences estimator, I examine the change in trading volume and price discovery of treatment firms that received WSJ earnings coverage before but not after the coverage-decreasing restructuring event and after but not before the coverage-increasing events. The model compares treatment firms to two types of control firms: those that always (both before and after) received coverage or those that never did (neither before nor after).

The model's identifying assumption is that treatment and control firms have parallel trends in the earnings-announcement market outcome variables. I find evidence consistent with this assumption in the pre-treatment years. In addition, having both always and never control firms provides some comfort that, if the treatment had never taken place, there would have been parallel trends in the post-treatment years. That is, I show that aggregating more-visible always firms and less-visible never firms creates a control group that is fairly similar (or matched) to the treatment group. As a result, there is limited potential for confounding

factors, such as the WSJ choosing to cover firms with extreme news, to drive the results. However, to ensure treatment and control firms have similar newsworthiness, I also perform my analyses after matching firms on the amount of, and changes in, recent firm news.

My first tests examine the effect of WSJ articles on the amount of earnings-announcement trading volume. I expect that the analysis in WSJ articles helps investors understand the implications of the earnings release for the firm's value, leading them to trade more. Consistent with this prediction, my estimates indicate that abnormal trading volume increases by about 5% of the unconditional sample mean when a WSJ article covers the earnings release.

Since observed trading volume is insufficient to discriminate between information content and investor disagreement [see 43], I also examine price discovery. If WSJ articles either help more investors become informed or make already-informed investors trade more or faster, then prices may reflect earnings sooner. To measure price discovery, my next tests consider the earnings response coefficient (ERC) and intraperiod timeliness (IPT). The ERC captures the magnitude of short-window price responses to earnings, and IPT captures the speed at which earnings is incorporated into price during the few days following the announcement. I show WSJ coverage increases the ERC by about 20%.[4] While WSJ articles do not significantly increase IPT on average, additional tests described below find that a subset of articles containing original analysis increase IPT by about 13%. Together, these results suggest earnings-related WSJ articles accelerate earnings responses.

Another challenge facing the media literature is distinguishing between analysis and dissemination. Fortunately, dissemination is relatively constant in my setting, allowing me to focus on analysis. Many outlets, including the Associated Press, DJ Newswire, and PR Newswire, broadly disseminate all S&P 500 earnings releases. Furthermore, this coverage is immediately and costlessly available on several ubiquitous market data platforms, such as Yahoo Finance. At least for these firms, there seems to be little to no dissemination shortage for the WSJ to address. However, because WSJ is so prominent, it is possible that (1) some

[4]For comparison, [16] find material restatements decrease the ERC by 56%, [33] find PCAOB inspection increases the ERC by 57%, and [30] find executive compensation disclosures increase the ERC by 20%. Such large percentage effects are due to the baseline ERC being fairly low [44].

investors (e.g., retail investors) do not regularly access other sources, relying only on the WSJ to receive earnings news, or (2) some investors refer to other dissemination outlets only after WSJ articles signal which announcements merit attention. If either (or both) of these is the case, then my results may be driven by the WSJ filling a dissemination role.

To further isolate the effects of analysis from dissemination, I measure the textual similarity between a firm's earnings press release and the associated WSJ article. Multiple studies, such as [69] and [17], use this measure in the context of media articles and firms' disclosures. Their findings suggest high similarity reflects dissemination of "stale" information and low similarity reflects the production of economically meaningful information (i.e., new analysis or content).[5] In my setting, WSJ articles that have a higher proportion of words in common with the earnings release likely serve more of a dissemination role, while those with fewer similar words likely contain more original analysis, which I hypothesize facilitates market reactions.

As expected, the effects are concentrated among articles containing original analysis. By contrast, the effect is absent among articles disseminating the most press release content, which helps falsify the dissemination hypothesis for WSJ articles. These results hold for both retail and institutional trading volume, highlighting the business press's diverse readership.

I cannot generalize these findings to non-S&P 500 firms or non-WSJ coverage, nor do I expect the findings to generalize given [12] and [24]. Overall, my tests suggest the primary role of WSJ earnings articles covering S&P 500 firms is analysis, not dissemination. Even in this restricted setting, my tests are limited because I measure a specific type of analysis. That is, while document similarity seems to effectively capture the amount of new content (or "private information") in an article, it does not capture the extent to which the article synthesizes information from the firm's disclosures, which may also be helpful to investors.

My study's most direct contribution is to research examining the role of media coverage

[5]To be specific, [69] finds firms' stock returns respond more to firm-specific news articles that have fewer words in common with past articles. [17] find that 10-K and 10-Q filings with words that significantly differ from past filings portend negative stock returns.

surrounding firms' earnings announcements. I show reporters provide additional information that helps investors better understand firms' earnings, in addition to making investors more aware of firms' earnings. I also contribute to studies that find the press creates information in a monitoring role, such as the evidence of [53] that the media identifies accounting irregularities. These investigative efforts to provide new information often take weeks or months and can make or break a reporter's career [26]. My study shows reporters also create value-relevant information during routine earnings coverage, a setting in which the stakes are relatively low but provide only a few hours to produce an article. These findings help explain the continued existence of full articles about earnings announcements.

In addition, I contribute to the broader literature on the role of information intermediaries in capital markets. My findings suggest that, like sell-side analysts [47], journalists create information about firms' future cash flows. Although they both create information, journalists are likely able to enrich a firm's information environment in ways that analysts cannot because journalists reach a broader audience and have incentives that make them more credible [29, 45]. Moreover, if the media's earnings analysis can improve market efficiency in as rich an information environment as the S&P 500, then the potential returns to investors' earnings processing efforts may be even greater among smaller firms that the media and other information intermediaries have fewer incentives to cover.

# 2. Prior Literature and Hypotheses Development

## 2.1 Market Reactions to Earnings News

An extensive literature in accounting and finance examines the impact of earnings news on investment decisions, as measured by capital market outcomes such as stock returns and trading volume (e.g., the seminal work of [3] and [4], respectively). The major takeaways from this literature are (1) investors' responses to earnings news are significant and swift and (2) hence earnings are a key input into investors' analysis of a firm's fundamental value [44]. However, the process through which earnings information is reflected in capital market activity is neither instantaneous nor trivial because of constraints such as limited attention and limited arbitrage [48, 36, 61]. As a result, researchers continue to try to understand which factors determine the speed and magnitude of investors' earnings announcement reactions [a recent example is 22].

Several theoretical models support the use of stock returns and trading volume as proxies for the extent to which disclosures change investors' opinions [73]. In these models, stock returns reflect updates to investors' expectations of firm value that occur when they receive disclosures, which are noisy signals of firm value [e.g., 38]. While prices move in parallel with investor beliefs, even without trade occurring, trading volume typically arises in theory as a result of some form of investor heterogeneity. For example, [43] model trading volume as the

product of differences in investors' pre-disclosure private information and the price change at the time of disclosure. [2] find empirical support for this model, showing earnings announcement trading volume is increasing in both pre-announcement information asymmetry and announcement returns. They suggest considering trading volume in conjunction with returns is likely to yield more refined insights than considering either in isolation.

In practice, many studies measure the market's reaction to earnings news using the coefficient from a regression of announcement-window returns on the deviation of earnings from its expected value (i.e., the earnings response coefficient or ERC). More recently, studies also consider intraperiod timeliness, which is an area-under-the-curve measure that captures the speed at which earnings information is incorporated into price during the few days following the earnings announcement [e.g., 13]. Intuitively, if most of the change in stock price happens within a day of an earnings announcement then investors' response was more timely than if most of the change had happened a few days later. In the former case, the area under the cumulative-return curve would be larger. Finally, trading volume is typically measured as share turnover during the announcement benchmarked against normal share turnover during non-announcement periods.

## 2.2 Information Intermediaries and Earnings News

Various information intermediaries devote significant resources to disseminating and analyzing firms' earnings for investors, another indication of the fundamental role of earnings in investors' decision-making. Hundreds of studies focus on the impact of sell-side analysts and independent auditors on the pricing of earnings information.[6] The main roles of analysts and auditors include forecasting earnings and assessing the reliability of earnings, respectively. An in-depth review of these intermediaries' roles is obviously beyond the scope of a paper about the business press. Suffice to say that the enormous demand for analyst and auditor services raises the question, which roles remain for the press to fill in the context of earnings?

[6]See [11] and [21] for recent reviews.

The traditional business press has one of the largest and broadest audiences of all potential information intermediaries (with perhaps the only exception being social media), certainly more than analyst reports and corporate filings that tend to circulate primarily among sophisticated investors and regulators [29, 12]. Consistent with the media catering to noise and liquidity traders, [68] finds high pessimism in a popular WSJ column predicts downward pressure on market prices that later reverses. However, a significant stream of empirical studies suggest the press improves capital market outcomes by alerting market participants to firms' information events. For example, [29] find investors of stocks with less media coverage earn higher risk premiums. In the context of earnings announcements, [67], [63], [12], [27], [24], and [7] find that increased dissemination of earnings press releases and articles summarizing these press releases reduces information asymmetry and enhances price discovery.

Another stream of research finds the media helps investors monitor firms' managers by creating information with high credibility, likely because the media is less affected by incentive concerns than managers or analysts [45]. [53] identifies several cases in which the media uncovered accounting irregularities before regulators or analysts. [18] document negative press coverage about CEOs with excess compensation and many option exercises. [55] show greater media coverage precedes deteriorating operating performance.

Together, these two streams of research suggest the media could influence market reactions to earnings news by (1) disseminating pre-existing information more broadly and/or (2) creating new information through original analysis or synthesis.[7] Realizing this, [12] and [24] examine whether the dissemination role, creation role, or both explain the media's influence at earnings releases. Their proxy for dissemination is the count of news wire services carrying the firm's press release and short-snippets or summary articles that often use technical, non-natural language produced by automated means. Their proxy for creation

[7]Like my paper, several studies in both streams of the literature address endogeneity concerns using sources of potentially exogenous variation in media coverage, including extreme weather events [27], scheduling of WSJ columnists [23], newspaper strikes [56], intraday trading [49, 57], geographic proximity to DJ branches [20], the amount of competing news [63], and the staggered introduction of automated media articles [7].

is full articles that add editorial content to firm-generated information and are written by journalists. Both papers' analyses support the dissemination channel but not the creation (or analysis) channel. These results fit the intuition that the business press has the rare potential to spread the large fixed costs of gathering firm-generated information across many investors [72, 49]. However, the remaining puzzle is why the press would devote significant journalistic resources (e.g., human reporters) to generating additional information through costly earnings-related full articles that do not influence market reactions to earnings news.

## 2.3 Hypotheses Development

The preceding literature indicates the financial press makes more investors aware of earnings announcements, a key source of investment information. Financial journalists also go beyond merely disseminating firm-generated earnings numbers and related discussion over press release wires. Specifically, they produce full articles that contain significant amounts of editorial content, which can include original analysis and synthesis of other information intermediaries' outputs.

It seems unlikely journalists would produce such costly content absent sufficient demand from readers for this type of coverage. Since most readers of earnings coverage are also investors, or at least potential investors, any value-relevant information in an article will likely be reflected in their subsequent trading and ultimately in market prices. Absent a press article, some or all of these investors would have to gather and process the information themselves before impounding it into stock prices. Because this information collection would require time and assumes access to the same sources the journalist would have relied on, trading and price discovery would be delayed. Motivated by this discussion, my first hypothesis is as follows:

**H1:** *Full press articles increase trading and enhance price discovery at earnings releases.*

My second hypothesis relates to the type of information in full articles that drives this

effect. Journalists' full articles can either disseminate information the firm generates or information they or other outsiders generate. As noted previously, full articles are almost always written about firms that are visible enough for their press release and basic earnings summaries to be disseminated by several outlets, such as newswires. Because investors can easily access these firms' earnings releases from alternative sources, retransmitting press release content does not seem likely to substantially enhance investors' information set.

Instead, providing editorial analysis in press articles seems to have more potential to improve investors' trading decisions in several ways, including the following:

- Firms' disclosures are technical and opaque, partly to comply with regulations [25]. The business press, made up of writers working with editors to attract readers, seems more likely to synthesize firms' disclosures in a way that helps investors parse value-relevant information from noise.
- Journalists often report outsider-generated information, such as analyst estimates or investor opinions, to supplement the information firms disclose. Considering a news event's implications from diverse perspectives may help investors, for example, identify the extent to which other investors are making similar investments [66].
- Because they are often assigned to cover a specialized beat, consisting of a few bell-wether firms, an industry, or a geographic location, reporters can put a firm's news in the context of broader industry- or economy-wide trends [23]. Doing so could, among other things, help investors evaluate the firm's performance relative to peers.
- Journalists tend to focus on sensational news, such as fraud, executive pay, and poor performance [53, 18, 55], that is likely to attract readers' attention. This propensity may help investors identify managers who are hiding bad (or sensitive) news [46].

Consequently, my second hypothesis is as follows:

**H2:** *The positive impact of full press articles on earnings-announcement reactions is driven by editorial content (or original analysis) created by the press.*

# 3. Sample and Research Design

This section details the WSJ restructuring events, sample, difference-in-differences regressions, and variables.

## 3.1 Changes to WSJ Earnings Coverage

This section details changes in the WSJ's earnings release coverage that resulted from three major restructuring events. In summary, the January 2007 (January 2008) [Summer 2013] event made covering earnings releases a lower (higher) [higher] priority at the WSJ.[8]

**January 2007.** WSJ publisher Gordon Crovitz and managing editor Paul Steiger implement a paper redesign, called "Journal 3.0," that increases (decreases) the proportion of articles reporting exclusive "scoops" (breaking news) from 50 to 80 (50 to 20) percent [19]. They argue readers can find out breaking news by referring to online sources and later learn the meaning of breaking news by referring to the WSJ.

[8]Besides the three changes highlighted in this section, two other aspects of Figure 1 deserve mention. First, earnings coverage gradually decreased from 2009-2013. The WSJ likely covered more earnings releases than normal during the 2009-2010 credit crisis because many firms that wouldn't routinely receive coverage faced increased uncertainty about their future prospects, including events such as extraordinary losses, bankruptcy, and liquidation [10]. Also, earnings coverage may have decreased during 2012-2013 because (1) DJ's CEO Lex Fenwick diverted resources away from WSJ to a project (called "Project DJX") that combined several DJ services for institutional clients [64], (2) the set of relevant firms contracted because many firms went out of business or merged during the crisis, and/or (3) other contemporaneous news (e.g., European debt crisis) merited relatively more coverage.

Second, the temporary decrease in earnings coverage during the second quarter of 2015 may be explained by DJ laying off dozens of journalists during fiscal 2015. Many internal and external sources attributed these lay-offs to the unsuccessful DJX project [65, 40].

**January 2008.** Robert Thomson, Rupert Murdoch's deputy at News Corp., takes over as WSJ publisher (and a few months later as WSJ managing editor). Over the next few months, he undoes much of the Journal 3.0 redesign, restoring the proportion of articles covering breaking news to previous levels. He felt it was "pretending the newspaper wasn't in the business of breaking news," adding "henceforth all Journal reporters will be judged, in significant part, by whether they break news for the Newswires" [26].[9]

**Summer 2013.** WSJ managing editor Gerard Baker announces "the integration of the newsrooms of DJ Newswires and the WSJ." [14]. The integration's primary aim is "accelerating the output of unrivaled news coverage for... professional and consumer audiences." New reporters and editors are hired, and some positions are consolidated [59, 60]. When positions are consolidated, the redundant WSJ reporter typically keeps his or her position and the redundant DJ Newswire reporter is laid off.[10] Notably, DJ had attempted this integration several times before to save costs because "the two newsrooms often covered the same story, be it an earnings release... or the market's movements," but prior integration attempts had failed because "such a combination was anathema to the proud staff of the WSJ, where many reporters saw themselves as storytellers with a powerful audience, not stenographers for the broker set" [26].

## 3.2 Sample Overview

My analyses in Section 4 examine media coverage, trading volume, and price changes in response to firms' quarterly earnings announcements. For several reasons, my analyses are limited to one media outlet, the WSJ, and a few hundred firms, the S&P 500.

[9]The proximity of the January 2007 and 2008 restructuring events to the global financial crisis raises concerns that the associated changes in earnings coverage were driven by a shift in editorial resources to crisis-related topics, rather than by a shift in focus on article types (i.e., breaking vs. exclusive). However, note that the January 2007 restructuring took place before the crisis started, and planning for this event began in early 2006. Also, Murdoch and Thomson had coveted the WSJ for years before the 2008 acquisition, and are quoted in [26] complaining about the WSJ's focus on exclusive articles as early as 2001. Thus, it seems unlikely the financial crisis drove their 2008 decision to focus more on breaking news.

[10]Source: Discussions with a WSJ reporter whose office was similarly consolidated.

The WSJ is one of the leading business press outlets, with about 2 million paid subscribers and 42 million unique visitors per month to WSJ.com.[11] Also, the WSJ specializes in producing relatively costly analysis (or editorial content), priding itself on "telling you about facts, trends, ideas, and analysis you won't see anywhere else" [19]. Finally, the WSJ natural experiments described above have several useful features for isolating the effects of media analysis.

The S&P 500 firms make up approximately 80% of the U.S. stock market's capitalization. They thus receive the bulk of the attention of media outlets, such as the WSJ, that provide in-depth coverage. For example, I identified at least one firm-specific WSJ article on the day of or the day after 40.2% of the S&P 500 earnings announcements but only 8.8% (3.1%) of the S&P MidCap 400 (S&P SmallCap 600) announcements during 2004-2015. In addition, because these firms' earnings releases are widely disseminated by several outlets (i.e., the level of dissemination is relatively constant), the effect of WSJ articles on trading in their stock, if any, is more likely due to analysis than incremental dissemination.

I use Compustat item SPMIM to identify S&P 500 firms and Compustat item RDQ to identify their announcement dates, denoted $t$, during 2004-2015. In 2004, the SEC began requiring firms to file earnings releases in form 8-K, which I collect from EDGAR as described below.[12] Because my tests examine changes around the WSJ natural experiments, the sample includes observations within a calendar year (plus or minus) of one of the three WSJ events. This requirement leaves 9,987 earnings announcements during a five-year period, including 2006 through 2008 and the fourth quarter of 2012 through the third quarter of 2014.

One of my main tests compares the text of the firm-initiated earnings press release to the text of the associated WSJ article (if any) published on the same or subsequent day. To avoid collecting press releases about the firm issued by other organizations [see Appendix 1 of 63], I collect firms' earnings press releases from the SEC EDGAR database. After announcing earnings, firms file an 8-K classified under Item 2.02 - "Results of Operations

[11] https://www.sec.gov/Archives/edgar/data/1564708/000119312516580084/d162014dex991.htm; http://www.wsjmediakit.com/files/uploads/201410/WSJ.com%20Audience%20Profile.pdf

[12] See section II.C of SEC release number 33-8176 (https://www.sec.gov/rules/final/33-8176.htm).

and Financial Condition," attaching the press release as an Exhibit 99.[13] I use the WRDS SEC Analytics Suite to identify the 8-K and Exhibit 99 file names associated with each earnings announcement date (RDQ) in the sample. I then download the Exhibit 99s that have press release identifying information directly from EDGAR. Appendix B.1 explains the algorithm I use to identify and clean press releases.

During the later years of the sample period, the WSJ typically posted articles reporting corporate earnings to WSJ.com within two hours of a firm publishing an earnings press release.[14] Specifically, firms announcing earnings before (after) market open (close) typically make the press release available between 6-7 am (4-5 pm), and the associated WSJ article is published by 9 am (7 pm).[15] Many (but not all) of these articles are also published in the print version of WSJ the next day. In the earlier part of the sample period, especially before 2007, when WSJ.com became much more integrated with the print version, articles covering after-market earnings announcements were sometimes not published until the next morning's print version. Thus, to correctly determine whether the WSJ covered an earnings announcement, I search Factiva for WSJ articles about the firm published on either the trading day of or the trading day after the earnings announcement. Appendix B.2 describes the filters I use to ensure an article is about the firm.

If multiple WSJ articles about the firm are published during this earnings announcement window, I keep the one that most resembles the press release, according to the textual similarity measure described in Section 4.3. This procedure likely identifies the WSJ article covering the earnings release in most cases but could identify an article covering a different topic (e.g., merger or executive turnover), introducing measurement error into my match of earnings press releases to earnings-related WSJ coverage. Since extremely large firms are most likely to be the subject of multiple articles, choosing the article that disseminates more

[13]The SEC explains this process in Section III.B.7 of its guide to the EDGAR database, available at https://www.sec.gov/investor/pubs/edgarguide.htm.

[14]This raises the question of how journalists are able to understand and communicate the implications of earnings so quickly. Since journalists often cover the same firm(s) over an extended period of time, it is certainly possible that much of their analysis effort occurs prior to the announcement.

[15]Source: Discussions with a WSJ reporter whose duties include covering earnings announcements.

of the press release biases against finding that analysis matters for them, even though they are the most likely to warrant in-depth analysis.

After disseminating an earnings press release, firms often hold a conference call to discuss quarterly results and take questions from analysts. Journalists listen to these conference calls, but their publication deadline often precedes the call. As a result, their articles are primarily based on the press release, in addition to prior knowledge and any quotes they can gather from analysts or other market participants. However, they (sometimes) subsequently update the article based on the proceedings of the conference call. In a cursory review of multiple versions of dozens of articles, I found these updates were usually minor, affecting or adding only a sentence or two by, for example, including a quote from the call. Nevertheless, to mitigate concerns that my results reflect the information content of conference calls, instead of WSJ articles, my analyses rely on the first version of the article that appears on WSJ.com.

To measure the main variables in the analyses, I obtain firm-specific volume and price data from CRSP, accounting variables from Compustat, and analyst forecasts from IBES. I omit firms with insufficient data to calculate these variables and control variables, reducing the sample to 9,547. Variables are defined in Section 3.4 and Table 2. I require sample firms to have at least one observation in the respective pre- and post-treatment periods so changes in sample composition do not affect the results [following, e.g, 42].[16] The final sample consists of 8,159 quarterly earnings announcements.

## 3.3 Research Design

My first tests of the effect of WSJ articles on market reactions to earnings news use the following difference-in-differences regression model:

[16] I also omit 238 observations of firms with coverage increases (decreases) after a coverage decreasing (increasing) restructuring event. These firms do not fit in the treatment group because their coverage changes were clearly not a result of the restructuring, and they do not fit in the control groups because their coverage was not constant between the pre- and post-periods.

$$Market\ Outcomes_{i,q} = \beta_1 \cdot Treatment_{i,q} \times Coverage_{i,q} + \Sigma_{k \in K} \beta_k \cdot Z_{i,q,k} + \alpha_i + \alpha_q + \epsilon_{i,q}, \quad (1)$$

where $i$ indexes firms, $q$ indexes year-quarters, and $Z_k$ denotes a set of $k$ control variables. *Market Outcomes* indexes the main dependent variables, including abnormal trading volume, earnings response coefficients, and intraperiod timeliness. The ERC regression differs slightly, as I explain in detail in Section 4.2, because the variable of interest is a coefficient.

Most importantly, $Treatment \times Coverage$ is one for treatment firms' earnings announcements during the period with WSJ earnings announcement coverage, which is the pre(post)-treatment period for the coverage decreasing (increasing) WSJ natural experiments; zero for treatment firms' earnings announcements during the period without WSJ earnings announcement coverage; and zero for all control firms' earnings announcements. The main effects, $Treatment$ and $Coverage$, are subsumed by firm and year-quarter fixed effects, respectively. The coefficient of interest, $\beta_1$, compares changes in the dependent variable between the treatment and control groups. $\beta_1 > 0$ would suggest WSJ articles increase trading volume and improve price discovery.

The regression uses a fixed effects structure to address concerns about violation of the parallel trends assumption, which I nonetheless test explicitly in the pre-treatment years (see Section 5.1). Firm fixed effects, $\alpha_i$, control for differences in the dependent variables across sample firms. Year-quarter fixed effects, $\alpha_q$, difference away quarter-specific factors (e.g., common time trend) affecting market reactions to firms' earnings announcements. Also, to account for dependence in the error terms, I report $t$-statistics using standard errors that are clustered by both firm and year-quarter, following [70].

As seen in Figure 1, the WSJ restructurings affect the amount of earnings announcement coverage gradually over one or two quarters. Accordingly, I assign firms to the treatment and control groups based on whether the WSJ covered their earnings release in the second

quarters before and after each natural experiment. Assigning treatment based on coverage in the quarters immediately preceding and following the restructurings would misclassify many firms whose WSJ coverage eventually changes because of the WSJ events (i.e., treatment firms) as control firms. In untabulated analyses, I find similar results to those reported below when I alternatively address this issue by using a continuous treatment variable, that is, defining *Treatment* as the difference between the proportion of the firm's quarterly earnings releases covered in the pre- and post-periods.

To illustrate my empirical design, the diagram below provides a timeline example of assignment to treatment and control groups for the January 2007 restructuring event. For convenience, *Treatment* × *Coverage* is abbreviated to $T \times C$ in the diagram.

**January 2007 WSJ Restructuring**

$\Downarrow$

| | y06q1 | y06q2 | y06q3 | y06q4 | y07q1 | y07q2 | y07q3 | y07q4 |
|---|---|---|---|---|---|---|---|---|
| Always Control | $T \times C = 0$ | | WSJ Coverage | | | WSJ Coverage | $T \times C = 0$ | |
| Treatment | $T \times C = 1$ | | WSJ Coverage | | | No WSJ Coverage | $T \times C = 0$ | |
| Never Control | $T \times C = 0$ | | No WSJ Coverage | | | No WSJ Coverage | $T \times C = 0$ | |

Table 1 further illustrates my research design. Panel A shows values of *Treatment* × *Coverage* for several firms, and Panel B shows the number and percentage of firms in the treatment and control groups during each quarter of the sample period.

## 3.4 Variable Measurement

Following a large body of research in accounting that began with [4] and more recently includes [7], I use abnormal volume as a proxy for the intensity of investors' reaction to earnings news. Specifically, *Abnormal Volume* is the difference between average daily share turnover (volume divided by shares outstanding) during the earnings announcement, $t-1$ to $t+1$, and during the 60 prior trading days, multiplied by 100.

To better understand the effect of WSJ articles on market efficiency, I also consider two measures of price discovery. First, the coefficient from the regression of earnings-announcement abnormal returns (*Abnormal Returns*) on the earnings surprise (*Surprise*) is the earnings response coefficient (ERC), a standard proxy for the market's short-window price response to earnings. *Abnormal Returns* is market-adjusted buy-and-hold returns during the earnings announcement window, $t-1$ to $t+1$, in percentage terms. *Surprise* is the analyst-based earnings surprise, defined as the difference between actual earnings per share (Compustat item EPSPXQ) and the median analyst forecast scaled by the stock price at the end of the prior quarter, in percentage terms.[17]

Second, I use an intraperiod timeliness measure to estimate the speed of price adjustment over trading days $t$ through $t+5$ [13, 71]. The measure is the sum of the cumulative market-adjusted buy-and-hold return from day $t$ through each day $t+i$ in the window ($CABRET_{t+i}$), scaled by the total cumulative return over the window:

$$Intraperiod\ Timeliness = \frac{1}{2}\sum_{i=0}^{5}\frac{CABRET_{t+i-1} + CABRET_{t+i}}{CABRET_{t+5}}. \tag{2}$$

Because the returns are cumulative, each unit of return realized on day $t$ appears in all six terms of the summation while returns realized on day $t+5$ appear only in the final term. As a result, *Intraperiod Timeliness* intuitively increases when more of the returns are realized earlier in the window, indicating faster (or more efficient) price discovery. Following

[17] I keep the most recent forecast of the upcoming earnings release for each analyst as long as the forecast was made during the 90 days before the earnings release, following [51].

[7], I reduce measurement noise due to small denominators by excluding observations with absolute $CABRET_{t+5}$ less than 2%, reducing the sample size to 6,080 for the *Intraperiod Timeliness* tests.

Panel A of Table 2 contains descriptive statistics for the main variables in my tests. Trading volume is 0.73%, or 73 basis points (bps), higher during earnings announcements than during non-announcement periods, on average, which is twice as much abnormal volume as the 36 bps in the sample of low-visibility firms studied by [7]. Information is also impounded into price faster for the high-visibility firms in my sample, with a mean (median) *Intraperiod Timeliness* of 4.01 (4.27) compared to their mean (median) of 3.78 (3.83). However, price reactions are more volatile in their setting, with an interquartile range for abnormal announcement returns of 7.3%, compared to 5.8% in mine. All these differences are expected because I (they) focus on high (low) visibility firms, so I mainly point them out to emphasize the different environments in which the samples' firms announce earnings and the resulting potential for the media's role to vary across samples.

My multivariate tests control for firms' attributes that are likely associated with media coverage and market reactions to earnings releases. Firm attributes are measured using data available at the end of the month prior to the announcement. Since several studies find many of the following variables are key determinants in a model of earnings announcement press coverage [e.g., 12, 49, 24, 71], I do not replicate such an analysis in this study. *Market Cap* is the firm's stock market capitalization, calculated as the product of stock price and shares outstanding. *Book-To-Market* is the book-equity-to-market-capitalization ratio. *Return Volatility* is the standard deviation of monthly returns over the prior year. *#Employees* and *#Owners* are the fiscal year-end number of employees and shareholders in hundred thousands, respectively [12]. *Institutional Ownership* is the proportion of shares held by institutional owners. *#Analysts* is the number of analysts forecasting the firm's earnings. *Prior Press Coverage* is the number of days in the prior year that the firm received DJ news coverage [71]. *#8-K Disclosures* is the number of 8-Ks filed during the prior year [34].

I also include announcement-specific control variables that are likely associated with

the media and market earnings-announcement reactions. *Complexity* is the first principal component of three variables that measure the complexity of the earnings press release, which could influence investors' (and journalists') ability to understand the implications of firms' earnings [52]. The three variables measure readability (*Readable*), quantitativeness (*Hard Info*), and specificity (*Specific*).[18] *Readable* is the product of negative one and the [35] Fog index, which is an increasing function of the average number of words in a sentence and the percentage of words in excess of two syllables [50]. *Hard Info* is a count of numbers (other than dates) in the text scaled by the total number of words, multiplied by 1,000 [6]. *Specific* is a count of entities (location, person, organization, money, percentage, date, and time) identified by the Stanford Named Entity Recognizer (NER), scaled by the total number of words, multiplied by 1,000 [39]. *Bad News* is an indicator set to one if the earnings surprise is negative and zero otherwise. *#Announcements* is the number of other Compustat firms announcing earnings during the firm's earnings announcement window, $t-1$ to $t+1$.

Panel B of Table 2 shows the means of the firm- and announcement-characteristics for the treatment sample, compared to the control samples. The first two columns include the firms that received WSJ coverage both before and after (always group) or neither before nor after (never group) WSJ restructuring. The third column combines the two groups of control firms, and the fourth column includes the treatment firms.

Naturally, the always firms are larger and receive more analyst and media coverage than the never firms, while the treatment firms fall between the two. One of the benefits of having control firms both above and below the treatment firms along these dimensions is that the combined control group is fairly similar to the treatment group. As a result, the control group provides a benchmark that limits the potential for confounding factors, such as the WSJ choosing to cover firms with higher investor recognition or extreme news, to drive the results. The only variable means that differ significantly between treatment and combined control groups are analyst coverage, media coverage, and the busyness of the earnings date.

[18]The first principal component, calculated by year, has an eigenvalue of 2 and captures 67% of the underlying variation, on average. All correlations between the complexity variables are greater than 0.43.

To mitigate the potential effects of outliers, I winsorize continuous variables, except for returns, by year-quarter at the 1st and 99th percentiles. For regression analyses, I standardize continuous variables to have unit variance to facilitate the interpretation of coefficients.

# 4. Main Results

## 4.1 Abnormal Volume

My first main tests examine the link between WSJ articles and abnormal trading volume at firms' earnings announcements. I predict that WSJ articles increase trading volume by decreasing the costs of processing firms' earnings news.

Table 3 contains the first main result of the paper. It shows that, as predicted, the coefficient on $Treatment \times Coverage$ is positive and significant at about the 10% level. The coefficient on $Treatment \times Coverage$ implies abnormal volume is 0.039 standard deviations higher with WSJ coverage. Based on the unconditional sample mean and standard deviation shown in Panel A of Table 2, this coefficient translates into an economically significant 5% increase in abnormal volume (i.e., $(0.039 \times 1.036)/0.731$) due to WSJ earnings announcement coverage. An alternative measure of economic significance considers how a WSJ article would impact a firm's location in the distribution of earnings-announcement abnormal volume. In this case, the effect of WSJ coverage entails an increase from the 50th to the 53rd percentile of the abnormal volume distribution (i.e., $0.417 + (0.039 \times 1.036) = 0.457 \approx$ 53rd percentile). Overall, these findings are consistent with WSJ articles increasing trading volume, relative to other firms that announce earnings in the same quarter but do not receive a WSJ article.

## 4.2 Price Discovery

My first tests suggest investors trade more when the WSJ covers an earnings announcement. However, increased volume alone does not imply a more efficient market reaction. Investors may trade more to exploit private information, to exploit what they *think* is private information (i.e., noise trading), or to obtain or provide liquidity. So my next tests consider whether WSJ articles lead to faster price discovery, which would suggest the increased trading volume is due to WSJ articles helping investors become better informed.

Earnings response coefficients (ERC) and intraperiod timeliness (IPT) capture the extent to which market prices reflect earnings news in the few days following an earnings announcement. As mentioned above, the IPT regression takes the form shown in equation (1), but the ERC regression has a slightly different structure. In particular, I add an interaction between *Surprise* and the $Treatment \times Coverage$ treatment indicator as well as a set of interactions between *Surprise* and the control variables to the variables included in equation (1), as follows:

$$\begin{aligned} Abnormal\ Returns_{i,q} = {} & \beta_1 \cdot Surprise_{i,q} + \beta_2 \cdot Surprise_{i,q} \times Treatment_{i,q} \times Coverage_{i,q} + \\ & \beta_3 \cdot Treatment_{i,q} \times Coverage_{i,q} + \\ & \Sigma_{k \in K} \beta_k \cdot Z_{i,q,k}(1 + Surprise_{i,q}) + \alpha_i + \alpha_q + \epsilon_{i,q}. \end{aligned} \tag{3}$$

As a result, the coefficient of interest in the ERC test is $\beta_2$, which reflects the effect of WSJ articles on the relationship between earnings and short-window stock returns.

Table 4 shows the results of these price discovery tests. Panel A provides evidence that prices react more strongly to earnings surprises when the WSJ covers the earnings announcement. In particular, $\hat{\beta}_2$ is 0.048 in the regression specification that includes the main effects of control variables and interaction effects between control variables and *Surprise*. This estimate implies the ERC is about 21% larger with than without WSJ earnings coverage (i.e., $0.048/0.233 = 0.21$).

However, Panel B does not show that the immediate (i.e., six trading day) price response to earnings releases is more timely when there is WSJ coverage, on average. Specifically, the coefficient on $Treatment \times Coverage$ is positive but not statistically significant. Consistent with prior research, such as [71] and [7], the control variables explain very little of the variation in intraperiod timeliness, highlighting the noise in the measure. Although these tests, which aggregate all articles, suggest WSJ articles do not quicken price discovery on average, additional tests in the next section show that high-analysis articles do make price discovery significantly faster.

## 4.3 Analyzing vs. Disseminating News

Thus far, my tests provide evidence of heightened investor reactions when a WSJ article covers an earnings announcement. In this section, I examine the content of the WSJ articles in an attempt to isolate the effects of analysis from dissemination. If the analysis provided by these articles is driving the result, then I expect the effect to vary with the extent to which the article provides original analysis versus disseminates firm-generated content. Also, WSJ articles may fill a dissemination role simply by making more investors aware (i.e., signaling) that the firm has announced earnings. Finding that variation in the impact of WSJ articles depends on their content would be inconsistent with signaling being the only explanation, since all WSJ articles have this feature.

To measure the extent of analysis and dissemination, I estimate the textual similarity between the firm's earnings press release and the associated WSJ article (when applicable). Specifically, I use the [41] similarity measure, which is the proportion of the total unique words in either document (i.e., union) that is used in both documents (i.e., intersection). Several studies use this measure and highly related variations as a proxy for the information content of media coverage and firms' disclosures. For example, [69] finds firms' stock returns respond more to firm-specific news articles that have fewer words in common with past articles. Also, [17] find that 10-K and 10-Q filings with text that significantly differs from

past filings portend negative stock returns. In my setting, WSJ articles with fewer words in common with the earnings release likely contain more new value-relevant information (or analysis), while those with more of the firm's words likely fill more of a dissemination role.

As discussed above, before calculating the measure, I identify and clean the press release and any WSJ articles associated with each earnings announcement (see Section 3.2 and Appendix B). Next, I remove common and near-duplicate words.[19] The remaining words comprise the documents' unique word sets. [41] similarity between firm $i$'s WSJ article $j$ and press release $k$ is given by

$$Similarity_{ijk} = \frac{\#(C_{ij} \cap P_{ik})}{\#(C_{ij} \cup P_{ik})}, \tag{4}$$

where $C_{ij}$ and $P_{ik}$ are the unique word sets for the WSJ article and press release, respectively, and the # operator denotes the number of elements in a set. I can calculate this measure for the 3,059 (out of 8,159 total) sample earnings announcements with a firm-specific WSJ article. The mean of *Similarity* is 0.133 and the interquartile range is 0.105 to 0.161, which resembles prior research [e.g., 69].[20]

My proxy for the amount of analysis in the WSJ article is *Analysis*, defined as the product of negative one and the residuals from a regression of *Similarity* on the log of the number of total words in the press release and WSJ article. Controlling for the documents' length mitigates concerns that variation in the *amount* of disclosure or WSJ coverage explains my results [37]. Press release length and WSJ article length together explain 52% of the variation in *Similarity*, demonstrating both that length is a major determinant of the measure and that a significant portion of the variation remains to be explained by other factors.

Because I multiply the residuals by negative one, *Analysis* is higher when the earnings

[19]In particular, I exclude a list of stop words made up of the generic, dates, and numbers lists available on Bill McDonald's website, http://www3.nd.edu/~mcdonald/Word_Lists.html, and any of the 100 most common English words, https://tinyurl.com/OxfordEnglishFacts, not in these lists. I use the Porter word-stemming algorithm available with Python's nltk.stem.porter package to equate similar word forms.

[20]Table 1 in [69] reports a mean of 0.117 and interquartile range of 0.075 to 0.133 for the analagous measure, "*Stale1*". However, note that our measures are not directly comparable: he compares news articles to past news articles while I compare them to concurrent firm disclosures.

release and WSJ article are *less* similar. I argue that low similarity (or high *Analysis*) results from the production of more original analysis that could help investors understand the implications of earnings. For ease of interpretation, in regression analyses I use ${}^{R}ANALYSIS$, defined as the quintile rank of *Analysis* scaled to range between 0 and 1.

In Table 5, I report tests of the hypothesis that WSJ articles impact market reactions primarily by providing analysis. These tests add interactions between $Treatment \times Coverage$ (or $Surprise \times Treatment \times Coverage$ in the ERC test) and ${}^{R}Analysis$ to the regressions in Tables 3 and 4. Since only observations with WSJ coverage have nonzero values for ${}^{R}Analysis$, its main effect is perfectly collinear with the interaction term and omitted from the regressions accordingly.

Panel A shows that the effect of WSJ articles on abnormal volume is concentrated in the articles that likely contain more original analysis and absent among articles that disseminate more information from the firm's press release. The coefficients on $Treatment \times Coverage \times {}^{R}Analysis$ are statistically significant and economically sizable. Specifically, the coefficient value of 0.084 implies that a WSJ article in the highest quintile of *Analysis* increases abnormal volume by about 12% (i.e., $(0.084 \times 1.036)/0.731$), compared to earnings announcements with no WSJ coverage or an article that largely disseminates press release content. This increase is equivalent to moving from the 50th to the 57th percentile of the abnormal volume distribution (i.e., $0.417 + (0.084 \times 1.036) = 0.504 \approx$ 57th percentile). Moreover, the insignificant coefficients on $Treatment \times Coverage$ suggest low-analysis WSJ articles do not increase abnormal volume, perhaps because they disseminate content that is widely available from other sources, such as the firm's press release or other media outlets and data providers.

Panel B tests the effect of WSJ analysis on the ERC. The findings are similar to those for abnormal volume. In particular, the coefficients on $Surprise \times Treatment \times Coverage \times {}^{R}Analysis$ are positive, statistically significant, and larger than the $Surprise \times Treatment \times Coverage$ coefficients in Panel A of Table 4. Articles in the highest *Analysis* quintile lead to an ERC that is about 40% larger than announcements without WSJ coverage or with low-analysis articles (i.e., $0.062/0.156 = 0.40$). This finding that WSJ articles make investors'

immediate response more closely aligned with reported earnings suggests that the media provides independent verification of the outputs of firms' accounting systems, similar to other information intermediaries such as external auditors and sell-side analysts.

Panel C similarly shows the stock price response to earnings is more timely when WSJ articles likely contain more original analysis than firm-generated content. The coefficients on $Treatment \times Coverage \times^{R} Analysis$ are positive and statistically significant, while the coefficients on $Treatment \times Coverage$ are statistically indistinguishable from zero. In terms of economic magnitude, the 0.054 coefficient on $Treatment \times Coverage \times^{R} Analysis$ implies WSJ articles in the highest quintile of *Analysis* increase intraperiod timeliness by approximately 13% of the unconditional sample mean (i.e., $(0.054 \times 9.389)/4.005$) or from the 50th to the 61st percentile of the distribution (i.e., $4.273 + (0.054 \times 9.389) = 4.78 \approx$ 61st percentile).

Together, the results in this section are consistent with WSJ analysis enhancing market reactions to earnings news. In addition, the absence of an effect among the articles that disseminate the most press release content mitigates concerns that my results are driven by the WSJ filling a dissemination role. This variation in articles' impact also raises a question for future research: what factors (e.g., journalists' experience, education, and network) explain why some articles inform investors and others do not?

# 5. Additional Analyses

## 5.1 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 have been parallel trends in the post-period absent treatment.

Following [7], 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, *Treatment*. I estimate this regression for each pre-treatment year: 2006, 2007, and the fourth quarter of 2012 through the third quarter of 2013. The coefficient on *Trend* reflects whether the dependent variable changes over the year for control firms, and the coefficient on $Trend \times Treatment$ reflects the difference in trends between the treatment and control groups. As seen in Panels A-C of Table 6, none of the $Trend \times Treatment$ coefficients are significantly different from zero, suggesting the treatment and control groups have similar pre-treatment trends.

## 5.2 Quarter-Specific Effects

As seen in Figure 1, WSJ restructurings affect the amount of earnings announcement coverage gradually over one or two quarters. Accordingly, I estimate versions of equations (1) and (3) that consider whether the effect of WSJ articles on market reactions takes one or more quarters to appear. Specifically, I replace $Treatment \times Coverage$ with $Treatment \times Coverage1$ - $Treatment \times Coverage4$, indicator variables set to one if the treatment quarter is the first through fourth quarter following the respective WSJ restructuring event.

Panel A of Table 7 shows the abnormal volume effect takes about a quarter to appear, consistent with WSJ coverage changing over a few months and supporting my choice to avoid assigning treatment based on coverage immediately preceding or following WSJ restructuring. Panel B shows the $Treatment \times Coverage1$ - $Treatment \times Coverage4$ coefficients are all positive across the different ERC specifications, demonstrating that the result is not concentrated in a single quarter. Finally, consistent with Panel C of Table 4, Panel C of Table 7 does not show that the immediate (i.e., six trading day) price response to earnings releases is more timely when there is WSJ coverage, on average. Specifically, the coefficients on $Treatment \times Coverage1$ - $Treatment \times Coverage4$ are positive in all specifications but not statistically significant in most cases.

## 5.3 Matched Samples

The typical endogeneity concern in the business press literature is that firms with changing prospects (i.e., significant news) experience heightened capital market activity, such as trading volume and extreme stock returns, that attracts additional media coverage. In this section, I match each treatment firm to a control firm based on the amount of recent firm news. This matching procedure helps ensure that treatment and control firms have similar newsworthiness, which provides further assurance that they have parallel trends in earnings-

announcement reactions.

To measure treatment and control firms' prospects, or news, I rely on the vast literature in accounting and finance that establish stock returns and earnings as key indicators of firm-specific news. In particular, I match firms on the announced earnings surprise, *Surprise*, and the volatility of daily stock returns in the month before the earnings announcement, *Volatility*. I also match on $\Delta Volatility$, which is the difference between volatility at the post- and pre-treatment earnings announcements used to assign treatment status. Matching on the change in volatility addresses the possibility that changes in WSJ coverage are driven by the WSJ selecting increasingly salient firms to cover, instead of the restructuring events impacting the WSJ resources available to cover already large, visible firms. I use nearest neighbor matching within caliper, whose width is set to one-half the variable's standard deviation [58].[21]

Table 8 repeats the analyses from Table 5 using the three matched samples, instead of the full control sample. These estimates corroborate the evidence in the prior section for all three dependent variables. In fact, the coefficients on the interaction of the treatment indicator and the analysis variable, $Treatment \times Coverage \times^{R} Analysis$, are slightly larger using the matched samples. As before, none of the main effects of $Treatment \times Coverage$ (or $Surprise \times Treatment \times Coverage$ in the ERC test) are significantly different from zero, so I omit them from the tables for brevity. Together, these tests suggest the coverage changes arose exogenously as a result of the WSJ adjusting the resources devoted to earnings coverage, instead of endogenously as a result of changes to firms' prospects. This evidence suggests that this study's variation in WSJ coverage is exogenous, further validating my inference that WSJ articles impact the market's reaction to earnings news.

[21] Depending on the matching variable, between three and 10 of the 1,973 treatment observations do not have a match fitting the one-half standard deviation criteria. Because these observations are outliers by definition, I omit them so they do not have undue influence on my estimates.

## 5.4 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 analysis 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 28], 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 analysis could help institutional investors too. In this section, I address this question empirically by separately considering retail and institutional earnings-announcement trading volume.

[9] explain how to determine 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, *% Retail Volume*. Table 9 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 68 bps. As one would hope, the two components sum to roughly the total abnormal trading volume shown in Table 2. Retail investors trade approximately 4.3% 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 Table 5 for both types of investor; that is, WSJ articles that contain original analysis increase both retail and institutional trading volume. Specifically, the coefficients on $Treatment \times Coverage \times^{R} Analysis$ have roughly the same magnitude, as well as statistical and economic significance, in the abnormal retail and institutional volume regression as in the total abnormal volume regression in Table 5. The coefficient on $Treatment \times Coverage \times^{R} Analysis$ in the *% Retail Volume* regression leads to a similar inference, that WSJ articles do not make retail investors trade 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 impact a diverse set of market participants.

## 5.5 Overreaction

Throughout the paper, I assume higher abnormal trading volume, earnings response coefficients, and intraperiod timeliness indicate a more efficient market response to earnings. However, it is well known that there is little, if any, mispricing of large firms' earnings [31, 5]. 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 untabulated tests, I consider the relationship between abnormal returns and earnings surprise in the quarter following the earnings announcement. Specifically, I repeat the tests in Panel A of Table 4 and Panel B of Table 5 using abnormal returns during the 60 trading days following the earnings announcement, instead of announcement returns, as the dependent variable. 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 $Treatment \times Coverage$ and $Treatment \times Coverage \times {}^{R}Analysis$ are statistically and economically insignificant, suggesting WSJ articles do not induce overreaction. Instead, my results are consistent with high-analysis WSJ articles helping investors fully impound earnings news into price a few hours or days earlier than they would otherwise.

## 5.6 Good vs. Bad News

Several studies highlight the role of the media in covering negative news events that managers and analysts may lack proper incentives to disclose or emphasize [e.g., 53, 18, 55]. Thus, in this section I consider whether earnings analysis is more helpful to investors when the firm reports negative news.

When I split the sample based on whether the firm reported a non-negative earnings surprise and repeat the tests of Table 5, I do not find the results are consistently stronger for negative news announcements (untabulated). This may be because the incentive for journalists to emphasize negative news is not as pronounced during routine earnings announcement coverage. That is, some (typically large) firms attract significant attention whether they report good or bad news, explaining why journalists cover them as a matter of course. It could also be that journalists' skepticism helps investors determine the reliability of managers' good news, offsetting their previously documented value in slanting towards negative news.

Alternatively, since [55] find negative coverage foreshadows negative earnings surprises, I might not find stronger effects at bad news announcements because the media has already conveyed the negative information to the market before the announcement.

# 6. Conclusion

Several studies provide evidence that media coverage affects the market's earnings response by disseminating the news to a wider audience [e.g., 12, 24, 7]. However, this is the first study to document evidence of editorial content (or analysis) in full articles impacting trading following earnings announcements. In particular, I find that trading volume is higher and price responses more efficient when an earnings announcement is the subject of a WSJ article. These results are stronger for WSJ articles with fewer words in common with the firm's earnings press release, meaning the article likely provides more original editorial content that helps investors evaluate the firm's news.

Three WSJ restructuring events produced plausibly exogenous variation in WSJ coverage. Because my empirical tests exploit this variation, the results likely reflect the causal impact of WSJ articles on the market's reaction to earnings news.

Providing earnings-related analysis that facilitates investors' decisions likely helps the business press continue to attract and retain readers in the face of increasing competition from alternative, often low-cost, information sources. In general, consumers are shifting from traditional media towards digital offerings. As a result, in 2016 global ad spending in newspapers and digital banners, the traditional business press's main sources of ad revenue, held steady, while ad spending in online videos and social media grew by almost 40% [15]. Even the WSJ's ad revenue decreased 21% during the first quarter of 2017.

In the context of earnings coverage, several studies document advances in dissemination technology, such as robo-journalism and Twitter, that increase investor awareness of

firms' earnings [7, 8]. However, these technologies may fail to capture relevant information in earnings releases that human reporters are more likely to notice [32]. Consistent with reporters still filling a role in investors' decision-making, the WSJ continues to devote significant resources, roughly $65 million each year, to earnings-related coverage.[22] My study improves understanding of the media's role in capital markets by providing evidence that these earnings articles facilitate investors' decisions, which helps explain why readers continue to demand and the media continues to supply this type of coverage.

[22] I arrived at this $65 million estimate as follows. News Corp.'s "News and Information Services" segment, which includes the WSJ, earned 64% of the firm's total revenue in 2016. About half the segment's subscribers are WSJ subscribers. As the segment's flagship publication, the WSJ has relatively high subscription and advertising prices. However, I conservatively assume it earned only half of segment revenues, or 32% of News Corp.'s revenue. Applying this percentage to the firm's operating expenses, the WSJ cost about $2.6 billion to operate. Finally, each year approximately 1,000 (out of 40,000 total, or 2.5%) WSJ articles cover corporate earnings, implying earnings coverage costs the WSJ roughly $65 million (i.e., $2.6 billion $\times$ 2.5%).

# Appendix A

# Example Articles

The following excerpt is from an article that was published on WSJ.com at 7:02 pm on January 22, 2014.[1] An almost identical version of the article was published on page B6 of the WSJ print edition the next morning. Additional examples of recent earnings announcement articles are available at https://www.wsj.com/news/types/earnings.

**General Dynamics in Deal to Buy Back $1.1 Billion in Stock; Company Reports Fourth-Quarter Profit but Warns of Lower 2014 Results**

By Doug Cameron

...The company's shares rose on the prospect of a buyback Wednesday, despite an outlook for lower profit and sales this year as another slide in its defense business outpaced more deliveries of its high-end Gulfstream business jets.

General Dynamics was behind its competitors in returning cash to shareholders in the second half of 2013 because of pension-accounting issues. Still, it announced plans to buy back more

[1]Used with permission from The Wall Street Journal, WSJ.com. Copyright 2014 Dow Jones & Company, Inc. All rights reserved. The full article is available online at https://www.wsj.com/articles/general-dynamics-swings-to-profit-1390397622. The title of the article was updated a few hours after publication, explaining the January 23, 2014 12:02 am time stamp of the online version.

stock in the first quarter and seek board approval next month for more repurchases ...

General Dynamics, the world's fourth-largest military contractor by revenue, is the first of the sector heavyweights to provide guidance in the wake of Congress passing a $582 billion military-spending bill for fiscal 2014. The budget is down $3 billion from 2013 levels. While it boosts spending for shipbuilders such as General Dynamics, planned cuts include segments such as the company's vehicles business, which was already under strain.

The army-focused combat-systems group has been the hardest hit by cuts in U.S. military spending and delays in completing overseas contracts. However, the unit's performance is expected to stabilize, with revenue projected down 4% to 4.5% in 2014 after a 23% year-over-year decline in 2013. The guidance assumes it secures a $1.2 billion deal from an unnamed overseas customer ...

The 2014 guidance came as General Dynamics reported a forecast-beating profit of $495 million, or $1.40 a share, compared with a year-earlier loss of $2.13 billion, or $6.07 a share. The latest period included a $129 million loss in discontinued operations related to the pending settlement of the long-standing A-12 fighter jet litigation. Per-share earnings from continuing operations were $1.76, a penny above analysts' expectations. Revenue increased 0.4% to $8.11 billion.

Order backlog declined to $46 billion at year-end from $51.3 billion at the end of 2012.

# Appendix B

# Content Analysis

## B.1 Identifying and cleaning press releases

Each SEC filing has a unique EDGAR index path that links to raw text of the complete filing content. For example, the path for the 01/30/2008 earnings release filed on form 8-K by Constellation Energy is https://www.sec.gov/Archives/edgar/data/1004440/0001104659-08-005536.txt. I first use the Python urllib.request package to download this text for each filing. Next, I use a regular expression to keep the Exhibit 99 part of the filing. The filename of the Exhibit 99 included with Constellation Energy's 8-K is a08-3933_1ex99.htm. The following regular expression[1] keeps the content of this Exhibit 99:

'<FILENAME>a08-3933_1ex99.htm\s(.*?)<\/DOCUMENT>'

Press release wires require firms to provide contact information [63]. Additionally, the press release description, header, footer, or body almost always include a variation of the phrase "press release." I use these two characteristics of press releases to distinguish Exhibit 99s that contain a release from those that do not. Specifically, I keep the Exhibit 99 if the following regular expression[2] finds a match in the document's text:

[1] I specify the re.DOTALL flag of the re package in Python to match any character, including new lines.
[2] I specify the re.I flag of the re package in Python to ignore the text's case.

'((?:press|immediate|news|media) release|contact[^e]+?)'

The '[^e]' part of the expression avoids matching the word contacted because in nonpress releases firms often refer to an entity that they have contacted or that has contacted them.

I also clean press releases before comparing them to press coverage. Because these press releases are taken from 8-K filings stored on EDGAR, I use procedures similar to those used by [50], [52], and [25] to clean EDGAR 10-K filings. Specifically, I remove all HTML text and remaining tags (e.g., <TEXT>, <DOCUMENT>) using Python's html.parser package. Tables are removed unless they contain more than 80 percent alphabetic characters because table tags are occasionally used to format text, especially when a firm files the press release as a .txt file. I delete lines with fewer than 20 characters or 15 alphanumeric characters to remove section headings and any remaining lines of just numbers. Finally, paragraphs are only kept if they have 50 percent or more alphabetic characters and 80 or more characters.

Occasionally, firms file multiple Exhibit 99s with the same 8-K. So I keep the first Exhibit 99 listed in the 8-K, which is almost always the earnings press release. However, in a very few cases this procedure identifies earnings conference call transcripts or legal documents, instead of the earnings press release. The conference call transcripts are typically much more than 7,000 words, and legal documents typically have a Fog-index greater than 30, while earnings press releases almost never fit either criteria. Accordingly, I remove 185 documents from the sample that have either more than 7,000 words or Fog-index greater than 30.

## B.2 Factiva search string

To ensure a firm is the focus of an article, I retain only WSJ print and online articles from Factiva that (1) are tagged with the firm's DJ company code, (2) include the firm's name in the headline, (3) mention the firm at least twice, and (4) contain at least 50 words [69].

I use Factiva's Free Text Search function to implement these criteria. For example, the

following search string identifies articles for Avery Dennison Corp.:

> 'fds=avryi and hd=(AVERY or DENNISON) and (atleast2 AVERY or atleast2 DENNISON) and wc>49 and (rst=j or rst=wsjo)'

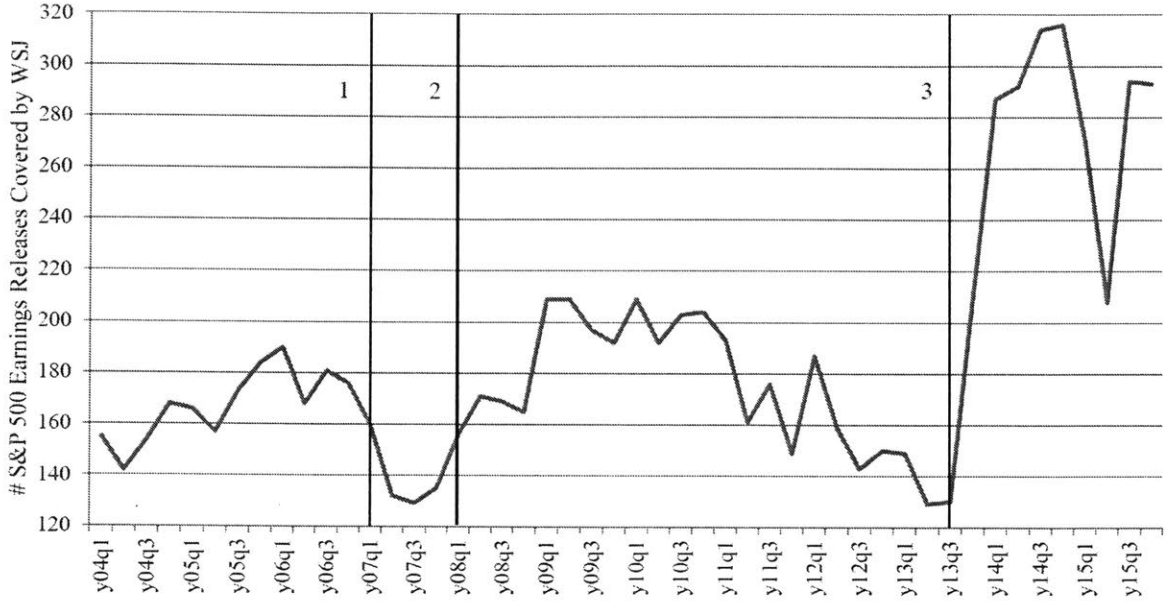
where fds= searches the company code field, hd= searches the article headline field, atleast2 is an operator requiring the subsequent search term to appear at least twice in the article body (including headline), wc is the word count operator, and rst= requires the source code field to equal j (WSJ print) or wsjo (WSJ online).

# Appendix C

# Figures

# Figure 1. WSJ Earnings Coverage Across Time

This figure shows the number of S&P 500 earnings reports covered by the WSJ during each quarter of 2004-2015. The timing of the three WSJ restructuring events is denoted by vertical lines and numbers in the figure.



# Appendix D

# Tables

# Table 1. Difference-in-Differences Research Design

Panel A presents an example of my difference-in-differences design, showing values of *Treatment* × *Coverage* for several firms across the sample period. *Treatment* × *Coverage* is an indicator variable set to one for treatment firms' earnings announcements that occur during the period with WSJ earnings announcement coverage, which is the pre(post)-treatment period for the coverage decreasing (increasing) WSJ restructuring events; zero for treatment firms' earnings announcements that occur during the period without WSJ earnings announcement coverage; and zero for all control firms' earnings announcements. I assign firms to the treatment and control groups based on whether the WSJ covered their earnings release in the *second* quarters before and after each event. Assignment quarters are highlighted gray in the table header (e.g., third quarter of 2006 or "y06q3"). To save space, the example only shows the two quarters on either side of the events. The "Groups" column summarizes the firm's assigned group in each event period; where A denotes the always control group, N denotes the never control group, T denotes the treatment group, and . denotes the firm was not in the S&P 500 during that part of the sample period. For example, Aflac's values "N/N/T" mean that Aflac was in the never (never) [treatment] group at the first (second) [third] event. Panel B shows the number and percentage of firms in the treatment and control groups during each quarter of the sample period.

## Panel A: Example of Research Design

| | | Event 1 - January 2007 | | | | Event 2 - January 2008 | | | | Event 3 - Summer 2013 | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **Firm** | **Groups** | **y06q3** | **y06q4** | **y07q1** | **y07q2** | **y07q3** | **y07q4** | **y08q1** | **y08q2** | **y13q2** | **y13q3** | **y13q4** | **y14q1** |
| Aflac | N/N/T | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 1 |
| Alcoa | A/A/A | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Barr Pharma. | T/T/. | 1 | 1 | 0 | 0 | 0 | 0 | 1 | 1 | . | . | . | . |
| Baxter Int. | ./T/T | . | . | . | . | 0 | 0 | 1 | 1 | 0 | 0 | 1 | 1 |
| DTE Electric | N/N/N | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Eaton Corp. | T/T/. | 1 | 1 | 0 | 0 | 0 | 0 | 1 | 1 | . | . | . | . |
| General Mills | A/A/A | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Genuine Parts | N/N/N | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| H&R Block | A/T/N | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 1 | 0 | 0 | 0 | 0 |
| Hasbro | A/A/A | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| ITT Inc. | N/T/. | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 1 | . | . | . | . |
| Lam Research | ././N | . | . | . | . | . | . | . | . | 0 | 0 | 0 | 0 |
| Leggett & Platt | N/N/N | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| McDonald's | A/A/A | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Nucor | N/T/T | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 1 | 0 | 0 | 1 | 1 |
| OfficeMax | T/./. | 1 | 1 | 0 | 0 | . | . | . | . | . | . | . | . |
| Omnicom Group | T/T/T | 1 | 1 | 0 | 0 | 0 | 0 | 1 | 1 | 0 | 0 | 1 | 1 |
| Ross Stores | ././N | . | . | . | . | . | . | . | . | 0 | 0 | 0 | 0 |

## Panel B: Sample Distribution by Year-Quarter

| Year-Quarter | #Always | #Never | #Treatment | | %Always | %Never | %Treatment |
|---|---|---|---|---|---|---|---|
| y06q1 | 83 | 209 | 59 | | 24 | 60 | 17 |
| y06q2 | 87 | 207 | 62 | | 24 | 58 | 17 |
| y06q3 | 94 | 228 | 67 | | 24 | 59 | 17 |
| y06q4 | 84 | 218 | 58 | | 23 | 61 | 16 |
| y07q1 | 89 | 213 | 66 | | 24 | 58 | 18 |
| y07q2 | 91 | 224 | 65 | | 24 | 59 | 17 |
| y07q3 | 101 | 261 | 58 | | 24 | 62 | 14 |
| y07q4 | 95 | 254 | 53 | | 24 | 63 | 13 |
| y08q1 | 95 | 256 | 60 | | 23 | 62 | 15 |
| y08q2 | 101 | 254 | 58 | | 24 | 62 | 14 |
| y08q3 | 98 | 243 | 53 | | 25 | 62 | 13 |
| y08q4 | 91 | 241 | 51 | | 24 | 63 | 13 |
| y12q4 | 106 | 173 | 153 | | 25 | 40 | 35 |
| y13q1 | 103 | 180 | 157 | | 23 | 41 | 36 |
| y13q2 | 112 | 181 | 163 | | 25 | 40 | 36 |
| y13q3 | 106 | 174 | 160 | | 24 | 40 | 36 |
| y13q4 | 107 | 176 | 161 | | 24 | 40 | 36 |
| y14q1 | 109 | 183 | 161 | | 24 | 40 | 36 |
| y14q2 | 108 | 175 | 155 | | 25 | 40 | 35 |
| y14q3 | 105 | 171 | 153 | | 24 | 40 | 36 |
| Total: | 1965 | 4221 | 1973 | Mean: | 24 | 52 | 24 |

# Table 2. Descriptive Statistics

Panel A reports distributional statistics for dependent and control variables of 8,159 S&P 500 earnings announcements within a year of three WSJ restructuring events. Panel B shows means of control variables for the treatment sample compared to the control samples. The first two columns include firms that received WSJ coverage both before and after ("Always" group) or neither before nor after ("Never" group) WSJ restructuring. The third column combines the two groups of control firms, and the fourth column includes treatment firms. *Abnormal Volume* is the difference between average daily share turnover (volume divided by shares outstanding) during the earnings announcement, $t-1$ to $t+1$, and during the 60 prior trading days, multiplied by 100. *Abnormal Returns* is market-adjusted buy-and-hold returns during the earnings announcement, $t-1$ to $t+1$, in percentage terms. *Surprise* is the difference between actual earnings per share (Compustat item EPSPXQ) and the median analyst forecast scaled by the stock price at the prior quarter end, in percentage terms. *Intraperiod Timeliness (IPT)* is defined as follows: $IPT = 1/2\sum_{i=0}^{5}(CABRET_{t+i-1} + CABRET_{t+i})/(CABRET_{t+5})$, where $CABRET_{t+i}$ is the cumulative market-adjusted buy-and-hold return from earnings announcement date, $t$, through day $t+i$, scaled by the total cumulative return over the window $t$ through $t+5$. *Market Cap* is the firm's stock market capitalization, calculated as the product of stock price and shares outstanding. *Book-To-Market* is the book equity to market capitalization ratio. *Return Volatility* is the standard deviation of monthly returns over the prior year. *#Employees* and *#Owners* are the fiscal year-end number of employees and shareholders in hundred thousands, respectively. *Institutional Ownership* is the proportion of shares held by institutional owners. *#Analysts* is the number of analysts forecasting the firm's earnings. *Prior Press Coverage* is the number of days in the prior year that the firm received Dow Jones news coverage. *#8-K Disclosures* is the number of 8-Ks filed during the prior year. *Complexity* is the first principal component of *Readable*, *Hard Info*, and *Specific*. *Readable* is the product of negative one and the [35] Fog index, which is an increasing function of the average number of words in a sentence and the percent of words in excess of two syllables. *Hard Info* is a count of numbers (other than dates) in the text scaled by the total number of words, multiplied by 1,000. *Specific* is a count of entities (location, person, organization, money, percent, date, and time) identified by the Stanford Named Entity Recognizer (NER) scaled by the total number of words, multiplied by 1,000. *Bad News* is an indicator set to one if the earnings surprise is negative, and zero otherwise. *#Announcements* is the number of other Compustat firms announcing earnings during the firm's earnings announcement window, $t-1$ to $t+1$. I winsorize continuous variables, except *Abnormal Returns*, at the 1st and 99th percentiles within each year-quarter.

## Panel A: Full Sample

| Variable | Mean | Std. Dev. | 25th | Median | 75th |
|---|---|---|---|---|---|
| *Abnormal Volume* | 0.731 | 1.036 | 0.152 | 0.417 | 0.926 |
| *Abnormal Returns* | 0.020 | 5.640 | -2.838 | -0.027 | 2.964 |
| *Surprise* | -0.320 | 2.368 | -0.256 | 0.000 | 0.136 |
| *Intraperiod Timeliness* | 4.005 | 9.389 | 2.823 | 4.273 | 5.608 |
| *Market Cap* | 27.354 | 43.131 | 7.376 | 13.853 | 26.721 |
| *Book-To-Market* | 0.441 | 0.313 | 0.234 | 0.364 | 0.570 |
| *Return Volatility* | 0.223 | 0.100 | 0.155 | 0.199 | 0.267 |
| *#Employees* | 0.459 | 0.687 | 0.087 | 0.208 | 0.501 |
| *#Owners* | 0.673 | 1.665 | 0.038 | 0.156 | 0.536 |
| *Institutional Ownership* | 0.699 | 0.251 | 0.648 | 0.767 | 0.855 |
| *#Analysts* | 13.402 | 8.746 | 7 | 14 | 19 |
| *Prior Press Coverage* | 116.511 | 74.868 | 73 | 117 | 157 |
| *#8-K Disclosures* | 16.364 | 8.422 | 11 | 14 | 20 |
| *Complexity* | 0.011 | 0.974 | -0.670 | -0.009 | 0.611 |
| *Bad News* | 0.473 | 0.499 | 0 | 0 | 1 |
| *#Announcements* | 633.726 | 335.998 | 367 | 579 | 903 |

**Panel B: Comparison of Treatment/Control Group Means**

| Variable | Control | | | Treatment | Difference | t-stat. |
|---|---|---|---|---|---|---|
| | Always | Never | All | | | |
| *Market Cap* | 46.639 | 16.566 | 25.615 | 24.305 | 1.310 | 0.47 |
| *Book-To-Market* | 0.401 | 0.432 | 0.423 | 0.456 | -0.033 | -1.42 |
| *Return Volatility* | 0.225 | 0.229 | 0.228 | 0.237 | -0.009 | -1.36 |
| *#Employees* | 0.793 | 0.285 | 0.432 | 0.399 | 0.033 | 0.76 |
| *#Owners* | 0.975 | 0.444 | 0.603 | 0.562 | 0.041 | 0.39 |
| *Institutional Ownership* | 0.691 | 0.699 | 0.695 | 0.696 | -0.001 | -0.04 |
| *#Analysts* | 15.986 | 11.306 | 12.686 | 15.949 | -3.263*** | -5.32 |
| *Prior Press Coverage* | 154.753 | 92.873 | 111.134 | 123.031 | -11.897** | -2.35 |
| *#8-K Disclosures* | 17.046 | 15.959 | 16.241 | 16.114 | 0.127 | 0.23 |
| *Complexity* | 0.023 | 0.000 | 0.006 | 0.097 | -0.091 | -1.51 |
| *Bad News* | 0.422 | 0.501 | 0.476 | 0.506 | -0.030 | -1.29 |
| *#Announcements* | 547.386 | 686.278 | 645.122 | 606.800 | 38.322** | 1.99 |
| Observations | 1,965 | 4,221 | 6,186 | 1,973 | | |

# Table 3. Effect of WSJ Coverage on Abnormal Volume

This table shows difference-in-differences estimates from abnormal volume regressions. That is, I regress *Abnormal Volume* on the treatment indicator (*Treatment* × *Coverage*) and control variables. The sample consists of 8,159 S&P 500 earnings announcements within a year of three WSJ restructuring events. Variables are defined in Table 2. I standardize continuous variables to have unit variance to facilitate the interpretation of coefficients. *t*-statistics are based on standard errors that are clustered by both firm and year-quarter using the procedure developed by [70]. All regressions include firm and year-quarter fixed effects. ***, **, and * indicate significance at the 1, 5, and 10 percent two-tailed level, respectively.

| Dependent Variable = | *Abnormal Volume* | | | |
|---|---|---|---|---|
| Variable | Coef. | t-stat. | Coef. | t-stat. |
| *Treatment × Coverage* | 0.039* | 1.75 | 0.038* | 1.70 |
| *ln(Market Cap)* | | | -0.057** | -2.23 |
| *ln(1+Book-To-Market)* | | | -0.013 | -0.57 |
| *Return Volatility* | | | -0.018 | -0.95 |
| *ln(1+#Employees)* | | | -0.024 | -1.47 |
| *ln(1+#Owners)* | | | 0.032* | 2.08 |
| *Institutional Ownership* | | | -0.019 | -1.70 |
| *ln(1+#Analysts)* | | | -0.013 | -0.70 |
| *ln(1+Prior Press Coverage)* | | | -0.026* | -1.74 |
| *ln(1+#8-K Disclosures)* | | | -0.027** | -2.51 |
| *Complexity* | | | 0.000 | -0.03 |
| *Bad News* | | | 0.076*** | 4.48 |
| *ln(1+#Announcements)* | | | -0.023 | -0.91 |
| Firm & Yr.-Qtr. FEs? | Yes | | Yes | |
| $R^2$ | 0.0456 | | 0.0559 | |
| Observations | 8,159 | | 8,159 | |

# Table 4. Effect of WSJ Coverage on Price Discovery

This table shows difference-in-differences estimates from price discovery regressions. In Panel A, I regress *Abnormal Returns* on the earnings surprise (*Surprise*), the treatment indicator (*Treatment* × *Coverage*), control variables, and interaction terms. In Panel B, I regress *Intraperiod Timeliness* on *Treatment* × *Coverage* and control variables. The sample consists of 8,159 S&P 500 earnings announcements within a year of three WSJ restructuring events. The IPT tests include 6,080 observations because I exclude observations with absolute cumulative return less than 2%. Variables are defined in Table 2. I standardize continuous variables to have unit variance to facilitate the interpretation of coefficients. *t*-statistics are based on standard errors that are clustered by both firm and year-quarter using the procedure developed by [70]. All 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

| Dependent Variable = | *Abnormal Returns* | | | | | |
|---|---|---|---|---|---|---|
| Variable | Coef. | t-stat. | Coef. | t-stat. | Coef. | t-stat. |
| *Surprise* | 0.160*** | 8.83 | 0.099*** | 5.29 | 0.233 | 0.51 |
| *Surprise* × *Treatment* × *Coverage* | 0.059*** | 2.88 | 0.047** | 2.26 | 0.048** | 2.31 |
| *Treatment* × *Coverage* | 0.023 | 1.15 | 0.020 | 1.00 | 0.020 | 1.01 |
| *ln(Market Cap)* | | | -0.147*** | -7.22 | -0.144*** | -7.00 |
| *ln(1+Book-To-Market)* | | | 0.041*** | 2.96 | 0.040*** | 2.91 |
| *Return Volatility* | | | 0.004 | 0.18 | 0.004 | 0.19 |
| *ln(1+#Employees)* | | | 0.019 | 1.52 | 0.016 | 1.32 |
| *ln(1+#Owners)* | | | -0.007 | -0.59 | -0.006 | -0.53 |
| *Institutional Ownership* | | | 0.012 | 0.89 | 0.011 | 0.82 |
| *ln(1+#Analysts)* | | | -0.026* | -1.86 | -0.025* | -1.82 |
| *ln(1+Prior Press Coverage)* | | | -0.016 | -0.72 | -0.014 | -0.64 |
| *ln(1+#8-K Disclosures)* | | | 0.021 | 1.47 | 0.022 | 1.55 |
| *Complexity* | | | 0.000 | 0.02 | -0.003 | -0.19 |
| *Bad News* | | | -0.117*** | -10.09 | -0.118*** | -9.00 |
| *ln(1+#Announcements)* | | | 0.022 | 1.44 | 0.022 | 1.40 |
| *Surprise* × *ln(Market Cap)* | | | | | 0.078 | 0.41 |
| *Surprise* × *ln(1+Book-To-Market)* | | | | | -0.042 | -0.56 |
| *Surprise* × *Return Volatility* | | | | | -0.041 | -0.50 |
| *Surprise* × *ln(1+#Employees)* | | | | | -0.135 | -1.26 |
| *Surprise* × *ln(1+#Owners)* | | | | | 0.099** | 2.29 |
| *Surprise* × *Institutional Ownership* | | | | | -0.009 | -0.30 |
| *Surprise* × *ln(1+#Analysts)* | | | | | 0.052 | 0.76 |
| *Surprise* × *ln(1+Prior Press Coverage)* | | | | | -0.041 | -0.78 |
| *Surprise* × *ln(1+#8-K Disclosures)* | | | | | 0.014 | 0.62 |
| *Surprise* × *Complexity* | | | | | -0.026 | -1.58 |
| *Surprise* × *Bad News* | | | | | -0.022 | -0.85 |
| *Surprise* × *ln(1+#Announcements)* | | | | | -0.170 | -1.04 |
| Firm & Yr.-Qtr. FEs? | Yes | | Yes | | Yes | |
| $R^2$ | 0.0314 | | 0.0652 | | 0.0674 | |
| Observations | 8,159 | | 8,159 | | 8,159 | |

**Panel B: Intraperiod Timeliness**

| Dependent Variable = | *Intraperiod Timeliness* | | | |
|---|---|---|---|---|
| Variable | Coef. | t-stat. | Coef. | t-stat. |
| *Treatment × Coverage* | 0.018 | 0.85 | 0.021 | 0.96 |
| *ln(Market Cap)* | | | 0.037 | 1.63 |
| *ln(1+Book-To-Market)* | | | 0.016 | 0.66 |
| *Return Volatility* | | | 0.010 | 0.55 |
| *ln(1+#Employees)* | | | -0.004 | -0.22 |
| *ln(1+#Owners)* | | | -0.009 | -0.67 |
| *Institutional Ownership* | | | -0.022* | -1.76 |
| *ln(1+#Analysts)* | | | -0.015 | -0.82 |
| *ln(1+Prior Press Coverage)* | | | 0.011 | 0.75 |
| *ln(1+#8-K Disclosures)* | | | 0.010 | 0.52 |
| *Complexity* | | | 0.007 | 0.45 |
| *Bad News* | | | -0.013 | -0.88 |
| *ln(1+#Announcements)* | | | -0.004 | -0.23 |
| Firm & Yr.-Qtr. FEs? | Yes | | Yes | |
| $R^2$ | 0.0061 | | 0.0077 | |
| Observations | 6,080 | | 6,080 | |

# Table 5. Effect of WSJ Analysis vs. Dissemination

This table shows difference-in-differences estimates from market reaction regressions that account for the amount of WSJ analysis versus dissemination. In Panel A (Panel B) [Panel C], I regress abnormal volume (earnings response coefficient, i.e., abnormal returns regressed on earnings surprise) [intraperiod timeliness] on the treatment indicator (*Treatment* × *Coverage*), the treatment indicator(s) interacted with the amount of analysis in the WSJ article ($^{R}$*Analysis*), and control variables. $^{R}$*Analysis* is the scaled (between 0 and 1) quintile rank of *Analysis*, which is the product of negative one and the residuals from a regression of the [41] similarity measure on the log of the number of total words in the press release and WSJ article. The main effect of $^{R}$*Analysis* is not included in the regressions because it is perfectly collinear with the interaction term, since only observations with WSJ coverage can have non-zero $^{R}$*Analysis*. Other variables are defined in Table 2. The sample consists of 8,159 S&P 500 earnings announcements within a year of three WSJ restructuring events. The IPT tests include 6,080 observations because I exclude observations with absolute cumulative return less than 2%. I standardize continuous variables to have unit variance to facilitate the interpretation of coefficients. *t*-statistics are based on standard errors that are clustered by both firm and year-quarter using the procedure developed by [70]. All regressions include firm and year-quarter fixed effects. ***, **, and * indicate significance at the 1, 5, and 10 percent two-tailed level, respectively.

## Panel A: Abnormal Volume

| Dependent Variable = | *Abnormal Volume* | | | |
|---|---|---|---|---|
| Variable | Coef. | t-stat. | Coef. | t-stat. |
| *Treatment* × *Coverage* | -0.027 | -1.22 | -0.025 | -1.19 |
| *Treatment* × *Coverage* × $^{R}$*Analysis* | 0.088** | 2.59 | 0.084** | 2.62 |
| Controls? | No | | Yes | |
| Firm & Yr.-Qtr. FEs? | Yes | | Yes | |
| $R^2$ | 0.0470 | | 0.0571 | |
| Observations | 8,159 | | 8,159 | |

## Panel B: Earnings Response Coefficients

| Dependent Variable = | *Abnormal Returns* | | | | | |
|---|---|---|---|---|---|---|
| Variable | Coef. | t-stat. | Coef. | t-stat. | Coef. | t-stat. |
| *Surprise* | 0.160*** | 8.82 | 0.100*** | 5.36 | 0.156 | 0.33 |
| *Surprise* × *Treatment* × *Coverage* | -0.005 | -0.23 | -0.001 | -0.04 | 0.001 | 0.05 |
| *Surprise* × *Treatment* × *Coverage* × $^{R}$*Analysis* | 0.083*** | 4.38 | 0.062*** | 2.97 | 0.062*** | 2.93 |
| *Treatment* × *Coverage* × $^{R}$*Analysis* | -0.033 | -1.71 | -0.035 | -1.69 | -0.037* | -1.74 |
| *Treatment* × *Coverage* | 0.047* | 1.89 | 0.045 | 1.71 | 0.045* | 1.73 |
| Controls? | No | | Yes | | Yes | |
| *Surprise* × Controls? | No | | No | | Yes | |
| Firm & Yr.-Qtr. FEs? | Yes | | Yes | | Yes | |
| $R^2$ | 0.0329 | | 0.0653 | | 0.0674 | |
| Observations | 8,159 | | 8,159 | | 8,159 | |

**Panel C: Intraperiod Timeliness**

| Dependent Variable = | *Intraperiod Timeliness* | | | |
|---|---|---|---|---|
| Variable | Coef. | t-stat. | Coef. | t-stat. |
| *Treatment* × *Coverage* | -0.021 | -1.03 | -0.020 | -0.94 |
| *Treatment* × *Coverage* × $^{R}$*Analysis* | 0.053** | 2.34 | 0.054** | 2.42 |
| Controls? | No | | Yes | |
| Firm & Yr.-Qtr. FEs? | Yes | | Yes | |
| $R^2$ | 0.0066 | | 0.0082 | |
| Observations | 6,080 | | 6,080 | |

# Table 6. 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 abnormal volume (earnings response coefficient, i.e., abnormal returns regressed on earnings surprise) [intraperiod timeliness] on a trend variable (*Trend*) that increases by one each quarter and the interaction between *Trend* and an indicator for treatment firms (*Treatment*). The main effect of *Treatment* is not included in the regressions because it is perfectly collinear with the interaction term, since the model includes firm fixed effects. Variables are defined in Table 2. *t*-statistics are based on standard errors that are clustered by both firm and year-quarter using the procedure developed by [70]. All regressions include firm and year-quarter fixed effects. ***, **, and * indicate significance at the 1, 5, and 10 percent two-tailed level, respectively.

## Panel A: Abnormal Volume

| Dependent Variable = | *Abnormal Volume* | | | | | |
|---|---|---|---|---|---|---|
| | 2006 | | 2007 | | 2013 | |
| Variable | Coef. | t-stat. | Coef. | t-stat. | Coef. | t-stat. |
| *Trend* | 0.062 | 0.27 | 0.235 | 1.73 | -0.289 | -0.61 |
| *Trend* × *Treatment* | 0.069 | 0.58 | -0.047 | -0.76 | -0.014 | -0.24 |
| Firm & Yr.-Qtr. FEs? | Yes | | Yes | | Yes | |
| $R^2$ | 0.0080 | | 0.0484 | | 0.0644 | |
| Observations | 1,456 | | 1,570 | | 1,768 | |

## Panel B: Earnings Response Coefficients

| Dependent Variable = | *Abnormal Returns* | | | | | |
|---|---|---|---|---|---|---|
| | 2006 | | 2007 | | 2013 | |
| Variable | Coef. | t-stat. | Coef. | t-stat. | Coef. | t-stat. |
| *Surprise* | 0.201** | 4.51 | 0.079* | 2.43 | 0.207** | 4.56 |
| *Surprise* × *Trend* | 0.022 | 0.48 | 0.089 | 2.08 | -0.046 | -1.19 |
| *Surprise* × *Trend* × *Treatment* | -0.082 | -1.48 | 0.012 | 0.23 | -0.016 | -0.30 |
| *Trend* × *Treatment* | -0.068 | -1.07 | -0.087 | -0.65 | -0.004 | -0.09 |
| *Trend* | -0.095 | -0.29 | -0.187 | -1.61 | 0.401 | 1.72 |
| Firm & Yr.-Qtr. FEs? | Yes | | Yes | | Yes | |
| $R^2$ | 0.0491 | | 0.0350 | | 0.0323 | |
| Observations | 1,456 | | 1,570 | | 1,768 | |

**Panel C: Intraperiod Timeliness**

| Dependent Variable = | *Intraperiod Timeliness* | | | | | |
|---|---|---|---|---|---|---|
| | 2006 | | 2007 | | 2013 | |
| Variable | Coef. | t-stat. | Coef. | t-stat. | Coef. | t-stat. |
| *Trend* | 0.082 | 0.63 | 0.055 | 0.43 | 0.094 | 0.47 |
| *Trend* × *Treatment* | 0.021 | 0.25 | -0.021 | -0.21 | -0.045 | -0.49 |
| Firm & Yr.-Qtr. FEs? | Yes | | Yes | | Yes | |
| $R^2$ | 0.0064 | | 0.0050 | | 0.0044 | |
| Observations | 1,048 | | 1,164 | | 1,297 | |

# Table 7. Quarter-Specific Effects

This table shows difference-in-differences estimates from abnormal volume and price discovery regressions in which I replace the treatment indicator with indicator variables set to one if the treatment quarter is the first through fourth quarter following the respective WSJ restructuring event. That is, in Panel A (Panel B) [Panel C] I estimate a regression of of abnormal volume (earnings response coefficient, i.e., abnormal returns regressed on earnings surprise) [intraperiod timeliness] on *Treatment* × *Coverage1* - *Treatment* × *Coverage4* and control variables. The sample consists of 8,159 S&P 500 earnings announcements within a year of three WSJ restructuring events. Variables are defined in Table 2. I standardize continuous variables to have unit variance to facilitate the interpretation of coefficients. *t*-statistics are based on standard errors that are clustered by both firm and year-quarter using the procedure developed by [70]. All regressions include firm and year-quarter fixed effects. ***, **, and * indicate significance at the 1, 5, and 10 percent two-tailed level, respectively.

## Panel A: Abnormal Volume

| Dependent Variable = | *Abnormal Volume* | | | |
|---|---|---|---|---|
| Variable | Coef. | t-stat. | Coef. | t-stat. |
| *Treatment* × *Coverage1* | -0.006 | -0.36 | -0.008 | -0.52 |
| *Treatment* × *Coverage2* | 0.036 | 1.32 | 0.036* | 1.34 |
| *Treatment* × *Coverage3* | 0.040* | 1.90 | 0.037* | 1.75 |
| *Treatment* × *Coverage4* | 0.017 | 1.38 | 0.018 | 1.45 |
| Controls? | No | | Yes | |
| Firm & Yr.-Qtr. FEs? | Yes | | Yes | |
| $R^2$ | 0.0460 | | 0.0563 | |
| Observations | 8,159 | | 8,159 | |

## Panel B: Earnings Response Coefficients

| Dependent Variable = | *Abnormal Returns* | | | | | |
|---|---|---|---|---|---|---|
| Variable | Coef. | t-stat. | Coef. | t-stat. | Coef. | t-stat. |
| *Surprise* | 0.159*** | 9.11 | 0.103*** | 5.72 | 0.229 | 0.50 |
| *Surprise* × *Treatment* × *Coverage1* | 0.036** | 2.83 | 0.021* | 1.71 | 0.022* | 1.78 |
| *Surprise* × *Treatment* × *Coverage2* | 0.061* | 1.89 | 0.041 | 1.30 | 0.042 | 1.31 |
| *Surprise* × *Treatment* × *Coverage3* | 0.041 | 1.54 | 0.025 | 0.90 | 0.027 | 0.96 |
| *Surprise* × *Treatment* × *Coverage4* | 0.048** | 2.85 | 0.033* | 1.73 | 0.032 | 1.67 |
| *Treatment* × *Coverage1* | 0.032 | 1.25 | 0.025 | 0.92 | 0.024 | 0.89 |
| *Treatment* × *Coverage2* | 0.010 | 0.76 | 0.008 | 0.62 | 0.007 | 0.56 |
| *Treatment* × *Coverage3* | 0.002 | 0.09 | 0.004 | 0.22 | 0.004 | 0.20 |
| *Treatment* × *Coverage4* | 0.000 | 0.01 | 0.000 | -0.03 | 0.001 | 0.12 |
| Controls? | No | | Yes | | Yes | |
| *Surprise* × Controls? | No | | No | | Yes | |
| Firm & Yr.-Qtr. FEs? | Yes | | Yes | | Yes | |
| $R^2$ | 0.0335 | | 0.0658 | | 0.0680 | |
| Observations | 8,159 | | 8,159 | | 8,159 | |

**Panel C: Intraperiod Timeliness**

| Dependent Variable = | *Intraperiod Timeliness* | | | |
|---|---|---|---|---|
| Variable | Coef. | t-stat. | Coef. | t-stat. |
| *Treatment* × *Coverage1* | 0.036** | 2.17 | 0.039** | 2.41 |
| *Treatment* × *Coverage2* | 0.006 | 0.19 | 0.007 | 0.23 |
| *Treatment* × *Coverage3* | 0.000 | 0.01 | 0.002 | 0.12 |
| *Treatment* × *Coverage4* | 0.005 | 0.22 | 0.006 | 0.24 |
| Controls? | No | | Yes | |
| Firm & Yr.-Qtr. FEs? | Yes | | Yes | |
| $R^2$ | 0.0064 | | 0.0080 | |
| Observations | 6,080 | | 6,080 | |

# Table 8. Matched Samples

This table shows estimates from the regressions in Table 5 using treatment and control firms that are matched based on proxies for the amount of recent firm news. Specifically, I match on *Surprise*, *Volatility*, and *ΔVolatility*. *Surprise* is the earnings surprise, as defined in Table 2. *Volatility* is the standard deviation of daily returns over the month preceding the earnings announcement. *ΔVolatility* is the difference between *Volatility* in the post- and pre-treatment periods, based on the earnings announcements used to assign treatment status (i.e., second quarters before and after the respective natural experiment). None of the main effects of *Treatment* × *Coverage* (or *Surprise* × *Treatment* × *Coverage* in the ERC test) are significantly different from zero, so I omit them from the tables for brevity. *t*-statistics are based on standard errors that are clustered by both firm and year-quarter using the procedure developed by [70]. Regressions include all control variables as well as firm and year-quarter fixed effects. ***, **, and * indicate significance at the 1, 5, and 10 percent two-tailed level, respectively.

## Panel A: Abnormal Volume

| Dependent Variable = | *Abnormal Volume* | | | | | |
|---|---|---|---|---|---|---|
| Matching Variable = | *Surprise* | | *Volatility* | | *ΔVolatility* | |
| Variable | Coef. | t-stat. | Coef. | t-stat. | Coef. | t-stat. |
| *Treatment* × *Coverage* × $^{R}$*Analysis* | 0.111** | 2.83 | 0.115** | 2.51 | 0.094** | 2.08 |
| Controls, Main- & Fixed-Effects? | Yes | | Yes | | Yes | |
| $R^2$ | 0.0737 | | 0.0635 | | 0.0688 | |
| Observations | 3,934 | | 3,940 | | 3,928 | |

## Panel B: Earnings Response Coefficients

| Dependent Variable = | *Abnormal Returns* | | | | | |
|---|---|---|---|---|---|---|
| Matching Variable = | *Surprise* | | *Volatility* | | *ΔVolatility* | |
| Variable | Coef. | t-stat. | Coef. | t-stat. | Coef. | t-stat. |
| *Surprise* × *Treatment* × *Coverage* × $^{R}$*Analysis* | 0.103*** | 3.04 | 0.111*** | 3.61 | 0.123*** | 4.26 |
| Controls, Main- & Fixed-Effects? | Yes | | Yes | | Yes | |
| $R^2$ | 0.1054 | | 0.1102 | | 0.1151 | |
| Observations | 3,934 | | 3,940 | | 3,928 | |

## Panel C: Intraperiod Timeliness

| Dependent Variable = | *Intraperiod Timeliness* | | | | | |
|---|---|---|---|---|---|---|
| Matching Variable = | *Surprise* | | *Volatility* | | *ΔVolatility* | |
| Variable | Coef. | t-stat. | Coef. | t-stat. | Coef. | t-stat. |
| *Treatment* × *Coverage* × $^{R}$*Analysis* | 0.056 | 1.49 | 0.069 | 1.64 | 0.068 | 1.59 |
| Controls, Main- & Fixed-Effects? | Yes | | Yes | | Yes | |
| $R^2$ | 0.0273 | | 0.0284 | | 0.0250 | |
| Observations | 2,900 | | 2,904 | | 2,890 | |

# Table 9. 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 [9] 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 7,452 of the 8,159 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. $t$-statistics are based on standard errors that are clustered by both firm and year-quarter using the procedure developed by [70]. 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* | 0.037 | 0.079 | 0.002 | 0.014 | 0.039 |
| *Abnormal Inst. Volume* | 0.681 | 0.951 | 0.148 | 0.401 | 0.869 |
| *% Retail Volume* | 4.299 | 2.333 | 2.575 | 3.830 | 5.564 |

## Panel B: Volume Regressions

| Dependent Variable = | *Abnormal Retail Volume* | | *Abnormal Inst. Volume* | | *% Retail Volume* | |
|---|---|---|---|---|---|---|
| Variable | Coef. | t-stat. | Coef. | t-stat. | Coef. | t-stat. |
| *Treatment × Coverage* | -0.034 | -1.18 | -0.027 | -1.09 | -0.012 | -0.39 |
| *Treatment × Coverage ×$^R$ Analysis* | 0.074*** | 2.93 | 0.068* | 1.89 | 0.011 | 0.65 |
| *ln(Market Cap)* | -0.056* | -1.75 | -0.045 | -1.61 | -0.023 | -1.03 |
| *ln(1+Book-To-Market)* | -0.018 | -0.92 | -0.013 | -0.50 | 0.025 | 1.58 |
| *Return Volatility* | 0.002 | 0.12 | -0.021 | -0.97 | 0.031* | 1.95 |
| *ln(1+#Employees)* | 0.010 | 0.67 | -0.027 | -1.58 | 0.015 | 1.12 |
| *ln(1+#Owners)* | 0.000 | -0.01 | 0.014 | 0.94 | -0.007 | -0.41 |
| *Institutional Ownership* | -0.025* | -2.10 | -0.025* | -2.10 | -0.012 | -0.70 |
| *ln(1+#Analysts)* | -0.019 | -0.89 | -0.016 | -0.94 | -0.016 | -0.92 |
| *ln(1+Prior Press Coverage)* | -0.028* | -1.87 | -0.021 | -1.36 | -0.026 | -1.57 |
| *ln(1+#8-K Disclosures)* | -0.037** | -2.80 | -0.025** | -2.27 | -0.024* | -1.95 |
| *Complexity* | -0.002 | -0.11 | -0.001 | -0.06 | 0.000 | 0.01 |
| *Bad News* | 0.048*** | 4.23 | 0.072*** | 4.84 | 0.016 | 1.48 |
| *ln(1+#Announcements)* | -0.057*** | -3.02 | -0.029 | -1.11 | -0.035* | -2.10 |
| Firm & Yr.-Qtr. FEs? | Yes | | Yes | | Yes | |
| $R^2$ | 0.0694 | | 0.0762 | | 0.3346 | |
| Observations | 7,452 | | 7,452 | | 7,452 | |

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