

The Roles of Data Providers and Analysts in the Production, Dissemination, and Pricing of Street Earnings

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ABSTRACT

In September 2009, Thomson Reuters (TR) discontinued its practice of relying on analysts to determine the treatment of unexpected charges and gains in favor of their immediate exclusion from GAAP earnings. Adopting a difference-in-differences approach, we show that this plausibly exogenous

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change in TR's methodology resulted in street earnings that are more predictive of future performance; and timelier, more accurate, and less dispersed analyst forecasts of future earnings, consistent with TR enhancing the properties of street earnings and analyst forecasts. Finally, using path analysis we show that a significant portion of TR's effect on price discovery is through its effect on analysts; and that the change in TR's treatment of unexpected items increased (decreased) the relative influence of TR (analysts) on the pricing of street earnings. We conclude that forecast data providers like TR are more than a conduit of information from analysts to investors.

JEL codes: D62, D83, D84, G14, G24, M40, M41

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1. Introduction

A nascent but growing literature examines the role of forecast data providers (FDPs) in capital markets and concludes that they add value by aggregating and disseminating information produced by analysts such as earnings estimates, stock recommendations, and street earnings (Akbas et al. [2018], Kaplan, Martin, and Xie [2021], Schaub [2018]). In this study, we exploit a plausibly exogenous change in how a prominent FDP, Thomson Reuters (TR), produces and distributes street earnings to examine whether TR also influences analyst information production activities and, more broadly, to deepen our understanding of how FDPs add value in capital markets.

Street earnings are GAAP earnings adjusted for non-cash or transitory items and distributed by FDPs (Black et al. [2018]).¹ Street earnings are prevalent, more predictive of future performance and more impactful than GAAP earnings (e.g., Black et al. [2018], Bradshaw et al. [2018], Doyle, Lundholm, and Soliman [2006]). With the properties of street earnings dependent on which items are excluded from GAAP earnings, there is a substantial interest in understanding forces that shape analyst exclusions. Prior literature has thoroughly documented the role of managers in influencing analyst exclusions and shaping the properties of street earnings (Black et al. [2019], Bentley et al. [2018], Christensen et al. [2011], Doyle, Jennings, and Soliman [2013]), but largely overlooked the role of FDPs for two main reasons. First, FDPs claim to report street earnings as determined by analysts. For example, TR states that its “goal is to present actuals on an operating basis, whereby a corporation’s reported earnings are adjusted to reflect the basis that the majority of contributors use to value the stock” (Thomson Reuters [2009]). Second, neither FDPs’ interactions with analysts nor FDPs’ own activities are directly observable. Stymied by lack of

¹ More specifically, street earnings in our study is the Earnings per Share (EPS) actual from the I/B/E/S database. Street earnings often differ from GAAP earnings, but not in all cases (Bentley et al. [2018]).

observational data, researchers have accepted FDPs' self-declared objective to report street earnings as determined by equity analysts as the representation of FDPs' actual role.

Drawing on institutional evidence, we suggest that reporting street earnings as determined by analysts may sometimes impose a cost on FDPs that they may not want to bear. Specifically, GAAP earnings often include unexpected gains and charges whose treatment by analysts is not known until after earnings are released. Delaying dissemination until all, or most, analysts process the earnings release to assess the nature of each of these line items and to determine street earnings increases the likelihood that investors obtain earnings information from alternative sources (analysts or media), reducing FDPs' usefulness to investors as a source of street earnings.

A 2009 change in how TR produces and distributes street earnings exemplifies TR's unwillingness to bear the cost of delayed street earnings reporting. According to TR's own policy documents, some earnings releases include unexpected charges and gains whose treatment by analysts—exclude from GAAP earnings or not—is known only after earnings are announced and processed by analysts (unexpected line items, henceforth). With the express goal of improving the timeliness of its reporting of street earnings, TR discontinued its policy of reporting street earnings after ascertaining analysts' treatment of unexpected line items in favor of reporting street earnings immediately and exclusive of unexpected line items. This plausibly exogenous change in TR's activities presents a unique opportunity to identify TR's influence on street earnings and analyst information production.²

We begin by investigating the effect of TR's methodology change on two key properties of TR-reported street earnings: timeliness and ability to predict future performance. Adopting a difference-in-differences (DiD) approach, we assign earnings announcements containing unexpected line items to the treatment group and all other announcements to the control group; and we define the pre- and post-periods as 2005–September 2009 and October 2009–2019. We find that the timeliness of street earnings dissemination in the treatment sample increases by approximately 6.1 hours relative to the control sample following the methodology change. We also find that the ability of street earnings to predict future cash flows, as captured by the slope coefficient in a typical cash flow prediction model (e.g., Doyle, Lundholm, and Soliman [2003]), is 35% lower in the treatment group prior to the methodology change, but increases approximately 21% relative to the control group as a result of the methodology change. This finding suggests that analysts did not exclude all unexpected line items

² Our conversations with TR staff reveal that the change in methodology occurred as part of TR's integration of its First Call and I/B/E/S databases, an event whose timing is likely exogenous to how analysts treat unexpected line items. See subsection 2.2 for institutional details.

from GAAP earnings, and that excluding these items, as per TR's new approach after 2009, improved the predictive ability of TR-reported street earnings. More broadly, the documented change in the predictive ability of TR-reported street earnings (induced by TR's decision to exclude all unexpected line items from GAAP earnings) suggests that FDPs do more than just aggregate and disseminate street earnings produced by analysts.

There are reasons to expect that the change in TR's treatment of unexpected line items may influence analyst decision-making. Specifically, analysts' exclusion decisions are informed by their imperfect understanding of the company's business and the nature of the unexpected line items (operating or nonoperating, permanent or transitory). Given this ambiguity, analysts may wish to conform or "herd" their treatment of these items toward TR's definition (e.g., Trueman [1994], Welch [2000]).³ Prior to the methodology change, analysts wishing to conform with TR's treatment of unexpected line items faced difficulties as TR waited to ascertain analysts' treatment of these items. Following the methodology change, TR's more timely dissemination and clear decision rule—exclude all unexpected line items—may facilitate wider conformity among analysts because of its salience, simplicity, and increased predictive ability of resulting street earnings.

We infer whether analysts' exclusion decisions are similar to TR's from the difference between TR-reported street earnings for quarter one and analysts' implied actuals (Brown and Larocque [2013]). Using the same DiD approach, we find that analysts' implied actuals become more similar to TR-reported street earnings, consistent with TR influencing analyst exclusion decisions.

We also find robust evidence that TR's methodology change enhances the informational properties of analyst forecasts of next quarter street earnings. Specifically, as a percentage of the sample mean, analyst forecasts become 5.94% less dispersed and 9.45% more accurate in the treatment group relative to the control group. These findings are consistent with analysts adopting TR's uniform treatment of unexpected line items and/or reallocating resources from street earnings measurement to forecasting future earnings.

In additional analyses, we examine whether the methodology change affected some analysts more than others. Prior to the methodology change, many analysts published their reports prior to observing TR's street earnings, as TR waited to observe analysts' treatment of unexpected line items. Consistent with the methodology change giving these analysts an opportunity to incorporate TR's street earnings information, we find that the effects of the methodology change are concentrated among analysts who tended to publish reports faster than TR in the pre-period.

³ More broadly, analysts may either incorporate TR's street earnings as their own actual, or reference TR's street earnings as a benchmark. Figure A1 provides anecdotal evidence that some analysts include TR-reported street earnings information in their reports.

Our findings so far suggest that FDPs' role in capital markets is broader than previously thought. They raise a strong possibility that FDPs make capital markets more efficient not only directly—by disseminating information to investors—but also indirectly—by enhancing the informational properties of analyst reports. We investigate the direct and indirect effects of FDPs on price discovery by modeling the timeliness of the market reaction to earnings as a function of the methodology change and the delay with which analysts revise their forecasts of the next quarter earnings, with the methodology change causally affecting analyst delay.⁴ We find significant direct effects of the methodology change on analyst forecasting delay and market timeliness, consistent with more timely dissemination of street earnings by TR improving analyst and investor timeliness. Importantly, we find a significant indirect effect of the methodology change on investors, mediated through analysts. We estimate that around 27.7% of the total effect of the 2009 methodology change on market timeliness is mediated by its effect on analysts' timeliness.

In our final analysis, we model market reaction timeliness as a function of TR delay and analyst delay, with TR delay causally affecting analyst delay and the methodology change moderating all these effects. This analysis allows us to quantify the relative roles of TR and analysts and examine how these roles change following the methodology change, subject to the caveat that the delay with which TR determines and disseminates street earnings is not exogenous to the delay with which the market determines and prices street earnings. These association tests are nevertheless informative. We find that the relative importance of TR's (analysts') delay in determining the timeliness of market response to earnings becomes much stronger (weaker) following the methodology change. Specifically, we find that the impact of TR (analysts) on market reaction timeliness is around 17% stronger (9% weaker) after the methodology change, relative to a one standard deviation change in market reaction timeliness. Collectively, these results demonstrate how changes in TR's street earnings processing introduced by the methodology change shifted some portion of analysts' street earnings dissemination role to TR.

Our study contributes to prior literature on the role of information intermediaries in capital markets. Specifically, prior studies investigate FDPs (Akbas et al. [2018], Kaplan, Martin, and Xie [2021], Schaub [2018]) and analysts (Yezegel [2015], Zhang [2008]) separately, assuming that FDPs' role is limited to the aggregation and dissemination of analyst-produced information. We use a plausibly exogenous change in how a prominent FDP collects and distributes information to show that FDPs influence analyst information production. Consequently, FDPs influence price discovery not

⁴We measure market reaction timeliness using an intraday area-under-the-curve metric that mirrors the five-day area-under-the-curve timeliness measure used in prior literature (e.g., Blankespoor, deHaan, and Zhu [2018] and Twedt [2015]).

only directly, by disseminating street earnings to investors, but also indirectly, through their effects on analysts. By accounting for analysts' reliance on FDPs, we paint a more complete picture of how these intermediaries add value in capital markets.

Our results that FDPs influence the measurement of street earnings are consistent with results in Kaplan, Martin, and Xie [2021] that FDPs influence the measurement of the analyst consensus but differ in two major respects. They speak to the information intermediary role of FDPs in a unique and economically important setting, earnings releases, and the ability of FDPs to influence analyst forecasting behavior.

We also contribute to the street earnings literature, most recently surveyed by Black et al. [2018]. Specifically, we are the first to document the role of FDPs in shaping the properties and consequences of street earnings in capital markets, complementing prior work that exclusively focuses on the role of corporate managers or analysts (e.g., Christensen et al. [2011], Bentley et al. [2018], Gu and Chen [2004], Baik, Farber, and Petroni [2009]). The role of FDPs has been conjectured by many (Bradshaw and Soliman [2007] and Lambert [2004]), but remained unexamined due to the difficulty of identifying exogenous variation in FDPs' activities.

2. Background

2.1 MOTIVATION

"Street earnings" refer to earnings actuals disseminated by FDPs (Black et al. [2018]) and are often measured on a non-GAAP basis (Bentley et al. [2018]). GAAP line items that are excluded from street earnings are called "exclusions," and generally represent items deemed transitory or non-cash, and thus less relevant for the firm's future performance. Consistent with this notion, a large body of research finds that street earnings are more value relevant to investors than GAAP earnings (Bradshaw et al. [2018]) and are more informative about future firm operating performance (Black et al. [2018]). However, street earnings can vary in "quality," where quality refers to the ability of current street earnings to predict future street earnings.

Prior studies typically focus on the role of managers and analysts in determining the quality of street earnings exclusions. For instance, Gu and Chen [2004] and Baik, Farber, and Petroni [2009] find that analysts' ability and incentives, respectively, determine street earnings exclusions. Managers also influence street earnings exclusions through their earnings guidance (Christensen et al. [2011]), conference call presentations (Black et al. [2019]), and pro forma reporting (Bentley et al. [2018]).

In this regard, prior literature has largely overlooked the role of FDPs in determining the quality of street earnings. Although the timeliness of street earnings dissemination by FDPs is associated with a more efficient market response to earnings announcements (Schaub [2018]), there is

no evidence documenting FDPs' impact on street earnings content. That is, FDPs are viewed as a mere conduit of information from analysts to investors. Several researchers have, however, conjectured a broader role of FDPs. Lambert [2004, p. 211] discusses that "a case can be made that it's the forecast database service (First Call) that makes the exclusion decision." Bradshaw and Soliman [2007, p. 736] argue that "there is no compelling evidence in the literature that identifies street earnings as a predominantly analyst-driven, manager-driven, or FDP-driven phenomenon."

Kaplan, Martin, and Xie [2021] provide initial evidence of a broader role for FDPs in shaping the properties of the information they produce. They show that TR is more likely to exclude optimistic forecasts from the I/B/E/S consensus, and that these discretionary exclusion decisions result in a more accurate *ex ante* consensus forecast. In this paper, we study FDPs' discretion in determining street earnings exclusions and explore its impact on the properties of street earnings and street earnings processing by other intermediaries. Specifically, we examine whether a change in TR's method for determining street earnings exclusions influences the quality of street earnings, analysts' information production, and the subsequent pricing of street earnings information. To the extent that TR's methodology influences analysts' behavior, it can also have indirect effects on the investors who rely on those analysts. Therefore, while prior literature has focused exclusively on the direct effects of FDPs' information dissemination on investors (Akbas et al. [2018], Schaub [2018]), we examine whether FDPs play a broader role in capital markets through their measurement of street earnings and their effect on other intermediaries such as financial analysts.

2.2 THE 2009 METHODOLOGY CHANGE

In this paper, we exploit a plausibly exogenous change in how a prominent data provider, TR, collects and disseminates street earnings when an earnings press release reports unexpected line items.⁵ Unexpected line items are charges or gains that were not anticipated by analysts prior to the earnings announcement.⁶ Absent unexpected line items, TR collects street earnings in accordance with the exclusion decisions made by the majority of contributing analysts in their forecasts of quarter t earnings, which we refer to as the "ex ante consensus forecast" for quarter t . However, if unexpected line items are present, it is impossible to infer analysts' treatment

⁵ We focus on TR for several reasons. First, TR's 2009 methodology change serves as a natural experiment for exploring the role of FDPs in the production and dissemination of street earnings. Second, TR is a leading financial data provider in today's capital markets. According to a 2018 Ipreo special report, there are four major consensus data providers, namely, TR, FactSet, Bloomberg, and S&P's Capital IQ, and each is equally likely to be cited as the headline number on news feeds (Ipreo [2018]). Finally, I/B/E/S is the most commonly used source of street earnings and analyst forecast data in prior literature (Call et al. [2021]).

⁶ TR provides several examples of line items that can sometimes be "unexpected" prior to the earnings announcement, including nonrecurring income taxes, settlement of litigation or insurance, and asset write-downs (Thomson Reuters [2009]).

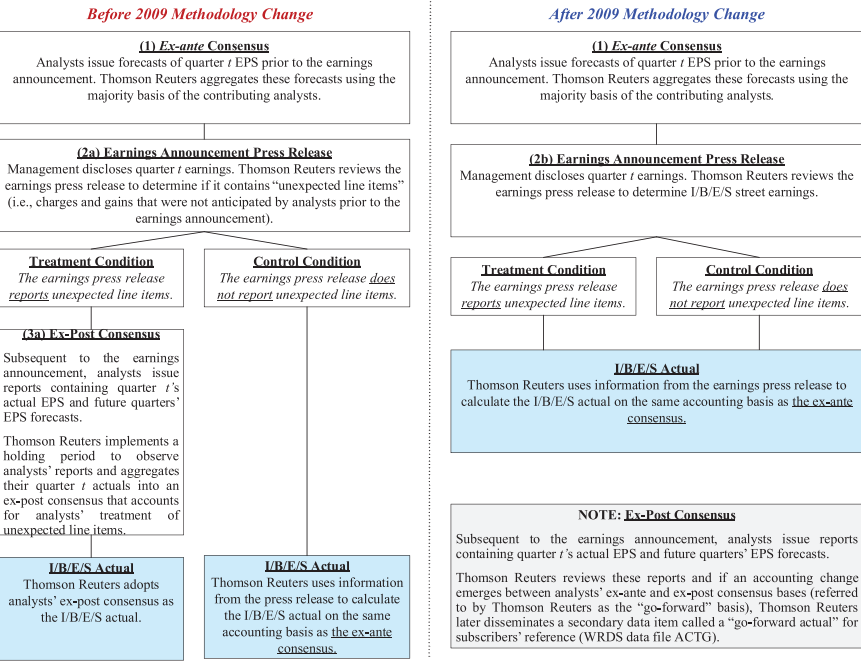


FIG 1.—2009 Thomson Reuters' methodology change. This figure illustrates TR's approach to street earnings processing before and after the 2009 methodology change.

of these items from their quarter t forecasts, as, by definition, they are unexpected by analysts.⁷ Therefore, prior to the 2009 methodology change, TR waited until it could observe analysts' ex post treatment of the unexpected line items in their subsequent reports.⁸ Once a sufficient number of analysts issue reports following the earnings announcement, TR aggregates analysts' quarter t actuals to form an ex post consensus actual that reflects analysts' treatment of unexpected line items. We refer to this as analysts' "ex post consensus actual" for quarter t .

Figure 1 illustrates TR's approach to street earnings processing before and after the 2009 methodology change. Prior to 2009, when an earnings

⁷ It is important to note that the term "unexpected line items" does not refer to the concept of "unexpected exclusions" in Bratten, Larocque, and Yohn [2021] and Bradshaw et al. [2018]. Unexpected exclusions include the mis-estimated portion of line items that were expected (but excluded) by analysts prior to the earnings announcement, as well as any portion of unexpected line items announced at the earnings announcement that are subsequently excluded from street earnings.

⁸ Analysts typically issue research reports immediately after earnings announcements ([e.g. Ivković and Jegadeesh 2004]). These reports generally provide the analyst's own calculation of "actual" quarter t earnings reported by the company, as well as potential updates to the analyst's future $(t + n)$ earnings forecasts and investment recommendations (see examples in figure A1).

press release reported unexpected line items (“treatment condition”), TR adopted analysts’ ex post *consensus actual* as the I/B/E/S street earnings actual. If no unexpected line items were present (“control condition”), TR specialists determined the I/B/E/S street earnings actual from the press release, in accordance with analysts’ ex ante *consensus forecast*. After the 2009 methodology change, TR specialists strictly calculate *all* I/B/E/S street earnings actuals in accordance with analysts’ ex ante *consensus forecast*, regardless of the presence of unexpected line items in the earnings press release.⁹

To illustrate the differences in TR’s street earnings processing before and after the 2009 methodology change, figure A2 provides an example of an earnings announcement containing unexpected line items for Fifth-Third Bank (FITB).¹⁰ The ex ante consensus forecast of FITB’s Q3 Earnings per Share (EPS) was \$0.40. In its Q3 earnings press release, FITB reported GAAP EPS of \$0.65, which included unexpected gains of \$0.22. According to TR, following the earnings announcement, “most of the brokers adopted \$0.65 actual in their model and some raised their full year estimates without changing [their Q4] EPS estimate” (Thomson Reuters [2017]). If this announcement had occurred prior to the 2009 methodology change, TR would have “held-out” the announcement until it observed analysts’ ex post consensus actual, and then recorded the street earnings actual as \$0.65. In reality, this announcement occurred after the 2009 methodology change (in 2016) and the street earnings actual was recorded at \$0.43 to reflect an earnings surprise of \$0.03 on the same measurement basis as the ex ante consensus forecast.

In the next sections, we examine the impact of the 2009 methodology change on the properties of street earnings, analyst information production, and investor response to earnings announcements.¹¹ Specifically, our analysis proceeds in three related steps. First, we examine the impact of the methodology change on the timeliness and predictive ability of street earnings. Second, we examine how TR’s methodology change impacted analysts’ reliance on TR’s street earnings and whether the methodology change impacted properties of analyst forecasts such as accuracy and dispersion. Finally, we examine the impact of the methodology change on the timeliness of investor response to earnings announcements, recognizing

⁹ Although the “hold-out” period was eliminated after September 30, 2009, TR continues to monitor analysts’ ex post consensus actuals for changes in analysts’ majority accounting basis. If an accounting basis change is observed, TR disseminates a secondary “go-forward” actual for that announcement (Thomson Reuters [2017]).

¹⁰ We cannot directly observe the presence or amounts of unexpected line items in the I/B/E/S academic database. However, TR provides the amount of unexpected line items for this example in a white paper explaining the difference between street earnings actuals and “go-forward” actuals (see Thomson Reuters [2017]).

¹¹ It is possible that changes in management disclosures, induced by the 2009 methodology change or independent of it, may have influenced the properties of street earnings and analyst forecasts. We address this possibility in subsection 7.3.

TABLE 1
Sample Selection

| Description | Observations |
|--|--------------|
| I/B/E/S firm-quarter actuals from 2005–2019 with valid activation delay, timestamp, and stock price data | 200,916 |
| Less: observations missing required WRDS data for equation (1) variables | (30,976) |
| Less: observations missing press release data from EDGAR | (26,760) |
| Less: fixed-effects singleton observations | (265) |
| <i>Full Sample</i> | 142,915 |
| Full Sample | 142,915 |
| Less: observations missing future earnings or cash flow data | (9,098) |
| <i>Table 4 Sample</i> | 133,817 |
| Full Sample | 142,915 |
| Less: observations missing forecast dispersion data | (47,279) |
| <i>Table 5 Sample</i> | 95,636 |

This table describes the initial sample selection procedure used to collect the data analyzed in our study. Data requirements specific to individual tables or analyses are provided in the descriptions of those analyses.

the possibility of FDPs’ indirect effect on investors through analysts. We view the methodology change as a unique setting to isolate the role of FDPs in the processing of street earnings by investors and by other intermediaries.

3. *Data and Sample Selection*

Before describing our empirical research design, we provide a brief overview of the data used in our analyses. Table 1 describes our sample selection procedure. To conduct our analyses, we collect the text of quarterly earnings press releases from 8-K filings. Therefore, our sample period begins in 2005, the first full year following the 2003–2004 SEC regulations expanding the scope of 8-K filing requirements to include all quarterly earnings press releases (McMullin, Miller, and Twedt [2019]). The sample period ends in 2019, the most recent year of data availability at the time of data collection.

We begin by collecting all quarterly street earnings actuals in I/B/E/S for firms whose stock is listed on NYSE, NYSE MKT, NYSE ARCA, or NASDAQ with a price greater than \$1 as of two trading days before the earnings announcement. Our analyses also require precise earnings announcement timestamps. Therefore, we obtain earnings announcement timestamps from multiple sources, and choose the earliest available timestamp, following procedures similar to those used by deHaan, Shevlin, and

Thornock [2015] and Johnson and So [2018].^{12, 13} This yields an initial sample of 200,916 quarterly earnings announcements.

We merge this initial sample with additional WRDS databases to compute the control variables defined in table A1. We then match each remaining observation to the text of the earnings press release from Form 8-K collected from the SEC EDGAR Web site. Finally, we exclude 265 observations that are “singletons” in any of the fixed effects included in our regressions (i.e., firm, year-quarter, or announcement hour fixed effects). These steps generate our main sample of 142,915 quarterly earnings announcements made by 5,758 unique firms between January 2005 and December 2019. We winsorize all continuous variables at the top and bottom 1% levels. When examining intraday market reaction timeliness, we winsorize all intraday timeliness variables to one full trading day, including extended hours trading (i.e., 960 trading minutes).

Some of our analyses have additional data requirements. Our tests examining the association between current street earnings and future street earnings and cash flows require data for these future outcomes over the next four quarters, reducing our sample to 133,817 observations for the analyses tabulated in table 4. Similarly, we require a minimum of three forecasts when calculating analysts’ forecast dispersion, which reduces the sample analyzed in table 5 to 95,636 observations.

Table 2 reports summary statistics for our variables of interest and controls over the full sample period from January 2005 to December 2019. The mean (median) time it takes TR to activate street earnings in the I/B/E/S database (*Activation Delay*) is 543 (46) minutes. Around 44.8% of the sample observations are in the treatment group (i.e., have at least one potential unexpected line item in the press release). Approximately 16.4% of sample announcements are by firms in the S&P 500 index, 32.8% of announcements report bad news, and around 5.6% of announcements are made on *Friday*. Approximately 25% of announcements in our sample disclose earnings for the fourth fiscal quarter (*QTR4*). However, the incidence of *Treatment* observations is higher for *QTR4* announcements (54%, untabulated), consistent with these announcements being affected by year-end

¹² Specifically, for announcements with timestamp data available from both Wall Street Horizon (WSH) and Ravenpack, we use the earlier of these two timestamps. When one or both of these data sources are unavailable, we use the earliest available timestamp from WSH, Ravenpack, or I/B/E/S. This results in approximately 56.79%, 20.75%, and 22.46% of timestamps sourced from WSH, Ravenpack, and I/B/E/S, respectively. Consistent with Bradley et al. [2014], we observe that the I/B/E/S announcement timestamps are delayed relative to the earliest available timestamp for approximately 36.29% of announcements with WSH and/or Ravenpack data available.

¹³ We eliminate observations with *Reporting Lag* greater than 120 days, zero or negative *Activation Delay*, or *Activation Delay* greater than 10 days, as these are likely due to irregular or erroneous timestamp data.

TABLE 2
Descriptive Statistics

| | N | Mean | Median | SD | Q1 | Q3 |
|-------------------------------------|---------|---------|---------|-----------|---------|---------|
| Dependent and independent variables | | | | | | |
| <i>Activation Delay</i> | 142,915 | 543.363 | 46.000 | 1,665.260 | 18.000 | 146.000 |
| <i>Post</i> | 142,915 | 0.672 | 1.000 | 0.470 | 0.000 | 1.000 |
| <i>Treatment</i> | 142,915 | 0.448 | 0.000 | 0.497 | 0.000 | 1.000 |
| <i>Future Street Earnings</i> | 133,817 | 0.013 | 0.038 | 0.156 | 0.007 | 0.079 |
| <i>Future Cash Flows</i> | 133,817 | 0.057 | 0.074 | 0.147 | 0.018 | 0.127 |
| <i>Street Earnings</i> | 133,817 | 0.004 | 0.010 | 0.040 | 0.002 | 0.021 |
| <i>DIFF</i> | 22,086 | 0.724 | 1.000 | 0.447 | 0.000 | 1.000 |
| <i>Forecasting Delay</i> | 95,636 | 437.526 | 339.000 | 278.389 | 235.000 | 617.000 |
| <i>MAFE</i> | 95,636 | 0.211 | 0.114 | 0.280 | 0.053 | 0.240 |
| <i>Dispersion</i> | 95,636 | 0.101 | 0.054 | 0.135 | 0.027 | 0.115 |
| <i>MRT</i> | 142,915 | 1.577 | 1.964 | 1.910 | 1.014 | 3.182 |
| Control variables | | | | | | |
| <i>Abs(GAAP-Street)</i> | 142,915 | 0.006 | 0.000 | 0.017 | 0.000 | 0.005 |
| <i>Abs(Surprise)</i> | 142,915 | 0.516 | 0.136 | 1.259 | 0.046 | 0.385 |
| <i>Bad News</i> | 142,915 | 0.328 | 0.000 | 0.470 | 0.000 | 1.000 |
| <i>EPS Guidance</i> | 142,915 | 0.191 | 0.000 | 0.393 | 0.000 | 0.000 |
| <i>QTR4</i> | 142,915 | 0.247 | 0.000 | 0.431 | 0.000 | 0.000 |
| <i>Reporting Lag</i> | 142,915 | 3.456 | 3.466 | 0.336 | 3.219 | 3.638 |
| <i>Size</i> | 142,915 | 13.867 | 13.751 | 1.777 | 12.579 | 15.016 |
| <i>Firm Age</i> | 142,915 | 2.639 | 2.750 | 0.939 | 2.052 | 3.294 |
| <i>Institutional Ownership</i> | 142,915 | 0.675 | 0.747 | 0.271 | 0.503 | 0.893 |
| <i>Analyst Following</i> | 142,915 | 1.910 | 2.079 | 1.063 | 1.386 | 2.708 |
| <i>S&P 500</i> | 142,915 | 0.164 | 0.000 | 0.371 | 0.000 | 0.000 |
| <i>Advertising</i> | 142,915 | 1.157 | 0.000 | 1.798 | 0.000 | 1.841 |
| <i>Unactivated Actuals</i> | 142,915 | 3.681 | 3.807 | 1.193 | 2.944 | 4.564 |
| <i>Friday</i> | 142,915 | 0.056 | 0.000 | 0.230 | 0.000 | 0.000 |
| <i>Press Release Words</i> | 142,915 | 8.041 | 7.996 | 0.512 | 7.694 | 8.338 |
| <i>HardInfoMix</i> | 142,915 | 0.050 | 0.044 | 0.028 | 0.032 | 0.060 |
| <i>Non-GAAP Words</i> | 142,915 | 0.011 | 0.009 | 0.011 | 0.002 | 0.018 |
| <i>Media Coverage</i> | 142,915 | 3.392 | 3.434 | 1.294 | 2.565 | 4.205 |

This table presents summary statistics for the main dependent and independent variables as well as the control variables used in our analyses. The main sample includes 142,915 quarterly earnings announcements made between January 2005 and December 2019. Variable definitions are reported in table A1.

adjustments and write-downs. Accordingly, we include *QTR4* as a control variable in all of our analyses.¹⁴

4. Effects of the 2009 Methodology Change on Properties of I/B/E/S Street Earnings

4.1 TIMELINESS OF STREET EARNINGS

TR’s elimination of the “hold-out” period for announcements with unexpected line items suggests that, before the 2009 methodology change, street

¹⁴In untabulated tests, we repeat all of our analyses on four subsamples partitioned by the fiscal quarter and find consistent results.

earnings activation should be incrementally delayed for these announcements relative to announcements that do not report unexpected line items. After the 2009 methodology change, this incremental delay should be reduced or eliminated, allowing us to employ a DiD research design to study the implications of TR information processing for analysts and investors. Conceptually, in this design, announcements reporting unexpected line items represent a natural “treatment” group relative to the “control” group of firm-quarter announcements that do not report unexpected line items.

The I/B/E/S database does not explicitly identify which announcements contain line items deemed “unexpected” by TR specialists. Therefore, we use Compustat data to define *Treatment* firm-quarters as those that contain any of the following eight items that appear in a list of common unexpected items discussed in the TR methodology guide (see Thomson Reuters [2009]): a large restructuring charge, a large acquisition expense or gain, net credit or charge to reserves for bad debts from loan recoveries or charge-offs, nonrecurring income taxes, settlement of litigation or insurance, asset write-down, goodwill impairment, and large special items.

We begin our empirical analyses by examining the effects of the 2009 methodology change on street earnings activation delay for two reasons. First, although there is little doubt as to whether the 2009 methodology change affected street earnings activation delay, documenting these effects serves as an opportunity to describe and validate our research design. Second, quantifying the effects of the 2009 methodology change on street earnings activation delay extends prior research on the capital market effects of FDPs’ methodology decisions (Kaplan, Martin, and Xie [2021]).

In figure 2(a), we plot the time-series averages of TR’s street earnings *Activation Delay* for *Treatment* and control observations during our sample period. Prior to the methodology change, both groups exhibit similar trends in *Activation Delay*, consistent with the parallel trends assumption, but the mean activation delay is always much higher for *Treatment* observations relative to the control group. In September 2009, we observe a significant drop in *Activation Delay*, from around 2,800 minutes to less than 200 minutes, for *Treatment* announcements, while the drop in *Activation Delay* for control announcements is roughly half as large. Observing a decrease in the activation delay for control firm-quarters suggests that our assignment of observations into treatment and control groups is imperfect. However, this imperfect assignment only biases our empirical tests against finding predicted results (i.e., the magnitude of the treatment effects we are able to identify empirically are likely smaller than the true treatment effects). To further validate our treatment and control groups, in figure 2(b), we plot the time-series averages of *Activation Delay* for observations that are randomly assigned into treatment and control groups. We find no discernible differences in *Activation Delay* for randomly assigned groups, suggesting that our initial classification based on unexpected charges/gains is meaningful.

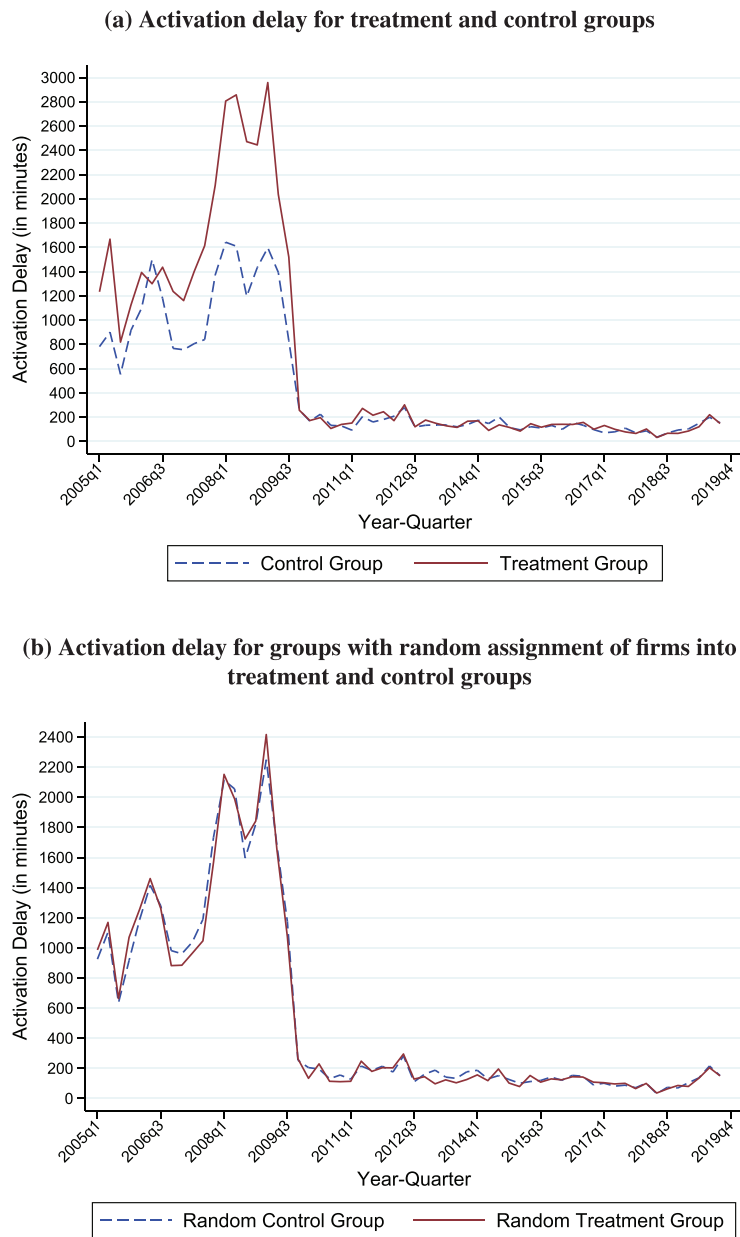


FIG 2.—Activation delay in the pre- and post-periods of Thomson Reuters’ methodology change. This figure plots the average street earnings activation delay (*Activation Delay*) for each quarter over the period January 2005–December 2019 for: (a) *Treatment* observations and *Control* observations, and (b) observations that are randomly assigned to treatment and control groups. *Activation Delay* is the time (in minutes) from the earnings press release time until TR’s street earnings activation time. *Treatment* observations are those that likely report unexpected items, as defined in table A1.

Next, to quantify the effect of the 2009 methodology change on street earnings activation delay, we estimate the following DiD model:

$$\begin{aligned} \ln(\text{Activation Delay}) = & \beta_1 \text{Treatment} + \beta_2 \text{Post} \times \text{Treatment} \\ & + A \times \text{Controls} + B \times \text{FirmFE} + C \\ & \times \text{YearQtrFE} + D \times \text{AnnHourFE} + \epsilon, \end{aligned} \quad (1)$$

where $\ln(\text{Activation Delay})$ is the natural logarithm of the time (in minutes) from the press release time until TR's street earnings activation time, *Post* equals 1 for observations after September 30, 2009, and 0 otherwise, *Treatment* equals 1 for observations that likely report unexpected line items, and 0 otherwise, *Controls* is a vector of variables listed as "Control variables" in table A1, and *FirmFE*, *YearQtrFE*, and *AnnHourFE* are firm, year-quarter, and earnings announcement hour fixed effects, respectively.^{15, 16} Throughout our analyses, we evaluate the statistical significance of our regression estimates based on standard errors clustered by firm and announcement date (Hirshleifer, Lim, and Teoh [2009], Petersen [2009]).

Column 1 of table 3 presents the results of estimating equation (1) without *Controls* and column 2 provides results of estimating the full model. Focusing on column 2, we find that while *Activation Delay* for *Treatment* observations is longer on average prior to the methodology change (coef. = 0.516, *t*-stat. = 22.28), this difference is significantly reduced in the *Post* period, as evidenced by a significantly negative DiD coefficient for *Post* × *Treatment* (coef. = −0.496, *t*-stat. = −19.35). Relative to the sample mean activation delay of 543 minutes, the marginal effect of the *Treatment* coefficient suggests that, *ceteris paribus*, the average treatment announcement is delayed by approximately 6.1 hours prior to the methodology change. The sum of the *Treatment* and *Post* × *Treatment* coefficients suggests that this incremental delay is eliminated following the methodology change.

Figure 3 plots the estimated marginal effects from estimating Equation (1) after replacing *Post* × *Treatment* with interactions between *Treatment* and quarterly event-time indicator variables. Consistent with the results tabulated in table 3 and figure 2, the estimated marginal effects for treatment and control observations follow parallel trends prior to the 2009 methodology change, and significant differences in the estimated activation delay between treatment and control observations become insignificant after the 2009 methodology change.

Collectively, these results demonstrate that our assignment of observations into treatment and control groups captures TR's methodology change

¹⁵Year-quarter fixed effects are indicator variables for the calendar year-quarter in which the earnings press release was announced. These fixed effects control for time-trends over the sample period unrelated to the 2009 methodology change.

¹⁶We control for the time of day of the earnings announcement in all of our tests because earnings announcements are clustered in the pre-market open and post-close hours, and street earnings activation delay, as well as many of our intraday outcome variables, also vary with the time of day.

TABLE 3
Effects of Thomson Reuters' Methodology Change on Street Earnings Activation Delay

| | (1) | (2) |
|-----------------------------------|-----------------------|-----------------------|
| <i>Treatment</i> | 0.582*** (24.34) | 0.516*** (22.28) |
| <i>Post</i> × <i>Treatment</i> | −0.508*** (−19.30) | −0.496*** (−19.35) |
| Control variables | | |
| <i>Abs</i> (GAAP-Street) | | 3.150*** (10.70) |
| <i>Abs</i> (Surprise) | | 0.058*** (14.63) |
| <i>Bad News</i> | | 0.060*** (7.19) |
| <i>EPS Guidance</i> | | −0.045** (−2.47) |
| <i>QTR4</i> | | 0.106*** (6.04) |
| <i>Reporting Lag</i> | | 0.074** (2.43) |
| <i>Size</i> | | −0.026** (−2.32) |
| <i>Firm Age</i> | | −0.119*** (−3.91) |
| <i>Institutional Ownership</i> | | 0.136*** (3.54) |
| <i>Analyst Following</i> | | −0.031*** (−2.83) |
| <i>S&P 500</i> | | −0.082* (−1.90) |
| <i>Advertising</i> | | 0.001 (0.10) |
| <i>Unactivated Actuals</i> | | 0.343*** (34.52) |
| <i>Friday</i> | | −0.053** (−2.03) |
| <i>Press Release Words</i> | | 0.133*** (6.78) |
| <i>HardInfoMix</i> | | −0.041 (−0.14) |
| <i>Non-GAAP Words</i> | | −0.594 (−0.72) |
| <i>Media Coverage</i> | | 0.005 (0.78) |
| <i>Firm, Year-Qtr, AnnHour FE</i> | Yes | Yes |
| Observations | 142,915 | 142,915 |
| Adjusted <i>R</i> ² | 0.499 | 0.519 |

This table reports the estimated coefficients from a regression of $\ln(\text{Activation Delay})$ on indicators identifying treatment and control observations before and after the 2009 TR methodology change, as defined in Equation (1). $\ln(\text{Activation Delay})$ is the natural logarithm of the time (in minutes) from the press release time until TR's street earnings activation time. *Post* is an indicator that equals 1 for earnings press releases announced after September 30, 2009 and 0 otherwise. *Treatment* is an indicator variable for observations that report unexpected items as defined in table A1. "Control variables" are as defined in table A1, and *Firm*, *Year-Qtr* and *AnnHour FE* are firm, year-quarter, and earnings announcement hour fixed effects, respectively. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively, using the two-tailed *t*-test. Standard errors are based on a two-way clustering at both firm level and earnings announcement date level.

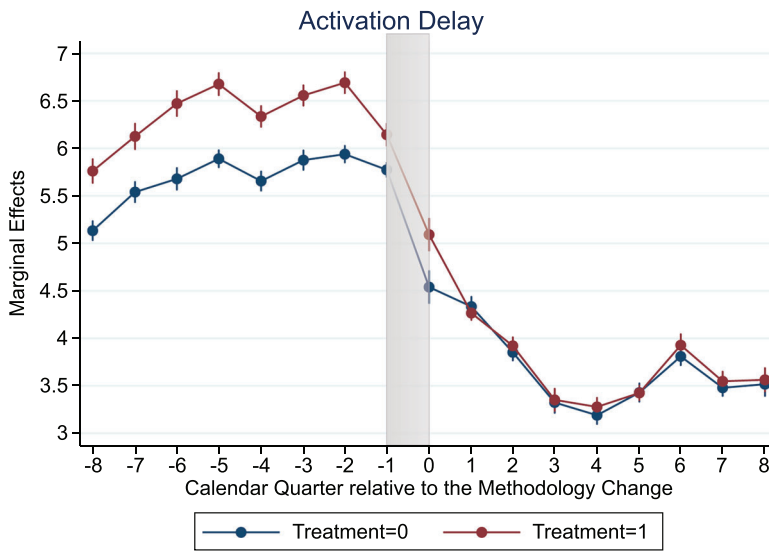


FIG 3.—Effects of Thomson Reuters’ methodology change on street earnings activation delay. This figure plots the estimated marginal effects from estimating Equation (1) after replacing $Post \times Treatment$ with interactions between $Treatment$ and each quarter event-time indicator variable (i.e., each $Year-Qtr$ FE from Equation (1)). The figure plots the estimated marginal effects from quarter $t - 8$ to quarter $t + 8$ relative to the 2009 methodology change. Quarter $t = 0$ is the quarter ending on September 30, 2009. The red (blue) line plots the estimated value of $Ln(Activation\ Delay)$ when $Treatment = 1$ ($Treatment = 0$). The error bars around each point provide the 95% confidence intervals for the estimated marginal effects. Quarterly indicators for each of the 60 quarters included in the sample period, with the exception of 2005Q1 (reference level) are included in the estimation. All other control variables and fixed effects are as specified in Equation (1). Variable definitions are reported in table A1.

reasonably well. Furthermore, these results extend prior research by Kaplan, Martin, and Xie [2021] by showing that FDPs’ discretionary methodology decisions affect their timeliness. Our results also demonstrate that TR’s methodology decisions have an economically meaningful effect on the activation delay that is incremental to the proxies for information complexity, investor demand, and limited attention examined by Akbas et al. [2018].

4.2 THE PREDICTIVE ABILITY OF STREET EARNINGS

As discussed in subsection 2.2, the 2009 methodology change affected both the timeliness and measurement basis of I/B/E/S street earnings for announcements containing unexpected line items. The measurement basis of street earnings changed from analysts’ ex post consensus actual, which might include or exclude the unexpected line items, to strictly excluding all unexpected line items following the methodology change, to stay consistent with analysts’ ex ante consensus forecast (Thomson Reuters [2017]).

The effect of the 2009 methodology change on the predictive ability of street earnings is an empirical question. The answer to this question

depends on (1) whether unexpected line items are persistent or transitory and (2) analysts' judgment as to which unexpected line items to include versus exclude in their ex post actuals. Street earnings have the greatest predictive ability for future operating performance when recurring line items are included and transitory line items are excluded. If analysts consistently include recurring line items and exclude transitory line items from their ex post actuals, the 2009 methodology change likely reduced the predictive ability of street earnings. However, Black et al. [2018, p. 270] note that "uninformative incentives can also influence analysts' exclusions." Therefore, to the extent that analysts include transitory unexpected line items in their ex post actuals, the methodology change may instead improve the predictive ability of street earnings by uniformly excluding these line items.¹⁷

In this section, we examine the impact of TR's 2009 methodology change on the ability of street earnings to predict future earnings and cash flows. The predictive ability of street earnings is an important area of study in prior literature. For example, a number of studies compare the predictive ability of street earnings with that of managers' pro forma reporting (e.g., Barth, Gow, and Taylor [2012] and Bentley et al. [2018]). Although prior literature finds that street earnings have greater predictive ability than pro forma earnings, the role of FDPs versus analysts in determining the predictive ability of street earnings remains unexamined.

We test how changes in the measurement basis of I/B/E/S street earnings due to the 2009 methodology change affected the predictive ability of those street earnings by estimating the following model:

$$\begin{aligned}
 \text{Future Outcome} = & \beta_1 \text{Treatment} + \beta_2 \text{Post} \times \text{Treatment} \\
 & + \beta_3 \text{Street Earnings} \\
 & + \beta_4 \text{Street Earnings} \times \text{Post} \\
 & + \beta_5 \text{Street Earnings} \times \text{Treatment} \\
 & + \beta_6 \text{Street Earnings} \times \text{Post} \times \text{Treatment} \\
 & + A \times \text{Controls} \\
 & + B \times \text{FirmFE} + C \times \text{YearQtrFE} \\
 & + D \times \text{AnnHourFE} + \epsilon,
 \end{aligned} \tag{2}$$

where *Future Outcome* is either *Future Street Earnings*, defined as the sum of I/B/E/S actual EPS for quarters $t+1$ to $t+4$, scaled by total assets per share, or *Future Cash Flows*, defined as the sum of cash flows from operations for quarters $t+1$ to $t+4$, scaled by total assets. *Street Earnings* is defined as the

¹⁷Returning to the FITB example in figure A3, a majority of analysts included the \$0.22 per share of unexpected gains in their ex post actuals. Before the 2009 methodology change, TR would have adopted analysts' treatment of these unexpected line items and included the \$0.22 in street earnings. However, this announcement occurred in 2016, after the methodology change, so they were excluded. If the \$0.22 of gains are persistent (transitory) in nature, then FITB's street earnings should be more useful in predicting future street earnings and cash flows before (after) the 2009 methodology change.

TABLE 4
Effects of Thomson Reuters' Methodology Change on the Predictive Value of Street Earnings

| | (1) Future Street Earnings | (2) Future Cash Flows |
|---|-------------------------------|--------------------------|
| <i>Treatment</i> | 0.003** (2.30) | 0.001 (0.98) |
| <i>Post</i> × <i>Treatment</i> | −0.004** (−2.26) | −0.003** (−2.21) |
| <i>Street Earnings</i> | 1.566*** (21.12) | 1.185*** (16.32) |
| <i>Street Earnings</i> × <i>Post</i> | −0.377*** (−5.62) | −0.115* (−1.67) |
| <i>Street Earnings</i> × <i>Treatment</i> | −0.594*** (−8.62) | −0.420*** (−6.52) |
| <i>Street Earnings</i> × <i>Post</i> × <i>Treatment</i> | 0.406*** (5.36) | 0.270*** (3.78) |
| <i>Controls</i> | Yes | Yes |
| <i>Firm</i> , <i>Year-Qtr</i> , <i>AnnHour FE</i> | Yes | Yes |
| Observations | 133,817 | 133,817 |
| Adjusted <i>R</i> ² | 0.834 | 0.798 |

This table reports coefficient estimates from regressing future street earnings and cash flows on current quarter street earnings, interacted with an indicator variable for Thomson Reuters' 2009 methodology change, *Post*, and an indicator variable for treatment observations, *Treatment*. *Street Earnings* is defined as the I/B/E/S actual EPS for quarter *t*, scaled by total assets per share. The dependent variable in column 1 is *Future Street Earnings*, defined as the sum of I/B/E/S actual EPS for quarters *t*+1 to *t*+4, scaled by total assets per share. In column 2, the dependent variable is *Future Cash Flows*, defined as the sum of cash from operations for quarters *t*+1 to *t*+4, scaled by total assets. *Controls* is the vector of variables listed as "Control variables" in table A1. *Firm*, *Year-Qtr* and *AnnHour FE* are firm, year-quarter, and earnings announcement hour fixed effects, respectively. Control variables and fixed effects are included in all columns, but not reported. Variable definitions are provided in table A1. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively, using the two-tailed *t*-test. Standard errors are based on a two-way clustering at both firm level and earnings announcement date level.

I/B/E/S actual EPS for quarter *t*, scaled by total assets per share. All other variables are as defined in equation (1).

In equation (2), *Street Earnings* is measured over one quarter, while *Future Street Earnings* and *Future Cash Flows* are measured over the following four quarters. Thus, perfectly permanent *Street Earnings* will have a coefficient of four while perfectly transitory *Street Earnings* will have a coefficient of zero (Bentley et al. [2018]). The coefficient on *Street Earnings*, β_3 , captures the predictive ability of *Street Earnings* for the control firms during the pre-methodology change period, while the coefficient on *Street Earnings* × *Treatment*, β_5 , captures the average difference in the predictive ability of *Street Earnings* for the treatment sample relative to the control sample in that same period. Our coefficient of interest is β_6 , the coefficient for *Street Earnings* × *Post* × *Treatment*, which captures the DiD for the predictive ability of *Street Earnings* in the treatment sample relative to the control sample following the methodology change.

Table 4 reports the results from estimating Equation (2)). In column 1, the dependent variable is *Future Street Earnings*. The positive *Street Earnings* coefficient of 1.566 (*t*-stat. = 21.12) implies that, for announcements in the

control sample prior to the methodology change, \$1 of street earnings in the current quarter is associated with \$1.56 of street earnings over the next four quarters.¹⁸

The negative *Street Earnings* \times *Treatment* coefficient (coef. = -0.594 , t -stat. = -8.62) indicates that the predictive ability of street earnings is lower for treatment announcements prior to the methodology change. However, the positive coefficient of 0.406 (t -stat. = 5.36) for *Street Earnings* \times *Post* \times *Treatment* suggests that the 2009 methodology change increased the predictive ability of street earnings for treatment observations relative to the control sample.

The results in column 2, where the dependent variable is *Future Cash Flows*, lead to similar inferences. Specifically, relative to the coefficient of 1.185 (t -stat. = 16.32) for *Street Earnings*, the coefficient of -0.420 (t -stat. = -6.52) for *Street Earnings* \times *Treatment* implies that the ability of street earnings to predict future cash flows is approximately 35% lower in the treatment group prior to the methodology change, all else equal (i.e., $0.420/1.185$). However, the positive coefficient of 0.270 (t -stat. = 3.78) for *Street Earnings* \times *Post* \times *Treatment* indicates that the 2009 methodology change increased the predictive ability of street earnings for treatment observations relative to the control sample by approximately 21%.

To visually illustrate these results, figure 4 plots the regression results from table 4. The slope of the solid blue (solid red) lines indicates the effect of the methodology change on the street earnings coefficient for the control (treatment) sample. The dashed red line provides the counterfactual path that would have resulted if the estimated coefficients from the treatment sample had followed a parallel trend of the control sample. The incremental increase among treatment observations due to the methodology change is noted as the effect of the methodology change. Taken together, these results suggest that the 2009 methodology change significantly improved the predictive ability of street earnings for announcements containing unexpected line items.

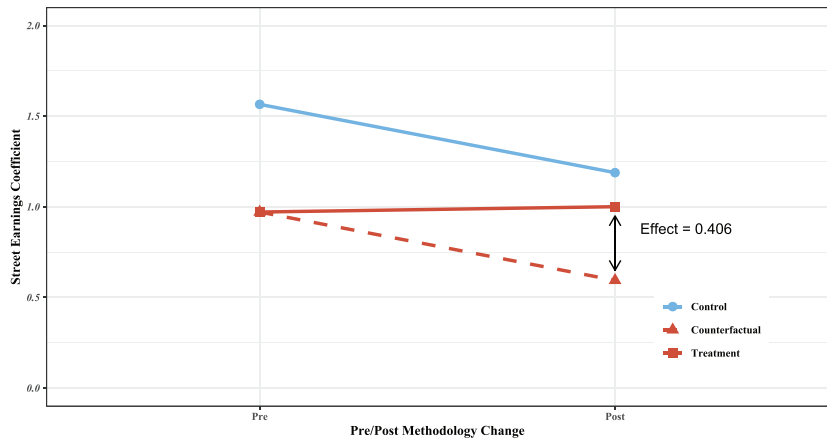
5. Effects of TR's 2009 Methodology Change on Analysts' Information Production

5.1 PREDICTIONS

The 2009 methodology change occurred as part of TR's systems integration between its I/B/E/S and First Call databases, making it a plausibly exogenous event with respect to analysts' and investors' responses to earnings announcements. In this section, we examine the implications

¹⁸ We note that the *Street Earnings* coefficients in table 4 are lower than those reported in prior studies (e.g., Bentley et al. [2018]). In untabulated tests, we find that this is due to the inclusion of firm and year-quarter fixed effects in our models.

(a) Association between current quarter street earnings and future street earnings



(b) Association between current quarter street earnings and future cash flows

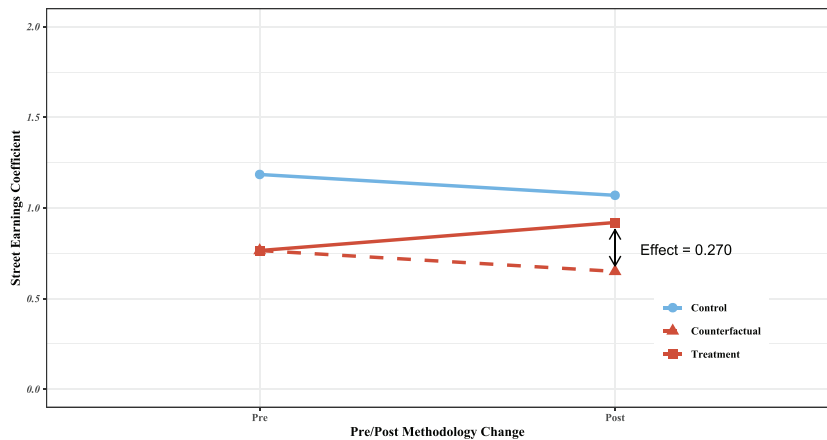


FIG 4.—Effects of Thomson Reuters’ methodology change on the predictive value of street earnings. This figure plots the estimated marginal effects of TR’s methodology change on the predictive ability of current street earnings for future street earnings and cash flows, based on the regression estimates tabulated in table 4. The dependent variable in panel (a) is *Future Street Earnings*. In panel (b), the dependent variable is *Future Cash Flows*. Full variable definitions are provided in table A1. In each panel, the solid blue (solid red) lines plot the estimated coefficients associating *Street Earnings* with the dependent measure for the control (treatment) group before and after TR’s methodology change. The dashed red line provides the counterfactual path that would have resulted if the estimated coefficients from the treatment sample had followed a parallel trend to the control sample over the same period. The variation in the *Street Earnings* coefficients between groups is calculated by summing the relevant interaction terms from table 4. For example, the *Street Earnings* coefficient for the treatment group in the pre-period is equal to the sum of the *Street Earnings* and *Street Earnings* × *Treatment* coefficients.

of the 2009 methodology change for analysts' post-earnings information production.

There are two reasons why the change in TR's treatment of unexpected line items may influence analyst decision-making. Specifically, analysts' exclusion decisions depend on their imperfect understanding of the company's operations and the nature of unexpected line items (operating vs. nonoperating, permanent vs. transitory). Given this ambiguity, analysts may conform their treatment of unexpected line items to TR's definition. Prior research finds that the observability of the consensus forecast causes individual analysts to skew their forecasts toward the consensus (e.g., Trueman [1994] and Welch [2000]). In our setting, we posit that similar herding forces may be in play after 2009 when street earnings become observable to individual analysts (as opposed to TR waiting to observe analysts' ex post actuals).

Second, even analysts who do not observe TR-reported street earnings but have become aware of TR's decision rule—exclude all unexpected line items—may adopt the rule as their own because of the rule's simplicity and beneficial effect on street earnings properties (i.e., higher predictive ability). Since changes in TR's policies and methodologies are announced to subscribers, we believe that at least some analysts and/or analysts-subscribers have been made aware of the 2009 methodology change. Moreover, figure A1 provides anecdotal evidence of analysts referring to I/B/E/S street earnings in their reports, making it more plausible that TR's methodology change had an impact on analysts.

5.2 METHOD AND RESULTS

To examine the effect of the 2009 methodology change on analysts' post-earnings information production, we modify Equation (1) as follows:

$$\begin{aligned} Property = & \beta_1 Treatment + \beta_2 Post \times Treatment + A \times Controls \\ & + B \times FirmFE + C \times YearQtrFE + D \times AnnHourFE + \epsilon, \end{aligned} \quad (3)$$

where *Property* is one of the following three properties of analysts' information production: *DIFF*, *MAFE*, and *Dispersion*. *DIFF* is a dummy variable that equals 1 if at least one analyst who issues an EPS forecast over the five days after the first fiscal quarter's earnings announcement differs by at least one penny with respect to her inferred actual EPS from I/B/E/S reported actual EPS. We infer an analyst's first fiscal quarter actual EPS by subtracting the sum of her next three quarters' EPS forecasts from the full fiscal year EPS forecast (see Brown and Larocque [2013]). As *DIFF* is defined using only the first fiscal quarter analyst EPS forecasts, our sample is reduced to 22,086 observations for this variable. *MAFE* is the mean absolute forecast error defined as the analyst EPS forecast of next quarter earnings minus

TABLE 5
Effects of Thomson Reuters' Methodology Change on Properties of Analyst Forecasts

| | <i>DIFF</i> (1) | <i>MAFE</i> (2) | <i>Dispersion</i> (3) |
|---|----------------------|----------------------|--------------------------|
| <i>Treatment</i> | 0.064*** (4.78) | 0.032*** (7.45) | 0.013*** (6.87) |
| <i>Post</i> × <i>Treatment</i> | −0.040*** (−2.68) | −0.020*** (−3.92) | −0.006*** (−2.62) |
| <i>Controls</i> | Yes | Yes | Yes |
| <i>Firm</i> , <i>Year-Qtr</i> , <i>AnnHour FE</i> | Yes | Yes | Yes |
| Observations | 22,086 | 95,636 | 95,636 |
| Adjusted <i>R</i> ² | 0.193 | 0.324 | 0.414 |

This table reports coefficient estimates from regressing analyst forecast characteristics on an indicator variable for Thomson Reuters' 2009 methodology change, *Post*, an indicator variable for treatment observations, *Treatment*, and the interaction term between *Post* and *Treatment*. The dependent variable in column 1 is *DIFF*, which is analogous to the *DIFF* indicator in Brown and Larocque [2013] and equals 1 if at least one analyst, who issues an EPS forecast over the five days after an earnings announcement, differs with respect to her first fiscal quarter inferred EPS actual from the I/B/E/S reported actual EPS for the same quarter by at least one penny. In column 2, the dependent variable is *MAFE*, defined as the mean absolute forecast error across all forecasts of next quarter earnings issued within five days after the earnings announcement scaled by the absolute value of actual EPS. The dependent variable in column 3, *Dispersion*, is calculated as the standard deviation of analyst next quarter earnings forecasts issued in the five days following an earnings announcement scaled by the absolute value of actual EPS. *Controls* is the vector of variables listed as "Control variables" in table A1. *Firm*, *Year-Qtr* and *AnnHour FE* are firm, year-quarter, and earnings announcement hour fixed effects, respectively. Control variables and announcement hour fixed effects are included in all columns, but not reported. Variable definitions are provided in table A1. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively, using the two-tailed *t*-test. Standard errors are based on a two-way clustering at both firm level and earnings announcement date level.

the actual EPS scaled by the absolute value of actual EPS.¹⁹ *Dispersion* is the standard deviation of analyst next-quarter EPS forecasts issued in the five days following an earnings announcement divided by the absolute value of actual EPS. All other variables are as defined in Equation (1).

As the methodology change improved the predictive ability of TR's street earnings, we expect analysts' individual quarter *t* actuals to conform with TR's street earnings more often. Further, as analysts may build from this improved quarter *t* actual in developing their quarter *t* + *n* forecasts, we expect an increase the accuracy of those forecasts. Finally, if analysts become more uniform in incorporating TR's quarter *t* street earnings in their *t* + *n* forecasts following the methodology change, their *t* + *n* forecasts may become less dispersed.

Table 5 reports the results from estimating Equation (3)). Columns (1)–(3) report the results using *DIFF*, *MAFE*, and *Dispersion* as dependent variables, respectively. The negative and significant coefficient on *Post* × *Treatment* (coef. = −0.040, *t*-stat. = −2.68) in column 1 suggests that, for the treatment group, analysts' ex post implied actuals become more similar to I/B/E/S street earnings relative to the control group. This finding is consistent with analysts adopting TR's treatment of unexpected line items.

¹⁹ Following Loh and Stulz [2018], denominator values smaller than \$0.25 are set to \$0.25 to limit the impact of small denominators.

In column 2, the negative and significant coefficient on $Post \times Treatment$ (coef. = -0.020 , t -stat. = -3.92) suggests that, following the methodology change, the mean absolute forecast error drops by 9.45% in the treatment group relative to the control group, as a percentage of the sample mean *MAFE*. Finally, in column 3, the negative coefficient on $Post \times Treatment$ (coef. = -0.006 , t -stat. = -2.62) suggests that, following the methodology change, forecast dispersion drops by 5.94% in the treatment group relative to the control group, as a percentage of the sample mean.

Figure 5 plots the estimated marginal effects from estimating Equation (3)) after replacing $Post \times Treatment$ with interactions between *Treatment* and quarterly event-time indicator variables.²⁰ As *DIFF* is only measured for fiscal Q1 earnings announcements, the estimated marginal effects exhibit large standard errors when plotted in event time. Nevertheless, consistent with the results tabulated in table 5, the estimated marginal effects for treatment and control observations follow roughly parallel trends prior to the 2009 methodology change for *DIFF*, *MAFE*, and *Dispersion*, and the estimated differences between treatment and control observations generally become smaller or insignificant after the 2009 methodology change.

Collectively, consistent with the improvements in the quality of I/B/E/S street earnings documented in section 4, the results in this section provide evidence of an increased similarity between analysts and TR with respect to the first quarter actual EPS, as well as improvements in forecast accuracy and lower dispersion. More generally, these results imply that, in addition to shaping the properties of street earnings, FDPs can affect the information production of other intermediaries.

5.3 CROSS-SECTIONAL ANALYSES

In this section, we examine whether the methodology change affected some analysts more than others. Prior to the methodology change, TR waited for a majority analyst basis to emerge with respect to the treatment of unexpected line items. Hence, many analysts issued their next quarter earnings forecasts prior to observing TR's street earnings (timely analysts, henceforth). TR's methodology change, which increased its timeliness from days to minutes, gave timely analysts an opportunity to incorporate TR's street earnings prior to issuing their next quarter forecasts.²¹ We expect these analysts to experience a greater increase in their forecast accuracy and reliance on TR after the methodology change.

²⁰ As discussed further in section 7, analysts' forecasts are significantly impacted by the 2008–2009 financial crisis (Loh and Stulz [2018]). Therefore, we also include a financial crisis indicator as an additional control so that the effects of the quarterly indicators better isolate the event-induced variation in the outcome variables.

²¹ After the methodology change, more than 99% of all individual analysts' post-earnings forecasts are announced after TR releases street earnings, for both treatment and control observations.

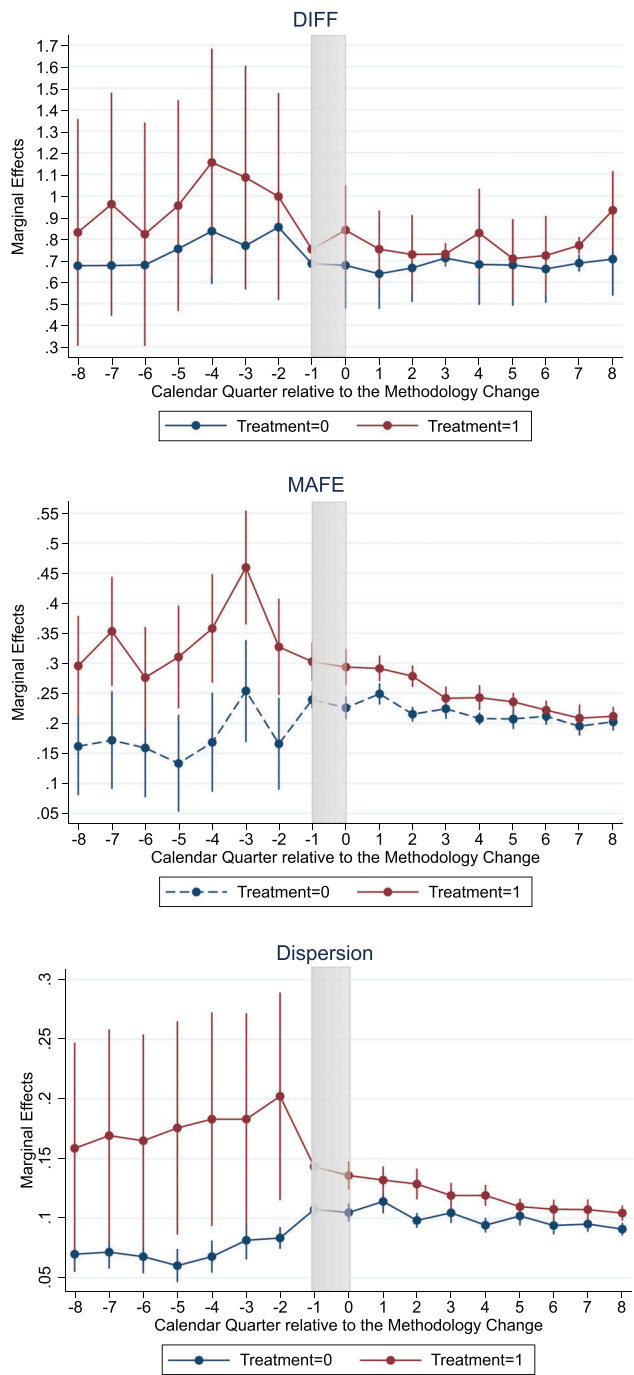


FIG 5.—Effects of Thomson Reuters' methodology change on analysts' information production. This figure plots estimated marginal effects for analysts' *DIFF*, *MAFE*, and *Dispersion*,

respectively, from estimating Equation (3) after replacing $Post \times Treatment$ with interactions between $Treatment$ and each quarter event-time indicator variable (i.e., each $Year-Qtr$ FE from Equation (3)). The figure plots the estimated marginal effects from quarter $t - 8$ to quarter $t + 8$ relative to the 2009 methodology change. Quarter $t = 0$ is the quarter ending on September 30, 2009. The red (blue) line plots the estimated value of each outcome variable when $Treatment = 1$ ($Treatment = 0$). The error bars around each point provide the 95% confidence intervals for the estimated marginal effects. Quarterly indicators for each of the 60 quarters included in the sample period, with the exception of 2005Q1 (reference level) are included in the estimation. To mitigate the confounding effect of the 2008–2009 financial crisis on analysts' information production, in each model, we also include an interaction between $Treatment$ and an indicator variable for the crisis period as defined in Loh and Stulz [2018]. All other control variables and fixed effects are as specified in Equation (3). Variable definitions are reported in table A1.

To examine the differential impact of TR's methodology change on timely analysts, we employ a two-stage analysis. In the first stage, we model the likelihood of an analyst to issue a forecast before TR disseminates street earnings in the pre-period as a function of various analyst characteristics (e.g., All-Star status, employer size, general forecasting experience, firm-specific forecasting experience, forecast accuracy, and analyst distraction). Intuitively, this stage uses the information in analyst attributes and the timing of analyst forecasts in the pre-period to identify analysts in the post period who would have likely released reports prior to TR's street earnings if not for the methodology change. In the second stage, we test whether the effects of the methodology change are stronger for these analysts.

Stated formally, in the first stage we estimate the following equation:

$$\begin{aligned} TimelyAnalyst_{i,j,t} = & \beta_0 + \beta_1 GeneralExperience_{i,t} + \beta_2 FirmExperience_{i,j,t} \\ & + \beta_3 Allstar_{i,t} + \beta_4 Accuracy_{i,t} \\ & + \beta_5 EmployerSize_{i,t} + \beta_6 ConcurrentEAs_{i,t} \\ & + A \times Controls_{j,t} + \epsilon_{i,j,t}, \end{aligned} \quad (4)$$

where $TimelyAnalyst_{i,j,t}$ is an indicator equal to 1 if analyst i announces a quarter $t+1$ forecast for firm j after firm j 's quarter t earnings are announced but before TR activates firm j 's quarter t street earnings, and 0 otherwise. $GeneralExperience_{i,t}$ ($FirmExperience_{i,j,t}$) is defined as the number of years for which analyst i issued at least one forecast for any firm (firm j), as of the date of firm j 's quarter t announcement. $Allstar_{i,t}$ is an indicator equal to 1 if analyst i was classified as an All-Star analyst in the year they issued their forecast, and 0 otherwise. $Accuracy_{i,t}$ is the mean absolute forecast error (scaled by actual earnings per share) across all of analyst i 's one-quarter ahead earnings forecasts issued over the one-year period leading up to analyst i 's forecast, multiplied by minus one. $EmployerSize_{i,t}$ is the number of analysts employed by analyst i 's broker over the one-year period leading up to analyst i 's forecast. $ConcurrentEAs_{i,t}$ is the number of firms

covered by analyst i that announce earnings within five days of analyst i 's forecast (Driskill, Kirk, and Tucker [2020]), and $Controls_{j,t}$ is the vector of firm-quarter controls listed as "Control variables" in table A1. We estimate Equation (4)) during the pre-period. This allows us to predict the likelihood that an analyst would have announced their report before TR in the post-period, if not for the methodology change.

In the second stage, we apply the estimated coefficients from Equation (4)) to calculate the probability that analyst i 's report will be announced before TR activates street earnings, $\widehat{TimelyAnalyst}_{i,j,t}$. We classify observations in the top (bottom) quartile of $\widehat{TimelyAnalyst}_{i,j,t}$ as those with high (low) probability of being forecast by a timely analyst. In other words, observations with high (low) values of $\widehat{TimelyAnalyst}$ are reports written by analysts who would be more (less) likely to announce before TR prior to the methodology change. We then re-estimate Equation (3)) separately for observations in the high and low $\widehat{TimelyAnalyst}$ subsamples, with analyst-firm-quarter measures of $DIFF_{i,j,t}$ and $AFF_{i,j,t}$ (absolute forecast error) as the dependent variables, respectively.

Table 6 reports results of estimating Equation (4)). The estimation sample is drawn from the individual next-quarter forecasts that were used to conduct our analyses in table 5. We restrict the sample to forecasts announced before the September 30, 2009 methodology change, and require available data to compute all of the measures included in Equation (4), resulting in an estimation sample of 181,685 analyst-firm-quarter observations.

We find that analysts with more *General Experience* and larger *Employer Size* are more likely to announce their forecasts before TR activates street earnings. We also observe that analysts who are less accurate over the prior year are more likely to announce their forecasts before TR, consistent with a trade-off between forecast timeliness and accuracy. We use the coefficients in column 1 of table 6 to compute $\widehat{TimelyAnalyst}_{i,j,t}$.

Table 7 reports the results of estimating Equation (3) after partitioning our sample into timely/untimely analysts based on the predicted probability $\widehat{TimelyAnalyst}_{i,j,t}$. In columns (1) and (2), we observe a significant reduction in $DIFF_{i,j,t}$ for observations with high values of $\widehat{TimelyAnalyst}_{i,j,t}$, but no significant results for observations with low values of $\widehat{TimelyAnalyst}_{i,j,t}$. In columns (3) and (4), we observe a similar pattern for $AFF_{i,j,t}$. These results suggest that analysts who were more likely to issue their reports before TR in the pre-period, and thus not have an opportunity to incorporate TR's street earnings information, are more likely to benefit from the methodology change.²² As TR disseminates street earnings

²²In untabulated analyses, we test for differential effects of the methodology change on analysts based on their All-Star status, level of resources, forecasting experience, or prior forecast accuracy and find weak evidence of significant differences based on individual analyst characteristics.

TABLE 6
Determinants of Analysts' Timeliness Relative to Thomson Reuters

| | Timely Analyst | |
|-----------------------------------|----------------------|----------------------|
| | (1) | (2) |
| <i>General Experience</i> | 0.042*** (3.47) | 0.019*** (2.85) |
| <i>Firm Experience</i> | 0.008 (0.93) | 0.009* (1.71) |
| <i>Allstar</i> | 0.028* (1.75) | −0.015 (−1.39) |
| <i>Accuracy</i> | −0.071*** (−7.36) | −0.026*** (−3.57) |
| <i>Employer Size</i> | 0.052*** (10.48) | 0.039*** (10.31) |
| <i>Concurrent EAs</i> | −0.019* (−1.71) | −0.008 (−0.97) |
| <i>Controls</i> | Yes | Yes |
| <i>Firm, Year-Qtr, AnnHour FE</i> | No | Yes |
| Observations | 181,685 | 181,540 |
| Adjusted R ² | 0.061 | 0.194 |

This table reports coefficient estimates from an OLS regression of an indicator variable for analysts' timeliness relative to Thomson Reuters, *Timely Analyst*, on analyst-firm-quarter characteristics. The estimation sample is drawn from the individual next-quarter forecasts that were used to conduct our analyses in table 5. We restrict the sample to forecasts announced before the September 30, 2009 methodology change, and require available data to compute all of the measures included in Equation (4)). The dependent measure, *Timely Analyst*, is an indicator equal to 1 if analyst *i* announces their quarter *t*+1 forecast for firm *j* after firm *j*'s quarter *t* earnings are announced but before TR activates firm *j*'s quarter *t* street earnings, and 0 otherwise. Full variable definitions are provided in table A1. *Controls* is the vector of variables listed as "Control variables" in table A1. *Firm, Year-Qtr* and *AnnHour FE* are firm, year-quarter, and earnings announcement hour fixed effects, respectively. Control variables and fixed effects are included according to the notes in each column, but not reported. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively, using the two-tailed *t*-test. Standard errors are based on a two-way clustering at both firm level and earnings announcement date level.

more quickly after the methodology change, without waiting for analysts to issue their reports, these analysts gain the opportunity to incorporate street earnings information in their reports, resulting in improved forecast accuracy. Collectively, observing consistent results for firm-level tests in table 5 and analyst-level cross-sectional tests in table 7 make it less likely that our results are subject to alternative explanations.

6. Street Earnings Information Processing and Market Reaction
Timeliness

6.1 THE EFFECT OF THE 2009 METHODOLOGY CHANGE ON PRICE
EFFICIENCY

This section examines the implications of our findings in sections 4 and 5 for investor processing of street earnings. Ultimately, we expect any meaningful changes in the properties of street earnings and analysts'

TABLE 7
Variation in the Effects of Thomson Reuters' Methodology Change on Individual Analysts

| | DIFF _{i,j,t} | | AFE _{i,j,t} | |
|----------------------------|-----------------------|---------------------|----------------------|----------------------|
| | Low (1) | High (2) | Low (3) | High (4) |
| Treatment | 0.015 (0.88) | 0.061*** (3.49) | 0.011** (2.11) | 0.053*** (4.56) |
| Post × Treatment | −0.008 (−0.42) | −0.039** (−1.99) | −0.008 (−1.41) | −0.046*** (−3.55) |
| Controls | Yes | Yes | Yes | Yes |
| Firm, Year-Qtr, AnnHour FE | Yes | Yes | Yes | Yes |
| Observations | 30,344 | 29,410 | 158,482 | 202,956 |
| Adjusted R ² | 0.119 | 0.099 | 0.315 | 0.281 |

This table reports coefficient estimates from regressing analyst forecast characteristics on an indicator variable for Thomson Reuters' 2009 methodology change, *Post*, an indicator variable for treatment observations, *Treatment*, and the interaction term between *Post* and *Treatment*, conditional on the predicted likelihood of an analyst to issue an earnings forecast prior to TR's announcement of street earnings, $\widehat{TimelyAnalyst}$. $\widehat{TimelyAnalyst}$ is determined based on the regression model in column 1 of table 6. The Low and High subsamples are defined as observations in the lowest and highest quartiles of $\widehat{TimelyAnalyst}$, respectively. The estimation sample is drawn from the individual analysts' next-quarter forecasts that were used to conduct our analyses in table 5. We restrict the sample to forecasts in the Low and High $\widehat{TimelyAnalyst}$ subsamples, and require available data to compute all of the measures included in Equation (4). The dependent variable in columns (1)–(2) is $DIFF_{i,j,t}$, an indicator that equals 1 if analyst i 's inferred EPS actual for firm j for quarter t differs from the I/B/E/S reported actual for the same firm-quarter by at least one penny (Brown and Larocque [2013]). In columns (3) and (4), the dependent variable is $AFE_{i,j,t}$, defined as the absolute forecast error for analyst i 's quarter $t+1$ forecast for firm j scaled by the absolute value of firm j 's actual EPS for quarter $t+1$. *Firm*, *Year-Qtr*, and *AnnHour FE* are firm, year-quarter, and earnings announcement hour fixed effects, respectively. Control variables and fixed effects are included in all columns, but not reported. Variable definitions are provided in table A1. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively, using the two-tailed t -test. Standard errors are based on a two-way clustering at both firm level and earnings announcement date level.

information production to affect investors. In this regard, Schaub [2018] finds that the timeliness of street earnings dissemination by FDPs is associated with the efficiency of the market response to earnings announcements. Therefore, to the extent that the 2009 methodology change improves the timeliness of street earnings dissemination for announcements with unexpected line items, we expect timelier market reactions for these earnings announcements.

Zhang [2008] and Yezegel [2015] find that the efficiency of the market response to earnings announcements also depends on the timeliness of analysts' forecasts and recommendation revisions. Our results in section 5 and examples in figure A1 suggest that analysts incorporate FDPs' street earnings information in producing their post-earnings reports. Therefore, analysts' timeliness might also depend on the timeliness of TR's street earnings dissemination. This dependence of analysts on TR may, in turn, mediate the relation between TR's activation delay and the market response to earnings announcements. That is, the 2009 methodology change can affect investors directly—by affecting the speed of TR's street earnings dissemination, and indirectly—by influencing the timeliness of financial analysts.

To test the direct and indirect (through analysts) effects of the 2009 methodology change on the timeliness of investor response to earnings, we estimate the following system of equations:

$$\begin{aligned} \ln(\text{Forecasting Delay}) = & \beta_1 \text{Treatment} + \beta_2 \text{Post} \times \text{Treatment} \\ & + A \times \text{Controls} + B \times \text{FirmFE} \\ & + C \times \text{YearQtrFE} + D \times \text{AnnHourFE} + \epsilon, \end{aligned} \quad (5)$$

$$\begin{aligned} \text{MRT}_{[0,16]} = & \alpha + \gamma_1 \ln(\text{Forecasting Delay}) \\ & + \gamma_2 \text{Treatment} + \gamma_3 \text{Post} \times \text{Treatment} \\ & + A \times \text{Controls} + B \times \text{FirmFE} \\ & + C \times \text{YearQtrFE} + D \times \text{AnnHourFE} + \epsilon, \end{aligned} \quad (6)$$

where $\ln(\text{Forecasting Delay})$ is the natural logarithm of the number of trading minutes from the earnings press release time to the announcement time of the first analyst forecast following the earnings press release, and $\text{MRT}_{[0,16]}$ is intraday market reaction timeliness. All other variables are as previously defined. We standardize all continuous variables in equations (5) and (6) to have a mean of zero and standard deviation of one.

We examine the effect of the 2009 methodology change on investors' market reaction timeliness during the first 16 trading hours after the earnings announcement (i.e., one full extended-hours trading day), as the median *Activation Delay* during our sample period is only 46 minutes.²³ To calculate $\text{MRT}_{[0,16]}$, we partition the 16-hour trading day beginning at the minute of the earnings press release into four 4-hour intervals. We then calculate $\text{MRT}_{[0,16]}$ as follows:

$$\text{MRT}_{[0,16]} = \frac{\text{RET}_{[0,4hr]}}{\text{RET}_{[0,16hr]}} + \frac{\text{RET}_{[0,8hr]}}{\text{RET}_{[0,16hr]}} + \frac{\text{RET}_{[0,12hr]}}{\text{RET}_{[0,16hr]}} + 0.5,$$

where $\text{RET}_{[0,t]}$ is the buy-and-hold return up to hour t following the earnings press release.²⁴ Intuitively, price discovery is more (less) efficient if earlier intervals over the 16-hour period account for a larger (smaller) por-

²³ Prior literature measures post-earnings information dissemination at the daily level, and examines variation in post-earnings announcement drift when dissemination is delayed beyond a trading day (Schaub [2018] and Zhang [2008]). Our focus on intraday MRT is consistent with the intraday variation in dissemination we examine, as well as the results in Martineau [2021] that the phenomenon of post-earnings announcement drift has become less significant over our recent sample period.

²⁴ Following Li et al. [2015] and Akbas et al. [2018], we use TAQ data to examine trading during both regular and extended trading hours, such that a full trading day covers 16 trading hours (960 trading minutes, from 4:00 a.m. to 8:00 p.m.), and winsorize each intraday return ratio at -1 and 1 to mitigate the effect of extreme outliers. For consistency, in this section, we also measure *Activation Delay* in trading minutes.

TABLE 8
Effects of Thomson Reuters' Methodology Change on Analysts' and Investors' Timeliness

| | (1) <i>Ln(Forecasting Delay)</i> | (2) <i>MRT_[0,16]</i> | (3) <i>Indirect Effect</i> |
|-----------------------------------|-------------------------------------|------------------------------------|-------------------------------|
| <i>Ln(Forecasting Delay)</i> | | −23.573*** (−36.96) | |
| <i>Treatment</i> | 3.422*** (5.13) | −1.512** (−2.34) | |
| <i>Post × Treatment</i> | −2.213*** (−3.03) | 1.361** (2.01) | 0.522*** (<i>p</i> < .01) |
| <i>Controls</i> | Yes | Yes | |
| <i>Firm, Year-Qtr, AnnHour FE</i> | Yes | Yes | |
| Observations | 142,915 | 142,915 | |
| Adjusted <i>R</i> ² | 0.447 | 0.561 | |

This table reports coefficient estimates from regressing measures of analyst and investor timeliness on an indicator variable for Thomson Reuters' 2009 methodology change, *Post*, an indicator variable for treatment observations, *Treatment*, and the interaction term between *Post* and *Treatment*. The dependent variable in column 1 is analyst forecasting delay, *Ln(Forecasting Delay)*. In column 2, the dependent variable is investors' intraday market reaction timeliness, *MRT_[0,16]*. Column (3) provides the indirect effect of *Post × Treatment* on *MRT_[0,16]*, mediated by *Ln(Forecasting Delay)*. Following Imai, Keele, and Yamamoto [2010], we test for the indirect effect using nonparametric bootstrapped standard errors of the product of coefficients on *Post × Treatment* from columns (1) and (2). *Controls* is the vector of variables listed as "Control variables" in table A1. *Firm, Year-Qtr* and *AnnHour FE* are firm, year-quarter, and earnings announcement hour fixed effects, respectively. Control variables and fixed effects are included in all columns, but not reported. Variable definitions are provided in table A1. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively, using the two-tailed *t*-test. Standard errors are based on a two-way clustering at both firm level and earnings announcement date level.

tion of the 16-hour buy-and-hold return, resulting in larger (smaller) values of *MRT*.²⁵

Table 8 presents the standardized coefficients (in percentage) from estimating Eqs. (5) and (6). In column 1, where the dependent variable is *Ln(Forecasting Delay)*, the coefficients of 3.422 (*t*-stat. = 5.13) and −2.213 (*t*-stat. = −3.03) on *Treatment* and *Post × Treatment*, respectively, indicate that prior to the methodology change, analysts' post-earnings reports were incrementally delayed by 3.4% for announcements with unexpected line items, and that this incremental delay was reduced by 2.2% following the methodology change. These results suggest that the timeliness of analysts' post-earnings reports depends on the timeliness of TR's street earnings dissemination, consistent with our earlier findings in section 5 that analysts incorporate TR's street earnings information in their post-earnings reports.

In column 2, where the dependent measure is *MRT_[0,16]*, the estimated coefficients are −1.512 (*t*-stat. = −2.34) and 1.361 (*t*-stat. = 2.01) on *Treatment* and *Post × Treatment*, respectively. These coefficients indicate that, prior to the methodology change, investors' market reaction was around 1.5% less timely for announcements with unexpected line items, and that the

²⁵ Our intraday market reaction timeliness measure is analogous to the "intraproduct timeliness" (IPT) measure used in prior literature (e.g., Butler, Kraft, and Weiss [2007], Bushman, Smith, and Wittenberg-Moerman [2010], Drake, Thornock, and Twedt [2017], Twedt [2015]).

methodology change improved investor timeliness by around 1.4% for these announcements. We also observe a significant negative coefficient on $\text{Ln}(\text{Forecasting Delay})$, consistent with longer analyst delays leading to less timely market reactions to earnings news (Yezegel [2015], Zhang [2008]).

Column (3) of table 8 provides the indirect effect of $\text{Post} \times \text{Treatment}$ on $\text{MRT}_{[0,16]}$, mediated by $\text{Ln}(\text{Forecasting Delay})$. Following Imai, Keele, and Yamamoto [2010], we test for the indirect effect using nonparametric bootstrapped standard errors of the product of coefficients on $\text{Post} \times \text{Treatment}$ in Equation (5) and $\text{Ln}(\text{Forecasting Delay})$ in Equation (6). The coefficient of 0.522 (bootstrapped p -value < 0.01) suggests that a significant portion of the overall effect of the 2009 methodology change on the market response to earnings is mediated through its effect on analysts' information production. Specifically, we estimate that approximately 27.7% of the total effect of the 2009 methodology change on $\text{MRT}_{[0,16]}$ is mediated by $\text{Ln}(\text{Forecasting Delay})$ [$0.522 / (1.361 + 0.522)$]. Taken together, these results provide evidence that FDPs influence investors directly—by promptly disseminating street earnings information, and indirectly—by influencing the information production of financial analysts.

6.2 THE RELATIVE ROLES OF TR AND ANALYSTS IN THE PRICING OF STREET EARNINGS

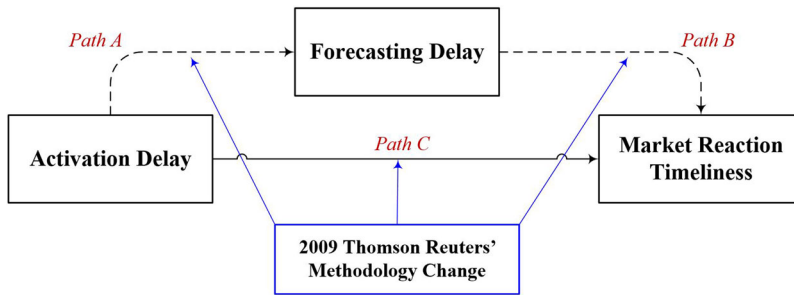
The analyses in subsection 6.1 use the 2009 methodology change as a shock to TR's activation delay that is plausibly exogenous to analysts and investors, allowing us to provide causal evidence that TR's activation delay has both direct and indirect effects on market reaction timeliness. In this section, we modify our analysis to more broadly examine the relative roles of FDPs and analysts in determining the efficiency of the market response to earnings. Specifically, we model market reaction timeliness as a function of TR and analyst delays, both measured in trading minutes. Our goal in this section is not to draw causal inferences from the difference between treatment and control groups. Rather, relying on the causal relations identified in our previous tests, we seek to disentangle analysts' and FDPs' roles in determining the timeliness of investor response to earnings and to examine how these roles change following the 2009 methodology change.

Prior studies examine the price effects of analysts' post-earnings timeliness (Yezegel [2015], Zhang [2008]) separately from the effects of street earnings timeliness (Schaub [2018]). Zhang [2008] focuses on the timeliness of analysts' forecast revisions, while Yezegel [2015] focuses on analysts' recommendation revisions. The extent to which investors value the timeliness of analysts' ex post actuals remains unknown. However, TR's practice of relying on analysts' ex post actuals to determine the treatment of unexpected line items suggests that they may provide investors with important street earnings information prior to the methodology change.

After the 2009 methodology change, TR discontinued its practice of relying on analysts' ex post actuals to determine the treatment of unexpected line items. This may decrease the importance of analysts'

post-earnings timeliness in determining market reaction timeliness. However, to the extent that the methodology change improves other aspects of analysts' input (e.g., accuracy of earnings forecasts), and these aspects are more important to investors, we may instead observe an increase in the importance of analysts' timeliness following the methodology change.

Thus, to examine the relative roles of *Activation Delay* and *Forecasting Delay* in determining MRT, we make two key changes to our research design from subsection 6.1. First, as noted above, we replace *Treatment* with *Activation Delay*. Second, in addition to allowing *Forecasting Delay* to mediate the relation between *Activation Delay* and $MRT_{[0,16]}$, we also allow the 2009 methodology change to moderate the effects of *Activation Delay* and *Forecasting Delay*. Thus, our expanded model represents a “moderated mediation” path analysis research design (Hayes [2017]), as follows.



Path A and path C model the direct effects of *Activation Delay* on *Forecasting Delay* and $MRT_{[0,16]}$, respectively, while path B captures the direct effect of *Forecasting Delay* on $MRT_{[0,16]}$. Consequently, path A \times path B captures the indirect effect of *Activation Delay*, mediated through *Forecasting Delay*, on $MRT_{[0,16]}$.

Stated formally, we estimate the following system of equations:

$$\begin{aligned} \ln(\text{Forecasting Delay}) = & \beta_1 \ln(\text{Activation Delay}) \\ & + A \times \text{Controls} + B \times \text{FirmFE} \\ & + C \times \text{YearQtrFE} + D \times \text{AnnHourFE} + \epsilon, \end{aligned} \quad (7)$$

$$\begin{aligned} MRT_{[0,16]} = & \alpha + \gamma_1 \ln(\text{Activation Delay}) + \gamma_2 \ln(\text{Forecasting Delay}) \\ & + A \times \text{Controls} + B \times \text{FirmFE} \\ & + C \times \text{YearQtrFE} + D \times \text{AnnHourFE} + \epsilon, \end{aligned} \quad (8)$$

where all variables are as previously defined. We estimate Eqs. (7) and (8) on the pre- and post-methodology change subsamples. We also run a fully interacted model with *Post* to test for differences in the effects of *Activation Delay* and *Forecasting Delay* on $MRT_{[0,16]}$ following the methodology change.

Figure 6 and table 9 present the standardized coefficients (in percent-

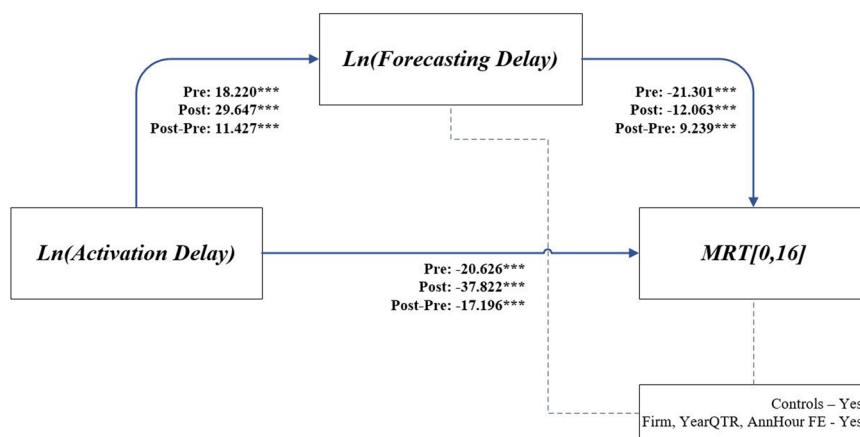


FIG 6.—Thomson Reuters' activation delay, analyst forecast delay, and market reaction timeliness. This figure plots the path diagram underlying the structural equation model of table 9, which explores the direct, indirect, and total effects of TR's activation delay and analyst forecast delay on the market reaction timeliness for the periods before and after TR's methodology change (i.e., September 30, 2009). The standardized coefficient estimates of the direct and indirect effects are obtained via the structural equation model of table 9 and reported as a percentage of one standard deviation of Market Reaction Timeliness (MRT). ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively, based on nonparametric bootstrapped standard errors (Imai, Keele, and Yamamoto [2010]).

age) from estimating Eqs. (7) and (8). Columns (1) and (2) of table 9 report the results of estimating Eqs. (7) and (8) on the pre- and post-methodology change periods. Column (3) reports the differences in the estimated coefficients. We find that while the effect of $\text{Ln}(\text{Activation Delay})$ on $\text{Ln}(\text{Forecasting Delay})$ (path A) is positive and significant in both pre- and post-periods, it increases substantially, from 18.220 to 29.674 following the methodology change. This increase of 11.427 is statistically significant at the 1% level, and suggests that analysts increase their reliance on FDPs' street earnings following the methodology change, consistent with the evidence from our DiD tests.²⁶

Prior to the methodology change, the direct effects of $\text{Ln}(\text{Activation Delay})$ and $\text{Ln}(\text{Forecasting Delay})$ on intraday MRT are relatively similar (−20.626 vs. −21.301), consistent with both intermediaries having similar importance in determining market timeliness. However, we find that the direct effect of activation delay on MRT becomes much stronger following the methodology change (−20.626 vs. −37.822), while the direct effect of forecasting delay becomes weaker (−21.301 vs. −12.063). Both differences are statistically

²⁶Note that prior to the 2009 methodology change, the direction of path A is reversed for some announcements (i.e., those with unexpected line items). After the 2009 methodology change, the expected causal direction goes from activation delay to forecasting delay for all announcements, consistent with the stronger association we observe in the post period.

TABLE 9
Thomson Reuters' Activation Delay, Analyst Forecast Delay, and Market Reaction Timeliness

| Outcome Variable: $MRT_{[0,16]}$ Mediating Variable: $Ln(\text{Forecasting Delay})$ | (1) Pre-Sample | (2) Post-Sample | (3) Difference Post-Pre |
|--|-------------------|--------------------|-------------------------------|
| Mediating Path (path A) | | | |
| $Ln(\text{Activation Delay}), Ln(\text{Forecasting Delay})$ | 18.220*** | 29.647*** | 11.427** |
| Direct Effects | | | |
| $Ln(\text{Forecasting Delay})$ (path B) | -21.301*** | -12.063*** | 9.239*** |
| $Ln(\text{Activation Delay})$ (path C) | -20.626*** | -37.822*** | -17.196*** |
| Indirect Effects | | | |
| $Ln(\text{Activation Delay})$ (path A \times path B) | -3.881*** | -3.576*** | 0.305 |
| Total Effects | | | |
| $Ln(\text{Activation Delay})$ | -24.507*** | -41.398*** | -16.887*** |
| $Ln(\text{Forecasting Delay})$ | -21.301*** | -12.063*** | 9.239*** |
| Effect Mediated (%) | 15.837*** | 8.639*** | |
| Controls | Yes | Yes | |
| Firm, Year-Qtr, AnnHour FE | Yes | Yes | |
| Observations | 46,887 | 96,028 | |
| R ² | 0.114 | 0.246 | |

This table reports the standardized coefficient estimates from a path analysis of the relation between street earnings activation delay ($Ln(\text{Activation Delay})$) and the intraday market reaction timeliness of street earnings news ($MRT_{[0,16]}$), recognizing the mediating role of analysts' forecasting delay ($Ln(\text{Forecasting Delay})$). We use a structural equation model to estimate the direct, indirect, and total effects of $Ln(\text{Activation Delay})$ and $Ln(\text{Forecasting Delay})$ on $MRT_{[0,16]}$, as defined in Equations (7) and (8) of the text. Column (1) reports the results for the period prior to Thomson Reuters' methodology change (i.e., the period prior to September 30, 2009), while Column (2) reports the results for the period after the methodology change. In column 3, we test the difference in estimated coefficients between the *Post* and *Pre* periods. *Controls* denotes the vector of variables listed as "Control variables" in table A1. *Firm FE*, *Year-Qtr FE*, and *AnnHour FE* are firm, year-quarter, and earnings announcement hour fixed effects, respectively. Control variables are included in all columns, but not reported. Variable definitions are provided in table A1. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively. Tests of direct effects are based on standard errors clustered by firm and earnings announcement date. Tests of indirect effects are based on nonparametric bootstrapped standard errors (Imai, Keele, and Yamamoto [2010]).

significant at the 1% level.²⁷ Collectively, these results highlight the relative roles of TR and analysts in aiding price discovery, and demonstrate how changes in TR's street earnings processing introduced by the methodology change shifted some portion of analysts' street earnings dissemination role to TR.

7. Additional Analyses and Sensitivity Tests

7.1 TREATMENT AND CONTROL ASSIGNMENTS

In this section, we describe additional analyses performed to check the robustness of our main findings to alternative research designs. First, as we cannot directly observe which announcements report unexpected line

²⁷The indirect effect of activation delay on intraday MRT (path A \times path B) depends on both path A and path B. As path A became stronger after the methodology change while path B became weaker, the overall indirect effect does not change significantly between the pre- and post-periods.

items, our treatment and control assignments introduce measurement error. Therefore, in table A1 of the online appendix, we repeat all of our tests using an alternative text-based classification of our treatment and control groups based on keywords from the earnings press release related to unexpected line items.²⁸ We obtain similar results across all of our tests. In table A2 of the online appendix, we perform placebo tests that randomly assign observations into treatment and control groups and find no significant results around the 2009 methodology change. This further validates that our *Treatment* variable proxies for the likelihood that an announcement contains unexpected line items. Collectively, these results provide further support that our main findings are not driven by a specific choice of the treatment variable.

7.2 PARALLEL TRENDS ASSUMPTION

Our research design also relies on the assumption that, if not for the 2009 methodology change, our treatment and control observations would exhibit parallel trends in our dependent variables over time. We perform a number of tests to validate the parallel trends assumption. First, figures 3 and 5 plot the estimated marginal effects from our main DiD analyses after replacing $Post \times Treatment$ with interactions between *Treatment* and quarterly event-time indicator variables. These figures provide visual evidence that the treatment and control groups follow roughly parallel trends prior to the 2009 methodology change, and the estimated differences between treatment and control observations become smaller or insignificant after the 2009 methodology change.

Second, in table A3 of the online appendix, we perform placebo tests that randomly shuffle announcements between the pre- and post-period. We find no significant DiD between the treatment and control groups when announcement dates are randomized into the pre- and post-periods. Finally, we acknowledge that the date of the 2009 methodology change occurs shortly after the end of the 2007–2008 financial crisis. Therefore, in table A4 of the online appendix we repeat all of our analyses after excluding announcements that occur during the financial crisis period specified by Loh and Stulz [2018], and find that all inferences remain qualitatively unchanged.

²⁸ List of keywords related to the discussion of unexpected line items: *one-time*, *onetime*, *non-recurring*, *nonrecurring*, *one-time charge*, *one-time gain*, *special item*, *special items*, *nonrecurring item*, *nonrecurring items*, *extraordinary item*, *extraordinary items*, *eps effect of*, *restructuring charge*, *restructuring charges*, *restructuring costs*, *restructuring cost*, *unusual*, *asset sale gains*, *asset sale losses*, *inventory adjustments*, *currency adjustments*, *realized securities gains*, *realized securities losses*, *acquisition expense*, *acquisition gain*, *failed acquisition*, *impairment of goodwill*, *goodwill impairment*, *impairment of investment*, *investment impairment*, *asset impairment*, *impairment of asset*, *dilution gain*, *dilution loss*, *litigation settlement*, *litigation gains*, *litigation charges*, *tax settlements*, *tax adjustments*, *nonrecurring income taxes*, *insurance settlements*, *insurance settlement*, *insurance recovery*, *insurance proceeds*, *write-off*, *write-offs*, *write-down*, *write-downs*, *charge-offs*, *charge-off*, *credit to reserves for bad debts*, *charge to reserves for bad debts*.

7.3 CHANGES IN MANAGEMENT DISCLOSURES

Although the focus of our study is on how a plausibly exogenous change in TR's activities influenced the properties of street earnings and analyst forecasts, we acknowledge the general possibility that managers' behavior, induced by the TR's methodology change or independent of it, may influence our inferences. We address this possibility in several ways. First, we examine whether managers change the complexity (i.e., readability) of press release sentences discussing one-time items around 2009. We find no evidence that they do, which alleviates the concern that our findings of enhanced informational properties of street earnings and analyst forecasts are due to earnings press releases becoming easier to process. Second, we examine a subset of earnings announcements with available conference call timestamps, and find that in the post-methodology change period, street earnings are almost always activated prior to the conference call. This makes it unlikely that our results are driven by changes in managers' conference call discussion. Third, in all of our tests we control for management guidance and textual properties of the earnings press release such as length, non-GAAP disclosure words, and the proportion of informative numbers. We acknowledge that these tests do not capture all of the ways in which managers may change their disclosures, and leave a more comprehensive analysis of management disclosures for future research.

8. Conclusions

In this study, we exploit a plausibly exogenous change in how a prominent FDP, TR, produces and distributes street earnings in order to deepen our understanding of FDPs' role in capital markets. In September 2009, TR discontinued its practice of relying on financial analysts to determine the treatment of unexpected line items in favor of their immediate exclusion from street earnings. Using a DiD research design, we document that FDPs play a broader role in capital markets through their influence on the properties of street earnings and their effects on other intermediaries. Specifically, we find that following the 2009 methodology change, street earnings are more timely and exhibit greater ability to predict future performance. We also find an increased reliance on TR street earnings by financial analysts, resulting in more accurate and less dispersed analyst forecasts. Next, we document that FDPs influence investors' response to earnings directly—by promptly disseminating street earnings information, and indirectly—by influencing the information production of financial analysts. As a consequence of the methodology change, we also find that TR's (analysts') relative role in the pricing of street earnings is stronger (weaker) following the methodology change.

Collectively, our findings extend a growing literature on the role of FDPs in the formation, measurement, and dissemination of capital market information (e.g., Akbas et al. [2018], Kaplan, Martin, and Xie [2021], Schaub

[2018]), and highlight that FDPs' role in capital markets is broader than the mere aggregation and dissemination of analyst-produced information. Our results also contribute to the street earnings literature by directly answering the Lambert [2004] call for accounting researchers to delineate the role of FDPs in the production of street earnings. Finally, our results contribute to prior literature examining institutional features of the I/B/E/S database. Our examination of the 2009 I/B/E/S methodology change extends prior work documenting material changes in the I/B/E/S database over time (e.g., Call et al. [2021]), and helps explain the differences between I/B/E/S actuals and analyst-inferred actuals documented by Brown and Larocque [2013]. Accordingly, our findings are relevant to both market participants and researchers who rely on FDP-produced data. Given the growing importance of FDP-produced information in capital markets (e.g., Hand et al. [2021]), and the vast discretion FDPs have in their methodology choices, our findings should also be relevant to regulators tasked with capital market oversight.

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APPENDIX

| MORGAN STANLEY RESEARCH | | | | | | MORGAN STANLEY & CO. LLC | | | | | |
|---|-------|--------|---------------------|--------|--|---|-------|--------|-------------------|--------|--|
| AT&T, Inc. (T.N, T US) | | | | | | Ford Motor Company (F,N, F US) | | | | | |
| Telecom Services / United States of America | | | | | | Autos & Shared Mobility / United States of America | | | | | |
| Stock Rating | | | Equal-weight | | | Stock Rating | | | Overweight | | |
| Industry View | | | Cautious | | | Industry View | | | Cautious | | |
| Price target | | | \$34.00 | | | Price target | | | \$15.00 | | |
| Shr price, close (Jan 27, 2015) | | | \$32.81 | | | Shr price, close (Mar 13, 2018) | | | \$10.78 | | |
| Mkt cap, curr (mm) | | | \$170,676 | | | Mkt cap, curr (mm) | | | \$43,177 | | |
| 52-Week Range | | | \$37.45-31.74 | | | 52-Week Range | | | \$13.33-10.14 | | |
| Fiscal Year Ending | 12/14 | 12/15e | 12/16e | 12/17e | | Fiscal Year Ending | 12/17 | 12/18e | 12/19e | 12/20e | |
| EPS (\$) ** | 2.51 | 2.44 | 2.33 | 2.21 | | EPS (\$) ** | 1.90 | 1.44 | 1.26 | 1.33 | |
| ModelWare EPS (\$) | 2.22 | 1.96 | 1.85 | 1.74 | | Prior EPS (\$) ** | - | 1.40 | 1.06 | 1.07 | |
| Prior ModelWare EPS (\$) | 2.22 | 1.94 | 1.91 | 1.98 | | Consensus EPS (\$) § | 1.80 | 1.57 | 1.51 | 1.54 | |
| P/E | 15.1 | 16.7 | 17.7 | 18.9 | | P/E | 6.5 | 7.5 | 8.6 | 8.1 | |
| Consensus EPS (\$) § | 2.51 | 2.55 | 2.64 | 2.87 | | ModelWare EPS (\$) | 1.90 | 1.44 | 1.26 | 1.33 | |
| Div yld (%) | 5.5 | 5.6 | 5.8 | 5.9 | | Unless otherwise noted, all metrics are based on Morgan Stanley ModelWare framework | | | | | |
| Unless otherwise noted, all metrics are based on Morgan Stanley ModelWare framework | | | | | | ** = Based on consensus methodology | | | | | |
| ** = Based on consensus methodology | | | | | | § = Consensus data is provided by Thomson Reuters Estimates | | | | | |
| § = Consensus data is provided by Thomson Reuters Estimates | | | | | | e = Morgan Stanley Research estimates | | | | | |
| e = Morgan Stanley Research estimates | | | | | | | | | | | |

FIG A1.—Analyst report examples. This figure presents two analyst report examples from Morgan Stanley Research citing Thomson Reuters as the source of the consensus EPS actuals and forecasts. The first column in each figure presents the actual street earnings for the current fiscal year, while the remaining columns report the estimates for the next three fiscal years.

**News Release**

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FOR IMMEDIATE RELEASE
October 20, 2016

FIFTH THIRD ANNOUNCES THIRD QUARTER 2016 NET INCOME TO COMMON SHAREHOLDERS OF \$501 MILLION, OR \$0.65 PER DILUTED SHARE

- 3Q16 net income available to common shareholders of \$501 million, or \$0.65 per diluted common share
 - Reported results included the following items which had a net positive \$0.22 impact on reported 3Q16 EPS:
 - A \$280 million pre-tax (~\$182 million after-tax) gain from the termination and settlement of gross cash flows from existing Vantiv tax receivable agreements (TRA) and the expected obligation to terminate and settle the remaining TRA cash flows upon the exercise of put or call options
 - A \$28 million pre-tax (~\$18 million after-tax) non-cash impairment charge related to previously announced plans to sell or consolidate certain bank branches and land acquired for future branch expansion
 - A \$12 million pre-tax (~\$8 million after-tax) charge related to the valuation of the Visa total return swap
 - An \$11 million pre-tax (~\$7 million after-tax) gain on the sale of a non-branch facility
 - A \$9 million pre-tax (~\$6 million after-tax) charge from the transfer of certain nonconforming investments affected by the Volcker Rule to held-for-sale
 - An \$8 million beneficial tax impact in connection with certain commercial lease terminations
- 3Q16 return on average assets (ROA) of 1.44%; return on average common equity of 12.8%; return on average tangible common equity** of 15.2%
- Pre-tax income of \$694 million and pre-provision net revenue (PPNR)** of \$774 million in 3Q16
 - Net interest income (NII) of \$907 million and NII on a fully taxable equivalent (FTE) basis** of \$913 million, up 1 percent from both 2Q16 and 3Q15; net interest margin (on an FTE basis)** of 2.88%, flat sequentially and down 1 bp year-over-year
 - Average portfolio loans and leases of \$93.5 billion, down \$420 million sequentially and up \$138 million from 3Q15; Period end portfolio loans and leases of \$93.2 billion decreased \$758 million sequentially and \$423 million from 3Q15 primarily driven by decreases in automobile, C&I, and home equity loans
 - Noninterest income of \$840 million compared with \$599 million in the prior quarter; primarily driven by the gain from the termination and settlement of the Vantiv tax receivable agreement and other items previously mentioned
 - Noninterest expense of \$973 million was \$10 million, or 1 percent, lower than the prior quarter primarily driven by lower compensation and benefit-related expenses and lower card and processing expense
- Credit trends
 - 3Q16 net charge-offs of \$107 million (0.45% of loans and leases) increased from 2Q16 NCOs of \$87 million (0.37% of loans and leases)
 - Portfolio nonperforming asset (NPA) ratio of 0.73% down 13 bps from 2Q16, nonperforming loan (NPL) ratio of 0.63% down 11 bps from 2Q16; total NPAs of \$783 million, including loans held-for-sale (HFS), decreased \$42 million sequentially
 - 3Q16 provision expense of \$80 million; \$91 million in 2Q16 and \$156 million in 3Q15
- Strong capital ratios*
 - Common equity Tier 1 (CET1) ratio 10.16%; fully phased-in CET1 ratio** of 10.08%
 - Tier 1 risk-based capital ratio 11.26%, Total risk-based capital ratio 14.87%, Leverage ratio 9.80%
 - Tangible common equity ratio of 9.24%**; 8.78%** excluding unrealized gains/losses
- 11 million reduction in common shares outstanding during the quarter due to the \$240 million accelerated share repurchase transaction initially settled on August 5, 2016
- Book value per share of \$20.44 up 2% from 2Q16 and up 12% from 3Q15; tangible book value per share** of \$17.22 up 2% from 2Q16 and up 13% from 3Q15

* Capital ratios estimated; presented under current U.S. capital regulations.

** Non-GAAP measure; see discussion of non-GAAP and Reg. G reconciliation beginning on page 33.

FIG A2. —Part A: Example of an earnings press release.

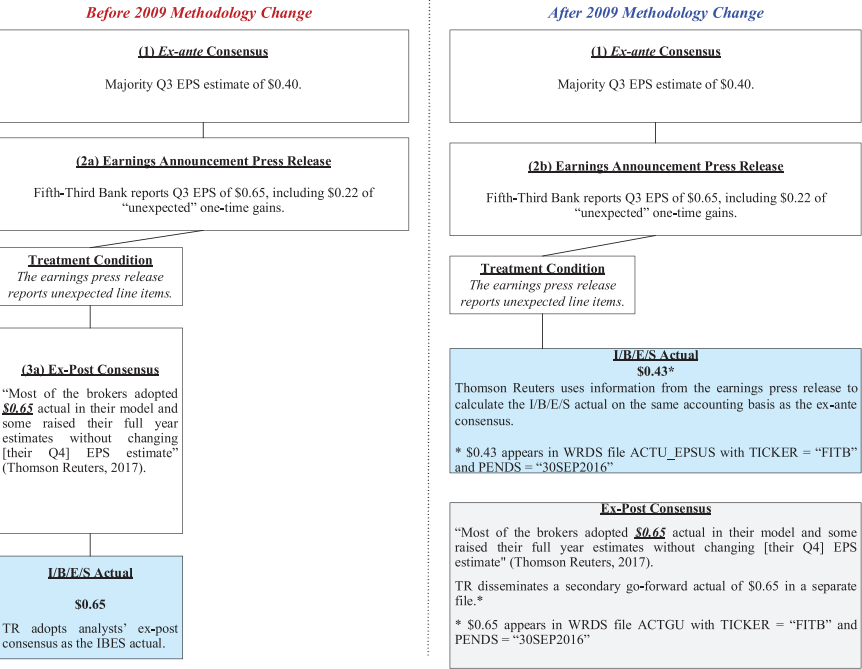


FIG A2.—Part B: Example of Thomson Reuters’ treatment of unexpected line items. Figure A2, part A, provides the first page of Fifth-Third Bank’s (FITB) 2016 Q3 earnings announcement press release. The red box describes \$0.22 of line items that TR indicates are an example of “unexpected line items” in a 2017 white paper (Thomson Reuters [2017]). Part B illustrates the differences in TR’s treatment of these \$0.22 of unexpected line items before and after the 2009 methodology change.

TABLE A1
Variable Definitions and Data Sources

| Variable | Definition |
|---|--|
| Main dependent and independent variables | |
| <i>Ln(Activation Delay)</i> | Natural logarithm of the time (in minutes) from the earnings press release time until TR's street earnings activation time (Source: I/B/E/S, WSH, RavenPack). |
| <i>Post</i> | A dummy variable that equals 1 for earnings press releases announced after September 30, 2009 and 0 otherwise. |
| <i>Treatment</i> | A dummy variable that equals 1 if a company reports any of the following eight unexpected charges/gains in a quarter: a large restructuring charge, a large acquisition expense or gain, net credit or charge to reserves for bad debts from loan recoveries or charge-offs, nonrecurring income taxes, settlement of litigation or insurance, asset write-down, goodwill impairment, and large special items. An item is classified as large if it is in the top decile of the sample distribution of its absolute value (Source: Compustat). |
| <i>Future Street Earnings</i> | The sum of I/B/E/S actual EPS for quarters $t+1$ to $t+4$, scaled by total assets per share. (Source: I/B/E/S, Compustat). |
| <i>Future Cash Flows</i> | The sum of cash flows from operations for quarters $t+1$ to $t+4$, scaled by total assets. (Source: Compustat). |
| <i>Street Earnings</i> | I/B/E/S actual EPS for quarter t , scaled by total assets per share. (Source: I/B/E/S, Compustat). |
| <i>DIFF</i> | A dummy variable that equals 1 if at least one analyst who issues an earnings per share forecast over the five days after the first fiscal quarter's earnings announcement differs by at least one penny with respect to her first fiscal quarter inferred earnings per share actual from the I/B/E/S reported actual earnings per share for the same quarter (Brown and Larocque [2013]). (Source: I/B/E/S) |
| <i>Ln(Forecasting Delay)</i> | Natural logarithm of the time (in trading minutes) from the earnings press release time to the announcement time of the first analyst forecast following the earnings press release. (Source: I/B/E/S, WSH, RavenPack). |
| <i>MAFE</i> | The mean absolute forecast error across all forecasts of next quarter earnings issued within five days after the earnings announcement scaled by the absolute value of actual EPS. Each analyst's absolute forecast error is calculated as the absolute value of their earnings per share forecast minus next quarter's actual earnings per share. Following Loh and Stulz [2018], denominator values smaller than \$0.25 are set to \$0.25 to limit the impact of small denominators. (Source: I/B/E/S, Compustat) |
| <i>Dispersion</i> | The standard deviation of analyst next quarter earnings forecasts issued in the five days following an earnings announcement divided by the absolute value of actual EPS. Following Loh and Stulz [2018], denominator values smaller than \$0.25 are set to \$0.25 to limit the impact of small denominators. (Source: I/B/E/S, Compustat). |

(Continued)

TABLE A1—Continued

| Variable | Definition |
|--------------------------|---|
| $MRT_{[0,16]}$ | Intraday market reaction timeliness measure, calculated as $\frac{RET_{[0,4hr]}}{RET_{[0,16hr]}} + \frac{RET_{[0,8hr]}}{RET_{[0,16hr]}} + \frac{RET_{[0,12hr]}}{RET_{[0,16hr]}} + 0.5$, where $RET_{[0,t]}$ is the buy-and-hold return up to hour t following the earnings press release. Following Li et al. [2015] and Akbas et al. [2018], we use TAQ data to examine trading during both regular and extended trading hours, such that a full trading day covers 16 trading hours (960 trading minutes, from 4:00 a.m. to 8:00 p.m.), and winsorize each intraday return ratio at -1 and 1 to mitigate the effect of extreme outliers. For trading occurring during regular trading hours, we keep only trades that meet all of the following criteria: (1) trades occurring on the NYSE, Amex, or NASDAQ; (2) trades made under regular market conditions (i.e., COND codes ‘’, ‘*’, ‘@’, ‘E’, ‘F’, ‘@F’, ‘6’, ‘@6’, ‘M’, ‘O’); (3) trades without subsequent cancellations; and (4) trades where the transaction price and the number of shares traded were both positive. For trades during extended trading hours, we include trades with COND codes ‘T’, ‘F’, ‘@’, and ‘@F’, which represent the bulk of all extended-hour trades. In terms of EX codes, we exclude “extended-hour trades” in NYSE and Amex as they are likely to represent regular session closing transactions that are reported after 4 p.m. (Source: TAQ) |
| Control variables | |
| $Abs(GAAP-Street)$ | The absolute value of the difference between Compustat earnings per share and I/B/E/S earnings per share, scaled by the absolute value of I/B/E/S earnings per share (Source: I/B/E/S, Compustat) |
| $Abs(Surprise)$ | The absolute value of <i>Surprise</i> , where <i>Surprise</i> is defined as actual earnings per share minus the median analyst earnings per share forecast over the last 90 days scaled by the absolute value of the median forecast (Source: I/B/E/S). |
| <i>Bad News</i> | A dummy variable that equals 1 if <i>Surprise</i> is negative and 0 otherwise (Source: I/B/E/S, CRSP). |
| <i>EPS Guidance</i> | A dummy variable that equals 1 if the firm has issued earnings guidance for quarter t and 0 otherwise (Source: I/B/E/S). |
| <i>QTR4</i> | A dummy variable that equals 1 if the firm is announcing its fourth fiscal quarter earnings and 0 otherwise (Source: Compustat). |
| <i>Reporting Lag</i> | Natural logarithm of the number of days between the fiscal period end and the earnings announcement day (Source: I/B/E/S, Compustat, RavenPack, WSH). |
| <i>Size</i> | Natural logarithm of the average market value of the firm from days $[-25, -20]$ relative to the earnings press release date (Source: CRSP). |
| <i>Firm Age</i> | Natural logarithm of the number of years since the firm’s initial public offering date (Source: CRSP, Compustat). |

(Continued)

TABLE A1—Continued

| Variable | Definition |
|---------------------------------------|---|
| <i>Institutional Ownership</i> | The percentage of shares held by 13F institutions as of the end of the most recent calendar quarter before the earnings announcement (Source: Thomson Reuters CDA/Spectrum Institutional Holdings s34). |
| <i>Analyst Following</i> | Natural logarithm of number of analysts covering the firm over the one-year period ending on the most recent month prior to an earnings announcement (Source: I/B/E/S). |
| <i>S&P 500</i> | A dummy variable that equals 1 if the firm is in the S&P 500 index on the day of the earnings announcement and 0 otherwise (Source: Compustat). |
| <i>Advertising</i> | Natural logarithm of one plus the total advertising expense as of the most recent fiscal year end before the earnings announcement day (Source: Compustat). |
| <i>Unactivated Actuals</i> | Natural logarithm of the number of all unactivated (as of the earnings announcement minute) earnings announcements by other firms over the $[-5, 0]$ calendar day window, where day 0 is the earnings announcement day (Source: I/B/E/S). |
| <i>Friday</i> | A dummy variable that equals 1 if the announcement day is Friday and 0 otherwise (Source: I/B/E/S). |
| <i>Press Release Words</i> | Natural logarithm of the total number of words in the earnings press release filed with the SEC (Source: SEC EDGAR). |
| <i>HardInfoMix</i> | The number of informative numbers (e.g., numbers that are not associated with dates or section headings) in the press release scaled by the total number of words and multiplied by 100 (Source: SEC EDGAR). |
| <i>Non-GAAP Words</i> | Number of non-GAAP trigger words or phrases contained in the earnings press release as in Bentley et al. [2018] (e.g., any variation of words such as “adjust,” “exclude,” “proforma,” “non-GAAP,” “remove,” “without,” “except for,” etc.) scaled by the total number of words in the press release and multiplied by 100 (Source: SEC EDGAR). |
| <i>Media Coverage</i> | Natural logarithm of the number of news stories published about the firm over the one month prior to the earnings announcement (Source: RavenPack). |
| Analyst-firm-quarter variables | |
| <i>Timely Analyst</i> | An indicator equal to 1 if analyst i announces their quarter $t+1$ forecast for firm j after firm j 's quarter t earnings are announced but before TR activates firm j 's quarter t street earnings, and 0 otherwise. (Source: I/B/E/S) |
| <i>General Experience</i> | The number of years for which analyst i issued at least one forecast for any firm as of the date of firm j 's quarter t announcement (Source: I/B/E/S). |
| <i>Firm Experience</i> | The number of years for which analyst i issued at least one forecast for firm j as of the date of firm j 's quarter t announcement (Source: I/B/E/S). |
| <i>Allstar</i> | An indicator equal to 1 if analyst i was classified as an All-Star analyst in the year they issued their forecast, and 0 otherwise. (Source: <i>Institutional Investor Magazine</i>). |

(Continued)

TABLE A1—Continued

| Variable | Definition |
|-----------------------|--|
| <i>Accuracy</i> | The mean absolute forecast error (scaled by actual earnings per share) across all of analyst i 's one-quarter-ahead earnings forecasts issued over the one-year period leading up to analyst i 's forecast, multiplied by minus one (Source: I/B/E/S). |
| <i>Employer Size</i> | The number of analysts employed by analyst i 's broker over the one-year period leading up to analyst i 's forecast (Source: I/B/E/S). |
| <i>Concurrent EAs</i> | The number of firms covered by analyst i that announce earnings within five days of analyst i 's forecast (Source: I/B/E/S). |
| $DIFF_{i,j,t}$ | An indicator that equals 1 if analyst i 's inferred EPS actual for firm j for quarter t differs from the I/B/E/S reported actual for the same firm-quarter by at least one penny (Brown and Larocque [2013]). |
| $AFE_{i,j,t}$ | The absolute forecast error for analyst i 's quarter $t+1$ forecast for firm j scaled by the absolute value of firm j 's actual EPS for quarter $t+1$. |