# When AI Distorts Investor Interpretation: Evidence from Forced-Balance Summaries

**Ji Ho Kim**
*Harvard Business School*

**Botir Kobilov**
*University of Texas at Dallas*

**Terrence Tianshuo Shi**
*UCLA Anderson School of Management*

**Yuan Zou**[*]
*Harvard Business School and AI Institute*

May 2026

**Abstract**

We examine the capital market effects of AI-generated summaries that impose forced-balance framing on financial information. Exploiting the introduction of Seeking Alpha's Virtual Analyst Reports (VARs), which present exactly three positives and three concerns for each firm regardless of the directional tone of underlying Seeking Alpha article coverage, we find that treated firms experience significantly higher abnormal trading volume around earnings announcements. Decomposing order flow, we show that retail investors shift toward against-the-news trading, while non-retail investors trade more in line with the direction of the earnings surprise, consistent with the forced-balance structure distorting the representation of earnings news and affecting investor types in different ways. Treated firms also exhibit wider bid-ask spreads, suggesting that greater heterogeneity in interpretation weakens market liquidity. These effects are concentrated among firms with weaker information environments and in settings where pre-earnings Seeking Alpha coverage is strongly one-sided, precisely when forced counterarguments contrast most sharply with the prevailing signal. Further evidence shows that treated firms experience slower price discovery. Overall, our findings show that the market consequences of AI depend not only on whether information is summarized, but also on how the summary is designed.

**JEL Classification:** G14, M41, O33

**Keywords:** AI summaries, forced-balance framing, trading volume, retail investors, price discovery, Seeking Alpha, social media

---

[*] Corresponding author contact information: Harvard Business School, Morgan Hall 361, Boston, MA 02163; Phone: (617) 495-3840; email: yzou@hbs.edu. This paper has benefited from comments and suggestions received from Jonas Heese, Darren Henderson (discussant), Allen Huang (discussant), Valeri Nikolaev, Tatiana Sandino, Charles Wang, Benjamin Yost, Ronghuo Zheng, and workshop participants at Annual Telfer Conference in Accounting, Auditing and Accountability, Harvard Business School, and HKU Summer Accounting Conference. We gratefully acknowledge financial support from Harvard Business School, University of Texas at Dallas, and UCLA Anderson School of Management.

## 1. Introduction

Technological advances have introduced new forms of information intermediation in capital markets, ranging from automated earnings articles to recent large language model (LLM) tools that summarize complex financial information into concise investor-facing outputs (e.g., Blankespoor et al. 2018; Bradshaw et al. 2026). By lowering investors' information-processing costs, these AI-enabled summary tools can broaden access to investment-relevant information and may be especially useful for investors who lack the time, expertise, or resources to process lengthy disclosures and analyst commentary on their own (e.g., Bertomeu et al. 2025; Wong et al. 2025; Chang et al. 2026; Cho et al. 2026). At the same time, growing reliance on AI tools has raised concerns that investors may place excessive weight on AI-generated summaries rather than independently evaluating the underlying information (Gensler 2023; Financial Stability Board 2024), leaving them vulnerable to being misled when these summaries do not faithfully represent the underlying information environment. Such misrepresentation can arise for several reasons, including limitations in the input data, algorithmic errors or hallucinations, and human design choices over how information is selected, organized, and displayed. While prior research has primarily examined how AI tools improve information processing, less attention has been paid to their potential risks, particularly when AI-generated summaries alter the framing of the underlying information. This paper addresses that gap by providing early evidence that AI-generated summaries can distort investors' interpretation of public information when their design changes how the underlying information is presented.

We study the potential distortion introduced by AI-generated summaries in the setting of Virtual Analyst Reports (VARs) on Seeking Alpha, the largest crowdsourced investment research platform (e.g., Farrell et al. 2022; Gomez et al. 2024; Gu et al. 2025). VARs reorganize existing

articles on the platform into a standardized format that presents exactly three "Positives Raised by Analysts" and three "Concerns Raised by Analysts" for each covered firm, thereby embedding a strong human design choice into how information is summarized and displayed, regardless of whether this balanced presentation reflects the underlying information environment.

We examine the capital market implications of this forced-balance structure around earnings announcements. Earnings windows provide a useful setting because earnings news helps resolve uncertainty about firm performance and often provides a relatively clear directional signal to the market. This directional signal can stand in sharp contrast to the VAR design, which mechanically presents both positive and negative points regardless of the underlying tone of the available Seeking Alpha coverage. As a result, VARs may distort investors' interpretation of earnings news by giving equal visibility to counterarguments that are not necessarily representative of the underlying information environment.

Appendix A provides an example VAR for Amazon.com (AMZN) in our sample preceding its 2025Q2 earnings announcement. The overall tone of the underlying coverage is highly positive: among the 84 articles published in the prior three months, more than 70% carry an explicitly positive tone about the firm, consistent with the positive earnings surprise reported on July 31, 2025. Nevertheless, the VAR displays the standard format of three positives and three concerns. The positive points align closely with themes that contributors emphasize most heavily, including AI strategy, robotics, and the strong growth of AWS, each of which is discussed and supported by a large share of articles on the platform. By contrast, two of the three concerns — high valuation and AWS lagging Azure — appear in 5% or fewer of the articles. Even when they are mentioned, the discussion is often brief and provides little detailed context to support the concern.[1] Yet under

[1] For example, the main support for the "AWS lagging Azure" concern appears to come from a seemingly unsupported sentence in one article on the platform: "Note that their largest competitor, Microsoft Azure, saw 33% growth in their

the VAR format, these concerns receive the same weight and visibility as the more widely shared positive views, despite their relatively anecdotal nature.

Our empirical design exploits cross-sectional variation in VAR coverage around its 2024Q4 introduction in a difference-in-differences framework. Our final sample consists of 37,095 firm-quarter earnings announcements for 3,955 unique firms spanning 2023 to 2025. We focus on earnings announcement windows because they are the setting in which investors update their beliefs most actively, and the directional implications for stock prices are typically clear.

We begin by examining how forced-balance VAR summaries affect market participation. On the one hand, VARs provide investors with a low-cost summary of the Seeking Alpha coverage available before an earnings announcement. Prior research shows that investors face meaningful costs in monitoring, acquiring, and analyzing public disclosures, and that presentation format can shape investor reactions when attention and processing capacity are limited (Hirshleifer and Teoh 2003; Blankespoor et al. 2020). Because VARs are easier to process than the underlying Seeking Alpha coverage, some investors may rely on them in place of more fully processing the broader information environment. For these investors, the counterarguments mechanically highlighted in the summary may receive greater weight than they otherwise would, making the earnings news appear more equivocal than its underlying directional signal would imply. To the extent that others continue to rely on that broader environment, the same public news is interpreted less uniformly across investors. Prior theory suggests that such differential interpretation can increase trading volume by generating more opposite-sided trade (Harris and Raviv 1993; Kandel and Pearson 1995; Verrecchia 2001). On the other hand, VARs may reduce market participation if the forced-

latest earnings report and so AWS seems to be losing ground." Although this statement stands in clear contrast to the broadly positive discussion of AWS's strong growth in the surrounding coverage, it nevertheless becomes the third concern in Seeking Alpha's fixed format because negative views are relatively scarce during that period.

balance format makes directional earnings news harder to interpret. By giving visible weight to counterarguments even when the broader information environment is relatively one-sided, the summary may weaken the clarity of the signal and increase investors' uncertainty about how to respond. Rather than trading immediately, some investors may refrain from trading, reducing participation around the announcement window. In addition, VARs may have little effect if investors recognize that the forced-balance format merely repackages existing Seeking Alpha content and therefore discount the mechanically highlighted counterarguments. Ultimately, the effect of VARs on market participation is an empirical question.

Using abnormal trading volume (turnover) around earnings announcements as a proxy for market participation, we find that firms with an available VAR (i.e., the treatment group) exhibit a significant increase in abnormal turnover, compared to firms without a VAR (i.e., the control group). Economically, VAR-covered firms exhibit an additional 0.532 percentage-point increase in abnormal turnover relative to non-VAR firms, which corresponds to 12.6% of the standard deviation.

Next, we examine what drives the increase in aggregate trading volume and whether the effect differs across investor types. Retail investors are likely to be more affected by VARs because they face greater attention and information-processing constraints and are more likely to rely on concise, salient summaries. Prior research shows that salient attributes receive disproportionate weight, especially when individual investors are more important, and that synthesized, easy-to-access information can stimulate retail trading by reducing processing costs (Lawrence 2013; Blankespoor et al. 2018; Cosemans and Frehen 2021). In our setting, this suggests that retail investors may place greater weight on the counterarguments highlighted in VARs and become more likely to trade against the earnings signal. More sophisticated investors, by contrast, may

also benefit from the low-cost summary because it reduces the cost of processing the broader information environment around the announcement. Because they are less likely to overweight the mechanically highlighted counterarguments, they may be less susceptible to the distortions introduced by the forced-balance format and instead trade more in line with fundamentals, especially when retail order flow creates opportunities to do so.

To test our predictions, we decompose trading activity into retail and non-retail components following Boehmer et al. (2021) and Barber et al. (2024). We find that both retail and non-retail trading volume increase following VAR coverage, but more importantly, the two investor types trade in opposite directions, suggesting that VARs affect different investor types in systematically different ways. Retail order imbalance shifts toward against-the-news trading — net selling following positive surprises and net buying following negative surprises — while non-retail order imbalance moves in the direction implied by the earnings signal. This mirror-image pattern is consistent with forced-balance summaries leading retail investors to place greater weight on mechanically highlighted counterarguments, thereby introducing noise into the market's processing of earnings news and creating opportunities for more sophisticated investors to trade in the direction of fundamentals.[2]

We next examine how forced-balance AI summaries affect market liquidity around earnings announcements. The opposing order flows between retail and non-retail investors suggest that VARs increase heterogeneity in how investors interpret earnings news. By leading some investors to place greater weight on mechanically highlighted counterarguments, VARs may amplify disagreement and make observed order flow less informative about fundamentals. This

[2] The retail against-news pattern is consistent with a growing body of evidence documenting contrarian tendencies among retail traders around earnings announcements (Glover 2025; Luo et al. 2025), attention-driven rather than informed retail trading (Michels 2025), and retail sensitivity to salient summary disclosures (Darendeli 2025).

greater uncertainty about the informational content of trades increases adverse selection risk for uninformed market participants. Consistent with models in which disagreement and information asymmetry impair liquidity (Harris and Raviv 1993; Kim and Verrecchia 1994; Kandel and Pearson 1995), we expect VAR coverage to widen bid-ask spreads. Consistent with this prediction, we find that VAR firms experience a significant increase in abnormal bid-ask spreads compared to non-VAR firms, following the VAR rollout. Taken together, the joint increase in volume and spreads, alongside the opposing retail and non-retail order flows, is consistent with forced-balance summaries fragmenting the market's processing of earnings news, as relatively unsophisticated retail investors place greater weight on the counterarguments highlighted in the VAR format, thereby decreasing market liquidity.

We conduct two sets of cross-sectional tests to further evaluate the forced-balance mechanism. The first examines whether the effects of VAR coverage are stronger when the forced counterarguments in the VAR stand in sharper contrast to the prevailing pre-earnings signal. We capture this setting using *Consensus Tone*, an indicator for firm-quarters in which pre-earnings Seeking Alpha coverage is strongly one-sided and the earnings surprise is material. In these cases, the surrounding information environment sends a relatively clear directional signal, so the VAR's balanced presentation makes opposing arguments unusually salient relative to the prevailing view. Consistent with this interpretation, we find that the effects on both abnormal turnover and abnormal spreads are significantly stronger when the pre-earnings signal is more directional, indicating that the counterarguments embedded in the VAR generate greater interpretive disagreement precisely when they most contradict the directional consensus.

The second cross-sectional test examines whether the effects are stronger in weaker information environments, where investors are more likely to rely on AI-generated summaries as

a primary source for evaluating earnings news. We proxy for the richness of the information environment using sell-side analyst coverage and firm size. For both proxies, the effects on abnormal turnover and abnormal spreads are significantly attenuated among firms with greater analyst coverage and among larger firms, consistent with forced-balance framing exerting less influence on belief formation when alternative sources of information are more abundant.

Finally, we examine whether forced-balance summaries affect the speed with which earnings news is incorporated into prices, a proxy for the price efficiency of covered firms. Using an adjusted intraperiod timeliness (IPT) measure over a 20-day post-announcement window (e.g., Drake et al. 2017), we find that VAR firms experience a significant decrease in the speed of price discovery following earnings announcements. This evidence complements our earlier results on trading volume, order flow, and spreads: VARs appear to distort the representation of information during the earnings window, lead relatively unsophisticated retail investors to trade against the direction of earnings news, and ultimately impair market efficiency by slowing price discovery.

Our study contributes to the literature in two ways. First, we contribute to the growing literature on AI-generated financial content and the broader debate over whether AI democratizes investing by lowering information-processing costs (Blankespoor et al. 2018; Cheng et al. 2025; Bradshaw et al. 2026). Recent research largely emphasizes the benefits of AI summaries in helping investors process complex information more efficiently (Wong et al. 2025; Cho et al. 2026). We extend this literature by showing that low-cost AI summaries need not simply facilitate information processing. When investors rely on an AI summary in place of more fully processing the underlying information environment, the summary can alter how public information is interpreted. In our setting, this distortion does not arise from factual inaccuracy or new information production, but from the way the summary represents existing information. More broadly, our evidence

suggests that AI tools may not uniformly democratize investing: to the extent that some investors rely more heavily on low-cost summaries than others, AI can widen, rather than narrow, differences in how sophisticated and unsophisticated investors process public information if the design of those summaries biases the representation of the underlying information environment, with corresponding implications for trading and market quality.

Second, we contribute to the nascent literature on retail investor trading behavior and disagreement around public news (Boehmer et al. 2021; Guest 2021; Glover 2025; Luo et al. 2025). Recent literature shows that retail investors often trade in a contrarian way around earnings announcements and that such behavior can slow the incorporation of information into prices. We identify a new channel through which AI-generated content can amplify these effects. Forced-balance summaries make directional earnings news appear more equivocal, inducing retail investors to trade against the earnings signal while non-retail investors trade more aggressively in the direction of fundamentals. The resulting divergence in trading behavior increases disagreement across investor groups, widens bid-ask spreads, and slows price discovery.

Lastly, our findings have practical implications for AI tools in financial markets. We do not interpret the results as implying that AI-generated summaries are unambiguously harmful, as such tools may reduce search costs and broaden access to financial information (Wong et al. 2025; Cho et al. 2026). Rather, we identify a more specific risk: when investors rely on low-cost AI summaries, the way those summaries are structured can shape how public news is interpreted, suggesting that platforms should exercise caution when designing AI-powered features.

## 2. Background, Research Design, and Sample

### *2.1. Seeking Alpha and Virtual Analyst Reports*

Seeking Alpha is the largest crowdsourced investment research platform in the United States, with approximately 20 million monthly users as of 2024 (Farrell et al. 2022). The platform publishes stock-specific articles written by independent contributors, including retail investors, financial advisors, and semi-professional analysts. Contributors publish under either pseudonyms or their own names and are compensated partly based on readership.

Seeking Alpha occupies an intermediate position between traditional sell-side research and unmoderated social media. Its contributors are not subject to the compliance review or institutional oversight that governs sell-side analysts, but the platform employs an in-house editorial team that screens submissions for minimum quality, factual accuracy, and disclosure requirements before publication. Prior research shows that Seeking Alpha is an important information intermediary for retail investors: Farrell et al. (2022) find that its articles improve the informativeness of retail order flow, and Gomez et al. (2024) show that Seeking Alpha coverage reduces bid-ask spread spikes around earnings announcements by helping less informed investors narrow the information gap.

In the fourth quarter of 2024, Seeking Alpha introduced Virtual Analyst Reports, AI-generated summaries that accompany individual stock pages. VARs use a large language model to process firm-specific content available on Seeking Alpha and reorganize it into a standardized five-part format: Company Description, Competitors and Unique Value, Rating Explanation, Positives Raised by Analysts, and Concerns Raised by Analysts. Our analysis focuses on the final two sections, which are the direct outputs of generative AI to summarize the underlying contributor articles into exactly three positives and three concerns for each firm.

This fixed format creates a forced-balance structure. Regardless of whether the underlying coverage is predominantly bullish or bearish, each VAR presents the same number of positive and negative points. Thus, a firm with uniformly positive Seeking Alpha coverage still receives three listed concerns, while a firm with uniformly negative coverage still receives three listed positives. This design is consistent with Seeking Alpha's broader positioning as a forum for competing viewpoints, but it may also make minority or weakly supported counterarguments unusually salient when the underlying information environment is strongly one-sided.

### *2.2. Related Literature*

#### *2.2.1. AI-Generated Financial Content and Investor Processing*

Our study first relates to the growing literature on AI-generated financial content and investor information processing. This literature generally shows that AI can lower processing costs and help investors incorporate complex information into trading decisions. Blankespoor et al. (2018) exploit the staggered rollout of Associated Press algorithm-written earnings articles and find that robo-journalism increases trading volume and liquidity, with effects concentrated among retail traders. Cardinaels et al. (2019) show experimentally that algorithmic summaries of earnings releases are less positively biased than management-produced summaries and lead to more conservative investor valuations. Bradshaw et al. (2026) study generative AI adoption on Seeking Alpha and document that AI-generated articles are individually less informative than human-written articles, while AI-enabled coverage expansion improves overall market liquidity. Cho et al. (2026) find that The Wall Street Journal's AI-generated article summaries increase trading volume and amplify price responses, consistent with summaries reducing processing costs and serving as interpretive cues. Wong et al. (2025) similarly show that investor-facing AI tools improve retail investors' processing of disclosure content.

A related set of studies examines how access to generative AI affects investor behavior more broadly. Cheng et al. (2025) use ChatGPT outages as a natural experiment and document significant declines in trading volume during outages, particularly for firms with concurrent news releases. Bertomeu et al. (2025) exploit Italy's sudden ChatGPT ban and find that losing access to AI widens bid-ask spreads and reduces analyst forecast accuracy, especially for firms with low institutional ownership. Ecker et al. (2026) show that stock-related queries on a Chinese generative AI platform increase around corporate disclosure events, consistent with AI substituting for traditional information search. Chang et al. (2026) document that retail traders' alignment with AI-derived earnings-call sentiment increases sharply after ChatGPT's release.

Taken together, this literature shows that AI-generated financial content can lower information-processing costs and meaningfully affect investor trading. While prior studies largely emphasize the benefits of AI in helping investors process financial information, we examine a potential downside: when investors rely on low-cost AI summaries, the way those summaries represent the underlying information can distort interpretation and lead investors to trade differently on the same public news.

*2.2.2. Framing and Presentation of Financial Information*

Our study also relates to research showing that the framing and presentation of financial information affects investor behavior even when the underlying content is unchanged. Tetlock (2007) shows that the linguistic content of financial media predicts aggregate market activity. Guest (2021) finds that Wall Street Journal articles whose editorial tone contradicts a firm's earnings narrative increase trading volume and slow price discovery, whereas corroborating articles accelerate price discovery. Fedyk (2024) shows that front-page news positioning affects market outcomes independent of the information conveyed, and Goldman et al. (2024) find that

politically divergent coverage of the same firm-level news generates trading volume through investor disagreement. We extend this literature by studying framing that arises from a mechanical AI design choice rather than from editorial discretion, media placement, or political ideology. In our setting, the forced-balance template gives equal visibility to positive and negative arguments regardless of the directional tone of the underlying coverage. This allows us to study how the structure of an AI-generated summary affects the interpretation of existing public information.

*2.2.3. Retail Reliance, Salience, and Earnings Trading*

Our paper also builds on research on retail attention, salience, and trading around earnings announcements. Retail investors face high costs of acquiring and processing primary disclosures, and presentation format can shape their reactions when attention and processing capacity are limited (Hirshleifer and Teoh 2003; Blankespoor et al. 2020). Under rational inattention, investors may substitute a low-cost summary for the more costly primary signal rather than process both (Sims 2003). At the same time, salient and easily retrieved information receives disproportionate weight, especially among less sophisticated investors and firms with greater individual ownership (Tversky and Kahneman 1974; Barber and Odean 2008; Lawrence 2013; Cosemans and Frehen 2021).

This channel is especially relevant around earnings announcements, where retail order flow is often attention-driven and contrarian. Boehmer et al. (2021) provide the trade classification method we use to identify retail activity; Glover (2025) and Luo et al. (2025) document retail contrarian trading around earnings; Michels (2025) shows that such trading is attention-driven rather than informed; and Darendeli (2025) shows that salient summary disclosures can induce retail overreaction.

*2.3. Research Design*

We investigate whether VAR coverage affects trading activity, the composition of trading across investor types, bid-ask spreads, and the speed of price discovery around earnings announcements. We use a difference-in-differences research design, comparing changes in outcomes around earnings announcements for Seeking Alpha firms that have a VAR summary page after VAR rollout (treated) with contemporaneous changes for Seeking Alpha firms that do not (control). The platform-wide nature of the rollout, combined with cross-sectional variation in coverage, allows us to separate VAR effects from contemporaneous market-wide shocks. We estimate the following difference-in-differences specification at the firm-earnings-announcement level:

$$Y_{i,t} = \beta(Treated_i \times Post_t) + \gamma' X_{i,t} + \alpha_i + \delta_t + \varepsilon_{i,t}. \qquad (1)$$

In Equation (1), *i* and *t* index the firm and the earnings announcement dates, respectively. *Y* represents our outcome variables: *Abnormal Turnover*, *Abnormal Retail/Non-Retail Volume*, *Against-News OIB*, *Abnormal Spread*, and *Adjusted IPT*. *Treated* is an indicator equal to one if the firm's Seeking Alpha page features a VAR, and zero otherwise. *Post* is an indicator equal to one for post-treatment quarters (2025Q1 onward) and zero otherwise. *X* is a vector of six control variables: *Size*, *BTM*, *ROA*, *Leverage*, *Analyst Coverage*, and *Return Volatility*. We include firm ($\alpha_i$) and year-month ($\delta_t$) fixed effects. Standard errors are clustered by firm.[3]

*2.4. Sample, Variables, and Descriptive Statistics*

Our sample consists of firm-earnings-announcement-level observations for Seeking Alpha-covered firms with earnings announcements between 2023 and 2025. We identify treated firms by

[3] Year-month fixed effects absorb within-quarter time-series variation that year-quarter fixed effects cannot. Year-quarter fixed effects yield qualitatively similar results (untabulated).

scraping the Seeking Alpha page in July 2025, seven months after the rollout.[4] Firms whose page displays a VAR at that time are coded as treated, and firms covered by Seeking Alpha contributors but without a VAR are coded as control. Seeking Alpha assigns VARs to "all equity stocks with Seeking Alpha Quant ratings, excluding ETFs and Funds/REITs".[5] We combine Seeking Alpha treatment indicators with data from four sources: firm fundamentals from Compustat, daily returns and trading data from CRSP, earnings announcement dates and analyst consensus forecasts from I/B/E/S, and bid-ask spreads from the WRDS Intraday Indicators (IID) database (Holden and Jacobsen 2014), which are constructed from NYSE TAQ tick data. We classify retail and non-retail trades using the BJZZ algorithm (Boehmer et al. 2021) applied to TAQ. For each earnings announcement, we expand the firm-quarter observation to trading days [-41, +20] around the announcement date.

Our sample period has seven pre-treatment quarters (2023Q1 through 2024Q3) and four post-treatment quarters (2025Q1 through 2025Q4). We exclude 2024Q4 because it is the quarter when VARs were rolled out on Seeking Alpha. Although VARs were widely announced in December 2024, the platform shows an example VAR dating back to late October 2024. Because we cannot precisely pinpoint when VAR coverage would have started affecting investor activity, we conservatively exclude the entire quarter to avoid misclassifying any firm-quarter as pre- or post-treatment. Table 1 details the sample construction. We begin with 44,715 firm-quarter observations for Seeking Alpha-covered firms with a valid CRSP-Compustat link and I/B/E/S analyst coverage during 2023Q1 through 2025Q4 (4,378 unique firms). We require I/B/E/S

[4] Because treatment is measured from a single July 2025 snapshot, firms that gained VAR coverage between December 2024 and July 2025 are classified as treated for all post-quarters regardless of when their VAR first appeared, which biases against finding an effect in the early post-period.

[5] https://web.archive.org/web/20260115021509/https://help.seekingalpha.com/what-type-of-securities/stocks-have-virtual-analyst-report

coverage to ensure the availability of analyst forecast data for computing earnings surprises and analyst coverage controls. After excluding observations with missing dependent variables or controls, the treatment quarter (2024Q4), penny stocks, and singleton firms, our final sample consists of 37,095 firm-quarter observations across 3,955 unique firms.

[Insert Table 1 about here]

We measure announcement-window outcome variables across four dimensions that capture how investors and markets process the earnings news: aggregate trading volume, volume and order imbalance by investor type, bid-ask spreads, and the speed of price discovery. First, we measure abnormal trading volume (*Abnormal Turnover*) as the firm's market-adjusted daily share turnover over [0, +2] relative to the earnings announcement, minus the equivalent over [-41, -11], multiplied by 100 (Blankespoor et al. 2018). The market adjustment subtracts the equal-weighted daily turnover across all sample firms.

Second, we decompose trading activity by investor type following Boehmer et al. (2021), as refined by Barber et al. (2024). Retail brokers, such as Charles Schwab, route their customer orders to wholesale market makers, such as Citadel, who execute them off-exchange at sub-penny prices to satisfy best-execution requirements under Regulation NMS. Because U.S. stocks trade in whole-cent increments, wholesalers' price improvements show up in the trade record as prices with a non-zero third decimal. Such "sub-penny" prices effectively occur only in off-exchange retail executions, making them a reliable fingerprint for retail trades. Accordingly, we classify a trade as retail if it is executed off-exchange, at a sub-penny price (excluding trades with sub-penny amounts between \$0.004 and \$0.006, which are more likely institutional), and with a trade value under \$100,000. We sign retail trades using the sub-penny digit: trades with a sub-penny amount below \$0.004 (closer to the bid) are classified as sells and those above \$0.006 (closer to the ask)

as buys. Trades near the half-penny are excluded because they cannot be reliably signed. All remaining trades are classified as non-retail and signed using the Lee and Ready (1991) quote midpoint method, where trades above the midpoint are buys and those below are sells.

We construct two sets of measures for retail and non-retail traders. For volume, we compute abnormal retail and non-retail volume (*Abnormal Retail Volume* and *Abnormal Non-Retail Volume*) as the average daily trade count over [0, +2] minus the average daily trade count over [-41, -11], measured in thousands of trades. For order flow direction, we compute against-news order imbalance (*Against-News OIB*) as (buys - sells) / (buys + sells), pooling trades across [0, +2] before forming the ratio. We subtract the benchmark OIB over [-41, -11] and sign the measure so that positive values indicate trading opposite to the direction of the earnings surprise. Specifically, for positive surprises (based on the I/B/E/S median forecast), we multiply the abnormal OIB by negative one, and for negative surprises, we leave it unchanged. As a result, a higher value of *Against-News OIB* indicates stronger net trading against the direction implied by the earnings surprise.

Third, we measure abnormal spreads (*Abnormal Spread*) using the percent effective spread from the WRDS Intraday Indicators database (Holden and Jacobsen 2014). For each trade $k$ on day $T$, the percent effective spread is computed as $2D_k(P_k - M_k)/M_k$, where $P_k$ is the execution price, $M_k$ is the bid-ask midpoint ($(B_k + A_k)/2$), and $D_k$ equals +1 for buys and -1 for sells. The daily percent effective spread is the simple average across all $N$ trades on that day: $ESpreadPct_AVG_{i,T} = \frac{1}{N}\sum_{k=1}^{n} Percent\ Effective\ Sprea\ d_k$ . We aggregate to the event window by weighting each trading day by its number of trades over [0, +2] and subtract the trade-count-weighted benchmark over [-41, -11].

Fourth, we measure the speed of price discovery using the adjusted intraperiod timeliness measure (*Adjusted IPT*), which accounts for price overreactions and reversals. Following Blankespoor et al. (2018), we compute $IPT_{Adj} = \sum_{t=0}^{T} \left(1 - \frac{|CumAR_T - CumAR_t|}{|CumAR_T|}\right)$, where $CumAR_t$ is the cumulative abnormal return from day 0 to day $t$ using the value-weighted market return as benchmark. Higher values indicate faster price discovery. We measure this over a [0, +20] window, consistent with the post-announcement horizon used in Drake et al. (2017), and exclude observations with absolute cumulative abnormal returns below 2% to reduce measurement noise from near-zero denominators (Blankespoor et al. 2018).

Our specification includes six firm-level control variables commonly used in earnings-announcement studies (e.g., Blankespoor et al. 2018): firm size (*Size*), book-to-market (*BTM*), return on assets (*ROA*), leverage (*Leverage*), analyst coverage (*Analyst Coverage*), and return volatility (*Return Volatility*). Size equals the natural logarithm of one plus lagged total assets; *BTM* equals book equity scaled by lagged market equity; *ROA* equals income before extraordinary items scaled by lagged total assets; *Leverage* equals total liabilities scaled by lagged total assets; *Analyst Coverage* equals the number of I/B/E/S analysts issuing a forecast; and *Return Volatility* equals the standard deviation of daily returns over [-41, -11]. We winsorize all continuous variables at the 1st and 99th percentiles. All variables are defined in Appendix B.

Table 2 reports descriptive statistics for the final sample. Our sample characteristics are broadly comparable to those of Blankespoor et al. (2018), who studied the capital market effects of AP newswire articles. Our *Abnormal Turnover* (mean 0.524) and *Abnormal Spread* (mean 0.030) are of comparable magnitude to theirs (0.37 and 0.05, respectively). Our mean *Analyst Coverage* (7.2) is higher than theirs (3.5), reflecting the SA Quant Rating requirement for sell-side analyst coverage, which also means our sample firms are, on average, larger. Our abnormal

turnover and spread measures are constructed following their methodology, facilitating comparison of economic magnitudes in Section 3.

[Insert Table 2 about here]

## 3. Main Results

### *3.1. Abnormal Turnover*

Our main analysis compares the change in abnormal trading volume around earnings announcements for Seeking Alpha-covered firms with a VAR summary page (treated firms) to that for firms without (control firms), following the 2024Q4 rollout. We estimate Equation (1) with *Abnormal Turnover*, measured over the three-day window [0, +2] relative to the earnings announcement date, as the dependent variable, and we report the results in Table 3. In column (1), we report estimates without firm-level controls; in column (2), we include the full set of six control variables. All specifications include firm and year-month fixed effects, and we cluster standard errors by firm.

[Insert Table 3 about here]

The coefficient on the interaction term, *Treated* × *Post,* is positive and statistically significant at the 1% level in both columns (coef. = 0.448; t-stat. = 2.62 in Column (1); coef. = 0.532; t-stat. = 3.32 in Column (2)). These results suggest that, following the rollout of VARs, firms experience an increase in abnormal trading volume around earnings announcements. The effect is economically meaningful: the coefficient in column (2) corresponds to 12.6% of a standard deviation and 26.6% of the mean absolute abnormal turnover, comparable to the 9.9% of a standard deviation that Blankespoor et al. (2018) document for the Associated Press rollout of algorithm-written earnings articles.[6]

[6] We self-calculate economic magnitude for Blankespoor et al. (2018) by dividing their *Post* coefficient on abnormal volume (0.102) in Table 3 by the standard deviation (1.03) in Table 1.

The validity of our difference-in-differences design rests on the assumption that treated and control firms would have followed parallel trends in the absence of VAR rollout. We assess this by estimating a dynamic specification that replaces the single *Post* indicator with quarterly indicators, normalizing 2024Q3 (*t-1*) as the baseline. Figure 1 plots the coefficients. None of the pre-treatment estimates is statistically significant, and the point estimates show no discernible trend, supporting the parallel trends assumption. The treatment effect emerges gradually in the post-period: it is small and insignificant in *t+1*, suggesting that the effects of VARs take time to materialize as investors gradually adopt the new summaries, and then becomes positive and significant, with the magnitude continuing to rise over time.

[Insert Figure 1 about here]

*3.2. Retail and Non-Retail Trading Volume and Order Imbalance*

To shed light on the mechanism behind the aggregate volume response, we compare the change in retail and non-retail trading volume around earnings announcements for treated firms relative to control firms, following the 2024Q4 rollout. If retail investors are more influenced by the salient counterarguments highlighted in VARs than more sophisticated investors, then the increase in aggregate trading volume should reflect divergent trading responses across investor types rather than a uniform shift in beliefs. We estimate Equation (1) separately for *Abnormal Retail Volume* and *Abnormal Non-Retail Volume* and report the results in Table 4, Panel A.

[Insert Table 4 about here]

Table 4, Panel A, reports the results for trading volume by investor type. The coefficient on the variable of interest, *Treated* × *Post*, is positive and statistically significant at the 1% level for both investor types (coef. = 0.375; t-stat. = 9.19 for retail, and coef. = 1.062; t-stat. = 6.24 for non-retail), suggesting that both types of investors trade more around earnings announcements

following the VAR rollout. In economic terms, VAR coverage increases retail volume by 17.3% of a standard deviation (35.2% of the mean absolute level) and non-retail volume by 8.4% of a standard deviation (14.5% of the mean absolute level).

In Table 4, Panel B, we examine whether the increased trading reflects heterogeneous interpretation across investor types. The dependent variable is *Against-News OIB*, signed so that positive values indicate net buying following negative earnings news and net selling following positive earnings news. In column (1), the coefficient on *Treated* × *Post* is positive and statistically significant at the 5% level for retail order imbalance (coef. = 0.012; t-stat. = 2.03), indicating that retail investors in VAR-covered firms shift toward against-the-news trading in the post-VAR period. In column (2), the coefficient is negative and statistically significant at the 5% level for non-retail order imbalance (coef. = -0.011; t-stat. = -2.17), indicating that non-retail investors shift in the opposite direction, toward with-the-news trading.

This mirror-image pattern is important for two reasons. First, it indicates that VARs generate heterogeneity in the interpretation of public information across investor types rather than uniformly shifting beliefs, helping explain the increase in abnormal turnover documented earlier. Second, the pattern is consistent with some retail investors placing greater weight on the mechanically highlighted counterarguments in the forced-balance summary and therefore trading against the earnings signal, while non-retail investors place less weight on those counterarguments and trade more in line with fundamentals. The opposite-signed non-retail response suggests that the retail flow reflects reliance on the summary rather than informed trading on fundamentals, with more sophisticated investors taking the other side.

*3.3. Bid-Ask Spreads*

We next examine whether forced-balance AI summaries affect market liquidity around earnings announcements. The opposing retail and non-retail order flows documented in Section 3.2 suggest that VARs increase heterogeneity in how investors interpret earnings news. By leading some investors to place greater weight on mechanically highlighted counterarguments while others trade on the underlying earnings signal, VARs may make observed order flow less informative about fundamentals. This greater uncertainty about the informational content of trades increases adverse selection risk for liquidity providers. Consistent with models in which disagreement and information asymmetry impair liquidity (Harris and Raviv 1993; Kim and Verrecchia 1994; Kandel and Pearson 1995), we expect VAR coverage to widen bid-ask spreads around earnings announcements. To test this prediction, we estimate Equation (1) with *Abnormal Spread,* computed from the WRDS Intraday Indicators database over the [0, +2] window, as the dependent variable, and we report the results in Table 5.

[Insert Table 5 about here]

Table 5 reports the results from estimating Equation (1) with *Abnormal Spread* as the dependent variable and shows that coefficient on the variable of interest, *Treated* × *Post,* is positive and statistically significant at the 5% level in both columns (coef. = 0.021; t-stat. = 2.38 in Column (1); coef. = 0.019; t-stat. = 2.16 in Column (2)). These results suggest that, following the rollout of VARs, bid-ask spreads widen around earnings announcements for VAR-covered firms. The effect corresponds to 9.1% of a standard deviation and 22.6% of the mean absolute abnormal spread.

Taken together, the results in Tables 3 through 5 present a consistent pattern. VAR coverage increases abnormal turnover, shifts retail order flow toward against-the-news trading

while non-retail order flow moves with the earnings signal, and widens bid-ask spreads. This joint evidence is consistent with forced-balance summaries fragmenting the market's processing of earnings news: when some investors rely on the VAR and overweight mechanically highlighted counterarguments, order flow becomes less informative about fundamentals, increasing adverse selection risk and reducing market liquidity.

## 4. Additional Analyses

### *4.1. Cross-Sectional Heterogeneity*

If the volume and spread effects documented in Section 3 reflect interpretive disagreement induced by forced-balance framing, they should be amplified where the forced counterarguments clash most sharply with the prevailing signal and where investors have fewer alternative information sources to Seeking Alpha. We test the cross-sectional predictions by comparing the change in *Abnormal Turnover* and *Abnormal Spread* around earnings announcements for treated firms relative to control firms, following VAR rollout, across subsamples that differ in the salience of the forced-balance structure or the strength of the information environment. To examine how the effect varies across firms, we interact *Treated* × *Post* with firm characteristics that capture the salience of the forced-balance structure (*Consensus Tone*) and the strength of the information environment (*Analyst Coverage* and *Size*). Table 6 reports the results.

[Insert Table 6 about here]

Panel A examines whether the treatment effect varies with the degree to which the VAR's forced-balance framing distorts a one-sided pre-earnings announcement sentiment environment. We capture this using *Consensus Tone* — an indicator for firm-quarters in which pre-earnings Seeking Alpha coverage is strongly one-sided and the earnings surprise is material. Specifically, it equals one when: (i) all pre-earnings Seeking Alpha articles point in the same tone direction,

either positive or negative; and (ii) the signal is economically meaningful, as reflected by both article-tone measures and the earnings surprise being at or above the first tercile. To be conservative, we measure article tone using both FinBERT (Huang et al. 2023) and the Loughran-McDonald dictionary (Loughran and McDonald 2011). These are the cases where the mismatch between the VAR's balanced template and the one-sided pre-earnings view is sharpest and where the earnings surprise is large enough for the resulting disagreement among investors to become visible in turnover and spreads.

Consistent with this logic, the triple-interaction coefficient on abnormal turnover is positive and statistically significant at the 5% level (coef. = 0.866, t-stat. = 2.01), and the coefficient on abnormal spreads is positive and significant at the 10% level (coef. = 0.023, t-stat. = 1.71). The volume and spread effects are stronger when pre-earnings sentiment is unanimous and the earnings surprise is meaningful. The result supports the interpretive-disagreement mechanism: when the forced counterarguments stand in sharper contrast to the prevailing signal, investors who read the VAR are exposed to prominently displayed concerns (or positives) that directly oppose the directional information environment, creating greater scope for divergent interpretations of the same earnings news.

Panel B examines whether the effects vary with the strength of the information environment, where investors with fewer alternative sources are more likely to rely on AI-generated summaries when evaluating earnings news. We interact *Treated* × *Post* with two proxies for information environment richness: sell-side analyst coverage and firm size. The triple-interaction coefficients on analyst coverage are negative for both volume (coef. = -0.121; t-stat. = -2.31) and spreads (coef. = -0.006; t-stat. = -2.38). The triple-interaction coefficients on firm size are also negative for both volume (coef. = -0.309; t-stat. = -2.82) and spreads (coef. = -0.011; t-

stat. = -1.84). These results indicate that the volume and spread effects are significantly attenuated among firms with greater analyst coverage and among larger firms, consistent with forced-balance framing exerting less influence on belief formation when alternative sources of earnings information are more abundant.

*4.2. Price Efficiency*

The volume, order imbalance, and spread results in Section 3 are consistent with investors relying on the forced-balance summary rather than the primary signal. When part of the order flow pushes against fundamentals because some investors anchor on the balanced digest rather than the directional earnings news, the signal is impounded into prices more slowly (Hirshleifer and Teoh 2003).

To test the price-discovery prediction, we compare the change in the speed of intraperiod price discovery around earnings announcements for treated firms relative to control firms, following VAR rollout. We estimate Equation (1) with *Adjusted IPT*, measured over the [0, +20] trading-day window following the earnings announcement, as the dependent variable, and we report the results in Table 7.

[Insert Table 7 about here]

The coefficient on the interaction term *Treated* × *Post* is negative and statistically significant at the 1% level in both columns (coef. = -1.078; t-stat. = -2.66 in Column (1); coef. = -1.129; t-stat. = -2.80 in Column (2)). These results suggest that, following the rollout of VARs, the speed of intraperiod price discovery around earnings announcements decreases for VAR-covered firms. The effect corresponds to 9.5% of a standard deviation and 9.3% of the mean absolute *Adjusted IPT*, consistent with disagreement-driven trading requiring additional rounds of trade to resolve heterogeneous beliefs across investor types. Overall, the price-discovery result

complements the volume, order-imbalance, and spread findings documented in Section 3: VAR coverage not only increases trading activity and widens spreads but also delays the process by which prices incorporate earnings news. The joint pattern is consistent with reliance on the forced-balance summary degrading the processing of earnings news rather than facilitating consensus.

*4.3. Robustness Tests*

In this section, we address three concerns that might otherwise complicate the interpretation of our main results: that the treatment effect reflects a contemporaneous macroeconomic shock rather than the VAR rollout, that our estimates capture pre-existing differential trends between treated and control firms, and that our inferences depend on specific sample or weighting choices.

First, our sample period coincides with significant U.S. tariff policy changes in early 2025 that disproportionately affected certain industries (The Budget Lab at Yale 2025). A concern is that VAR-covered and non-covered firms differ systematically in industry composition, so that the treatment effect could in part reflect differential industry exposure to the tariff shock rather than the forced-balance framing itself. To address this concern, we re-estimate Equation (1) after excluding firms in Primary Metal (SIC 33) and Transportation Equipment (SIC 37), the two industries most affected by the tariffs, and we report the results in Table 8. The coefficient on *Treated* × *Post* remains positive and statistically significant at the 1% level for *Abnormal Turnover* (coef. = 0.511; t-stat. = 3.19) and at the 5% level for *Abnormal Spread* (coef. = 0.018; t-stat. = 2.04). Our inferences do not change when we exclude tariff-affected industries, reducing the concern that the main results reflect industry-level exposure to concurrent trade-policy shocks.

[Insert Table 8 about here]

Second, the validity of our difference-in-differences design relies on the assumption that treated and control firms would have followed parallel trends in the absence of the VAR rollout. Although the dynamic evidence in Figure 1 is consistent with this assumption for trading volume, a residual concern is that our estimates could pick up pre-existing differential trends between treated and control firms that happen to coincide with the 2024Q4 rollout. To assess this concern, we conduct a placebo test that assigns a counterfactual treatment date one year earlier, to 2023Q4, and restricts the sample to the pre-VAR period (2022Q1 through 2024Q4). Table 9 reports the results and shows that the coefficient on *Treated* × *Placebo Post* is statistically insignificant for both *Abnormal Turnover* (coef. = 0.077; t-stat. = 0.79) and *Abnormal Spread* (coef. = 0.013; t-stat. = 1.45). The absence of a placebo effect suggests that our estimates do not reflect pre-existing differential trends between treated and control firms and further supports the parallel-trends assumption underlying our research design.

[Insert Table 9 about here]

Finally, a remaining concern is that our results could depend on specific sample or weighting choices that, individually or jointly, might drive the estimated effect. We therefore assess sensitivity to three alternative specifications. In columns (1) and (2) of Table 10, we reweight control firms using entropy balancing on pre-period covariate means to address selection on observables (Hainmueller 2012); in columns (3) and (4), we drop mega-cap firms (size decile 10) to assess whether the largest firms drive the estimates; and in columns (5) and (6), we remove the share-price filter to include penny stocks.[7] The coefficient on *Treated* × *Post* for *Abnormal*

[7] We first estimate a logit model predicting VAR coverage using pre-period means of our six control variables and compute propensity scores. Following Crump et al. (2009), we trim firms with estimated propensity scores outside the [0.10, 0.90] range. These are firms that are almost certainly treated or almost certainly control based on observables alone, so the treatment-control comparison is unreliable. We then apply entropy balancing (Hainmueller 2012) on the trimmed sample to address selection on observables, reweighting control firms so that their pre-period covariate means match those of treated firms. To limit the influence of any single control firm, we cap individual entropy-balancing weights at two.

*Turnover* is positive and statistically significant at the 1% level across all three specifications, ranging from 0.409 (t-stat. = 2.69) under entropy balancing to 0.829 (t-stat. = 5.10) without the share-price filter. The coefficient for *Abnormal Spread* is positive and statistically significant at the 5% level across all three specifications, ranging from 0.017 (t-stat. = 1.99) under entropy balancing to 0.022 (t-stat. = 2.40) without the share-price filter. These results suggest that our main inferences are not sensitive to differences between treated and control firms along observable dimensions and are not driven by the specific sample filters we impose.

[Insert Table 10 about here]

Overall, the results in Tables 8 through 10 suggest that our main findings are robust to concerns that the treatment coincides with industry-specific tariff shocks, that treated and control firms follow pre-existing differential trends, and that our estimates depend on specific sample or weighting choices.

## 5. Conclusion

In this paper, we examine how AI-generated financial summaries that impose a forced-balance structure affect investor behavior around earnings announcements. Exploiting the 2024Q4 rollout of Seeking Alpha's Virtual Analyst Reports, which mechanically present three positives and three concerns regardless of the underlying analyst consensus, we find that VAR coverage increases abnormal trading volume, shifts retail order flow toward against-the-news trading while non-retail flow moves in the opposite direction, widens bid-ask spreads, and slows intraperiod price discovery around earnings announcements.

Taken together, our findings suggest that when investors rely on a low-cost AI summary rather than more fully processing the broader information environment, the structure of that summary can shape market outcomes even though the summary introduces no new fundamental

information. More broadly, the results imply that AI-enabled summaries may not uniformly democratize investing: to the extent that some investors rely more heavily on low-cost summaries than others, AI can widen, rather than narrow, differences in how sophisticated and unsophisticated investors process public information when the design of those summaries biases the representation of the underlying information environment. Our study also has practical implications: when investors rely on low-cost AI summaries, the way those summaries are structured can shape how public news is interpreted, suggesting that platforms should exercise caution when designing AI-powered features.

## References


Barber, B. M., X. Huang, P. Jorion, T. Odean, and C. Schwarz. 2024. A (sub)penny for your thoughts: Tracking retail investor activity in TAQ. Journal of Finance 79 (4): 2403–2427.

Barber, B. M., and T. Odean. 2008. All that glitters: The effect of attention and news on the buying behavior of individual and institutional investors. Review of Financial Studies 21 (2): 785–818.

Bertomeu, J., Y. Lin, Y. Liu, and Z. Ni. 2025. The impact of generative AI on information processing: Evidence from the ban of ChatGPT in Italy. Journal of Accounting and Economics 80 (1): 101782.

Blankespoor, E., E. deHaan, and I. Marinovic. 2020. Disclosure processing costs, investors' information choice, and equity market outcomes: A review. Journal of Accounting and Economics 70 (2–3): 101344.

Blankespoor, E., E. deHaan, and C. Zhu. 2018. Capital market effects of media synthesis and dissemination: Evidence from robo-journalism. Review of Accounting Studies 23 (1): 1–36.

Boehmer, E., C. M. Jones, X. Zhang, and X. Zhang. 2021. Tracking retail investor activity. Journal of Finance 76 (5): 2249–2305.

Bradshaw, M. T., C. Ma, B. P. Yost, and Y. Zou. 2026. Generative AI use by capital market information intermediaries: Evidence from Seeking Alpha. Journal of Accounting Research, forthcoming.

Cardinaels, E., S. Hollander, and B. J. White. 2019. Automatic summarization of earnings releases: Attributes and effects on investors' judgments. Review of Accounting Studies 24 (3): 860–890.

Chang, A., X. Dong, X. Martin, and C. Zhou. 2026. AI democratization and trading inequality. Journal of Accounting Research 64 (3): 1287–1331.

Cheng, Q., P. Lin, and Y. Zhao. 2025. Does generative AI facilitate investor trading? Early evidence from ChatGPT outages. Journal of Accounting and Economics 80 (2): 101773.

Cho, T., A. H. Huang, J. Pacelli, and K. Rennekamp. 2026. Generative AI and investor processing of financial media. Working paper, Hong Kong University of Science and Technology.

Cosemans, M., and R. Frehen. 2021. Salience theory and stock prices: Empirical evidence. Journal of Financial Economics 140 (2): 460–483.

Crump, R. K., V. J. Hotz, G. W. Imbens, and O. A. Mitnick. 2009. Dealing with limited overlap in estimation of average treatment effects. Biometrika 96 (1): 187–199.

Darendeli, A. 2025. How do retail investors respond to summary disclosure? Evidence from mutual fund factsheets. Review of Accounting Studies 30 (2): 1222–1266.

Drake, M. S., J. R. Thornock, and B. J. Twedt. 2017. The internet as an information intermediary. Review of Accounting Studies 22 (2): 543–576.

Ecker, F., X. Li, Y. Li, and F. Wu. 2026. How stock market participants use generative artificial intelligence: Evidence from user-platform interaction data. Journal of Accounting Research, forthcoming.

Farrell, M., T. C. Green, R. Jame, and S. Markov. 2022. The democratization of investment research and the informativeness of retail investor trading. Journal of Financial Economics 145 (2): 616–641.

Fedyk, A. 2024. Front-page news: The effect of news positioning on financial markets. Journal of Finance 79 (1): 5–33.

Financial Stability Board. 2024. The financial stability implications of artificial intelligence. Report, Financial Stability Board, Basel.

Gensler, G. 2023. Remarks before the Financial Stability Oversight Council: 2023 annual report. Speech, U.S. Securities and Exchange Commission.

Glover, A. 2025. Heterogeneous retail investors and their trading behavior. Working paper, University of Washington.

Goldman, E., N. Gupta, and R. D. Israelsen. 2024. Political polarization in financial news. Journal of Financial Economics 155: 103816.

Gomez, E., F. Heflin, J. Moon, and J. Warren. 2024. Financial analysis on social media and disclosure processing costs: Evidence from Seeking Alpha. The Accounting Review 99 (5): 223–246.

Gu, P., B. P. Yost, and Y. Zou. 2025. Firm visibility and acquisition likelihood: Evidence from Seeking Alpha coverage. Working paper, Boston College.

Guest, N. M. 2021. The information role of the media in earnings news. Journal of Accounting Research 59 (3): 1021–1076.

Hainmueller, J. 2012. Entropy balancing for causal effects. Political Analysis 20 (1): 25–46.

Harris, M., and A. Raviv. 1993. Differences of opinion make a horse race. Review of Financial Studies 6 (3): 473–506.

Hirshleifer, D., and S. H. Teoh. 2003. Limited attention, information disclosure, and financial reporting. Journal of Accounting and Economics 36 (1–3): 337–386.

Holden, C. W., and S. Jacobsen. 2014. Liquidity measurement problems in fast, competitive markets. Journal of Finance 69 (4): 1747–1785.

Huang, A. H., H. Wang, and Y. Yang. 2023. FinBERT: A large language model for extracting information from financial text. Contemporary Accounting Research 40 (2): 806–841.

Kandel, E., and N. D. Pearson. 1995. Differential interpretation of public signals and trade in speculative markets. Journal of Political Economy 103 (4): 831–872.

Kim, O., and R. E. Verrecchia. 1994. Market liquidity and volume around earnings announcements. Journal of Accounting and Economics 17 (1–2): 41–67.

Lawrence, A. 2013. Individual investors and financial disclosure. Journal of Accounting and Economics 56 (1): 130–147.

Lee, C. M. C., and M. J. Ready. 1991. Inferring trade direction from intraday data. Journal of Finance 46 (2): 733–746.

Loughran, T., and B. McDonald. 2011. When is a liability not a liability? Textual analysis, dictionaries, and 10-Ks. Journal of Finance 66 (1): 35–65.

Luo, P., E. Ravina, M. C. Sammon, and L. M. Viceira. 2025. Retail investors' contrarian behavior around news, attention, and the momentum effect. NBER Working Paper No. 34086.

Michels, J. 2025. Retail investor trade and the pricing of earnings. Review of Accounting Studies 30: 575–610.

Sims, C. A. 2003. Implications of rational inattention. Journal of Monetary Economics 50 (3): 665–690.

Tetlock, P. C. 2007. Giving content to investor sentiment: The role of media in the stock market. Journal of Finance 62 (3): 1139–1168.

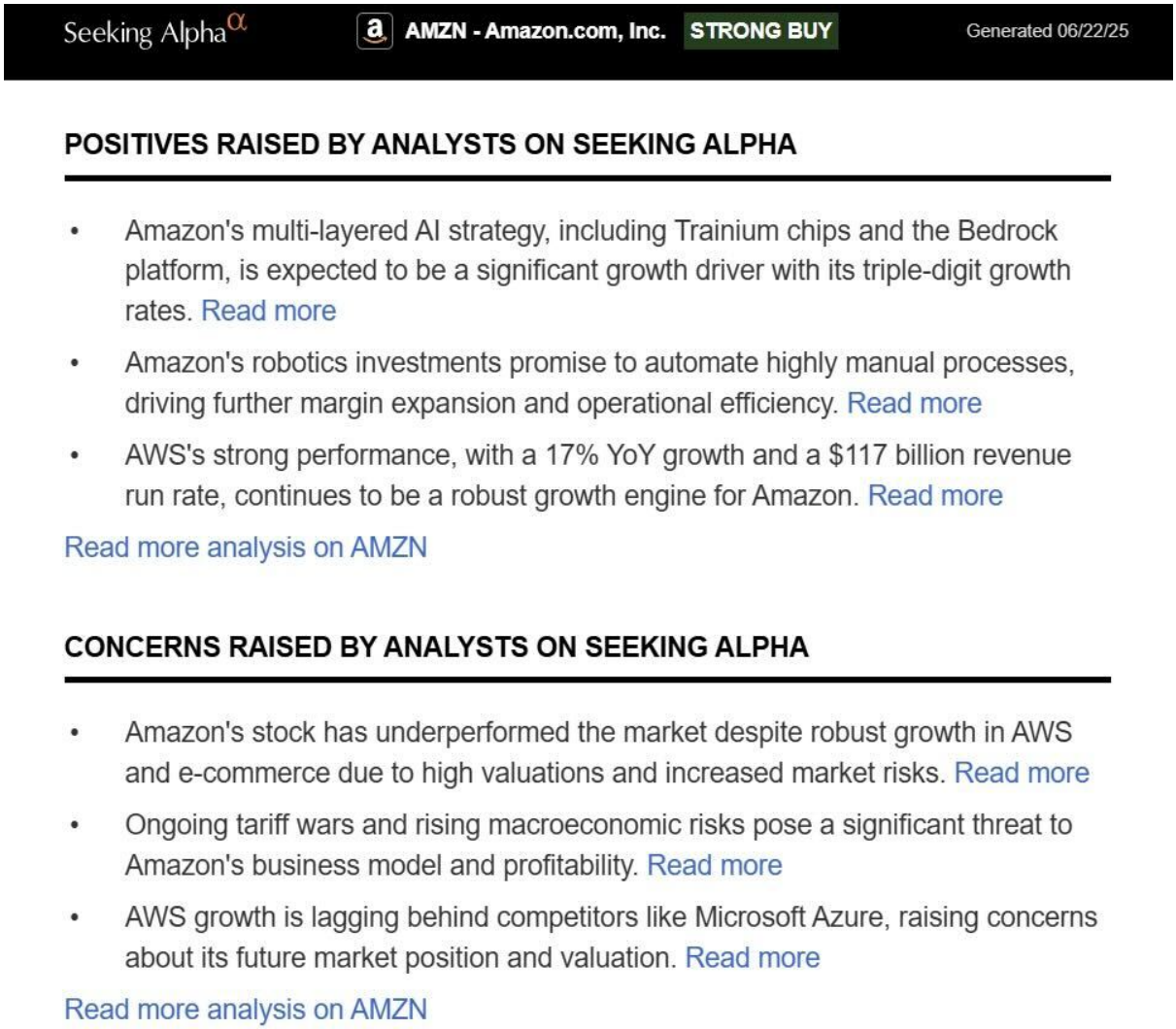
Tversky, A., and D. Kahneman. 1974. Judgment under uncertainty: Heuristics and biases. Science 185 (4157): 1124–1131.

The Budget Lab at Yale. 2025. Where we stand: The fiscal, economic, and distributional effects of all US tariffs enacted in 2025 through April 2. Report, The Budget Lab, Yale University.

Verrecchia, R. E. 2001. Essays on disclosure. Journal of Accounting and Economics 32 (1–3): 97–180.

Wong, T. J., Y. Yi, G. Yu, S. Zhang, and T. Zhang. 2025. Enhancing corporate communication with AI-summarized disclosures. Working paper, University of Southern California.

**Appendix A. Seeking Alpha's Virtual Analyst Report Example**




Based on 84 Seeking Alpha articles on Amazon published three months preceding the VAR date.

| Positives/Concerns Raised by Analysts on Seeking Alpha | Share of Articles with Discussion |
|---|---|
| Positive 1: Amazon's AI strategy (Trainium, Bedrock) | 45% |
| Positive 2: Robotics investments / margin expansion | 46% |
| Positive 3: AWS strong performance / growth | 31% |
| Concern 1: High valuations / stock underperformance | 5% |
| Concern 2: Tariffs / macroeconomic risk | 64% |
| Concern 3: AWS lagging Microsoft Azure | 4% |

## Appendix B. Variable Definitions

Dependent Variables:

| Variable | Description | Source |
|---|---|---|
| *Abnormal Turnover* | The firm's market-adjusted daily share turnover over [0, +2] minus the equivalent over [-41, -11], multiplied by 100. Market adjustment subtracts the equal-weighted daily turnover across firms in the sample. | CRSP |
| *Abnormal Retail Volume* | Average daily retail trade count (buys + sells) over [0, +2] minus the average daily count over [-41, -11], measured in thousands of trades. A trade is classified as retail if it executes off-exchange at a sub-penny price with a trade value under $100,000. Retail trades are signed by the sub-penny digit, where < 0.004 is a sell and > 0.006 is a buy. | TAQ, Boehmer et al. (2021), Barber et al. (2024) |
| *Abnormal Non-Retail Volume* | Constructed identically to the retail measure using non-retail trades (all trades not classified as retail above, signed using the Lee-Ready (1991) midpoint method where trades above the midpoint are buys and those below are sells), measured in thousands of trades. | TAQ, Boehmer et al. (2021), Barber et al. (2024), Lee-Ready (1991) |
| *Against-News OIB (Retail)* | Abnormal retail order imbalance signed to capture against-news trading. Retail order imbalance is (buys - sells) / (buys + sells), with trade counts pooled over [0, +2]. Benchmark order imbalance pooled over [-41, -11]. Abnormal OIB equals event OIB minus benchmark OIB, multiplied by -1 for positive earnings surprises so that positive values indicate trading against the direction of the earnings news. Earnings surprise is computed as actual quarterly EPS minus the median analyst forecast, scaled by quarter-end stock price. | I/B/E/S, TAQ, Boehmer et al. (2021), Barber et al. (2024) |
| *Against-News OIB (Non-Retail)* | Constructed identically to the retail measure using non-retail trades. | I/B/E/S, TAQ, Boehmer et al. (2021), Barber et al. (2024) |
| *Abnormal Spread* | Trade-count-weighted average of the daily percent effective spread over [0, +2], minus the equivalent over [-41, -11], multiplied by 100. The percent effective spread for trade $k$ on day $T$ is $2D_k(P_k - M_k)/M_k$, where $P_k$ is the execution price, $M_k$ is the bid-ask midpoint, and $D_k$ equals +1 for buys and -1 for sells. The daily percent effective spread is the simple average across market-hour trades. | WRDS IID (TAQ) |
| *Adjusted IPT [0,20]* | Adjusted intraperiod timeliness measure of the speed with which earnings information is impounded into price. Adjusted IPT = $\Sigma$(t=0 to 20) [1 - $\lvert CumAR_{20} - CumAR_t \rvert$ / $\lvert CumAR_{20} \rvert$], where $CumAR_t$ is the cumulative abnormal return from day 0 to day t using the value-weighted market return as benchmark. We exclude observations with $\lvert CumAR_{20} \rvert$ < 2% to reduce measurement noise from near-zero denominators. | CRSP |

Independent Variables:

| Variable | Description | Source |
|---|---|---|
| *Treated* | Indicator equal to one if the firm has a Virtual Analyst Report (VAR) on Seeking Alpha. Determined from Seeking Alpha firm-level data as of July 2025. | Seeking Alpha |

| | | |
|---|---|---|
| *Post* | Indicator equal to one for earnings announcements in 2025Q1 or later (post-treatment quarters). The treatment quarter (2024Q4) is excluded from the sample. | I/B/E/S, Compustat |
| *Consensus Tone* | Indicator equal to one if (i) every Seeking Alpha article in the three months prior to the earnings announcement is classified in the same direction under both FinBERT and Loughran-McDonald, (ii) the absolute mean FinBERT and Loughran-McDonald tones are both at or above the 33rd percentile within calendar quarter, and (iii) the absolute earnings surprise is at or above the 33rd percentile of same-signed surprises. Defined only for firm-quarters with at least one Seeking Alpha article in the prior three months. | Seeking Alpha, I/B/E/S |

Control Variables:

| **Variable** | **Description** | **Source** |
|---|---|---|
| *Size* | Natural log of one plus lagged total assets. Also used as a cross-sectional variable. | Compustat |
| *BTM* | Book equity divided by lagged market value of equity. | Compustat |
| *ROA* | Income before extraordinary items divided by lagged total assets. | Compustat |
| *Leverage* | Total liabilities divided by lagged total assets. | Compustat |
| *Analyst Coverage* | Number of analysts issuing earnings forecasts for the firm-quarter. Also used as a cross-sectional variable. | I/B/E/S |
| *Return Volatility* | Standard deviation of daily stock returns over trading days [-41, -11] relative to the earnings announcement. | CRSP |

**Figure 1. Dynamic Analysis**

This figure plots the difference-in-differences coefficients and 90% confidence intervals for abnormal turnover. For brevity, coefficients are plotted at the quarterly level; the regression specification uses firm and year-month fixed effects. Q–1 (2024Q3) is the omitted baseline. Q0 (2024Q4) is excluded due to partial treatment exposure. Full controls are included. Standard errors are clustered by firm.

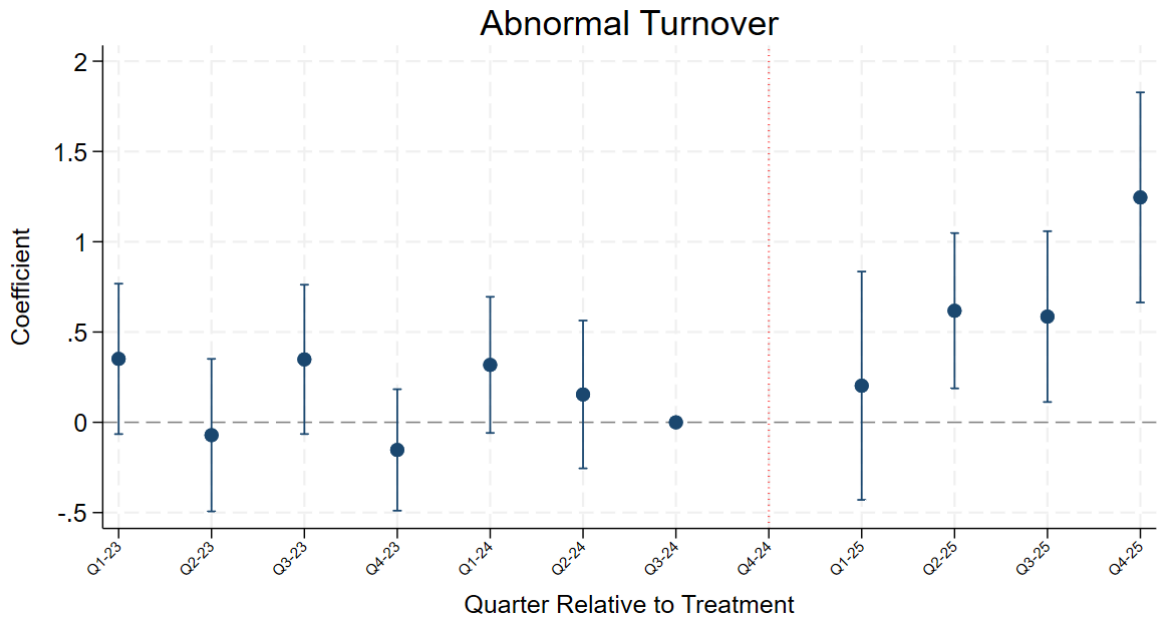


**Table 1. Sample Selection**

This table reports the sample construction process. The sample begins with all Seeking Alpha-covered firms in Compustat with a valid CRSP link and I/B/E/S analyst coverage during 2023Q1 through 2025Q4. The treatment quarter (2024Q4) is excluded due to partial treatment exposure. We exclude penny stocks.

| | Firm-Quarters | Unique Firms |
|---|---|---|
| SA-covered firms in Compustat with CRSP link and I/B/E/S coverage, 2023Q1–2025Q4 | 44,715 | 4,378 |
| (Less: missing dependent variables or controls) | (2,357) | (264) |
| (Less: treatment quarter 2024Q4) | (3,709) | (9) |
| (Less: stock price ≤ $1) | (1,451) | (47) |
| (Less: singleton fixed effects) | (103) | (103) |
| **Final regression sample** | **37,095** | **3,955** |

**Table 2. Summary Statistics**

This table presents summary statistics for the main regression sample. All variables are defined in Appendix B. All continuous variables are winsorized at the 1st and 99th percentiles. Abnormal retail and non-retail volume are measured in thousands of trades.

| Variable | N | Mean | SD | P25 | Median | P75 |
|---|---|---|---|---|---|---|
| *Abnormal Turnover* | 37,095 | 0.524 | 4.206 | -0.362 | 0.381 | 1.634 |
| *Abnormal Spread* | 37,095 | 0.030 | 0.205 | 0.001 | 0.015 | 0.041 |
| *Abnormal Retail Volume* | 37,043 | 0.993 | 2.175 | 0.016 | 0.234 | 0.981 |
| *Abnormal Non-Retail Volume* | 37,043 | 7.006 | 12.664 | 0.259 | 2.487 | 7.974 |
| *Against-News OIB (Retail)* | 37,043 | -0.006 | 0.161 | -0.089 | -0.005 | 0.078 |
| *Against-News OIB (Non-Retail)* | 37,043 | -0.003 | 0.131 | -0.068 | -0.002 | 0.064 |
| *Adjusted IPT [0,20]* | 31,724 | 8.405 | 11.863 | 6.015 | 11.672 | 15.071 |
| *Size* | 37,095 | 7.640 | 2.166 | 6.204 | 7.726 | 9.069 |
| *BTM* | 37,095 | 0.597 | 0.663 | 0.210 | 0.468 | 0.869 |
| *ROA* | 37,095 | -0.019 | 0.089 | -0.016 | 0.003 | 0.015 |
| *Leverage* | 37,095 | 0.598 | 0.329 | 0.372 | 0.578 | 0.799 |
| *Analyst Coverage* | 37,095 | 7.246 | 5.886 | 3.000 | 6.000 | 10.000 |
| *Return Volatility* | 37,095 | 0.029 | 0.021 | 0.016 | 0.023 | 0.035 |
| *Treated* | 37,095 | 0.838 | 0.368 | 1.000 | 1.000 | 1.000 |
| *Post* | 37,095 | 0.365 | 0.481 | 0.000 | 0.000 | 1.000 |

**Table 3. VAR and Abnormal Turnover**

This table presents the results from difference-in-differences regressions examining the effect of Virtual Analyst Reports on abnormal turnover around earnings announcements. The sample consists of firm-quarter observations from 2023Q1 to 2025Q4, excluding 2024Q4. The dependent variable is abnormal turnover over [0, +2], scaled by 100. Column (1) includes firm and year-month fixed effects. Column (2) adds controls. All variables are defined in Appendix B. The t-statistics are reported in parentheses, and standard errors are clustered by firm. *, **, and *** indicate statistical significance at the 0.10, 0.05, and 0.01 levels, respectively. Pr. Sign denotes the predicted sign of the coefficient. The coefficient of 0.532 represents 12.6% of a standard deviation and 26.6% of the mean absolute abnormal turnover.

| Dep. Var. = | | *Abnormal Turnover* | |
|---|---|---|---|
| | Pr. Sign | (1) | (2) |
| *Treated × Post* | + | 0.448*** | 0.532*** |
| | | (2.62) | (3.32) |
| *Size* | | | 0.079 |
| | | | (0.44) |
| *BTM* | | | 0.105 |
| | | | (0.95) |
| *ROA* | | | 2.861*** |
| | | | (3.63) |
| *Leverage* | | | -0.044 |
| | | | (-0.16) |
| *Analyst Coverage* | | | 0.077*** |
| | | | (4.67) |
| *Return Volatility* | | | -63.683*** |
| | | | (-15.01) |
| Firm FE | | Yes | Yes |
| Year-Month FE | | Yes | Yes |
| N | | 37,095 | 37,095 |
| Adj. $R^2$ | | 0.251 | 0.294 |

## Table 4. Retail and Non-Retail Decomposition

This table presents the results from difference-in-differences regressions decomposing the trading response to VAR coverage by investor type. The sample is restricted to observations with non-missing BJZZ trade classification data (N = 37,043). Panel A examines abnormal trading volume, measured in thousands of trades. Panel B examines against-news order imbalance, signed so that positive values indicate trading opposite to the direction of the earnings surprise. In both panels, column (1) reports results for retail trades and column (2) for non-retail trades. All variables are defined in Appendix B. All specifications include firm and year-month fixed effects and the full set of controls. The t-statistics are reported in parentheses, and standard errors are clustered by firm. *, **, and *** indicate statistical significance at the 0.10, 0.05, and 0.01 levels, respectively. In Panel A, the retail coefficient represents 17.3% of a standard deviation (35.2% of mean absolute abnormal retail volume), and the non-retail coefficient represents 8.4% of a standard deviation (14.5% of mean absolute level). In Panel B, the retail OIB coefficient represents 7.3% of a standard deviation (10.1% of mean absolute OIB), and the non-retail OIB coefficient represents 8.3% of a standard deviation (11.7% of mean absolute OIB).

**Panel A: Abnormal Volume**

| Dep. Var. = | | *Retail* | *Non-Retail* |
|---|---|---|---|
| | Pr. Sign | (1) | (2) |
| *Treated × Post* | + | 0.375*** | 1.062*** |
| | | (9.19) | (6.24) |
| *Size* | | 0.218*** | 1.415*** |
| | | (3.05) | (4.31) |
| *BTM* | | 0.042 | -0.233* |
| | | (1.30) | (-1.78) |
| *ROA* | | 0.729*** | 4.342*** |
| | | (3.22) | (4.02) |
| *Leverage* | | 0.244** | 1.500*** |
| | | (2.18) | (2.62) |
| *Analyst Coverage* | | 0.057*** | 0.326*** |
| | | (6.85) | (6.91) |
| *Return Volatility* | | -7.035*** | -36.738*** |
| | | (-9.92) | (-11.12) |
| Firm FE | | Yes | Yes |
| Year-Month FE | | Yes | Yes |
| N | | 37,043 | 37,043 |
| Adj. $R^2$ | | 0.590 | 0.684 |

**Panel B: Against-News Order Imbalance**

| Dep. Var. = | | *Retail* | *Non-Retail* |
|---|---|---|---|
| | Pr. Sign | (1) | (2) |
| *Treated × Post* | +/- | 0.012** | -0.011** |
| | | (2.03) | (-2.17) |
| *Size* | | 0.010* | 0.013*** |
| | | (1.87) | (2.83) |
| *BTM* | | 0.000 | -0.005 |
| | | (0.06) | (-1.57) |
| *ROA* | | -0.068*** | -0.087*** |
| | | (-3.00) | (-4.20) |
| *Leverage* | | -0.006 | -0.007 |
| | | (-0.76) | (-0.94) |
| *Analyst Coverage* | | -0.001* | 0.000 |
| | | (-1.73) | (0.87) |
| *Return Volatility* | | -0.016 | 0.004 |
| | | (-0.22) | (0.06) |
| Firm FE | | Yes | Yes |
| Year-Month FE | | Yes | Yes |
| N | | 37,043 | 37,043 |
| Adj. $R^2$ | | 0.015 | 0.019 |

**Table 5. VAR and Abnormal Spread**

This table presents the results from difference-in-differences regressions examining the effect of Virtual Analyst Reports on abnormal effective spreads around earnings announcements. The dependent variable is the trade-count-weighted effective spread over [0, +2] minus [-41, -11], multiplied by 100. Column (1) includes firm and year-month fixed effects. Column (2) adds controls. All variables are defined in Appendix B. The t-statistics are reported in parentheses, and standard errors are clustered by firm. *, **, and *** indicate statistical significance at the 0.10, 0.05, and 0.01 levels, respectively. The coefficient of 0.019 represents 9.1% of a standard deviation and 22.6% of the mean absolute abnormal spread.

| Dep. Var. = | | *Abnormal Spread* | |
|---|---|---|---|
| | Pr. Sign | (1) | (2) |
| *Treated × Post* | + | 0.021** | 0.019** |
| | | (2.38) | (2.16) |
| *Size* | | | 0.017** |
| | | | (2.10) |
| *BTM* | | | -0.001 |
| | | | (-0.09) |
| *ROA* | | | -0.094** |
| | | | (-2.27) |
| *Leverage* | | | -0.014 |
| | | | (-0.88) |
| *Analyst Coverage* | | | 0.000 |
| | | | (-0.60) |
| *Return Volatility* | | | 0.648*** |
| | | | (4.12) |
| Firm FE | | Yes | Yes |
| Year-Month FE | | Yes | Yes |
| N | | 37,095 | 37,095 |
| Adj. $R^2$ | | 0.095 | 0.097 |

## Table 6. Cross-Sectional Tests

This table presents the results from triple-interaction regressions examining cross-sectional variation in the treatment effect. Panel A examines whether the effect varies with the strength and one-sidedness of pre-announcement Seeking Alpha coverage paired with the magnitude of the earnings surprise. Consensus Tone equals one for firm-quarters with strong, one-sided coverage and a large earnings surprise. The sample for Panel A is restricted to firms with at least one Seeking Alpha article in the prior three months. Panel B examines whether the effect varies with the firm's information environment. Analyst Coverage is the number of I/B/E/S analysts; Firm Size is the natural log of one plus lagged total assets. All variables are defined in Appendix B. The t-statistics are reported in parentheses, and standard errors are clustered by firm. *, **, and *** indicate statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

**Panel A: Article Consensus Tone**

| Dep. Var. = | | *Abnormal Turnover* | *Abnormal Spread* |
|---|---|---|---|
| | Pr. Sign | (1) | (2) |
| *Treated × Post × Consensus Tone* | + | 0.866** | 0.023* |
| | | (2.01) | (1.71) |
| *Treated × Post* | | 0.087 | -0.002 |
| | | (0.46) | (-0.39) |
| *Post × Consensus Tone* | | -0.472 | -0.026* |
| | | (-1.15) | (-1.94) |
| *Treated × Consensus Tone* | | -0.290* | -0.003 |
| | | (-1.68) | (-0.41) |
| *Consensus Tone* | | 0.265 | 0.006 |
| | | (1.59) | (0.85) |
| *Size* | | 0.238 | 0.005 |
| | | (1.24) | (1.04) |
| *BTM* | | 0.126 | -0.002 |
| | | (0.99) | (-0.43) |
| *ROA* | | 2.218** | 0.005 |
| | | (2.39) | (0.21) |
| *Leverage* | | 0.684** | -0.002 |
| | | (2.46) | (-0.27) |
| *Analyst Coverage* | | 0.066*** | -0.000 |
| | | (3.87) | (-1.55) |
| *Return Volatility* | | -36.786*** | -0.266*** |
| | | (-8.85) | (-3.16) |
| Firm FE | | Yes | Yes |
| Year-Month FE | | Yes | Yes |
| N | | 27,636 | 27,636 |
| Adj. $R^2$ | | 0.271 | 0.167 |

**Panel B: Information Environment**

| Dep. Var. = | | *Abnormal Turnover* | *Abnormal Spread* | *Abnormal Turnover* | *Abnormal Spread* |
|---|---|---|---|---|---|
| | | (1) | (2) | (3) | (4) |
| | Pr. Sign | X = Sell-Side Analyst Coverage | | X = Firm Size | |
| *Treated × Post × X* | - | -0.121** | -0.006** | -0.309*** | -0.011* |
| | | (-2.31) | (-2.38) | (-2.82) | (-1.84) |
| *Treated × Post* | | 1.067*** | 0.045** | 2.800*** | 0.099* |
| | | (3.34) | (2.47) | (3.24) | (1.94) |
| *Post × X* | | 0.119** | 0.005** | 0.202* | 0.010* |
| | | (2.30) | (2.19) | (1.89) | (1.66) |
| *Treated × X* | | 0.003 | 0.005 | -1.451*** | 0.031 |
| | | (0.05) | (1.47) | (-3.20) | (1.25) |
| *X* | | 0.069 | -0.005 | 1.165*** | -0.008 |
| | | (1.17) | (-1.43) | (2.74) | (-0.33) |
| *Size* | | 0.053 | 0.017** | — | — |
| | | (0.30) | (2.08) | | |
| *BTM* | | 0.103 | -0.001 | 0.121 | -0.001 |
| | | (0.93) | (-0.10) | (1.11) | (-0.08) |
| *ROA* | | 2.865*** | -0.094** | 2.492*** | -0.092** |
| | | (3.64) | (-2.27) | (3.20) | (-2.23) |
| *Leverage* | | -0.053 | -0.014 | -0.020 | -0.016 |
| | | (-0.20) | (-0.93) | (-0.08) | (-1.00) |
| *Analyst Coverage* | | — | — | 0.075*** | -0.000 |
| | | | | (4.60) | (-0.74) |
| *Return Volatility* | | -63.758*** | 0.648*** | -63.252*** | 0.643*** |
| | | (-15.01) | (4.11) | (-14.93) | (4.10) |
| Firm FE | | Yes | Yes | Yes | Yes |
| Year-Month FE | | Yes | Yes | Yes | Yes |
| N | | 37,095 | 37,095 | 37,095 | 37,095 |
| Adj. $R^2$ | | 0.294 | 0.097 | 0.296 | 0.097 |

**Table 7. VAR and Price Efficiency**

This table presents the results from difference-in-differences regressions examining the effect of Virtual Analyst Reports on price discovery. The dependent variable is adjusted intraperiod timeliness over [0, +20], restricted to observations with $|CumAR_{20}| \geq 2\%$. Higher values indicate faster price discovery. Column (1) includes firm and year-month fixed effects. Column (2) adds controls. All variables are defined in Appendix B. The t-statistics are reported in parentheses, and standard errors are clustered by firm. *, **, and *** indicate statistical significance at the 0.10, 0.05, and 0.01 levels, respectively. The coefficient of −1.129 represents 9.5% of a standard deviation and 9.3% of the mean absolute adjusted IPT.

| Dep. Var. = | | *Adjusted IPT [0,20]* | *Adjusted IPT [0,20]* |
|---|---|---|---|
| | Pr. Sign | (1) | (2) |
| *Treated × Post* | - | -1.078*** | -1.129*** |
| | | (-2.66) | (-2.80) |
| *Size* | | | 0.809* |
| | | | (1.65) |
| *BTM* | | | -0.556* |
| | | | (-1.95) |
| *ROA* | | | -1.527 |
| | | | (-0.75) |
| *Leverage* | | | 0.236 |
| | | | (0.31) |
| *Analyst Coverage* | | | 0.030 |
| | | | (0.52) |
| *Return Volatility* | | | -10.764 |
| | | | (-1.52) |
| Firm FE | | Yes | Yes |
| Year-Month FE | | Yes | Yes |
| N | | 31,724 | 31,724 |
| Adj. $R^2$ | | 0.052 | 0.053 |

**Table 8. Tariff Robustness Test**

This table presents the results after excluding firms in SIC 33 (Primary Metal) and SIC 37 (Transportation Equipment), industries most affected by tariffs during the sample period. All variables are defined in Appendix B. The t-statistics are reported in parentheses, and standard errors are clustered by firm. *, **, and *** indicate statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

| Dep. Var. = | | *Abnormal Turnover* | *Abnormal Spread* |
|---|---|---|---|
| | Pr. Sign | (1) | (2) |
| *Treated × Post* | + | 0.511*** | 0.018** |
| | | (3.19) | (2.04) |
| *Size* | | 0.057 | 0.017** |
| | | (0.31) | (2.00) |
| *BTM* | | 0.097 | -0.002 |
| | | (0.87) | (-0.30) |
| *ROA* | | 2.869*** | -0.101** |
| | | (3.57) | (-2.35) |
| *Leverage* | | -0.077 | -0.017 |
| | | (-0.28) | (-1.10) |
| *Analyst Coverage* | | 0.075*** | -0.000 |
| | | (4.48) | (-0.51) |
| *Return Volatility* | | -62.760*** | 0.647*** |
| | | (-14.76) | (4.05) |
| Firm FE | | Yes | Yes |
| Year-Month FE | | Yes | Yes |
| N | | 35,938 | 35,938 |
| Adj. $R^2$ | | 0.294 | 0.097 |

**Table 9. Placebo Test**

This table presents the results from a placebo test using a sample that is restricted to the pre-actual-treatment period (2022Q1 through 2024Q4). The placebo treatment quarter (2023Q4) is excluded. All variables are defined in Appendix B. The t-statistics are reported in parentheses, and standard errors are clustered by firm. *, **, and *** indicate statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

| Dep. Var. = | *Abnormal Turnover* | *Abnormal Spread* |
|---|---|---|
| | (1) | (2) |
| *Treated × Placebo Post* | 0.077 | 0.013 |
| | (0.79) | (1.45) |
| *Size* | 0.148 | 0.014 |
| | (1.21) | (1.64) |
| *BTM* | -0.031 | -0.000 |
| | (-0.33) | (-0.06) |
| *ROA* | 1.924*** | -0.026 |
| | (2.64) | (-0.55) |
| *Leverage* | 0.301 | -0.024 |
| | (1.45) | (-1.57) |
| *Analyst Coverage* | 0.018* | 0.001 |
| | (1.81) | (1.57) |
| *Return Volatility* | -50.326*** | 0.378** |
| | (-12.52) | (2.21) |
| Firm FE | Yes | Yes |
| Year-Month FE | Yes | Yes |
| N | 36,193 | 36,193 |
| Adj. $R^2$ | 0.339 | 0.061 |

**Table 10. Robustness Tests**

This table presents the results from robustness tests for abnormal turnover and abnormal spread. Columns (1)-(2) apply entropy balancing to match pre-period covariate means between treated and control firms. Columns (3)-(4) exclude mega-cap firms (size decile 10). Columns (5)-(6) remove the price filter (price > $1). All variables are defined in Appendix B. The t-statistics are reported in parentheses, and standard errors are clustered by firm. *, **, and *** indicate statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

| Dep. Var. = | *Abnormal Turnover* | *Abnormal Spread* | *Abnormal Turnover* | *Abnormal Spread* | *Abnormal Turnover* | *Abnormal Spread* |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Entropy Balancing | Entropy Balancing | No Mega-Cap | No Mega-Cap | No Price Filter | No Price Filter |
| *Treated* × *Post* | 0.409*** | 0.017** | 0.637*** | 0.020** | 0.829*** | 0.022** |
| | (2.69) | (1.99) | (3.79) | (2.17) | (5.10) | (2.40) |
| Firm FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year-Month FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| N | 22,080 | 22,080 | 32,520 | 32,520 | 38,566 | 38,566 |
| Adj. $R^2$ | 0.241 | 0.057 | 0.303 | 0.094 | 0.284 | 0.084 |